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THE WAR AT HOME: ANTIWAR PROTESTS AND CONGRESSIONAL VOTING, 1965 TO 1973

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Time-series analysis is used to assess the relationship between antiwar protests and congressional voting on war-related roll calls during the Vietnam era. Using protest event data coded from The New York Times and counts of roll-call votes generated from congressional voting data, we test for three specific mechanisms: disruptive protest, signaling, and public opinion shift. Extreme forms of disruptive protest are hypothesized as having a direct positive effect on congressional voting. Lohmann's signaling model posits exactly the opposite relationship between protest and policy. Especially extreme protests are expected to have a negative effect on both the pace and pro-peace direction of congressional action. Conversely, large (and more moderate) protests are expected to have a positive effect on House and Senate voting. The final mechanism, public opinion shift, depicts the relationship as indirect, with protest encouraging public opinion change, which, in turn, encourages increasingly favorable congressional voting. The results are somewhat mixed with respect to all three mechanisms, but suggest an interesting general pattern. The most extreme or threatening forms of protest (e.g., those featuring violence by demonstrators and/or property damage) simultaneously increase pro-peace voting while depressing the overall pace of congressional action. The reverse is true for more persuasive forms of protest (e.g., large demonstrations), which appear to increase the pace of voting while depressing the likelihood of pro-peace outcomes.

Although the study of power and politics is central to both political science and sociology, sharp disciplinary divisions persist with regard to a host of topics that fall under these general headings. The study of social movements affords an interesting case

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in point. Over the past two decades, political sociologists have made social movements a central object of study. Political scientists, on the other hand, have paid comparatively little attention to social movements. No doubt much of this discrepancy is due to the central importance accorded formal political institutions within political science. Sociology, by

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contrast, has long been interested in informal or emergent social and political processes. But we suspect there is another influence at work here. From the outset, social-movement scholars within sociology appear to have assumed the efficacy of movements as vehicles of social and political change, whereas political scientists have tended to view social movements as politically ineffectual, stressing instead the role of elections and public opinion as the main popular mechanisms mediating policy shifts.

It is time, in our view, to broaden the study of social movements by incorporating insights from both disciplines. Some of this is already happening. For example, sociologists have taken to systematically assessing the impact of social movements on specific policy outcomes, rather than simply assuming effects. Still, with few exceptions, sociologists continue to evince little interest in the popular policy mechanisms of concern to political scientists. The disregard for public opinion is especially regrettable in our view. More specifically, the links between protest activity, shifts in public opinion, and policy change should be of central concern to social-movement scholars.

If sociologists assume the impact of movements without testing for effects, political scientists, with few exceptions (Fording 1997; Lohmann 1993; Rochon 1998; Tarrow 1998), continue to make the opposite error: assuming the ineffectuality of movements. Some of this rests on a stereotypic—and somewhat outdated—view of social movements as small collections of marginal outsiders, easily ignored by policy elites. But some movements are neither small (e.g., women's, environment, civil rights, etc.) nor properly conceived of as outside of mainstream institutions. The work of Katzenstein (1990, 1998), McCann (1994), Meyer and Tarrow (1997), and others represents an important corrective to the simplified insider/outsider view of formal institutions and social movements. The increasing institutionalization of the social-movement form in the United States and other Western democracies makes it all the more important for political scientists to take social movements seriously as a potential influence on policy processes (McAdam 1998; McCarthy and McPhail 1997; Meyer and Tarrow 1997).

Burstein's (1979, 1998; Burstein and Freudenburg 1978) and Lohmann's (1993) work is especially promising in this regard, as both theorize the complex relationships between protest activity, public opinion, and policy outcomes. We draw heavily on their work while also seeking to extend it. Specifically, we identify three mechanisms—*disruptive protest, signaling, and public opinion shift*—that we believe may help account for the variable impact social movements have on policy processes.

Besides this theoretical contribution, the empirical focus of our work bears mention as well. We seek to understand the predictive mechanisms that account for social-movement impacts in one of the most significant, yet curiously understudied, movements in recent U.S. history. Although the Vietnam era antiwar movement remains one of the most intense, large-scale, and divisive movements in American history, it has been almost totally ignored by social-movement scholars. One of the few exceptions is Burstein and Freudenburg's (1978) article on the impact (or lack thereof) of antiwar demonstrations on Senate voting on Vietnam War-related measures. Alas, the Burstein and Freudenburg article did not inspire other similar efforts. Instead, it remains a singular piece topically and ahead of its time in its creative use of systematic quantitative data to address the issue of movement outcomes. This lack of systematic research on the antiwar struggle by social-movement scholars is in marked contrast to the voluminous literatures on the civil rights struggle and modern women's movement. So while extending our understanding of the dynamics of social-movement outcomes, we hope as well to shed empirical light on the dynamics of contention associated with this neglected social movement. We begin by offering a brief history of the Vietnam War and the domestic struggle it set in motion.

VIETNAM: THE WAR ABROAD AND AT HOME

Our research focuses on antiwar protests, Congressional voting, and public opinion during the crucial period, 1965 to 1973. We focus on these years because they nicely demarcate the active period of domestic con-

tention over U.S. involvement in Vietnam. Although the Tonkin Gulf Resolution that led to a dramatic escalation in American involvement in Vietnam was passed by Congress on August 4, 1964, a visible antiwar movement only coalesced the following year. And even though the United States remained deeply enmeshed in Vietnam until the fall of Saigon in 1975, domestic contention over that involvement dropped to almost none following the signing of the Paris Peace Accords in 1973. Hence the temporal focus for our research.

In popular imagination, America's Vietnam misadventure roughly corresponds to the years of our research. In actual fact, U.S. involvement in Vietnam spans nearly the entire Cold War era. U.S. concern over Vietnam (then known as French Indochina) first surfaced in 1948–1949 when a strong Communist-inspired nationalist movement arose to challenge the existing French colonial regime. Unlike similar rebellions that developed in the same period throughout Southeast Asia, the Vietnamese movement proved more enduring, ultimately inflicting on the French the twin humiliations of crushing military defeat (at Dien Bien Phu in May 1954) and a negotiated peace settlement that marked the end of French rule in Vietnam. Under the terms of that settlement, Vietnam was to be unified through a popular election that was to take place in the summer of 1956. Fearing the outcome of the contest, the United States worked with allies to block the election, leaving the fate of the country unresolved and requiring ever greater commitments of American financial, diplomatic, and military resources to maintain the illegal arrangement. These efforts continued under three Presidents—Eisenhower, Kennedy, and Johnson—until the still-controversial Tonkin Gulf incident in August 1964 allowed Johnson the freer hand he sought to expand America's military involvement in the country.

The Tonkin Gulf Resolution marked a decisive turning point in America's long Vietnam saga. It did not, however, immediately engender significant opposition to the stepped-up U.S. war effort. Indeed, on August 4, the Resolution passed the House unanimously and occasioned only two dissenting votes in the Senate. Nor did the fall

presidential race occasion any electoral protest against Johnson's Vietnam policy. On the contrary, Johnson defeated his opponent, Barry Goldwater, in one of the most lopsided races in presidential history. But if antiwar dissent lay dormant through 1964, the situation changed markedly the following year. As troop levels rose rapidly and combat deaths grew apace of those levels, so too did public attention and opposition to the war. By May of 1965 the war had replaced civil rights as the "most important problem confronting the country" (Gallup 1972). And while a sizable majority of the American public still expressed support for the war effort, a burgeoning antiwar movement rooted on campus and in established peace groups expanded rapidly.

By the middle of 1966, 55 percent of the American public had come to regard the war as the country's most pressing problem (Gallup 1972). Although still in the minority, the percentage that had come to view the war as a "mistake" had risen to 35 percent. Reflecting these trends, antiwar protests grew ever larger and more disruptive. The October 1967 March on the Pentagon represented a new and disturbing high-water mark in antiwar protest, featuring perhaps 20,000 participants, repeated clashes with police, and some 647 arrests and 47 injuries requiring hospitalization (DeBenedetti 1990).

The Tet Offensive in February, 1968 deepened the domestic crisis still further by undermining public confidence in the government's handling of the war, and especially its continued rosy assurances that the war was "winnable." Having consistently portrayed the enemy as limited in its military capacity, the coordinated assaults that marked Tet, and for a time threatened several major South Vietnamese cities, soured public opinion all the more. So much so that Lyndon Johnson was forced to withdraw from the 1968 presidential race on the strength of growing antiwar sentiment. Ironically, it was that ultimate Cold warrior, Richard Nixon, who was to be the ultimate beneficiary of this sentiment as he defeated Johnson's Vice President, Hubert Humphrey, in the November election.

