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**BEYOND STRUCTURAL ANALYSIS:
TOWARD A MORE DYNAMIC UNDERSTANDING OF SOCIAL MOVEMENTS**

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It is common today to decry the “structural bias” in social movement research. For instance, in their critical survey of political process theory, Goodwin and Jasper (1999: xx) argue that “there has also been a strong structural bias in the way political opportunities are understood and in the selection of cases for study. Even those factors adduced to correct some of the problems of the political opportunity structure approach—such as ‘mobilizing structures’ and ‘cultural framing’—are subject to the same structural distortions.” As will become clear from this chapter, I have considerable sympathy with this general line of critique. But before offering a qualified endorsement of the cultural turn in social movement studies, I want to provide a brief sociology of knowledge account of how structure came to be privileged in the field *and* tout the very real scholarly gains that followed from this emphasis. For to say that contemporary movement theory is overly structural is to miss the essential contributions that 25 years of structurally oriented research have made to our understanding of non-routine, or contentious, politics.

From Psychology to Structure

My first exposure to the academic study of social movements came in 1971 when, much to my surprise, the professor in my Abnormal Psychology class devoted several weeks to a discussion of the topic. I say “surprise” because, as an active participant in the anti-war movement, it certainly came as news to me that my involvement in the struggle owed to a mix of personal pathology and social disorganization. But, reflecting the dominant theories of the day, those were the twin factors emphasized in the course.

At a macro, sociological level, social disorganization was seen as the immediate precipitant of movement emergence. Movements were held to arise when rapid social change (e.g. industrialization, urbanization) occasioned a generalized breakdown of social order (Kornhauser, 1959; Lang and Lang, 1961; Smelser, 1962; Turner and Killian, 1957). Movements, in this view, were groping, if ineffective, collective efforts to restore social order and the sense of normative certainty disrupted by change. As such they owed more to psychological, than political or economic, motivations.

But even in the face of generalized social disorder, everyone was not equally “at risk” of being drawn into movement activity. Supplementing the macro relationship between social disorganization and movement emergence were a host of micro “theories” arguing for a link between this or that set of individual and/or personality factors and movement participation (Adorno et al., 1950; Feuer, 1969; Hoffer, 1951; Klapp, 1969; Kornhauser, 1959; Rothman and Lichter, 1978). Though differing in their specifics, social marginality or isolation was a general theme in most of these accounts. So various social or psychological “deficits” were held to dispose individuals toward movement participation, even if social disorganization furnished the general impetus to collective action.

Both the macro emphasis on social disorganization and the micro stress on isolation and marginality accorded well with the then-dominant pluralist model of America politics (Dahl, 1961, 1967; Polsby, 1963; Truman, 1953). Pluralists viewed the U.S. as a broad, open, and at least minimally responsive political system, featuring bargaining and negotiation by a wide array of groups who shared relatively equally in power. The presence of social movements could be seen as inconsistent with the theory, *unless* those movements are seen, not as instrumental

political efforts, but as therapeutic vehicles through which needy people cope with the ill effects of social and personal disorganization. And so a tidy scholarly division of labor emerged: with pluralists explaining the workings of institutional politics and social movements, in Gamson's (1990: 133) felicitous phrase, left to "the social psychologist whose intellectual tools prepare him to better understand the irrational."

The turbulence of the 1960s simultaneously undermined scholarly faith in pluralism and made the apolitical view of social movements increasingly untenable. If American politics was less an open arena than an entrenched "power structure," then one hardly needed psychologists to understand the impulse to protest. Movements were simply politics by other means; rational efforts to generate leverage by groups (e.g. minorities, women) normally denied access to institutional channels. Working from these assumptions, a new generation of more structurally oriented movement analysts began to formulate very different theoretical accounts of movements *and* to produce consistent empirical results quite at odds with traditional expectations. This was true at both the group and individual level. While movements often did emerge against a backdrop of rapid change and generalized instability, it was rarely the most disorganized segments of society who were in the forefront of these struggles. On the contrary, analysts of contention began to amass impressive evidence attesting to the catalytic role of established groups or networks—"mobilizing structures" as they would come to be dubbed (McAdam, McCarthy, and Zald, 1996)—in the origin of movements. Studies of insurgency in nineteenth-century Europe showed that collective action tended to develop within stable neighborhood and work contexts (Aminzade, 1981; Margadant, 1979; Merriman, 1978; Tilly, Tilly, and Tilly, 1975). The civil rights movement was shown to have emerged within the central institutions of

the southern black community (McAdam, 1999; Morris, 1984). The anti-war movement developed on northern college campuses, with residential colleges experiencing higher rates of participation than commuter campuses (Orum, 1972). The two wings of the U.S. women's movement developed out of the State Commissions on the Status of Women (Freeman, 1973) and established networks of women who had been active in the southern civil rights movement (Evans, 1980). The list of studies could be multiplied many times over without altering the central point: structural stability, not disorder, appears to facilitate movement emergence.

