Out-Group Threat and Xenophobic Hate Crimes: Evidence of Local Intergroup Conflict Dynamics between Immigrants and Natives

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This study examines the relationship between crimes attributed to immigrants and hate crimes against refugees at the local level. We argue that local crime events can lead natives to engage in vicarious retribution against uninvolved out-group members—refugees in our setting. Our empirical analysis relies on fine-grained geocoded data on more than 9,400 hate crimes and 17,600 immigrant-attributed crime events that occurred in Germany between 2015 and 2019. Using a regression discontinuity in time design, we show that the daily probability of a hate crime against refugees rises sharply in the immediate aftermath of an immigrant-attributed crime event in a local community. Additional analyses suggest that immigrant-attributed crime acts as an emotional "trigger," particularly in areas with strong radical-right support and recent demographic change. Our findings imply that individual commonplace crime incidents can give rise to intergroup conflict dynamics at the local level.

At 3:00 a.m. on August 25, 2018, in the East German city of Chemnitz, a group of young men begins to argue about cigarettes. Three men are stabbed; one—35-year-old Daniel H.—dies in a hospital later that night. The suspects are refugees from Syria and Iraq, while the victims are Germans. Protests against immigration ensue, the radical right mobilizes, and a dynamic of hatred and violence unfolds, culminating in an organized manhunt on ethnic minorities and refugees in Chemnitz, just one day after the knife attack (Grunert 2018; Kampf, Pittelkow, and Riedel 2019). What started off as a dispute about cigarettes between two groups had severe consequences for intergroup relations in the city. Chemnitz is an extreme example of how crimes attributed to immigrants can trigger violent intergroup conflict dynamics.

This article studies such local-level dynamics: how behavior attributed to migrants elicits retributive reactions on the part of the native population. We argue that everyday crimes by migrants—news about which is spread by local media and in personal networks—can act as a trigger for individuals to commit hate crimes.[1] In this study, the targets of these hate crimes are uninvolved out-group members, in our case refugees, a vulnerable, weakly integrated, yet highly visible part of the immigrant population. The hate crimes we study are thus a form of "vicarious retribution" (Lickel et al.

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1. We conceptualize hate crimes as criminal behavior motivated by prejudice toward the victim's putative social group (Dancygier and Green 2010; Green, Mcfalls, and Smith 2001). While a variety of factors can lead to hate crimes, our focus in this article are hate crimes driven by vicarious retribution dynamics.

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2006): "punishment" of uninvolved out-group members by in-group members themselves uninvolved in the original act. We argue that the events in Chemnitz exemplify a more general dynamic: threatening events attributed to migrants—the out-group in our setting—can lead to a temporary surge in the rate of xenophobic hate crimes against refugees in a local community.

We test this argument in the German context. Similar to other European democracies, Germany has experienced both a sharp increase in the foreign-born population and a surge in racist and xenophobic hate crimes in recent years. Our empirical analysis draws on two original data sources. First, we collected event-level data on all 9,400 hate crimes against refugees that were recorded by official agencies between January 2015 and March 2019. We complement these data with detailed geocoded information on more than 17,600 crimes attributed to immigrants in Germany during the same time period. These data are based on official police press releases and reports in the local news media. We focus on crimes attributed to perpetrators from African and Muslim-majority countries, which are most likely to trigger backlash against refugees. We argue that due to (i) the objective distribution of refugees across nationalities and (ii) racialized patterns of categorization, natives are most likely to link perpetrators from these countries to the out-group of refugees. To causally identify the effect of immigrant crimes on hate crimes against refugees in the same locality, we use a sharp regression discontinuity in time (RDIT) design. We estimate the rate of xenophobic hate crimes right before and after the occurrence of immigrant-attributed crime events within a small temporal bandwidth of two to five days.

We find a significant increase in antirefugee hate crimes right after crime events attributed to immigrants. We estimate that the daily probability of a xenophobic hate crime in a county increases by about 1 percentage point in the immediate aftermath of an immigrant crime event. This corresponds to a 65% increase relative to the pretreatment baseline. Additional analyses reveal that our results are driven by violent crime events (e.g., stabbings, sexual assaults). In line with our theoretical expectations, we do not find any evidence that nonviolent petty crimes or crimes attributed to nonrefugee immigrant groups (e.g., Romanians or Bulgarians) lead to a backlash against the local refugee population.

Previous research has emphasized that hate crimes are linked to specific local contexts—such as support for far-right parties or a recent influx of migrants (Jäckle and König 2018; Krueger and Pischke 1997; Smångs 2017). Building on this line of research, we tested whether characteristics of a locality—such as economic, social, and political conditions—moderate the vicarious retribution dynamics we identify. We find suggestive evidence that migrant-attributed crimes are more likely to lead to hate crimes against refugees in areas with deeper support for far-right parties. This suggests that the hate crime dynamics we uncover can be linked to broader local political trends that may legitimize vicarious retribution or create in-group dynamics that foster prejudice and aggression. In addition, vicarious retribution is more likely where the foreign population has recently increased, that is, where immigrant-attributed crime may be perceived as more threatening and act as a stronger motivational prime. These additional analyses suggest that vicarious retribution against migrant crime events is linked to threat perceptions and to in-group norms and dynamics even if, naturally, the motives of individual hate crime perpetrators are difficult—if not impossible—to isolate in our setting.

Our research adds to a large and growing literature on hate crimes and attitudes toward out-groups more generally (e.g., Enos 2014; Hangartner et al. 2019; Hopkins 2010; Liebe and Schwitter 2021). First, we build on prior research that has shown a surge in hate crimes after salient "focusing events" that attract national media attention. For example, Frey (2020) demonstrates a notable spike in antirefugee hate crimes after the 2015/16 New Year's Eve sexual assaults in Germany. Prior research has identified similar patterns in the aftermath of the July 7 bombings in London (Hanes and Machin 2014), the 9/11 terrorist attacks (Disha, Cavendish, and King 2011), contentious court cases (King and Sutton 2013), and Islamic terrorist attacks more generally (Jäckle and König 2018; King and Sutton 2013). These prior studies have mostly examined overtime variation in the countrywide rate of hate crimes before and after singular events that attract nationwide or even international media attention. However, such events are relatively rare and account for only a small fraction of natives' overall exposure to out-group crime. In contrast, citizens are frequently exposed to highly localized information about crimes in their vicinity. In our setting, about one-third of local news articles cover crime—most often events that occurred within the local coverage region of a given newspaper (van Um, Huch, and Bug 2015). By drawing on a large-scale data set of more than 17,000 localized crime events attributed to immigrant perpetrators, we substantially extend the scope of prior related work. Most importantly, our study demonstrates that vicarious retribution dynamics are much more widespread and frequent than previous research suggests. Moreover, local events may help to explain regionally specific dynamics in intergroup conflict. Beyond salient events that attract nationwide attention, vicarious retribution occurs at the local level on an everyday basis. Our work thus establishes the generalizability of vicarious retribution dynamics to a much larger set of cases and contexts than prior research suggests.