Nixon's antiwar "honeymoon" proved short-lived, however. The nationally coordinated Moratorium days held on October 15

and November 15, 1969 proved to be the largest single actions mounted by the antiwar movement. That same November, antiwar sentiment among the general public peaked as well, with 55 percent of those polled pronouncing the war a "mistake" (Boettcher 1985:446). The spring of 1970 brought heightened outrage, as leaks to the press laid bare Nixon's secret bombing campaign against suspected Vietcong bases in neutral Cambodia. Student reaction was immediate and intense. Protest peaked following the May 4 killing of four antiwar protestors at Kent State University, as countless colleges and universities shut down or otherwise suspended normal activities in deference to the deaths and the broader domestic crisis occasioned by the war. It was something of a surprise, then, when campus antiwar protest waned markedly following the reopening of campuses in the fall of 1970. As Nixon announced further troop reductions (even while maintaining his massive air campaign) and stepped up his efforts to forge a negotiated peace agreement with Hanoi, antiwar activity declined still further. By the time the Paris Peace Accords were signed in January of 1973, the movement was largely moribund.

MOVEMENT OUTCOMES/ POLICY CHANGE

We began by highlighting the very different conclusions reached by political scientists and sociologists concerning the impact of social movements on the policy process. With notable exceptions (e.g., Lipsky 1968), the conventional view in political science has been one that attributes little influence to social movements as vehicles of policy change. At least two dominant lines of research and theory in the field have converged to support this conclusion. The first is the voluminous literature spawned by Olson's (1965) book, *The Logic of Collective Action*. Concluding that it is irrational for someone to engage in costly collective action when they cannot be denied the benefits of such action, Olson and his many disciples have fashioned a powerful perspective that strongly implies the ineffectiveness of social movements as a force for policy change. There are actually two distinct im-

plications here. The first concerns the possibility of mounting collective action. If rational actors refrain from such action, then it should be nearly impossible to organize a movement in the first place. The second implication has to do with those who might take part in a movement, given its demonstrated irrationality. If rational actors can be counted on to refrain from such action, then only those with nonrational motives (e.g., extreme ideologues) are likely to gravitate to social movements, and this will generally keep the movements small and politically impotent.

The latter implication of the rational choice perspective shades into the second body of work alluded to above. This is the rich, if amorphous, work in political science that stresses the strategic preference of elected policymakers for broad centrist policies that can attract majority support. Downs's (1957) work on the "median voter" is both emblematic and influential in this regard. When coupled with research showing that movement activists are typically far more ideological and extreme in their policy preferences than the population at large (Finkel, Muller, and Opp 1989; Nie and Verba 1975), the implication is clear: Their characteristic nonrepresentativeness renders social movements inconsequential as a political force.

The study of social movements emerged as a significant subfield within sociology during the 1980s and has flourished ever since. For most of this period, the basic assumption of sociological analysts of social movements tended in the opposite direction from their colleagues in political science. Far from assuming the policy irrelevance of movements, sociologists asserted the impact of movements without, however, typically subjecting this assumption to systematic empirical tests.

In recent years this has changed, and while more work is still needed on the topic, a discernible literature on "movement outcomes" has begun to emerge in sociology (Amenta, Carruthers, and Zylan 1992; Andrews 1997, 2001; Banaszak 1996; Burstein 1979, 1993, 1998; Burstein and Freudenburg 1978; Button 1989; Cress and Snow 2000; Earl 2000; Gamson [1975] 1990; Giugni 1998; McAdam 1989; McCammon et al. 2001;

Mirowsky and Ross 1981; Schumaker 1975; Soule et al. 1999; Steedly and Foley 1979; Tarrow 1993). While certainly a positive development, the focus of this burgeoning literature remains elementary. The main question motivating this work has been "Do social movements matter?" That is, scholars have simply sought to assess the impact (or lack thereof) of specific movements on particular outcomes. Taken together, these various studies confirm the potential of social movements to serve, under certain circumstances, as effective vehicles of policy or other social change. That said, we know almost nothing about the "how" of the question. Having found that some movements appear to produce significant change effects, identifying the factors that account for the variability of those outcomes becomes the central analytic task. Accordingly, we take up three mechanisms that have been proposed as keys to understanding the variable outcomes of social movements.

SEARCHING FOR MEDIATING MECHANISMS

McAdam, Tarrow, and Tilly (2001) have called 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" (McAdam et al. 2001:11). The implication of this approach for the study of movement outcomes should be clear: Besides establishing an empirical relationship between movement activity and a given outcome, analysts should also attend as well to the identification and measurement of whatever mix of mechanisms appear to mediate the activity/outcome relationship.

Our outcome of interest is House and Senate roll-call votes on Vietnam War-related issues. The choice of roll calls is salutary for two reasons. First, the close temporal connection between antiwar demonstrations and the roll calls makes the establishment of a plausible causal connection a lot easier than if we were trying to assess the long-term impact of a given movement. Second, the central importance of these votes to the prosecu-

tion of the war means we have picked for study an outcome of demonstrable significance. The undeclared nature of the Vietnam conflict may have undercut Congress's constitutionally mandated war-making power. Nonetheless, Congress remained the central policy arena within which contentious debate over the prosecution of the war was waged. We distinguish two dimensions of Congressional action. The first is the *pace* of action as measured by the monthly count of House and Senate roll calls on war-related measures. The second dimension is the *direction* or *valence* of the action, defined as the proportion of votes cast for the pro-peace position(s) in any given month.

Below we hypothesize three different mechanisms through which antiwar protests may affect the pace and valence of Congressional action. These mechanisms reflect our interest in differentiating two general ways in which public protest may shape the policy responses of state actors. These two ways are *threat/disruption* and *persuasion*. At its core, democratic theory asserts the central importance of persuasion as the primary mechanism of policy change. Juxtaposed to this view is a well-developed theme in social movement studies that equates the effectiveness of social movements with their ability to achieve bargaining leverage through the disruption (or threatened disruption) of public order. Needless to say, these two general emphases/accounts are in tension. By deploying the following three mechanisms and various subsidiary hypotheses, we hope to learn more about the role that persuasion and threat/disruption play in mediating the impact of social movements.

We turn now to those mechanisms—*disruptive protest*, *signaling*, and *public opinion shift*—and test for their influence in predicting the relationship between antiwar protests and Congressional voting on Vietnam War-related measures.

DISRUPTIVE PROTEST

A recurrent debate in the literature concerns the tactical effectiveness of disruptive versus more moderate forms of movement action. Starting with Lipsky's (1968) classic work on "protest as a resource," many analysts have endorsed the general idea that

movement success typically depends on the ability of challenging groups to create "negative inducements to elite bargaining" through the disruption of public order and the threat such disruption poses to the realization of elite interests (Astin et al. 1975; McAdam 1982; Tarrow 1998; Tilly, Tilly, and Tilly 1975). This basic tenet underlies McAdam's (1983) work on "tactical innovation" as well as the influential argument advanced by Piven and Cloward (1993). For McAdam, strategic innovation is not effective in and of itself, but only to the extent that the introduction of a new tactic occasions renewed disruptions of public order that compel action by authorities. For Piven and Cloward, rapid expansions in welfare rolls should generally be seen as ameliorative, if cooptative, responses to violence on the part of the urban poor. In a more contemporary vein, Cress and Snow (2000) find that, in combination with other factors, disruptive protest helps explain the success of mobilization of the homeless in a number of the cities they studied.

Additional support for the mediating role of disruption/violence in specifying the relationship between movement action and social change comes from Gamson ([1975] 1990). In his most controversial finding, Gamson shows that groups which deployed either "violence" or "nonviolent constraints" tended to enjoy a higher rate of success than their more moderate or restrained counterparts (Gamson [1975] 1990: chap. 6). Because of the univariate character of Gamson's analyses, several critics discounted his results, often singling out the violence effect as a main suspect in this regard. However, on reanalysis, several investigators confirmed the general thrust of Gamson's original finding and interpretation (Mirowsky and Ross 1981; Steedly and Foley 1979).

One can also glean some support for the disruption/violence thesis from two other literatures: those on strikes/strike outcomes and on the "urban disorders" that rocked the United States in the 1960s. With respect to strikes, Shorter and Tilly (1974) found a positive correlation between the use of violence and strike outcomes in their study of French strike activity. Several other studies, however, yielded opposite results, including

work by Taft and Ross (1969) on labor unrest in the United States and Snyder and Kelly (1976) on strikes in Italy.

Similarly mixed results characterize the "urban disorders" literature. Some studies appear to show moderately positive effects at the city level for those urban areas that experienced rioting (Colby 1982; Eisinger 1973; Jennings, 1979). Others appear to show negative effects (cf. Welch 1975). The bulk of studies, however, assert no effect or mixed results that do not allow for a definitive answer to the question (Berkowitz 1974; Button 1978; Feagin and Hahn 1973; Kelly and Snyder 1980; Sears and McConahay 1973).

So our survey of existing empirical work has turned up mixed or inconclusive findings on the relationship between social-movement disruption and policy outcomes. These results could well be attributable to other important contextual factors, as Button (1978) and others argue (e.g., Cress and Snow 2000). But there may be an important methodological factor contributing to the mixed results as well. Quite simply, these various studies adhere to no single operational definition of "disruption." In fact, in most cases, there is no explicit operationalization mentioned in connection with the concept. But one can imagine protest events being "disruptive" in a variety of ways. A disruptive protest might feature violent tactics by demonstrators, or property damage as a byproduct of a large, yet generally peaceful protest march; or injuries to protesters resulting from overzealous police actions, and so on. In thinking through the various dimensions of public protests, we identified four features that might be thought of as contributing to the overall "disruptive" intensity of a protest event. These features are: (1) the use of violent tactics by demonstrators, (2) the use of violence by law enforcement personnel, (3) property damage as a result of the protest, and (4) injuries resulting from the protest.

