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

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October 30, 2020

Abstract

This study examines the relationship between crimes attributed to immigrants and xenophobic hate crimes at the local level. We argue that localized crime events attributed to immigrants can lead to xenophobic responses whereby natives exact retribution against uninvolved out-group members. We examine such intergroup conflict dynamics between immigrants and natives in Germany, a country that has experienced a sharp increase in the foreign-born population in recent years. Our empirical analysis leverages fine-grained geo-coded data on more than 9,400 hate crimes and 60,000 immigrant-attributed crime events between 2015 and 2019. Using a regression discontinuity in time design (RDiT), we show that the daily probability of hate crimes doubles in the immediate aftermath of an immigrant crime event in a local community. Our results speak to growing concerns about xenophobic violence in Western democracies.

Early-stage work – please do not cite or circulate

full manuscript (*excluding abstract*)= 8178.

Please send correspondence to riaz@g.harvard.edu. We are thankful for research assistance by Lukas Aebi, Nikolaus Kowarz, and Sebastian Weber. We are thankful for comments from Carl Mueller-Crepon, Anselm Hager, Hanno Hilbig, Macartan Humpreys, Hunter Rendleman, Alex Scacco, and seminar participants at Harvard and WZB Berlin. Daniel thankfully acknowledges funding from the Swiss National Science Foundation (SNF Ambizione Grant, NO. 179938).

1 Introduction

At 3 am in the morning on the 25th of August 2018 in the East German city of Chemnitz, a group of young men begin arguing about cigarettes. Three of the men are injured by stabbing; one – 35-year old Daniel H. – dies in a hospital later that night. The suspected perpetrators are all refugees from Syria and Iraq, while the victims are Germans (ZEIT Online 2018). Protests against refugees follow the event, the radical right starts to mobilize; a dynamic of hatred and violence unfolds, culminating in an organized manhunt on suspected migrants and refugees in the city of Chemnitz, just one day after the knifing (Grunert 2018; Kampf, Pittelkow, and Riedel 2019).[1] 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 committed by out-group members can trigger violent intergroup conflict dynamics in high-income Western democracies.

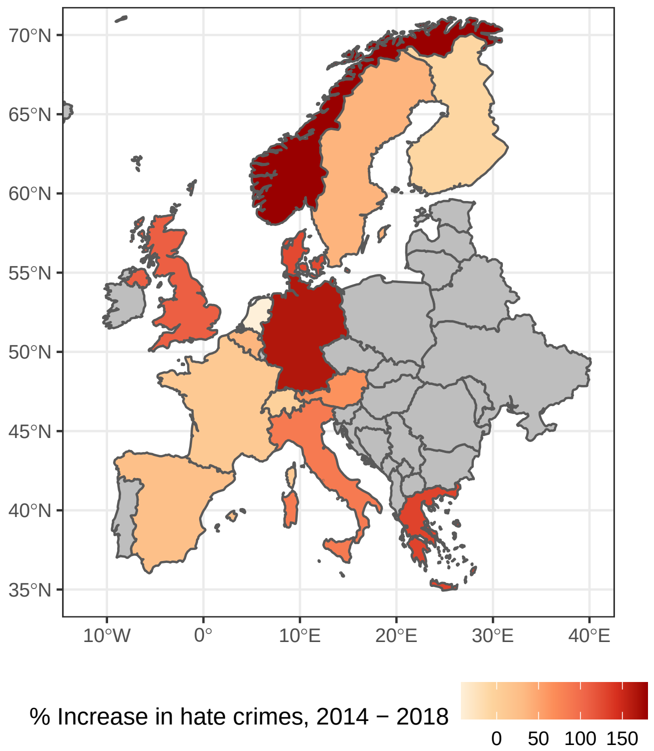
This paper studies such everyday, local dynamics of out-group behavior and in-group reaction. We argue that the events in Chemnitz exemplify a more general dynamic: localized threatening events attributed to out-group members can lead to a temporary surge in the rate of hate crimes. Crimes committed by an out-group member against an in-group member may thus lead to 'vicarious retribution' (Lickel et al. 2006), specifically hate crimes, perpetrated by other ingroup members. Here, we conceptualize hate crimes as 'criminal behavior motivated by bigotry' (Dancygier and Green 2010: 294), with bigotry defined as 'prejudice toward the victim's putative social group' (Green, Mcfalls, and Smith 2001: 480). We argue that everyday crimes conducted by migrants and reported in the media can lead to such vicarious retribution in the form of hate crime. In the wake of crime events perpetrated by *individual* out-group members, some in-group members therefore collectively exact retribution against out-group members as a whole.

Hate crimes are a serious challenge in many democratic societies. In Europe and the United States, hate crimes against minorities, and Muslims in particular, have surged in recent years (Kishi 2017; OSCE 2020). In Figure 1, we illustrate the increase in hate crimes between 2014 and 2018 across Western Europe. The OSCE reports 5,735 cases in 44 states in 2018 alone (ODHIR 2020).[2] In Germany,

[1] The *"Landeskriminalamt"* (Land Office of Criminal Investigation) analyzed chat protocols on several mobiles of radical right actors involved in the manhunt and found evidence for organization and the intention to hunt "foreigners" (Kampf, Pittelkow, and Riedel 2019).

[2] Notice, however, that the actual number of hate crimes is certainly higher as law-making on reporting of hate crimes is still underdeveloped and many hate crimes remain unreported.

Figure 1: Increase in hate crimes across Europe



Note: The map shows the percentage increase in the number of recorded hate crimes across European countries. The illustration is based on official data compiled by the OECD. Grey shading indicates missing values. For Denmark, we record the increase between 2015 and 2018 because data for 2014 is missing.

the number of hate crimes has more than doubled between 2014 and 2018.

Our focus on the local intergroup dynamics leading to hate crimes contributes to the exceptionally rich and varied body of social science research on intergroup conflict, which is generally studied through the lens of either group threat theory (Blumer 1958; Blalock 1967) or Allport's (1958) contact hypothesis. Much of this work makes use of public opinion polls and, more recently, survey experiments (e.g., Hopkins 2010; Hangartner et al. 2019). Often, this research studies how outgroup presence shapes ingroup attitudes and beliefs. However, empirical research on behavioral outcomes studying out- and ingroup behavior is still scarce (Paluck, Green, and Green 2019). Moreover, research on the *dynamics* of intergroup conflict in Western Democracies is still in its infancy. In the rare cases where behavioral dynamics between out- and in-groups are studied, scholars tend to focus on events that attract attention at the *national* level (King and Sutton 2013; Frey 2020), such as for instance the 9/11 terrorist attacks in the US (e.g. Legewie 2013; Hanes and Machin 2014). Our focus on the dynamics of behavioural outcomes at the local level therefore provides new insights into how intergroup conflict emerges and escalates.

Our empirical analysis draws on two original data sources. First, we collected detailed geocoded information on more than 60,000 crimes attributed to refugees and other immigrants in Germany between 2015 and 2019. This data is based on official police press releases and reports in the local news media. While the immigrant status of the criminal offenders is not always explicit, these reports contain explicit perpetrator descriptions that are clear cues of first- or second-generation immigrant status. We complement this data with official information released by the German federal government on hate crimes that occurred in Germany during the same time period. To causally identify the effect of immigrant crimes on subsequent hate crime events in the same locality, we use a sharp regression discontinuity in time (RDiT) design. We estimate the probability of xenophobic hate crimes right before and after the occurrence of immigrant crime events within a small temporal bandwidth of several days.

We find that the daily probability of a xenophobic hate crime in a county increases by more than 100% in the immediate aftermath of an immigrant crime event. 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. This effect is driven by a native backlash against violent migrant crimes in particular (e.g. stabbings). We find significantly smaller effect size estimates for petty crimes such as insults, fare evasion, and other minor offenses. Our results are robust across a variety of model specifications.

To shed further light on the mechanisms driving our results, we examine whether localized threatening events lead to an increase in prejudice against out-group among the general public, thus possibly shifting local norms towards a higher acceptance of violence against minorities. To test this, we analyze how public opinion shifts in the wake of *nationally salient* migrant crime events. However, we do not find evidence that natives become less supportive of immigration in the aftermath of salient migrant crimes such as the events described in Chemnitz. This aligns with previous research on the stability of attitudes towards migration (Kustov, Laaker, and Reller 2019) and suggests that a small subset of natives with pre-existing xenophobic attitudes vicariously exacts retribution for migrant crimes. The general public, on the other hand, seems rather unresponsive to salient migrant crime events. If anything, we find that natives, on average, are increasingly concerned about xenophobic violence against minorities during periods of intergroup conflict.

Our paper makes two important contributions to research on intergroup conflict. First, we introduce a new perspective on hate crimes as triggered by local, commonplace outgroup behavior. In times in which everyday racism is ever more uncovered and publicly debated, our work goes beyond previous research by studying the dynamics of 'everyday racism' in Western democracies. We disentangle the internal dynamics within small local communities, and show that hate crimes are not only triggered by large-scale focusing events but instead seem to be at least partly driven by everyday intergroup conflict. Second, we are amongst the first to provide causal evidence for the drivers of hate crimes (but see: Marbach and Ropers 2019). By leveraging detailed geo-coded event-level data on both crimes committed by immigrants and hate crimes in a regression discontinuity setup, we introduce a new way of studying xenophobic violence in the social sciences.

2 What we know about the drivers of intergroup conflict

A rich body of research in the social sciences investigates intergroup relations (see, e.g., Paluck, Green, and Green 2019). The theoretical underpinnings mostly consist of Allport's (1958) contact hypothesis and group threat theory (Blumer 1958; Blalock 1967). Despite this wealth of research, recent work has provided new insights in two ways.

