

2. We examined the following oral histories: Raines 1977 and the Youth of the Rural Organizing and Cultural Center [YROCC] 1991 for the U.S. civil rights movement, and Lindner and Grueneberger 1992, Neues Forum Leipzig 1990, and Philipsen 1993 for the East German movement. In all, we examined approximately 102,600 words of text for the U.S. case and 111,900 words of text for the East German case. Nari Choi, Jordon Peugh, and Michael P. Young assisted us in this research.

3. The primary limitation of the data utilized in this chapter is undoubtedly their silence concerning those who did *not* participate in the two movements that we are examining. The oral histories that we have examined, for example, do not include interviews with nonparticipants; in sociologese, our data sample or select on the dependent variable. However, this problem is mitigated to a large extent by the fact that we are less interested in why specific people joined these movements than in why participants engaged in, and persisted in engaging in, high-risk activities.

4. Similar patterns of emotional ties and community building can be seen in movements outside of the GDR's (and the Southern civil rights movement's) authoritarian context. Examples include the nonviolent direct action groups within the peace and ecology movements of the United States during the 1970s and 1980s which Epstein (1991) has studied.

Conclusion

Second that Emotion?: Lessons from Once-Novel Concepts in Social Movement Research

Francesca Polletta and Edwin Amenta

Applying new concepts to old areas in sociology carries with it enormous potential. New concepts may complement existing theories by filling gaps in their claims, or, more daringly, they may provide different and better explanations for processes explained by previous theories. By highlighting previously unaddressed issues, they may suggest new questions and open up new lines of research.

The social movement literature provides some well-known examples. When sociologists began to see organizations in collective action rather than just atomized individuals and social contagion, they not only refuted claims that protest was irrational and protesters social isolates but also drew on organization theory to generate new hypotheses about movements' emergence and trajectories. When students of Western European mobilizations around nuclear energy, local autonomy, and homosexuality argued that these "new social movements" differed from earlier movements in seeking recognition for new identities rather than political and economic concessions, they prompted wide attention to the role of identity in all movements—new and old.

New approaches bring dangers with them as well, however. Scholars may define a new concept so broadly or vaguely as to make it difficult to use in constructing plausible theoretical arguments. They may highlight a neglected dimension of social movements without specifying what outcome or process it is meant to explain, or its contribution to ex-

plaining protest may be a trivial one. An intriguing concept may be impossible to operationalize and arguments based on it therefore impossible to substantiate.

The social movement literature provides instances of the downside of innovation, too. When they were new, ideas such as resources, collective identity, frames, free spaces, movement impacts, poor people's movements, and political opportunities brought with them analytical problems, not all of which have been resolved. For instance, scholars still employ concepts such as "free spaces" and "identity" in vastly different ways (Polletta 1998; Polletta and Jasper 2001). Scholars also struggle to define political opportunities in ways that are not overly broad, and to connect specific opportunities to specific movement outcomes and processes (McAdam 1996). "Resources" have been expanded in some accounts to include anything that makes protest possible—producing an argument that is difficult either to refute or establish (Steinberg 1998). Movements undoubtedly have "cultural impacts" (Amenta and Young 1999), but these have not been well conceptualized, much less assessed and explained.

The challenge is to demonstrate, rather than just state, that the new concept matters, and to show when and where it matters. How well has recent work on emotions in social movements responded to that challenge? The question can be divided into four parts: (1) Have scholars made arguments based on emotions that convincingly fill gaps in existing models, for example, by supplying mediating mechanisms or extending the model to previously unaddressed processes? (2) Have scholars employing emotions advanced coherent and empirically appraisable alternative explanations for aspects of movements' emergence, trajectory, or fates? (3) Have these arguments been substantiated in research? (4) Have scholars identified wholly new questions that we need to answer in order to understand social movements? We give an emphatic yes to the first question, but increasingly more qualified assessments as we move down the list. In exploring these answers we concentrate on the tasks left undone and the problems confronting those who would do them, drawing on the experiences of other once-novel concepts in social movement research. These concepts help us identify parallel problems and ways around them.

