Discursive Opportunities and the Evolution of Right-Wing Violence in Germany[1]

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This article explores the link between violence and public discourse. It suggests that media attention to radical right violence and public reactions to violence affect the clustering of targets and the temporal and spatial distribution of violence. The notion of "discursive opportunities" is introduced, and the article argues that it can serve to link political opportunity structure and framing perspectives on collective action. Using a cross-sectional and time-series design to model event counts in states in Germany, this study finds that differential public visibility, resonance, and legitimacy of right-wing violence significantly affected the rate of violence against different target groups.

Since the reunification of Germany in 1990, a wave of radical right violence has killed over 100 persons and wounded thousands (see, e.g., Björgo and Witte 1993; Kurthen, Bergmann, and Erb 1997; Ohlemacher 1994). Immigrants, Jewish synagogues and cemeteries, memorials to World War II and the Holocaust, left-wing groups, handicapped persons, gays, and the homeless were targeted by this violence. Despite this heterogeneity, by the mid-1990s asylum seekers and immigrants predominated as victims. In Germany, the United States, and elsewhere, previous explanations

of this type of violence have often emphasized the socioeconomic sources of violence, including unemployment, poverty, and factors increasing ethnic competition between natives and immigrants. These perspectives have not proven wholly satisfactory, because they have received ambiguous empirical support (Wimmer 1997; but see Lubbers and Scheepers 2001) and because they have been unable to explain why some minority groups are targeted more often than others (Olzak 1992; but see Olzak and Shanahan 2003).

In this article, we address these shortcomings by building on existing theories of collective action and media influence to suggest how public discourse provides opportunities for mobilization. Our study explores and tests arguments that public discourse significantly shapes the targets and the temporal and spatial patterns of radical right violence in Germany. A bridge is built between two theoretical perspectives in social movement theory, political opportunity structure and framing perspectives, and theoretical consequences are drawn from the critical observation that political opportunity structures (hereafter POS) affect movement action only when they are perceived as such by (potential) movement activists (Gamson and Meyer 1996). In contrast, framing theory emphasizes the internal perspective of movements' own meaning-making strategies. Thus, framing theories have difficulty in explaining why some such strategies meet with favorable responses while others do not. Our goal is to push this debate forward by assessing the role of public discourse in producing, amplifying, and dampening rates of ethnic violence.

In the public sphere, movement activists communicate messages to fellow activists and potential adherents, and they thereby gain crucial information about the actions and reactions of authorities, political opponents, allies, and sympathizers. To capture this role of the public sphere, we develop the notion of *discursive opportunities*. We argue that media attention to radical right violence, public reactions by third actors to radical right violence, and public controversies surrounding the targets of such violence can encourage or discourage violent acts in a number of ways. We distinguish three elements of discursive opportunity—differential public visibility, resonance, and legitimacy—that amplify the rate of some types of violence while diminishing or leaving unaffected the rate of other types.

EXISTING EXPLANATIONS OF ETHNIC VIOLENCE

Debates on ethnic violence in Germany have long argued that socioeconomic deprivation and disintegration of community ties are primary causes of radical right violence (e.g., Heitmeyer et al. 1992; Krell, Nicklas,

199

and Ostermann 1996; McLaren 1999).[2] This familiar argument holds that under worsening economic conditions social groups threatened with marginalization designate specific racial or ethnic minorities as responsible and therefore worthy of exclusion and violence. The socioeconomic situation in Germany after reunification lends credibility to this argument. The merger of East and West Germany has so far failed to produce the "flowering landscapes" promised on the eve of reunification but has instead precipitated severe economic problems. Germany now finds itself below the European Union average on just about any indicator of socioeconomic performance.

However plausible these deprivation accounts are, detailed investigations at the individual level have not provided support for them. For instance, Helmut Willems and his collaborators (1993) found that the perpetrators of radical right violence tended to be fairly average young people from normal family backgrounds who were not significantly more likely to be unemployed than others among their age group. The trajectory of radical right mobilization shows no temporal overlap with trends in socioeconomic development such as economic growth, unemployment, or inflation (Koopmans 2001).

Ethnic competition theorists (e.g., Barth 1969; Olzak 1992; Myers 1997; Nagel 1996) provide an alternative explanation for ethnic conflict, suggesting that competition among racially or ethnically differentiated groups for the same resources releases forces of competitive exclusion, which in turn engenders conflict. Competition theorists have argued that competition need not be objective (Carroll and Hannan 2000). In this view, a high influx of immigrants into a formerly homogeneous region may increase subjective perceptions of increased ethnic competition (even if perceptions are not justified; Bélanger and Pinard 1991; Scheepers et al. 2002). There is scattered research from Germany showing that supporters of the radical right complain that foreigners take away "German" jobs and profit unreasonably from the German social security system, that they are a threat to "German" cultural values, and that—a view especially widespread among the young—"they are after our women" (Willems et al. 1993; Bergmann and Erb 1994). Although perceptions of relative deprivation and ethnic competition may therefore seem relevant, they have not proven to be sufficient conditions for violent mobilization. Thus, we raise two questions: under what conditions will such feelings and perceptions arise and why is hostility directed against certain outsider groups and not against others? To answer such questions, it is necessary to turn to the political and cultural context in which radical right mobilization occurs.

[2] See also Falter, Jaschke, and Winkler (1996) and Kowalsky and Schroeder (1994).

200

POLITICAL OPPORTUNITY STRUCTURES

The concept of POS (see, e.g., McAdam 1999; Tarrow 1994; Kriesi et al. 1995) has gained widespread popularity in the literature on social movements and collective action. The basic idea is that the capacity to mobilize depends on opportunities and constraints offered by the political-institutional setting in which collective action takes place. There is little agreement about indicators, but proponents of POS theory often include measures of elite division, electoral competition, electoral instability, the composition of government, and the state's capacity for repression (McAdam 1996).

Gamson and Meyer (1996) observe that political opportunities must be perceived and are subject to interpretation or framing before they can effectively influence movement activists' decisions (see also Goodwin and Jasper 1999). Yet this amendment to POS raises the questions of why certain perceptions and interpretations of political reality spread (while others do not) and why certain actors may effectively succeed in opening new windows of opportunity (when most do not). We suggest that the public sphere mediates between political opportunity structures and movement action. Most people, including most activists, are not full-time political analysts who closely follow and gather independent information on what is going on in the corridors of power, and who have an intimate knowledge of the institutional intricacies of the political system. What most people know about politics comes from the media. POS variables such as electoral instability, elite divisions, or availability of elite allies have no meaning if people do not become aware of them. For most people, such awareness comes from the limited information about political statements, actions, and events that is made public. Just as protests that receive no media coverage at all are, as Gamson and Wolfsfeld (1993, p. 116) call them, "nonevents," regime weaknesses and openings that do not become publicly visible may be considered "nonopportunities," which for all practical intents and purposes might as well not exist at all. In the next section, we develop the notion of "discursive opportunities," to denote those opportunities and constraints that become publicly visible and that can thereby affect mobilization.

How do right-wing activists learn of these opportunities? The perpetrators of radical right violence are young people with generally low levels of interest in institutional politics (Willems et al. 1993). Therefore, it is unrealistic to assume that radical right activists follow developments in politics closely in order to rationally calculate chances of success. Instead we assume that radical right activists learn in a trial-and-error fashion about the efficacy of different mobilization strategies by gauging the public reactions their actions provoke (or fail to provoke). Through the mass

201

media, radical right activists not only learn about their own failures and successes but also gain information about the results of actions undertaken by other activists. In this way, successful strategies are adopted and replicated. Thus, while we do not assume that radical right activists are people who closely read and watch political news, we follow other researchers in assuming that movement activists have a keen interest in following the reactions in the media to their own actions and see media coverage and political response as a measure of success (e.g., Molotch 1979, p. 72; Kielbowicz and Scherer 1986, pp. 84–85; Gitlin 1980).

