

## The Rhetoric of Sociological Facts<sup>1</sup>

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*To develop inductively a preliminary list of rhetorical tools that sociologists use in their presentation of facts, we examine three general claims that are widely accepted as having been demonstrated empirically in the field of social movements: social networks are necessary for recruitment of new members; individual mental traits do not matter; and political opportunities are necessary for movement emergence. We identify eight rhetorical tools that helped promulgate these claims as facts. We all use techniques like these, but awareness about them can help us evaluate our arguments and find better ways to test them.*

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**KEY WORDS:** rhetoric; social movements; sociology of sociology; facts; claimsmaking.

### INTRODUCTION

Most sociologists today agree that we live in a world that is to a large extent socially constructed, in which it is hard to get at underlying, objective truths. Even the most scientific claims about the world rely on rhetoric to persuade others (Latour and Woolgar, 1979), and it is no accident that the study of rhetoric is the world's oldest tradition of social constructionism (Booth, 2004). Yet most debunking of ideas as socially constructed merely shows there are interests behind the ideas, without specifying how those in power convince others that their ideas are true. Habermas (1990:202) calls for the ideal speech situation, for instance, defined as the ability of all involved to say yes or no, without describing all the ways it becomes distorted in the first place. Spector and Kitsuse (1987), in a

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famous work on claims making about social problems, concentrate on who makes claims in what arenas—but say nothing about the rhetorical thrust of those claims. Power never does anything by itself. How do we persuade others in sociological arguments, for instance? What are the rhetorical tools we use when we make factual claims, tools that influence whether we succeed or fail in persuading others?

In a preliminary inductive effort to catalogue some of the rhetorical tools that sociologists use, we examine three claims about social movements that have been widely accepted in the last 40 years, taking them from efforts to summarize the field. Although we raise some theoretical questions about them, we are primarily interested in the regularly cited evidence. Were the claims made in a falsifiable form? How many independent empirical tests were there? Were the tests conducted on diverse social movements, from different times and places? What kind of evidence was taken as supportive of the claims? Were evidentiary standards relaxed for findings that fit readers' predispositions especially well? In general, how are empirical findings presented in ways that make them more likely to be accepted?

Our reading suggests that these claims owe their high standing to rhetorical accomplishments as well as to empirical demonstration. We identify eight rhetorical techniques scholars used to present these claims in the 1980s and 1990s. Like all rhetoric, these mechanisms bolster the credibility of truth claims whatever their accuracy. Claims for which there is considerable evidence are presented rhetorically, just like those for which there is less evidence. But we must try to distinguish the oratory from the facts as best we can ascertain them. The very success of these claims may have led to some "theoretical degeneration" into complacency and tautology (Rule, 1997:194; Smelser, 1992:35). This article is intended to contribute to the skepticism necessary for avoiding that.

As with any work examining the rhetorical techniques of constructing facts, some readers will think we are arguing that these three claims are "wrong" (and thus that we are falling into our own trap, acting as though we can somehow escape from rhetoric to the truth beneath). In fact, we believe that these claims represent some of the most extensively documented knowledge about social movements we yet have. Even the most empirical research relies on rhetorical techniques to reach its audiences.

## RHETORIC IN SOCIOLOGY

It is now widely accepted that scientific arguments depend for their force on rhetorical techniques as well as on the evidence conveyed. This

has been demonstrated repeatedly for the natural sciences (e.g., Gilbert and Mulkay, 1984; Latour and Woolgar, 1979; Mulkay *et al.*, 1983) as well as occasionally for the social (Edmondson, 1984; McCloskey, 1985; Nelson *et al.*, 1987; Simons, 1990). Texts are strategic efforts to gain recognition in interpretive communities (Knorr Cetina, 1999; Latour, 1988). Rhetoric focuses on how these audiences are persuaded, often through appeals to the theoretical and (in the case of science) methodological commitments they already hold. (Not to mention moral, political, and emotional commitments, which we do not address.) These cognitive commitments are commonly described as a paradigm, a web of explicit theory, implicit assumptions, findings, exemplars, and methods (Eckberg and Hill, 1980; Kuhn, 1977). Because most sociologists accept this constructionist view, there has been little debate over it. As a result, the importance of rhetoric and paradigms is asserted in social theory but rarely examined in sociological practice (cf. Bourdieu, 2004; Edmondson, 1984; Simons, 1990). Empirical fields are not used to deepen our understanding of rhetorical processes, and our appreciation of rhetoric is not used to evaluate the knowledge generated in those fields.

An ancient anxiety runs through rhetoric since Socrates: although rhetoric is unavoidable, its tricks can mislead us. The same rhetoric is used to construct falsehoods as to construct truths. Ancient and ongoing philosophical debates demonstrate how difficult it is to distinguish the two. For our purposes, it is enough to isolate the rhetorical tricks without taking a stand on what is left after discounting for them. Some of these techniques represent evidence in the most favorable light; others lead us to draw inferences from it which may or may not be correct. (Other techniques, which we do not examine here, might make a finding seem new, trendy, or distinctive as opposed to true.) In the works we examine, scholars employ a number of rhetorical mechanisms to construct and sometimes exaggerate their shared knowledge. These tools are common throughout sociology. (Table I summarizes them.) In our review of the presentations of the three claims, we found eight such techniques. Four involve how to characterize evidence; four are about drawing implications from the evidence. All are rhetorical moves meant to persuade others of a claim's truth.

*Theoretical ambiguity:* Often, causality is simply fudged in vague theoretical arguments that cannot quite be pinned down, a problem that more precise descriptions might resolve. In place of causal clarity, sociological descriptions frequently substitute a sensibility or language: they rely on

**Table I.** Eight Rhetorical Techniques

Tool	
Representation of Evidence:	
1. Theoretical ambiguity	Fuzzy theoretical and causal arguments, which rely on audiences' unstated assumptions and understandings to fill them in.
2. Conceptual ambiguity	Related concepts substituted for one other, with evidence about one taken as evidence about the others.
3. Methodological formalism	Numbers not interpreted for their substantive, as opposed to statistical, meanings.
4. Proxy evidence	Previous studies cited indiscriminately or out of context, even when their findings are mixed, speculative, irrelevant, or even disconfirming.
Implications Drawn from Evidence:	
5. Exaggeration of causal effects	Weaker causal effects exaggerated into stronger ones.
6. Extrapolation from limited cases	Claims tested with case studies rather than samples of cases, allowing selective use of evidence.
7. Self-testing	General claims made from limited cases; claims not tested directly by other scholars using independent evidence.
8. Misplaced concreteness	Variables inflated into concepts and theories, just as theories are reduced to one or two variables.

the precommitments of their audiences. Sometimes this is a form of political sympathy or antipathy, especially in research on social movements; at other times it represents a theoretical allegiance, such as that to structural imagery or to individualism. As Rorty (1991) and other philosophers have pointed out, there is nothing wrong with this in science: progress comes from one vocabulary for understanding replacing or recontextualizing another. But we must refrain from inflating these sensibilities into stronger causal claims that we insist have been compared to and shown superior to other claims.