Filling In and Going Beyond Political Process Explanations

Many of the essays collected here can be read as supplements and correctives to the dominant political process model. Their authors aim to dislodge narrow constructions of the model's key explanatory vari-

ables—political opportunities, mobilizing structures, and collective action frames. They do so by opening up black boxes in causal chains, identifying new mediating mechanisms, and extending the models to previously uncharted terrain.

Consider political process theorists' argument that people mobilize when political realignments make it more likely that extra-institutional protest will have substantial impact. People have to believe that the opportunities are there; thus the importance of what McAdam (1982) calls "cognitive liberation" in which people come to believe that insurgency will be effective. The emotions come from optimism and confidence: people mobilize "on the basis of some optimistic assessment of the prospects for successful insurgency weighted against the risks involved in each action" (1982: 34). Closer attention to the emotions that actually precede and accompany protest, however, suggests something different. First, people are often motivated by anger, indignation, fear, compassion, or a sense of obligation, not optimism about the possibilities of securing political concessions through extra-institutional protest. And second, even when protesters' main feeling is one of optimism, it is based on more complex perceptions and assessments than a calculation of the objective probabilities of substantial political impact.

Both points are illustrated in black southerners' experience of the landmark *Brown v. Board of Education* ruling. As the editors note in the introduction, the 1954 decision, while a tremendous victory for the movement, brought increased violence and intimidation against blacks by southern whites. As Adam Fairclough (1986) points out in his account of the 1955 Montgomery bus boycott, it was blacks' anger at the white racism unleashed by *Brown* that led them to mount a full-scale assault on segregated busing rather than the more moderate compromise arrangement they had originally sought. This decision may have motivated protest more through white backlash and black indignation than through its demonstration of a receptive political system.

Similarly, as Elisabeth Jean Wood shows in her chapter, Salvadoran peasants mobilized not because they saw new potential for undermining a repressive regime, nor because they hoped to gain material benefits like access to land or freedom from control by state forces. These were available to residents in contested areas whether they participated or not. Rather, they sought dignity. They protested to reclaim the sense of agency that had been denied them in years of political and economic marginalization. Like others in the volume, Wood demonstrates the inadequacy of viewing the decision to protest as a cool assessment of costs and benefits and the probability of success.

Several other authors in this volume provide analyses that may refine or extend political process accounts. Sharon Nepstad and Chris Smith show that the prior networks theorists have identified as crucial to mobilization supply not only information but sympathy, trust, and emotional identification. When American churchgoers heard about U.S. actions in Central America, their prior contacts with Central American refugees and clergy made that information trustworthy—and personally galling. Nancy Whittier responds to a major gap in recent theorizing on movement “framing” by identifying some of the factors that shape and constrain activists’ framing of injustice and its victims. The legal changes that opened up new opportunities for adult survivors of sexual abuse to press claims also structured the kinds of emotional displays considered appropriate in those settings.

The next step for scholars of emotions is to specify just what kinds of changes in existing models are compelled by these emotion-rich analyses. For example, if we recognize that indignation and anger often accompany protest, do we then want to argue that these emotions can generate a social movement even in the absence of what scholars have called political opportunities? Or is the indignation sharpened, experienced by more people, or responded to differently in the context of political opportunities? How far do analysts want to go in revising the political process model and the assumptions on which it rests? If activists talk more about dignity than about material interests, does this mean that they are not motivated to protest by the latter? In what kinds of movements is this likely to be the case? The analytical challenge in these cases is to specify the relation between newly recognized emotional processes and existing explanations for mobilization.

Other writers in the volume are more explicit in *challenging* key tenets of the political process model. They take up the model’s building blocks—opportunities, mobilizing structures, and frames—not to refine but to rebut the model. Movements are dependent neither on political opportunities nor on prior networks, they argue. James Jasper (1997: 106) argues notably that “moral shocks” can raise “such a sense of outrage in a person that she becomes inclined toward political action, with or without the network of personal contacts emphasized in mobilization and process theories.”

