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Watch the Crowd: Bystander Responses, Trickle-Down Politics, and Xenophobic Mobilization

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Abstract

Social movement scholars have struggled with the question how abstract political opportunities affect activists without much knowledge of politics. We argue that the relationship between institutional opportunities and mobilization may take the form of trickle-down politics. In this view, activists are affected by political opportunities indirectly through the changes that political developments bring about in the immediate setting of protest. The political climate determines the distance between general public opinion and activists' view on society. The smaller this distance, the more likely it becomes that activists receive positive feedback, which results in further mobilization. We investigate how activists are influenced by bystander responses that are evoked by the wider political context. Statistical models indeed indicate that spatiotemporal fluctuations in political opportunities and public sentiments are translated into mobilization after activists receive feedback from bystanders. This suggests that bystander responses play a crucial role in linking political opportunities to mobilization.

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Introduction

Following reunification, Germany witnessed an upsurge in extreme right violence targeting asylum seekers and other immigrant groups, resulting in about one hundred casualties, thousands of injuries, and extensive material damage, and making it the most serious series of attacks against ethnic minorities in postwar Western Europe (Kurthen, Bergmann, & Erb, 1997). The violent protests fundamentally altered the landscape of immigration politics in the reunified republic by fueling a fierce public debate on immigration and creating a momentum for restrictive immigration policies that subsequently lowered the influx of new immigrants (Koopmans, 1996).

Early explanations of protest waves conceptualized outbursts of violence as spontaneous (Feierabend & Feierabend, 1966), dysfunctional, and irrational (Le Bon, 1897). Participants in riots were depicted as unorganized individuals disconnected from mainstream society (Kornhauser, 1959). More recent scholarship emphasizes that collective action in general and collective violence in particular is not markedly different from other forms of institutionalized behavior. In this view, organizational structures play a central role in orchestrating violent protests by recruiting and encouraging individuals to take part (McCarthy & Zald, 1977). These social movement organizations are in turn embedded in wider political opportunity structures (POSS; McAdam, 1982; Tarrow, 1994), which reveal themselves either directly or indirectly through the mass media (Gamson & Meyer, 1996; Koopmans & Olzak, 2004).

The German case provides evidence for the classical and more recent views on social movements. On one hand, several empirical studies have demonstrated that extreme right violence was partly produced by an intense conflict within the German political elite on how to respond to the large increase in immigration from war-torn Yugoslavia, the former Soviet Union, and other parts of the world (Koopmans, 1996; Ohlemacher, 1994). On the other hand, profiles of arrested perpetrators suggest that they were mainly lower educated, unemployed youngsters who had few, if any, links with extreme right organizations and little interest in politics more generally (Willems & Hill, 1993).

These contrasting findings pose an interesting puzzle. How are youths who are hardly reached by right-wing movement leaders and express no interest in institutional politics influenced by political opportunities for

mobilization? We conceptualize the relationship between movements and the state as a form of trickle-down politics. Mobilization and participation decisions are to a large extent influenced by the immediate social setting in which protest takes place. If this social setting provides positive feedback, activists are encouraged to undertake more acts of mobilization. The immediate social setting, however, is itself shaped by the wider structure of political opportunities. Political debates and decisions create pools of sentiments (McCarthy & Zald, 1977) that draw general public opinion closer to the opinion of activists, making direct positive feedback more likely. As a result, changes in the political climate trickle down through interactions between activists and their immediate environment, even if the former do not themselves follow political developments closely.

We explore this process by zeroing in on the relationship between protesters and bystander audiences, that is, nonparticipating audiences present at protest events, a topic that has received only scant scholarly attention (Favre, 1990; Lofland, 1996; Turner & Killian, 1987). Our central argument is that bystander responses can stimulate violence by emboldening local activists. Because bystander responses themselves are shaped by the political environment, they establish a link between political opportunities and mobilization.

To test the general validity of this argument, we conduct two statistical analyses. In the first, we model overtime fluctuations in political opportunities, bystander responses, and right-wing violence in Germany for the period 1990-1999. In line with existing arguments, political debates and decision-making processes have an important impact on the intensity of racist attacks. However, these effects turn out to be mediated by bystander responses.

In the second analysis, we model the outbreak of ethnic violence and responses by bystanders in all German *Kreise*, a geographical unit roughly comparable with a U.S. county and the most fine-grained unit for which statistical data are available in Germany. Event history models are deployed to analyze the exact timing of xenophobic violence for the period 1990-1995. The analysis again supports the bystander argument. A further escalation of violence was much more likely in counties where bystanders responded supportively to anti-immigrant violence in the previous month. This suggests that bystander responses not only determine when political opportunities trickle down but also where and to whom.

The article will proceed as follows. In the section "From Structure to Action: Trickle-Down Politics," we theoretically outline how interactions with bystanders reveal information about political opportunities. In the second section, we illustrate this argument with a description of the riots that took place in Hoyerswerda and Rostock. In the next section, we discuss the

methods and data sources used. We describe the results in the next section. In the concluding section, we discuss some limitations and possible future extensions.

From Structure to Action: Trickle-Down Politics

POS theories hold that mobilization depends on opportunities offered by the political context. Early applications mainly tried to explain the intensity of mobilization (Jenkins & Perrow, 1977) and the outcomes of movement activities (Gamson, 1975). Although what constitutes a political opportunity is not set in stone, most scholars include a state's capacity to repress government composition, electoral competition, and elite divisions (McAdam, 1982; Tarrow, 1994).

A question that has occupied both POS theorists (Gamson & Meyer, 1996) and their critics (Goodwin & Jasper, 1999) for many years is how political opportunities get translated into collective action (McAdam, Tarrow, & Tilly, 2001). This question is particularly relevant for the case at hand because most right-wing activists were low-educated youths and skinheads used in lower segments of the labor market (Willems & Hill, 1993), most of whom did not express any interest in institutional politics (Wahl, 2001).