In distinguishing these various dimensions, we aim to move beyond the undifferentiated conception and assessment of disruption. We seek, instead, to explore the variable impact of different components of public protest events. We do so by positing two contrasting pairs of the above protest

dimensions. In the first, we differentiate violence by demonstrators from violence by police. The second pairing—property damage versus injuries—mirrors the first, with property damage resulting mostly from extreme action by demonstrators, and injuries (overwhelmingly to demonstrators) owing to the aggressive policing of protest. These pairings can also be tied to the general distinction between *threat* and *persuasion*. Although none of the four dimensions match clearly to the notion of *persuasion*, violence by demonstrators and property damage are reasonable proxies for *threat*—both represent extreme and/or violent forms of protest by movement groups. Drawing on the traditional movement literature, our first hypothesis posits a significant impact of these more *threatening* dimensions of protest events.

Hypothesis 1: Protest events featuring violence by demonstrators and/or property damage will be related to an increase in House and Senate voting and an increase in the pro-peace results of those votes.

Hypothesis 1 rests on a simple view of the relationship between disruption and Congressional action; that is, the more extreme or violent the forms of action, the more and more favorable the Congressional response.

But the movement literature suggests a second, more qualitative link between disruption and state response. In particular, studies of the civil rights movement suggest that it is not disruption per se, but disruption characterized by violence directed against the movement that is especially productive of favorable government response (McAdam 1982, 1983). This account suggests a second way in which disruption may be linked to state action.

Hypothesis 2: Protest events featuring violence by law enforcement officials and/or injuries to demonstrators will increase the pace and pro-peace valence of House and Senate roll calls.

SIGNALING

Lohmann (1993:319) proposes a “signaling model of informative and manipulative po-

litical action” in which elected officials use mass political activity to better understand the policy preferences of the electorate. While Lohmann is at least as interested in using her model to explain the incentives that allow individual activists to overcome the “free-rider problem,” she also seeks to characterize the functional benefits to elected officials of attending to the information conveyed by mass political activity. She begins with a familiar characterization of elected officials: “The political leader wishes to make a decision that is advantageous for a majority of the population. . . . A leader fully informed about the state of the world would choose a policy preferred by a majority under the true state of the world” (Lohmann 1993:320). But how does a leader discern the “true state of the world”? Lohmann’s insight is to recognize that public protest may serve as an informative signal to policymakers about the state of the world. But how to read those signals? For Lohmann, one important cue comes from the number of “activist moderates” involved in protest action. In her words, “one implication of the model is that the political leader shifts policy if the political action turnout exceeds a critical threshold” (Lohmann 1993:322). This leads to our third hypothesis.

Hypothesis 3: There will be a significant, positive relationship between size of protest event and the pace and pro-peace valence of House and Senate roll calls. That is, large events will be related to more roll calls and an increased likelihood of pro-peace outcomes.

Beyond the “size of the protest movement” (Lohmann 1993:319), Lohmann offers few specifics on how elected officials read public protest. But she suggests another meaningful feature of mass political activity when she writes that “the political leader discounts the observed turnout for extremist political action and shifts policy [only] if the estimated number of activist moderates exceeds a critical threshold” (p. 319, italics added). If elected officials are motivated to read public protests for whatever signs of majoritarian policy sentiment they may reveal, it makes sense—following Lohmann—to hypothesize a negative effect of protests

characterized by the use of especially violent or “extremist” tactics. Although Lohmann’s stress is on the revealed “policy preferences” of activists, it seems reasonable to suggest that extremist tactics (operationalized here as violence by demonstrators and/or property damage) would be interpreted by policymakers as synonymous with extreme, and therefore nonmajoritarian, policy positions. Use of such tactics are thus likely to motivate elected officials to signal opposition to the protesters by voting to sustain the policy status quo.

Hypothesis 4: Protest events featuring violence by demonstrators and/or property damage will be related to a decrease in House and Senate voting and a decrease in the pro-peace results of those votes.

Note that Hypothesis 4 runs exactly counter to the functional view of disruption sketched in Hypothesis 1. That is, rather than viewing extreme and/or violent forms of protest as strategically effective, Hypothesis 4 posits that certain forms of disruption signal extremist views, and thus work to the detriment of the movement. These opposite predictions can be traced back to the fundamental distinction between *threat* and *persuasion*. Hypothesis 1 assumes the strategic effectiveness of threat, while Lohmann’s notion of signaling is consistent with the stress on persuasion in traditional democratic theory. The general notion of persuasion also underlies our third and final mechanism: public opinion shift.

PUBLIC OPINION SHIFT

The link between public opinion and policy outcomes has interested political scientists (and to a lesser extent, economists) for years (Arnold 1990; Arrow 1963; Bartels 1991; Dahl 1956; Erikson, Wright, and McIver 1993; Monroe 1998; Page and Shapiro 1983; Sen 1970; Stimson, MacKuen, and Erikson 1995). Viewing elected officials as rational actors intent on staying in office, these scholars have hypothesized that politicians will generally modify their own policy preferences to fit shifting public opinion in an effort to retain electoral support. Theory aside, several broad studies have yielded

empirical evidence supporting a reasonably strong positive association between opinion shift and policy change. For instance, in their exhaustive survey of the opinion/policy link, Page and Shapiro (1983:175) find “considerable evidence that public opinion is often a proximate cause of policy.” More recently, Stimson et al. (1995) have assessed the dynamic responsiveness of all branches of government to shifts in public opinion over the period 1956–1993, and, allowing for variable responsiveness by period and institution, they “find [strong support] that policy responds dynamically to public opinion change” (p. 543).

All of this suggests a less direct link between protest, public opinion, and policy change than the one imagined by Lohmann. Here protest does not work directly—as a *signal*—to change the policies of leaders, but rather does so only indirectly by first shifting public opinion in the direction of movement goals. Once opinion has shifted in this way, it then acts, in the manner consistent with the aforementioned research (and classical democratic theory) to alter the policy preferences of those public officials who are subject to electoral pressures. This is the third and final mechanism—*public opinion shift*—alluded to above.

Alas, as noted previously, political sociologists have evinced far less interest in public opinion than have political scientists. Especially regrettable is the almost total ignorance of the topic among social movement scholars.¹ Indeed, the absence of attention makes it somewhat difficult to formulate hypotheses regarding the relationship between protest activity and public opinion. Drawing on sketchy scholarly evidence, we offer the following two provisional hypotheses:

Hypothesis 5: Protest events that feature violence by police and/or injuries to demonstrators will be positively related to increases in public opinion against the war.

Hypothesis 6: The simple pace of protest events will be positively related to increases in public opposition to the war as measured by public opinion polls.

¹ Burstein (1998) is conspicuous among social movement scholars in doing work on the topic.

Hypothesis 5 rests on what might be termed the “functional victimization” view articulated by scholars of the civil rights movement. Indeed, that account posits the identical two-step process imagined here. That is, violence against demonstrators led to increased public attention and support for insurgents, which, in turn, prompted increasingly favorable government action in relation to the movement. Hypothesis 6 draws exclusively on Schuman’s (1972) empirical work on the changing nature of public opinion about the Vietnam War in relation to the antiwar movement. Schuman shows that increasing protests led to both growing opposition to the war *and* a decided public opinion backlash against the movement.

Our final hypothesis concerns the link between public opinion and House and Senate voting. The paucity of research on protest and public opinion made it difficult to formulate the previous two hypotheses, but the vast literature on the link between public opinion and policy outcomes makes this last hypothesis easy to state:

Hypothesis 7: Increased public opinion against the war will be positively related to increases in the pace of Congressional voting and the likelihood of pro-peace outcomes from the votes.

PROTEST EVENT RESEARCH

The term protest event research refers to empirical work on social movements that utilizes content coding of newspaper accounts of movement events to study the dynamics of contention involving movement and other actors (i.e., countermovements, state actors, etc.). While some social scientists had made use of a rudimentary form of events analysis to study the patterning, correlates, and consequences of the “urban disorders” that took place in the United States in the 1960s (cf. Lieberson and Silverman 1965; Spilerman 1970, 1976), Tilly and various colleagues pioneered the method as a tool for social movement research (Shorter and Tilly 1974; Tilly et al. 1975). Making systematic use of newspaper and other published sources of “collective action events,” Tilly and his colleagues mapped the ebb and flow of popular protest in Italy, Germany,

and France over much of the nineteenth and early twentieth centuries.