Deborah Gould challenges political opportunity arguments by way of this concept. She argues that militant AIDS activism emerged at a time when lesbians, gay men, and AIDS advocates lacked access to political power and influential allies, benefited from no splits in ruling alignments, and confronted increasingly repressive legislation. Yet a 1986 Supreme

Court ruling upholding a Georgia anti-sodomy statute nevertheless galvanized militant protest around the country. *Bowers v. Hardwick* confronted gays and lesbians with the extent of their marginality and in doing so crystallized their anger at the government’s inaction on AIDS. When experienced as a “declaration of war,” what might be viewed as a constriction of political opportunities can produce the kind of rage that compels protest.

In another challenge to political process theories, several authors point out that people may develop such a strong emotional identification with a cause that they join organizations in which they don’t know anyone. They are motivated neither by selective incentives nor by prior obligations to co-workers, congregation, or community. Instead, it may be a religious sensibility—a temperament—that commands participation on behalf of people they have read about, as Michael Young shows in his account of Protestant evangelicals’ embrace of a militant abolitionism.

These challenges to the dominant political process account are provocative and sometimes persuasive. They would be strengthened, however, by better specifying the conditions in which particular emotional dynamics are likely to occur. We can take a cue from one of the missteps of earlier theories. As critics have observed (e.g., Amenta forthcoming), political process analyses have had a post-hoc quality—calling “opportunity” any political development that preceded mobilization. The concept of “moral shock” is one of the most intriguing in emotions theorists’ repertoire, but it is susceptible to the same overextension. Virtually any event or new piece of information can be called in retrospect a moral shock. So we need to ask what it is about certain events that create such anger, outrage, and indignation in those exposed to them that they are driven to protest. Are some *kinds of issues* more likely to generate moral shocks than others? Do moral shocks have any relation to what are usually counted as political opportunities?

To continue with the example of 1980s AIDS activism, Gould points out that the *Bowers v. Hardwick* decision was by no means the first indication of the government’s indifference to the concerns of gay, lesbian, and AIDS activists. Yet previous government action and inaction had been met with stoicism, prayer vigils, community service, and quiet lobbying. This was a function, Gould argues, of lesbians’ and gay men’s continuing ambivalence about their homosexuality—they were proud but also ashamed. Why then did *Bowers v. Hardwick* have such an effect? Gould’s account suggests several different possibilities. One is that it simply came after a series of governmental betrayals that were already shifting the emotion culture of gay and lesbian activists from acceptance

to rage. So the moral shock was not such a shock after all but simply confirmation of what had been felt, if inchoately, before. Would militant AIDS activism then have emerged even without *Bowers v. Hardwick*, if probably somewhat later? An alternative explanation for *Bowers v. Hardwick*'s impact lies in the decision itself, and perhaps in the emotionality that surrounds judgments about rights in the United States. That explanation suggests we look to Supreme Court decisions, or perhaps high-profile judicial decisions generally, to find likely moral shocks. A third possibility is that the importance of emotion work historically in gay and lesbian movements—of changing how people feel about themselves and others—predisposed activists to turn emotional affronts into collective action, and made them skilled in doing so. Emotional affronts may galvanize action when there is a history of activism that emphasizes emotional transformation.

Together, these alternatives suggest that collective action motivated by indignation rather than political opportunities may be more likely in some political contexts than others (after a series of governmental affronts), in response to certain kinds of governmental actions (rights rulings), and for certain movements (those that emphasize emotion work). Specifying *when* moral shocks are likely to occur seems crucial. Otherwise, we risk the circularity characteristic of some political process arguments: anything that preceded protest was a political opportunity—or, here, a moral shock.

