A POLITICAL THREAT MODEL OF INTERGROUP VIOLENCE: JEWS IN PRE–WORLD WAR II GERMANY

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Research often links minority group size and economic conditions with levels of intergroup violence, in line with facets of group threat and structural theories of intergroup crime. Building on the group threat perspective, we investigate the political antecedents of intergroup violence. This work tests the theoretical premise that violence against minority groups increases with the strength of political parties associated with minority group interests, independent of group size and economic conditions. This model is tested empirically for the case of violence against Jews in pre–World War II Germany, where Jews constituted a small proportion of the German population but were often associated with the leadership of the political left. Findings suggest that the gross domestic product and Jewish population size did not have predicted effects on major violent incidents against Jews. It was in fact the rising strength of leftist political parties that ignited anti-Semitic

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violence. Other venues where this model could be applied are proposed, and the findings are discussed in the context of intergroup violence and theories emphasizing minority group threat.

Research on intergroup conflict often generates hypotheses from Hubert Blalock’s (1967) theory of intergroup relations. Blalock’s model, often associated with a group or power threat perspective, maintains that intergroup conflict is a product of dominant group desires to hold power and suppress perceived threats from minority groups. Minority group threat is largely manifested through two key factors: minority group size and economic conditions. In line with this framework, existing research suggests a correlation between economic conditions, minority group size, and intergroup crime and violence. For instance, deteriorating economic conditions increased levels of southern lynching in the United States (Corzine, Huff-Corzine, and Creech, 1988; Hovland and Sears, 1940; Tolnay and Beck, 1995), anti-Semitism in Europe (Brustein, 2003), and ethnic conflict in the United States around the turn of the nineteenth to the twentieth century (Olzak, 1989). Variation in intergroup crime and violence is also associated with shifts in minority group size. Interracial hate crimes, for example, increase with influxes of minority groups into previously homogenous areas (Green, Strolovitch, and Wong, 1998). That finding is consonant with historical research suggesting that immigration ignited conflict between whites and ethnic minorities during labor disputes (Olzak, 1989, 1992).

Whereas the threat perspective makes assumptions about the motivation behind intergroup violence, structural explanations of intergroup crime and violence in the tradition of Peter Blau (1977a, 1977b) emphasize opportunity structure and the probability of intergroup contact. Although not sharing the assumptions of group threat models, structural accounts also propose that intergroup crime and violence increase with racial or ethnic heterogeneity (Messner and South, 1992; O’Brien, 1987; Sampson, 1984; South and Messner, 1986).

Less pronounced in extant research is an explicitly political model of intergroup violence. Although Blalock (1967) explains the role of political threat, his model largely assumes that minority group size indicates the capacity for political mobilization. We offer an alternative conceptualization of political threat, akin to political threat models advanced in recent research on homicide (Jacobs and Wood, 1999). Rather than viewing violence as a function of minority group size and economic conditions, we emphasize the strength of political movements associated with minority groups as a precursor of intergroup violence. We thus align this model in the group threat tradition (Blalock, 1967), yet challenge conceptualizations of threat based solely on economic and demographic
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variation. We view intergroup violence as partly reactionary, or as a means of informal social control (Black, 1983), that often entails political motive. We test this supposition using data on violence against Jews in pre–World War II Germany.

PERSPECTIVES ON INTERGROUP VIOLENCE

Arguably the most consistent correlates of intergroup violence are group heterogeneity and sudden fluctuations in minority group size. The association between ethnic heterogeneity and violence is largely explained in the context of two theories that make different assumptions concerning motive. One such theory, in the tradition of Blau’s (1977b) “primitive theory” of social structure, suggests that the opportunity for intergroup crime increases where there is greater heterogeneity. Such structuralist accounts make no assumptions concerning motivation, but rely instead on the axiom that intergroup association of any kind is not feasible where no heterogeneity exists. To that end, racial or ethnic heterogeneity is directly associated with the probability of both affectionate and conflict-laden intergroup contact. For instance, both interracial marriage and interracial crime are more likely in racially heterogeneous social settings (South and Messner, 1986). Related work further supports this thesis, finding a positive correlation between racial heterogeneity and interracial crime (McCall and Parker, 2005; Messner and South, 1992; O’Brien, 1987; Sampson, 1984; Tremblay and Tremblay, 1998).

Whereas structuralist accounts draw attention to opportunity structure and the probability of intergroup contact, other explanations of intergroup violence make explicit assumptions concerning motive. A rich literature in this domain suggests that intergroup conflict and its manifestation in violence is a function of perceived threats from out-groups and intergroup competition for scarce economic and political capital. This body of work is largely consistent with theories of crime that highlight group conflict (Turk, 1969) and minority group threat perspectives on intergroup relations (Blalock, 1967) and prejudice (Blumer, 1958). Explanations of intergroup violence as reactionary violence cite perceived threats from other groups as the lynchpin linking heterogeneity and violence. In line with such threat and competition arguments, interracial violence is a function of ethnic heterogeneity. The number of blacks in an area, for instance, was positively associated with lynching in the American South (Tolnay, Deane, and Beck, 1996). Related work adds that rising competition resulting from mass immigration incited interracial violence (Olzak, 1990). Green, Strolovitch, and Wong (1998) find that increases of minority group members into previously homogenous settings yields greater risk of interracial hate crime offending (for related argument, see
Glaser, Dixit, and Green, 2002). Other research suggests that interracial rioting in the United States was a product of histories of interracial strife and increases in residential contact between blacks and whites (Olzak, Shanahan, and McEneaney, 1996). The underlying assumption pervading this literature is that violence is a mechanism for maintaining social control or expressing a grievance against members of a different, and competing, ethnic or racial group. To wit, violence is a response to group threat.