Some have asserted that a core of more sophisticated political entrepreneurs lead mobilizations of uninformed activists (Wilkinson, 2006). This is not likely in case of the German extreme right. Perpetrator profiles suggest that most attackers were not involved in supralocal organizations but operated mostly in informal networks or local gangs (Wahl, 2001). In addition, statistical analyses have demonstrated that rates of right-wing violence were actually lower in regions where the parliamentary right and extreme right organizations were stronger (Braun & Koopmans, 2010).

More recently, scholars have argued that the missing link between institutional politics and activists' decisions is provided by the mass media (Gamson & Meyer, 1996; Koopmans & Olzak, 2004). Although this is a fruitful idea, which has inspired a large body of research (e.g., Vliegenthart, Oegema, & Klandermans, 2005), we take a somewhat different route. Given that low-educated people are not likely to consume political news attentively (Price & Zaller, 1993) and most right-wing activists were not interested in institutional politics, it seems unrealistic to assume that they closely follow political news coverage. Instead, we believe that most of them are more attuned to the immediate social setting in which they protest and will seek confirmation from actors they interact with directly.

In as far as bystanders have received attention by social movement scholars, they have been portrayed as passive actors who consider protests an

annoying interruption of their daily rhythm (Lofland, 1996; Turner & Killian, 1987) or whose role is not specified at all (Favre, 1990). Turner and Killian (1987) for instance argue that bystanders only care about the collateral damage that collective action causes and “do not want to hear about” (p. 217) the issues that movements raise. In a similar vein, Lofland (1996) defines bystanders as “that portion of the public primarily concerned with the risks and inconvenience an SMO may be creating for them and who demand that authorities do something to end their risk and inconvenience” (p. 307).

Contrary to Lofland (1996), who defines bystanders based on their actual stance toward activists, we define bystanders as the section of a general public that is physically present at the site of a protest event but does neither actively participate in it, nor has gathered to mobilize against it. We follow Schattschneider’s (1960) classical recommendation: “If a fight starts, watch the crowd, because the crowd plays the decisive role” (p. 3). Bystanders are relevant for social movements because their responses provide feedback that inspires or discourages activists to engage in more attacks. During protest waves, uncertainty is high because existing norms are contested and new ones get established (Tarrow, 1994). A guiding principle in social psychology is that under conditions of uncertainty, the perception of reality is shaped by social influence and comparisons with opinions expressed by actors in the immediate environment. Experimental work that demonstrates the powerful influence other actors’ opinions have on an individuals’ own behavior abounds in research literatures on conformity, attitude change, and social identification (e.g., Asch, 1952; Festinger, 1954).

Of particular relevance for this article is experimental work on interethnic relations. Classic theories of interethnic relations are predicated on the notion that consensuality is a key feature of out-group hostility (Pettigrew, 1998). Individuals tend to hold negative views of other groups if they perceive that others in their direct environment hold those same beliefs (Tajfel, 1982). More recently, research has demonstrated that people become more extreme and convinced of their own beliefs if information about congruent beliefs of others is provided (Wittenbrink & Henly, 1996).

Responses by bystanders suggest that acts of violence resonate with public sentiments that live among people in the immediate environment. These sentiments in turn are shaped by the wider political context. Although lower educated activists might not follow politics, this is certainly not true for the entire population. Therefore, issues that people consider salient are partly put on the agenda by political structures and media discussions (Vliegenthart, 2007). When the political climate about immigration is negative, the general public opinion and the opinion of activists converge making positive feedback from bystanders more likely. If we conceptualize political opportunities

and bystander responses in this way, it follows that political opportunities trickle down to activists on the ground through bystander responses. To use McAdam et al.'s (2001) terminology, we blend cognitive and relational mechanisms that link structures to outcomes. Interactions between bystanders and activists shape how the latter perceive anti-immigrant sentiments in their direct environment.¹

In the next section, we illustrate this trickle-down process by zeroing in on how riots in Hoyerswerda and Rostock revealed information about elite divisions in German politics. These two cases are chosen because they are relatively well researched and a considerable amount of secondary literature is available (Karapın, 2007; Koopmans, 2004).

Extreme Right Violence in Germany

Political opportunities in general and elite divisions in particular were present in the German context. Due to the collapse of the Soviet Union and civil conflicts in Africa, Asia, and the former Yugoslavia, millions of individuals sought refuge in Western European countries. A disproportional amount of these immigrants entered Germany that was well known for its liberal asylum policies. These developments sparked an intense political debate about the rights of immigrants. This debate centered on the question whether a constitutional restriction of asylum rights was required. To implement such a reform, however, a two-thirds majority was necessary, which turned out to be difficult to achieve. Elite divisions guaranteed that debates about immigration problems dominated the political agenda, while a straightforward resolution of elite conflicts was out of reach. It was not until July 1993, when a constitutional amendment was introduced that restricted the inflow of new asylum seekers, that tensions were resolved (Koopmans, 1996).

The fact that the immigration issue occupied center stage on the parliamentary agenda also affected public opinion on immigration. From January 1990 onward, there was a steady increase in the number of Germans who considered asylum seekers and other immigrants the most important problem that the government should deal with (Ohlemacher, 1994). This public discontent with immigration policies came to the surface rather suddenly during the 1991 riots in the eastern German town of Hoyerswerda, which was strongly affected by the social and economic dislocations following German reunification (Karapın, 2007). Attacks on homes of foreign workers lasted for almost 4 days. Bottles, chains, baseball bats, and Molotov cocktails were used to scare immigrants out of their hostels. These attempts were successful and several Mozambican immigrants fled their homes. After 4 days, the attacks shifted their focus to an asylum seeker shelter and lasted until the

police interfered and removed the asylum seekers to a safe location elsewhere in Germany (Heitmeyer, 1993).

What made these riots special was that large groups of bystanders actively responded to the attacks. Attackers were joined in their racist chants by hundreds of individuals. Moreover, bystanders tried to hinder police officers who tried to intervene and commanded them to act tougher against immigrants. Some even fought against the police while encouraging the skinheads to burn down houses (Karapin, 2007).