Event research has now developed into one of the methodological staples of social movement researchers. Indeed, many of the classic empirical works in the field make use of the technique (Andrews 1997; Burstein and Freudenburg 1978; Duyvendak 1995; Jenkins 1985; Jenkins and Perrow 1977; Koopmans 1993, 1995; Kriesi et al. 1995; McAdam 1982, 1983; Olzak 1987, 1989, 1992; Tarrow 1989). The larger project of which this study is a part is in the main tradition of protest event research. In designing the project, however, the principal investigators—who, in addition to the first author, include John McCarthy, Susan Olzak and Sarah Soule—sought to improve on the approach by adding a few methodological innovations of their own.

To be fair, protest event research has been criticized on a number of grounds by social movement and other analysts (Danziger 1975; Davenport 2001; Franzosi 1987; McCarthy, McPhail, and Smith 1996; Mueller 1997; Oliver and Myers 1999; Snyder and Kelly 1977). At the heart of these critiques is skepticism that newspapers represent anything like an unbiased source of information on collective action events. Much of this skepticism is informed by systematic evidence attesting to various forms of bias in newspaper accounts of protest events. For example, McCarthy et al. (1996), show that size, location, and violence predict newspaper coverage of protest events. In their study of collective action in Madison, Wisconsin, Oliver and Myers (1999) find several sources of bias in the coverage of “public events.” They find that large events, or those involving “conflict,” are more likely to be covered by local newspapers. In addition, business-sponsored events and those taking place in more central locations in Madison were more likely to be reported in the papers. Davenport’s (2001) study of Black Panther activity in the San Francisco Area in the late 1960s/early 1970s bears mention as well. Rather than predicting coverage on the basis of various factors (e.g., size, violence, etc.), Davenport simply tracks Panther activity using five different newspapers, the *The New York Times*, *The Oakland*

Tribune, *The Berkeley Barb*, the *Black Panther Intercommunal News Service*, and the *Sun Reporter*.² What is noteworthy in Davenport's data is the absence of any real convergence in the temporal distribution of events produced by the five newspapers. What this marked divergence in activity patterns results from is not clear. Davenport does not attempt to resolve the inconsistency by attributing greater accuracy to one or more sources over the others. Instead, as his provocative title suggests, these various sources of data represent a real-life example of the "Rashomon Effect." Like the witnesses in the movie of the same name, Davenport's papers represent distinctive takes on a complex phenomenon rather than consistent windows on an underlying pattern. The methodological implications of these findings are sobering to say the least.

We take this evidence of newspaper bias very seriously, and needless to say, it poses serious challenges to those who would use such data to study social movement or other public collective action. But the challenges vary considerably depending on the specific nature of the event research. For instance, comparing the results of "national" versus more local studies of protest activity would seem to support a straightforward conclusion: All things equal, the more local the focus of attention, the more credible the use of newspapers as a source of event data. The other strong conclusion that we draw from the multiple critiques of the method is that studies focusing on events as a dependent variable are far more questionable than those treating those same events as an independent, or predictor, variable. To treat events as an outcome variable would seem to require that the analyst view events as a credible proxy for some underlying temporal pattern of action, an assumption strongly challenged by the various studies we reviewed above. To study the impact of events—more accurately the impact of *reported events*—on some later outcome variable (e.g., public opinion, state action, etc.) does not oblige the analyst to make this

same assumption. On the contrary, when events are deployed as an independent variable, the method honors the central insight that informs the various critiques reviewed above. That is, rather than treat newspapers as a passive channel of communication, the use of events as a predictor variable acknowledges the active role of the print media as an influence on the dynamics of contention.

The empirical case at issue here can be used to illustrate our point. The principal dependent variable in our analysis is Congressional voting. If we were to try to identify the main sources of influence on votes by individual Senators or members of Congress, what would the prime candidates be? It might be nice to believe that individual conscience would be decisive in this regard, but most Congressional analysts have long asserted the primacy of two more self-interested influences. One is party loyalty, and the other is the perceived "electoral returns" to a given pattern of voting. The latter factor involves aligning one's voting record to the preferences of whatever electoral constituency the Senator or member of Congress sees as key to his or her reelection. But how do these elected officials monitor electoral preference? Today, regular tracking polls and focus-group analyses have become key to this effort. But neither technique was widely used in the Vietnam War era. Instead, in trying to gauge how the war was "playing in Peoria," Senators and members of Congress would have relied primarily on letters from constituents in their home states, irregular public opinion polls, and media reports of pro-war and antiwar demonstrations. And no newspaper probably played a more central role in these assessment efforts than *The New York Times*. Indeed, the criticisms of the paper by scores of politicians during these years—Nixon, Johnson, and Agnew are only the most prominent to come to mind—only serves to make the point. In its coverage of reported events, no less than in its editorial policy, the *Times* was an active influence on public discourse and policy-making during the war. Indeed, our efforts to assess the impact of antiwar protests on Congressional voting depends on the mediating influence of the *Times*.

² *The Panther Speaks* was the official newspaper of the Black Panther Party during the period in question. *The Berkeley Barb* was a prominent left "underground" paper during the same period.

THE STUDY

Since 1997, the aforementioned consortium of social-movement researchers have been involved in a massive data collection effort designed to better understand the causes, consequences, and changing nature of public protest in the United States in the post-World War II period. To date, this effort has centered on assembling time-series data sets on public protest events and a host of theoretically relevant covariates for the period 1960–1980. Eventually, however, we hope to extend these data sets both backward and forward in time to encompass the full 50-year period, from 1950 to 2000.

PROTEST EVENTS

As with all protest-event research, the empirical centerpiece of the project has involved the ongoing identification and coding of newspaper accounts of public protest events. *The New York Times* has functioned as the newspaper of record as we have worked to assemble the 20-year event time series (1960–1980) for which we now have coded and cleaned data. We will not, however, make use of all of this data in this article. Instead, we will rely only on events from 1965 to 1973 and only for one *issue area*: peace.

We define a *public protest event* as any event or action “in which individuals collectively make a claim or express a grievance on behalf of a social movement organization or [aggrieved] social category” (Uhrig and Van Dyke 1996:1). Our operational definition of a protest event specifies three “defining features” that the event must exhibit to be included in the data set. First, the act must involve a group of people rather than a single individual. Second, it must involve “contentious claim-making.” That is, at the heart of the event there must be, implicitly or explicitly, a demand for either a change in society or an avowed desire to resist a proposed change. In the case of the Vietnam War, this would mean that protests would need to be motivated by either a desire to change the nature of U.S. war-making policy or to resist efforts to change that policy.³ The

third and final qualifying feature of the event concerns the identity of the initiating actors. We accept two types of actions: those initiated by named social movement organizations (SMO), or those by any “actor performing in a manner outside that actor’s official capacity to express a grievance, claim, belief, or opinion” (Uhrig and Van Dyke 1996:2). The intent here was to demarcate a terrain of *protest* action distinct from other more conventional forms of claim-making or other political activity.

Although not identical to the coding conventions employed by other event researchers, these definitions are nonetheless of a piece with earlier work in the tradition. Even while employing these definitions, however, we treat events differently than many past researchers. Traditionally, event researchers have utilized simple counts of events to measure movement activity. Our data will allow us to do that as well, but in addition to these event counts, however, we will want to assess the impact of movement activity by attending to various dimensions or features of protest events.

DISRUPTIVE EVENTS

As noted above, a good many analysts have argued that social movements become a significant force for social change only when they succeed in generating “bargaining leverage” through the disruption of normal social or political routines. But simple event counts tell us next to nothing about the disruptive *intensity* of the aggregated actions. To get at this theoretically important aspect of movement actions, we code all of our events along the following four dichotomous dimensions⁴: (1) *injuries*—were there any injuries reported in connection with the event? (2) *violence by demonstrators*—did

in pro-war as in antiwar protests during this period, the paucity of the former has led us to focus exclusively on antiwar protests.

⁴ We originally intended to employ a sixth dimension in this part of our research. The sixth dimension focused on the presence/absence of deaths in connection with the event. Assuming one values the generally nonviolent character of American politics, the good news is that too few events featured deaths (three by actual count) to allow us to employ this dimension in the study.

³ But while we are theoretically as interested

the event feature the use of violence by demonstrators? (3) *violence by police*—did the event feature the use of violence by police? and (4) *property damage*—did the event result in property damage?

In addition to these four dimensions, we differentiate protest events based on the number of movement participants involved. We identify those events with more than 10,000 participants and those with less than 1,000 participants. Our use of these five dimensions (the four above plus size) is straightforward. Besides employing simple event counts, we want to determine whether the predictive power of our models is increased when we substitute any one or various combinations of these dimensions for the event counts. The theoretical significance of these substitutions is worth highlighting. If, as many have argued, movements owe their force as social change vehicles to their disruptive capacity, then these dimensions—especially those capturing especially extreme or violent movement action (e.g., violence by demonstrators, property damage)—ought to produce stronger predictive effects than models employing simple event counts.

THE DEPENDENT VARIABLE: HOUSE AND SENATE ROLL-CALL VOTES

We derive our two main dependent variables from all House and Senate roll-call votes on Vietnam War-related measures between 1965 and 1973. We operationalize *pace* as the monthly counts of all relevant roll calls, and *valence* as the proportion of legislators voting the peace position in a given month. In employing these two dependent variables, we seek to distinguish between two critically important forms of political influence: *agenda setting*—or “agenda responsiveness,” as Schumaker (1975) termed it—on the one hand, and—*policy change* on the other. Too often, in our view, movement analysts adopt some version of policy change as the only salient metric for assessing impact, while ignoring the fact that agenda setting is both a significant achievement in its own right and a prerequisite for policy change.