We can ask the same kinds of questions with respect to other emotional processes. Is the ambivalence that Gould describes characteristic of all groups whose difference is treated as deviance? If the changed religious beliefs of evangelical Christians motivated their activism against slavery, as Young argues, then under what conditions more broadly are religious beliefs likely to be transformed into political action? Among whom are such beliefs likely to lead to political action? Under what circumstances are people likely to battle for their dignity, as did the campesinos Wood studied, rather than for material benefits?

Note that these conditions are likely to be cultural as much as structural, to include distinctive ways of seeing the world as much as formal rules and resources. To understand why “dignity” or “interest” dominates participants’ self-accounts, we need to know much more about the broader emotion cultures within which protest unfolds. Several writers in this volume identify the “rules” for feeling and expressing emotions (Hochschild 1979) that operate within a movement. But emotion cultures extend beyond particular movements, and they encompass more than dominant feeling rules. They include also “social epistemologies of emo-

tion” (Thoits 1989): beliefs about who is likely to experience what kinds of emotions, how emotions affect behavior, how emotions are repressed or expressed or transformed, beliefs about the dynamics of emotional “contagion,” about the effects of emotional “repression” or “denial,” about the relationship between affect and biology, etc. In social movements, they include the assumptions that activists as well as nonactivists make about what particular emotions signify, how emotions should be interpreted and expressed, and how they should be responded to.

Analyzing such cultures can help to develop more accurate models of individual and group choice than those currently dominant in social movement research. Simply revealing the variety of individual motivations that accompany protest has allowed emotions researchers to expose the inadequacy of cost-benefit-calculating rational actor models. To go beyond simply describing the varied motivations evident in protest, however, and instead begin to account for them, emotions researchers should try to explain why a particular “vocabulary of motives” (Mills 1940) comes to prevail within a historical period. Why did “rage” become a legitimate rationale for protest in the 1960s? Thomas Haskell (1985) argues that the rise of capitalism made “empathy” into a strong and common emotion (see also C. Taylor [1989] on the rise of benevolence). Can we trace the predominance of other emotions at different times to structural factors such as transformations in the state, capital, or other institutions?

Theorizing the consequences of emotion cultures is also a way to counter the instrumentalist bias of reigning models, not only in accounting for people’s decisions to participate, but in explaining movements’ trajectories. Activists’ understandings of efficacy and appropriateness, their views of what is feasible and what is rational, are all shaped by the cultures they inhabit. So, for example, the animal rights activists interviewed by Julian Groves often used the term “emotional” to criticize activists they considered unprofessional, irrational, or feminine—that is, if they were women. Men’s expressions of emotion, especially anger, were considered legitimate. The female activists who made up the bulk of the movement believed that it was necessary to substantiate their feelings about animal cruelty with scientific arguments and the support of men. Their insights were probably right, but they nevertheless reveal activists’ normative assumptions about gender, feeling, and rationality.

The authors in this volume argue persuasively that movement tasks ranging from recruiting members and sustaining their participation (Jeff Goodwin and Stephen Pfaff) to securing public support and winning legal battles (Nancy Whittier) depend on activists’ capacity to elicit, manage,

and transform people's emotions. But leaving it at that, and viewing emotions only as the raw materials for activists' mobilization efforts, risks the same instrumentalist bias that has limited political process accounts. To understand why activists choose the strategies, tactics, targets, and organizational forms they do, we need to understand that emotions configure their very criteria of instrumental rationality.

In sum, theorists of emotions might simultaneously be more cautious and bolder in their claims. They should demonstrate how their theories improve on existing models by better specifying the conditions in which the alternative processes they describe are likely to operate. But they should also be bolder in theorizing about the role of emotions in shaping activists' assumptions about strategy, rationality, and interest.

Methodological Issues in Assessing Emotions and Supporting Claims about the Influence of Emotions on Social Movements

Whenever scholars make theoretical claims that complement or replace political process arguments, methodological strategies are key to supporting these claims. Some of the methodological challenges faced by scholars of movement emotions are similar to those confronting most movement scholars. These are standard problems, and there are well known ways to get around them. Yet the subject matter of emotions also poses unique methodological dilemmas.