In addition to out-group size, economic conditions and competition for scarce economic resources are also important factors in the group threat thesis. To this end, economic competition is associated with interracial violence in the United States (D’Alessio, Stolzenberg, and Eitle, 2002; Jacobs and Wood, 1999; Olzak, 1989; McCall and Parker, 2005). Heinous violent acts, such as lynching, also fluctuated with economic conditions, particularly changes in the cotton market (Hovland and Sears, 1940; Olzak, 1990; Tolnay and Beck, 1995). Again, the threat perspective suggests that deteriorating economic conditions, particularly where groups compete for employment within the same sector (niche overlap), pose a threat to ethnic group interests that begets violence.

Regardless of whether economic conditions or minority group size is specified, an underlying theme in conflict explanations of violence is that competition, as reflected through the capacity for collective action or perceived threats to employment and economic capital, fosters intergroup violence. A third factor, the political efficacy of minority group interests, increasingly receives attention in research on violence and intergroup conflict (D’Alessio, Stolzenberg, and Eitle, 2002; Jacobs and Wood, 1999; Olzak, 1990). Building on that body of work and the group threat framework, we articulate an explicitly political model of intergroup violence.

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As Jacobs and Wood (1999: 158) aver, “many neo-Marxists and some Weberian conflict theorists see crime as inarticulate political resistance against unequal social arrangements.” In line with their assessment, the literature on intergroup violence includes theoretical and empirical assessments of the political antecedents of violence. The political threat model implies that violence is a reaction to either perceived loss of political clout by majority groups or a form of grievance expressed by minority groups to vent feelings of alienation and political powerlessness. Partly in response to influential work on the role of politics in the study of race relations (Bobo and Hutchings, 1996; Wilson, 1978), criminological
research increasingly entertains the political threat hypothesis but reaches different conclusions. D’Alessio, Stolzenberg, and Eitle (2002), for instance, measure political threat via the ratio of black-to-white votes cast in a state election but they find no significant correlation between black voting and interracial violence. Related research employs similar measures of political competition to generate hypotheses relative to political competition and violence. Olzak (1990), for example, suggests that lynching blacks was more frequent when the Populist political movement challenged the established white supremacy in the South. Challenges to the existing political power arrangements, Olzak maintains, elicited perceptions of threat among dominant group members. Violence became a tool for suppressing such threats and, hence, electoral competition was a fundamental mechanism for triggering intergroup violence.

Both Olzak (1990) and D’Alessio, Stolzenberg, and Eitle (2002) operationalize “political challenge” as the mere presence or absence of an election or the prevalence of voting by minority groups, respectively. An alternative and potentially viable indicator of political threat is the electoral success of candidates associated with promoting minority group interests. This informs Jacobs and Wood’s research (1999) on the causes of interracial homicide (see also McCall and Parker, 2005). Building on Wilson’s assessment (1978) that interracial conflict has shifted away from the economic sector and into the sociopolitical realm, Jacobs and Wood explore how political leverage accruing from minority group success in municipal elections influences interracial violence. They find empirical support for their thesis that the presence of a black mayor yields elevated levels of white-on-black killings yet mitigates black-on-white killings. Hence, minority group political power elicits reactions from majority groups as expressed through violence. Minority political power, or the perception of it, has the potential to instigate intergroup violence.

This research builds on the burgeoning political threat thesis and uses Blalock’s (1967) seminal work on minority group relations as a starting point for our model. According to Blalock, prejudice results from dominant group perceptions that minority (or subordinate) groups will gain power at the expense of the dominant group. Prejudice—as expressed through voting suppression (Matthews and Prothro, 1963), judicial decision making (Vines, 1964), negative attitudes (Quillian, 1995) or formal social control (Liska, 1992)—thus increases with minority group size. As it grows in size, the minority population may gain political leverage that threatens existing power balances and incites reactions from state (Behrens, Uggen, and Manza, 2003) or nonstate (Tolnay and Beck, 1995) entities.

1. McCall and Parker (2005) find less empirical support for this association.
An implicit assumption in Blalock’s theory is that minority groups endeavor to shift the balance of power in society to their advantage. That is, dominant groups generally perceive the minority population as poised to challenge existing power arrangements. We do not challenge this assumption on empirical grounds, but use it to generate two theoretical extensions of Blalock’s theory. First, it follows that minority group size is not related to intergroup conflict if the subordinate group is content with the status quo and is not associated with a political agenda different from that of the dominant group.