These two roll-call measures were generated using a unique and invaluable data set

compiled by Rosenthal and Poole (1991) entitled “United States Congressional Roll Call Voting Records, 1789-1987: Reformatted Data.”⁵ The data set includes every roll-call vote taken in the Senate or House between 1789 and 1987. Each entry in the data set includes information about the date the vote was taken, the sponsor of the vote, and the number of yes and no votes recorded for the measure. The entry also included a brief summary of the measure that allowed us to code the valence of the vote’s outcome. To extract from this massive data set the votes relevant to our case, we first used the issue codes devised by Poole and Rosenthal (1997). To ensure that we did not miss relevant measures, we included in our initial list of “candidate votes” all roll calls between 1965 and 1973 that fell into any one of the following four issue codes: “Vietnam War,” “Selective Service,” “Peace Movements/Pacifism/Anti-Military,” and “Communists/Communism/Un-American Activities.” We then winnowed the large pool of candidate events by carefully reading the summary of each one and including in the final data set only those votes that bore directly either on (1) the prosecution of the war, or (2) the workings and legislative status of the Selective Service System. Votes that bore a more tangential relationship to the Vietnam conflict (e.g., those having to do with the repression of domestic movements or U.S. relations with Communist countries such as China and the Soviet Union) were excluded from the final data set.

These procedures yielded a final data set of 236 votes, 80 taken in the House and 156 in the Senate. For each vote, we recorded the date, the distribution of yes/no votes, and, in most cases, the valence of the outcome. Forty-seven of the 236 votes yielded no clear valence and were therefore coded as missing on this dimension.

⁵ We gratefully acknowledge the generosity shown us by Howard Rosenthal and Keith Poole. Besides making their extraordinary data set available to us—a courtesy that is generally available to the scholarly community—they also were gracious enough to answer numerous questions, which allowed us to better adapt our methodological procedures to the ones they used in compiling their data set.

Table 1. Descriptive Statistics of Variables in Models Predicting Vietnam War-Related Congressional Roll-Call Voting 1965 to 1973

Variable	Observations (Months)	Mean	S.D.	Min.	Max.	Data Source
<i>Congressional Roll-Call Voting</i>						
Roll-call counts	97	2.19	5.43	0	45	Rosenthal and Poole (1991); Poole and Rosenthal (1997)
Pro-peace votes	42	40.73	21.32	.23	93	Rosenthal and Poole (1991); Poole and Rosenthal (1997)
<i>Anti-Vietnam War Protest Events, 1965 to 1973</i>						
All events	97	10.61	13.69	0	90	<i>New York Times</i> (1965–1973)
Types of Events:						
Events with more than 10,000 participants	97	.38	.89	0	4	<i>New York Times</i> (1965–1973)
Events with violence by demonstrators	97	.96	2.20	0	15	<i>New York Times</i> (1965–1973)
Events with violence by police	97	1.00	2.20	0	14	<i>New York Times</i> (1965–1973)
Events with injuries	97	.75	1.60	0	9	<i>New York Times</i> (1965–1973)
Events with property damage	97	.64	1.55	0	10	<i>New York Times</i> (1965–1973)
<i>Other Covariates</i>						
U.S. military deaths in Vietnam	97	533.95	514.86	0	2,413	Vietnam War Memorial Fund
Call-ups in Selective Service	97	15,494.74	10,778.74	1,666.67	35,366.67	Selective Service System (1967–1974)
Conscientious objectors	97	2,850.66	1,205.19	1,616.00	5,456.5	Selective Service System (1967–1974)
Percentage of public believing war is a mistake	22	51.69	14.82	26.38	76.99	Gallup International, Inc. (1965–1973)
Published articles related to Vietnam	97	73.52	42.01	3	171	<i>Reader's Guide to Periodical Literature</i>
Period Dummy Variables:						
January 1965 to October 1967 (Pentagon March) (t_1)						[Reference category]
November 1967 to December 1968 (t_2)				0	1	
January 1969 (Nixon election) to May 1970 (Kent State) (t_3)				0	1	
June 1970 to December 1973 (t_4)				0	1	

Our measure of *pace* is simply the monthly counts of these roll-call votes. Our *valence* measure is a bit more complicated. We construct it in two steps. First, using only those individual votes cast by members of Congress for which the valence is clear, we sum the total of pro-peace votes cast in

that month (e.g., “yes” votes if the framing of the roll call is pro-peace; “no” votes if the framing of the roll call is pro-war) and then divide the total by the number of roll calls in the month. In order to have a compatible measure for both chambers, we scale the pro-peace votes in the House to fit with the

metric used for the Senate (from 0-435 to 0-100). The descriptive statistics for these two dependent variables are shown in Table 1.

OTHER COVARIATES

Besides broadening our analysis of events to include an assessment of the impact of disruptive events on the pace and outcome of Congressional action, the study is marked by another, if not quite so distinctive, innovation. We refer to the general project's commitment to assemble a full slate of time-series covariates required for a systematic empirical assessment of movement outcomes. These covariates include over-time measures of public opinion change, mass media coverage of the war, and the pace of the Vietnam War (e.g., combat deaths, troop call-ups, etc.); the pace of the war being thought of as the objective pressure or "demand" for protest against the war. Before turning to our results, we take up each of these covariates in turn. (Table 1 shows the descriptive statistics for these covariates.)

U.S. MILITARY DEATHS IN VIETNAM. We use the monthly count of military deaths in Vietnam to measure the pace of the war. It is reasonable to hypothesize that the more deaths, the stronger the opposition to the war among the general public and U.S. policymakers alike. Such effects need to be controlled in our examination of the impact of protests on congressional action. We rely on statistics provided by an organization called the Vietnam War Memorial Fund. From their website (in the searchable section of "Virtual Wall") we obtain death statistics for the 108 months between 1965 and 1973.⁶ In the regression model, we change the unit from per person to per 100 person in order to obtain reader-friendly coefficients.

CALL-UPS BY THE SELECTIVE SERVICE. Call-ups are the number of young men drafted by the U.S. military at various points during the study period. This is a

measure of shifting U.S. involvement in Vietnam. We draw information from a series of publications entitled *Semi-Annual Report of the Director of Selective Service* (U.S. Government Printing Office, July 1967–December 1973). The report gives us call-up numbers only for every half a year, so we assign the mean of the semiannual count in our unit month. It is also weighted by 1/100 to provide reader-friendly coefficients for the regression models.

CONSCIENTIOUS OBJECTORS. Conscientious objectors were those individuals who registered for a deferment or exemption from the draft "on the grounds of religious training and belief" (U.S. Government Printing Office, 1967:9). In the Vietnam War era, such registration for deferment or exemption became a widespread tactic of protest and resistance against the war. The mean semiannual count is assigned to each month and is weighted by 1/100 in the models.

PUBLIC OPINION. Among various polls released during the Vietnam conflict, the question that was asked most frequently and persistently is the following one by Gallup: "In view of developments since we entered the fighting in Vietnam, do you think the U.S. made a mistake in sending troops to fight in Vietnam?" (Gallup International, Inc. 1965–1973). There are 22 data points in the period of interest. Because the data show a strong linear trend over time ($R^2 = .83$), the percentages are regressed on time to provide estimates for months when the question was not asked. For months for which observations are available, they are used; for other months, regression estimates (OLS) are used. This treatment of public opinion as an independent variable follows the approach to this issue by Burstein and Freudenburg (1978:117). In our analysis, however, we also treat public opinion as a dependent variable to see whether protests have an impact on public opinion. In those models, we include only the 22 data points mentioned above. In order to account for the intrinsic linear trend, we use as a dependent variable the difference between the observed percentages and the value estimated from the above mentioned regression line.

NUMBER OF ARTICLES PUBLISHED IN PERIODICALS. To measure media attention to the Vietnam War issue, we count the num-

⁶ We accessed the website in December 2000; the statistics could change slightly because of newly available data on Vietnam deaths. We should also caution that we do not know whether these numbers are the same as the numbers of deaths accessible to the public during the Vietnam conflict.

ber of related articles published in the month. Under the alphabetically ordered headings, each volume of the *Readers' Guide to Periodical Literature* (1965–1974) lists the title, publishing date, and name of the periodical for all the articles published in the previous year. We photocopy the pages under the heading "Vietnam" and count the number of published articles in the 108 months of our period.⁷ It is weighted by 1/100 to yield more accessible coefficients in our models.

METHOD: TIME-SERIES ANALYSIS WITH ZERO-INFLATED Poisson REGRESSION

The result of our data collection effort is a time-series data set spanning the 108 months of 1965 through 1973 and consisting of monthly counts for most of the variables defined above. In most of our models we lag observations in the previous month to predict the likelihood of congressional voting in the month following.