DISCURSIVE OPPORTUNITIES

Diffusion processes influence ethnic violence, race riots, and protest cycles in general (see, e.g., Tarrow 1989; Koopmans 1993; Myers 2000; Olzak and Shanahan 1996). Yet the theoretical implications of this highly consistent finding have not been sufficiently explored with respect to public discourse in the mass media. For diffusion to occur, channels of communication are necessary, and, at least in modern democracies, the mass media occupy a central role in this regard. The recent rise of protest event analysis as a methodological tool for social movement studies has sensitized researchers to the dependency of protest on media attention (Mueller 1997; Rucht, Koopmans, and Neidhardt 1999). In the age of mass communication, protests that are completely ignored by the media are unlikely to diffuse to wider constituencies or have an impact on policies.[3]

Only a minority of all attempts at public claim making receive the media attention necessary for widespread recognition. We define *discursive opportunities* as the aspects of the public discourse that determine a message's chances of diffusion in the public sphere (Koopmans and Statham 1999*a*; see also Ferree 2003). Our argument starts from the assumption that the public sphere is a bounded space for political communication characterized by a high level of competition (see Hilgartner and Bosk [1988] for a similar argument). To be sure, the boundaries of the public sphere are not fixed but expand and contract over time (for instance, consider the increasing numbers of channels of communication such as the Internet or the multiplication of existing ones through cable and satellite television). The scope of media attention may also be affected by short-term trends, such as the media's greater attention to political

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[3] Diffusion also happens in the absence of mass media attention, by way of a movement's indigenous channels of communication, e.g., interpersonal networks or movement media. However, we would argue that the scope of interpersonal diffusion is more limited.

topics during periods just prior to an election than during times of routine politics.

On a typical day in a medium-sized democratic society, thousands of press statements are issued, hundreds of demonstrations, pickets, and other protests are staged, thousands of individuals write letters to the media or call in on radio and television programs, and dozens of press conferences vie for the attention of the public. The number of channels of communication (newspapers, magazines, radio stations, television networks, and so forth) and the size of their respective news holes (pages, broadcasting time) act as constraints on various inputs. Thus, the media have a finite carrying capacity at any point in time.

Visibility

The discrepancy between the available space in the public sphere and the much larger supply of messages implies that there is competition among groups who aim to get their messages across in the public discourse. To understand these dynamics, we need to distinguish two categories of actors: the *gatekeepers* of the public discourse, on the one hand, and the *claim makers* that appear as speakers in the media, on the other (Neidhardt 1994; Koopmans and Statham 1999*b*). The gatekeepers of public discourse are the editors and journalists who have the ability to select, shape, amplify, or diminish public messages. The selectivity of coverage and the mechanisms of allocating prominence to covered messages are quite well known for the traditional mass media and include decisions about the size and placement of articles or about the amount and primacy of airing time. The actions of gatekeepers produce the first and most basic type of discursive opportunity that we can distinguish: *visibility*. Visibility depends on the number of communicative channels by which a message is included and the prominence of such inclusion. Visibility is a necessary condition for a message to influence the public discourse, and, other things being equal, the amount of visibility that gatekeepers allocate to a message increases its potential to diffuse further in the public sphere (see Trouillot 1995; Schudson 1995).

From communications and media research we know that "news values" of reporters and editors shape decisions that make a given story newsworthy. For instance, (geographical) proximity, the prominence and prestige of the speaker, and the level of violence and/or conflict, possibilities for dramatization and personalization, and the novelty of a story all influence the likelihood of its being reported in newspaper accounts (Galtung and Ruge 1965; Schulz 1997; McCarthy, McPhail, and Smith 1996; Mueller 1997; Hug and Wisler 1998; Oliver and Myers 1999; Oliver and Maney 2000). Yet, with the partial exception of proximity, news values

203

are not objective in the sense that these characteristics of events, actors, or messages exist outside of and prior to the discursive realm. Notions of who is considered to be prominent and which issues are considered relevant or controversial have emerged from previous rounds of public discourse. They are social products that serve as a lens through which the vast array of events in public and private life are observed and on the basis of which a small proportion of these events are selected for coverage. Social movement organizers and other public actors anticipate these media selection mechanisms. Thus, many modern protests, including Greenpeace-style professional organizations involved in direct action, are to an important extent scripted and staged to maximize the chances of drawing media attention (Kielbowicz and Scherer 1986; Ryan 1991).

Resonance and Legitimacy

While the relevance of media coverage has been widely acknowledged, less attention has been paid to the fact that the diffusion chances of a given actor's messages also depend on how other, nonmedia claim makers relate to them in the public sphere (but see Ellingson 1995; Steinberg 1999). Other speakers may publicly express support for a movement's actions, or they may react with indignation and rejection to messages that challenge their own position in the public discourse. Sometimes public actors choose to ignore social movement actors in an attempt to deny them the attention crucial for replication. We envision the communication environment of any particular public actor as the source of two further types of discursive opportunity: *resonance* and *legitimacy*. In developing these concepts we have been inspired by the work on collective action frames of David Snow and his colleagues (e.g., Snow et al. 1986; Snow and Benford 1992; see also Gamson and Modigliani 1989). Our focus, however, is on the (often unanticipated) external reactions that radical right mobilization encounters rather than on the framing strategies of movement activists themselves.

Although gaining visibility is a necessary condition for communicative impact, the career of a discursive message is likely to remain stillborn if it does not succeed in provoking reactions from other actors in the public sphere. We refer to this dimension as *resonance*. Resonance has two types of ripple effects. First, resonance enhances reproduction of a message, because, in the eyes of journalists and editors, the message has become more relevant and the actors articulating the message seem more "prominent." Second, messages that resonate travel farther. Through the reactions of other claim makers, the message of the original speaker is at least partially reproduced and may reach new audiences. This happens if established political actors express support for a social movement's actions

204

or demands. This form of supportive resonance we will call *consonance*. However, for movements as for other public actors, even negative resonance, or *dissonance*, is often preferable to no resonance at all (Molotch 1979, p. 72). The rejection of a demand signals its relevance to other actors. Moreover, even a strongly negative public reaction has to reproduce the original message to at least some extent and thereby always runs the risk of providing potential imitators with a model for successful public action (e.g., Holden 1986 for the case of airplane hijackings).

Thus far we have treated consonance and dissonance as having similarly positive effects on the discursive opportunities of a message. Yet it might matter whether there are more negative or positive responses in the public sphere. We define public *legitimacy* as the degree to which, on average, reactions by third actors in the public sphere support an actor's claims more than they reject them. Defined in this way, legitimacy can vary independently of resonance. Highly legitimate messages may have no resonance at all because they are uncontroversial, while highly illegitimate messages may have strong resonance (e.g., for obvious historical reasons, anti-Semitic violence in Germany). The predicted effects of legitimacy on a message's diffusion chances are complicated. All other things being equal, one might expect legitimacy to have a positive effect on diffusion, because it signals agreement with a movement's position. But things will rarely be equal. Ideally, speakers would prefer their messages to have high resonance and high legitimacy, but they usually will have to settle for less. This is because high resonance is often only achieved at the cost of an increase in controversy, which results in a net decrease in legitimacy. Conversely, highly legitimate statements usually provoke few reactions from other claim makers, and the media will not be interested in endlessly repeating messages that are accepted by everybody.[4] This discussion leads us to expect a curvilinear relation between chances of diffusion and legitimacy, with messages whose legitimacy is controversial generally better positioned for replication.