*Conceptual ambiguity:* Distinct but related concepts are sometimes jumbled together. In “Fact 1” (see below), informal networks, organizational affiliations, feelings of solidarity, and face-to-face contact are often treated in the same way, despite obvious differences. Evidence about the importance of one is taken as evidence for all. Careful conceptual distinctions are not sustained. This is similar to what Abbott (1997) calls “semantic ambiguity,” that is, when one indicator represents more than one concept.

*Methodological formalism:* The absence of clear causal claims goes hand in hand with unclear interpretations of numbers. What does it mean for 30% of protestors to be recruited through personal networks? Seventy percent? Elaborate traditions of explanatory statistics focus attention on other aspects of numbers, especially how representative they are, more than what they really mean substantively. A tradition of case studies should be better at the latter, but even here numbers are often taken as the resting place for our explanations, sufficient unto themselves. We often need to pay more attention to the substantive meaning of numbers.

*Proxy evidence:* Careless treatment of concepts and causality facilitates the indiscriminate citation of previous studies. Data created to wage one intellectual battle are taken as sacred truths into future battles in which they are less relevant. (Abbott [1997] calls this “contextual ambiguity.”) Much regularly cited research is actually designed (and most appropriate) for testing the claims of an even earlier theory. Data are shaped by their polemical context, and it is hard to obtain just the right data to test your own claims. Using other people’s data, gathered to test other claims, is especially treacherous. Research dismissing pejorative psychoanalytic constructs like narcissism or anal retention (out of favor today even among psychoanalysts) as causes of protest is cited to reject all mental states in our explanations. But this tool goes further. Mixed and even disconfirming studies are sometimes included in long citation lists of supposedly supporting evidence.

*Exaggeration of causal effects:* Weak forms of causality are sometimes inflated into stronger ones. Contributing causes are presented as necessary or sufficient. In structural imagery, for instance, variables are frequently presented as invariant causes (Goodwin and Jasper, 2003). Social movements must mobilize support through social networks. Political opportunities determine the emergence of movements. The probabilistic assumptions of multivariate statistics avoid this problem, but case studies of social movements cannot usually avail themselves of this protection.

*Extrapolation from limited cases:* Case studies have their purposes but can be misused (Snow and Trom, 2002). One part of a theoretical argument can be propped up with a single study that seems to support a hypothesis, with a citation to place at the end of a summary sentence. A case study is a fine way to extend substantive understanding and create new concepts, but testing should be based on a sample of cases. Summaries tend to

proceed as though causality has been established through testing, but it rarely has. Research on social movements is dominated by case studies—almost inevitably, given the time it takes to master a complex case. This case-study method requires extra care and precision in our theoretical statements. Case studies (although not only case studies) tolerate a selective use of evidence. Scholars are likely to notice evidence that confirms their theories, when they should be looking for disconfirming evidence. Evidence that neither confirms nor disconfirms, but could provide alternative explanations, tends to be ignored. In addition, cases are often selected precisely because they seem a good example of the theoretical phenomenon in question, and theories are selected because they are a good fit for the case at hand. Finally, in cases where evidence is unclear or unavailable, speculation sometimes supplements the empirical story, but that speculative evidence is projected on the basis of favored theories.

*Self-Testing:* Case studies exacerbate another common problem throughout sociology: few scholars test other scholars' hypotheses unless they come into direct conflict with their own. In a field of a thousand small experiments, like psychology, direct testing is common, but sociologists prefer to develop and "test" their own theories. To the extent that they use case studies and write books rather than articles, this is even more of a problem. But, of course, claims cannot be tested adequately with the same evidence used to develop them (and perhaps not by the same people).

*Misplaced concreteness:* Another rhetorical move is to inflate variables and concepts into theories. The language of political opportunities was a useful metaphor that could be flexibly applied to virtually any political setting. Indeed, scholars use the same underlying root metaphor of opportunity to construct very different lists of what count as opportunities (some narrow, others quite broad). This useful sensitizing concept was transformed into a theoretical claim, which in turn was thought to have been tested. A variable becomes a hypothesis, then a theory, then an entire approach or paradigm (Meyer, 1999). As a result, it can no longer easily take its place alongside other variables in encompassing explanations. The same kind of oversimplification through conflation is used to "disprove" theories. If participants can be shown to belong to organizations, or if grievances are shown to be unimportant, then the entire collective-behavior tradition can be rejected, and with it the mental variables it highlighted. (A more sensible approach might have been to incorporate some of that tradition's insights in the form of variables, which in turn could have been merely

redefined in the light of the cognitive/cultural revolution.) Polemical stands lead us to exaggerate findings.

To see the rhetorical tools at work, we turn to three prominent findings in the field of social movements. All three were crucial to the new view of collective action which emerged in the 1970s, as not being driven by grievances or social and psychological strain, as an older generation had seen it. We do not look at the latest evidence about these three claims. Rather, we begin with several widely read efforts, from the last 20 years, to summarize knowledge about social movements (namely, della Porta and Diani, 1999; Klandermans, 1997; McAdam, 1996; McAdam *et al.*, 1988; McCarthy, 1996; Tarrow, 1998). Then we examine the empirical studies they cite. In this way, we can see what evidence was used, and how it was used, to establish the three facts as “what is known” about movements. Whether the claims have held up well in recent years is irrelevant.

### **FACT 1: SOCIAL NETWORKS ARE NECESSARY FOR RECRUITMENT OF NEW MEMBERS**

Most scholars would call this the most thoroughly documented finding about social movements. It has not lacked critics, who have argued that social networks are ubiquitous in social life and so explain little (Piven and Cloward, 1992), that powerful emotional symbols can sometimes administer moral shocks that substitute for personal ties (Jasper and Poulsen, 1995), and that networks vary enormously in ways not yet researched (Oliver, 1984). But most summary discussion of social movements has elaborated the main theme that networks are key to recruitment.

“However large a movement’s mobilization potential may be,” says Klandermans (1997:24), “it will be unable to activate its potential if it lacks a network for recruitment into action. To form and activate recruitment networks, a movement must join forces with other organizations and establish ties with already existing formal and informal networks, and it must develop its own organization at both the national and local levels (Ferree and Miller, 1985; Wilson and Orum, 1976).” He additionally cites Snow *et al.* (1980) and McAdam and Paulsen (1993). At first it is not clear if Klandermans is talking about a movement’s maximum potential or any of its potential, but it becomes apparent he means the latter. Networks are necessary for mobilization.