Before we choose appropriate models to generate estimates, however, we discuss three issues germane to an understanding of outcome variables in time-series data (Wonnacott and Wonnacott 1990). The first is *autocorrelation*—whether the observations in different units are correlated to one another. By conducting a Durbin-Watson test, we find the assumption of independence

⁷ There are two reasons that such counts may not reflect the exact number of Vietnam war-related articles. First, some articles related to the war may be classified under headings other than "Vietnam," such as "defense," "medical relief work," and so on. The task would become unmanageable if we tried to search for possible entries under all possibly related headings. This may result in an undercount of relevant articles. On the other hand, the articles under the heading "Vietnam" are not always directly related to the war. Some entries are about the geographical, cultural, and political background of Vietnam. We include all the entries under "Vietnam" because we find it difficult to set criteria to gauge how close an entry is related to the war by reading its title. This may result in an overcount of articles. Because these two sources of error are counter to one another, we are confident that the measure is a broadly credible proxy for temporal variation in media attention paid to the Vietnam War.

holds; autocorrelation is not significant for our outcome measures. Substantively, this means that the likelihood of congressional voting in a month is not contingent upon how many roll calls have occurred in previous months. The second issue is whether the outcome variable has an intrinsic *trend* that may not be explained away by the independent variables. If we examine the shape of the temporal distribution of roll calls, there is a discernable pattern of period effects (see Figure 1): There appears to be an increase in Congressional action early in 1967, following a two-year period of inaction; and another period of heightened activity late in 1970 that does not subside until 1973. It is empirically unknown whether such a pattern can be accounted for by the protest events and our other covariates. With this consideration in mind, we introduce categorical variables dividing the 108 months into four periods (in turn coded into three dummy variables). We use three historical events as cutting points: the October 1967 Pentagon March, Richard Nixon's January ascension to the White House, and the May 1970 killings at Kent State. In doing so, our model will allow the rate of likely roll-call voting to differ in the four periods.

We use zero-inflated Poisson regression (ZIP) models to generate estimates for our predictions. While event-count data in principle are suitable for Poisson regression, there are two problems with using regular Poisson regression in our case. First, our outcome variables violate a key assumption for using poison regression: The variance must equal the mean. As shown in Table 1, the variances of the three outcome variables are greater than the mean, a problem called overdispersion. Second, there is a substantial number of months in which our dependent variables will have zero value; as a result regular Poisson regression will underpredict the chances of being a zero count. ZIP models are designed to adjust the difference between the mean and variance, as well as to add more predictions of zero values on the outcome variable (Greene 1997; Long 1997).

The general form of the models can be expressed as the following equation:

$$\mu_i = \exp(x_i\beta), \quad (1)$$

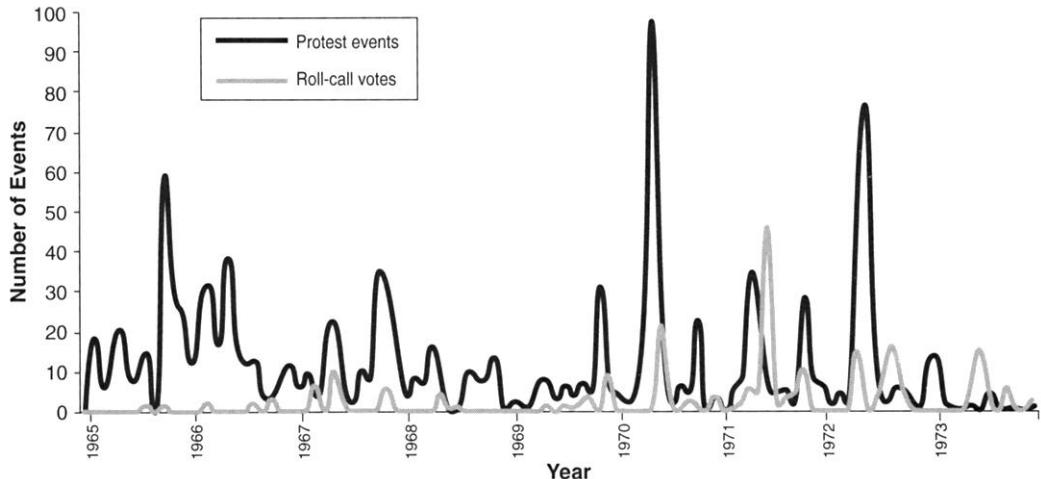


Figure 1. Temporal Distribution of Protest Events and Congressional Roll-Call Votes, 1965 to 1973

where μ_i is the predicted count of roll calls in i^{th} month; x_i is a set of values of our covariates in the given month; and β is a vector of parameters to be estimated. Maximum-likelihood estimates are used. That is, β is a set of parameters that maximizes the likelihood of the prediction and whose function is based on the two-stage probability function of the ZIP model (see Long 1997:243–45).

RESULTS

We turn now to the results of our various analyses. Most of our findings are presented in two tables: Table 2 shows various models predicting the pace of Congressional voting; Table 3 shows the same models predicting the direction or valence of House and Senate roll calls. We organize our results, however, not according to these two dependent variables but rather in relation to the three mechanisms and related hypotheses discussed above.

DISRUPTIVE PROTEST

We begin by assessing the relationship between various measures and dimensions of *disruptive protest* and the pace and direction of House and Senate voting on war-related roll calls. Table 2 reports the results of various models predicting the monthly count of roll-call votes (e.g., pace), and Table 3 re-

ports the same models applied to the net directional outcome of the votes (e.g., “pro-peace” or “pro-war”).

With Hypotheses 1 and 2 we distinguish two very different ways in which disruptive protest may shape the pace and direction of Congressional roll calls. The first hypothesis rests on the notion that social movements derive their effectiveness by posing a disruptive threat to the established order. Accordingly, the more extreme the event—as measured by the presence of demonstrator violence and/or property damage—the stronger the predictive relationship with our two dependent variables. The idea is simple: Through extreme disruptions of public order, otherwise powerless groups are able to compel favorable state action motivated by the need to restore “business as usual.” The results, as reported in Models 2 and 3 in these two tables, support interesting, but opposite, conclusions. More disruptive events do indeed predict a rise in the proportion of pro-peace votes, but these same events bear a significant *negative* relationship to the overall pace of Congressional voting. Increased disruption may have inclined members of Congress to vote pro-peace, but in this case at least, it also seems to have depressed the rate at which the House and Senate considered such measures.

Hypothesis 2 posits a very different view of the link between disruption and government action. Here the spur to increased and

Table 2. Maximum-Likelihood Estimates of Poisson Regression Models Predicting the Monthly Count of War-Related Congressional Roll Calls, by Protest Events

Independent Variable	Model 1	Model 2	Model 4	Model 3	Model 5	Model 6
Intercept	-5.600** (1.183)	-3.873** (1.109)	-4.811** (1.285)	-3.015** (1.132)	-2.239* (1.082)	-3.019* (1.240)
<i>Protest Events</i>						
All events	.016 (.015)	-.003 (.037)	.018 (.013)	-.018* (.009)	-.030 (.016)	-.008 (.015)
Violence by demonstrators	—	-.122 (.069)	—	—	-.101 (.080)	—
Violence by police	—	.157** (.036)	—	—	.122** (.040)	—
Property damage	—	—	-.469** (.122)	—	—	-.355** (.130)
Injury	—	—	.770** (.085)	—	—	.613** (.091)
More than 10,000 participants	—	—	—	.613** (.078)	.566** (.078)	.362** (.082)
<i>Other Covariates</i>						
U.S. military deaths in Vietnam	-.028 (.037)	-.020 (.037)	.080 (.041)	-.012 (.041)	.018 (.041)	.062 (.042)
Call-ups in Selective Service	.003* (.001)	.000 (.002)	-.002 (.002)	.002 (.002)	.000 (.002)	.001 (.002)
Conscientious objectors	.032** (.009)	.023* (.009)	.025** (.009)	.015 (.010)	.010 (.010)	.012 (.010)
Percentage of public believing war is mistake	.087** (.023)	.068** (.022)	.083** (.023)	.048* (.023)	.040 (.022)	.052* (.023)
Published articles related to Vietnam	1.016** (.290)	1.023** (.295)	-.151 (.338)	.502 (.320)	.618 (.324)	-.152 (.342)
Period Dummy Variables: ^a						
<i>t</i> ₂	-.946 (.503)	-1.051* (.521)	-3.856** (.559)	-.996 (.538)	-1.182* (.552)	-2.766** (.584)
<i>t</i> ₃	-1.362* (.589)	-1.037 (.595)	-1.732* (.568)	-2.093** (.592)	-1.627** (.595)	-1.598** (.564)
<i>t</i> ₄	-.767 (.838)	-.777 (.860)	-.814 (.816)	.174 (.879)	.113 (.901)	.039 (.869)
-2 Log-likelihood (d.f.)	564.00 (9)	541.02 (11)	465.12 (11)	500.93 (10)	489.76 (12)	446.68 (12)
Pseudo R ²	.21	.25	.35	.30	.32	.38

Note: Numbers in parentheses are standard errors; number of observations = 97.