Before moving to the empirical analysis, we wish to clarify that we do not want to be interpreted as presenting a purely mechanical (and unrealistic) argument suggesting that public discourse simply causes ethnic violence. Furthermore, we are aware that our insistence on a connection between public discourse in the mass media and radical right and racist violence is not wholly original (see van Dijk 1993; Jäger and Link 1993). While we share the assumption with earlier scholars that a connection between public discourse and racist violence exists, the mechanisms we

[4] Social movement research illustrates the point that tactics seen as disruptive, violent, or innovative will raise rates of protest (McAdam 1983), but only up to a point (Olzak and Uhrig 2001).

offer do not require a direct causal linkage between elite discourses and popular racism. In our view, the public discourse in the mass media affects radical right mobilization not by planting negative stereotypes in activists' heads but by acting as a dynamic selection process that differentially affects the diffusion chances of different types of radical right mobilization. The public visibility and resonance of violence against a particular target group may increase because the position of this group is hotly debated in the public discourse. As a result, the diffusion chances of violence against the target will improve, even if nobody in the public debate refers to the target group by taking an explicitly negative stand.

DATA, VARIABLES, AND HYPOTHESES

We collected and analyzed information on violence by radical right and xenophobic groups and on public discourse on immigration and ethnic relations from newspaper and official police reports. We include statements on immigration control and legislation, as well as all claims by, against, or on behalf of radical right and ethnic minority groups. The units of analysis are not articles, as is often the case in media content analysis, but are claims made by nonmedia actors. Such claims include public statements, interviews, and press conferences as well as political decisions, judicial actions, demonstrations, and violence.

From our newspaper sources, we gathered information on 11,204 instances of claim making during the period 1990–99. Among these, we identified 930 instances of radical right violent attacks as one of our two measures of right-wing violence (the other is from official police reports). An example of a right-wing violent event is captured with this excerpt: "A crowd of 200 local youth shouting 'foreigners out!' and throwing stones last night attacked a hostel for foreign workers in Hoyerswerda." We use reports of public discourse expressing claims on different categories of immigrants or on the radical right to calculate measures of visibility, consonance, dissonance, and legitimacy (discussed below). The following report illustrates a claim that was included in our consonance measure because it expresses a negative opinion toward one of the radical right's target groups: "In a television interview yesterday, Chancellor Kohl said that the strong rise in the numbers of asylum seekers has taken the form of a state crisis." The following is an example of a dissonant claim, which expresses a negative attitude toward the radical right: "Federal President von Weizsäcker condemned the arson attack on the former concentration camp at Sachsenhausen, saying this is an outrageous act that brings shame on Germany."

The data were coded from all Monday, Wednesday, and Friday issues

206

of the national newspaper *Frankfurter Rundschau* between 1990 and 1999.[5] This newspaper was chosen because pretests indicated that it paid more attention to the topics of interest than did alternative sources. For shorter periods of time, samples were drawn from other newspaper sources to check the representativeness of the primary source for the wider media landscape. These other newspapers were the national tabloid newspaper *Bild-Zeitung*, the Turkish immigrant daily *Hürriyet*, as well as three East German local newspapers. Comparisons of these newspapers displayed a consistent pattern. First, in any paired comparison, the *Frankfurter Rundschau* was by far the more inclusive source in terms of the number of claims reported. Second, these quantitative differences had only very small qualitative consequences.[6] For instance, although the *Rundschau* reported more than four times as many claims as *Bild-Zeitung* did, the distributions of claims across actors, issues, and positions with regard to issues were almost the same. This indicates that the *Frankfurter Rundschau* can be considered representative for the wider German media landscape, at least regarding the type of information that we use for our analysis.[7]

We are well aware of the problem of selection bias that affects the use of newspaper data for many research purposes (e.g., McCarthy et al. 1996; Barranco and Wisler 1999; Oliver and Maney 2000). However, in this article we are primarily interested in positive and negative feedback relations between different types of claims *within* the public discourse, as represented in the media. Regarding our dependent variable—radical right violence—we are of course also interested in analyzing to what extent public discourse dynamics affect the rhythm, location, and targets of radical right violence outside the media. Here we are fortunate that the German Federal Office for the Protection of the Constitution (Bundesamt für Verfassungsschutz) publishes statistics on this type of violence that are based on police statistics gathered in the different federal states. The correlation coefficient between radical right violence in our newspaper data and in the official statistics is .53.

The newspaper and police sources did not vary much regarding the

[5] Trained coding assistants used a standardized codebook to identify and code all relevant claim-making activity from newspaper sources (available on request from the first author). Researchers did not rely on a set of keywords but instead reviewed all sections of the papers for relevant articles. Comparisons across six coders yielded $\alpha = .92$.

[6] A more specialized audience will naturally shape the type of events addressed by a paper. Thus, *Hürriyet* obviously reported more claims by Turkish organizations, while regional papers reported more claims made in their own region.

[7] An additional coding of editorials revealed important qualitative differences. For instance, the *Bild* editorials were more in favor of restricting immigration than those of the *Rundschau*. To minimize the influence of editorial bias, we exclude all editorials and opinion pieces.

207

distribution of events over time, but they vary substantially across the federal states. These differences are not, as one might have expected, determined by geographical distance: the West-based *Frankfurter Rundschau* reported a larger proportion of Eastern events than did official police statistics. It is difficult to ascertain precisely what causes this difference, but it may be due to a tendency by Eastern police to minimize right-wing violence. Over the 1990s, there were continuous complaints about the lack of attention of Eastern police to radical right violence. To capture some sources of systematic bias, we conduct two separate analyses: first we use data on right-wing violence gathered from police statistics, and, second, we analyze data on events from newspaper sources. For all other measures relevant to economic hardship, competition, and extradiscursive political opportunity structures, we draw on data from the Federal Statistics Office (Statistisches Bundesamt).

Unit of Analysis: Federal States in Germany across Time

All variables, whether drawn from official statistics or from our newspaper data, are aggregated by year and by federal state to construct a cross-sectional time series data set with 160 cases (16 federal states for the 10-year period 1990–99). Variables thus consist of counts of claims of a specific type (e.g., radical right violence) per year-state combination, year-state averages for variables such as unemployment, or year-state totals as in the case of immigration levels. Thus, we seek to explain variation across states and over time in a panel design.

   The choice for this level of analysis was made for a number of reasons. First and importantly, this is the only level for which police statistics on radical right violence are available.[8] Second, below the state level, different official institutions sometimes use different spatial units to gather statistical information. The most often used unit is the *Kreis* (county), but some important data, such as unemployment levels, are gathered on the basis of different spatial units (namely the areas of responsibility of Labor Offices), which only partly coincide with Kreis boundaries. Third, during the 1990s the number of *Kreise* in East Germany was strongly reduced, not only by aggregating existing units but also in many cases by drawing completely new boundaries. As a result, continuous time series at the Kreis level are available for East Germany only from 1994–95 onward, when the reform of Kreis boundaries had been completed. This means effectively that no consistent data are available on the substate

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[8] The Federal Office for the Protection of the Constitution also publishes *monthly* statistics on radical right violence, but these are not differentiated for the 16 states.