Second, our results shed new light on the mechanisms underlying vicarious retribution dynamics. Going beyond prior work, our empirical strategy allows us to trace the temporal persistence of vicarious retribution at the local level. We find a surge in the rate of hate crimes in the immediate aftermath of crime events attributed to immigrants. After about four days, the likelihood of hate crimes reverts back to its pretreatment level. This suggests that out-group crime events primarily lead to hate crimes by acting as an emotional "trigger" rather than through more long-term processes of attitudinal change. In addition, we do not find evidence that vicarious retribution dynamics "spill over" to geographically proximate, neighboring local communities. This suggests that the effects we identify are highly localized and thus distinct from the focusing events studied in prior research.

Finally, our findings suggest that vicarious retribution dynamics are driven by different contextual factors at the local rather than at the national level. For instance, Frey (2020) finds that hate crimes primarily increased in localities with previously low levels of anti-immigration sentiment after the sexual assault cases in Cologne. In contrast, we find that vicarious retribution at the local level primarily occurs in regions with high levels of far-right support. We interpret this as suggestive evidence that distinct dynamics underlie the focusing events studied in prior work and the more localized events we examine in this study.

A DYNAMIC PERSPECTIVE ON INTERGROUP CONFLICT TRIGGERED BY GALVANIZING LOCAL EVENTS

While much has been learned about the cross-sectional causes of intergroup conflict (see Liebe and Schwitter 2021), researchers have recently started to focus on the dynamics of intergroup conflict, aiming to identify variable drivers of tension and aggression rather than comparatively stable factors (e.g., Balcells, Daniels, and Escribà-Folch 2016; Colussi, Isphording, and Pestel 2021).[2] Our study adds to this research by putting forward a dynamic perspective on hate crimes at the local level: in a nutshell, we argue that localized threatening events attributed to an out-group—crimes attributed to immigrants in our setting—can lead to hate crimes by acting as a trigger for natives with preexisting prejudice against the out-group.

Our theoretical framework builds on a large literature that highlights how salient, galvanizing events can be a potent trigger for aggression and violence. Cognition and affect are two key pathways in this context (Anderson and Bushman 2002). First, galvanizing events may increase negative attitudes toward the relevant out-group—immigrants in our case—as well as perceptions of how threatening this group is (Sniderman, Hagendoorn, and Prior 2004). Heightened threat perceptions can galvanize action (Sniderman et al. 2004), especially if an event can be interpreted as an attack on the in-group (Lickel et al. 2006). In addition, threatening events may act as a prime, heightening the salience and centrality of xenophobic attitudes, even if the attitudes themselves remain stable (Allen, Anderson, and Bushman 2018). As Jungkunz, Helbling, and Schwemmer (2019) and Peffley, Hutchison, and Shamir (2015) suggest, such attitudinal effects may occur mainly among individuals with preexisting xenophobic grievances. In our setting, crimes attributed to immigrants—violent crimes in particular—may reinforce anti-immigrant attitudes and increase threat perceptions among natives. However, shifts in attitudes do not necessarily translate into changes in behavior (Frey 2020; Guyer and Fabrigar 2015). Hence, affect (i.e., emotional reactions) may play an important mediating role (Anderson and Bushman 2002). For example, a galvanizing threat may increase anger among the in-group members predisposed toward hate crimes (Nussio 2020). This anger can act as an important motivational driver of vicarious retribution (Zeitzoff 2014).

Prior work on galvanizing events has almost exclusively focused on highly salient events that receive sustained national media attention.[3] With regard to attitudes, research has shown that such events (e.g., terrorist attacks) have led to reinforced negative stereotypes about immigrants and Muslims, as well as an increase in the perceived threat posed by these groups (Disha et al. 2011; Frey 2020; Godefroidt 2023; Hopkins 2010; Kam and Kinder 2007; Legewie 2013; Peffley et al. 2015). However, the effects of threatening events are not limited to attitudes and perceptions. Prior research has demonstrated a surge in hate crimes after, for example, the July 7 bombings in London or the 2015/16 sexual assault cases in Cologne (Christensen and Enlund 2019; Devine 2021; Frey 2020; Hanes and Machin 2014; Ivandic, Kirchmaier, and

2. There has been extensive sociological and criminological work on the kinds of people who engage in hate crimes against out-groups such as migrants or LGBTQ persons (for summaries see, e.g., Dancygier and Green 2010; Green and Spry 2014; Green et al. 2001; Walters 2011). Perpetrators of hate crimes tend to hold biased perceptions of and grievances toward the targeted out-group (King and Sutton 2013), and many engage in hate crimes out of a desire to exact revenge and protect the in-group (Lickel et al. 2006).

3. One exception to this national focus is research on lynching (e.g., Green et al. 2001; Smångs 2016), which largely examines a different kind of retribution that is generally more direct and more public than hate crimes.

Machin 2019; King and Sutton 2013; Lickel et al. 2006; Nussio, Bove, and Steele 2019). King and Sutton (2013) even suggest that "considerable publicity" is a necessary precondition for antecedent events to spark vicarious retribution.

Yet, focusing on highly salient national events is potentially misleading, as it might imply that vicarious retribution is an unusual occurrence limited to exceptional periods of nationwide tension. However, we argue that salient local events—specifically, crimes attributed to immigrants—can also lead to retributive hate crimes. Importantly, these local events occur with much higher frequency than nationally salient events. While the kinds of highly salient events studied hitherto are natural candidates for strong reactions, localized threatening events may also be effective in causing a shift in attitudes or acting as an affective trigger for individuals with high levels of preexisting prejudice against immigrants. Indeed, because localized crimes will appear more immediate than distant national events, it is plausible that local events may be particularly effective in changing threat perceptions and generating affect. The type of local event should also matter for its impact: in our setting, we expect particularly violent events (e.g., sexual assault cases, homicides) to elicit stronger reactions compared to relatively minor crimes attributed to immigrants. More threatening types of crimes likely constitute a stronger trigger for hate crimes through vicarious retribution. The events in Chemnitz described in the introduction are an extreme example of such dynamics.

How do natives—including potential hate crime perpetrators—hear about immigrant-attributed crime in their locality? We suggest that information about local crime events attributed to immigrants can reach xenophobic natives through two principal channels: (1) direct exposure to media reporting and (2) indirect exposure through social networks. Social media can amplify both of these channels (Müller and Schwarz 2021), especially if social media contacts or broader organizations share related content. The Chemnitz incident cited in the introduction, for example, was coordinated on social media (Kampf et al. 2019). We revisit this point and provide a more detailed discussion of how information about individual crime events reaches hate crime perpetrators in our setting when we discuss our data sources below.

Against the backdrop of our theoretical framework outlined above, our main empirical hypothesis is:

> **H1.** The probability of a xenophobic hate crime in a given county increases in the immediate aftermath of immigrant-attributed crime in the same county.

Our empirical application focuses on hate crimes directed specifically against refugees. Importantly, we assume that citizens—especially potential hate crime perpetrators—are not discerning when it comes to out-group membership: crimes attributed to a broad range of immigrant groups may lead to hate crimes against refugees. We argue that—for perpetrators of hate crimes—refugees are part of broader out-group population composed of immigrants that are perceived as particularly threatening and culturally distant, namely, those from African and Muslim-majority countries (Czymara and Schmidt-Catran 2016; Di Stasio et al. 2021). In addition, hate crimes against refugees are a particularly plausible reaction, as this subset of the population is particularly vulnerable and less socially embedded (Smångs 2017). We provide a more detailed discussion of the connection between crimes attributed to perpetrators from Muslim-majority or African countries and hate crimes against refugees when we discuss our specific data sources below.