After a similar claim, qualified by the phrase “most often” (implying “not always”), Tarrow (1998:22) writes: “[The importance of networks to

recruitment] has been made clear through recent research both in the laboratory and in the real world of movement mobilization.” However, he only cites laboratory studies: Gamson *et al.* (1982) and Dawes *et al.* (1988). Tarrow distinguishes social networks from “face-to-face groups” and “connective structures” (themselves defined obscurely as things “that link leaders and followers, center and periphery, and different parts of a movement sector, permitting coordination and aggregation between movement organizations and allowing movements to persist even when formal organization is lacking” [1998:124]). It is hard to see how these two differ from social networks, but at any rate Tarrow insists that networks are more important for recruitment than the others: “Social networks at the base of society [note the unnecessary structural imagery] have emerged as the most common sources of recruitment into social movements” (Gould, 1995; McAdam, 1988). The term “sources” is vague enough that this could mean either that networks have some causal effect in and of themselves, or that they are simply the site where other causal processes operate. The qualification “most common” does suggest a more precise hypothesis: more than 50% of recruits come through social networks.

McCarthy (1996:142) also states: “Kinship and friendship networks have been shown to be central to understanding movement recruitment (Snow, Zurcher, and Eckland-Olson 1980) as well as the formation of emergent social movement groups (McCarthy and Wolfson, 1988).” In their encyclopedia article on social movements, McAdam *et al.* (1988:707) cite the longest list of studies in support of the claim that “prior contact with another movement participant” has “the strongest relationship to activism”—Briët *et al.* (1987), Bolton (1972), Gerlach and Hine (1970), Heirich (1977), McAdam (1986), Orum (1972), Snow (1976), Snow *et al.* (1980), Von Eschen *et al.* (1971), and Zurcher and George Kirkpatrick (1976).

We see a variety of formulations in these summaries. They are clearly causal statements, but few are clear causal statements. Some seem to see social networks as necessary causes, while for others they are merely contributing factors. So what is the empirical evidence the summaries cite?

The central citation, a meta-analysis, is Snow *et al.* (1980), who found 10 empirical efforts to measure how many participants in a movement were recruited through personal networks. Eight were studies of religious movements, one the large March of Dimes study, and one a study of 31 recruits to an anti-abortion group. The authors added their own evidence from a religious movement (the same as Snow, 1976) and a study of University of Texas undergraduates. Two of the studies of religious movements, both of the exclusive Hare Krishna, found that less than 5% of recruits were attracted through interpersonal associations and networks.

The remaining six studies of other religious movements reported considerable variation in the percentage of recruits attracted through relatives, friends, or acquaintances: anywhere from 100% in one study of the Sokagakkai movement to 59% in a study of Catholic Pentecostals. The overall mean for these religious movements was 83% recruitment through informal networks. In their own survey of University of Texas undergraduates, Snow *et al.* found that 63% of the 81 students involved in political movements were recruited through networks as compared to 80% of the 54 students involved in religious movements. Snow *et al.* modestly claim that most participants in most movements are recruited at least partly through social networks. They do not say that network ties increase the chances of participation (which would require samples of nonparticipants as well), even though many later authors present them as saying just that (e.g., della Porta and Diani, 1999:113, Tool 4; see Table I). Snow *et al.* also point to variations in network recruitment, a suggestion lost in most later citations (Tools 5 and 8; see Table I).

Heirich's (1977) study of Catholic Pentecostals, cited by McAdam *et al.* (1988), is consistent with these findings. Of the converts he interviewed, almost two-thirds claimed to have been introduced to the movement by a trusted associate or to have known someone in the movement. However, without similar data for nonconverts, Heirich cautions against generalizing about the effects of networks. Gerlach and Hine's (1970) study, also cited by McAdam *et al.* (1988), is a comparative study of Pentecostals and the Black Power movement, but data on recruitment were collected only for Pentecostals. (These data are included in Snow *et al.*, 1980.)

The second most cited empirical referent is probably McAdam's (1986, 1988) research on Freedom Summer applicants in 1964. The explanatory cachet of this research comes from comparing participants with "no-shows" in the pool of accepted applicants. This pool was almost entirely made up of students from elite northern colleges. In this case, activists' explicit strategy was to recruit students through networks (campus civil rights groups cast a "narrow recruiting net" [1988:41]), so the evidence says little about the importance of networks versus other mechanisms. In McAdam (1986), logit analysis found that interpersonal network ties to other participants predicted who participated versus who dropped out of the process along the way. However, as later reported in McAdam and Paulsen (1993), these findings resulted from a misspecified model. The influence of interpersonal ties was washed out by "salient" identifications with key organizations, or what the authors termed "recruitment contexts," a variable (and another unnecessarily structural image) absent from earlier reports. As McAdam and Paulsen (1993:663) explained the

discrepancy, “the volunteers’ ties to other volunteers were themselves the function of the participants’ greater integration into specific recruitment contexts.” Formal organizational affiliation and the cultural work of identity construction explained the recruitment formerly attributed to network ties (Tools 2 and 6; see Table I).

So far, almost all the evidence is based on religious movements and college students (Tool 7; see Table I). Jasper and Poulsen (1995) point out that both may differ considerably from other political movements. Normally, an adult joins a political movement because it resonates with deeply held beliefs and values. Religious movements differ in that they involve a fuller “conversion” to new beliefs. College students are less likely to have well-formed and tested belief systems, so that recruitment to a movement can also involve considerable conversion of beliefs (thus possibly leaving them more open to the influence of social ties). Finally, the social networks of students are mediated by the formal organizational structures of college life. At any rate, such differences are rarely explored in network research on recruitment (Tool 1; see Table I).

What other evidence is there for the network hypothesis? Wilson and Orum (1976) and Ferree and Miller (1985), cited by Klandermans, are purely theoretical. Others give impressionistic evidence. Bolton’s (1972) study of 95 members and 222 nonmembers of suburban peace organizations is cited as supporting the hypothesis. In what is almost an appendix to a study that otherwise finds that individuals’ political attitudes, feelings of alienation, and modes of explaining social phenomena matter when it comes to participation, Bolton briefly concludes from 25 interviews that recruitment is “less often the result of self-selection of the group by the recruit than of being recruited through belonging to social networks” (1972:557). Unlike the ideological and psychological claims, no data are given for this one (Tool 4; see Table I).

Other key cites are also studies from the early 1970s, concerned mainly with countering older mass-society explanations of collective behavior. They demonstrated that participants in movements and civil disobedience are not socially atomized or isolated, but embedded in networks and organizations (as indeed most people are). Orum’s (1972) analysis of a 1964 survey of 3,500 black college students found that the more “parapolitical” organizations a student belonged to, the more likely he or she was to participate in civil rights activities. (But his gamma statistic was a + 0.26, no doubt because even 69% of students who belonged to no organizations nonetheless participated.) Von Eschen *et al.* (1971:533) reported that 62% of the 386 participants in sit-in protests along Route 40 in Maryland in 1961 were centrally involved in at least one organization other than a church and/or those organizations sponsoring the

demonstration. First, because they were simply concerned to disprove earlier “anomie” explanations, many early studies did not distinguish initial recruits from ongoing participants. Part of being in a movement entails joining organizations and developing social ties. That participants had such affiliations does not mean they joined because of them. Second, like the Freedom Summer data, these studies primarily found organizational affiliations, not personal networks, to be important—a distinction frequently glossed over (Tools 2, 4, and 8; see Table I).