208

level for the region and period in which radical right violence dispro-
portionately occurred, namely East Germany before 1994.

Why not analyze newspaper data (where the locations are specified) at
the local level of analysis? It turns out that there are also difficulties
applying information from newspaper sources to a lower level of spatial
aggregation. As indicated above, we have 930 cases of radical right vi-
olence in our newspaper data. Practically, this means that using an ap-
propriate "at risk" sample of local units (to avoid sample selection bias)
would not be feasible, given the enormous number of zero events for the
majority of smaller units over most time periods.

Dependent Variable

Our choice of the number of radical right violent events as our dependent
variable also requires some clarification. Pragmatically, this choice is dic-
tated by the police statistics, which do not record numbers of participants
in violence and do not provide reliable information on nonviolent events.
Because linking media and extramedia data is crucial to our theoretical
argument, we prefer to focus on numbers of radical right violent events.
As it turns out, violence was by far a favored tactic in the radical right's
action repertoire, accounting for 70% of all radical right protests in our
newspaper data. Our data suggest that the relevance of the radical right
was driven by the frequency and intensity of the violence it produced,
rather than by the small numbers of participants at these events.[9]

Techniques for Estimating Event Counts in Panel Data

Our events are arrayed as panel data. We expect that the disturbance
process (i.e., error terms that are correlated within states across time) will
be correlated across observations due to gradually changing but unob-
served characteristics within states. Moreover, we assume that autocor-
relation processes will be strongest in adjacent years and less correlated
in distant periods. We experimented with several specifications of the
correlation matrix of these unobserved correlations and found, consistent
with other panel models of collective action, that a first-order autocor-
relation specification provided a relatively good fit with the data, when

[9] The total number of participants in all radical right violent events in our data set
across the whole 10-year period and all 16 states amounts to only about 17,000 (i.e.,
slightly more than 30 participants on average per event), but this probably includes
sequential participation by the same set of activists. For about 40% of the events,
exact numbers of participants are missing, which precludes any systematic analysis of
event size.

209

compared with other possible specifications (including random effects models and models of unconstrained correlated errors).

We used an estimation procedure appropriate for analysis of event counts, the method of generalized estimation equations (using the XTGEE routine in STATA, ver. 7). Because variables consist of nonnegative counts with overdispersion, a negative binomial distribution for the dependent variable was modeled, as well as a first-order autoregressive correlation structure, which is typical for time series (King 1989). The first part of our analysis focuses on explaining the volume of radical right violence and uses police statistics to calculate the dependent variable. The second step focuses on explaining the targets of violence, using newspaper data on radical right violence (the police data cannot be differentiated according to the target of violence).

Operationalizations and Hypotheses

The following independent variables—all measured for each year-state combination separately and lagged one year relative to the dependent variable—are used in the analyses.

Measures of socioeconomic deprivation.—We use the state-level gross domestic product on a per capita basis and yearly changes in the state-level unemployment rate.[10] The hypothesis derived from socioeconomic deprivation accounts is that low levels of the gross domestic product and strong increases in the unemployment rate should be associated with high levels of radical right violence.

Measures of ethnic competition.—These are net immigration to a state from outside Germany per 1,000 inhabitants (number of immigrants from abroad minus number of emigrants to other countries) and the interaction term between net immigration and yearly changes in the unemployment rate.[11] The ethnic competition model predicts that immigration and unemployment and their interaction will have strong, positive effects on the level of radical right violence.

Measures of extradiscursive political opportunity structures.—The political complexion of the state government is measured on a right-left scale

[10] We considered both the rate itself and yearly changes in preliminary analyses, but the results were more robust in the models using yearly changes and so we report these results.

[11] In preliminary analyses, we also considered the gross number of immigrants, uncorrected for emigration, which performed less well. Moreover, the net immigration variable more accurately indicates potential competition pressures as a result of migration. In addition, we investigated whether there was any effect from migration flows within Germany, which was not the case. Finally, an alternative measure of immigration, foreigners as a percentage of the population, had no effect.

210

ranging from "1" (if the right-wing Christian Democrats [CDU] ruled alone) to "6" (for left-wing coalitions of Social Democrats [SPD] and Greens).[12] Years in which a change of government occurred were coded according to the government that was in power for the largest part of the year. We use the absolute difference between the percentage of CDU votes (or Christian Social [CSU] in Bavaria) and SPD votes in state-level elections as a measure of the degree of electoral competition between the two major parties; the scores were then given a negative sign so that high (i.e., close to zero) scores on this variable indicate that both parties capture comparable shares of the vote, which will generally imply a higher level of competition than when one of the two parties clearly dominates.[13]

Studies have shown that the levels of extrainstitutional protest by distinctly political social movements are inversely related to the presence of allies in positions of power within established politics (Kriesi et al. 1995). This is because political allies in office will be less inclined to support extrainstitutional action, and there is less need for social movements to resort to extrainstitutional pressure when political friends are in power. This implies that we expect the radical right to mobilize less under right-wing governments. Political opportunity theorists further stress the importance of elite conflict and competition, which open up opportunities for social movements to intervene in the political process (Tarrow 1994). Therefore, we expect the level of violence to be higher where the two main political parties are in close competition with one another.

Measures of discursive opportunities.—To measure visibility, we exploit the fact that we have both media data and police data on radical right violence (the police data are more inclusive). In addition, among the newspaper-reported radical right violent events, we can distinguish between those that were reported on the front page and those that were reported less prominently. Combining these, we have two indicators of visibility: (1) front-page violence as a percentage of police-registered violence and (2) front-page violence as a percentage of all newspaper-reported violence. The first measure of visibility will be used in analyses using the police data as the dependent variable, the second in analyses using newspaper-reported violence as the dependent variable.[14] Dissonance is measured by all claims directed against the radical right and

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[12] The other codes used were "2" for a government of CDU and the centrist Liberal Democrats (FDP), "3" for coalitions of CDU and SPD, "4" for coalitions of SPD and FDP, and "5" for the SPD ruling alone.

[13] For years in between elections, the vote percentages were interpolated.

[14] We also aggregated all counts of media-reported violence (front-page or not) as a percentage of police-registered violence for a third measure of visibility. As anticipated, this alternative indicator of visibility had similar but slightly weaker effects.

211

xenophobia, including a wide range of forms such as public condemnations and countermobilization, as well as state repression against the radical right. Consonance (with the radical right) is measured by all claims directed against immigrants and minorities, mostly in the form of public statements. In order to keep dependent and independent variables separate, our consonance measure excludes any anti-immigrant claims that were made by radical right organizations, spokespersons, or groups. Finally, legitimacy is measured by the share of consonant claims among all claims on the radical right, immigrants, and minorities. Here, too, we exclude claims made by radical right actors.

In line with the theoretical arguments outlined above, high levels of visibility, consonance, and legitimacy are expected to lead to higher levels of violence. As we have argued, high levels of legitimacy may dampen rates of protest when a movement's claims (or tactics) become seen as uncontroversial and uncontested and thereby fail to gain media attention. However, given the position of the radical right at the margins of the German polity, this argument makes less sense. The expectation with regard to dissonance is less clear: on the one hand, dissonant claims may further contribute to diffusion of violence by the (unintended) publicity that they give it; on the other hand, dissonance also undermines the legitimacy of violence and signals the mobilization of countermovements and popular rejection of the radical right.