A similar, but even more severe incident happened about a year later in Rostock, also situated in the former East Germany. In August 1992, 500 youths attacked an asylum seeker shelter in the Lichtenhagen neighborhood. On the third night, the shelter was set on fire and hundreds of asylum seekers had to run for their lives. Again, the extremists' actions received approval from spectators. More than three thousand locals watched and chanted "foreigners out" and cheered every time a bottle or stone was thrown through the windows of the shelter.

The debate that haunted German politics had spilled over into the streets where bystanders who either supported or, as happened in several other instances, denounced the attacks in the streets reflected elite divisions. After these incidents, Hoyerswerda and Rostock experienced an upsurge in right-wing activity. Encouraged by feedback they got from bystanders, activists believed they were pursuing an important agenda worth fighting for. Interestingly, both counties had experienced violent attacks on foreigners before. In May and July 1990, very similar attacks had occurred. However, these attacks did not evoke any responses from bystanders and remained isolated and irrelevant events that did not ignite any further conflict. The fact that bystanders did not respond to these earlier attacks reflects the fact that the immigration issue was not yet very salient on the public agenda, as Germany was still busy arranging monetary and institutional reunification (Koopmans, 2004). To see whether the two-step relationship between political debates, bystander responses, and violence also holds in a larger set of cases, we will now move over to statistical analyses.

Data and Method

Two separate analyses are presented below. First, we analyze the monthly development of extreme right violence, POSs, and bystander responses at the national level, to see whether the latter mediate the relationship between the other two sets of variables. Second, we conduct an analysis across all German counties for the period 1990-1995 to see whether bystander responses also make certain localities more violence-prone than others.

National-Level Time Series: Dependent Variable

The dependent variable in the time-series analysis is the monthly level of extreme right violence from January 1990 until December 1999. Information on xenophobic violence was retrieved from newspaper reports coded in the context of the MERCI project (Koopmans, Statham, Giugni, & Passy, 2005). These data contain information on violent incidents and strategic public statements in the political field of immigration and integration that are covered in Monday, Wednesday, and Friday issues of the German national newspaper *Frankfurter Rundschau* (FR). This database includes the date, geographic location, casualties, arrests, targets, and numbers of participants of, as well as responses to, 692 instances of antiforeigner violence for the time period 1990-1999. These events are aggregated over 120 months.

As with all sources of event data, it is possible that newspaper data contain selection and description biases. Therefore, the file's yearly aggregates were correlated with official police statistics obtained from the *Bundesamt für Verfassungsschutz* (Federal Office for the Protection of the Constitution; 1991-1999). The high correlation of .82 between the two sources gives confidence that the data reflect real fluctuations in xenophobic violence. For the period from January 1990 until August 1994, police statistics are available on a monthly basis. Our data correlate .87 with these monthly police statistics. To check the robustness of our results, we also estimated a model using the monthly police statistics for the 1990-1994 period. Results were in line with the ones presented below and can be obtained from the authors upon request.

In addition, the data file was compared with coverage on extreme right violence in the *Bild-Zeitung* for the 1991-1992 period and three East German newspapers for the 4 months between June and September 1991. Weekly aggregates of the data used in this article correlated highly with all these sources (between .89 and .99). Moreover, the MERCI data gave the most inclusive picture of extreme right violence compared with the other media sources. This suggests that our data adequately reflect temporal fluctuations in xenophobic violence.

National-Level Time-Series Analysis: Estimation

In this article, we try to explain the level of right-wing violence, a continuous process, by measuring discrete events, that is, the number of violent attacks. Linear regression is inappropriate here because it assumes that continuous processes generate continuous events (King, 1989). Therefore,

often a generalization of the Poisson regression model is used as a technique to analyze continuous processes based on event counts (Long, 1997). This model assumes independency among events within the unit of observation, something that is quite problematic in the current field of study. Collective action in general and collective violence in particular tends to cluster nonrandomly in time due to imitation processes (Tarrow, 1994). The statistical name for this phenomenon is overdispersion (King, 1989). Inspection of the dependent variable indeed indicated that overdispersion was present in the data.

Another thorny issue in time-series analysis is autocorrelation, that is, a correlation between the residuals of different observations. The presence of autocorrelation inhibits conventional statistical estimation because it violates the assumption that observations are independent. To solve this problem, one needs to include measures that model away autocorrelation. Inspection of the autocorrelation and partial autocorrelation functions indicated that the intensity of xenophobic violence depended on violence in the previous month (McCleary, Hay, Meidinger, McDowall, & Land, 1980).

Hence, we need a technique to model autoregressive count data with overdispersion. Following Brandt and Williams (2001) and studies in epidemiology (Katsouyanni et al., 1996; Schwartz et al., 1996), we use a Poisson autoregressive model, which has the advantage that it allows to simultaneously model overdispersion and autocorrelation (see Mitchell & Moore, 2002). After experimenting with several specifications, we opted for a first-order autoregressive term. After inclusion of this term, the residuals were white noise, which indicates that they no longer correlate with each other across time. A first-order autoregressive term also produced a lower Akaike's info criterion than other specifications, suggesting that it fits the data relatively well (Enders, 2004). Analyses were conducted in STATA, using the ARPOIS procedure developed by Tobias (Tobías, Díaz, Saez, & Carlos Alberdi, 2001).

Following our ideas about trickle-down politics, we expect that effects of bystander responses mediate the effects of other environmental variables. We deploy a four-step strategy to test this idea (Baron & Kenny, 1986). In Step 1, we model the effects of all political and media context variables on mobilization. In Step 2, we add our bystander measures and assess whether they influence mobilization. In Step 3, we determine whether the effects of the political context variables become weaker after the bystander responses are included. Fourth, we regress bystander responses on the other independent variables to determine whether bystander responses are actually evoked by opportunity structures. In this last step, we determine the proportion and significance of bystander mediation through Sobel mediation tests in combination with bootstrapped standard errors (Preacher & Hayes, 2008).

National-Level Time Series: Independent Variables and Controls

The following independent and control variables were included in the analysis. All measures are lagged 1 month to establish causal order. All variables were stationary.