Zurcher and George Kirkpatrick’s (1976) study of two anti-pornography organizations provides direct evidence on recruitment, but it hardly favors the network claim. In one organization, 75% of the leading members joined because of “official” contacts from leaders that followed inter-organizational lines, and only 25% indicated that contact with “friends” led to affiliation. In the other, 56% joined because of official contacts, 44% because of contacts with friends. (The study’s more central finding is that 89 and 94% of members were affiliated with one or more community organizations [Tool 4; see Table I].)

These early studies provided evidence that social movements contain many participants with one or more ties to voluntary associations. Those who cite this research in favor of the network thesis on recruitment see evidence that organizational ties lead to bloc recruitment, but the studies do not unequivocally support this conclusion (Tools 2 and 4; see Table I). The evidence could just as easily fit social-capital arguments about political participation or constructionist arguments that variations in cultural know-how explain differential participation. Organizational membership may be an aspect of participating, not a cause of it. Without control groups of nonparticipants, neither social networks nor organizational affiliations can be said to have any particular effects. (It is possible, furthermore, that many recruits with personal ties to other participants would be recruited even in the absence of those ties [Tool 1; see Table I].)

Gould’s (1991, 1995) research on resistance to the national army during the Paris commune of 1871 is also cited in support of the network claim. Gould’s main concern (1995) was to show the impact of group solidarity on participation and that of membership in formal organizations on solidarity, but he also argued that the examples provided by one’s fellows affected participation. He had no systematic data about this, but instead quoted interrogations of those later arrested (1995:177–182). Yet none of the participants he quoted mentions preexisting ties with others. (In fact, one of Gould’s general points was that organizations can create ties [1995:154]). Instead, many mentioned the threat of being shot or beaten up if they did not go along: hardly the stuff of social network solidarity. Gould’s data on battalions and neighborhoods, while powerful,

demonstrate aggregate variations in commitment or persistence and not variations in individual recruitment. The causal mechanisms remain obscure and, like most complex historical cases, many potential causal variables are not included in the models. Gould's data do not exactly show what network proponents often claim (Tools 2 and 4; see Table I).

The experimental studies of Dawes *et al.* (1988) and Gamson *et al.* (1982), cited by Tarrow (1998), provide little more than equivocal support for the network hypothesis. These studies point to the influence of solidarity on collective action, but they do not directly address recruitment. Dawes *et al.* (1988) found that group identity fosters cooperative solutions in prisoner's dilemma games, establishing a nonegoistic solution to free riding by isolating the variable of intergroup communication in experiments that control for side payments and reputational effects, that is, non-solidarity solutions (Tool 4; see Table I).

The same is true for Gamson *et al.*'s (1982) fabricated encounters with "unjust authority." Subjects in experimental hoaxes were recruited through newspaper ads to participate in a study in which a "research firm" claimed to represent an oil company in a lawsuit that turned on issues of community standards. The encounters were fabricated to look as if the company were trying to manipulate participants to benefit its clients. Researchers looked for the factors leading to successful rebellion by the group, concluding (among other things) that for groups to rebel, "participants must come to see their fellow group members as comrades" (1982:107).

Both experiments show how face-to-face interaction can lead to solidarity and collective action. But this solidarity does not require preexisting networks; it formed among strangers. These experiments do not show that solidarity is limited to face-to-face interactions (Brewer and Kramer, 1986; Kramer and Brewer, 1984). Solidarity can be imagined or mediated over stretches of time and space through oral tradition, the written word, telegraph, radio, television, or the Internet. As such, various communication channels might contribute to movement recruitment and coordination. Indeed, in some cases, social movement activists are less tied to proximate social networks like neighborhoods than are sympathetic nonparticipants or "free riders" (Walsh and Warland, 1983). Solidarity, a crucial cultural and psychological factor, is consistently downplayed in favor of networks, the structural concept (Tools 2 and 5; see Table I).

Della Porta and Diani (1999) cite Walsh and Warland (1983) in support of the network claim. In a survey of 149 activists in four communities around the Three Mile Island nuclear plant and a random sample of 288 free riders in the same area, Walsh and Warland found that previous ties to activists significantly affected participation in protests after the nuclear

accident. In a logit regression, however, this influence was not as strong as that of education, liberal ideology, or discontent—factors of a sort that (as we will see in the next section) are usually claimed to be insignificant in explaining movement participation. Because of missing data they did not include organizational affiliations in their final analysis (Tools 4 and 5; see Table I).

In the best study, with proper control comparisons, Briët *et al.* (1987) studied the recruitment efforts of three women's groups in one Dutch town. In a survey of more than 600 women, they compared modes of contact made by the groups and their influence on participation. There was considerable variation across the groups, suggesting that the way people are recruited may differ significantly by type of movement and organization. In general, however, for the 31 women who did participate, contacts through mass media were least effective, face-to-face contacts slightly more effective, and a combination of the two most effective—partial support for the network hypothesis (Tools 4 and 5; see Table I).

In sum, the evidence for the hypothesis is mixed, with studies testing a variety of claims. Some are about preexisting ties, others about ties created through protest. Some examine organizational affiliations, others informal personal ties, others both together. A series of questions are conflated. Is one more likely to hear about a movement if one knows people in it? If contacted by the movement, is one more likely to join if one knows people in the movement? If contacted by friends or family, is one more likely to join than if contacted by strangers? Looking at the percentage of people already in a movement who say they were recruited through friends and family (as most studies do, sampling on the dependent variable) does not isolate the effects of networks. How about nonparticipants? How many of them know people in the movement? What do networks supply—ideas, information, practical aid, or emotional loyalties? Only Briët *et al.* (1987) surveyed people prior to participation and examined what effects different kinds of contact had on them.

The evidence also varies in the numeric results. What kind of numbers should count as evidence for the network claim? McAdam *et al.* (1988) cite Zurcher and Kirkpatrick even though only 25 and 44% of participants were recruited through friends in their cases—hardly the 51% minimum implied by Tarrow. Snow *et al.* are cited even though two of their cases yielded figures under 5%. Any recruitment through networks, no matter how small a proportion, is accepted as positive evidence. This seems to demonstrate the power of preconceptions (Tool 3; see Table I).