The mechanisms outlined above imply that migrant-attributed crime might be more likely to lead to hate crimes in specifically fertile contexts, which are areas where such crimes are perceived to be more threatening and more likely to act as an affective trigger (for an overview, see Green et al. 2001; Smångs 2017). A diverse set of local social and economic aspects may be relevant here, with a given migrant-attributed crime potentially more likely to trigger backlash in contexts where there are many immigrants or where their number has been increasing rapidly (Dinas and van Spanje 2011; Hopkins 2010). Indeed, empirical evidence suggests that natives' perceptions of immigration are primarily shaped by overtime increases in the immigrant population rather than the overall size of that group (Hopkins 2010; Newman and Velez 2014). The argument is that rapid change in immigrant populations is "more likely to capture the attention of local citizens than the size of these populations" (Newman and Velez 2014, 293). It might also be the case that the social cleavage between immigrants and natives—a crucial scope condition for the dynamics we describe—tends to fade over time, that is, in localities that have experienced immigration inflows for decades. In localities where immigration has been a more recent phenomenon, however, immigrants might be perceived as more threatening and different from the native population. Following these arguments, we would expect vicarious retribution dynamics to be more likely in places that have only recently experienced a rapid rise in the immigrant population.

In addition, migrant crime may be seen as more threatening in areas with weaker economies and higher unemployment rates, particularly among natives (Dancygier 2010; Green, Glaser, and Rich 1998; Krueger 2007), but the evidence here is not conclusive (see, e.g., Krueger and Pischke 1997); similar debates characterize research on lynching (Green et al. 2001; Smångs 2017). As noted above, certain types of crimes,

violent crimes in particular, might likewise be perceived as particularly threatening.

Second, local contexts may vary in the extent to which vicarious retribution is seen as legitimate, that is, whether group norms hinder or foster such reactions (Allen et al. 2018). Perceptions of hate crime legitimacy among radicalized individuals may be greater where there is a sustained presence of and electoral support for radical-right parties in a community, as well as the associated levels of anti-immigrant discourse and hate speech (Bischof and Wagner 2019; Dancygier and Green 2010; Karapin 2002). When a community fails to condemn hate crimes, this can lead to further legitimization (i.e., the erosion of antiviolent social norms; Romarri 2020; Weaver 2019). In Germany, such patterns of legitimization may be particularly relevant given the recent rise of the Alternative for Germany (Alternative für Deutschland, AfD). More generally, organizations may act as mobilizing forces (Smångs 2016). Indeed, there is cross-sectional evidence that support for the radical right is associated with higher levels of hate crimes (Jäckle and König 2018; Romarri 2020), while lynchings were less likely to occur in Republican-dominated areas (Smångs 2017).

In addition to creating a legitimizing environment, there may be a purely numeric effect, with migrant-attributed crime more likely to elicit vicarious retribution if the number of individuals predisposed to such action is higher. As the number of radicalized individuals increases, so will the likelihood that one or more of these opt for vicarious retribution. Moreover, group dynamics among radicalized individuals may also make aggression more likely (Allen et al. 2018; Smångs 2016). Such group dynamics will be more likely where sizable groups already exist. Again, support for radical-right parties such as the AfD is a useful proxy in this regard. We revisit these hypotheses and empirically examine effect heterogeneity along a number of potential moderator variables below.

DATA AND EMPIRICAL STRATEGY

Data

Hate crimes against refugees. To measure hate crimes, we collected detailed information on more than 9,400 xenophobic hate crime incidents that occurred in Germany between January 2015 and March 2019. The German police do not release event-level information on hate crimes. However, the German socialist party (Die Linke) inquires about hate crimes directed at refugees and refugee accommodations that occurred in Germany through quarterly parliamentary requests (*kleine Anfrage*). The federal government responds with a detailed list of hate crimes based on information from the German police and intelligence services. Along with a description of the law violated in each specific incident, we observe the precise location and date of each event. We provide more details on the hate crime data in appendix section SI.3.2.[4]

We emphasize that our data only cover hate crimes specifically targeted at refugees: they cover arson, raids, damage to property, and violent and verbal attacks directed against refugees and refugee housing.[5] Our data do not cover other forms of hate crimes, for example, crimes motivated by anti-Semitism or religious fundamentalism. The fact that our data only cover a subset of all hate crimes in Germany could constitute an issue if one were to study the determinants of hate crimes more generally. However, this study is interested specifically in immediate vicarious retribution against refugees. Our data hence allow us to focus on precisely the kind of xenophobic violence and bigotry we intend to measure. The three most common types of offenses we observe are (1) hate speech, (2) criminal damage, and (3) battery. Crimes in these three categories cumulatively account for more than half of the hate crime events recorded in our data set.

We visualize the temporal and spatial distribution of hate crimes in figures SI 11 and 12 (figs. SI 1–37 are available online). Hate crimes occur more frequently in East Germany (Krueger and Pischke 1997), even though the overall share of the foreign-born population in East Germany only stands at about 5%, compared to about 15% in the West. The number of hate crimes has increased sharply in the wake of the influx of refugees into Germany since 2014, in particular after the 2015–16 New Year's Eve sexual assaults in Cologne (Frey 2020). We investigate the cross-sectional correlates of hate crimes in more detail in figure SI 13, where we follow Krueger and Pischke (1997) and regress the total number of hate crimes in a given county on a variety of county-level covariates.

A potential concern regarding our hate crime data is underreporting by either German authorities or victims of hate crimes (FRA 2021). Some incidents might not appear in our data because refugees are hesitant to report attacks against them to the German police. Likewise, it might be the case that

4. We provide background information on the German context in app. sec. SI.1. We discuss Germany's historical experience with immigration, the issue salience of immigration during our study period, and the role of radical-right parties.

5. It is, however, likely that our arguments travel beyond "refugees" as a group. Perpetrators almost certainly use descriptive characteristics, such as skin color and language, to identify their victims, making it difficult to differentiate between minority groups using their legal status. However, we also believe that locating refugees may be easier in some cases. This is because refugees are often required to reside in specific buildings designated for their use, which defines the locations where they live and frequently go, e.g., to purchase groceries. This information is likely known by the local community, which may make it easier for perpetrators to target refugees compared to other migrants.

the German police fail to register some incidents targeted specifically at refugees. As a result, the true rate of xenophobic hate crimes might be substantially higher than what official numbers suggest. For our study, it would be particularly problematic if underreporting systematically varied across space and time. Our identification strategy that compares the rate of hate crimes right before and after crimes attributed to migrants in a given county requires that the probability of hate crime reporting is constant right before and after immigrant-attributed crime events. We view this assumption as likely satisfied but note that we cannot test it formally. Measuring the probability of hate crime reporting would require that we observe all hate crime incidents, including those that were only ever known to the victim and perpetrator. To partially address concerns about reporting bias, we validated our hate crime data by comparing them to information on hate crime events compiled by independent nongovernmental organizations (Benček and Strasheim 2016). While these data likewise do not allow us to observe unreported attacks against refugees, we argue that they allow us to alleviate concerns about bias on the part of the police. We provide a detailed discussion of the validation checks we performed in appendix section SI.3.3.