Bystander Responses. Information on bystander responses is obtained from the MERCI-file described above. For each extreme right attack, it was coded whether bystander publics openly responded to the attacks. In line with our definition of bystander publics, this variable includes spontaneous approval or disapproval by spectators but not organized countermobilization.² Cases in which bystanders merely watched were also excluded. In total, 81 bystander responses could be tracked. A fourth of these responses were supportive of right-wing mobilization. Supportive reactions include applauding, obstructing the police, and joining in racist chants. Disapproving responses were, however, more prevalent. The most common form was bystanders helping attacked foreigners or booing the rioting youngsters. Based on this information, we constructed three measures: an approving bystander response (BRSUPPORT), a disapproving bystander response (BRDISAPR), and a general response count (BRGEN). The first two measures are dichotomized to deal with their skewness. We expect that bystander responses in general signal the political salience of social problems and intensify subsequent protest. In addition, we expect that approving responses have a stronger effect than negative responses.

POSs. Three measures are used to gauge POSs, all retrieved from the MERCI data set. First, we grasp the political decision-making process by counting the number of decisions by state authorities on immigration issues. A distinction is made between decisions restricting the rights of immigrants, asylum seekers, and foreign residents (DECISNEG) and decisions improving or sustaining their rights (DECISPOS). In addition, we look at the effects of repressive measures. Following Koopmans (1997), we zero in on formal institutional repression, which includes acts instigated by governmental authorities (such as bans), the judiciary (trials and court rulings), and security agencies (large-scale police actions) to combat the extreme right movement. A monthly count of repressive acts was used (REPRESSION). We expect that negative political decisions ended the immigration debate that sparked political violence and took away much of the popular discontent. Moreover, we expect that institutional repression dampened violence by damaging the legitimacy and mobilizing capacity of the extreme right movement.

The bystander argument suggests that effects of POSs are mediated by bystander responses. Activists learned about structural opportunities through

their interactions with bystander publics. This implies that the effects of the opportunity structure variables should weaken or disappear after the bystander measures are included in the analysis.

Opinion Climate. As the riots in Hoyerswerda and Rostock illustrated, outbreaks of violence were accompanied by shifts in public debate and public opinion. Immigration issues became more salient in the mass media and among the general population, which subsequently resulted in more xenophobic mobilization. To see whether this relation also holds in general, we add measures that tap the intensity of immigration debates and anti-immigrant attitudes among the general population.

We measure the intensity of immigration debates by counting the number of verbal statements in the MERCI data set, irrespective of the actor, referring to the politics of immigration (DEBATE). We expect that intense debates lead to more violence. We also modeled the valence of the debate by including positive and negative statements separately and by modeling negative and positive statements as a proportion of all statements. The analysis suggested that valence did not affect mobilization.

We make use of the *Politbarometer* survey to capture the general opinion climate. In monthly polls, representative samples of the population were asked what their opinion was about the most important problem in Germany. Three answer categories were used to construct our anti-immigrant opinion measure (ATTITUDE). For each month in the 1990-1999 time period, we took the percentage of respondents that reported asylum seekers or foreigners as either the most or the second-most important problem. From this score, we subtracted the percentage of respondents that reported the extreme right as the most or the second-most important problem. Some have argued that most-important-problem-questions are problematic for tapping sentiments because they pick up two distinct issue characteristics: the extent to which things are considered a problem and the extent to which an issue is considered important (Wlezien, 2005). Boomgaarden and Vliegenthart (2009), however, demonstrate that the problem measures of the *Politbarometer* are highly correlated with negative attitudes. Therefore, we can be fairly confident that our measure taps anti-immigrant sentiments. During some years, fewer polls were conducted in the summer. Moreover, for the 1990 period, no data are available for the Eastern part of Germany. Missing months are imputed by means of linear interpolation. Missing values for the Eastern part are imputed based on values for the Western region, using the relationship between the East and West scores in the subsequent 9 years to estimate the value for 1990. To see whether these imputations affected our results, we also estimated a model in which we added a dummy that marked imputed observations. This did not

alter the results. Again, we expect that bystander responses mediate the relationship between opinion climate and mobilization. This implies that effects of anti-immigrant sentiment and media debates should weaken after we include the bystander measures.

Ethnic Competition. Ethnic competition theorists have mainly focused on labor market competition, holding that economic contraction and the presence of immigrants spark violent ethnic mobilization (Bélanger & Pinard, 1991). Previous studies have found a moderate relationship between ethnic competition and right-wing violence in Germany (Braun & Koopmans, 2010; Lubbers & Scheepers, 2000). We model the monthly change in the number of asylum seekers (ASYLUM), who made up the largest group of immigrants during the period of study, and fluctuations in the unemployment rate (UNEMPLOY) as measures of ethnic competition. To reduce the number of digits, we divide the asylum seekers measure by 100. We take difference scores because the trend component in the absolute measures would distort the results of the time-series analysis. We also model an interaction term of both variables ($AS \times ASYLUM$) as this taps the concept of ethnic competition better (Olzak, 1992). We acknowledge that objectively asylum seekers were not an important source of labor market competition for native Germans, as they are not allowed to take up work as long as their asylum requests are being processed. However, they did compete with unemployed Germans for social welfare benefits. Alternatively, we modeled ethnic competition using changes in overall immigration and using only immigration from non-Western countries. Results were identical to the ones presented below. Data on asylum seekers were obtained from the *Bundesamt für Migration und Flüchtlinge* while unemployment figures are provided by the Statistisches Bundesamt.

County-Level Analysis: Dependent Variable

To explore the effect of bystander responses on the geographical spread of right-wing violence, data for all 444 German *Kreise* are analyzed for the period 1990-1995. To get enough information on local differences, we tracked all incidents in two independently collected databases. The first data set was collected as part of a large project covering European protest and coercion in 28 European countries (Francisco, 1996). The data were collected from the *Reuters Textline Library*, which covers more than 400 international, national, and regional wire services, newspapers, and magazines. For each instance of contention, the date, geographical location, number of arrests, number of participants, initiating group, and target were coded. This data set includes a

total of 253 violent incidents targeting foreigners in Germany for the time period 1990-1995. The second file used is again the MERCI data set (Koopmans et al., 2005) but like the Francisco data set limited to the period 1990-1995. Monthly aggregates of both files correlated highly (.91) indicating the comparability and reliability of the measurements. After removal of doubles, the final analysis included 687 events that took place during the period 1990-1995 in 220 of the 444 *Kreise*.³

County-Level Analysis: Estimation

Event history models, focusing on the duration of time between violent events in each of the individual counties, are used to test the hypotheses formulated above. This type of analysis enables us to exploit all available information on the exact dates of violent events (Olzak, 1992).