At the micro level, similar findings began to proliferate. Though isolation and social marginality had long been assumed to be predictors of individual activism, numerous studies showed that people with structural ties to movements were far more likely to be drawn into the struggle than were their more isolated counterparts (Bolton, 1972; Briet, Klandermans, and Kroon, 1987; Broadbent, 1986; Diani, 1995; Fernandez and McAdam, 1988; Gould 1993, 1995; Klandermans and Oegema, 1987; McAdam, 1986; McAdam and Paulsen, 1993; Opp and Gern, 1993; Rosenthal et al., 1985; Snow, Zurcher, and Eklund-Olson, 1980; Walsh and Warland, 1983).

For all their narrowness, these exclusively structural studies have shaped the field in important and generally salutary ways. Let me highlight what I see as the two most important contributions to come from this structural research program. First, it had the effect of overturning the traditional psychological conception of social movements and reorienting the field to the study of organizations, networks, power, and politics. This has meant that political sociologists, political scientists, organizational scholars and network researchers have tended to dominate the study of social movements. And while this approach to the field comes with its

own set of blinders—some of which I hope to explore here—I am quite willing to betray my bias and say for the record that it is far more accurate and analytically useful to regard movements fundamentally as organized political phenomena rather than as spontaneous expressions of personal and social disorganization.

The second great contribution of the structural research program is that it has been, well, . . . a *research* program. That is, maybe even more important than the fundamental theoretical change noted above, was the *methodological* shift which accompanied it. While proponents of the older collective behavior school had primarily engaged in a form of armchair theorizing, the newer generation of movement scholars shared a general commitment to systematic empirical research. As much as anything, it was this commitment that stimulated the phenomenal growth of the field over the past two decades and established the consistent empirical findings touched on above.

These findings have been reproduced in so many discrete studies that they can now be regarded as empirical regularities in the emergent mobilization of a movement. The establishment of these “facts” underscores the very real contributions of the structural research program in the study of social movements. That said, the fact that we know very little about the dynamic *processes* and *mechanisms* that account for these regularities, points up the limits of the structural program and suggests that we may, at least for the study of movement origins, have reached the limits of the program’s usefulness.

Beyond Structural Variables: Searching for Explanatory *Mechanisms*

Motivated by the same general conclusions regarding the contributions and limits of the

structural research program, McAdam, Tarrow and Tilly (2001) call, in their recent book, for a move away from static structural models to a search for the dynamic *mechanisms* (and concatenated *processes*) that shape “contentious politics.” By mechanisms, the authors mean “a delimited class of events that alter relations among specified elements in identical or closely similar ways over a variety of situations” (2001: 11). I will offer some clarifying examples of such mechanisms later in the chapter. For now, I merely want to underscore the important methodological implication that follows from the approach urged by the authors. Rather than seeking to confirm the aforementioned structural regularities for yet another discrete movement, scholars should invest instead in methods designed to identify and better understand the interactive dynamics that account for the consistent structural findings. So, for example, if movements tend to develop within established social settings, what are the specific *mechanisms* that typically serve to transform a church, a college dorm, a neighborhood, etc., into a site of emergent collective action? Similarly, if certain network variables predict movement participation, what interactive dynamics help account for the relationship?

To begin to answer these questions, movement researchers will need to supplement the traditional macro and micro staples of movement analysis—case studies or event research in the case of the former and survey research in connection with the latter—with a more serious investment in ethnography and other methods designed to shed empirical light on the *meso-level* dynamics that shape and sustain collective action over time. I will have a bit more to say on the issue of methods at the close of the chapter. Here I am more interested in highlighting the important conceptual point implicit in this methodological injunction. I remain:

convinced that the real action in social movements takes place at some

level intermediate between the macro and micro. It is there in the existing associational groups or networks of the aggrieved community that the first groping steps toward collective action are taken Most of our research has missed this level of analysis. We have focused the lion's share of our research energies on the before and after of collective action. The "before" research has focused on the macro and micro [structural] factors that make movements and individual activism more likely. The "after" side of the research equation is composed of the few studies that focus on the outcomes of collective action. But we haven't devoted a lot of attention to the *ongoing accomplishment of collective action*. How do [structural] propensities get translated into specific mobilization attempts? What are the actual dynamics by which movement activists reach decisions regarding goals and tactics? How concretely do SMOs seek to recruit new members? To answer these question, what is needed is more systematic qualitative fieldwork into the dynamics of collective action at the intermediate meso level.