Consider the following hypothetical example. Let Group M represent the minority population and Group D represent the dominant population. If Group M expresses no discontent with its subordinate status, then changes in the size of Group M would theoretically yield no substantive effect on the attitudes or actions of Group D. This hypothetical situation rarely, if ever, exists in the empirical world, but it begets a second and more consequential proposition. Specifically, it follows that if no change in minority group size occurs but a political movement associated with the minority group gains strength, intergroup conflict will ensue. Behrens, Uggen, and Manza (2003) produce a related argument in their work on felon disenfranchisement legislation. According to their political threat model, the sizeable increase in restrictive felon disenfranchisement legislation did not follow a sudden increase in black population size. Rather, such laws coincided with the constitutional enfranchisement of blacks via the Fourteenth and Fifteenth Amendments. It was the change in potential political leverage as opposed to the subordinate group size alone that dictated the implementation of disenfranchisement laws. Expanding on the concept of political threat, we propose that, independent of minority group size, the perceived success of political movements associated with the minority group increases intergroup conflict and violence.

It is this latter proposition that we assess in this study, focusing on violence against Jews in pre–World War II Germany. We view this time and place as an intriguing case for understanding group dynamics and intergroup violence because Jewish population size fluctuated only slightly during this period and never reached a proportion of the population large enough to constitute a viable threat according to classic power threat models (Blalock, 1967). Yet this period witnessed tremendous gains by leftist political movements, notably in Eastern Europe (the Bolshevik Revolution) but also in countries holding periodic elections, such as Germany. The rise of the political left during this period is important for our purposes because Jews were frequently perceived as leaders of leftist

2. The Jewish population in Germany rarely exceeded 1 percent of the population.
political movements in the Western world (Novick, 1999). Numerous press reports in the West claimed that Jews were overrepresented in the leadership of the Bolsheviks and the Communist Party. For instance, in the infamous “Jewish Peril” article published on May 8, 1920 in the prestigious *Times of London*, Jews were linked to worldwide subversive activities. A series of articles in the popular British *Morning Post* also warned its readers that the Russian Revolution was part of a global plan hatched by international Jewry to dominate the world and alerted its readers to the purported dangers of spreading Bolshevism. In 1920 the *Morning Post* published a list of the fifty key Bolshevik leaders, claiming that forty-two of the fifty were Jews (see Brustein, 2003, chapter 5). A related discourse concerning Jews and leadership of leftist parties pervaded the United States (Beim and Fine, 2006; Brown, 1942; Glazer, 1961: 220–21; Novick, 1999: 92; Szajkowski, 1972) and, important for our case, the German state. A large proportion of the German population believed that Jews were responsible for the so-called Bolshevik threat, particularly after 1917. Media publications pointed to the disproportionate number of Jews in the German Communist movement in both Berlin and Munich at the end of World War I—Luxemburg, Eisner, Jogiches, Levine, Landauer, Toller (see Kauders, 1996). To that end, and particularly in the aftermath of World War I, German newspapers were critical of Jewish revolutionaries for instigating labor unrest (Brustein, 2003: 269; Kauders, 1996: 56–58, 67, 77; see also Novick, 1999: 92). Such claims were also fueled by the Jewish heritage of intellectuals and activists such as Karl Marx, Ferdinand Lassalle, and later Leon Trotsky in his leadership of the Red Army in Russia. For many Europeans, the threat of communism was associated with a threat to the existing social, economic, and religious order. Many anti-Semites cited Jews as the founders of revolutionary socialism and anarchism and saw the hand of Jews in periodic labor disputes (Kauders, 1996). The purported association between Jews and the leftist movement gained footing with the ascension of the German Social Democratic Party, which increased its representation in the early twentieth century and included a number of Jewish representatives ten times their proportion in the general German population (Brustein, 2003: 289). Such representation was fodder for right-wing outlets that spread propaganda about the conspiring tendencies of “international Jewry” and the “Jewish election” (Judenwahlen) of 1912, where the political left subsequently gained considerable strength (Sperber, 1997). Partly as a consequence of media attention linking Jewish leadership of the Communist and related leftist parties, in the minds of a large proportion of the general population there was a strong belief that Jews were responsible for the communist threat after 1917 (Novick, 1999), in Germany as in other parts of the Western world.
Because of the confluence of the perceived association between leftist politics and Jews and the periodic success of leftist parties in the late nineteenth and early twentieth centuries, we expect a correlation between violence against Jews and the electoral strength of leftist parties. We specifically test the hypothesis that violent acts against Jews increased with electoral support for the political left, net of economic conditions and size of the Jewish population.

DATA, VARIABLES, AND METHODS

To analyze temporal variation in violence against Jews before World War II in Germany, we systematically examined the volumes of the American Jewish Year Book (AJYB), which has been published annually since 1899. Containing a section dedicated to summarizing leading news events of the previous year from around the world, it also includes information concerning special religious events, laws that concern Jewish affairs, and accounts of discrimination or violence against Jews. We derive our information from records of violent victimization. Specifically, we read the annual accounts of Jewish incidents in Germany between 1899, the first year of the volumes, and the onset of World War II in 1939. The coding began with a research assistant reading through all relevant volumes of the AJYB and coding acts that entailed Jewish violent victimization. This coding included a short description of incidents and quotes from the AJYB text. To increase reliability in the coding, two researchers who had received identical training on the coding read through the forty-one volumes again, each researcher alternating years. The task was first to resolve any questionable entries that the first reader had noted and to code any additional incidents that the first reader had overlooked. The second reading did yield a few additional incidents. In the event of uncertainty, the project director and the coders would meet to discuss the most appropriate coding for the entry. The coding of major violent incidents yielded few discrepancies.