We use partial likelihood estimation as developed by Cox (Cox & Oakes, 1984). Cox regression, as opposed to other event history techniques, has the advantage that one does not need to specify the baseline hazard. In specifying a Cox model, two considerations should be taken into account. First, it posits that variables included in the model shift the baseline hazard multiplicatively and that these shifts are constant over time: the proportional hazard assumption. This assumption can be tested by means of a Schoenfeld residual test (see Gould & Cleves, 2004). Inspection of the Schoenfeld residuals indicated that the proportional hazard assumption of the models was not violated. Second, the baseline hazard for event occurrence might vary across entities facing different structural settings. In this study, it is likely that the baseline hazard varies between East and West Germany because soon after Germany's unification there was still a high degree of social and economic divergence between the two regions. Therefore, all observations are stratified by East/West region.⁴ Stratified models allow the baseline hazard to vary over groups but at the same time estimate coefficients that are constrained to be homogeneous and therefore allow for the inference of general causal relations for both regions.

The analysis starts on January 1, 1990, and ends on December 31, 1995. This end date was chosen because the Francisco data set not only runs until the end of 1995, but it also makes sense substantively. The data as well as police statistics and historical records (Kurthen et al., 1997) indicate that the wave of extreme right violence in Germany had subsided by the beginning of 1996. In total, 1,131 spells are analyzed: 687 that ended in racist violence and 444 spells that do not.

Another major methodological concern here involves unobserved heterogeneity. Because 130 counties in the data experience more than one event, the

durations we analyze are partly nested in counties and therefore not completely independent from each other, violating one of the basic assumptions of regression analysis. This violation is likely to introduce a downward bias in our standard errors. We follow Myers (2000) in solving this problem by including a variable that taps the history of racist violence for every *Kreis* by counting the number of previous attacks (RIOT HISTORY). In addition, we estimate standard errors without considering durations in the same county as independent from each other.⁵

County-Level Analysis: Independent Variables and Controls

We use the same data as in the previous analysis to tap bystander responses, but instead of taking national counts, we count the number of positive, negative, and overall bystander responses that took place in a specific *Kreis* during the previous 30 days (BRGEN, BRDISAPR, BRSUP). We dichotomized these variables to deal with skewness. In line with the central argument, we expect to find a positive relationship between violence and bystander responses, especially when these responses are positive.

To make sure that the relationship between bystander responses and subsequent waves of violence is not spurious, we control for several other factors that have proven to make local communities more conducive to the outbreak of racist violence. The controls are listed in Table 1. All data, unless indicated otherwise, are collected from the *1994 Statistical Yearbook for Germany* (Statistisches Bundesamt, 1994).⁶ A more detailed description of these variables can be found in Braun and Koopmans (2010). Two control variables that tap local POSs, however, deserve some explicit attention.

In addition to making predictions about the relationship between decision making and protest, POS theories also posit that a strong local representation of the anti-immigrant agenda of the extreme right in the parliamentary arena will dampen the rate of extraparliamentary violence against immigrants (Koopmans et al., 2005). We therefore hypothesize that the rate of extreme right violence will be lower in *Kreise* where the parliamentary extreme right is strong, and higher in *Kreise* where parties with a prominority program are strong. The percentage of votes in the 1994 elections for the main extreme right party, the *Republikaner*, is used to tap the strength of the parliamentary extreme right (EXRIGHT). The strength of the prominority agenda is measured by multiplying the separate percentages of votes for all nonextreme right parties (*CDU*, *SPD*, *PDS*, *FDP*, and *Grüne*) times their individual stance on multiculturalism and minorities (PROMIN). The data on percentages of votes have been collected from the election atlas.⁷ The separate party stances on multiculturalism have been retrieved from the party manifesto file (Budge, Klingemann,

Table 1. Control Variables County-Level Analysis.

Variables	Description	Predicted effect
UNEMPLOY	Unemployment rate	+
FOREIGNER	% foreigners ^a	+
DISLOCATION	Population dislocation: Sum migration in- and out-flow	+
EMIG	Emigration surplus	+
LIFEEX	Average life expectancy	—
EXRIGHT	% votes for the extreme right Republikaner party	—
PROMIN	% Votes × Prominority Statements	+
DIF	Diffusion: Number of attacks previous month	+
DIF/DIS	Diffusion (geographically weighted): Number of attacks previous month/inversed distance	+
POPULATION	Logged population size in thousands	+
CAPITAL	Capital city	+
SUMMER	Summer month	+
WEEKEND	Weekend day	+

^aUnfortunately, no local information about asylum seekers is available. However, they are very evenly spread across the country because by law, regions are obliged to take up asylum seekers in proportion to their population sizes. We also modeled an interaction term between proportion of foreigners and unemployment (Olzak, 1992). This had no effect and did not alter the other results.

Volkens, Bara, & Tanenbaum, 2001).⁸ It straightforwardly follows from POS theory that xenophobic violence should be more intense in *Kreise* where the *Republikaner* score low and promminority parties score high.

In addition to models with control variables, we also estimated models with fixed effects for *Kreise* to make sure we capture all unobserved variable bias at the local level. In these models, we excluded all variables that vary between *Kreise* but not over time and did not deploy a stratified model.