Crimes attributed to immigrants. We complement our hate crime data with detailed information on salient "immigrant crime events" recorded between January 2015 and March 2019. Event-level data on crimes committed by foreigners are not published by the German police or any other agency. Our data source for immigrant crime events is the website refcrime.info, which explicitly seeks to draw attention to crimes committed by first- and second-generation migrants in order to reduce public support for immigration.[6] In addition to the date and precise location of each crime event, we observe the type of crime that occurred (homicide, battery, theft, rape, etc.). We are hence able to distinguish between the effects of violent and nonviolent crimes. A detailed overview of how we classified violent and nonviolent crimes can be found in the appendix (see app. sec. SI.2.8). We provide summary statistics of the immigrant crime data in table SI 1 (tables SI 1–6 are available online) and figure SI 1.

In total, about 60,000 crime events were recorded on the website between January 2015 and March 2019. However, we only consider a subset of these events for our main analysis, in order to align the measurement of our outcome variable, treatment variable, and theoretical framework. We first note that this study examines group-based vicarious retribution at the local level. Empirically, our outcome variable measures hate crimes against refugees—a highly salient out-group during our study period. For the definition of our treatment, we accordingly focus on those crime events that xenophobic natives are most likely to link to the group of refugees as a whole. We assume that this cognitive process is partly determined by objective factors, namely, the true distribution of asylum seekers by nationality during our study period, and partly determined by racial categorization and stereotypes.

We argue, for example, that crimes attributed to Syrians, Eritreans, or Afghans can lead to backlash against refugees because the vast majority of individuals from these countries are in fact refugees in our setting. As of 2020, 83% of Eritreans, 79% of Afghans, and 75% of Syrians who resided in Germany entered the country as refugees (Statistisches Bundesamt 2019). In other words, the probability of refugee status conditional on nationality is very high for these immigrant groups. In contrast, the share of refugees is very low for European countries such as Romania, Poland, or Bulgaria. Fewer than 30 individuals from these countries applied for asylum in Germany in 2016 (BAMF 2016). While a large share of crime events in our data set is attributed to Eastern European perpetrators, we would not expect these events to lead to a backlash against the local refugee population.

Beyond the true distribution of asylum seekers, we expect racialized stereotyping and categorization to be an important factor in our setting (Blumer 1958; Meuleman et al. 2019). For example, while the share of refugees is comparatively low for immigrants from Senegal, Kenya, and Tunisia, we nevertheless expect perpetrators from these countries to be categorized as Muslims or Africans, rather than Senegalese, Kenyans, or Tunisians. In other words, we argue that potential perpetrators of hate crimes do not make fine-grained differentiations between nationalities but rather use nationalities as heuristics to distinguish between broader categories of out-groups. Against this background, we use the subset of 17,600 crimes attributed to perpetrators from Muslim-majority or African countries for our main analyses. We provide a detailed overview of our nationality classification in appendix section SI.2.9. The nationalities we retain in our sample account for more than 91% of all asylum applications in 2016, and thus they cover the vast majority of refugees during our study period (BAMF 2017).

Most of the crimes we exclude from our analysis are attributed to Eastern Europeans—mainly immigrants from Romania, Poland, or Bulgaria. We also drop cases when the nationality of the perpetrator is not explicitly mentioned in the police press release. These cases generally contain cues (e.g., "southern appearance") but cannot be clearly linked to specific nationalities. The construction of our sample aligns with prior research that suggests that anti-immigrant attitudes

6. We note that the website went offline in late 2020/early 2021.

and xenophobia are most pronounced for immigrants from Muslim-majority and African countries in European societies (Di Stasio et al. 2021; Valentino et al. 2019) and Germany specifically (Czymara and Schmidt-Catran 2016, 2017). In a supplementary analysis, we use crimes attributed to perpetrators from other nationalities for a series of placebo tests (see table 2). In line with our theoretical expectations, we do not find any evidence that crimes attributed to other groups (e.g., Romanians or Poles) lead to backlash against the local refugee population.[7]

How do perpetrators of hate crimes learn about the immigrant-attributed crime events covered by our data set? While we do not have access to a survey sample of hate crime perpetrators to directly answer this question, we argue that two primary channels are most likely. First, a small subset of xenophobic natives might directly obtain information from refcrime. In addition to its website, the events recorded on refcrime were also publicized on Twitter (@RefugeeCrimeMap) and Facebook. While the audience of refcrime is likely a very small share of the German population, potential hate crime perpetrators are likewise a highly selected, small group with high levels of preexisting prejudice against immigrants. Against this background, it seems plausible that a subset of potential hate crime perpetrators directly obtains information on immigrant-attributed crimes from refcrime. Second, perpetrators might learn about immigrant-attributed crime events from other news sources (e.g., local news media, social media) that rely on the same primary data source as the refcrime website, namely, police press releases. During our study period, about one in two Germans consumed local news—either online or in print—on a weekly basis (Reuters Institute 2018). Moreover, reporting on crime is a central facet of German local news, with one study—a quantitative analysis across 31 German local newspapers—showing that one-third of local news articles are about crime. Violent crimes such as homicides and sexual assaults receive disproportionate attention compared to their overall relative frequency (van Um et al. 2015). Prior research also highlights that German newspapers frequently report the nationality of crime perpetrators, if such information is available from police press releases (Hestermann 2019). In appendix sections SI.2.4 and SI.2.5 we conduct further tests to establish the plausibility of this channel. Specifically, we demonstrate that (i) local news outlets base most of their crime reporting on information released through police press releases and (ii) refcrime—in addition to police information—draws on information covered in a large number of different outlets across the German media landscape. Naturally, information about local crime events might also be shared within social networks and through interpersonal communication, both online and offline (Müller and Schwarz 2021).

The political motivation behind our data source raises concerns that the website might misreport crimes committed by migrants or that there might be other systematic biases in the data. We address such concerns systematically in appendix section SI.2.2 by implementing a number of quality checks of the immigrant crime data to ensure that (i) the reported information is accurate and (ii) reporting is not systematically biased. We find that the events reported on the website are, in almost all cases, confirmed by credible sources: 93% of the reported events are substantiated by official police press releases. The remaining cases are mostly based on local news articles and, in some cases, the websites of TV channels or local radio stations. In appendix section SI.2.4, we provide a list of all news sources that refcrime draws on in addition to police press statements. The website draws on a large number of established national and local news outlets.

The total number of crimes recorded by the German police in 2019 was 5.43 million. For these crimes, the police identified about 2 million crime suspects, of which about 700,000 had foreign citizenship (34.6%). Our data hence only cover a small subset of all crimes in Germany, with a focus on the most egregious types of crimes that can most easily trigger a response (e.g., stabbings, sexual assault). While many crimes were committed by foreigners but not recorded on the website, our data source does capture the most salient events that likely attracted the most attention in the media and right-wing (online) networks.