When reading these accounts from the AJYB, the coders noted all violent incidents entailing Jewish victims and sorted them by year. The recorded acts largely refer to significant, large-scale incidents, and many include multiple victims. Minor acts such as insults or attempted attacks, akin to what might be recorded in contemporary hate crime statistics, were less frequently reported. Our analysis is thus of major violent episodes during the period. We included both violent acts against persons and forceful and destructive acts against Jewish property, such as synagogue arsons. Examples of coded events cited in the AJYB include:

- “Munich: anti-Semites and Fascists attack Jews and wound many” (1923)
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- “On Yom Kippur, several hundred Hitlerites attacked worshipers leaving synagogue at Oppenheim” (September 24, 1928)
- “Berlin: University students armed with clubs raid Jewish section. Many injured and police arrest six of the ringleaders” (February 27, 1921)
- “Jewish community completely wiped out; synagogue burned and fourteen scrolls burned in Segenheim” (April 2, 1915)
- “Synagogue entered by force and damaged in Dusseldorf” (1928)

In all, our coding revealed sixty-two such incidents during the 41 years of interest. As illustrated in figure 1, the prevalence of incidents included many peaks and lulls in violent activity.

Although the AJYB is a rich source of information on Jewish affairs, it shares the limitations of newspaper narratives (Danzger, 1975; Franzosi, 1987; Olzak, 1992, chapter 4). The reports of events were sent to the editors by local and national Jewish organizations and the accuracy of those reports might raise concerns about measurement validity. Several facets of our research, however, strive to address the issue. For instance, reporting biases are likely less problematic for within country analyses over time relative to cross-national comparisons, where Jewish organizations are organized quite differently. Furthermore, had we investigated less severe acts, such as slurs made against individual Jews or minor altercations between Jews and other groups, this source would be far less valid. However, it is likely that major instances of Jews or Jewish organizations being attacked would garner the attention of the community and surface in the year book of significant events. Snyder and Kelly (1977) cite the intensity of an event, measured by its size, violence, and duration, as a major criterion that increases the likelihood of valid event reporting in periodicals (see also Franzosi, 1987; Kielbowicz and Schere, 1986; Olzak, 1992; Tilly, 1968). To that end, we isolate our cases to violent events that typically entail multiple victims. In addition, Snyder and Kelly’s assessment of historical data garnered from periodicals suggests that the occurrence of an event is reported with greater validity and reliability than the descriptive characteristics. We thus limit our dependent variable to the presence of violent incidents, as opposed to identifying the characteristics of perpetrators or the number of victims in a given incident through

3. Cross-sectional counts from periodicals appear particularly subject to biased reporting (Snyder and Kelly, 1977: 110).
4. As Franzosi (1987: 7) observes, reporting error decreases as event magnitude increases.
5. Snyder and Kelly (1977) also cite media sensitivity as a key factor in assessing the validity of event counts in historical periodicals. On that regard, we consider the American Jewish Year Books as highly sensitive to violent events having an impact on the Jewish community.
accounts in the AJYB. Hence, we are mindful of the limitations of historical data that precede systematic state collection of data on crime and violence. We nonetheless argue that the information contained in the AJYB is the most useful, and perhaps the only data available to assess the frequency of violence against Jews before the Holocaust.

Garnering data from multiple data sources with information on violence against Jews would provide a reliability check on our data. We thus sought independent data sources for precisely that purpose. Jewish newspapers within Germany were one possibility, yet such comparison would not shed light on reliability because the AJYB had garnered some of its information from local Jewish newspapers. That is, Jewish periodicals would thus not be truly independent. Comparisons with widely circulating German newspapers were also suspect because such outlets unlikely reported on violent incidents against Jews if the editors themselves expressed prejudice against Jews or if the state exerted control over the press (post–1933 censorship, for example). Hence, there is no singular source against which we can compare our data, which partly speaks to the novelty of our data.

The only independent sources useful for comparison are accounts from historical publications, though we can identify no sources that systematically chronicle violent incidents for the period from 1899 to 1939. However, historians have given considerable attention to the Hitler era, and Fronzosi (1987) suggests that comparing the validity of data for a subset of events is often the only available test of reliability in historical research. Historians writing on violence against Jews suggest that sizeable violent anti-Jewish pogroms occurred at two times between 1933 and 1939—in 1935 and again in 1938 (Hagen, 1996). These respective peaks in violence clearly align with our count from the AJYB (see figure 1). Faced with a dearth of comparable data sources, we find apparent congruence between our count and that of historians for at least one subset of our data.