Control variables.—The dummy variable East is one control variable. The former regions of East and West Germany are still very different in many respects. This variable measures whether there is a difference in the level of radical right violence between the East and the West that cannot be explained by the other variables in the model. Net of all other explanatory variables, based on its history of ethnic homogeneity and lack of a democratic tradition, we expect a positive effect of Eastern location on the level of radical right violence. The second control variable is the natural logarithm of the state population in the thousands. Since the dependent variable is a count of instances of radical right violence, it is of course likely to depend on the population size of a given state. Finally, we have the dependent variable lagged one year, which captures diffusion processes unexplained by the other variables in the model.

RESULTS

Explaining the Level of Radical Right Violence

Figure 1 compares the total number of radical right violent events from the official police data with the number from newspaper sources for Germany during 1990–99. While the number of events in police reports (on
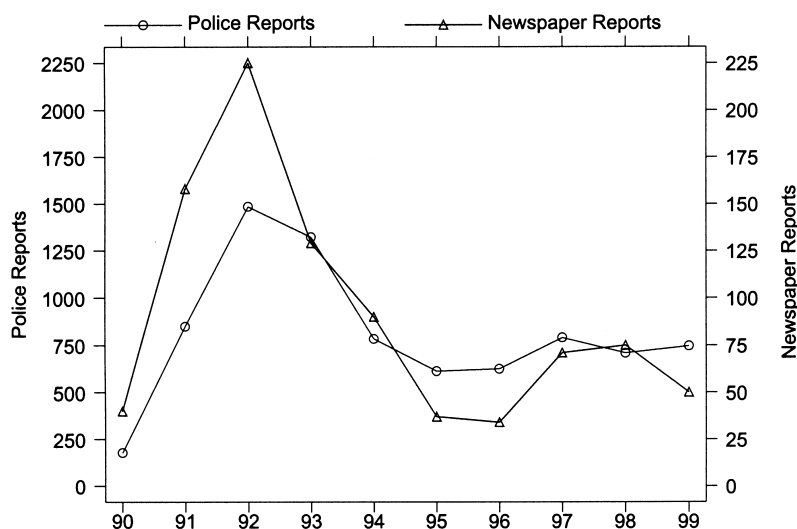
212

Fig. 1.—Total number of right radical violent events in Germany during 1990–99, newspaper reports compared with police data.

the left-hand scale) is about 10 times higher than the number of newspaper events (using the right-hand scale), the peaks and valleys are strikingly similar. From 1990 to 1992 we see a steep increase in violence, with the police reports numbering nearly 1,500; these then decline until about 1995–96. In the final years of the decade, we see again a slight increase in the number of events reported, both by the police and by the newspaper. In addition to these fluctuations over time, there was considerable variation among the federal states. In absolute numbers, the highest levels were recorded in Northrhine-Westphalia (an average of 165 yearly events according to the police data) and the lowest in Bremen (six yearly events). Since these also happen to be the most and least populous states, it is more illuminating to compare per capita rates of violence. Per million inhabitants, the average yearly number of radical right violent events ranged from 29 in the Eastern states of Brandenburg and Mecklenburg-Vorpommern to three in Bavaria in the West. Controlling for population size, the rate is clearly higher in the East than in the West. Berlin, in line with its mixed East and West roots, displays an intermediate violence level (15 yearly events per million inhabitants).

To what extent can these temporal and regional differences be explained with traditional theories of ethnic violence? Table 1 compares a baseline model including only past violence levels, population size, and Eastern location to deprivation and ethnic competition models. As the table shows,

213

TABLE 1
GENERALIZED ESTIMATION EQUATION EFFECTS OF DEPRIVATION AND ETHNIC
COMPETITION ON LEVELS OF RADICAL-RIGHT VIOLENCE

	BASELINE MODEL		DEPRIVATION MODEL		ETHNIC COMPETITION MODEL	
	B	Z-Score	B	Z-Score	B	Z-Score
Radical right violence[a]	.004***	3.52	.004**	3.05	.005***	3.67
Log population size	.693***	5.82	.686***	5.92	.676***	6.17
Location in East Germany	.669***	4.12	.773**	3.12	1.01***	3.93
Per capita domestic product (in Euros)			.006	.30	.018	1.23
Unemployment rate			−.042	1.11	−.036	0.75
Net immigration/ 1,000					.029***	3.96
Interaction of unemployment and immigration					−.001	.00
Wald χ^2	143***		124***		189***	
N[b]	148		148		148	

NOTE.—The table refers to Germany in 1990–99. B = unstandardized regression coefficient.
[a] All independent variables (including the lagged dependent variable) are lagged one year.
[b] Missing data left us with 148 state-year combinations with full information.
[+] $P < .10$.
[*] $P < .05$.
[**] $P < .01$.
[***] $P < .001$.

the deprivation model performs poorly. This finding is consistent with results of many earlier investigations of ethnic violence. In spite of stark differences in economic and social conditions in East and West Germany, the gross domestic product (measured at the state level) has no effect on radical right violence. Perhaps most surprisingly (given prior theories), changes in the unemployment level have no impact on violence. The available information for the substate level does not indicate a connection between unemployment and radical right violence, either. Well-known hot spots of radical right violence such as Hoyerswerda, Rostock, Cottbus, Greifswald, and Frankfurt (Oder) all had unemployment levels well below the East German average.[15]

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[15] This information is based on data for the year 1994, the first year after the East German Kreis reform. Matching as far as possible the spatial units of unemployment statistics with the Kreis boundaries, we find a negative (but not significant) correlation

In columns 5 and 6 of table 1, we include several measures commonly used to indicate measures of ethnic competition. These do not fare well. In particular, the interaction term between immigration and (changes in) unemployment levels is not significant. However, the level of immigration does have a positive and significant impact on violence in the expected direction. The coefficient of .029 tells us that as the log of net immigration size rises one standard deviation above its mean (from 5.3 to 10.5; see appendix table A1), the rate of right-wing violence rises about 17%. This is so because $\exp(5.3)^{.029} = 1.16$, compared to the effect of immigration one standard deviation higher, which is $\exp(10.5)^{.029} = 1.36$.[16] While this impact of immigration is substantial, in the absence of a main effect of unemployment and no effect for the interaction term, we interpret this pattern as providing only partial support for previous competition perspectives. Clearly, we must look beyond economic measures for answers to variation in ethnic violence in Germany.

To this end, we investigate the impact on violence of discursive opportunities. In table 2, we retain the variables that were significant in table 1 and add our key measures of discursive opportunities and two measures of extradiscursive political opportunity structures. Taking the first two columns showing the results for all states, we see that prior violence, population size, and immigration remain important predictors of violence. Of the POS variables, high levels of party competition significantly raise levels of violence. The composition of government does not have a significant effect.

Table 2 shows support for the hypothesis regarding the effect of discursive opportunities. Visibility increases rates of violence in the following year (see also Brosius and Esser 1995). Consonance (measured by the number of negative claims by other actors than the radical right on migrants and minorities) also raises rates of right-wing violence. Using the means and standard deviations in appendix table A1, we can calculate that as the number of consonant claims increases by one standard deviation above the mean, the effect of the coefficient for this measure (.017) indicates that the rate of radical right violence in the following year rises by 25%. Conversely, dissonance (measured by claims by other actors against the radical right and xenophobia) significantly decreased the rate of violence. As the number of statements in opposition to the radical right increased by one standard deviation, the rate of right-wing violence sub-

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between unemployment levels in 1994 and levels of violence across the period 1990–99 in the East, whereas in the West the correlation is significantly positive, although rather weak (.19).