The standard style of social movement research—case studies of individual movements or movement organizations—makes it possible to cast doubt on theoretical claims, but difficult to support them in more than an illustrative or anecdotal manner. It is difficult to gain enough observations in a case study to substantiate empirical claims beyond showing that the evidence for the case at hand is consistent with one's theoretical claims. But without comparative evidence it is difficult to cast doubt on other plausible explanations. For example, if one scholar is claiming that a moral shock motivated movement activity while another is arguing for a political opportunity, and both seemed to be present, how would one adjudicate between them?

Like other scholars in this area, researchers attempting to establish the importance of emotions in answering standard questions about social movements need to do more work in substantiating their claims. A common strategy is to compare cases to ascertain whether one argument is better supported than another. One might compare across many movements, as William Gamson (1990) did to address the success and failure of a large number of American challengers. One might also make com-

parisons across smaller numbers of largely similar challengers. For instance, comparing movements supposedly influenced by moral shocks with movements not subject to shocks may be a useful research strategy. Such a comparison would aid in appraising arguments about the impact of moral shocks as well as in elucidating the emotional processes thought to be involved. Benefits of the small-N comparison can be seen in Rebecca Allahyari's contribution to the volume. She argues that the very different emotional orientations of two voluntary organizations, The Salvation Army and Loaves & Fishes, were responsible for their contrasting styles of activism on behalf of the homeless. Whereas The Salvation Army had an ideology and identity centered on rehabilitation, Loaves & Fishes' "felt politics of caring" was centered on compassion. This comparison helps to support her claim that the groups' distinctive emotion cultures shaped their orientation toward political activity. Allahyari might develop the point further by comparing groups that were more similar than these two. Unlike Loaves & Fishes, The Salvation Army relied largely on state funding and was staffed largely by beneficiaries rather than conscience constituencies—conditions that may have inhibited political action. It would be worth exploring other groups with private funding and conscience constituencies but with different emotion cultures than that of Loaves & Fishes in order to establish what it was about its felt politics that induced political action.

Even for those working with one challenger, there are comparative opportunities to appraise claims. One is to examine a movement or similar challengers across polities in which they mobilized (Kitschelt 1986; Jasper 1990; Amenta and Zylan 1991). For instance, comparing activists' claims-making in legal systems that diverge from the American one would help to develop Whittier's argument that abuse survivors' emotional displays in court accommodated features of their legal setting. To take another example, gay rights movements have mobilized throughout Western Europe, and undoubtedly have had different experiences with respect to moral shocks. Another related methodological strategy is to follow a challenger historically, or upstream and downstream, as Charles Tilly (1999) puts it.

In studies at a lower level of analysis, where the focus is on people's participation in protest, one can compare participants with otherwise similar persons who did not participate, as Doug McAdam (1988) did in his examination of volunteers in the Freedom Summer project. This sort of comparison is useful in supporting claims that certain emotional states underlie movement participation. In her chapter, Arlene Stein argues that feelings of shame motivated Christian right activists. Those

feelings were induced by prior experiences of economic or social hardship such as drug problems or bad relationships. To further buttress these claims, she might interview evangelical Christians who had suffered similar hardships but who did not turn to anti-gay protest. Along the same lines, Nepstad and Smith might interview Central American peace-movement participants who were not members of communities of faith in order to strengthen their argument about the role of church networks in building moral outrage. Gould might interview gay men and lesbians not involved in AIDS activism.

Analyzing movement emotions also presents methodological difficulties that go beyond the ones common in movement research, however. The first involves conceptualizing emotions in ways that can guide empirical research. The same problem has confronted those studying "resources." Just as it is possible to identify almost anything as a "resource," so the wide standard usage of "emotion" makes it easy to define terms broadly, perhaps too broadly. Also, scholars may disagree on how to conceptualize emotions, hindering research. Most of the contributors to the volume use them in the standard noun form. People have emotions and manage and act on them. Colin Barker suggests that emotions should be thought of as coloring cognition and should be employed in an adjectival or adverbial way. A lack of clarity on conceptual matters will make it difficult to formulate and appraise theoretical claims. If, for instance, indignation is left loosely defined, it may be possible for scholars to see it almost everywhere or only rarely, or confuse it with resentment or pique or outrage.