**INDEPENDENT VARIABLES AND MODELING**

Our analysis of leftist political strength and violence against Jews statistically controls for several variables, including economic conditions, Jewish population size, the presence of an election, Hitler's period of control in Germany, and lagged measures of violence. We measure economic well-being by the gross domestic product (GDP). Specifically, we measure GDP as total GDP per capita for a given year, expressed in units of 1,000.\(^6\) Units are 1990 dollars and drawn from Maddison (1995)

\(^6\) To reduce decimals in the regression model, we divide the GDP per capita measure by a constant of 100.
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and Good and Ma (1998). Our measure of Jewish population is the percentage of Jews residing in Germany in a given year, using AJYB data. Estimates of the Jewish population were not updated yearly, thus we interpolate population estimates for years in between counts. Specifically, counts of the Jewish population were furnished in the AJYB for only 12 years. We assume linear increases or decreases between counts. The largest span requiring interpolation was 6 years. Using alternative estimates, such as carrying over estimates from the previous available data point, had no substantive bearing on the results. We also add that fluctuations in the size of the German Jewish population were very small throughout most of the period and generally hovered around the mean of .91. The only notable decrease followed the exodus of many Jews following Hitler’s election, and a subsequent increase in Jewish population size in 1939 due to the annexation (Anschluss) of Austria.

We measure our focal independent variable, leftist party electoral success, using the percentage of all votes cast for leftist parties in Germany—Social Democrats, Independent Social Democrats, and Communist Parties. For the period of our study, each of these parties called for the German economy to be transformed from private ownership to socialized production (Milatz, 1965; Mommsen, 1952). In off-election years we carried over the electoral results from the previous year. A dummy variable indicating an election year is also included in the analyses in light of an alternative hypothesis holding that the mere presence of an election, rather than the outcome, yields politically motivated violence (Olzak, 1990). We lag that variable by one year because elections can be late in the calendar year (for example, September 1930). We also include a dummy variable for the period from 1933 to 1939. Anti-Semitism took on a qualitatively different character after 1933 and emanated from the state more so than in earlier years. Descriptive statistics for dependent and independent variables are provided in table 1.

ESTIMATION

Our outcome variable is positively skewed and includes several zero counts, making Ordinary Least Squares (OLS) regression problematic. The distribution of violent acts against Jews in pre–World War II Germany is better suited for a negative binomial estimator. The negative

7. These were 1899, 1902, 1903, 1908, 1909, 1913, 1920, 1922, 1923, 1928, 1935, and 1939.
8. Alternative estimates are available from the authors on request.
9. Our substantive results with respect to leftist voting did not change when omitting this dummy variable for the period from 1933 to 1939 or when analyzing only the period from 1899 to 1932. The latter results are reported below.
binomial model is often employed in the study of infrequently occurring events, such as homicides (see Messner, Baller, and Zevenbergen, 2005; Osgood, 2000). Negative binomial regression models include a parameter that allows the conditional variance of the dependent variable to exceed the conditional mean, thus providing more efficient and less biased estimates than Poisson and OLS models. Like the Poisson model, negative binomial coefficients can be interpreted as the percentage change in the expected count of the dependent variable per unit change in the independent variable (Long, 1997) by taking the anti-log of the coefficient. In all models we include the raw number of Jews living in Germany in a given year as an exposure factor to account for opportunity structure. Finally, all models specify robust standard errors (Huber-White sandwich estimator; see Rogers, 1993) that correct for deviations from the assumed probability distribution (estimated using Stata 9.2).

Table 1. Descriptive Statistics

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>(SD)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dependent variable</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Violent acts against Jews</td>
<td>1.51</td>
<td>(2.20)</td>
</tr>
<tr>
<td>Focal independent variables</td>
<td></td>
<td></td>
</tr>
<tr>
<td>GDP</td>
<td>36.00</td>
<td>(6.19)</td>
</tr>
<tr>
<td>Percentage of Jews in general population</td>
<td>.91</td>
<td>(.09)</td>
</tr>
<tr>
<td>Electoral support for leftist parties</td>
<td>33.70</td>
<td>(4.73)</td>
</tr>
<tr>
<td>Election year (lagged)</td>
<td>.25</td>
<td>(.44)</td>
</tr>
</tbody>
</table>

Note: Variables cover the 41 years from 1899 to 1939.

Dealing with time series data can result in significant autocorrelation, which in turn may yield inflated t-values. Although OLS is not suitable for the distribution of our dependent variable, we nonetheless make limited use of an OLS model to permit a Durbin-Watson (DW) test for serial correlation in our model (not shown here). The DW test detected significant autocorrelation for the full time period, though not for the pre-Hitler era (1899 to 1932). We thus specify a set of generalized linear models (GLM) with a negative binomial estimator. GLM models enable us

10. Our results replicated when using a zero-inflated negative binomial estimator that specifies only an intercept in the equation that determines whether the count is zero. Those results are available from the first author on request.

11. Subsequent tests suggested that autocorrelation was no longer significant when 1- and 2-year lagged dependent variables were both included in the model. Although we largely rely on the Newey-West correction of the standard errors to address the problem of autocorrelation, we also show the results of negative binomial models that include 1- and 2-year lagged dependent variables.
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to use the Newey-West correction to provide consistent estimates of the standard errors in the presence of autocorrelation (Newey and West, 1987). Several models also include lagged measures of violence against Jews to further control for potential influences of serial correlation. Although including lagged measures limits the analysis to yearly incremental change in violent acts and reduces the sample size by the number of lagged measures, we note that omitting the lagged variables did not change the substantive results with respect to leftist electoral success and violent acts against Jews.