Media Bias

There is a possibility that the relationship between violence intensity and bystander responses is caused by media bias. Newspaper sources are likely to include more information on acts of violence that are unprecedented, extremely severe, or have some other form of symbolic value. This might affect the likelihood that a bystander response is recorded in the news story of such events. To

control for this, we also estimate models including measures that grasp the level of media attention (MEDIA ATT) for violent events during the preceding month in the data sources we relied on. In the time-series analysis, this measure consists of a Visibility Scale ($\alpha = .73$) constructed out of six items:

- the number of times a violent event got covered in the previous month
- the number of times a violent event appeared on the front page during the previous month
- the number of times an incident was accompanied by a photo during the previous month
- the number of casualties due to extreme right violence during the previous month
- the number of injuries due to extreme right violence during the previous month
- the number of arrests due to extreme right violence during the previous month

The Visibility Scale ($\alpha = .87$) for the *Kreis*-level analysis is constructed from the same six items. The only difference is that it measures the visibility of violent events in the previous 30 days for each *Kreis* separately. If some violent incidents are more important than others, it is likely that this gets reflected in the coverage of all their characteristics. Controlling for this information therefore partly deals with media bias.

Results

As a starting point, Figure 1 plots the monthly evolution of right-wing violence and bystander responses between 1990 and 1999. Most peaks in violence were preceded by an upsurge in bystander responses, suggesting that these responses indeed triggered the outbreak of violence.

To see whether bystander responses link political opportunities to violence, we first model in Table 2 ethnic competition, opportunity structure, and anti-immigrant attitude measures without bystander responses.⁹ Model 1 in the table confirms existing evidence that suggests that the elite conflict about constitutional immigration reforms shaped the evolution of right-wing violence. While political debates about immigration fueled ethnic violence, decisions resolving contentious issues had the opposite effect. In particular, decisions that restricted the rights of immigrants mitigated violence. This suggests that political reforms took away much of the frustrations and sentiments in which right-wing mobilization was rooted. Debates, however, made immigration issues more salient and sparked violence. Anti-immigrant sentiments among

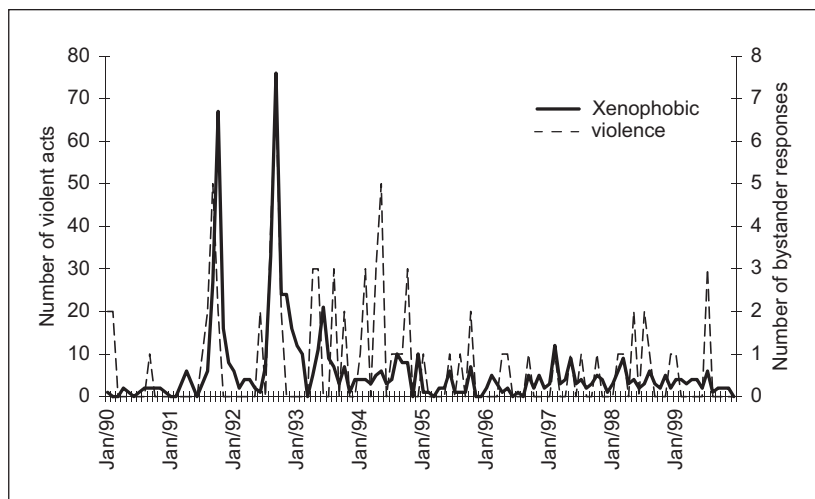


Figure 1. The monthly evolution of xenophobic violence and bystander responses in Germany from 1990 to 1999.

the population also affected the outbreak of violence. Violence targeting immigrants was more likely in time periods in which the general population considered immigrants a problem. Support is also found for ethnic competition theory. When combined, high unemployment rates and a large influx of asylum seekers promoted violence. Independently of each other, the two measures do not have an effect.

To investigate what role bystander responses play in transmitting information about the wider political context, we include the general bystander measure in the second model. The model provides suggestive evidence for the notion that bystander responses form conduits through which abstract political opportunities trickle down. The model shows that bystander responses are positively associated with the number of right-wing attacks. More importantly, however, after inclusion of the bystander measure, the effects of the opinion and repression variable decrease while the effects of negative political decisions and the political debate disappear altogether. This indicates that bystander responses indeed mediate between abstract opportunity structures and activism. In the third model, we see that the tone of the responses also plays a crucial role. Responses supportive of right-wing activists have a strong positive effect on the rate of violence while such an effect is absent for disapproving responses. Next to political decisions and debate, the effect of repression now also becomes insignificant.

Table 2. Poisson Autoregressive Model of Xenophobic Violence in Germany 1990 to 1999.

	Model 1		Model 2		Model 3		Model 4		Model 5	
	B	SE	B	SE	B	SE	B	SE	B	SE
BRGEN			0.265 ^{****}	0.086	0.815 ^{****}	0.277	0.191 ^{**}	0.105	0.628 ^{**}	0.342
BRSUP					331	0.211			0.289	0.216
BRDISAPR					0.000	0.002	0.001	0.002	0.000	0.002
ASYLUM	-0.002	0.002	0.000	0.002	-0.763	0.248	-0.359	0.243	0.152	0.247
UNEMPLOY	-0.2581	0.309	-0.078	0.245	0.010 ^{**}	0.005	0.009 ^{**}	0.005	0.009 ^{**}	0.005
AS x UNEM	0.011 ^{***}	0.006	0.010 ^{***}	0.005	-0.007	0.036	-0.010	0.037	-0.002	0.036
POSDEC	-0.019	0.045	-0.023	0.037	0.001	0.043	-0.008	0.041	-0.002	0.043
NEGDEC	-0.084 ^{**}	0.049	-0.008	0.042	-0.021	0.016	-0.017	0.017	-0.014	0.018
REPRESSION	-0.042 ^{**}	0.021	-0.027 ^{**}	0.016	0.006	0.004	0.004	0.004	0.004	0.004
DEBATE	0.012 ^{***}	0.005	0.006	0.004	2.497 ^{***}	0.999	1.932 ^{**}	1.078	2.092 ^{**}	1.083
ATTITUDE	2.907 ^{***}	1.260	2.338 ^{***}	1.016			0.151	0.129	0.140	0.127
MEDIA ATT.							1.147 ^{***}	0.199	1.020 ^{***}	0.205
INTERCEPT	1.130 ^{***}	0.255	1.041 ^{***}	0.186	0.894 ^{***}	0.186	0.374 ^{***}	0.109	0.333 ^{***}	0.107
AR	0.282 ^{***}	0.102	0.371 ^{***}	0.112	0.344 ^{***}	0.109				
N	118		118		118		118		118	
ADJ R ²	.422		.632		.667		.632		.681	

^{**} $p < .10$ (one-tailed). ^{***} $p < .05$ (one-tailed). ^{****} $p < .01$ (one-tailed). ^{*****} $p < .001$ (one-tailed).