[16] Because of missing unemployment data, table 1 reports analyses for 148 cases, while we present descriptive statistics in app. table A1 for 154 cases. This did not affect this calculation, though—the effect of immigration is the same.

TABLE 2
GENERALIZED ESTIMATION EQUATION EFFECTS OF DISCURSIVE OPPORTUNITIES ON
LEVELS OF RADICAL RIGHT VIOLENCE

	ALL STATES		WESTERN STATES (Includes Berlin)		EASTERN STATES	
	B	Z-Score	B	Z-Score	B	Z-Score
Radical right violence[a]	.004***	4.59	.004***	3.78	.006**	2.96
Log population size	.638***	6.72	.623***	7.12	.399**	2.54
Eastern location	1.09***	6.44	. . .	. . .	. . .	. . .
Net immigration/ 1,000	.021***	3.45	.025**	3.64	−.012	.08
Government coalition (high = left coalition)	.061	1.48	.054	.372	.094[+]	1.28
Party competition ...	.019*	2.55	.025**	2.11	.003	.201
Visibility	.980**	2.10	1.96***	6.56	−.315	.322
Anti-immigrant statements (consonance)	.017**	.00	.208***	3.91	.023	.385
Anti–radical right statements (dissonance)	−.008**	.00	−.008[+]	1.92	−.014***	3.64
Legitimacy	−.015	.21	−.127	.608	.662***	5.60
Wald χ^2	782***		2,559***		290***	
N	154		109		45	

NOTE.—Table refers to Germany, 1990–99. B = unstandardized regression coefficient.
[a] All independent variables (including the lagged dependent variable) are lagged one year.
[+] $P < .10$.
* $P < .05$.
** $P < .01$.
*** $P < .001$.

sequently decreased by about 10%, across all states in Germany. When we compare the effect of dissonance between the western and eastern states, we see that the negative effect of anti–radical right statements on the diffusion of radical right violence is especially potent in the East. The inhibiting effect of dissonant claims on violence is interesting, given our argument that dissonance could have either positive or negative effects. The evidence in table 2 suggests that public disapproval dampens subsequent violence. Contrary to the expectations, legitimacy does not play a role in explaining violence (at least not when Eastern and Western states are analyzed together).

In view of the strong differences in history, economics, and immigration patterns between the East and West, it is not surprising that some of the results across this divide diverge. In particular, the net immigration level

216

has no systematic effect on the rate of violence in the East (see the third through sixth columns of table 2). Although violence against immigrants is more widespread in the East, there are fewer immigrants in this part of the country. Moreover, during the period in which anti-immigrant violence rose most dramatically (from 1990 to 1992; see fig. 1 above) the number of foreigners in most parts of East Germany declined because the German government forced many former GDR guestworkers from third-world communist states such as Cuba and Angola to return to their countries of origin. Thus, in large parts of East Germany—including the state of Saxony, where the first big riots occurred—antiforeigner violence escalated at a time when the number of immigrants was small and declining.

The other differences between the East and West are more marginal. Regarding political opportunity structures, the party competition variable is not relevant in the East, but the effect of the composition of government is in the expected direction. This means that in the East radical right violence tended to be somewhat more prevalent when the left was in office in a state. A further difference is that the number of prior consonant claims does not affect rates of radical right violence in the East; however, the effect of legitimacy in the East is highly significant and positive. In addition, we find no effect of visibility in the East, which may be due to the fact that the readership of our newspaper source is concentrated in the West. Despite these regional differences, our general hypothesis regarding the effect of discursive opportunities on the level of radical right violence finds support in both regions.

Explaining the Targets of Radical Right Violence

We now ask if discursive opportunities can also explain the choice of targets of right-wing violence during the 1990s. Table 3 gives an overview of the targets of violence for this decade using the newspaper sources (recall that the police data does not disaggregate violence by target). Right-wing violence had a broad range of variation of targets. The "miscellaneous targets" category includes attacks against homeless persons, disabled persons, tourists, and journalists. One sizable category was that of "unspecific targets," which includes random destruction of property by radical right groups, disturbances at festivals, or attacks where the victims were nonminority Germans in everyday settings (e.g., people leaving a discotheque). We ask, did this distribution represent a relatively fixed rank-ordering of attacks on different groups and targets (Pettigrew 1998, pp. 80–81) or were there significant shifts in targets over time that require explanation?

Figure 2 suggests that there were indeed important shifts in the radical

217

TABLE 3
Distribution of Targets of Radical Right Violence

Targets	%
Asylum seekers	36.8
Other immigrant groups/"foreigners" unspecified	31.7
Jewish targets	3.9
Left-wing groups	8.1
Police	1.2
World War II memorials	2.5
Miscellaneous targets	1.2
Unspecific targets	12.8
Total	100.0
No. of events	930

Note.—Table refers to Germany, 1990–99.

right's choice of targets. The figure shows the development over time of violence against the categories of asylum seekers, other immigrant groups, and unspecific targets, corresponding to three of the categories in table 3. The category "other targets" combines all the remaining target categories from table 3. The figure shows that the level of violence against "other" and "unspecific targets" remained virtually unchanged across the decade. The trajectory of violence appears to be driven mainly by fluctuation in violence against two particular categories—asylum seekers and other immigrant groups.

The evidence that, initially, immigrants and asylum seekers did not play a predominant role as victims of radical right violence is crucial evidence in support of our public discourse argument. If targets had been constant over time, then our hypothesis about the amplification role played by the public discourse would be less compelling. In the year 1990, violence against asylum seekers and other immigrants made up only 5% and 15%, respectively, of all radical right violence. A year later, 48% of all violence was directed against asylum seekers and an additional 32% against other immigrants. By 1992, asylum seekers alone accounted for 63% of the radical right's targets. Subsequently, the relative importance of these targets declined again, especially in the case of asylum seekers.

Moreover, the evidence suggests that these shifts across targeted groups were systematically related to the differential discursive opportunities open to the radical right. The first column of table 4 shows for different target groups the average visibility of attacks against these targets, calculated as the fraction of events reported on a newspaper's front page. Attacks against asylum seekers were more likely than any other type of radical right violence to be reported on the front page of the newspaper, with one-quarter of events directed against asylum seekers receiving front-
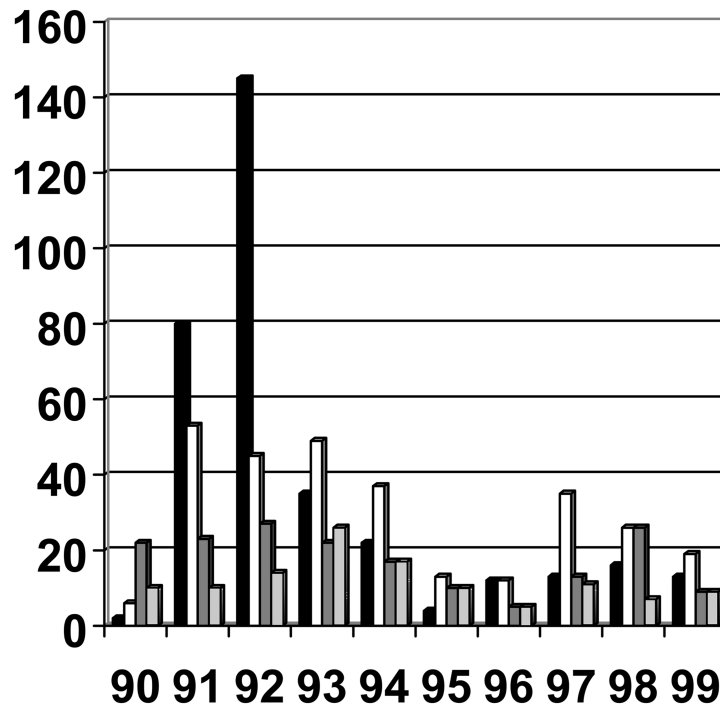
218

Fɪɢ. 2.—Development over time of violence against targets corresponding to categories in table 3; black indicates asylum seekers, white indicates other immigrants, dark gray indicates other targets, light gray indicates unspecific targets.

page coverage. Violence against the other two categories of immigrants was slightly less prominently publicized, with 22% of violence against ethnic German *Aussiedler* ("re-settlers") and 20% of violence against other foreigners (mostly former guestworkers such as the Turks) being reported on the front page. Radical right violence that did not target immigrants was least likely to be prominently covered (16% front-page coverage).