First, researchers are increasingly focusing on behavioural rather than attitudinal indicators of intergroup conflict. A large part of the copious research on intergroup relations had focused on studying attitudes and beliefs, with scholars for instance trying to understand whether the influx of refugees changes the perception of migrants by the 'host' population (Enos 2014; Bansak, Hainmueller, and Hangartner 2016; Hangartner et al. 2019; Rudolph and Wagner 2020). While engaging with the question of how bias towards out-groups are shaped by their presence, these studies tend to leave aside whether the actual behavior of out-groups plays a role in shaping the in-group's response to increasing heterogeneity. In contrast, recent field experimental research more carefully studies intergroup conflict not only in terms of attitudes but also in terms of the behavior (Paluck 2012; Paluck, Green, and Green 2019). For instance, Mousa (2020) shows that intergroup contact within football teams reduced the prejudice held by Christians towards Muslims in post-war Iraq. In Europe, research has examined whether immigrant presence shapes voting behaviour, in particular in the context of recent refugee arrivals (Dinas et al. 2018; Schaub, Gereke, and Baldassarri 2020; Evans and

Ivaldi 2020). In summary, a rich body of research in political science investigates various aspects of intergroup conflict but until recently has largely focused on attitudes instead of behavioral outcomes such as hate crimes.

Second, recent work has provided new insights into the dynamics of intergroup conflict, turning attention towards variable drivers of tension rather than comparatively stable factors (e.g., McDoom 2013; Balcells, Daniels, and Escribà-Folch 2016). The study of hate crimes provides a good example of these trends. There has been extensive sociological and criminological work on what kinds of people engage in hate crimes (for summaries see, e.g., Green, Mcfalls, and Smith 2001; Dancygier 2010; Dancygier and Green 2010; Green and Spry 2014). Almost by definition, perpetrators of hate crimes hold biased, xenophobic perceptions of the targeted out-group (King and Sutton 2013). Hate crimes are committed because offenders have grievances and strong stereotypes against another group (Craig 1999). Wahl (2003) points to a history of delinquency among hate crime offenders (though see McDevitt, Levin, and Bennett 2002). Hate crime perpetrators are seldom motivated by ideological conviction, with offenders often more characterized by a difficult family background and rather incidental motivation (Dancygier and Green 2010). Hence, McDevitt, Levin, and Bennett (2002) see hate crime offenders mostly as 'thrill-seekers'. A large amount of studies have investigated what kinds of individuals are more likely to hold negative out-group attitudes and commit crimes based on these.

However, the occurrence of hate crimes wax and wane over time, so the question remains what contextual drivers lead to this form of intergroup conflict. One strand of research focuses on the long-term societal context in which hate crimes occur. This literature is generally focused on explaining when out-groups are perceived as threatening, as such threat perceptions are a key factor in explaining negative attitudes towards out-groups (Stephan and Stephan 2000; Huddy et al. 2005). This perceived threat may give rise to an 'urge for vengeance' (Jungkunz, Helbling, and Schwemmer 2019: 273). Here, explanations of when hate crimes occur have turned to structural factors, in particular the characteristics of regions or communities that correlate with higher levels of hate crimes. The negative out-group attitudes that form the basic impetus for hate crimes are usually not formed overnight; instead, they often have roots in long-standing factors (Voigtländer and Voth 2012, 2015). Hence, this research still concerns comparatively slow-moving factors, though relating to societal context rather than psychological predispositions.

However, the role of salient, galvanizing events has recently been investigated in a number of studies (King and Sutton 2013; Lickel et al. 2006; Frey 2020). Such key events – e.g. terrorist events – are central to the development of perceptions of out-groups as threats (Frey 2020). From a psychological perspective, hate crimes can be seen as ingroup reactions to threatening events. Indeed, there is extensive evidence that terrorist attacks can reinforce negative stereotypes about immigrants and Muslims in particular (Legewie 2013; Hopkins 2010; Echebarria-Echabe and Fernández-Guede 2006; Bozzoli and Müller 2011; Kam and Kinder 2007; Peffley et al. 2015; Bar-Tal and Labin 2001; Schüller 2016; Disha, Cavendish, and King 2011).[3]

3 A dynamic perspective on intergroup conflict triggered by galvanizing local events

Drawing on recent research on the role of galvanizing events, we view hate crimes as a form of 'vicarious retribution': events where, after out-group members attack an in-group member, (uninvolved) out-group members are 'punished' for this by (uninvolved) in-group members (Lickel et al. 2006). Against this background, perpetrators engage in hate crimes out of a desire to exact revenge and protect the in-group.

The galvanizing events considered in research so far all received focused and sustained national and even international media attention – such as the 2015/16 New Year's Eve sexual assaults in Germany (Frey 2020). King and Sutton (2013) suggest that such 'considerable publicity' is a necessary precondition for antecedent events to spark a violent reaction of retribution. However, it is unclear whether this means that the antecedent events need to be national headline news or whether salient *localized* events can also lead to hate crimes. In this paper, we investigate the effects of smaller-scale, local events on hate crimes.

Why might localized threatening event leads to in-group backlash? We propose three channels through which localized threatening events may lead to hate crime.

The main effect of a localized threatening event may be to act as a trigger to engage in crimes directed at the out-group. A local threat may galvanize action, especially if it lends itself to being interpreted as an outgroup attack (Lickel et al. 2006). Emotions may play a particular mediating role

[3]However, the evidence is not unequivocal on the effects of terrorist attacks (Castanho Silva 2018; Van Hauwaert and Huber N.d.).

here. For example, the threat may increase anger on the part of in-group members predisposed towards hate crimes. This anger could then act as an important motivational driver of retribution (Unkelbach, Forgas, and Denson 2008; Spanovic et al. 2010; Leonard, Mackie, and Smith 2011; McDoom 2012; Zeitzoff 2014).

However, localized threatening event may be more than a trigger and lead to individual-level changes in attitudes and beliefs. For example, they might lead to persuasion, with the events leading to more negative out-group bias. When a criminal act by an immigrant occurs locally and receives public attention, this may increase negative attitudes towards the out-group as well as perceptions of how threatening this group is.[4] This mechanism fits with evidence that events such as terrorist attacks increase perceptions of out-group threat (Stephan and Stephan 2000; Huddy et al. 2005) and negative out-group attitudes (Legewie 2013). We would not expect this negative impact of threatening events to occur across the board. As Peffley et al. (2015); Jungkunz, Helbling, and Schwemmer (2019) suggest, it may be individuals with pre-existing xenophobic grievances who react more strongly to the threat. Hence, local threatening events may lead to hate crimes because they lead to attitude change among those already predisposed towards out-group attacks.

Another way in which localized threatening event may change individual-level beliefs is through legitimation. When a criminal act by an immigrant occurs locally and receives public attention, this may affect the perceived acceptability of hate crimes among the broader population (Lickel et al. 2006; Dancygier and Green 2010). When a community fails to condemn hate crimes, this may legitimise such acts of bigoted violence (see also Weaver 2019). Hate crimes may depend on whether the broader community encourages - or at least fails to dissuade - such acts. The legitimacy of violence towards out-groups has been linked to legimitation in other cases, such as lynching (Weaver 2019). In our case, perpetrators of hate crimes may not change their attitudes due to localized threatening events; instead, they might become more likely to act on these attitudes because they perceive that doing so would be accepted – or at least not condemned – by other ingroup members in the community. Local galvanizing events may provide the trigger for the belief that attacks against out-groups would be tolerated.

[4]Another possibility is that hate crimes increase perceptions of entitativity, that is, how homogenous the out-group appears to be (Lickel et al. 2006). This effect may push some ingroup members to the level where they contemplate a hate crime.

Against the backdrop of these mechanisms, we formulate our main empirical hypothesis:

> H: The probability of a hate crime in a given county increases in the immediate aftermath of immigrant-attributed crime in the same county.

4 Research design

4.1 Data

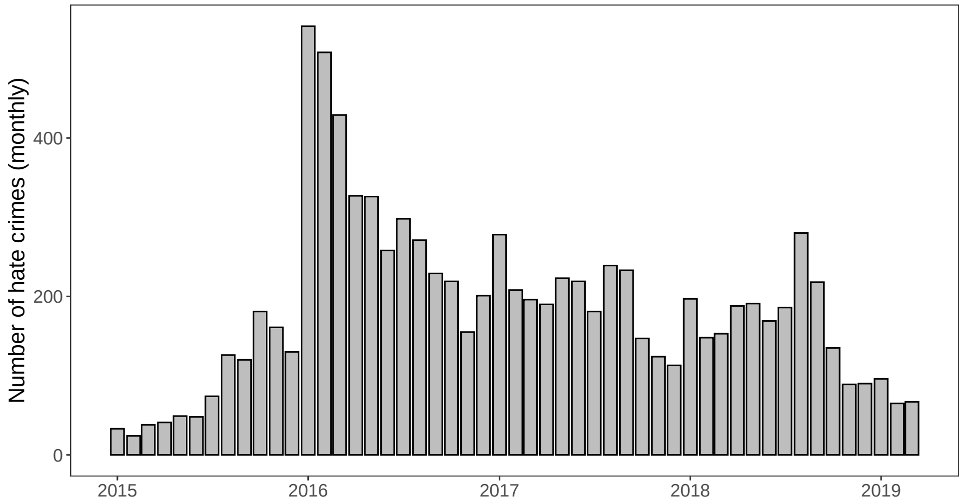
To examine the relationship between migrant crime events and xenophobic hate crimes, we rely on two novel data sources. To measure hate crimes, we collected detailed information on more than 9,400 xenophobic hate crime incidents that occurred in Germany between January 2015 and March 2019. The German police, by default, does not release event-level information on hate crimes. However, the German socialist party – *Die Linke* – inquires about hate crimes that occurred in Germany through 'parliamentary requests' (*'kleine Anfrage'*) every quarter. 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 also observe detailed information on the precise location and date of each event. Our comprehensive dataset hence covers all hate crime incidents recorded by state agencies in Germany between January 2015 and March 2019.