(McAdam, McCarthy and Zald, 1988; emphasis in original)

This plea to substitute qualitative fieldwork for more traditional quantitative methods of movement analysis, would appear to reverse what most researchers see as the conventional sequence of methodological strategies. That is, one often thinks of qualitative research as an exploratory strategy designed to yield formal hypotheses that can be tested using the techniques of quantitative research. A cursory review of methods textbooks confirms that, to the extent

that qualitative and quantitative techniques are linked at all, it is in the aforementioned sequence (Nachimas and Nachimas, 1976; **other cites**). But the two can be deployed in reverse order as well. Quantitative analysis can be used, as in the study of social movements, to uncover certain recurrent empirical relationships, that can then be interrogated more fully using systematic qualitative methods. It is this somewhat unconventional reversal of the relationship between qualitative and quantitative approaches that I am advocating here.

Illustrating the Program: Three Exemplars

I want to use the balance of the chapter to illustrate the approach being advocated. Drawing wherever possible on the other contributions to this volume, I want to take three established structural “facts” associated with the origins of contention and speculate a bit about the dynamic mechanisms that may account for the recurrence of these findings in a host of empirical studies of movement emergence. At present, simple structural explanations are typically advanced to account for all three facts.

- *Fact #1:* recruits to a movement tend to know others who are already involved.
Account: by providing prospective recruits with a mix of information and solidary incentives, prior social ties encourage entrance into the movement.
- *Fact # 2:* most social movements develop within established social settings.
Account: established social settings provide insurgents with the various resources (e.g. recognized leaders, communication channels, networks of trust, etc.) needed to launch and sustain collective action.
- *Fact # 3:* emerging movements tend to spread along established lines of interaction.

Account: information, rendered credible by prior contact with the innovator, mediates the spread of a movement.

In my view, these structural accounts are, at best, incomplete explanations of the facts, masking, in all cases, the play of far more interesting and contingent mechanisms, which combine structural and cultural elements. I take up each fact in turn.

Prior Social Ties as a Basis of Movement Recruitment - As Gould notes in his chapter for this volume, “one of the first and most frequently cited facts about social ties and activism is that activists are frequently drawn into a movement by people they know” (pp. xxx). But, as he goes on to say, “simply observing that social ties affect mobilization is not much of a contribution. It is a bit like noticing that people who are stricken with plague have had contact with other plague victims” (pp. xxx). To their credit, network analysts have not simply hypothesized these affects; as noted above and reviewed by Passy (p. xxx) in her chapter, these analysts have also produced a great deal of empirical work attesting to the relationship. “We are now aware that social ties are important for collective action, but we still need to theorize the actual role of networks” (Passy, p. xxx). Without specifying the mechanisms that account for the affect, movement researchers are guilty of assaying a structurally determinist explanation of movement recruitment. We are left with the unfortunate impression that individuals who are embedded in movement networks are virtually compelled to get involved by virtue of knowing others who are already active. There are a host of good reasons why we should reject this simple structural imperative, but here I will limit myself to three.

First, the above account skirts the important question of origins. That is, to say that

people join movements because they know others who are involved, ignores the obvious point that on the eve of a movement, there are no salient others already involved to pull ego into activism. Second, the structural account fails to acknowledge conceptually or address empirically the fact that potential recruits invariably possess a multitude of “prior social ties” that are likely to expose them to conflicting behavioral pressures. Here we confront the hoary problem of sampling on the dependent variable. Overwhelmingly, the studies of movement recruitment start by surveying activists after their entrance into the movement. But showing that these activists were linked to the movement by some prior social tie does not prove the causal potency of that tie. No doubt there are also many non-activists with ties to someone in the movement who did not participate, perhaps because salient others outside the movement put pressure on them to remain uninvolved.

The final key lacuna of the structural account of movement recruitment is the one mentioned above and which Gould and Passy take as the starting point for their exemplary chapters: proponents of the structural account have generally failed to sketch a distinctive model to explain the observed effects. The structuralists are not alone in this. For all the importance they attach to social construction and human agency, many culturalists advance an implicit view of the individual that is curiously determinant in its own right. Individuals act, not on the basis of structural/network influences, but the impact of disembodied culture. But in both cases, the effect is similar: the potential for individual autonomy and choice is largely denied, replaced by a conception of the individual as acted upon, rather than acting.

For their part, rational choice theorists have articulated a model (with multiple variants) of entrance into collective action. And while I think it implies a truncated view of individual

motivation and action, I nonetheless take seriously the need for such a model and for the identification of dynamic mechanisms and processes that bridge the micro, meso, and macro dimensions of movements. I do not pretend to deliver on a complete model of this sort here. But, in keeping with the efforts of Passy and especially Gould, I think we can move a ways in this direction. I begin by making a single foundational point: in my view a viable model of individual action must take account of the fundamentally social/relational nature of human existence. This is not to embrace the oversocialized conception of the individual that I see informing the work of most structuralists and some culturalists. Consistent with the rationalists, I too stress the potential for individual autonomy and choice. Where I part company with the rationalists is in the central importance I attach to one powerful motivator of human action. I think most individuals act routinely to safeguard and sustain the central sources of meaning and identity in their lives. As a practical matter, this means frequently prizing solidary incentives over all others and, in particular, conforming to the behavioral dictates of those whose approval and emotional sustenance are most central to our lives and salient identities.