A related issue with time series analysis concerns nonstationarity, which can result if the dependent and independent variables move in the same direction over time due to an unmeasured factor. We thus statistically control for year in our analyses. There is some debate in the econometrics literature concerning the use of time as a predictor variable in regression models as opposed to using first differences (that is, assessing the change in the outcome variable \( Y_t - Y_{t-1} \) as predicted by a change score in the independent variable). A unit root test is useful for determining whether the model assumes a trend stationary or difference stationary process (see Raffalovich, 1994: 495ff on unit root; for a useful discussion and application in criminological research, see O’Brien, 1996). We assume a trend stationary process in our models for a combination of methodological and theoretical reasons. Methodologically, the null hypothesis of unit root was rejected for the outcome variable (violent acts) when employing the Dickey-Fuller and the related Phillips-Perron tests for unit root. Following Raffalovich (1994), rejecting the null hypothesis suggests a trend stationary process. Still, our theoretical justification is more important. First differencing our leftist voting independent variable would be problematic for two reasons. First is that elections are not yearly and we carry over the previous level of leftist electoral support in off-election years. Because there is no risk of change, first differencing would thus yield several zeros in off-election years. This relates to our second point, which is that the level of leftist support is critical. For example, leftist electoral support was sizeable in 1923, leading us to expect elevated rates of violence against Jews. First differencing would not capture elevated leftist power because 1923 was an off-election year. We thus heed Raffalovich’s suggestion that, “given the low power of these [unit root] tests, theory must play a large role. Researchers should consider the theoretical implications of their estimated model” (1994: 513). Given that elections, unlike most economic and crime indicators, are not phenomena tabulated annually, first differencing our predictor variable becomes problematic for addressing our research question. We thus analyze the effect of leftist voting net of time.
Figure 1. Leftist Voting and Violence against Jews in Germany, 1899–1939

Note: The correlation from 1899 to 1939 is +.40. For the pre-Hitler era (from 1899 to 1932), it is +.61.
RESULTS

Time trends for leftist electoral support and violent acts against Jews in pre–World War II Germany are depicted in figure 1. We see that violent acts against Jews varied substantially over this time period (gray line). The trend includes few acts prior to World War I, a spike in activity between World War I and Hitler’s ascension to power, and two spikes in violence during the Hitler era. Violence against Jews peaked in 1938, the year of the infamous Kristallnacht pogrom. Such fluctuation in violence against Jews appears to correlate with variation in electoral support for leftist parties (black line in figure 1). This is particularly apparent during the interwar years that coincide with the rise of a revolutionary left in Russia until the Nazi Party gained power in 1933. The correlation coefficient for leftist electoral success and violence against Jews is +.40 for the 1899 to 1939 period. When measuring the correlation only for the pre-Hitler period (1899 to 1932), the correlation between leftist electoral support and violence against Jews is +.61.

The bivariate association depicted in figure 1 finds initial support in the negative binomial model reported in model 1 of table 2. The positive coefficient (\( b = .128 \)) indicates that each percentage increase in electoral support for leftist parties yields an expected increase of 14 percent (\( e^{.128} \)) in the number of violent incidents against Jews. Yet, as described, many factors could intervene in this association and autocorrelation in the model warrants attention. Thus, model 2 shows the results when entering the control variables, including a 1-year lag on the dependent variable. The negative binomial model (model 2) indicates that neither the size of the Jewish population nor economic conditions as measured by GDP significantly influenced violence against Jews. The lack of an association for Jewish population size is not necessarily surprising. Group threat theories suggest that minority groups constitute a threat when they reach sizeable proportions of the total population. At around 1 percent, Jewish population size alone unlikely constituted a threat or provided substantial opportunity for intergroup contact. In a similar vein, economic conditions may not significantly influence violence because Jews in Germany were disproportionately employed in the financial sector (Brustein, 2003; Ruppin, 1934). The construction, woodworking, and textile sectors, where there was less likely to be competition between Jews and non-Jews in Germany, were hit harder by the unemployment spikes of the 1920s and early 1930s (Brustein, 1996). However, model 2 further supports our thesis that, in the absence of a sizeable population and controlling for general economic conditions, election outcomes are associated with intergroup violence. Compared to model 1, the coefficient for leftist electoral support...
is slightly stronger with respect to slope and significance when adding the statistical controls ($b = .188$).\textsuperscript{12}

The remaining models in table 2 (models 3 through 6) change the specifications to provide additional tests of robustness. For instance, because a Durbin-Watson test suggested autocorrelation was no longer present in models with 1- and 2-year lags (see footnote 11), model 3 shows the negative binomial coefficients when including both lags. The size of the leftist voting coefficient ($b = .171$) is slightly reduced in magnitude relative to model 2, but remains significantly associated with violence against Jews. The coefficients shown in models 4 and 5 are generated using the GLM estimator. Those results are entirely consistent with the negative binomial models with respect to the impact of leftist party electoral success and violence against Jews.\textsuperscript{13} Finally, model 6 shows that the effect of leftist political party electoral success on violence against Jews is consistent for a GLM model with a Newey-West correction that includes 1- and 2-year lags on the dependent variable for the 1899 to 1932 (pre-Hitler) period. The statistical association between leftist voting and violence against Jews thus appears robust.