There is no magic cut-off number for distinguishing favorable from unfavorable evidence. Instead, the evidence all along suggested a range of figures, from very high to very low. Instead of directing research into this

obvious variation, distinguishing different kinds of movements and recruitment mechanisms, scholars with a structural vision made a variable model into an invariant one, contributing factors into necessary ones (Goodwin and Jasper, 2003) (Tools 5 and 8; see Table I).

Another variation in the impact of social networks on participation has been even more ignored: the negative one (Goodwin, 1997; Gould, 1995:186; Walsh and Warland, 1983). Because scholars have typically sampled on the dependent variable (i.e., looked at those recruited to a movement), they cannot say whether tight network ties encourage participation more or less frequently than they discourage it. Much less can they say when ties have one effect or the other. Numerous causal effects are ignored. Despite the mixed tests, the network hypothesis has at least been examined for a range of movements in various countries, eventually far beyond the religious movements and student populations that first supported it.

## FACT 2: INDIVIDUAL MENTAL TRAITS DO NOT MATTER

According to Klandermans (1997:4), “research on social movement participation demonstrates time and again that to become active in a social movement, one doesn’t need any special personality traits” (cf. Keniston 1968; Klandermans 1983, 1989; McAdam *et al.*, 1988).<sup>4</sup> McAdam *et al.* (1988:705–706), after reviewing “psychological” accounts that emphasize “character traits or stressful states of mind that dispose the individual toward participation,” merely quote two summary statements: “Mueller [they mean Muller] (1980:69) concludes that ‘psychological attributes of individuals, such as frustration and alienation, have minimal direct impact for explaining the occurrence of rebellion and revolution per se.’” Echoing this view, Wilson and Orum (1976:189) offer a similar assessment of the empirical record. ‘We conclude,’ they say, ‘that the many analyses ... of collective actions during the past decade, impress upon us the poverty of psychology; or, at the very least, the limitations of psychology.’” Few other accounts even bother to dismiss personality.

Even attitudes matter little. Most summaries refer to the “vast” psychological literature showing that attitudes are poor predictors of

<sup>4</sup> Some confusion enters through the unsettled usage concerning “cf.” From the Latin *confer*, meaning to bring together in one place, but especially in a hostile way, “cf.” traditionally listed exceptions, in the sense of “But on the other hand, see ...” Now, it is more often used to cite supporting references. In this case, Keniston (1968) does not seem to support Klandermans’ claim, while the others do.

behavior. The central cite here is Wicker (1969). McAdam *et al.* (1988:706) quote McPhail (1971) to the effect that "individual predispositions are, at best, insufficient to account" for participation. But they admit that certain attitudes may preclude participation in certain movements. (You are unlikely to join the abortion-rights movement if you believe abortion to be a sin.) Like everyone else, they mostly cite Klandermans and Oegema (1987) to clinch the relative unimportance of attitudes. So does Klandermans himself (1997), curiously adding Walsh and Warland (1983), who found ideology and discontent to be significant predictors of protest. The implication is that attitudes are hardly worth bothering with.

Nor are grievances, in some visions. Jenkins and Perrow claim that research "casts doubt on the classic 'discontent' formulations" (1977:250). In their study of farmworkers, they assume that discontent is relatively constant and hence a poor predictor of insurgency. Skocpol (1979:115) makes the same claim for peasants, arguing that they "always have grounds for rebellion" and that grievance models try to "turn a constant feature of the peasant condition into an explanatory variable." (Not that she provides any evidence about peasants' states of mind. For evidence that peasant discontent may well vary with economic conditions, see White [1995].)

What is the empirical evidence cited? McPhail's (1971) meta-analysis reported on 10 studies of participation in five riots in the 1960s, evaluating support for deprivation or frustration accounts of collective violence or civil disorder. He computed measures of strength of association for 173 contingency tables showing the relationship between riot participation and deprivation and frustration. Because only 8% of these revealed even a moderate association, McPhail concluded that "there is considerable reason for rejecting the sociological and popular cliché that absolute or relative deprivation and the ensuing frustration or discontent or despair is the root cause of rebellion" (1971:1064). Muller's (1980) exhaustive review of similar survey research on the relationship between frustration and protest or violence qualifies McPhail's conclusion. Muller notes that the studies reviewed by McPhail did not measure the real source of frustration, the discrepancy between expectations and achievement. Muller points to a number of studies employing measures of the discrepancy between "just deserts" and achievements that found modest relationships with participation in violence and protest. Although he concludes that the strong claim that frustration is the main cause of violence and protest must be rejected, he argues that frustration or relative deprivation does matter (Tool 4; see Table I).

Wilson and Orum (1976), convinced of the poverty of psychology, mobilize no empirical data. Klandermans (1997) cites his own 1983 article as evidence that personality traits are irrelevant. The paper itself was a review of 31 studies on the relationship between “Rotter’s I.E. scale” and political action. It is not altogether clear how the equivocal findings of this review on the internal versus external locus of action disproved any possible relationship between personality traits and activism (Tools 2, 4, and 5; see Table I).

Wicker (1969) reviewed 46 studies of attitude-behavior relationships, including those between employees’ work attitudes and job performance, people’s attitudes toward minorities and overt behavior when interacting with them, and maternity-ward patients’ attitudes toward breastfeeding and success at it. He concludes: “Taken as a whole, these studies suggest it is considerably more likely that attitudes will be unrelated or only slightly related to overt behaviors than that attitudes will be closely related to actions” (1969:65) (Tools 2 and 4; see Table I). Walsh and Warland (1983), as discussed above, found that attitudes and alienation were not sufficient to explain participation in movements, but made a significant impact (Tool 4; see Table I).

Klandermans and Oegema (1987) interviewed 114 people over the phone before a 1983 peace demonstration in The Hague, and reinterviewed three-quarters of this random sample after the demonstration. They found that 75% of the sample agreed with the goals of the demonstration but only 4% actually attended. They conclude that attitudes and grievances help account for why 75% were sympathetic, but not why 4% ended up participating. No one cites the same article for another finding: that 70% of sympathizers took the milder step of signing a 1985 peace petition—a high rate of participation that is at least as remarkable as the 4% who went to the 1983 demonstration. As with social networks, it is often hard to say what is a high or a low number: the same one can be taken as evidence for or against the effect of attitudes. Again, it might be more useful to try to explain variations than to proclaim that one invariant relationship (or lack thereof) has been discovered (Tools 3, 4, 5, and 8; see Table I).