How might this foundational tenet translate into a specific account of movement recruitment? In 1993, I co-authored an article with Ronnelle Paulsen in which we offered a sequential account of the onset of individual activism. We argued (p. 647) that:

the ultimate decision to participate in a movement would depend on four . . . [mechanisms]: (1) the occurrence of a specific recruiting attempt, (2) the successful linkage of movement and [salient] identity, (3) support for that linkage from persons who normally serve to sustain the identity in question, and (4) the absence of strong opposition

from others on whom other salient identities depend.

Echoing the central theme of this chapter, we then closed the article with the following plea (p. 663):

the most important implication of this research is as much social psychological as structural. Network analysts of movement recruitment have been overly concerned with assessing the structure of the subject's relationship to the movement without paying sufficient attention to the social psychological processes that mediate the link between structure and activism [P]rior ties would appear to encourage activism only when they (a) reinforce the potential recruit's strong identification with a particular identity and (b) help to establish a strong linkage between that identity and the movement in question. When these processes of *identity amplification* and *identity/movement linkage* take place, activism is likely to follow Movement analysts, then, need to be as attuned to the [cultural] *content* of network processes as to the *structures* themselves.

Though we did not use the term *mechanism* in the article, we clearly had the underlying concept in mind when we identified *identity amplification* and *identity/movement linkage* as key “processes” in movement recruitment. Translated more fully into the language of this chapter, we were attempting, in the earlier article, to sketch a dynamic recruitment *process* composed of four component *mechanisms*. Figure 1 presents the *process* schematically.

[figure 1 about here]

The process sketched above, and the empirical results reported in the 1993 article, accord nicely with the central themes and conclusions of the work by Passy and Gould in this volume. In closing this section, it is worth highlighting the various points of convergence between the three chapters. In her effort to better specify the structural account of recruitment, Passy ascribes three crucial *functions* to network ties. These she terms the *structural-connection*, *socialization*, and *decision-shaping functions*. These three functions bear a striking similarity to three of the four mechanisms identified in figure 1. Passy's *structural-connection* function is essentially the same as the *recruitment attempt* in figure 1. That is, prior ties can connect a potential activist through a concrete recruitment attempt. What Paulsen and I termed *identity-movement linkage*, Passy calls the *socialization function* of networks. Finally, Passy's *decision-shaping function* is more or less synonymous with the *positive influence* mechanism shown in figure 1. The important point for Paulsen and me was that although these various mechanisms could occur independently, the likelihood of successful recruitment was dramatically increased when they occurred sequentially through the efforts of a single, highly salient prior tie. So if, for example, a very close friend asked you to take part in a demonstration (*recruitment attempt*), and worked both to create a plausible link between the action and an identity s/he knew you prized (*identity-movement linkage*) and to argue for the correctness of the action in general terms (*positive influence attempt*), I think it is very likely that you would take part, especially if you encountered little or no opposition to the action from salient others.

This hypothetical example is very close to the one that Gould models toward the end of his chapter. The unique, and I think entirely justified, supposition that Gould builds into this

model is that a recruitment attempt by a prior tie works, not because of any threatened loss of sociability for non-participation (which is what rationalists argue) but because shared involvement comes to be viewed as a way of enhancing the existing relationship. But this, as Gould notes, will only work if the two individuals in question are very close friends. Indeed, the model predicts that the closer, more affectively salient the tie, the more likely the recruitment effort will succeed. This is entirely consistent with what Paulsen and I argue in the earlier article. To the extent that a single, highly *salient* (read: Gould's "strong") tie orchestrates the three mechanisms identified in figure 1, the successful recruitment of the target into activism is very likely.

Established Social Settings as the Locus of Movement Emergence - As noted previously, it has become part of the received wisdom of movement theory that episodes of contention almost always develop within established social settings (for a summary of this literature, see McCarthy, 1996). Besides the classic studies that helped establish the fact (Aminzade, 1981; Evans, 1980; Freeman, 1973; Margadant, 1979; McAdam, 1999; Morris, 1984; Tilly, Tilly, and Tilly, 1975), a host of more recent works have served to confirm it. In his work on the 1989 Chinese student movement, Zhao (1998, 2001) shows how the dense ecological concentration of college campuses in Beijing served as the locus of initial mobilization. Glenn (2001), among others, documents the role that a network of independent theater companies played in the origins of the Civic Forum movement in Czechoslovakia. In some earlier work, Osa (1987) highlights the central structural importance of the Catholic Church to the dissident movement in Poland. In her chapter for this volume, Osa (p. xxx) expands on the earlier work by showing how the

growth of opposition networks in Poland, predict both “movement emergence and . . . [the] peak periods of protest activity within a cycle of contention.”