Our data only allows us to analyze those crimes that are classified as 'hate crimes' under German law by the German police and intelligence services.[5] This raises the concern that German authorities might have incentives to under-report xenophobic hate crimes, which could introduce measurement error into our main outcome variable. The true number of hate crimes might be higher than what the official sources suggest. To address this concern, we verified that our data strongly correlates with information collected by independent NGOs. The bivariate correlation between the total number

[5] It is important to note that the precise definition of 'hate crimes' varies from country to country. This makes cross-national comparisons particularly difficult and research on hate crimes by definition a fairly local endeavor. For instance, German law forbids the display of fascist/Nazi symbols such as the Hitler salute. Most other European countries and the US do not treat the display of these symbols as hate crimes. That said, "only" 13% of all hate crimes in our data relate to the display of fascist symbols while the remaining crimes range from verbal abuse to assault all the way up to murder (0.14%) – acts which are generally also categorized as hate crimes outside of the German context. It is important to note that hate crimes were not treated as a single, separate form of crime in Germany up until 2001. Since 2001 the police and courts treat crimes as "politically motivated crimes" if they are subject to right-wing extremist thinking and backgrounds.

of events recorded by county in our dataset and the data released by Benček and Strasheim (2016), based on NGO reports, is 0.98.[6] We present an overview of the most common types of hate crimes observed in our dataset in Table SI 3. The three most common types of offenses 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 dataset.

Figure 2: Hate Crimes 2015 – 2019



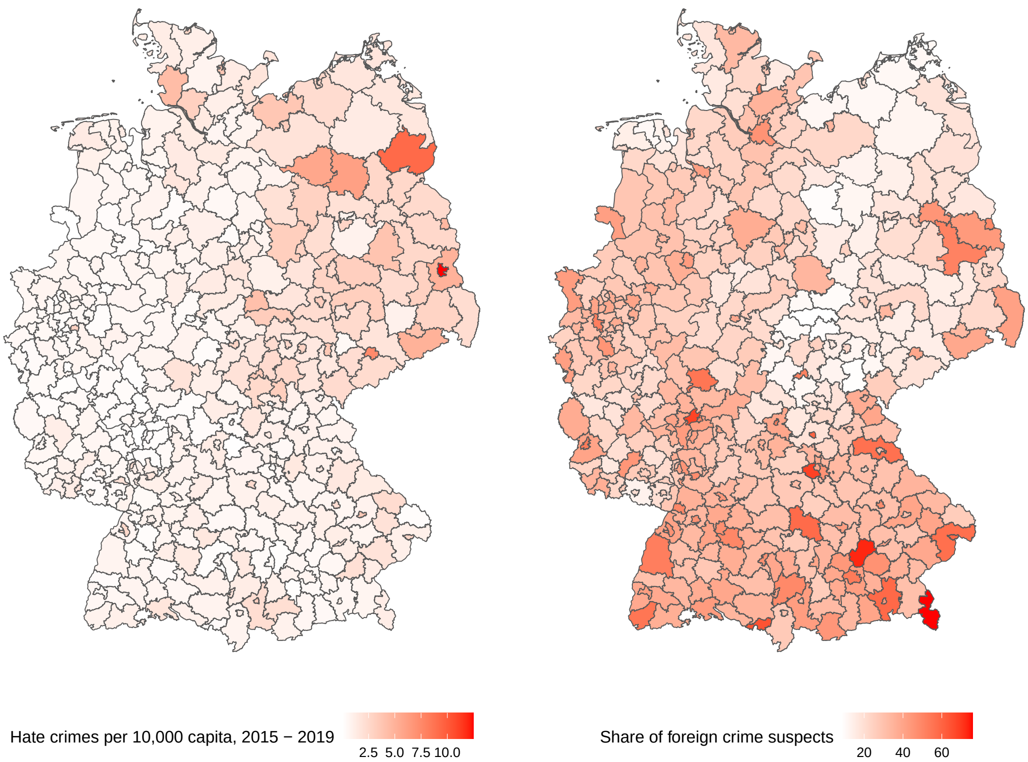
Note: The figure shows the number of hate crime incidents in Germany in each month between January 2015 and March 2019.

We visualize the temporal and spatial distribution of hate crimes during our study period in figures 2 and 3. A number of descriptive facts stand out. First, hate crimes occur more frequently in East Germany. This is particularly noteworthy as the overall share of the foreign-born population in East Germany only stands at about 5%, compared to about 15% in the West. Second, we note that the number of hate crimes has increased sharply in the wake of the influx of refugees into Germany since 2014 (see also Frey 2020; Marbach and Ropers 2019). In particular, we observe a sharp increase in the level of hate crimes after the 2015–16 New Year's Eve sexual assaults in the city of Cologne, where women in public places had been surrounded and sexually assaulted by groups of mostly foreign men. These events received an enormous amount of media attention at the national level, and arguably constitute the most prominent migrant crime incident in Germany in recent years.

We complement our hate crime data with detailed information on more than 60,000 "immigrant

[6] Because the time period covered in this alternative hate crime dataset is much shorter (only up until November 2017), we do not use it for our main analysis.

Figure 3: Spatial distribution of hate crimes and migrant crimes



Note: The left figure shows the spatial distribution of the total number of hate crime events that occurred between January 2015 and March 2019 across German counties. We scale the number of recorded events by county population size. The right figure shows the share of foreigners among all crime suspects identified by the German police in 2019 across German counties. The visualization on the right is based on official data provided by the German police (*Polizeiliche Kriminalstatistik, PKS*).

crime events" that occurred between January 2015 and March 2019. 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. This 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 section SI.1.2 in the Supporting Information. We implemented 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 generally based on local news articles. Local newspaper in Germany generally report true information and constitute a highly trusted news sources among constituents (Nic et al. 2018: p.

81). As an additional check of face validity, we demonstrate that number of reported events across counties strongly correlates with the overall share in the foreign-born population, but does not seem to be systematically associated with anti-immigrant sentiment at the local level.

It should be noted that we do not directly observe the citizenship (or second-generation migration background) for the perpetrators of many crime events listed on refcrime.info. In 40% out of the 200 random refcrime events we validated, the original source does not *explicitly* state that the perpetrators are foreign citizens or refugees. However, what almost all (95%) of the sources share are *heuristics* which suggest that the perpetrators are not part of the “German ingroup”; e.g. “spoke in broken German”, “black Africans”, “Southeast European appearance”, “foreign speaking”. Our validation, thus, clearly highlights that the web-hosts list any crimes which are associated with a cue that a perpetrator is potentially not German as a migrant crime. While some of these perpetrators could be first-generation migrants, they might also be German citizens whose ancestors migrated to Germany. Crucially for our analysis, the original sources contain *clear* outgroup heuristics in virtually all cases.

In addition to the date and precise location of the reported crime events, we also observe the type of crime that occurred (homocide, battery, theft, rape, etc.). We are hence able to distinguish between the effects of violent and non-violent ‘petty’ crimes. A detailed overview of how we classified violent and non-violent crimes can be found in the SI (see Table SI 2). We provide summary statistics of the immigrant crime data in table SI 1 in the Supporting Information.

4.2 Estimation & identification strategy

To estimate the causal effect of migrant crimes on hate crimes, we use an interrupted time-series approach. More specifically, we use a regression discontinuity in time design (RDiT) – a regression discontinuity design using time as the running variable (Hausman and Rapson 2018; Mummolo 2018). Our data is well-suited for this approach as we observe the precise date and location for all recorded incidents of migrant crimes and xenophobic hate crimes.

Most previous studies using RDiT models rely on a single discontinuity in time – such as the day of a strike in LA or a specific one-time policy intervention (see e.g. Anderson 2014). In this setting scholars can rely on the modelling strategy as outlined in detail elsewhere (Hausman and Rapson

2018). In contrast, we observe more than 60,000 individual migrant crime events. To analyze this rich dataset in an RD-framework, we transform our raw data into a panel where immigrant crime events are the main units of analysis.

For each migrant crime, we consider the two-week period before and after the event. Hence, 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 the county in which it occurred, and t is the date. The outcome $Y_{i,t,c} \in 0, 1$ is a binary indicator that takes on the value one if at least one hate crime occurred in county c at a given date t. 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, i.e. $T_{i,t,c} = 1$ if $P_{i,t,c} >= 0$. We provide details on how we deal with (partially) overlapping event periods in section SI.1.5 in the Supporting Information. We also establish that the counties in our final sample are representative for Germany as a whole.

We then estimate a sharp regression discontinuity 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 regression discontinuity 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. This allows us to estimate the local average treatment effect on the treated:

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

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 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 *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, etc.).

Unless otherwise noted, we use a local linear polynomial to reduce the sensitivity of our inferences. In all presented analyses, we estimate robust bias-corrected standard errors and use a triangular kernel function as recommended by Cattaneo, Idrobo, and Titiunik (2019). 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 MSE of our treatment effect estimates. In our preferred specification, we use an optimal bandwidth of just 4 days before and after migrant crime events. In section 5.2 we demonstrate the robustness of our results to different functional forms of the regression function and varying bandwidths around the treatment assignment cutoff.