It is also not clear how any of this research discredits findings such as Keniston’s (1968), based on 17 intensive interviews with leaders of Vietnam Summer, each 6 to 8 hours in length and coupled with equal time spent observing the person at work. None of the research rejecting psychological factors has tried to duplicate studies of this kind (Tool 7; see Table I). Although each of the radicals he interviewed followed idiosyncratic pathways to a life of activism, Keniston claimed that certain common psychological factors emerged through comparative analysis:

development of a strong moral compass at an early age, acute ambivalence toward parental authority, a sense of being special as a child, a mild identity crisis during adolescence, dissatisfaction with early successes as a young adult, and a shock of moral outrage that aligns with an already strong sense of personal responsibility. (Research on rescuers under the Nazis find some of the same personality traits [Monroe *et al.*, 1991; Oliner and Oliner, 1988].) It seems odd for Klandermans to cite Keniston in favor of the mental-traits hypothesis, although if he used “cf.” in the traditional way then he counted it as an exception (Tool 4; see Table I).

Many of these claims, as with those concerning networks, mix necessary, sufficient, and merely contributing factors. Are “root” causes different from other kinds, for instance? To say that certain personality traits are not absolutely necessary for participation is not to say they do not increase the odds. If grievances are always present, they may still vary in cognitive and emotional salience, as well as in specific formulations that resonate more or less. Nor are personality, attitudes, and discontent adequately distinguished—much less the relationship between them explored—before they are dismissed (Tools 1, 2, and 5; see Table I).

No evidence delves deeply enough to counter claims for an association between personality or mental attributes and activism. We know almost nothing about the effects of internal mental states and personalities—properly measured—on protest (Jasper, 1997:chap. 9). Scholars also ignore the fact that, the closer in time and detail to the actions that those attitudes are measured, the better they are as predictors. (Indicators of “Machiavellian” thinking, for instance, help predict manipulative behavior [Christie and Geis, 1970].) It is also possible that the concept of attitudes is a misguided way to think about mental attributes (Turner, 2002).

The animus against “mental traits” derives from the intellectual situation more than 30 years ago when structuralists were attacking the older collective-behavior approach. The mental traits posited as crucial by that generation and dismissed as irrelevant by their structural successors tended to be pejorative pathologies such as a “need to belong” (Hoffer, 1951) or a personal identity crisis (Klapp, 1969). The best work in the collective-behaviorist tradition, such as Blumer or Smelser’s mix of cultural and social-psychological insights, may have lacked robust tools for expressing causal mechanisms, but it was never seriously engaged by their critics (Jasper, 2004; Young, 2007). In the face of more recent work on frames, identities, ideologies, and emotions, it is possible that we have not properly tested the most promising mental or personality traits at all. Research aimed at disproving anomie and related pathologies theories did not do so (Tool 4; see Table I).

### FACT 3: POLITICAL OPPORTUNITIES ARE NECESSARY FOR MOVEMENT EMERGENCE

This claim was central to the exciting emergence of the political-process perspective out of resource mobilization, and it remains the centerpiece of that well-documented approach. Indeed, variations in political opportunities are also said to explain the creation of aggrieved groups (Schneider and Ingram, 1993), choices of tactics and strategies (Kitschelt, 1986), and a movement's effects (Klandermans, 1997; McAdam *et al.*, 1996:12).

"Grievances, resources and meaning," says Klandermans (1997:167), "may be significant concepts in theories of movement participation, but without the concept of 'opportunities' any attempt at theorizing will necessarily fall short. Political opportunities determine a movement's trajectory and chances of success." European scholars have focused most on durable structural opportunities that vary across nations (e.g., Kitschelt, 1986; Kriesi *et al.*, 1995), whereas Americans more often concentrate on briefer windows of opportunity that change over time (e.g., McAdam, 1982; Meyer, 1990; Smith, 1996). "Precisely because opportunities alter, movements wax and wane" (Klandermans, 1997:180). "My strongest argument," Tarrow says in introducing his book (1998:7), "will be that changes in political opportunities and constraints create the most important incentives for initiating new phases of contention." In this second edition of a book that crowned the political-process model in U.S. research, he concedes that movements sometimes create their own opportunities and that people also act on the basis of perceived threats and outrage. However, these modifications did not disturb the thrust of his argument from the first edition.

According to McAdam *et al.* (1988:699): "Within the context of nation states considerable evidence now exists suggesting the crucial importance of changes in the 'structure of political opportunities' (Eisinger, 1973) to the ebb and flow of movement activity." In addition to Eisinger, they cite Lipsky (1970) for the theory. For evidence, they cite Jenkins and Perrow (1977), McAdam (1982), and Gale (1986). They also cite two cross-national studies of political structures, which do not even seem to fall under Tarrow's purposely narrow definition of political opportunities (Tool 2; see Table I).

Indeed, there is considerable disagreement over what is and is not a political opportunity (Tools 1 and 2; see Table I). McAdam, McCarthy, and Zald distinguished them from regime crises and lessening repression in 1988, but in 1996 McAdam (1996:27) presented this "consensual list" (although it turned out to be less consensual than he had hoped: Goodwin

and Jasper [2003]): the openness of a political system; the stability of its elite alignments; elite allies for the movement; and the state's capacity and propensity for repression. As supporting evidence, McAdam in the same essay (1996:23–24) cites Costain (1992), Smith (1991), Brockett (1991), Meyer (1993), and Tarrow (1989).

Jenkins and Perrow (1977) explained the different fortunes of two attempts to organize farmworkers: the unsuccessful effort by the National Farm Labor Union (NFLU) from 1946 to 1952 and the successful one by the United Farm Workers (UFW) from 1965 to 1972. Their study used the *New York Times Index* to measure “insurgent” activity and favorable or unfavorable actions by government, liberal and labor organizations, and agribusiness (thus restricting themselves to extremely visible interventions subject to the *Times*' reporting biases). They attributed the different outcomes of the two campaigns to changes in the political environment. The NFLU received little external financial support and confronted resistance from public authorities; the UFW attracted resources from liberal organizations and faced a more tolerant or neutral response from authorities.

We are lucky to have an evaluation of this classic by Ganz (2000), a longtime UFW staffer. Ganz agrees that the greater political responsiveness of an emergent liberal coalition was important for the UFW to organize farmworkers, but observes that Jenkins and Perrow cannot explain why the UFW succeeded while its more affluent, contemporary rival, the AFL-CIO's Agricultural Workers Organization Committee (AWOC), failed. A theory of political opportunity structures cannot explain what Ganz calls strategic capacity, which depends on internal organizational structures and networks (Tools 6 and 8; see Table I).

McAdam's (1982) study of black insurgency is possibly the most cited case for political opportunity structures. McAdam argues that political opportunities for African Americans gradually expanded from 1930 to 1954. The decline of “King Cotton” in the 1930s weakened the political conditions that had enforced the political impotence of African Americans. Black migrants out of the rural South left disenfranchisement behind, and in the 1930s African Americans emerged as a component of the new Democratic coalition that undermined the South's veto power in party matters. In the Cold War, U.S. racism emerged as a communist propaganda weapon. In this context, the federal government started to reverse its discriminatory racial policies. These gradually improving political conditions provided insurgents increased leverage necessary to mount a social movement. In 1954, *Brown v. Board of Education* signaled to potential protestors that the federal government was on their side.