Table 3. Poisson Model of General Bystander Responses (Model 6), Logit Model (Model 7) of Supportive Responses, and Corresponding Sobel Mediation Analysis.

	Model 6				Model 7			
	B	SE	ME	SE (ME)	B	SE	ME	SE (ME)
NEGDEC	−0.022	0.058	−0.259	0.257	−0.645**	0.365	−0.365*	0.247
REPRESSION	0.002	0.029	−0.008	0.093	−0.067	0.122	−0.101	0.395
DEBATE	0.022***	0.008	0.066***	0.026	0.104**	0.051	0.435**	0.229
ATTITUDE	2.445*	1.900	12.744**	6.867	6.471	9.698	0.159	0.370
OTHER CONTROLS	Y				Y			
INTERCEPT	−2.506****	0.381			−8.315****	2.674		
N	118				118			
Pseudo R ²	.151				.286			

ME = mediated effect.

p* < .10 (one-tailed). *p* < .05 (one-tailed). ****p* < .01 (one-tailed). *****p* < .001 (one-tailed).

Together, Models 2 and 3 provide support for our notion of trickle-down movement politics. Debates in the media created awareness of the immigrant problem among the general population, which got translated into bystander activities, which in turn encouraged activists to conduct more attacks. In a similar vein, the mitigating effect of restrictive immigration measures disappears when we introduce bystander reactions, suggesting that supportive bystander responses became rarer as a result of these decisions, resulting in less xenophobic mobilization. Repression lowered the legitimacy of extreme right mobilization, but this effect too seems to have been mediated by a decrease in supportive bystander responses. In Models 4 and 5, we include the media attention measure to deal with coverage bias. Although inclusion of this measure, which itself does not attain statistical significance, decreases the size of the bystander effects, they remain significant. All the other relationships remain intact.¹⁰

To get further purchase on whether bystander responses indeed mediate the effects of political opportunities, we conducted an analysis in which we model bystander responses as a function of the environmental measures that were associated with the outbreak of violence in the earlier models before the bystander measures were included. If bystander responses operate as a trickle-down mechanism, one would expect that their intensity is shaped by these other environmental factors. We also conducted a mediation analysis to see whether the mediated pathways of these opportunity structure variables through bystander responses are statistically significant.

Table 3 presents the results of these analyses. Models 6 and 7 show the relationship between political context variables and bystander responses.

Both negative decision making and intense public debate affected the likelihood of supportive bystander responses. Whereas decision making dampens these responses, the opposite is true for media debates, which made the immigration issue more salient and problematic in the eyes of the general public. In line with this idea, we see that public opinion also shaped the overall intensity of bystander responses, although it does not directly affect the probability of supportive responses. Repression, however, had no effect on bystander responses, suggesting that the effects of repression do not trickle down through bystander interactions. In a way, this makes sense because repressive state activities are likely to be picked up by activists directly as they have an immediate impact on their lives.

As bystander responses in turn intensified mobilization, this provides evidence that bystander responses mediated the effects of political decision making, public opinion, and media debates. The mediated effects, based on Sobel tests with bootstrapped standard errors,¹¹ displayed in Table 3, represent the degree to which specific political context variables affected violence via bystander responses. They indeed confirm that the effects of public opinion, decision making, and debate shaped violence via bystander reactions.

We now move to the second part of our analysis, where we ask whether bystander responses also determine *where* violence spreads. We do this by regressing the local outbreak of violence on local bystander responses. Table 4 presents the results of the stratified Cox regression. The coefficients represent hazard ratios, the hazard of a particular case divided by the hazard of a case that scores 1 point lower on the relevant covariate. Hazard ratios are preferred over regular coefficients because they allow for a more straightforward interpretation: A hazard ratio of 1.100 indicates that a 1-point increase in the independent variable increases the violence hazard by 10%. In the first model, we include the measure that counts the number of bystander responses in a specific *Kreis* during the previous month, while controlling for other factors that tap the conduciveness of *Kreise* to violence. In line with our earlier results, Model 8 in Table 4 shows that the likelihood that a violent incident occurs is higher in counties in which bystander publics have openly responded to previous acts of violence. A bystander response in the previous month increases the violence risk by almost 28%. This provides evidence for the notion that bystander responses not only determine when political opportunities are revealed but also *where* these opportunities become manifest to activists.

In Model 9 of Table 4, we investigate the separate effects of supportive and disapproving bystander responses. Disapproving responses have a negative but insignificant effect on violence. Supportive responses have a very strong positive effect on the outbreak of violence. The hazard rate is almost

Table 4. Cox Regression of Instances of Antiforeigner Violence in Germany 1990-1995.

	Model 8			Model 9			Model 10			Model 11		
	Hazard ratio	Clustered SE		Hazard ratio	Clustered SE		Hazard ratio	Clustered SE		Hazard ratio	Clustered SE	
BRGEN	2.281***	0.713					2.198***	0.597		5.486***	3.161	
BRSUP				4.790***	1.730					0.929	0.399	
BRDISAPR				0.909	0.409							
UNEMPLOY	1.014	0.018		1.013	0.017							
FOREIGNER	0.985	0.022		0.985	0.022							
DISLOCATION	1.602***	0.209		1.617***	0.222							
EMIG	4.436***	2.479		3.769***	2.147							
LIFEEX	0.864***	0.043		0.868***	0.042							
EXRIGHT	0.862***	0.075		0.858***	0.075							
PROMIN	1.055***	0.020		1.055***	0.020							
DIF	1.021***	0.002		1.020***	0.002		1.023***	0.002		1.022***	0.002	
DIF/DIS	1.000	0.000		1.000	0.000		1.000	0.000		1.000	0.000	
POPULATION	1.848***	0.205		1.837***	0.199							
CAPITAL	1.266	0.191		1.307***	0.199							
SUMMER	1.333***	0.136		1.307***	0.135		1.154	0.107		1.155	0.107	
WEEKEND	2.220***	0.209		2.217***	0.208		2.166***	0.184		2.158***	0.184	
RIOT HISTORY	1.008	0.014		1.010	0.013							
MEDIA ATT							1.034	0.096		1.069	0.108	
FIXED EFFECTS							Y			Y		
LOG-PSEUDO-LIKELIHOOD	-3471.375			-3468.118			-3510.051			-3518.345		
SPELLS	1131			1131	1131		1131			1131		
FAILURES	687			687	687		687			687		

*** $p < .05$ (one-tailed). *** $p < .01$ (one-tailed). **** $p < .001$ (one-tailed).

5 times higher in *Kreise* where in the previous month bystanders responded supportively to racist attacks.