We examine the cross-sectional predictors of immigrant-attributed crime events in our database in figure SI 7. We find a strong, statistically significant relationship between the events recorded in the refcrime database and the local presence of foreigners. As expected, this empirical relationship is particularly strong for the share of immigrants from outside the European Union (see also fig. SI 2). We do not find a statistically significant association between refcrime event counts and other covariates, including electoral support for the AfD (see also fig. SI 3). Finally, we recognize the ethical challenges involved in relying on data collected by anti-immigrant political activists. As discussed above, we see the data as a proxy for crime attributed to foreigners. Using these data does not legitimize the motivation and goals by the data distributor. On the contrary, as our results and implications show, we see the efforts by refcrime as highly problematic and adding to an atmosphere of out-group anger, fear, and hatred.

7. We also demonstrate that our main results are robust to sequentially dropping nationalities from the sample and thus to not appear to be driven by idiosyncratic choices about the sample composition (see fig. SI 26).

Estimation and identification strategy

To estimate the causal effect of migrant crimes on hate crimes, we use an interrupted time series approach. More specifically, we use an RDIT design in which time is used as the running variable (Hausman and Rapson 2018). Our data are well suited for this approach, as we observe the precise date and location for all recorded incidents of migrant crimes and xenophobic hate crimes.

In contrast to previous studies using RDIT models on a single event such as the September 11 attacks or the sexual assault cases in Cologne, we observe more than 17,600 individual migrant crime events. To analyze this rich data set in an RD framework, we transform our raw data into a panel in which immigrant-attributed crime events are the main unit of analysis. Before delving into the formalization of our approach, we illustrate our procedure for one exemplary event in our data set: the stabbing that occurred in Chemnitz on August 26, 2018, which we discussed in the introduction of the article. In figure 1, we see that between August 12 and 25, no hate crimes occurred in Chemnitz. Starting on August 26—the day immediately following the crime attributed to refugees—the rate of hate crimes surged. Ten hate crimes occurred on August 26 alone. Multiple additional hate crimes occurred over the following days. After about four days, the effect fades and the rate of hate crimes in the county of Chemnitz reverts back to its pretreatment level. We apply this data transformation procedure to every crime event recorded in the refcrime data: for each event, we measure the rate of hate crimes in a given locality in the two-week period before and after the event. We then use an RD approach to compare the average rate of hate crimes before and after immigrant-attributed crime events.

More formally, for each migrant-attributed crime, we consider the two-week period before and after the event.[8] Hence, for each migrant-attributed crime event, we start out with 29 daily observations of our outcome variable—hate crime $Y_{i,t,c}$. Here, i denotes the index of the immigrant crime event, c is the county in which it occurred, and t is the date. Our main outcome $Y_{i,t,c}$ variable is the daily number of hate crimes that occurred in county c on a given day t. To facilitate the interpretation of the effect estimates, we conduct an additional analysis in which we model the daily probability of hate crime before and after a migrant crime event. To do this, we code a binary indicator variable that takes on the value 1 if at least one hate crime occurred in county c at a given date t (see table 1).

8. We examine longer-term effects of immigrant-attributed crime on hate crimes in fig. SI 16.

Our running variable is the time in days before and after a migrant crime event. We denote this period variable by $P_{i,t,c} \in [-14, 14]$. It captures the temporal distance from the immigrant crime event and determines the treatment assignment. Counties are considered treated when an immigrant crime occurred; that is, $T_{i,t,c} = 1$ if $P_{i,t,c} \geq 0$. We provide details on how we deal with (partially) overlapping event periods in appendix section SI.2.7. At 5,332 events, the effective sample size we use within the optimal bandwidth is substantially smaller than our baseline sample. We systematically investigate and discuss the differences between the two samples in figure SI 9. We find that our effective sample is generally representative for the larger sample of events in the refcrime database. It contains a slightly higher share of violent crime events compared to the full refcrime data. We also note that crime events are more likely to overlap in large cities or West Germany, where the share of the foreign-born population is higher.

We estimate a sharp RD design in which the treatment assignment is a deterministic function of the time period as formalized in equation (1). We follow the standard practice in RD designs and approximate the regression function $E[Y_{i,t,c}|P_{i,t,c} = p]$ by fitting local polynomials on each side of the treatment assignment cutoff. In a nutshell, we model the daily rate of hate crimes right before and after migrant crime events using a polynomial function. This allows us to estimate the local average treatment effect:

$$\tau = E[Y_{i,t,c}(1) - Y_{i,t,c}(0)|P_{i,t,c} = 0] \quad (1)$$

Unless otherwise noted, we use a local linear polynomial to reduce the sensitivity of our results. In all presented analyses, we use robust bias-corrected standard errors and use a triangular kernel function as recommended by Cattaneo, Idrobo, and Titiunik (2019). For our main analyses, we do not use clustered standard errors. However, in a supplementary robustness test we establish that our results remain unchanged when we cluster at different levels (see fig. SI 27). The choice of the bandwidth in days h involves a bias-variance trade-off. For our main results, we use an optimal bandwidth selection algorithm to minimize the mean squared error of our treatment effect estimates. Across a multitude of robustness checks, we demonstrate that our results hold using different functional forms of the regression function and varying bandwidths around the treatment assignment cutoff (see app. sec. SI.6). We provide more details on local polynomial estimation and optimal bandwidth selection in RD designs in appendix section SI.2.7.

Identification rests on the standard assumptions of RD designs, in particular continuity of the potential outcomes around the treatment assignment cutoff. This means that no

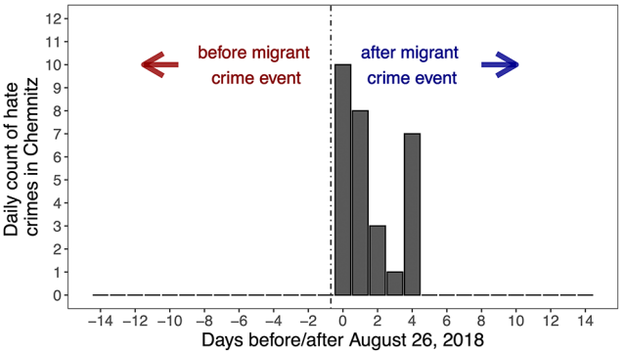

Figure 1. Transformation of event-level data to the data we use for the RDIT design. We use the example of a stabbing that occurred in the East German city of Chemnitz on August 26, 2018. The running variable is the time in days relative to an immigrant-attributed crime event. We consider the two-week period before and after each crime event. For this illustration, we discard the possibility of partial overlap between events (see app. sec. SI.2.7).

other determinant of hate crimes systematically coincides with migrant crime events within small temporal bandwidths. We stress that because we exploit variance in the rate of hate crimes within a given county over time, all factors that are invariant within small temporal bandwidths are held constant by design (e.g., election results, local wages, unemployment, bureaucratic efficiency). While the propensity to report hate crimes or crimes attributed to immigrants might vary across counties, our design only requires that the propensity to report either crime is constant around the temporal cutoff before and after crimes attributed to immigrants (see also app. sec. SI.3.3).