But it is possible to read the same facts differently. In becoming dependable Democratic supporters, African Americans essentially removed themselves from strategic calculations. Democratic (and, of course, Republican) presidential candidates would henceforth cater more to white southerners (Phillips, 1970). The potential window of opportunity provided by partisan cleavages never really opened. Furthermore, *Brown* ignited a white backlash in 1954: an increase in violence, the spread of White Citizens Councils, and segregationist rhetoric in that year's elections. The decision provided hope and inspiration, but no federal support for action, no police or military protection for movement participants. The combination of increased threat and a symbol of hope formed the emotional underpinning of the movement despite shrinking political opportunities. This story is just as plausible as one that sees an opening of opportunities (Jasper, 1997:118 (Tools 6, 7, and 8; see Table I).

Smith (1991) employed McAdam's model to explain the emergence of Liberation theology. He presents Liberation theology as a revitalization movement within the Catholic Church, and argues that it emerged because of the confluence of political opportunities, organizational strength, and insurgent consciousness. However, the "political opportunities" he identifies—the New Theology from Europe in the years after World War II, Catholic Social Teaching, Vatican II, and organizational crisis within the Latin American church—were mainly doctrinal and theoretical developments, showing no overlap with McAdam's "consensual list" (Tools 1, 6, 7, and 8; see Table I).

Gale (1986) illustrates a six-stage model of social-movement development with selected evidence from the environmental movement, highlighting interaction with government agencies and countermovements in explaining movement emergence, maturity, and demise. The empirical dimension of this paper, however, is not meant to test the model but simply to clarify its dimensions (Tool 4; see Table I).

Costain's (1992) study of the women's movement focuses on the lobbying and legislative activities of interest-group feminism, which flourished under supportive administrations and retrenched under others. But this correlation between the movement and state policies may simply be a function of including only the political wing of the movement. As Taylor (1996) has argued, in the 1980s, when the movement was supposed to be in decline, women were actually involved in vigorous and creative mobilizations in a variety of self-help organizations. Costain supports only the most general language of political opportunities (Tools 1 and 6; see Table I).

Brockett (1991) looked at peasant mobilization in five Central America countries from 1960 to 1984, broadly defining political opportunity

structures as “the configuration of forces in a (potential or actual) group’s political environment that influences that group’s assertion of its political claims” (1991:254). Brockett identifies three salient variables: access to the political system, capacity and propensity of the state for repression, and elite fragmentation and conflict. He does not provide systematic data on peasant mobilizations so it is not always clear what the variance is that these political opportunities help explain (Tool 1; see Table I). Moreover, some forces do not always work in the same direction. For example, repression does not have an “invariant” effect, as government violence in the late 1970s in El Salvador, Guatemala, and Nicaragua fueled peasant activism (Tool 5; see Table I). No wonder Brockett concluded that even with a firm grasp of the varying political opportunities in Central America, “it would be impossible to predict with any certainty the future direction of peasant movements” (1991:269) (Tool 4; see Table I).

Meyer (1993) sees even grievances as political opportunities, analyzing fluctuations in U.S. anti-nuclear protest since the dropping of the atomic bombs on Hiroshima and Nagasaki. He argues that changes in policy and political alignments opened “windows” for the movement. Using the *New York Times Index* and the *Reader’s Guide to Periodical Literature*, Meyer identified two periods of mobilization: 1955 to 1963 and 1981 to 1984. It was during these periods that Eisenhower, Kennedy, and Reagan planned to fight and win nuclear wars. Policy therefore provided “focal points” for mobilization—leaving open the direction of causality. This move contradicts the original insight of political-process approaches, that opportunities to act are more important than grievances in explaining mobilization (e.g., Jenkins and Perrow, 1977). Three years later, Gamson and Meyer (1996) would criticize the concept of political opportunities for being a sponge that tended to soak up any sort of advantage for a movement (Tools 1, 2, 6, 7, and 8; see Table I).

Tarrow (1989) argues that postwar Italian politics consolidated around Communist and Christian Democratic poles. A symbiosis between these patronage-based parties stifled the entrance of new ideas or new groups into politics. In the 1960s this came to an end with a center-left coalition between Christian Democrats and Socialists. The coalition placed new issues on the agenda, introduced conflicts within the ruling government, and opened new spaces to the left of the government for mobilization. All these were exploited by university students, who triggered a cycle of nationwide protests in the late 1960s and early 1970s (Tool 6; see Table I).

In all these studies, political opportunities represent a vague language used to characterize complex situations open to many other interpretations as well (Tools 1 and 8; see Table I). Despite McAdam’s “consensual

list” of opportunities, almost anything that inspires or aids a movement has made someone’s list, from the boll weevil to radical theology, from Reagan’s policies to a new governing coalition in Italy, making this claim unfalsifiable. It may be a useful language, but it is not a testable theory or hypothesis. Plus, every scholar develops his or her own list—from his or her own case study (Tool 6; see Table I). Almost no one tests other people’s lists (Tool 7; see Table I). But if your theory is derived from your case, you can’t very well use that case to test it. Political opportunities are a procrustean concept that can be applied to any case—but only by changing its shape.

A few scholars have tried to test specific opportunities as necessary for mobilization, and the results are not impressive. In studying the Iranian Revolution, for instance, Kurzman (1996) found none of the regular opportunities; it was only necessary for potential revolutionaries to think they had some chance (although even that is not certain). Even more persuasively, Goodwin organized a panel of tests of McAdam’s “consensual list” of four opportunities across 100 disparate cultural, political, and revolutionary movements (summarized in Goodwin, 2000). In 41 of these, none of the four opportunities was present, even by fairly liberal definitions. In another 24, only one was present. Out of the 59 cases in which one or more political opportunities were relevant, furthermore, the opportunity was contracting rather than expanding in 20 of them! Again we see considerable variation.

## DISCUSSION AND CONCLUSION

Rhetorical techniques help cover over the kinds of weaknesses in evidence that are almost inevitable in social science. The tools are used to make the evidence more plausible to readers, especially to the extent that the rhetoric is invisible. Let us review the evidence, especially that available at the time the summaries were written.

Were the claims made in a fully falsifiable form? The network hypothesis could have been made into a testable statement—perhaps by specifying a numeric cutoff—but it rarely was. Instead, any number, no matter how low, was taken as positive evidence. The mental-traits claim could also have been put in more specific, falsifiable forms, but no one formulated it so as to test it. The political-opportunity claim was vague and shifting in its meaning. Falsifiability is not everything in science, but its absence demands modesty in our claims about what has been demonstrated. (On the difficulty of falsifiability in this field, see Rule [1988].)