Consistent with these studies, proponents of the political process model have long emphasized the role of established organizations or associational networks in the onset of contention. Absent any such “mobilizing structure,” incipient movements were thought to lack the capacity to act even if afforded the opportunity to do so. As straightforward and seemingly self-evident as this proposition would appear to be, it must be remembered that, when first voiced, it contradicted the emphasis on social breakdown and disorganization so central to the collective behavior tradition. Since this particular debate was joined, it seems clear that the weight of empirical evidence favors the organizational camp over the breakdown school. All well and good. But, as with the notion of “prior tie,” the “mobilizing structure” concept has too often been treated as an objective structural facilitator of protest, rather than a contested site of interaction that can give rise to opposite lines of action. The point is, existing groups or networks (as well as prior ties) are as apt to constrain as facilitate protest. Bottom line: it is not prior ties or group structures that enable protest, but rather the interactive conversations that occur there and succeed in creating shared meanings and identities that legitimate emergent collective action.

The point can be made more concretely by revisiting one of the classic cases that helped confirm the critical importance of established groups in the origins of contention. Movement scholars have thoroughly documented the central role played by the black church in helping to launch the civil rights movement (McAdam, 1999; Morris, 1984; Oberschall, 1973). But while the movement’s debt to the black church is widely acknowledged, the standard narrative account

of the origins of the civil rights struggle obscures an organizational and *cultural* accomplishment of enormous importance. Until the rise of the movement, it was common for social observers—black no less than white—to depict the black church as a generally conservative institution with a decided emphasis, not on the “social gospel in action,” but rather on the realization of rewards in the afterlife (Johnson, 1941; Marx, 1971; Mays and Nicholson, 1969). Nor did the inherent conservatism of the institution entirely disappear as a result of the civil rights movement. As Charles Payne’s (1996) magnificent book on the movement in Mississippi makes clear, the conservative nature of local black clergy remained an obstacle to local organizing even during the movement’s heyday.

Given this more complicated portrait of the black church, the importance and highly contingent nature of initial mobilization attempts should be clear. To turn even some black congregations into vehicles of collective protest, early movement leaders had to engage in a lot of creative cultural/organizational work, by which the aims of the church and its animating collective identity were redefined to accord with the goals of the emerging struggle. This is a collaborative *cultural* project that has far more to do with social construction and redefinition than with the kind of objective inventory of organizational resources suggested by the current *structural* account of movement origins. Prior organization and all the resources in the world matter little if their use is not governed by shared meanings and identities legitimating contention.

This discussion brings us, inevitably, back to the limits of the structural research project. By starting from the accomplished fact of collective action, and then working back in time to note that movements tend to arise in established social settings, structural analysts exaggerated

the link between organization and action. By, once again, selecting on the dependent variable—emergent collective action—they could detect the exceptional cases where existing groups birthed movements, but not the far more numerous examples of such groups constraining action. Moreover their methods—principally case study and event research—were essentially those of the outsider, making it impossible for them to observe the interactive dynamics that shaped the exceptional cases. The “insider” approach embodied in qualitative fieldwork would redress this problem.

But what concrete social mechanisms should movement ethnographers attend to in such settings? Even if one embraces the rigorously inductive ideal of “grounded theory” (Glaser and Strauss, 1967), it is probably impossible (and undesirable in my view) to enter the field without some sensitizing hunches concerning what one is going to find. Adapted from previous work (McAdam, 1999; McAdam, Tarrow, and Tilly, 2001), figure 2 represents a provisional attempt to do that for the *process* by which an existing social group *mobilizes* for contentious action. I see three *mechanisms* as key to this process.

[figure 2 about here]

1. Attribution of Threat or Opportunity - The routine monitoring and interpretation of events and environmental conditions is the foundation for all group life, routine as well as contentious. Accordingly, for me, the initial catalyst for insurgent mobilization comes in the form of an emergent group-level account of some new threat to, or opportunity for, the realization of group interests. That said, for all their importance, these crucial interpretive dynamics are largely absent from the traditional structural account of movement emergence. As Goodwin and Jasper (1999) note, rather than emphasize the culturally constructed nature of perceived opportunities

(or threats), movement theorists have characterized the environmental impetus to action as owing to the objective features of a particular “political opportunity *structure*.” But, rather than look upon “opportunities and threats” as objective structural factors, we see them as subject to attribution [and emergent construction]. No opportunity, however objectively open, will invite mobilization unless it is (a) visible to potential challengers and (b) perceived to be an opportunity. The same holds true for threats, an underemphasized corollary of the model Attribution of opportunity or threat is an activating mechanism responsible in part for the mobilization of previously inert populations (McAdam, Tarrow, Tilly, 2001: xxx).