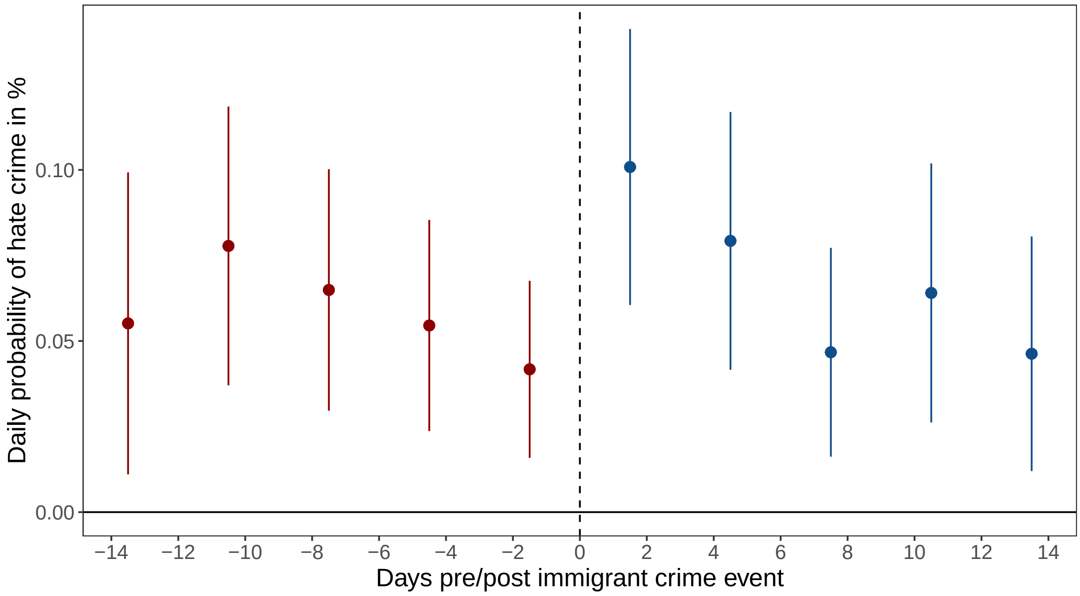
5 Results

5.1 Main findings

Before presenting our main results, we first examine the raw correlation between migrant crime events and hate crimes. In Figure SI 6 in the Supporting Information, we show the bivariate relationship between the number of recorded migrant crime events and hate crimes in a given month across counties. Descriptively we find a positive correlation between the two variables ($r = 0.2$). Of course, other factors such as economic prosperity or police capacity might drive this correlation. In addition, we are unable to disentangle the temporal ordering of migrant crimes and hate crimes in this analysis. While being a first and simple indication that hate crimes might in part be driven by migrant crimes, such a correlation – even if robust in a time-series-cross-sectional analysis – does not provide sufficient evidence for a causal relationship. Below we address these concerns by relying on our RDiT estimation strategy as outlined in detail in section 4.2. In Figures 4 and 5 we present our main results from this analysis.

We find that the daily probability of a xenophobic hate crime in a county increases by more than 100% in the immediate aftermath of an immigrant crime event. 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. We see a strong increase in the daily probability of hate crimes in the 4-day period following migrant crime events from about 0.04% to more than 0.1% per day. After about 4 days, the likelihood of hate crimes reverts back to its pre-treatment level. The effect sizes may appear small at first. We stress however that we estimate the causal effect of migrant crimes on the *daily* probability of hate crimes in a given county, which explains the very low baseline level of the outcome variable. A back of the envelope calculation on the basis of our findings suggests that about 120 additional hate crimes occurred because of the 60,000 migrant crime events we observe.

Figure 4: RD Plot



Note: The plot shows the daily probability of a xenophobic hate crime in a county (in %) in the two-week period before and after an immigrant crime event occurred. Error-bars indicate 95% confidence intervals. Each bin summarizes 3 days.

In Table 1, we show that our results are robust across different functional forms for the local polynomial that we fit on both sides of the treatment assignment cutoff and also hold when simply comparing the difference in means before and after an immigrant crime event. Reassuringly, we also find that our results are driven by violent migrant crime events (see Table SI 2), which are most likely to engender outrage among natives. We find much smaller, statistically insignificant effect size estimates for the subset of petty crimes such as insults, fare evasion, and other minor offenses. Our results also remain unchanged when we only consider the subset of those migrant crime events which are backed up by official police reports.

In section SI.4 we test for treatment effect heterogeneity depending on regional characteristics including local support for right-wing parties, East vs. West Germany, and the share of the foreign population. We do not find evidence that the treatment effect we identify depends on regional level characteristics. Retribution against migrant crime events appears as a more general phenomenon in our data, regardless of local contextual characteristics.

Table 1: Main results for different model specifications. The outcome variable is the probability of a hate crime in a county on a given day in percentage points. The first two rows show the results from simple difference in means analyses before and after immigrant crime events. For these models, we manually choose a bandwidth of 6 days before and after the treatment events. Rows 3 to 6 show our main results from regression discontinuity in time analyses using optimal bandwidth selection and robust bias-corrected standard errors. Rows 7 - 9 show results from RDiT models for different subsets of migrant crime events: 1) violent crimes, 2) petty crimes, and 3) migrant crime events substantiated by official police reports. The number of observations that fall within the bandwidth, i.e. the sample size, is shown in the last column.

$\hat{\tau}$	$\hat{\sigma_\tau}$	p-value	Estimation	Subset	State FE	h	Polynomial	n
0.04 **	0.020	0.044	Difference in means	Full sample	No	6	/	81481
0.04 **	0.020	0.044	Difference in means	Full sample	Yes	6	/	81481
0.157 **	0.065	0.015	RDiT	Full sample	No	4	Linear	58133
0.158 **	0.065	0.015	RDiT	Full sample	Yes	4	Linear	58133
0.165 *	0.088	0.061	RDiT	Full sample	No	6	Quadratic	81481
0.165 *	0.088	0.061	RDiT	Full sample	Yes	6	Quadratic	81481
0.155 *	0.084	0.067	RDiT	Violent crime	No	3	Linear	43106
0.068	0.075	0.364	RDiT	Petty crime	No	5	Linear	70116
0.172 **	0.070	0.014	RDiT	Police reports	No	4	Linear	54893

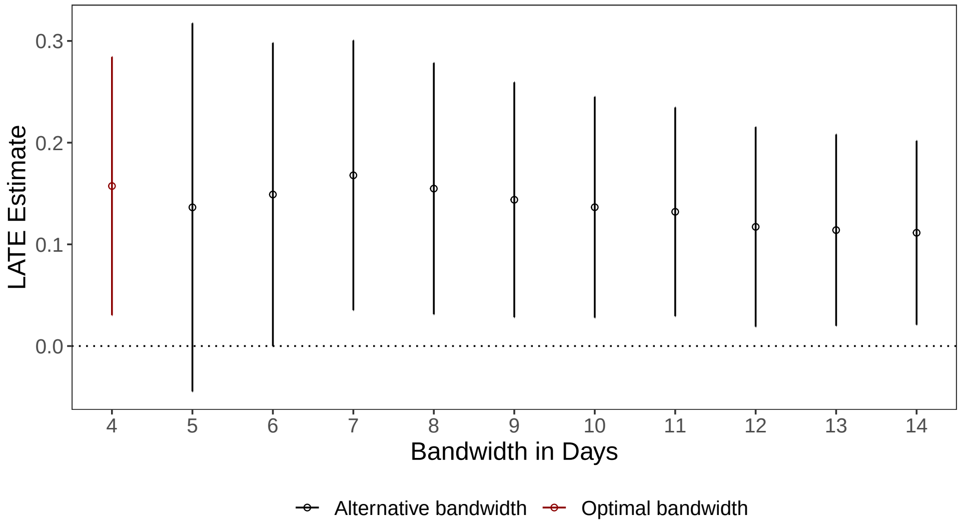
5.2 Robustness

We conduct a series of tests to ensure the robustness of our results. First, one potential concern in RD studies is that the results might be sensitive to the choice of the bandwidth around the treatment assignment cutoff. To address this concern, we report the results from RDiT models using varying bandwidths in Figure 5. Even for bandwidths as small as 4 days around the cutoff (the optimal bandwidth), we find strong and statistically significant local average treatment effect estimates. The point estimates remain fairly stable across all specifications we report.

Second, spillover effects might be a concern in our study design. It is likely that hate crimes are either subject to local clusters (e.g. Chemnitz and adjacent counties) or that the occurrence of hate crimes themselves lead to an increase of hate crimes in adjacent counties. To address this concern, we devised a test for spillover effects. Specifically, we repeat the same RDiT analysis as for our main results but now examine whether immigrant crime events in county c are associated with hate crime events in the closest *adjacent* county.[7] In Figure SI 8, we present the results. We find no evidence for spillover effects: migrant crimes do not predict hate crime events in adjacent counties. This analysis can also be viewed as a placebo test: replacing the outcome of interest, i.e. hate crimes in the *same* community in which the migrant crime occurred, with a placebo outcome, we find no discontinuity at

[7] We determined the closest adjacent county by calculating the geographic distance between county centroids.

Figure 5: RD results for varying bandwidths



Note: Local average treatment effect on the treated estimates from RDiT models. We vary the bandwidth from 4 to 14 days on both sides of the cutoff. The outcome variable is the daily probability of a hate crime in a given county in percentage points.

the treatment assignment cutoff.

Third, we conduct another placebo test in which we switch the outcome and treatment variables. We reproduce the same analysis as for our main results but now examine the likelihood of migrant crimes before and after hate crime events. Reassuringly, we do not find any evidence that migrant crimes are more likely to occur after hate crime incidents (see Figure SI 9).

For our main analysis, we consider counties as treated starting on the day a migrant crime occurs. However, it may take a while until information about such events spreads to the native population. To account for this, we devise a test akin to the logic of a 'donut-RD' design. We leverage the fact that we observe the exact timing of a subset of migrant crimes in our dataset. Reassuringly, we find that our results remain unchanged when we rely only on the day of the migrant crime event for events that occurred before 7 am in the morning (see Figure SI 10).

We rule out that we simply identify temporal noise in the data. It might be the case, for instance, that migrant crimes and hate crimes simply tend to cluster on similar days of the week. We verify that this is not generally the case in figure SI 7: both migrant crimes and hate crimes approximate a uniform distribution over the days of the week. If anything, hate crimes are most likely to occur on Saturdays, whereas migrant crimes are most frequently observed on Tuesdays in our dataset.

Finally, we provide evidence that the days before and after migrant crime events are similar in terms of observable climatic characteristics. Drawing on daily rainfall data recorded on over 2,000 weather stations all across Germany, we show that rainfall recorded at the county-day level is constant around the treatment assignment cutoff (see Figure SI 11).

5.3 Mechanisms

So far we established a causal relationship between migrant crimes and subsequent hate crimes. Yet, we know little about the mechanisms standing behind this effect. However, it is clear that hate crimes constitute an extreme response to migrant behavior; even though hate crimes are fairly frequent in Germany today, they are far from being legitimized everyday actions. Based on previous research, we argue that perpetrators of hate crimes generally hold strong xenophobic attitudes. Thus, the fundamental question for us is to find out *how these attitudes are activated and result in xenophobic violence.*

As outlined in our theoretical section, at least two mechanisms seem plausible. First, migrant crimes may legitimate hate crimes in the eyes of the general public, which may reduce inhibitions to commit hate crimes. Alternatively, our causal effect might be driven by persuasion and 'activation' of a small subset of natives with pre-existing prejudices against minorities. The latter process describes a more isolated, individual-level phenomenon, while the former emphasizes the societal context in which hate crimes take place.