We provide additional evidence in support of our key identification assumption in appendix section SI.6. In addition to a battery of robustness tests, we demonstrate that (i) hate crimes do not predict migrant crime events (fig. SI 21), (ii) migrant crimes and hate crimes do not generally cluster on the same days of the week (fig. SI 23), and (iii) the days before and after migrant crime events are similar in terms of observable climatic characteristics (fig. SI 28). We also conduct an empirical test for spillover effects, where we demonstrate that crimes attributed to immigrants do not predict hate crimes in directly contiguous or geographically proximate counties (see fig. SI 18).

RESULTS

Before moving on to our main results, we descriptively examine the association between migrant crime events and hate crimes in appendix section SI.5. We begin by plotting the number of recorded migrant crime events against the number of hate crimes in a given county in a given month. We document a positive correlation between the two variables ($r = 0.21$). Next, we investigate the empirical relationship between immigrant-attributed crimes and hate crimes using a variety of standard panel models. Among other things, we conduct a first-difference analysis in which we regress the change in the number of hate crimes in a given county in a given week on the change in the number of immigrant-attributed crimes in the same county in the same week (see table SI 5). Across a variety of model specifications we find a positive, statistically significant association between crimes attributed to immigrants and hate crimes recorded in a given county in a given week/month. Of course, other factors might drive this correlation. Most importantly, the analyses presented in appendix section SI.5 do not precisely disentangle the temporal ordering of migrant crimes and hate crimes. We rely on our RDIT identification strategy described above to address concerns that this correlation—while robust across a variety of standard panel models—does not establish a causal relationship.

Main findings

In figure 2, we plot the daily rate of hate crimes in the two-week period before and after an immigrant crime event occurred in a given county. We find a sharp spike in hate crimes in the immediate aftermath of crimes attributed to immigrants. After about four days, the likelihood of hate crimes reverts back to its pretreatment level.

This descriptive pattern is mirrored in our main results from RDIT models, which we present in table 1. We find a positive, statistically significant effect of immigrant-attributed crimes on the propensity of hate crimes across a variety of model specifications. Our results hold for both the binary and count measurement of hate crimes and for different functional forms of the local polynomial that we fit on both sides of the treatment assignment cutoff. In line with our theoretical expectations, these results are driven by violent crime events (e.g., stabbings, rape) that are most likely to generate perceived out-group threat. Using the models with a binary outcome variable, we estimate that the daily probability of a

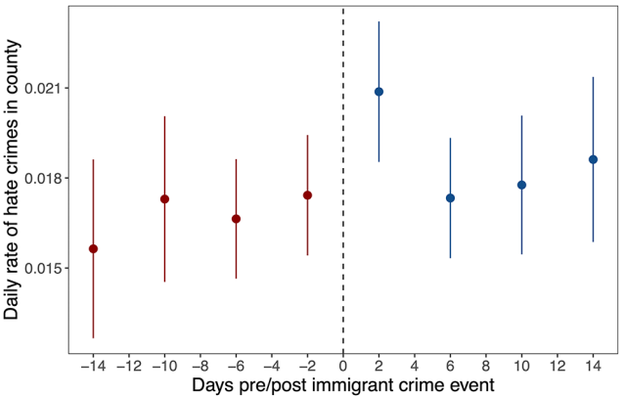

Figure 2. Daily rate of hate crimes in the two-week period before and after an immigrant crime event occurred. Error bars indicate 95% confidence intervals. We show RD plots including a linear polynomial fit in appendix section SI.10.

xenophobic hate crime in a county increases by 1–2 percentage points in the immediate aftermath of an immigrant crime event. The magnitude of the effect estimates varies moderately across model specifications, with larger estimates when we use a quadratic rather than linear functional form.

While the baseline probability of a hate crime occurring in a given county on a given day is low, the estimated treatment effects are sizable. In relative terms, an increase of 1–2 percentage points in the daily probability of hate crimes in a given county corresponds to a 65%–130% increase relative to the pretreatment baseline. A back-of-the-envelope calculation on the basis of our findings suggests that about 500 additional hate crimes occurred because of the 17,600 migrant crime events we observe.[9] This is a lower-bound estimate of the total net effect of immigrant-attributed crimes on hate crimes since our data source does not cover the entirety of crimes attributed to immigrants. To further put the magnitude of our findings into perspective, we estimated the share of variance in hate crimes across counties that can be accounted for by different covariates in fixed effects ordinary least squares regressions (see fig. SI 14). We find that our effect size estimates are larger compared to other covariates (e.g., native unemployment) that prior research has identified as important determinants of hate crime.

Do immigrant-attributed crime events increase the rate of hate crimes through processes of cognition (shifting attitudes), affect (emotional reactions), or both? While we cannot conclusively answer this question, we view the temporal persistence of our effects as suggestive evidence in this regard. As noted above, we observe a surge in the rate of hate crimes in the immediate aftermath of crime events attributed to immigrants. After about four days, the likelihood of hate crimes reverts back to its pretreatment level. We argue that the short-term effects we identify speak in favor of affect as a central driver of our results. If attitude shifts were the primary mechanism driving our results, we would expect a more persistent shift in the rate of hate crimes rather than a temporary, short-term surge. Accordingly, we interpret our results as evidence that out-group crime events act as an emotional trigger for individuals who already hold negative anti-immigrant attitudes.

In appendix section SI.6 we present the results from a battery of additional tests we conducted to ensure the robustness of our results. We demonstrate that our results are robust to (1) different choices of the bandwidth in days, (2) jackknife resampling at the county/state level, (3) excluding events that are not substantiated by official police press releases, (4) leveraging the full sample of more than 17,600 events regardless of partial overlap between events, and (5) conducting a simple difference-in-means comparison around the cutoff (see table SI 6). We address the possibility of spillover effects, reverse causality, and time-varying confounders through a number of additional tests, which we discuss in detail in appendix section SI.6. Notably, we do not find evidence for spillover effects: migrant crimes do not predict hate crime events in neighboring counties (see fig. SI 18). This suggests that the effects we identify are highly localized and thus distinct from the focusing events studied in prior research.

Moderators and mechanisms

We now turn to investigating the mechanisms that drive our results. Above, we argued that migrant-attributed crimes lead to cognitive and affective reactions based on perceived threat.

9. We arrived at this estimate by multiplying the effect estimate from the first model in table 1 (count outcome measure, linear polynomial) by the total number of refcrime events attributed to Muslim/African perpetrators (17,600) for a bandwidth of two days.

Table 1. Main Results

$\hat{\tau}$	p-Value	Crime Subset	DV	Bandwidth	Polynomial	N
.015	.01	Full sample	Count	2	Linear	26,131
.024	.03	Full sample	Count	3	Quadratic	36,215
.011	.03	Full sample	Binary	2	Linear	26,131
.021	.04	Full sample	Binary	3	Quadratic	36,215
.010	.047	Violent crime	Count	3	Linear	33,349
−.005	.55	Nonviolent crime	Count	4	Linear	16,482

Note. Results from regression discontinuity analyses. The outcome variable is hate crimes measured at the county-day level. The first column shows the local average treatment effect estimates ($\hat{\tau}$). The number of daily observations that fall within the bandwidth is shown in the last column (N). Because we use an optimal bandwidth selection algorithm, the bandwidth differs across samples and model specifications. Results for manual bandwidths are presented in fig. SI 17. We use linear or quadratic polynomials for estimation (see also app. sec. SI.2.7). For the results shown in rows 5 and 6, we subset to violent/nonviolent immigrant crime events before data transformation (see app. sec. SI.2.8 for more details). We use robust bias-corrected standard errors for all analyses. DV = dependent variable.