**DISCUSSION**

The case of violence against Jews in pre–World War II Germany raises an apparent challenge to theories of violence that emphasize the predictive

\textsuperscript{12} Year is likely positive and significant in this model for two reasons. First, higher values of year (that is, closer to 1939) overlap with the Hitler era, during which violence against Jews increased in magnitude. Consistent with that assessment, the dummy variable representing the Hitler regime (from 1933 to 1939) is positive when year is omitted from the models. Second, year may pick up the ease of transmitting information to the editors of the *American Jewish Year Book* over this period, which was one of our justifications for including year as a control. We also point out that the lagged measure of violence against Jews is inversely associated with the contemporaneous measure, which speaks to the vast and sudden variability of major violent incidents against Jews during this time period.

\textsuperscript{13} We interpret the significant coefficients for GDP, Jewish population size, and the election year dummy variable in models 4 and 5 very cautiously. The coefficients and standard errors for those coefficients were highly susceptible to model specification. For instance, none of the coefficients were statistically significant when specifying the negative binomial estimator (see models 2 and 3). The GDP coefficient was actually negative and significant in GLM models that control only for year, and the GDP coefficient did not approach statistical significance when omitting the lagged dependent variable. Given the inconsistency of the coefficient in relation to the standard error, we are careful about interpreting the GDP coefficient in models 4 through 6. The election year dummy variable and the percentage Jewish are similarly subject to model specification. However, the leftist voting coefficient and standard errors appear robust and consistent across estimators and control variables.
Table 2. Regression Coefficients

<table>
<thead>
<tr>
<th>Variable</th>
<th>1 Negative Binomial</th>
<th>2 Negative Binomial</th>
<th>3 Negative Binomial</th>
<th>4 GLM</th>
<th>5 GLM</th>
<th>6 GLM 1899–1932</th>
</tr>
</thead>
<tbody>
<tr>
<td>Electoral support for leftist parties</td>
<td>.128*</td>
<td>.188**</td>
<td>.209**</td>
<td>.192**</td>
<td>.220**</td>
<td></td>
</tr>
<tr>
<td>GDP</td>
<td>(.057)</td>
<td>(.064)</td>
<td>(.052)</td>
<td>(.025)</td>
<td>(.023)</td>
<td>(.028)</td>
</tr>
<tr>
<td>Percentage of Jews in general population</td>
<td>- .846</td>
<td>-1.410</td>
<td>-1.066</td>
<td>-2.073**</td>
<td>-1.721</td>
<td></td>
</tr>
<tr>
<td>(2.232)</td>
<td>(2.043)</td>
<td>(.800)</td>
<td>(.914)</td>
<td>(1.081)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Election year (lagged)</td>
<td>.484</td>
<td>.566</td>
<td>.564</td>
<td>.645**</td>
<td>.517</td>
<td></td>
</tr>
<tr>
<td>(2.369)</td>
<td>(.337)</td>
<td>(2.35)</td>
<td>(.201)</td>
<td>(3.111)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Year</td>
<td>.110**</td>
<td>.144**</td>
<td>.111**</td>
<td>.143**</td>
<td>.119**</td>
<td></td>
</tr>
<tr>
<td>(0.363)</td>
<td>(0.373)</td>
<td>(0.11)</td>
<td>(0.016)</td>
<td>(0.017)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Germany 1933–1939</td>
<td>-.355</td>
<td>-1.133</td>
<td>-2.85</td>
<td>-1.035**</td>
<td>—</td>
<td></td>
</tr>
<tr>
<td>(7.888)</td>
<td>(8.81)</td>
<td>(3.19)</td>
<td>(3.79)</td>
<td>(3.79)</td>
<td>(3.79)</td>
<td></td>
</tr>
<tr>
<td>Violent acts against Jews (1-year lag)</td>
<td>-.296</td>
<td>-.290</td>
<td>-.339</td>
<td>-.340*</td>
<td>-.330</td>
<td></td>
</tr>
<tr>
<td>(0.848)</td>
<td>(0.872)</td>
<td>(0.31)</td>
<td>(0.25)</td>
<td>(0.23)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Violent acts against Jews (2-year lag)</td>
<td>-.189</td>
<td>-.164</td>
<td>-.160</td>
<td>-.160</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(0.868)</td>
<td>(0.857)</td>
<td>(0.36)</td>
<td>(0.36)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>-17.29**</td>
<td>-231.39**</td>
<td>-296.13**</td>
<td>-234.77**</td>
<td>-293.74**</td>
<td>-251.15**</td>
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<tr>
<td>(2.17)</td>
<td>(62.64)</td>
<td>(64.22)</td>
<td>(20.07)</td>
<td>(29.85)</td>
<td>(32.01)</td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>41</td>
<td>40</td>
<td>39</td>
<td>40</td>
<td>39</td>
<td>32</td>
</tr>
<tr>
<td>-2LL</td>
<td>132.16</td>
<td>103.22</td>
<td>99.23</td>
<td>106.73</td>
<td>105.45</td>
<td>79.33</td>
</tr>
</tbody>
</table>