^a See Table 1 for definitions of period variables.

p* < .05 *p* < .01 (two-tailed tests)

more favorable state action comes not from disruption per se but from the apparent victimization of demonstrators. We test this idea by looking at the relationship of two kinds of events—those that feature violence by police and those resulting in injuries to

demonstrators—to the pace and direction of House and Senate voting. Reflecting a very different mechanism, the results of these analyses yield findings inconsistent with those reported above. Models 2 and 3 in Table 2 provide strong support for the vic-

Table 3. Maximum Likelihood Estimates of Poisson Regression Models Predicting the Monthly Pro-Peace Votes in Congress, by Protest Events

Independent Variable	Model 1	Model 2	Model 4	Model 3	Model 5	Model 6
Intercept	3.089** (.358)	3.173** (.364)	3.190** (.359)	2.539** (.385)	2.851** (.395)	2.577** (.385)
<i>Protest Events</i>						
All events	.006 (.003)	.013* (.006)	-.011 (.006)	.012** (.003)	.014* (.006)	-.005 (.006)
More than 10,000 participants	—	—	—	-.148** (.034)	-.082* (.037)	-.176** (.035)
Violence by demonstrators	—	.055* (.002)	—	—	.049* (.022)	—
Violence by police	—	-.092** (.018)	—	—	-.073** (.019)	—
Injury	—	—	.085** (.031)	—	—	.097** (.034)
Property damage	—	—	.095** (.036)	—	—	.113** (.032)
<i>Other Covariates</i>						
U.S. military deaths in Vietnam	-.077** (.012)	-.083** (.011)	-.064** (.013)	-.080** (.011)	-.083** (.011)	-.061** (.013)
Call-ups in Selective Service	.002* (.001)	.003** (.000)	.003** (.000)	.002** (.001)	.003** (.001)	.002** (.000)
Conscientious objectors	.000 (.003)	-.002 (.004)	-.001 (.003)	.005 (.004)	.001 (.004)	.001 (.004)
Published articles related to Vietnam	-.070 (.140)	-.083 (.149)	-.081 (.176)	.082 (.146)	.009 (.156)	.030 (.177)
Percentage of public believing war is mistake	.007 (.008)	-.000 (.007)	.005 (.008)	.018* (.008)	.007 (.008)	.016* (.008)
<i>Period Dummy Variables:^a</i>						
<i>t</i> ₂	.641** (.150)	.910** (.160)	.364** (.194)	.573** (.145)	.812** (.162)	.138 (.203)
<i>t</i> ₃	.363 (.199)	.517* (.204)	.274 (.211)	.376 (.199)	.495* (.206)	.219 (.212)
<i>t</i> ₄	-.028 (.305)	.488 (.315)	.116 (.313)	-.316 (.313)	.237 (.335)	-.261 (.317)
-2 Log-likelihood (d.f.)	585.17 (9)	556.19 (11)	574.24 (11)	570.36 (10)	551.34 (12)	548.48 (12)
Pseudo R ²	.15	.20	.18	.18	.21	.21

Note: Numbers in parentheses are standard errors; number of observations = 42.

^a See Table 1 for definitions of period variables.

p* < .05 *p* < .01 (two-tailed tests)

timization thesis in relation to the pace of Congressional action. So events that feature either police violence or injuries to demonstrators are, in fact, highly predictive of House and Senate roll calls on war-related measures. The same two models in Table 3,

however, report mixed results as regards the victimization thesis. The strongest finding in this regard concerns events featuring police violence: Far from aiding the movement, police violence is negatively related to pro-peace outcomes.

SIGNALING

In sharp contrast to notions of *threat* and *disruption*, Lohmann (and others) emphasize *persuasion* as the hallmark of successful movements. As discussed above, one mechanism of persuasion is Lohmann's (1993) notion of *signaling*. To operationalize the concept, we speculate, in Hypothesis 3, that large protests (e.g., ones that involve 10,000 or more demonstrators) constitute credible signals of majority support for policy change, and thus should encourage increased and more favorable government action on war-related measures. Once again, we see that the hypothesis is born out with respect to the pace of House and Senate action, but not its direction. The relevant models are Models 5 and 6 in both tables. Large demonstrations bear a strong, positive relationship to the monthly counts of roll calls, but in fact have an equally strong negative effect on the likelihood of pro-peace outcomes from those votes. So our results offer only partial support for the idea that protests, as moderate expressions of a preference for change, may shape Congressional action.

But this is only half of Lohmann's argument regarding *signaling*. The other half concerns the opposed policy positions that forms of extremist action are likely to encourage among elected officials. In contrast to Hypothesis 1, we speculate in Hypothesis 4 that protests featuring either violence by demonstrators or property damage will decrease both the pace of Congressional action and the likelihood of pro-peace voting.

We already know the results of these analyses: The more violent or extreme the events, the fewer the roll calls. There is, however, no evidence that these same *extremist signals* negatively impact the direction of Congressional action. Indeed, in Table 3 both of these proxies are positively related to the likelihood of pro-peace outcomes.

Besides contradicting the hypothesized effect of extremist *signals* on Congressional action, this latter finding sharpens the interesting contrast between *threat* and *persuasion* that appears to run through the data. *Persuasive* mechanisms (e.g., signaling) appear to have been effective in compelling increased Congressional voting, but not in

Table 4. OLS Regression Estimates of the Preferred Model Predicting the Percentage of the Public Who Believed Vietnam War Was a Mistake, 1965 to 1973

Independent Variable	Coefficient	(S.E.)
Intercept	-7.363	(5.116)
<i>Protest Events</i>		
All events	-.230	(.109)
Violence by demonstrators	1.429	(.720)
Violence by police	.533	(.372)
<i>Other Covariates</i>		
U.S. military deaths in Vietnam	.621**	(.174)
Call-ups in Selective Service	.016	(.010)
Conscientious objectors	.018	(.116)
Published articles related to Vietnam	-1.606	(1.957)

Note: Number of observations = 22; F (7, 14) = 6.56; R² = .767; adjusted R² = .650.

*p < .05 **p < .01 (two-tailed tests)

shaping favorable outcomes on those votes. In contrast, the most extreme, violent, and generally *threatening* of the protests do appear to increase the likelihood of pro-peace voting, but at the same time they depress the pace of House and Senate roll calls.

PUBLIC OPINION MOBILIZATION

What about our third and final mechanism? We sought, in our initial discussion of *public opinion mobilization*, to delineate a mechanism very different from *disruptive protest* or *signaling*. Instead of directly shaping the pace of Congressional action through the use of disruptive tactics, or a moderate signal of the public's preference for policy change, we wondered whether antiwar protesters might exert an indirect influence on House and Senate voting through the mediating mechanism of public opinion mobilization. The hypothesized mechanism involves two linked but clearly sequenced dynamics. In the first, protest activity helps to mobilize increased public opposition to the War. In turn, this shift in public opinion contributes to an increase in either Congressional attention or opposition to the war.

We begin with the link between antiwar protests and public opinion change. We offer, in Hypotheses 5 and 6, two very different characterizations of this relationship. Speculating that the perceived "victimization" of demonstrators would generate sympathy and support for the movement, we posit in Hypothesis 5 that events featuring injuries to demonstrators or violence by the police would be related to growing antiwar sentiment among the general public. In contrast, we assume, in Hypothesis 6, that it is the simple pace of protest events that shapes public opinion on the war. In Table 4 we test these two possibilities.

Before we turn to these results, however, we again call attention to a significant methodological lacuna: We are only working with 22 public opinion data points in this phase of the analysis. The best public opinion time-series concerning the war involves a single question ("... do you think the U.S. made a mistake in sending troops to fight in Vietnam?") that was asked of the American public 22 times during our study period. As a result of the small number of public opinion observations we do not have the statistical leverage to introduce many independent variables into the analysis. In general, the limited number of observations recommends considerable caution in interpreting the following results.

Now to the data at hand. In Table 4 we regress changes in the percentage of the public pronouncing Vietnam to have been a "mistake" on a handful of significant independent variables. Not surprisingly, growing public opposition to the War appears to have been powerfully shaped by the pace of the war itself—the number of deaths in Vietnam. So one of our strain measures bears a strong predictive relationship to shifts in public opinion on the war. In contrast, most of our event proxies appear to be unrelated, or only weakly related, to these same public opinion trends. (Failing to attain significance in our process of stepwise elimination, they are excluded from the model reported here.) Somewhat surprisingly, given the "victimization" thesis, this includes our measures of events with either police violence or injuries to demonstrators. There is no significant public opinion shift associated with these kinds of protest events. Even more surprising—in

light of Hypothesis 6—is the *negative* relationship (marginally significant at $p < .053$) between lagged protest events and shifts in public opinion. Clearly the growing antiwar sentiment among the general public was not responsive to the overall pace of movement activity.