How many independent empirical tests were there? The network hypothesis was “tested” most often, although most of these tests were not very rigorous. Many were addressing older hypotheses about anomie; others were qualitative impressions. The hypothesis about mental traits does not seem to have had serious tests—perhaps because it was not formulated in clearly testable forms, and perhaps because intellectual trends so heavily favored it. Political-opportunity language was applied to many cases, but almost none of them constituted rigorous tests of specific predictions.

Were the tests conducted on different types of social movements? After an initial concentration on religious and student recruitment, scholars examined the network hypothesis regarding several other kinds of movements as well. Studies rejecting the significance of mental traits drew their evidence mainly from surveys in the 1960s and 1970s on riots or collective violence. The political-opportunity language was applied to different types of movements in different places and times but, again, these did not constitute tests.

Finally, what kind of evidence was accepted as support? Any use of networks in recruitment was taken as evidence of the hypothesis. No serious evidence was thought to be needed to dismiss the mental-traits hypothesis. Almost everything was, at one time or another, interpreted as evidence for the political-opportunity hypothesis. None of the hypotheses had clear criteria as to what evidence would count for or against them.

The causal mechanisms cited in the three claims are all variables. Social networks are more or less present and used in recruitment; mental traits are sometimes more important and sometimes less; political opportunities, if defined crisply, may encourage protest in some cases and not in others. But at the height of the structural consensus 10 to 20 years ago, all were summarized as though they were proven to be necessary and/or sufficient conditions. Their importance was thus inflated. As a result, scholars were discouraged from theorizing about sources of variation, the conditions that make them more or less important. Perhaps, in a field based largely on case studies, scholars of social movements tend not to think in multivariate terms. But losing the probabilistic power of statistics may have weakened our insights rather than strengthened them.

It is possible that summaries of the literature tended to cite the original theoretical formulations for basic claims rather than subsequent empirical tests of them—yet they occasionally cite some of the latter as well. The evidence for the three claims was not as robust as partisans claimed—perhaps because most of the findings came from those same partisans (hardly a rigorous test). More recent work has tried to make opportunities into testable, measurable variables (Jenkins *et al.*, 2003; Meyer

and Minkoff, 2004). We have looked primarily at the early “tests” used to establish the three facts, and the rhetoric used to describe them. We do not think these claims will go the way of an older generation of hoary truths (e.g., that protestors are alienated, or working out Oedipal rebellions), but we hope, by understanding the rhetoric on which the facts were established, to encourage more accurate testing, new paths of theorizing, and theoretical modesty and skepticism. Skepticism is especially important.

Underpinning the eight rhetorical moves that we found are at least three psychological factors, one specific to a structural view and the others more common. First, the structural imagination suggests that there are unseen, indeed, unseeable, entities out there shaping actions. Considerable faith is invested in them, while skepticism is aimed at unseen mental traits inside individuals’ heads. In our examples, we saw several unnecessary structural terms used to lend an air of rigor to presentations. Faith in structures is at the core of sociology as a discipline, giving us a professional stake in structural images. For this reason we readily dismiss more individualistic and strategic models and variables (Jasper, 2006), and in doing so attribute undue causal influence to structures. We risk misspecifying our models in this way. And while this example happens to be structural, other views have their own precommitments that welcome some facts more readily than others: this is the well-known power of a paradigm.

Scholars working in the same area often have another set of blinders: they operate through constant self-congratulation. If enough scholars produce work under a perspective’s banner, then it must be successful. Rule (1997:198) calls this “safety in numbers.” A paradigm’s central claims may not have been well established, but the fact that people use its language is itself taken as a sign of its utility. At the extreme, even research that casts doubt on those central claims can be taken as evidence of the theory’s utility in stimulating research!

A related psychological factor that makes us all more willing to accept a claim as fact is pride in our fields of research (Rule, 1997:187). We want to believe we know something solid about social movements. Today’s scholars are especially proud that the field has advanced since the days of “armchair” theorists. We all want to work in rigorous and productive fields of inquiry.

This article has focused narrowly on rhetorical methods involved in presenting facts. There are lots of other strategic tools for promoting one’s ideas. Some of these are also rhetorical, such as promoting an idea because it represents a new or distinctive approach or uses a new language; presenting a fact or approach as politically or morally preferable to alternatives;

attacking an alternative fact or approach as outdated; using an idea or approach to align oneself with others; and using one's prestige to increase belief in a fact. Other tools for promoting an idea are strategic but not rhetorical, aimed at promulgating (or suppressing) an idea rather than persuading others it is true (or false). These include institutional gate-keeping activities, some of them meant to dampen the spread of alternative ideas and some to expedite the spread of one's own. Sitting on prize committees, reviewing manuscripts for journals or presses, editing book series, promoting young scholars with the right ideas, and evaluating grants for foundations or government agencies are all examples. Factors like these have more to do with positions and personal networks than with persuasion. But even at the heart of science, in the presentation of empirical generalizations and observations, we have seen considerable rhetoric at work.

We cannot avoid rhetorical techniques, but we can become better aware of how they affect what we take to be facts. Reassuring rhetoric can be especially effective when a field thinks it has emerged from preparadigmatic chaos to attain a certain level of consensus on questions and answers because it becomes less likely to test its knowledge claims carefully. Why test what we already know? To some extent this happened to the field of social movements in the 1990s, although the consensus soon cracked (Goodwin and Jasper, 2003). Alas, lack of controversy is a bad sign, not a good one. Debate and conflict are the best incentives to test theories. We should be wary when scholars begin to speak of the data as though they were free from any theoretical influence, and when scholars reject theoretical debate in favor of a deceptive empiricism (Lofland, 1993). Pretending to be crude empiricists only hides our theoretical commitments and rhetorical techniques. We all feel some stake in our theoretical perspectives and claims, making us stubborn about changing them in the face of contradictory evidence. But we must at least be frank about those commitments, and not hide them behind supposedly neutral data.

In the end, the best way to challenge theoretical commitments—and do creative research—is through the clash of several approaches. In creative communities, says Collins (1998:32), “great truths are not so much an advantage as an obstacle.” Any field, to be fruitful, “must be both making great discoveries and also overturning them, and not just once but over and again.” We must always question facts, no matter how persuasively they are packaged. All fields of sociology deploy rhetorical tools, but if we are unaware of them, we can allow greater gaps to develop between accepted truths and the empirical evidence for them. Several factors may make this more likely. One is the kind of evidence available and accepted. Social-movement scholars' reliance on case studies makes it harder to test claims, as the same case is used to develop theory and to provide

empirical evidence for it. Second, efforts to develop broad general theories (as opposed to conceptual vocabularies or middle-range theory) may demand or allow greater divergence from the evidence—although explicit theories should also encourage testing. Finally and foremost, a consensus about the accuracy of a theory (or paradigm), and the absence of competition among various theories, may encourage assumptions to masquerade as findings. Confidence kills inquiry.

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