2. Social Appropriation - An emerging group-level account of threat or opportunity is hardly sufficient to ensure a movement, however. For collective attributions of threat or opportunity to key emergent action the interpreters must command sufficient resources and numbers to provide a social/organizational base for mobilization. When this is the case, the ideational challenge gets inherent in fashioning an account of threat/opportunity gets joined to a more narrowly organizational one. As a prerequisite for action, would-be insurgents must either create an organizational vehicle and its supporting collective identity or, more likely, *appropriate* an existing organization and the routine collective identity on which it rests. In short, the collective account of threat or opportunity must become the animating frame for an organizationally able collective.

In my view, Gould’s two person model of recruitment has implications for collective mobilization as well. Basically, he is describing a process of interpersonal appropriation in the

service of a movement. That is, the recruiter is seeking to appropriate the solidaristic bonds of trust and affect s/he shares with the friend on behalf of the movement. But there is a group level analog to this dyadic process. In a close-knit group situation, an established (or emergent) leader or organizer can also call on the existing loyalties of group members in an effort to leverage action. Indeed, in his chapter on local environmental movements in Japan, Broadbent offers a clear example of this kind of leader based appropriation and its crucial importance to the broader process of mobilization. Broadbent (p. xxx) explains:

In Japan's vertical society, overt resistance was a daring step. It contravened the norm of going along. Moreover, though violent repression was rare, resistance had possible harmful consequences, both materially and in terms of social prestige. In a society of "other-directed" personalities . . . high status members controlled a great deal of collective motivational affect. As noted earlier, bosses running the local political "machine" worked through kin and neighborhood networks. . . . They solidified their power in part by assiduously currying personal favor—presenting generous contributions at funerals and weddings, holding sake parties to build camaraderie, distributing small bribes at election time, finding jobs and even marriage partners for your children. . . . [O]ther services flowed through personal networks as well. As a result, either requested by the boss or just out of concern for their loss of services, locals would personally urge their activist relatives to

desist from movement participation. This network pressure posed a formidable barrier to mobilization in village context. It makes sense then that one crucial theme for successful movements should be the “breakaway boss.” The three fully successful communities, plus one failed community, exhibited this quality. A breakaway boss broke free from his status within the conservative social control machine to “lead his flock” into activist resistance.

Or, to phrase it in my terms, drawing on the considerable moral and interpersonal capital each had mobilized during his tenure as boss, these leaders were able to successfully *appropriate* the structures they commanded on behalf of a local movement. In stressing the importance of local leaders in the mobilization of local activism, Broadbent suggests that Japan may be distinctive in this regard. In such a traditional, “other regarding,” society, leaders may play more of a role in the appropriation and mobilization of local social structures than in western countries. Perhaps, but it is worth noting that in his chapter on the rise of the Nazi movement in Germany, Helmut Anheier, offers fascinating empirical evidence of much the same leader-based appropriation process described by Broadbent. Even if further comparative study of movement origins confirms Broadbent’s surmise, the role of local leaders in the appropriation of existing social groups or networks is very likely remain a key mobilizing mechanism across a wide variety of contentious episodes.

3. Innovative Action - The final component mechanism of the mobilization process is

innovative contentious action, defined simply as action that, by its contentious nature, departs from previous collective routines and signals to other parties a fundamental change in the action orientation of, and relationship to, the group in question. Such action is extremely likely to develop when shared perceptions of threat or opportunity come, through appropriation, to serve as the motivating frame of an established group. But it is important to underscore that such action is a contingent accomplishment in its own right. Even should a group view its situation as newly threatening or opportune, it may refrain from innovative action for strategic reasons. For that reason, innovative action is identified as a third contingent mechanism shaping mobilization.

The Spread of a Movement Along Existing Lines of Interaction - The vast majority of contentious episodes never spread beyond the local settings in which they first develop. But in the case of major movements, at least some degree of *scale shift* takes place (McAdam, Tarrow, Tilly: chapter 10). By the *process* of *scale shift*, we mean “a change in the number and level of coordinated contentious action leading to broader contention involving a wider range of actors and bridging their claims and identities” (McAdam, Tarrow, and Tilly, 2001: xxx). The process of scale shift, or movement spread, has not received the same level of attention as either *emergent mobilization* or *movement recruitment*. But, as with these other two processes, the work that has been done on the spread of contention tends, once more, to reproduce the structural bias inherent in the field (Jackson et al., 1960; McAdam, 1999; McAdam and Rucht, 1993; Pinard, 1971; Soule, 1997; Strang and Meyer, 1993). The general tendency has been to interpret the spread of contention on the basis of traditional diffusion theory, which holds that innovations or new cultural items diffuse along established lines of social interaction (Rogers, 1983). But as

Oliver and Meyer make abundantly clear in their contribution to this volume, the spread of contention is a very complicated phenomenon that almost certainly does not conform to a single unvarying pattern.