Unfortunately, we cannot test these mechanisms directly: perpetrators of hate crimes are a small population that is difficult to sample. However, we can derive and test observable implications for key mechanisms. We proceed in three steps.

First, we analyze what types of migrant-attributed crimes drive our results. As we outlined above, we expect that violent migrant-attributed crimes (e.g., stabbings, rape) would present a greater threat to the in-group and should thus be more likely to lead to hate crimes. To test this, in table 1 we reestimate our main model for two different subsets of our data: violent crimes and nonviolent crimes.[10] In line with our theoretical expectations, we find that our results are driven by violent migrant-attributed crime events, which are most likely to engender outrage among natives (Couttenier et al. 2019). We find smaller, statistically insignificant treatment effect estimates for the subset of nonviolent crimes such as traffic offenses, fare evasion, and other minor offenses.

In addition, we expect backlash to be less likely in reaction to crimes committed by perpetrators that are neither Muslim nor African. Following our theoretical framework, crimes committed by Eastern Europeans, for instance, should not result in xenophobic violence against refugees. To test this observable implication, we reestimate our main specifications for (i) crimes in which at least one perpetrator was reported to be from a European country, (ii) crimes in which no group-relevant information on the perpetrator was given, and (iii) crimes attributed to perpetrators of any nationality except Muslim-majority or African countries. We present the results in table 2. We do not find a backlash effect for any of these perpetrator groups. These findings suggest that we are indeed identifying group-based vicarious retribution dynamics against refugees. Perceived threat generated by out-group crime leads to xenophobic backlash.

Next, we test whether our results are driven by regions where the share of potential perpetrators who could be triggered by immigrant-attributed crimes is high or where relevant group dynamics might be more likely. We measure far-right mobilization potential using two complementary proxy measures: (i) the county-level vote share of the AfD in the 2017 federal election and (ii) state-level AfD membership (Niedermayer 2020).

We find that our results are driven by localities in which the potential for far-right mobilization is high (see fig. 3). We find large, statistically significant effect estimates in the subset of counties in which electoral support for the AfD is high and many individuals supported the AfD not just in elections but as active party members. Xenophobic backlash against immigrant crimes is most likely to occur in localities where the radical right enjoys deep-rooted local support. In contrast, we find small, statistically insignificant effect estimates when we subset to localities where the potential for far-right mobilization is low. We interpret this as evidence supporting the hypothesis that out-group crimes are more likely to trigger hate crimes when local group dynamics and norms are more permissive of hate crimes and when a substantial numeric reservoir of potential hate crime perpetrators exists.[11]

10. We provide more details on the classification of crime events in app. sec. SI.2.8.

11. We note that formal tests of the statistical significance of the treatment-moderator interactions in the RDIT setup require additional assumptions about the covariance between local average treatment effect estimates across subsets. We elaborate on this point in app. sec. SI.11, where we conduct significance tests for the results presented in fig. 3.

Table 2. Placebo Tests, Nonrefugee Perpetrator Groups

$\hat{\tau}$	p-Value	Perpetrator Subset	Crime Subset	Bandwidth	N
.004	.45	European	Full sample	3	30,732
−.003	.62	European	Violent crime	3	26,594
.002	.78	European	Nonviolent crime	5	20,022
.002	.62	Nationality unknown	Full sample	5	65,794
−.004	.34	Nationality unknown	Violent crime	3	42,221
−.006	.47	Nationality unknown	Nonviolent crime	4	16,719
.003	.58	Excluding Muslims/Africans	Full sample	3	31,352
−.005	.47	Excluding Muslims/Africans	Violent crime	3	27,221
.0004	.95	Excluding Muslims/Africans	Nonviolent crime	5	20,670

Note. Results from regression discontinuity analyses. The outcome variable is the number of hate crimes measured at the county-day level. The first column shows the local average treatment effect estimates ($\hat{\tau}$). The number of daily observations that fall within the optimal bandwidth is shown in the last column (N). We subset to (i) crimes committed by European perpetrators, (ii) cases for which the nationality of the perpetrator is unknown, and (iii) all perpetrator nationalities except Muslim-majority and African countries (see also app. sec. SI.2.9). We use a linear polynomial and robust bias-corrected standard errors for all analyses.

We also test whether the mere local number of and increase in migrants—a proxy for potential threat perceptions—increases the probability of backlash. We find that hate crimes in reaction to migrant-attributed crimes are more likely in areas where there has been a significant demographic change over a short time span, that is, where there has been a large proportional increase in the foreign-born population in recent years. Vicarious retribution is if anything more likely in areas with few foreigners. A recent increase in the immigrant population, not their absolute number, appears to create a context of out-group threat conducive to backlash. This finding aligns with prior research on the importance of overtime changes in the immigrant population as a determinant of attitudes and perceptions (Hopkins 2010; Newman and Velez 2014).[12]

Finally, we provide additional evidence on the broader social effects of migrant-attributed crime at the aggregate level. We draw on nationwide public opinion data to examine attitudes toward immigration before and after two of the most salient migrant crime events we discussed previously: the events in Chemnitz and the sexual assaults in Cologne on New Year's Eve 2015–16. These data do not show any meaningful public opinion shifts in the aftermath of these salient crime events. If anything, we find that natives, on average, are increasingly concerned about xenophobic violence against minorities during periods of intergroup conflict (for detailed results, see app. sec. SI.8). We interpret this as suggestive evidence that out-group threats do not appear to increase the legitimacy of xenophobic hate crimes in the broader population. We stress that our analysis focuses on averages across representative samples of the German population; perceived out-group threats might of course increase the legitimacy of xenophobic acts in some segments of the electorate. Moreover, we cannot rule out the possibility that perceptions of the legitimacy of vicarious retribution shift among potential perpetrators, regardless of the true shift in public opinion in the aftermath of crimes attributed to immigrants.

In sum, the evidence we find aligns with our theoretical account that treats out-group threat as a mobilizing event among already radicalized individuals. This account also aligns with anecdotal evidence from prominent cases such as the events in Chemnitz described in the introduction, where individuals with preexisting prejudice against minorities were triggered to commit hate crimes. We do not find evidence for broader legitimization of hate crimes in the aftermath of crimes attributed to immigrants. However, it is of course possible that perceptions of hate crime legitimacy shift among radicalized potential perpetrators, rather than objective indicators of legitimacy itself. We caution against a conclusive interpretation of our evidence on the individual-level mechanisms driving our results, as hate crimes are a rare phenomenon and perpetrators are generally not identifiable in public opinion surveys. It is possible that other mechanisms might be relevant in the context of our study.