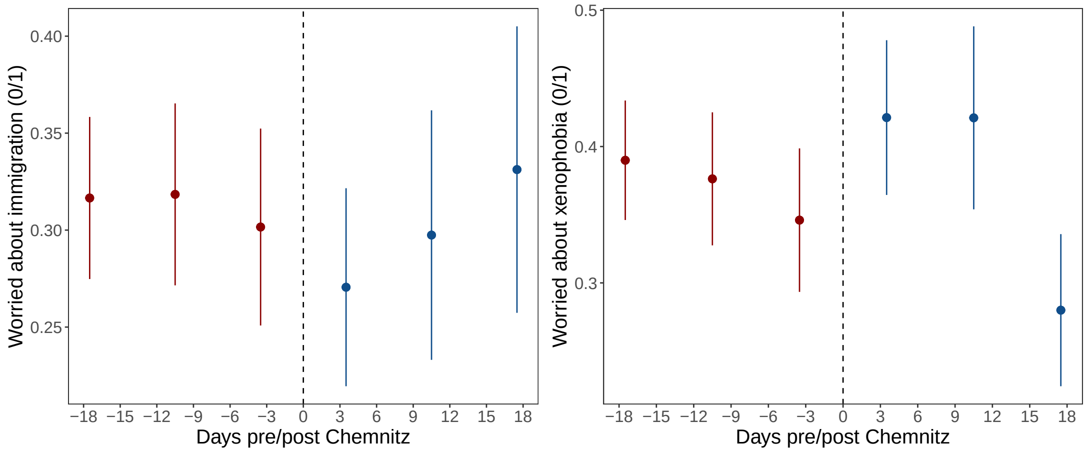
While it is difficult to test the latter mechanism – we do not have access to perpetrators of hate crime – the former mechanism is partially testable. To do so, we turn to public opinion data from the 2019 German socio-economic panel (SOEP). The SOEP is a long-running annual panel study of roughly 13'000 German households and 30'000 respondents. It is representative at the federal level and interviews respondents from January until November each year. Along with a rich amount of socio-economic questions, the interview date for each respondent, the SOEP also indludes some questions on migration along with an extensive 'worry scale' – e.g. *"How worried are you about migration in general?"*.[8] We also leverage the 2015/2016 survey waves of the *German Longitudinal Election Study* (GLES).

We test whether attitudes towards migrants shifted after the two migrant crime events that

[8] Unfortunately the SOEP provides an ordinal 3 point scale to answer this question. Thus, like previous research we re-coded this into a dummy, being 1 whenever a respondent replies to be "very concerned" (=3).

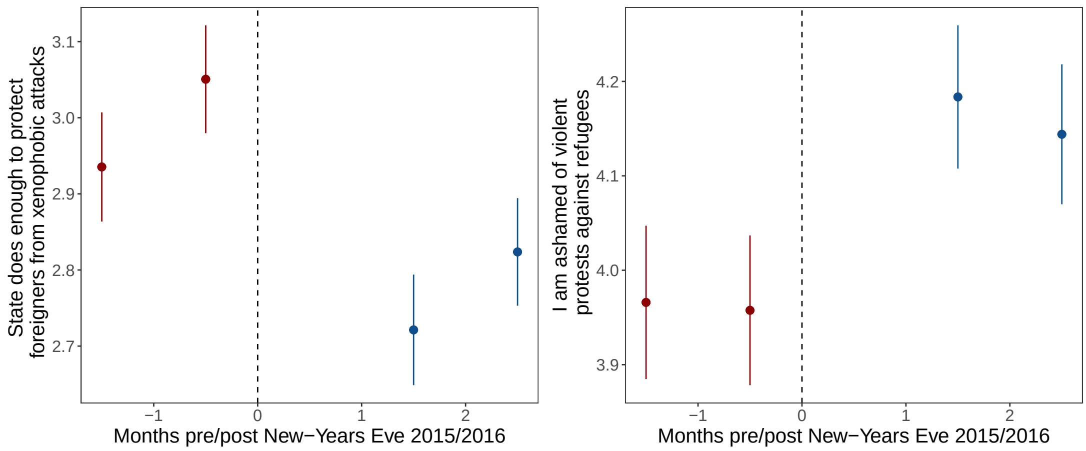
arguably attracted most attention nationwide in Germany: the events in Chemnitz described in the introduction, as well as the sexual assaults in Cologne on New Year's Eve 2015–2016. These events provide an excellent test case to study how public attitudes are affected by migrant crimes gaining national attention.

Figure 6: Attitudes towards migration before and after Chemnitz (SOEP)



Note: The figure shows attitudes towards migration among a representative sample of Germans in the two-week period before and after the Chemnitz incident on August 25, 2018. We compare respondents interviewed right before and after the event ($n = 1216$). We aggregate the responses to seven different outcome variables measuring attitudes towards migration in weekly bins. We use data from the German Socio-Economic Panel (SOEP) 2018 survey wave.

Figure 7: Attitudes towards migration before and after New-Years Eve 2015/2016 (GLES)



Note: The figure shows attitudes towards migration among a representative sample of Germans in the months before and after the sexual assaults on New-Year's eve 2015-2016 in Cologne. We use data from the 2015 and 2016 survey waves of the German Longitudinal Election Study (GLES).

In figures 6 and 7 we report our findings from this analysis.[9] We do not find evidence that natives became less supportive of immigration in the aftermath of salient migrant crimes such as the events

[9]We present the results for additional outcome variables relating to migration attitudes in figures SI 12 and SI 14 in the Supporting Information.

described in Chemnitz. The general public seems rather unresponsive to salient migrant crime events. If anything, we find that natives, on average, are increasingly concerned about xenophobic violence against minorities during periods of intergroup conflict. Importantly, these results remain unchanged when zooming in on respondents who hold negative attitudes towards migration from the start. In figure SI 13 in the appendix, we exploit the panel structure of the SOEP survey and replicate the same analysis for a subset of respondents who in 2017, prior to the Chemnitz events, indicated that they are 'somewhat' or 'very' concerned about immigration to Germany.

Our survey evidence aligns with previous research on the stability of attitudes towards migration (Kustov, Laaker, and Reller 2019) and suggests that a small subset of natives with pre-existing xenophobic attitudes vicariously retributes against migrant crimes. This leaves us with persuasion as a potential mechanism. As discussed above we do not have access to perpetrators of hate crimes. But what we do have is access to newspaper articles reporting about these crimes. And in the case of Chemnitz we learned that the perpetrators were already organized in radical right networks and exploited the events to use violence against outgroups. Are such acts of persuasion a common mechanism standing behind hate crimes?

6 Conclusion

A rich body of research investigates the correlates of hate crimes (Green, Glaser, and Rich 1998; Green, Mcfalls, and Smith 2001; Dancygier and Green 2010), and recently made tremendous efforts in trying to understand how salient terrorist events drive negative attitudes towards minorities (e.g., King and Sutton 2013; Frey 2020). Yet, how everyday interactions and perceptions of immigrant behavior affect hate crimes remains unstudied. However, given the recent rise of hate crimes across Europe and North America we believe that it is important to understand the *everyday dynamics of out-group hatred and violence in our communities*, especially in the context of democratic societies.

In this paper, we set out to study the dynamics between out-group behavior and in-group reaction. We hypothesize that localized threatening events by out-group members – here approximated with reporting on crimes attributed to immigrants – can lead to a temporary surge in the rate of hate crimes in an effort to exact 'vicarious retribution'. Using new and unique geo-coded data on hate crimes and crime attributed to immigrants, we use a regression discontinuity in time modeling strategy to estimate the causal effect of such 'migrant crime' on hate crimes. We find a clear effect of crimes attributed to migrants. We also provide evidence that the most plausible mechanism standing behind this effect is by providing a trigger for action, rather than persuasion or legitimation.

While our study advances existing research by providing a local and everyday xenophobia perspective for research on hate crimes, it comes with several limitations future research might be able to address. Most prominently, questions about the mechanism underlying the retribution effect remain. We do not have a data-set on the characteristics of hate crime perpetrators and are also not able to interview such felons ourselves. This means that beyond population-based surveys and subsamples based on voting patterns we do not have the means to learn more about the mechanisms connecting immigrant behaviour to hate crimes.

Moreover, more research on the policy implications of our findings is needed. 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. While it is perhaps uncontroversial that news outlets should refrain from descriptions such as "[...] a dark-skinned man with big lips, speaking broken German [...]"[10], this is not a conclusion necessarily to be drawn *only* from our research, as it is rather obvious advice that journalists should refrain from stereotypical, racist descriptions. To the extent that natives hold biased beliefs about the relative frequency of native vs. out-group perpetrators of crime, omitting (factual) identifying information from news articles might indeed perpetuate biased beliefs about the prevalence of migrant crime. In order to better understand how the media could report on crimes without calling for general censorship, one might use survey experimental methods with varying treatments on perpetrator descriptions. This would allow researchers to test how the public reacts to variation in media reporting on migrant crimes.

A final note is necessary on the conclusion to be drawn from our research. By no means do we want to suggest that hate crimes are legitimized by the "misbehavior" of out-groups. To the contrary, we believe it is worrisome that media reporting on the behaviour of individual out-group members can have such a severe effect by leading to retribution through hate crimes. While our main aim is to improve our understanding of where and when hate crimes occur, we also believe that the public

[10]"dunkelhäutig, wulstige Lippen, er sprach gebrochenes Deutsch", Source: https://www.hna.de/kassel/23-jaehrige-frau-in-kassel-vergewaltigt-und-mit-messer-verletzt-9459385.html

debate as well as journalists need to engage more critically on how to report on out-group behavior, particularly if this reporting has drastic consequences for intergroup relations.