The effort to model the spread of contention as but a specialized instance of diffusion once again truncates our understanding of the phenomenon in question. To say that most such instances benefit from prior ties between innovators and adopters tells us no more about the contingent dynamics of scale shift than the previous two structural “facts” tell us about the processes of recruitment and mobilization respectively. It is reasonable to assume that many, if not most, instances of strictly local contention involve groups whose members are also linked to others beyond their local context. So why do so many cases of local contention fail to spread elsewhere? As with *mobilization* and *recruitment*, certain structural conditions may be necessary, but hardly sufficient, to insure the process in question. As before, the question becomes: what contingent social-cultural mechanisms mediate movement spread? This is essentially the same question that Oliver and Meyer take up in their chapter. Their innovative approach involves “struggling with what empirical data patterns actually look like, and trying to model the underlying processes that could be giving rise to these patterns” (p. xxx). I take a different tack, identifying, as before, a set of linked mechanisms that would seem to condition the likelihood of scale shift. I see scale shift as a robust process consisting of two distinct pathways, though the two can, and frequently do, co-occur in a given movement. This process is shown in figure 3.

[figure 3 about here]

Before taking up each mechanism in turn, let me first describe the process in general

terms. Localized collective action spawns broader contention when information concerning the initial action reaches a geographically or institutionally distant group (through either *diffusion* or *brokerage*) which, on the basis of this information, defines itself as sufficiently similar to the initial insurgents (*attribution of similarity*) as to motivate *emulation*, leading ultimately to *coordinated action* between the two sites.

I am using the term diffusion here in a somewhat specialized way to refer only to the transfer of information along established lines of interaction, while brokerage entails information transfers that depend on the linking of two or more previously unconnected social sites. (In his interesting empirical contribution to the volume, Diani explores some of the empirical correlates of brokerage in the Milanese environmental movement.) I make this distinction to call attention to a significant difference in the nature and likely impact of scale shift depending on whether diffusion or brokerage tends to predominate as the mediating mechanism. Movements that spread primarily through diffusion will almost always remain narrower in their geographic and/or institutional locus than contention that spreads through brokerage. Why? Because such movements will not be able to transcend the typically segmented lines of interaction which characterize social/political life.

While diffusion and brokerage represent different pathways to scale-shift, both work through the two additional mechanisms shown in figure 3. The first of these, *attribution of similarity*, I define as the identification of actors in different sites as being sufficiently similar as to justify common action. This mechanism is one that some scholars of diffusion have stressed as mediating between information and adoptive action (Strang and Meyer, 1993; McAdam and Rucht, 1993). The idea is simple enough. Information alone will not lead someone to adopt a

new idea, cultural object, or behavioral practice. Adoption, in turn, depends on at least minimal identification between innovator and adopter.

What factors make such identification more likely? It results, first, from the deliberate attempts of agents of diffusion/brokerage to frame the claims and identities of influence targets as sufficiently similar to their own as to justify coordinated action. We see such deliberate influence attempts all the time in contentious politics: the brokering of a clerical-monarchist-regionalist coalition against Paris in the Vendée revolt in France (Tilly, 1964); the appropriation of existing ministerial networks to forge a regional alliance of movement leaders in the early days of the civil rights struggle (McAdam, 1999); the Free Men-Free Soil-Fremont electoral campaign of 1856 by the Republicans in antebellum America (McAdam, Tarrow, Tilly, 2001). Movement entrepreneurs who wish to increase their appeal to either previously connected or disparate groups work constantly to draw parallels between the group they represent and the targets of their influence attempts. Indeed, Snow and Benford (1988, 1992) have termed this process “frame bridging,” and highlighted its importance in the unfolding of a protest cycle.

However, attribution of similarity need not be so purposive or strategic a process. A second factor encouraging identification among different actors is Strang and Meyer’s (1993) concept of “institutional equivalence.” The authors highlight the tendency of policymakers within particular institutional domains (e.g. urban planning) to identify with their counterparts in other countries, thus facilitating the spread of policy innovations, in the absence of purposive influence attempts by the innovators. In the history of contentious politics we see institutional equivalence encouraging scale shift in the channeling effect of mass production on industrial action; workers in mass production units with similar relations to management have historically

found it much easier to join their struggles to others in similar situations than, say, to handicraft workers in isolated workshops.

The second and final mechanism mediating scale shift is *emulation*, defined here simply as collective action modeled on the actions of others. While straightforward as a mechanism, its inclusion in figure 3 underscores a point made earlier in connection with the discussion of emergent mobilization. Awareness of a prior action, even when accompanied by strong identification with the actor, does not necessarily guarantee emulative action on the part of the observing group. We can well imagine groups learning of and strongly identifying with a contentious action by another group and yet refraining from action out of fear or a sensible desire to monitor the reaction of authorities before deciding whether to act themselves. The point is, emulative action is a contingent outcome in its own right and therefore properly modeled as a mechanism distinct from diffusion/brokerage and attribution of similarity.