There is, however, one category of protest event that may be related to public opinion trends on the issue. Protests featuring violence by demonstrators may be related (marginally significant at $p < .067$) to growing opposition to the war.⁸ This finding is consistent with the association reported in Table 3 between demonstrator violence and the outcome of House and Senate voting. How should we interpret these results? The suggestion is that protests *per se* did not affect public opinion, but that especially violent ones did, perhaps by attracting more public attention and dramatizing the growing societal rift occasioned by the war. We should not, however, take this growing public opposition to the war as any kind of endorsement of the movement. As Schuman's (1972) research shows, escalating antiwar activity was related to both an increase in opposition to the war *and* a public opinion backlash against the movement.

So much for the impact of antiwar protests on public opinion. But what of the hypothesized effect of public opinion on Congressional action? Is the growing opposition to the war positively related to either the pace or direction of House and Senate roll calls? The answer, reflected in the results in Tables 2 and 3, is, once again, mixed. While growing public opposition to the war is highly predictive of the pace of Congressional action, it is not related (or only weakly so) to the direction of voting.

DISCUSSION

In closing, we underscore what we see as the most significant implications of the results

⁸ The relationship between "events with violence by demonstrators" and "public opinion" is significant at only the $p < .10$ level. But, given how difficult it is to achieve statistical significance with only 22 data observations, we are inclined to regard this result as real and theoretically meaningful.

reported above. To do so, we return to the specific issues with which we led the article.

REFINING OUR UNDERSTANDING OF MOVEMENT OUTCOMES

With respect to the general issue of movement outcomes, our research supports a marked departure from the normal approach to the topic. We now have a substantial body of systematic empirical studies that convincingly link movement activity to outcomes of various kinds. What we do not yet have is anything approaching a theory or theories of what factors or dynamic mechanisms help account for these effects. Our focus on *disruptive protest, signaling, and public opinion mobilization* should not be mistaken for such a theory. We do, however, think that the findings related to these mechanisms are suggestive and encouraging of the more general mechanism-based approach we are advocating here. So much so that we want to underscore the potential significance of each of them.

PUBLIC OPINION MOBILIZATION—A NEGLECTED COMPONENT OF MOVEMENT DYNAMICS

Given the prominence assigned by political scientists to public opinion shift as a crucial mechanism of policy change (and the voluminous empirical evidence consistent with this view), it is incumbent on social movement analysts to pay far more attention to the impact (or lack thereof) of movement activity on public opinion change. If shifts in public opinion generally presage changes in state policy (Page and Shapiro 1983; Stimson et al. 1995), then social movements may be able to indirectly shape governmental action by stimulating changes in public opinion. We wondered whether this two-step mechanism of *public opinion mobilization* was operating during the Vietnam War era. Interestingly, only one of the two steps is clearly verified in our findings. While growing public opposition to the war is related to the pace and valence of Congressional voting, antiwar protests (with the exception of those featuring violence by demonstrators) do not appear to have been the catalyst for the public opinion change.

SIGNALING

But Lohmann's (1993) provocative conceptual work on the *signaling* function of mass movements suggests a more direct link between state action and what might be thought of as a mobilized, action-oriented proxy for public opinion. Is there evidence of this second *persuasive* mechanism in our data? The answer would appear to be no. Not only is our operational measure of moderate movement *signals* (e.g., large demonstrations) negatively related to the direction of House and Senate voting, but our proxies for extremist action (e.g., events featuring violence by demonstrators and property damage) bear a strong positive association with pro-peace outcomes.

DISRUPTIVE PROTEST

In sharp contrast to the two preceding persuasive mechanisms, some analysts have long seen *threat/disruption* as the *sine qua non* of successful movements. Seeing merit in this general argument, we nonetheless sought to distinguish between two very different mechanisms that are often lumped together under the general heading of "disruption." The first emphasizes the strategic advantage gained by movements through the use of extreme and/or violent forms of protest. The second view stresses the functional benefits of public protest that sparks violence against movement activists.

Granting qualified support to the second, or "functional victimization" view, large protests and those featuring police violence are highly predictive of increases in war-related Congressional roll calls, but surprisingly they are just as strongly *negatively* related to pro-peace outcomes. The reverse is true for the most extreme forms of movement-initiated protest. That is, those demonstrations that feature violence by demonstrators or property damage depress the pace of voting, but substantially increase the likelihood of pro-peace votes.

Whether these results are peculiar to this movement or revealing of more general links between these dimensions of disruption and various outcomes remains to be seen. The point is, by consistently attending to these dimensions of protest events, we may be

able to fine-tune our understanding of the kinds of mechanisms that shape outcomes across a good many contentious episodes.

PERSUASION VERSUS THREAT IN MOVEMENT DYNAMICS

While interesting in themselves, the results per the specific mechanisms appear to conform to a more general—and somewhat counter intuitive—pattern. Our measures of extreme threat/disruption (e.g., violence by demonstrators and property damage) are, in fact, positively related to pro-peace voting, but also to depressed rates of monthly roll calls. In contrast, our two persuasive mechanisms—signaling and public opinion shift—predict increases in roll-call votes, but little or no (or even a negative) effect on vote outcomes. Even the results concerning the “functional victimization” hypothesis can be interpreted in light of this general pattern. Courting violence by one’s opponents can be seen as an extreme form of persuasion—an effort to curry favor by mobilizing sympathy on behalf of the movement. Interpreted in this way, these particular results again fit the general pattern. As with our other persuasive mechanisms, violence by police and/or injuries to demonstrators increase the pace of roll-call voting, without improving the chances of pro-peace outcomes.

This general pattern underscores the daunting strategic dilemmas that social movements confront in their efforts to shape outcomes and the need of movement analysts to attend much more closely to the dynamic relationships among tactics, targets, and the ways in which certain outcomes (e.g., the successful courting of media attention through victimization) may preclude others (policymaker support for policy change). In the case of the U.S. antiwar movement, it appears that certain forms of protest were effective in compelling Congressional action (e.g., large demonstrations, those involving police violence, etc.) but not in shaping the outcome of that action, while others (violence by demonstrators) had exactly opposite effects. Only by attending to the variable patterning of such findings can we begin to understand the general dilemmas confronting social movements and the

generic mechanisms on which these dilemmas may turn.

TOWARD A MORE SYSTEMATIC UNDERSTANDING OF THE “WAR AT HOME”

Besides the general theoretical and methodological goals we established at the outset of this article, we are also concerned with shedding light on a contentious episode—the Vietnam era antiwar movement—that has been oddly neglected by social-movement scholars. From this perspective, we think our findings provide an interesting, systematic confirmation of what many people experienced during the peak period of domestic contention over the Vietnam War. We conclude by relating two of our findings to the lived experience of those years.

First, reviled though it was by many, the antiwar movement nonetheless exerted a powerful agenda-setting force on the federal government and U.S. society more generally. As our results show, antiwar protests—especially those of a large or injurious nature—compelled public and Congressional attention. That said, our data also clearly underscore the limits of what the movement was able to achieve in relation to U.S. policy in Vietnam. Although extreme forms of public protest may have helped shift public opinion against the war, the protests themselves had contradictory effects on the direction of House and Senate voting. Certain dimensions or forms of protest (e.g., large demonstrations, violence by police, etc.) appear to decrease the likelihood of pro-peace outcomes. Finally, in their earlier groundbreaking work, Burstein and Freudenburg (1978) showed that whatever effect antiwar protests had was confined to the period prior to 1970.

These results provide statistical confirmation of what analysts and activists experienced during the period. Drawing on the perceived lessons of the civil rights struggle, antiwar activists were motivated by an implicit understanding of, and faith in, conventional democratic theory. Educate the public about the evils of the war and mobilize and demonstrate that growing opposition, and eventually policymakers would be persuaded to modify their actions and bring the

conflict to a close. Or so antiwar activists believed. As our results confirm, this expected responsiveness to popular protest and public opinion was only partly realized. The kind of extreme tactics that attracted media attention, shaped public opinion, and influenced Congressional action also depressed the overall rate of House and Senate voting. *Threat/disruption* may have shaped certain vote outcomes, but it did so at the cost of a generalized backlash in Congress.

Why didn't the "politics of protest" deliver the gains achieved by, say, the civil rights movement, whose tactical repertoire also mixed *threat* and *persuasion*? Perhaps because the antiwar movement never mobilized anything like the general public support and sympathy that the *early* civil rights struggle achieved. In turn, we view this general antipathy to the movement as a by-product of its perceived lack of commitment to democratic practices and the general politics of *persuasion*. Indeed, when in the late 1960s, many black activists abandoned—or appeared to abandon—nonviolence, policymakers were less consistently responsive to the movement than they had been in the earlier civil rights phase of the struggle.

This observation motivates us to close by speculating on the paradoxical nature of politics in the United States and the peculiar strategic challenge it poses to movements. To be maximally effective, movements must be *disruptive/threatening*, while nonetheless appearing to conform to a democratic politics of *persuasion*. Democratic theory notwithstanding, threat and disruption (and even violence) have been effective means of mobilizing power in the United States, but typically not when practiced by groups perceived as antidemocratic. The early civil rights movement mastered this strategic sleight of hand. The antiwar movement never did, producing the odd mix of agenda setting impact and minimal policy responsiveness reflected in our results.

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