The only way out of this difficulty is conceptual clarity. Many of the contributors to the volume do well in identifying specific emotions and distinguishing them from other, similar ones. In the introduction, the editors indicate that emotions can be arrayed across two continua, according to their length and to the degree to which they are directed at specific objects. They suggest that certain long-term and object-oriented emotions are most likely to lead to social movement activity. One could go further perhaps in identifying the types of action one would expect from specific long-term and object-oriented emotions. Or one could hypothesize standard sequences in which undirected and short-term emotions are transformed into the long-term and object-oriented emotions hypothesized to lead to protest—as from unfocused anger to directed indignation or outrage—and the conditions under which such transformations would occur.

Other methodological challenges have to do with gaining the evidence needed to support claims about the prevalence and character of

emotions in collective action. In some circumstances emotions in general carry negative societal evaluations, and certain emotions almost always do. These evaluations and the sanctions that often accompany them provide good reasons for people to manage their emotions—to channel them in certain approved directions or to maintain that they have done so. Because our emotions are often fleeting and because we are often compelled to attempt to manage them, it seems likely that we can manage to forget or revise our more inconvenient emotional impulses. Probably anyone who has kept a journal has been surprised by the feelings expressed in previous entries. One's feelings often change over time, and one's emotions at one time may be overwhelmed or transformed by one's reactions to intervening events. Similarly, former movement participants may retrospectively exaggerate emotions or make them correspond to current feelings or what are deemed more culturally appropriate feelings. It is probably difficult to get interviewees to admit having been animated by short-term, undirected, and negative emotions like hate, envy, or boredom, even if they were. Gaining reliable information on people's emotions may therefore pose problems that go beyond those involved in reconstructing activists' programmatic statements and political beliefs. Because there are good reasons to manage and hide one's feelings, it may be difficult to rely on retrospective accounts of emotions by activists.

These problems are not insuperable. Sociologists may need to rely, in the manner of historians, on primary materials that provide little opportunity for face-saving, after-the-fact emotional reconstruction—such as diaries, contemporaneous testimony, transcripts of meetings, and other statements and actions recorded at about the time they happened. Participant observation is probably even more useful, perhaps with the researcher including the trajectory of his or her own feelings.

Several of the authors in this volume rely on psychoanalytical concepts that are sophisticated and promising, but that also pose unusual challenges for researchers. Determining whether shame motivates certain kinds of movement activity, to take Stein's chapter again, requires a style of interviewing that few sociologists are trained to do. It is far easier to ascertain whether someone has a friend who is also an activist, as expected by some political process arguments, than to diagnose whether someone is feeling shame (although see Scheff 1990). Also, even if one is trained to conduct such interviews, the time burden on the researcher may be more substantial than is usually the case in interviewing. It may be difficult to be certain about diagnoses in brief meetings with subjects who might be suspicious of or hostile to the motivations of the interviewer. One solution is to go beyond what is customary in sociological

interviewing. To use some concepts it may be necessary to gain psycho-analytical training. It may also be helpful to do more interviews than is usual and to interview subjects at greater length than is standard among sociologists.

In short, like other movement scholars, scholars of movement emotions would do well to avoid the methodological problems inherent in case studies. Opportunities for comparisons, even within a given movement, should be exploited to substantiate theoretical claims and to cast doubt on likely alternatives. Scholars of movement emotions have the opportunity to do more research at the individual level, but they face distinctive methodological challenges. Some of these have to do with the newness of thinking about these matters and others with the nature of emotion concepts.