CONCLUSION

A growing literature notes that salient national events drive negative attitudes toward minorities (e.g., Devine 2021; Frey

12. In app. sec. SI.7, we conduct supplementary analyses for additional moderator variables, including local economic conditions and East/West Germany. These additional analyses do not show clear patterns of effect heterogeneity.

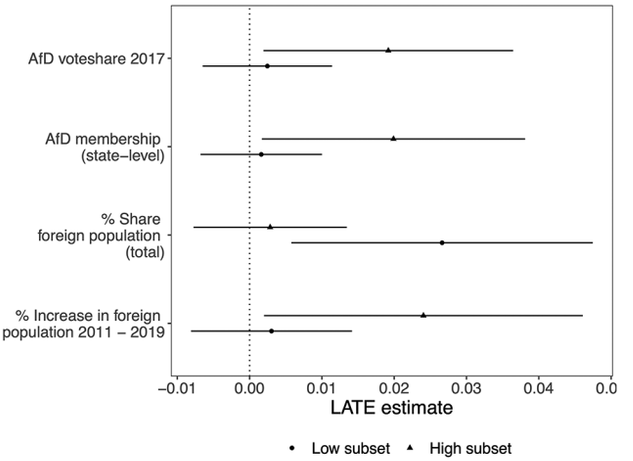

Figure 3. Local average treatment effect estimates from RDIT models. To test for effect heterogeneity, we reestimate the same model as for our main results in different subsets of the data. We created the subsets by first dichotomizing each moderator variable using a median split. We then subset our data to a "low" subset in which the moderator variable takes on values below its median value and a "high" subset in which the moderator is above its median value. Error bars indicate 95% confidence intervals.

2020; King and Sutton 2013). Our research adds a local intergroup perspective: localized threatening acts committed by out-group members—here approximated with crimes attributed to immigrants—can lead to a temporary surge in the rate of hate crimes. Using unique geocoded data on hate crimes and crime attributed to immigrants, we estimate the causal effect of such "migrant crime" on hate crimes using an RDIT design. We find a clear increase in the rate of hate crimes against refugees immediately after crimes attributed to immigrants from Muslim-majority or African countries. We also provide indicative evidence that the most plausible mechanism underlying this effect is a trigger for action among already radicalized individuals. Thus, the surge in hate crimes is clearer for violent acts attributed to immigrant perpetrators. Moreover, we find suggestive evidence of the extent to which immigrant-attributed crimes are seen as threatening matters, as the increase in hate crimes is greater where there has been a larger proportional increase in foreigners in recent years. In addition, in-group legitimization and dynamics may matter, as we find a greater retributive reaction in areas where there are more radicalized individuals, as measured by radical-right party strength. Recent research indicates that hate crimes may in turn foster radical-right support (Eger and Olzak 2023). We also find suggestive evidence that vicarious retribution does not emerge from a change in attitudes toward foreigners among the broader native population but rather stems from those who already hold strong negative prejudice toward immigrants and refugees.

Are our findings generalizable to other cases? We view it as plausible that the vicarious retribution dynamics we document in this article might apply in other countries and time periods. As we outlined in appendix section SI.1, the German case is typical in many ways, particularly in that (i) Germany hosts a large and growing immigrant population, (ii) immigration was a salient policy issue during our study period, and (iii) hate crimes against minorities have surged in recent years. The AfD is now an established radical-right party, making Germany much less exceptional in the European context. Against this background, we view Germany as a typical and "most likely case"—at least since the so-called refugee crisis. At the same time, more research is needed to better understand how political and societal contextual conditions moderate the mechanisms we lay out in this article.

Naturally the fact that migrant-attributed crime leads to hate crimes in no way legitimizes these acts. Instead, our findings are consistent with studies that show how perceived immigration-based threats have a galvanizing effect on those already predisposed toward negative attitudes, rather than a broader persuasive effect (Sniderman et al. 2004). Importantly, our findings do not provide support for an account in which two clearly defined social groups oppose each other, as would be the case for more broad-based ethnic tension and violence. Instead, crimes attributed to migrants—themselves a diffuse group—appear to trigger a narrow set of in-group members to commit hate crimes against (usually uninvolved) out-group members. This dynamic is not one that fits a simple story of community-based ethnic conflict, even if those who perpetrate hate crimes may see it that way. Interestingly, there is evidence that hate crimes in turn facilitate jihadi recruitment (Mitts 2019); these terrorist sympathizers are of course just as unrepresentative of the out-group as hate crime perpetrators are of the in-group.

While our study advances existing research by providing a local perspective on hate crimes and vicarious retribution, it comes with several limitations that future research might be able to address. First, questions about the mechanisms underlying local vicarious retribution dynamics remain. For data privacy reasons, we are unable to identify the perpetrators of hate crimes and thus cannot complement our quantitative work with in-depth interviews to tease apart the individual-level motives of offenders. We view this as a general gap in the literature on hate crimes: research on the motivations of hate crime perpetrators is rare, as offenders generally remain anonymous. We note, however, that those selected studies that have analyzed perpetrators cite "retaliatory motives" as an important driver of hate crimes (Byers and Crider 2002; McDevitt, Levin, and Bennett 2002).[13] Nevertheless, qualitative or quantitative data on the characteristics and motivations of hate crime perpetrators in our setting could help clarify the individual-level mechanisms driving our results.

We also want to highlight the possibility of measurement error in both our treatment and outcome variables. While we conducted a variety of robustness tests to verify that our results are not driven by systematic measurement error, underreporting remains a key challenge for the study of crime and hate crime specifically. Recent research by the European Union Agency for Fundamental Rights suggests that as many as nine out of 10 hate crimes remain unreported and hence practically invisible to researchers (FRA 2021).

Finally, more research is needed on the policy implications of our findings. At first sight, one might conclude that media outlets should not report on migrant crimes or, at the very least, avoid the use of language that conveys the out-group status of perpetrators (Couttenier et al. 2019). While it is perhaps uncontroversial that news outlets should refrain from descriptions such as "a dark-skinned man with thick lips, speaking broken German,"[14] this is not a conclusion necessarily to be drawn only from our research. The advice that journalists should refrain from stereotypical, racist descriptions does not depend on the consequences of such reporting. Indeed, to the extent that natives hold biased beliefs about the relative frequency of native versus immigrant perpetrators of crime, omitting (factual) identifying information from news articles might instead perpetuate biased beliefs about the prevalence of migrant crime and political slant in media reporting. More research is needed to understand how media outlets can disseminate information about salient local events—including crimes committed by both natives and immigrants—without facilitating the dynamics of prejudice and hate we have identified in this study.

13. McDevitt et al. (2002), e.g., draw on police records for about 160 hate crimes in Boston and identified a retaliatory motive in about 8% of cases. In a unique qualitative study of hate crime offenders, Byers and Crider (2002) identify similar motives driving hate crimes against the Amish. In a series of narrative interviews of eight hate crime offenders, perpetrators frequently justified their actions on the grounds of "deservingness," "either because of something that a specific Amish person had done to them, or for behaviors that the Amish, as a whole engage in which the offender found disturbing" (124).

14. "Dunkelhäutig, wulstige Lippen, er sprach gebrochenes Deutsch" (Pflüger-Scherb 2017).

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