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

Note: All models include the number of Jews in Germany as an exposure factor and specify robust standard errors ('robust' in Stata 9.2). Standard errors are in parentheses.
capacity of minority group size. Jews in Germany rarely constituted more
than 1 percent of the population and that level was largely stable during
the early twentieth century, yet violence against Jews varied
tremendously. Although working within a threat framework (Blalock,
1967), our model of intergroup violence goes beyond the prominent
proposition that intergroup violence elevates with minority population
size. Group size has been a focal variable in both group threat (McCall and
Parker, 2005) and structural accounts of intergroup violence (Messner and
South, 1992). With notable exceptions (Jacobs and Wood, 1999; Olzak,
1990), we contend that previous work has largely underestimated the
theoretical importance of political movements associated with minority
groups as precursors to violence. We suggest that population size is
theoretically irrelevant if the minority group accepts the status quo.
Although rarely the case in reality, that proposition generates our related
hypothesis that when the minority group either advocates a subversive
political agenda or is perceived as associated with a subversive political
movement, changes in politics can affect intergroup violence
independently of population dynamics and economic conditions.

We do not dispute the general theoretical claim that larger minority
group size and deteriorating economic conditions often yield greater
potential for intergroup violence. To deny that correlation is to challenge a
surfeit of existing research on topics ranging from American lynching
(Tolnay, Deane, and Beck, 1996) to European anti-Semitism (Brustein,
2003) showing that group size is often a salient predictor of intergroup
violence and conflict. Yet, our work contributes to extant research on the
politics of violence by illustrating that if a minority group is associated
with a subversive political movement, then the strength of that political
movement partly dictates intergroup violence net of minority group size
and general economic performance. This thesis is supported by our
analysis of leftist voting and violence against Jews in pre–World War II
Germany.

Our findings have implications beyond the singular case of violence
against Jews before World War II. The thesis that violence covaries with
political outcomes has a number of corollaries. Consistent with Jacobs and
Wood (1999) and our own findings, the likelihood of violence by majority
group members against minorities in the contemporary United States may
increase when minorities gain political power. Future research might also
retrace violence against blacks in the Reconstruction South in light of
these results. We would expect that places experiencing no increase in
black population size may nonetheless have witnessed antiblack violence if
political movements favoring civil rights gained traction. Other potential
empirical tests include violence against immigrants and the success of the
Social Democrats relative to the conservatives in contemporary Belgium,
A POLITICAL THREAT MODEL

Netherlands, and Denmark. These are empirically testable hypotheses that would align with a political threat model of violence.

Although our specific case in this research concerned leftist political strength, our findings have implications for both ends of the political spectrum. Left-wing extremist groups would theoretically resort to violence more frequently when rightist political parties garner political power. Environmental terrorism would increase in political climates where environmental protection policies are rescinded. Violence committed by antidemocratic groups in nascent democracies would increase in the wake of successful democratic elections, and religious fundamentalist violence would increase when secularists gain political power. Each of these cases would be consistent with our political threat model, and each lends itself to empirical analysis.

We suggest that shifting the focus away from minority group size alone provides leverage for understanding intergroup violence at times and places where population dynamics change very little but political power is contested and includes ideologically opposed agendas. That is, political threat is apt to culminate when groups at the fringes of the political spectrum threaten to gain a foothold on power. Leftist movements in pre–World War II Europe were associated with a perceived revolutionary agenda that would drastically challenge the status quo. Likewise, progressive political movements in American history that challenged segregation and Jim Crow elicited reactionary violence because they posed a radical alternative to the one-party dominance of the American South (Olzak, 1990). We would expect less severe ideological shifts, such as contemporary fluctuations between Republican and Democrat electoral success in the United States, to have less impact on intergroup violence.

We acknowledge both the contributions and limitations of this research. We use a rich data source in the American Jewish Year Book, but this is less elaborate and systematic compared to contemporary warehouses of crime information such as the Uniform Crime Reports (UCR) or the National Crime Victimization Survey. In addition, we could not control for the general level of violence against non-Jews during this period, because crime statistics akin to the modern UCR did not exist. Finally, data on the relative deprivation of Jews and non-Jews were unavailable for our study period. We thus acknowledge an alternative hypothesis that economic deprivation of non-Jews could, theoretically, drive both electoral support for leftist parties and violence against Jews. Future work might test the political threat argument relative to economic threat by focusing on groups that are not organized along religious lines, thus allowing additional indicators of both political and economic threat. Mindful of these limitations, we find empirical support for a model that relaxes emphasis on minority group size and brings political threat and electoral
success to the fore. We have suggested other avenues for future research to test this model. To this end, focusing on electoral shifts may add explanatory leverage to the study of intergroup violence.

Finally, we position this argument in the context of Hagan and his colleagues' (2005) recent claim that criminological theory has much to offer the study of genocide and its precipitants. We focused on events immediately preceding the near extermination of European Jewry in the Holocaust. Our thesis parallels the Hagan et al. conclusion that violence in contemporary Darfur entails concerns about power, control, out-group animus, and the subordination of threats. The threat of a subversive political movement associated with a minority group in pre–World War II Germany similarly played a salient role in violence against Jews in the years leading up to the Holocaust. Criminological theory thus adds explanatory power to an area of inquiry seldom investigated in empirical criminological research.

REFERENCES


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