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SI Supporting Information

Contents

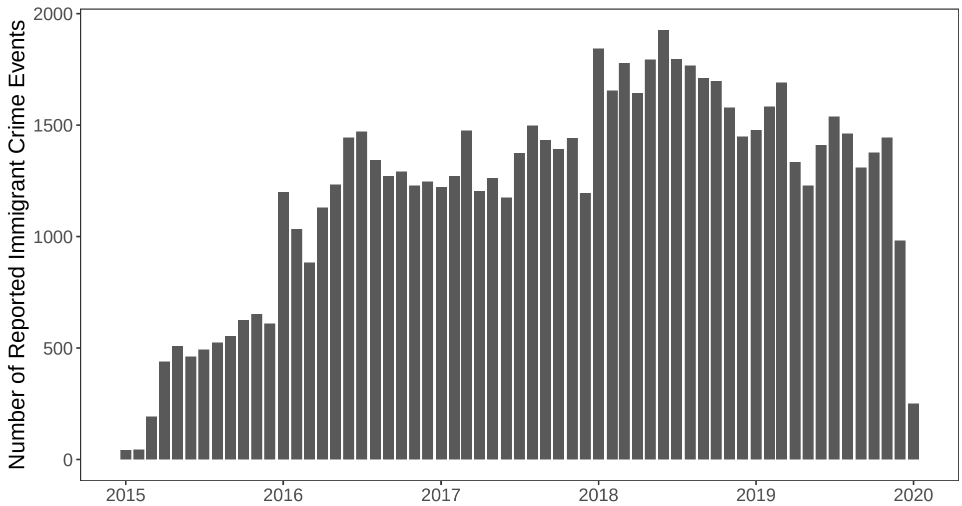
SI.1 Refcrime data

SI.1.1 Summary statistics

Table SI 1: Summary statistics, refcrime data

Variable	Mean	S.D.	N	Min	Max
Year	2017.41	1.24	73549	2015	2020
Hour of Day (0 - 24)	13.27	6.88	44582	0	24
Violent crime (0/1)	0.37	0.48	73549	0	1
Perpetrator: MENA origin (0/1)	0.29	0.46	32344	0	1
Perpetrator: African origin (0/1)	0.23	0.42	32344	0	1
Female victim (0/1)	0.47	0.50	40654	0	1
Perpetrator: refugee (0/1)	0.31	0.46	73549	0	1
Perpetrator: other migrant (0/1)	0.25	0.43	73549	0	1
Perpetrator: unknown status (0/1)	0.43	0.50	73549	0	1
East (0/1)	0.13	0.34	73549	0	1

Figure SI 1: Temporal Distribution of Migrant Crime Events



Note: The figure shows the monthly number of reported immigrant crime events between 2015 and 2019.

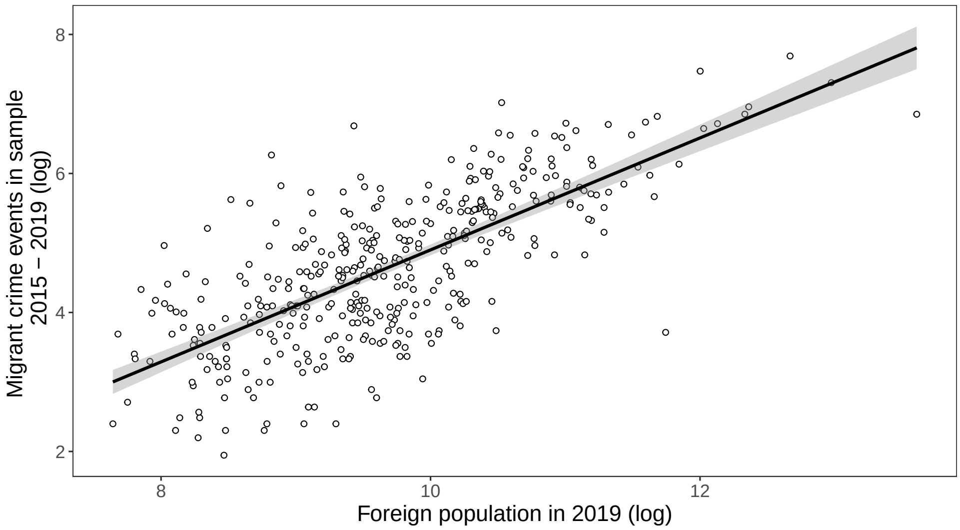
SI.1.2 Data quality

We implemented 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 first asked three research assistants to independently validate the information reported on the website. To do this, we leveraged the fact that the refcrime website lists an original source for each reported event (see SI.1.3). We find that in virtually all cases, the reporting by refcrime is in line with the listed sources.[11] Furthermore, in most cases, "migrant crimes" are based on press releases by the local police authorities. In few instances, they are based on reporting by local news outlets. The source material used by refcrime appears to be reliable and, thus, constitutes a credible source to obtain event-level information about crimes committed by out-group members.

It should be noted that we do not directly observe the citizenship (or second-generation migration background) for the perpetrators of many crime events listed on refcrime.info. In 40% out of the 200 random refcrime events we validated, the original source does not *explicitly* state that the perpetrators are foreign citizens or refugees. However, what almost all (95%) of the sources share are *heuristics* which suggest that the perpetrators are not part of the "German ingroup"; e.g. "spoke in broken German", "black Africans", "Southeast European appearance", "foreign speaking". Our validation, thus, clearly highlights that the web-hosts list any crimes which are associated with a cue that a perpetrator is potentially not German as a migrant crime. While some of these perpetrators could be first-generation migrants, they might also be German citizens whose ancestors migrated to Germany. Crucially for our analysis, the original sources contain *clear* outgroup heuristics in virtually all cases.

Figure SI 2: Immigrant crimes and foreign-born population



Note: The plot shows the bivariate relationship between the total foreign population and the total number of migrant crime events recorded in our web-scraped dataset. Both variables are log-transformed. Each observation represents one county. The Pearson correlation between the two measures is 0.72.

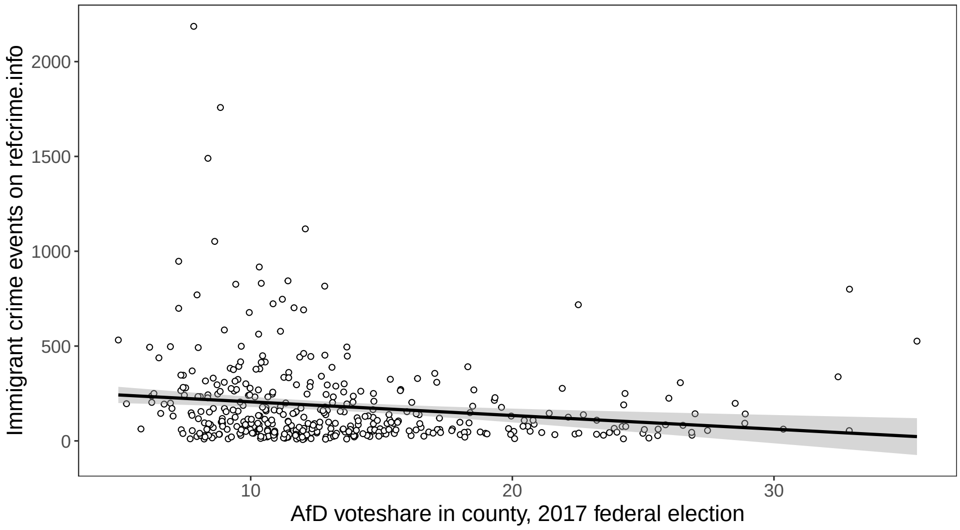
As an additional step, we also examine the face validity of the migrant crime data by examining the correlation

[11] We find only few instances in which the date (0.02%), location (0.03%), sex of the perpetrator (0.03%), or nationality of the perpetrator (0.01%) – if listed – is not in line with the source material.

between the number of events reported at the county level with the size of foreign population in the same county. We assume that there should be a correlation between the amount of migrants living in county and the number of crimes committed. As expected, the bivariate correlation between the number of foreigners in a county and the number of reported migrant crimes in our sample is strong at $r = 0.72$ (see figure SI 2).

We also verify that refcrime reporting is not simply determined by pre-existing anti-immigrant attituides at the local level. In particular, we examine the correlation between the number of recorded migrant crime events and support for the anti-immigrant party Alternative fuer Deutschland (AfD) in the 2017 federal election at the county level. We do not find evidence that this is the case. If anything, there is a weak negative correlation between AfD support and refcrime reporting (see figure SI 3).

Figure SI 3: Immigrant crimes and AfD support



Note: The plot shows the bivariate relationship between the vote share for the Alternative fuer Deutschland (AfD) in the 2017 federal election and the total number of migrant crime events recorded in our web-scraped dataset. Each observation represents one county. The Pearson correlation between the two measures is -0.16.

Importantly, we do not assume that the refcrime data comprehensively covers *all* crimes committed by immigrants across Germany. Instead, for our purposes the dataset needs to provide a good approximation to migrant crime events that received media coverage, most commonly in the local news media. In other words, the dataset covers incidents that natives could feasibly be aware of, and they were likely unaware of crime events that are not covered by our data source.

SI.1.3 Validation by hand-coding

We asked three of our research assistants to handcode information on the crime events data we used in our paper. The key reason for doing so is to validate the migrant crime data we scraped from `refcrime`. Luckily `refcrime` reports for each crime event the sources from which the information on the crime is retrieved from.

This allows us to *validate* whether or not the information reported by `refcrime` is true according to the sources used by refcrime. Given the political leaning (conservative/far right) by refcrime we have good reason to assume that refcrime overreports migrant crime. The webpage has been created with the goal to report the "true" amount of "migrant crime" in Germany. This already suggests that the information publicly available is false to some extent. Also the webpage allows everyone to report crimes. This might again result in overreporting of fake events by far right agitators.