Although diffusion and brokerage often combine in major movements, there are significant differences in the character and likely impact of scale shift depending on which of the two predominates as the mediating mechanism. Contention that spreads primarily through diffusion may be dramatic and consequential in its effects, but because it never transcends existing lines of interaction, it will almost always remain narrower in its reach and impact than contention that spreads substantially through brokerage.

By the same line of reasoning, diffusion is far more likely to be the mediating mechanism of movement spread than brokerage: more likely because actors who are connected through lines of interaction are already more likely to attribute similarity to themselves; and also because diffusion requires a much lower investment in time and entrepreneurial energy than brokerage. It

follows that, brokerage, though less common than diffusion, is likely to be far more consequential in its effects. To the extent that brokered ties succeed in encouraging previously disconnected groups to identify with one another, contention can quickly spread beyond narrow geographic, institutional, and/or categorical boundaries and produce new identities that are more durable than the incidents that gave rise to them. To take but a single well know case of this, by brokering a set of ties between the southern civil rights movement and northern white college campuses, the 1964 Freedom Summer project set in motion a significant “revolution beyond race” by encouraging many seemingly disparate groups (e.g. white college students, women, chicanos, gays and lesbians, etc.) to identify with and draw action implications from African-Americans (McAdam, 1988, 1995).

Conclusion

The structural research program on social movements has produced several consistent empirical findings. However, the meaning of these findings and, more importantly, the actual social processes that account for them remain opaque. What, for example, does it mean to say that movements develop within established social settings when most such settings appear to constrain rather than facilitate mobilization? Not much, unless we can identify the intervening *mechanisms* that condition these divergent outcomes. That is what I have tried to do here for for three important *processes*—*individual recruitment*, *emergent mobilization*, and *scale shift*—that figure prominently in the movement literature. In doing so, it should be clear that I am not so much abandoning the structural approach to the study of contention as seeking to supplement it with the insights and methods from more culturally (and rationally) oriented movement scholars.

After all, networks are centrally implicated in all three of the processes discussed above. But instead of focusing on the formal network component of each, I have chosen instead to speculate a bit about the interactive dynamics that may animate these network based processes.

Needless to say, these three processes hardly exhaust the movement dynamics one might be interested in mapping in the more dynamic, mechanistic terms discussed here. Indeed, the chapter that perhaps most fully realizes the programmatic aims I have tried to articulate, is Anne Mische's work on "Cross-Talk in Movements." The process of interest for Mische is none of the three discussed here, but rather the neglected phenomenon of *movement coalition formation and maintenance*. Given that most successful movements necessarily involve some degree of coalition formation, Mische's focus is germane to a host of critical movement issues, including the all important topic of movement outcomes. To account for variation in her process, Mische identifies and discusses in great detail, a host of promising *conversational mechanisms* that would seem to have important implications for a number of other movement processes, including those touched on in this chapter.

In bringing her impressive chapter to a close, Mische takes up an issue that is highly germane to the realization of the ambitious research program called for here. I refer to the all important issue of systematic method. As Mische (p. xxx) puts it:

Another important challenge is the question of *measurement*.

While the research proposed here stresses contingency and context, it still makes a claim to move beyond thick description to find pattern in complexity in the search for . . . generalizable

mechanisms. To that end, ethnographic or textual analysis must often be complemented by data reduction techniques of various kinds.

While space constraints prevent a full exploration of methodological possibilities, I want, at the very least, to heartily second Mische's call for a more ambitious and strategic mix of qualitative and quantitative research strategies in the study of social movements and contention. And what might some of these approaches be? Besides the obvious need for thick ethnographic descriptions of movement processes, a range of other options are open to us, if we are willing to set aside the narrow methodological divisions that currently characterize the social sciences. Modeling of either the formal sort employed by Gould in his article, or the more inductive, data driven approach used to good effect by Oliver and Meyer, can be valuable tools in this effort. So central is conversation to the cultural mechanisms discussed here, that we would be remiss to ignore the methodological riches inherent in the various forms of conversational and discourse analysis. Finally, the experimental method could be harnessed to this effort as well. Besides small group studies in the lab, we could also take a page from Gamson's seemingly endless book of methodological tricks and try to simulate contention through naturalistic experiments of the sort that he and his colleagues devised in *Encounters with Unjust Authority* (1982).

These few suggestions hardly exhaust the topic, nor do they begin to convey the daunting methodological challenges that await anyone who would seek to take up the research program advocated here. Challenges aside, what I feel confident about is that, when joined with the theoretical pluralism embraced here, this more catholic approach to research methods promises to move us well beyond the essentially static and descriptive structural facts that currently pass

for theory in the study of social movements and contention. This volume is dedicated to that end.

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