Next to visibility, our argument stresses the importance of the degree to which violence resonates with ongoing public debates that refer to particular target groups. In this case, asylum seekers were a more important focus of public debates than any other potential target group of the radical right. As the second column in table 4 shows, asylum seekers featured prominently in the public debate in the 1990s, with almost 1,400 negative statements on this group over the course of the decade. Survey data from this period show that the Germans considered the asylum issue to be "the most important current political issue in Germany," ranking

219

TABLE 4
Visibility and Consonance Indicators for Radical Right Violence against
Different Target Groups

	Visibility (%)	Consonance
Asylum seekers	25	1,376
Other immigrant groups/foreigners	20	723
Ethnic German Aussiedler	22	86
Other targets	16	. . .
All targets	21	. . .

Note.—Table refers to Germany, 1990–99. Visibility = percentage of violent actions against target group reported on newspaper front page. Consonance = negative claims on target group by actors other than radical right. We did not collect data on negative claims on nonimmigrant target groups of the radical right, such as homosexuals, the handicapped, or the homeless. During the period under study, none of these groups generated as much political controversy as did the issue of immigration.

above issues such as unemployment and the costs and consequences of reunification (Roth 1994). To a lesser (but still important) extent public controversies also raged over other immigrant groups, but, as the second column of table 4 shows, negative statements on other immigrant groups were only half as frequent as those on asylum seekers.

The third group, the Aussiedler, consists of immigrants from Eastern Europe and the former Soviet Union who are officially defined as "ethnic Germans" (*Volksdeutsche*). They are the descendants of (originally) German-speaking groups who (mostly several hundreds of years ago) migrated eastward. According to the German constitution, they have the right to migrate to Germany and to receive German citizenship upon arrival (Bade 1992; Münz, Seifert, and Ulrich 1997). Table 4 shows that there were few negative statements on Aussiedler compared to those on other immigrant groups. Negative statements on Aussiedler (86) were sixteen times less frequent than those on asylum seekers (1,376). Similarly, the level of violence against this group remained very low. Only 2% of all radical right violence was directed against Aussiedler, that is, 18 times less than the frequency with which asylum seekers were targeted (compare table 3).

Why would the rate of violence and public discourse surrounding this group be so low? At first glance, this finding might be puzzling (especially for competition theories), because the competitive advantage of Aussiedler (in terms of social and political rights) compared to other immigrant groups was substantial during this period. The Aussiedler were the largest immigrant group in the 1990s, surpassing even the already massive influx of asylum seekers (in 1990 alone, 400,000 Aussiedler came to Germany; over the whole decade, they numbered more than 2 million). Unlike other immigrant groups, Aussiedler had immediate access to the same rights and entitlements as native Germans; programs were set up to help Aus-

220

siedler find jobs, and they had priority access to housing.[17] In contrast, asylum seekers were prohibited from employment, received only a very low level of social assistance, and were denied access to the regular housing market. Therefore, there can be no doubt with which immigrant group German families and workers were most strongly in competition, namely the Aussiedler. However, unlike all other immigrant groups, the Aussiedler were not designated in the public discourse as ethnically distinct. Thus, even if competition for resources among individuals occurred, it did not adopt an ethnic or racial character (even though they were not necessarily welcomed equally by all Germans; see Pfetsch 1999). As a result of their favored "ethnic Germans" identity, this group experienced little violence, and their status generated little controversy.

In order to further explore the effects of public discourse on the targets of radical right violence, our final step in the empirical analysis compares the effects of public discourse across events with different targets in a multivariate context. Table 5 displays the results of regression analyses with three types of violence as the dependent variables: against asylum seekers, against other immigrant groups, and against all other targets (this category includes violence against Aussiedler, for which numbers were too low to be analyzed separately). The results in the first two columns of table 5 show that discursive opportunities play a crucial role in explaining violence against asylum seekers, with all four variables attaining significant levels in the expected direction.[18]

Discursive opportunities show weaker effects on violence against other immigrants when compared to violence against asylum seekers. However, we do find significant effects of visibility and legitimacy on violence against other immigrants. Finally, the measures of discursive opportunity do not contribute to the model of violence against all other targets. As we have seen above, public discourse during the 1990s was heavily focused on the issue of asylum seekers and to a lesser extent on other immigrant groups. Our argument predicts that measures of discursive opportunities should favor the diffusion of violence against targets that were the focus of public debate. Conversely, other forms of radical right violence, which

[17] By the end of the 1990s, Germany had gradually moved away from an ethnic conception of citizenship. As a consequence, the privileges of Aussiedler were reduced, and the contrast between the treatment of Aussiedler and other immigrants became less sharply defined.

[18] Apart from the consonance variable (see table 5, n. b), none of the discursive variables could be calculated by target group. It would have been ideal—and might have strengthened the results—if legitimacy, visibility, and dissonance could have been computed for each target separately. However, this was not possible as there were too many empty cells of state-year combinations with no violence against a target group for which legitimacy and visibility measures are undefined.

221

TABLE 5
GENERALIZED ESTIMATION EQUATION EFFECTS OF DISCURSIVE OPPORTUNITIES ON
VIOLENCE AGAINST DIFFERENT TARGET GROUPS

	VIOLENCE AGAINST ASYLUM SEEKERS		VIOLENCE AGAINST OTHER IMMIGRANTS		VIOLENCE AGAINST OTHER TARGETS	
	B	Z-Score	B	Z-Score	B	Z-Score
Violence against target group[a]	.055**	2.96	.030	1.29	−.025	.40
Log population size	.567***	3.63	.544**	2.83	.358**	2.10
Eastern location	2.77***	6.62	1.57***	3.20	1.99***	6.39
Net immigration/1,000	.112***	7.68	.570***	3.23	.056***	4.26
Government coalition (high = left coalition)	.095	1.47	−.071	.69	.031	.31
Party competition	−.009	.47	−.016	.91	.004	.23
Visibility	.200**	2.41	.048**	3.07	−.010	.44
Anti-immigrant statements (consonance)[b]	.063**	2.10	−.025	.67	−.002	.91
Anti–radical right statements (dissonance)	−.026[+]	−1.76	.007	.81	.002	.89
Legitimacy	1.06[+]	1.66	.567[+]	.311	−.06	.14
Wald χ^2	469***		263		589	
No. cases	138		138		138	

NOTE.—Table refers to Germany, 1990–99. B = unstandardized regression coefficient.