The model also sheds light on other local-level predictors of violence. Population size, being a capital city, high migration flows, net population losses due to emigration, and the strength of the proimmigrant agenda increased the violence rate, whereas areas with a higher life expectancy and strong extreme right parties experienced fewer xenophobic events.

The number of violent events during the previous 30 days in the rest of Germany, which taps general diffusion processes, has a strong and positive influence on subsequent rates of antifoiegnr violence in a particular *Kreis*. This diffusion process is not clustered in space because the geographically conditioned diffusion term has no separate effect on violence. Finally, the model demonstrates that violence is more likely during summer and weekend days.

In Models 10 and 11 in Table 4, we include the visibility measure and *Kreis* -level fixed effects to deal with media and omitted variable bias. The results remain remarkably similar. Most importantly, the positive and significant coefficients of the bystander parameters remain intact. Therefore, we can be more confident that unobserved variable bias or selection effects do not cause our findings.¹²

Conclusion

Students of social movements have long struggled with the question how abstract political opportunities, such as elite division or electoral competition, influence activists. We have argued that the relationship between institutional opportunities and mobilization may take the form of trickle-down politics. In this view, activists are affected by political opportunities indirectly through the changes that political developments bring about in the immediate social setting of protest. To investigate this idea, we focused on the ways in which local bystander publics transmit information about the wider political context to unorganized extreme right activists without much awareness of national politics. The empirical analysis indicated that temporal fluctuations in opportunity structures and public sentiments affected youths and skinheads after they received positive feedback from local bystanders. This suggests that bystander responses play a crucial role in how political opportunities become manifest.

Interestingly, while the movement studied here reacts strongly to favorable reactions from its social environment, it seemed to be relatively immune to negative reactions. Disapproving reactions of bystanders did not lead to significant decreases in extreme right mobilization. A possible explanation for this phenomenon, which would require further investigation, can be taken

from the psychological theory of cognitive dissonance, which states that people's perceptions tend to be biased toward that part of social reality that is consonant with their prior views (Festinger, 1954).

Some of the components of our argument might be specific to the case at hand. Not all movements are constituted by low-educated and weakly organized groups that lack profound knowledge of political affairs. One should, however, not draw the opposite conclusion that the extreme right movement in Germany is an exceptional case. Research on recent waves of anti-Muslim violence in Western Europe also suggest that perpetrators have little interest in, and knowledge of institutional politics (Van Donselaar & Rodriguez, 1998). Moreover, local interactions are likely to play a role in more "sophisticated" movements as well. Although a large part of the interaction between these social movements and political institutions consists of mediated encounters in the mass media, the importance of direct physical encounters with local audiences should not be underestimated. Several studies on political communication suggest that media content only matters if it resonates with information retrieved from extramedia sources (Robinson, 1976). Media coverage on political opportunities is therefore more likely to have an effect if direct audience feedback during demonstrations confirms its central message. Our results provide strong evidence that social movement activists follow Schattschneider's advice to "watch the crowd." Scholars of contentious politics will get a better understanding of the mechanisms linking institutional politics, public opinion, and social movements if they do the same.

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Notes

1. This mechanism comes close to what they call *certification*. The key difference is that activism is not validated by authorities but by citizens.

2. In an additional analysis, we included countermobilization as a further control variable. This did not alter the results.
3. This is different from the 692 events used in the time-series analysis because we analyze different data sets and time periods.
4. Whether the xenophobic mobilization studied in this article indeed took place in two completely different social settings can be checked by inspecting how the hazard of xenophobic violence evolved over time in the two regions separately. The estimated cumulative baseline hazard functions for East and West Germany show that xenophobic violence indeed evolved differently in both regions.
5. Using the cluster option in STATA 9.
6. Statistics for East German *Kreise* were not immediately available after reunification in October 1990. In addition, in the years immediately after reunification, many borders of East German *Kreise* were redrawn. We were therefore constrained to measure many independent variables time invariant, for the 1st year for which data are available after the redrawing of *Kreis* boundaries, that is, 1994. For some variables of potential interest—such as the local gross domestic product—data are incomplete for the entire period of the study. We therefore had to exclude these variables from the analysis.
7. www.wahlatlas.de
8. The stance is calculated by subtracting all negative quasi sentences on multiculturalism from the sum of all positive quasi sentences on multiculturalism plus all positive quasi sentences on minorities in the specific party programs.
9. Note that due to the lagging of the independent variables and the additional use of a difference score for the growth in the number of asylum seekers (see above), we lose 2 months at the beginning of our period, reducing the number of months analyzed from 120 to 118.
10. As a robustness check, we investigated whether our results might be affected by some unobserved characteristics of the events during the crucial periods of September 1991 and from August to September 1992, which saw peaks in violence and bystander responses. To investigate this, we ran models with dummies marking these time periods. However, the effects of bystander responses in this analysis turned out to become even stronger (general bystander responses: coefficient = .300, p value = .001; positive bystander responses: coefficient = 1.317, p value = .000).
11. We conducted 500 bootstrap replications.
12. We also estimated models with dummies that marked the infamous September 1991 and from August to September 1992 periods (see Footnote 10). The effects were in line with the presented model (general bystander responses: coefficient = 2.07, p value = .02; positive bystander responses: coefficient = 4.339, p value = .003).

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