Yet, fortunately for us in most instances (XXX) refcrime relies on highly trust-able sources such as police reports or national/local newspapers (e.g. Bild, XXX). For instance:

Bundespolizeidirektion Sankt Augustin

BPOL NRW: Abrupte Beendigung der Reiseabsicht Bundespolizei am Flughafen Köln/Bonn verhaftet jungen Mann

Köln, Flughafen Köln/Bonn (ots)

Heute (16.08.2018), am frühen Morgen gegen 04:00Uhr erschien ein 19-jähriger türkischer Staatsbürger zur grenzpolizeilichen Ausreisekontrolle am Flughafen Köln/Bonn. Der junge Mann wollte nach Kaisery in die Türkei fliegen. Die Überprüfung seiner Personalien führte schließlich zu einem Hinweis, der seine Reiseabsichten abrupt stoppte. Im Fahndungssystem war der 19-jährige durch das Amtsgericht Koblenz zur Verhaftung ausgeschrieben. Ihm wird ein Vergehen nach dem Betäubungsmittelgesetz vorgeworfen. Zur Hauptverhandlung Anfang Juni 2018 erschien der Angeklagte nicht vor Gericht. Als Folge daraus wurde der Haftbefehl erlassen und heute Morgen vollstreckt. Nachdem alle Maßnahmen vor Ort beendet waren, wurde der junge Mann in den Polizeigewahrsam beim PP Köln eingeliefert.

Rückfragen bitte an:

Bundespolizeiinspektion Flughafen Köln/Bonn
Pressestelle
Telefon: +49 (0)2203 9522 - 1500
E-Mail: presse.cgn@polizei.bund.de
Twitter: https://twitter.com/bpol_nrw
www.bundespolizei.de

Postfach 980 125
51129 Köln

Based on publicly available information by the police press portal (Presseportal Bundespolizei) refcrime reports that a 19 year old "Turk" was captured by the police at the airport in Cologne/Bonn on the 08/15/2018. The reason for his arrest is that he missed his court meeting in which he was accused of violations against the narcotics regulation. As the screenshot shows this information is fully in line with information shared by the Presseportal Bundespolizei.

We randomly draw 100 events from our data for each RA. We made sure that out of the 100 events, 50 events are the same for all three RAs. This allows us to test the reliability of the handcoding conducted by our three RAs.

Instructions for our coders

The goal of the handcoding is to *validate* the data we retrieved from refcrime. To do so we ask you to double-check a range of informations reported in the data and outlined below. By double-checking we mean that you 1) open the excel provided to you and 2) check if the information we report in the excel can be retrieved by the sources reported in the first column of the excel. As you will see for each variable the excel contains three columns. The first column is the information as we retrieved it from `refcrime` the second and third columns are for you to be filled.

The coding is simple, if the information reported in our data is in line with the information you find in the source material you code '1' into the second column (called $*\_true$), otherwise '0'. Whenever you code '0' in $*\_true$ – meaning that the information reported by `refcrime` is *not* in line with the information in the source material – you will need to report the information you found in the source material in the third column called $*\_source$. In case you code '1' in $*\_true$ – meaning that the information reported by `refcrime` is in line with the information in the source material – please leave $*\_source$ empty.

Please double-check the following information in the Excel provided to you:

1. date of the event
2. location of the event
3. gender of the victim
4. gender of the perpetrator(s)
5. origin of the perpetrator(s) *only* as MENA 0/1 and Africa 0/1
6. crime type
7. *migrant*$_{yn}$ Is there *clear* information provided suggesting that the perpetrator(s) are of "migrant origin"?
   - 1 = Yes, there is clear information that the perpetrator(s) are of "migrant origin".
   - 0 = unclear information.
   - -1 = No, there is no information even suggesting that the perpetrator(s) are of "migrant origin".

   E.g. what makes the crime a migrant one? Which information is provided about the perpetrator(s) citizenship/naturalization status?

SI.1.4 Crime categories

Table SI 2: Violent Migrant Crime Coding

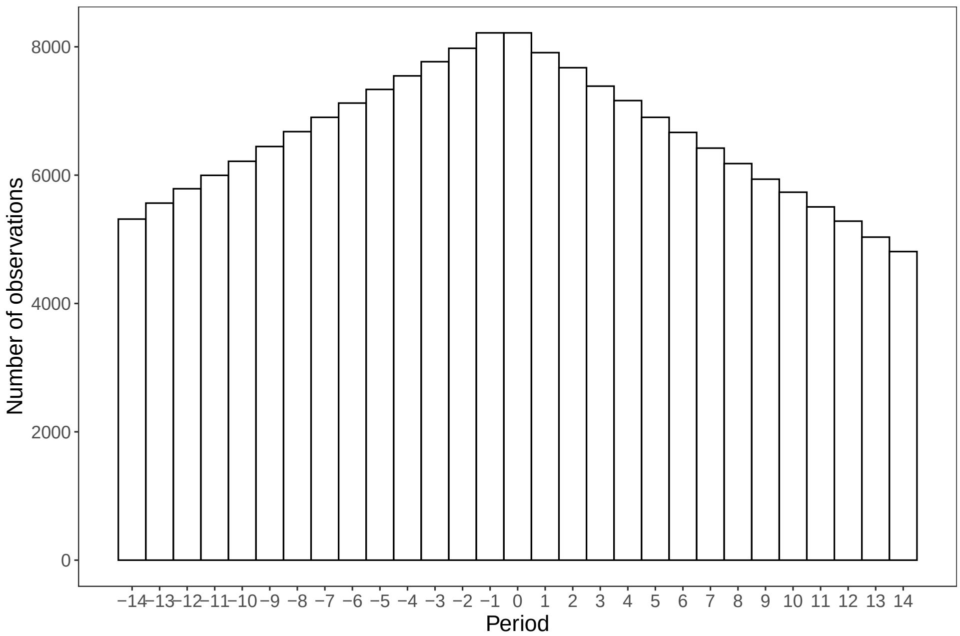
Crime Category	Classification
Burglary (attempted	Non-violent / Petty
completed)	Non-violent / Petty
Confidence trick	Non-violent / Petty
Disturbing the peace	Non-violent / Petty
Driving without a license	Non-violent / Petty
Drugs	Non-violent / Petty
drugs in traffic	Non-violent / Petty
Drunkenness	Non-violent / Petty
Exhibitionism	Non-violent / Petty
Falsification of documents	Non-violent / Petty
Fare evasion	Non-violent / Petty
Illegal entry	Non-violent / Petty
Illegal Possession of Firearms	Non-violent / Petty
Illegal stay	Non-violent / Petty
Incendiary (intentional)	Non-violent / Petty
Incendiary (negligent)	Non-violent / Petty
Insult	Non-violent / Petty
Miscellaneous	Non-violent / Petty
Other frauds	Non-violent / Petty
Other property crimes	Non-violent / Petty
Other sexual crimes	Non-violent / Petty
Other traffic offenses	Non-violent / Petty
Property damage	Non-violent / Petty
Receiving [stolen goods]	Non-violent / Petty
Riot	Non-violent / Petty
Robbery (attempted	Non-violent / Petty
Smuggling of migrants	Non-violent / Petty
Social welfare fraud	Non-violent / Petty
Theft (attempted	Non-violent / Petty
Threat	Non-violent / Petty
Verbal sexual harassment	Non-violent / Petty
Battery	Violent
Forced prostitution	Violent
Hit and Run	Violent
Honor killing	Violent
Killing (attempted)	Violent
Killing (completed)	Violent
Knife attack	Violent
Murder	Violent
Other violent crimes	Violent
Physical sexual harassment	Violent
Rape (attempted)	Violent
Rape (completed)	Violent
Sexual abuse in swimming pools	Violent
Stabing	Violent
Terrorism	Violent
Violence against executory officers	Violent
Violence in asylum shelters	Violent

SI.1.5 Overlapping events

We only retain a single immigrant crime event for each county on one single day. If multiple immigrant crime events occur on a single day in the same county, we retain only one single event.

- If another immigrant crime event occurs within the next 14 days after the treatment event i, we drop all observations of $Y_{i,t,c}$ after this next immigrant crime event (i.e. after the county is treated again).
- If another immigrant crime event occurred 28 to 14 days before the treatment event i, we only retain pre-periods that are at least 14 days apart from the most recent immigrant crime event. For instance: if an immigrant crime event occurred in the same county 20 days before the treatment event i, we only retain the pre-periods $P_{i,t,c} \in [-6, -1]$ and drop all other observations of $Y_{i,t,c}$.
- If another immigrant crime event occurred within 14 days before the treatment event, the county's treatment status is unclear. We drop such events.
- We only retain events for which we 1) observe both pre and post periods and 2) observe at least a total of 14 periods

Figure SI 4: Valid observations by period



Note: The histogram shows the number of observations by period after recoding the migrant crime events as described in section SI.1.5.

Our final sample is fairly representative of Germany as a whole: the mean population size of counties in our sample is 171,558 (206,465 across all counties); 17.7% of all recorded events occur in the East (while 19% of all German counties are in East Germany). The slightly lower mean population size in our final sample likely stems from the fact that immigrant crimes are more likely to cluster temporally in large cities (e.g. Berlin), which constitute a single county.