Provoking New Questions and Lines of Research

New concepts can reinvigorate a field by illuminating previously unrecognized processes, dynamics, or effects, or by causing us to see phenomena that we have long studied in different ways. Either way, they lead us to ask new questions.

The problem here, though, lies in convincing other scholars that the questions are important ones. Scholars of emotions might follow a lead from those studying movements' consequences and impacts, an area of research which has taken off in recent years. Scholars of impacts argued that since social movements come into being in order to effect some important social change, scholars should examine the conditions in which they do effect some change, and what kind of change. One can perhaps make a similar argument with respect to emotions and social movements. People participate in movements in order to meet psychological or emotional needs that cannot easily be met otherwise or to find emotional benefits in movements as they go along. From this perspective, emotional experiences are an important purpose of social movements, a key reason for their existence or maintenance, and explaining why movements have different emotional contents becomes an important question in its own right. Although a number of chapters in this volume hint at this line of argument, it has yet to be made explicitly.

Another way to gain support for lines of questioning based on new concepts is to demonstrate that the new concepts and questions make it easier to answer the old ones. Again the literature on the impact of social movements provides a useful example. Piven and Cloward (1977) argued that what they called "poor people's movements" were different from

those with middle- or working-class bases. Poor people's movements depended on different political circumstances to be successful and needed to engage in different kinds of action. Similarly, the kinds of emotions that animate a movement—rage or love, shame or compassion—may influence its recruitment process, the strategies it chooses, and the impacts it has.

A third way to justify new questions is to show that the old ones were limited by the level of analysis at which they were pitched. Political process theories have been criticized for their meso-level focus, and scholars of emotions have responded aptly by analyzing the experiences of shame, anger, and fear that precede and accompany protest—a micro-level analysis. However, in some cases, political process theorists have asked larger questions about why protest changes in form—from reactive to pro-active in Tilly's (1978) formulation—or varies systematically across countries—from assimilative to disruptive in Kitschelt's (1986) model. From the start, new social movement theorists asked macro-level questions, notably about changes in the goals and targets of protest, setting off lines of research focused on explaining the role of identity in social movements and protest. Movement emotions scholars would do well to ask similarly macro-level questions. Can we identify historical changes in the dominant emotional content of protest? For instance, has moral outrage replaced resentment or fear or vengeance as the central emotion in protest? Is moral outrage more common or commonly expressed among social movements in some countries rather than others? Once scholars have documented shifts and differences like these we can begin to account for them.

♦ ♦ ♦

We conclude by offering a few tips on how to realize the promise of incorporating emotions into theory and research on social movements. First, some theoretical and analytical injunctions: Indicate where a new argument stands with regard to reigning political process ideas. Does it supplement these ideas or challenge them? If the idea is complementary to political process arguments, show how the idea improves upon the existing model and why the improvement is needed. If the new argument challenges the existing model, make clear the basis for its challenge. Indicate the likely scope conditions of the argument—how applicable the argument is to other contexts and movements. Make sure that the new concepts are not so wide as to preclude or confuse empirical analysis.

This leads into our methodological suggestions. To avoid the standard epistemological limitations of case studies, look for comparative op-

portunities. Compare processes across movements or compare people who participate with those who do not. As always, these sorts of comparisons will help in appraising arguments as well as refining them. In research, be sensitive to the fact that emotions are often of varying duration and some emotions are difficult for respondents to acknowledge.

Finally, a few suggestions for advancing an emotions research agenda. Do not abandon the meso-level questions on which political process theorists have concentrated. Scholars of emotions need not restrict themselves to describing emotional experiences or restrict themselves to elaborating a social psychology of emotions. They can contribute to better understanding why movements emerge, and why they take the forms and have the impacts they do. On the other hand, don't hesitate to go beyond the meso-level. Ask big historical questions about how dominant emotion cultures come into being and how they shape widespread perceptions of what is strategic, what is political, what is an interest, and what is good. Wrestling in new ways with the questions that have bedeviled students of social movements may secure for emotions the place in our analyses that they deserve.

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