[a] All independent variables (including the lagged dependent variable) are lagged one year.

[b] Cols. 1 and 2: consonance (with the radical right) includes only negative statements on asylum seekers. For cols. 3 and 4, consonance includes negative statements on other immigrant groups only. For cols. 5 and 6, consonance includes negative statements against all immigrant groups.

[+] $P < .10$.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

do not resonate with ongoing public controversies, should be left unaffected by our public discourse measures. The evidence supports the contention that differential stimuli from the public sphere explain the evolution of the target repertoire of the radical right that we saw in figure 2 above.

Two other findings in table 5 deserve mention. First, the effects of POS measures (measured by government coalitions and party competition) are not significant in any models in table 5. This bolsters our earlier argument that publicly manifest, rather than latent, structural political opportunities are decisive for explaining movement mobilization. Second, the persistence of a strong effect of immigration in all three regressions supports

222

contentions from competition theory that hold that immigration provokes ethnic violence. Rising immigration may provoke violence against a wide range of targets, including nonimmigrant groups. Yet over time, with the growing focus of the public discourse on particular immigrant groups, we see violence concentrating on these publicly resonant targets, which were not necessarily the groups that stood most clearly in socioeconomic competition with the perpetrators of violence.

DISCUSSION AND CONCLUSIONS

Why have previous explanations—ranging from economic deprivation to ethnic competition models and POS theories—proved unsatisfactory in explaining right-wing violence? The POS perspective offered us a point of departure for answering this research question. Taking up the long-standing criticism that political opportunities can affect movement action only when they are perceived as such, we emphasized that opportunities will influence the trajectory and targets of protest to the extent to which they have become visible in the mass media. We have introduced the notion of "discursive opportunities" to capture these publicly visible opportunities and constraints for movement action.

We have argued that the expansion of the mobilization of the radical right—like any other type of collective action—depends on diffusion processes. The three types of discursive opportunities that we have distinguished, visibility, resonance (with its two variants consonance and dissonance), and legitimacy, act as mechanisms in positive and negative feedback processes that differentially affect the diffusion chances of various types of radical right action. This dynamic, evolutionary approach rests on processes of selection and differential replication of variations of radical-right collective action as a function of if and how journalists report them and third actors react to them in the public sphere.[19] Actions and tactics that are publicized by the mass media offer a model for successful public action to others who share the same goals (or who simply want the same degree of publicity). We further found that acts of violence that provoke more public reactions by third actors (what we have called resonance) have better chances of reproduction. If public reactions by other actors to a particular type of violence are at least partly positive (i.e., it has a certain degree of legitimacy), the likelihood of replication will be further increased. Imitation of violence that becomes visible and resonant in the public sphere need not be considered as unreflexive (or irrational),

[19] For a further elaboration of this evolutionary approach to political contention, see Koopmans (2004).

223

as sometimes implied by the "contagion" metaphor used to describe diffusion processes. To the contrary, given the fact that most attempts to achieve public attention are unsuccessful, the replication of models of collective action that have high visibility and resonance in the public sphere is demonstrably rational, since their adoption may be expected to confer similar media attention on the imitators.

We believe that our model of the dynamics of the public discourse and the role of discursive opportunities in shaping the evolution of political contention has relevance beyond an explanation of ethnic violence. In principle, the theoretical assumptions of our model apply to interaction in the public sphere, regardless of its form or content. The type of analysis we have presented here can be applied to other issue fields and other types of collective action. For instance, one might study how the public discourse on environmental issues affects the diffusion chances for environmental protest by making some types of protest addressing certain topics and directed against certain adversaries more visible, resonant, and legitimate, and others less so. Applications of our approach need not be limited to social movements or protest mobilization. The model also seems applicable to the study of the careers of more conventional forms of public action, for instance in explaining the differential success of presidential or legislative candidates' attempts to set the public agenda during election campaigns. We hope to have convinced other researchers working on similar topics of the potential gains of taking into account the discursive context of mobilization and collective action in the public sphere. We also hope that our notion of discursive opportunities may help to bridge the gap between political opportunity structure and framing perspectives in the social movement literature, and that it may suggest some common grounds for a dialogue between the (often juxtaposed) "political" versus "cultural" approaches to collective action.

Finally, our analysis suggests new ways in which newspaper sources can be used to study collective action. So far, event analyses of collective action have captured little of the communicative context in which protest occurs. Our analysis shows that the mass media may be a rich source of information about discursive context variables that may significantly improve our explanations of collective action. There is more to the public sphere than just newspapers, which are only one source of public discourse about collective action and its aims and targets. Television, radio, books, magazines, specialized journals, and Internet Web sites all carry information about events and debates surrounding them that could be coded and used in models of public discourse dynamics. Clearly our work is just a beginning step toward understanding how and why the dynamics of public discourse shape the evolution of collective action.

224

APPENDIX

TABLE A1
VARIABLES USED IN THE ANALYSIS

	Mean	SD	1	2	3	4	5	6	7	8	9	10
1. Right-wing violence[a]	52.4	50.0	. . .									
2. Log population size	8.2	.87	.48	. . .								
3. Former part of East Germany (0, 1)	.29	.46	.15	−.21	. . .							
4. Net immigration (per 1,000 population)	5.2	5.3	.02	.08	−.28	. . .						
5. Government coalition (high = left coalition) ...	3.9	1.8	.09	−.19	−.23	.14	. . .					
6. Party competition	10.1	7.1	−.17	.14	.12	−.19	−.35	. . .				
7. Visibility	.02	.05	.03	.09	.13	−.02	−.05	−.14	. . .			
8. Number of anti-immigrant statements	8.6	9.1	.22	.52	−.44	.06	.13	−.00	−.09	. . .		
9. Number of anti-radical right statements	18.1	14.6	.63	.40	.03	.06	.05	−.13	.05	.30	. . .	
10. Legitimacy	.38	.31	−.19	.15	−.39	.09	−.17	.12	−.13	.53	−.28	. . .

NOTE.—This table refers to 154 states analyzed in table 2, cols. 1 and 2. The means, SDs, and correlations of the variables are given here.
[a] All covariates were lagged and measured annually at $t − 1$.

TABLE A2
VARIABLES USED IN THE ANALYSIS

	Mean	SD	1	2	3	4	5	6	7	8	9	10
1. Violence against asylum seekers[a]	2.32	4.5	. . .									
2. Log population size	8.2	.87	.06	. . .								
3. Former part of East Germany (0, 1)	.29	.46	.31	−.21	. . .							
4. Net immigration (per 1,000 population)	5.5	5.5	.09	.08	−.29	. . .						
5. Government coalition (high = left coalition) ...	3.9	1.8	−.07	−.18	−.26	.14	. . .					
6. Party competition	9.2	7.1	.05	−.11	.16	−.20	−.43	. . .				
7. Visibility	.47	1.3	−.04	−.09	.15	−.03	−.03	−.17	. . .			
8. Number of anti-immigrant statements	5.7	6.3	.20	.49	−.39	.21	.00	−.05	−.16	. . .		
9. Number of anti–radical right statements	18.7	15.0	.23	.43	.00	.03	.05	−.15	−.05	.33	. . .	
10. Legitimacy	.38	.31	−.03	.11	−.39	.10	−.14	.14	−.24	.46	−.29	. . .

NOTE.—This table refers to 138 states analyzed in table 5, cols. 1 and 2. The means, SDs, and correlations of the variables are given here.

[a] All covariates were lagged and measured annually at $t − 1$.

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