SI.2 Other descriptive statistics

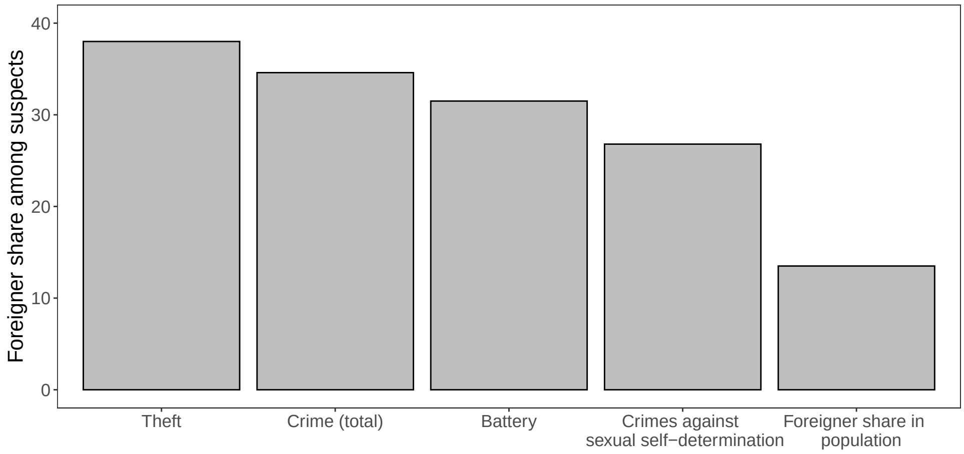
Table SI 3: Most frequent hate crime offenses

Offense	Total count	German translation
Hate speech, incitement of masses	2809	Volksverhetzung 130 StGB
Criminal damage	1573	Sachbeschaedigung 303 StGB, 304 StGB, 305 StGB
Battery	1387	Koerperverletzung 224 StGB, 223 StGB, 226 StGB
Use of banned (right-wing) symbols	1198	Kennzeichen verfassungswidriger Organisationen 86(a) StGB, Verunglimpfung von Verfassungsorg. 90b StGB, 86 StGB
Insult	1021	Beleidigung 185 StGB, Verleumdung 187 StGB, Ueble Nachrede 188 StGB, Ueble Nachrede 186 StGB, 189 StGB
Threat	348	Bedrohung 241 StGB
Arson	223	(Schwere, Fahrlaessige) Brandstiftung 306StGB, Herbeifuehren einer Sprengstoffexplosion 308 StGB
Public incitement to commit offences	197	Oeffentliche Aufforderung zu Straftaten 111 StGB
Coercion	88	Noetigung 240 StGB
Unlawful assembly	75	Verstoss gegen das Versammlungsgesetz

Table SI 4: Counties with highest number of hate crime incidents 2015 - 2019

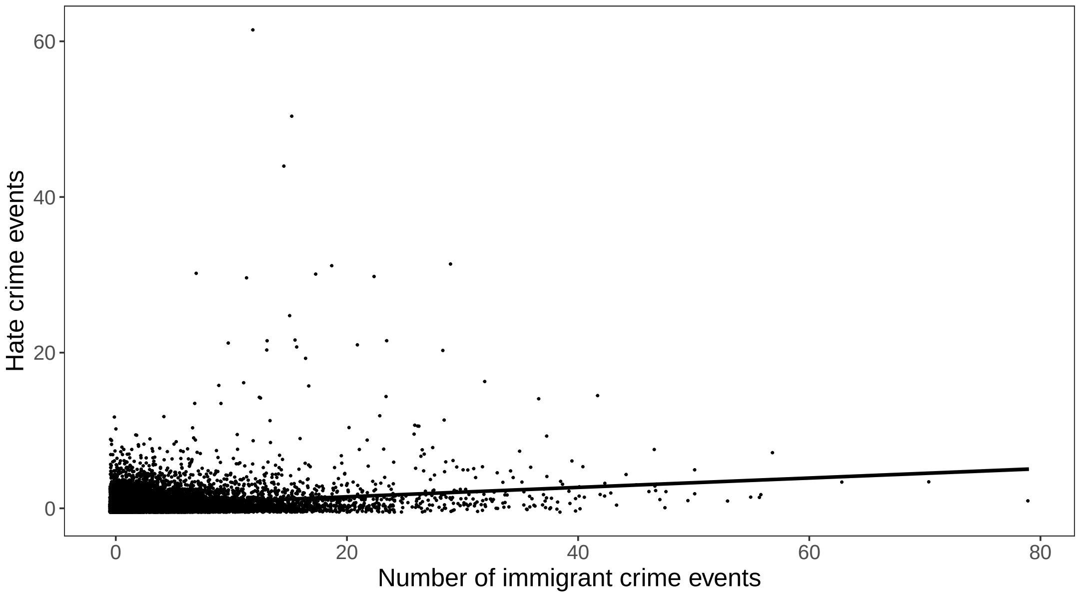
County	Hate crimes, 2015 - 2019	Region
Chemnitz, Stadt	184	East Germany
Cottbus, Stadt	126	East Germany
Saechsische Schweiz-Osterzgebirge	124	East Germany
Uckermark	111	East Germany
Ostprignitz-Ruppin	61	East Germany
Dithmarschen	58	West Germany
Spree-Neisse	58	East Germany
Prignitz	44	East Germany
Nordhausen	35	East Germany
Frankfurt (Oder), Stadt	27	East Germany

Figure SI 5: Foreign crime suspects in Germany, 2019



Note: The bar chart shows the proportion of crime suspects who are foreign citizens for different crime categories in 2019. The data source are the official crime statistics released by the German police.

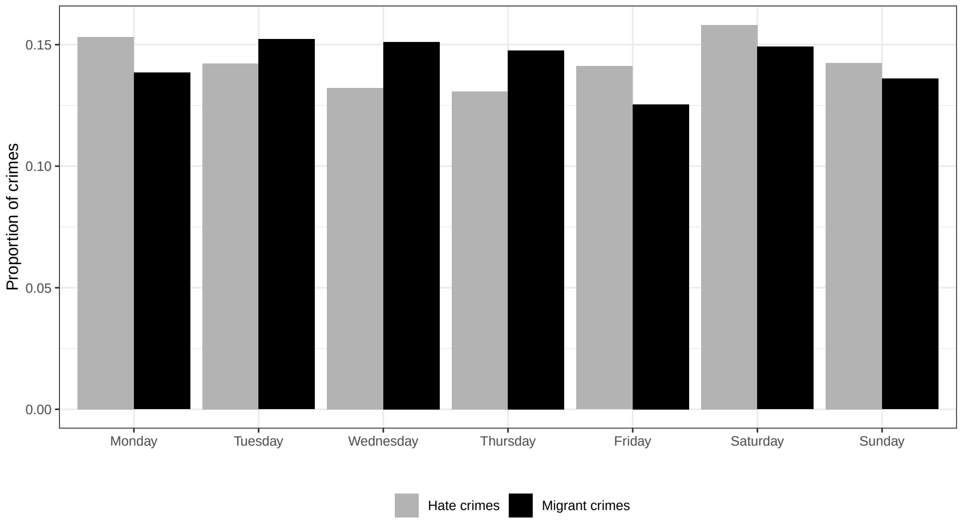
Figure SI 6: Migrant crime & hate crimes, scatterplot



Note: The figure shows the bivariate relationship between the number of recorded migrant crime events and hate crime events. Each point represents one month in one county. $\hat{\beta} = 0.05, \hat{\sigma} = 0.002$. The bivariate correlation is $r = 0.2$.

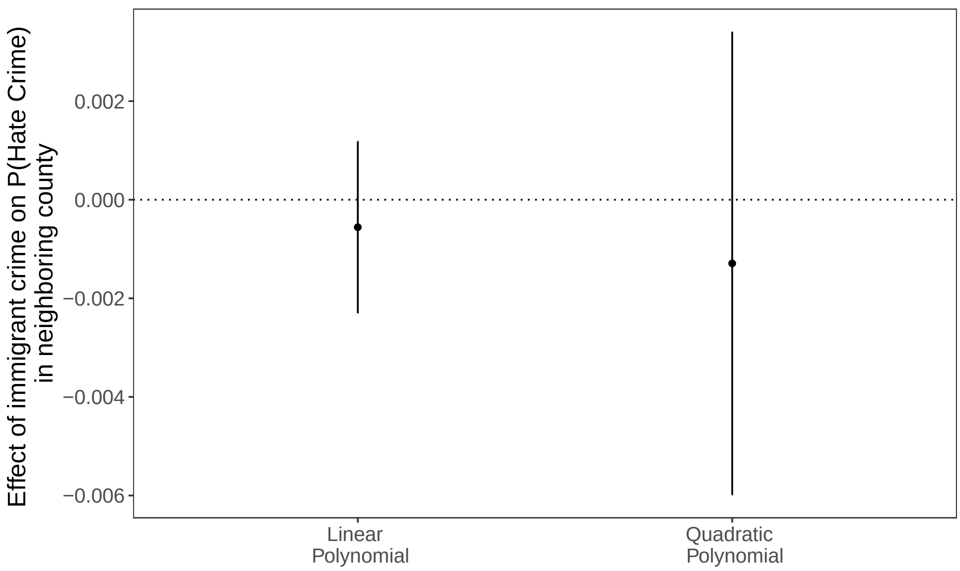
SI.3 Robustness checks

Figure SI 7: Migrant crimes and hate crimes by day of the week



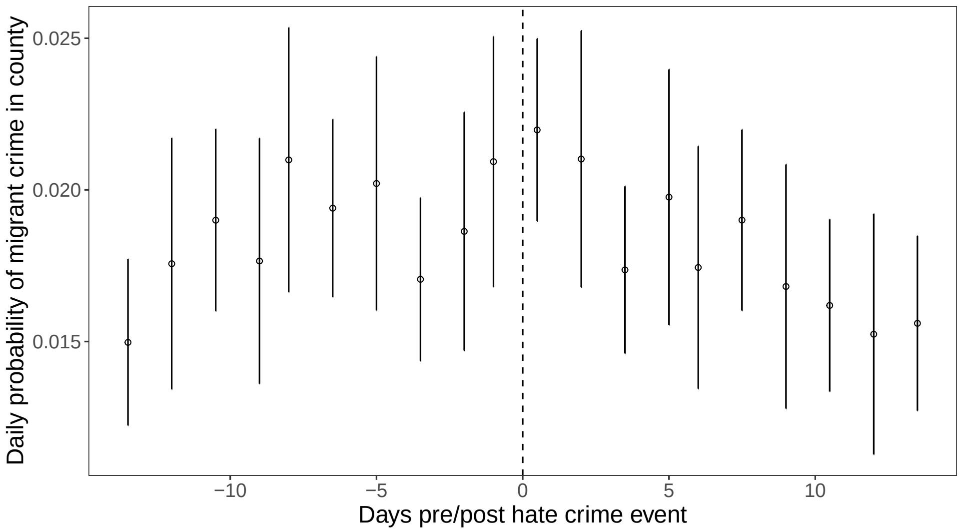
Note: The figure shows the proportion of all migrant crimes and hate crimes in our dataset by day of the week.

Figure SI 8: Test for Spillover Effects



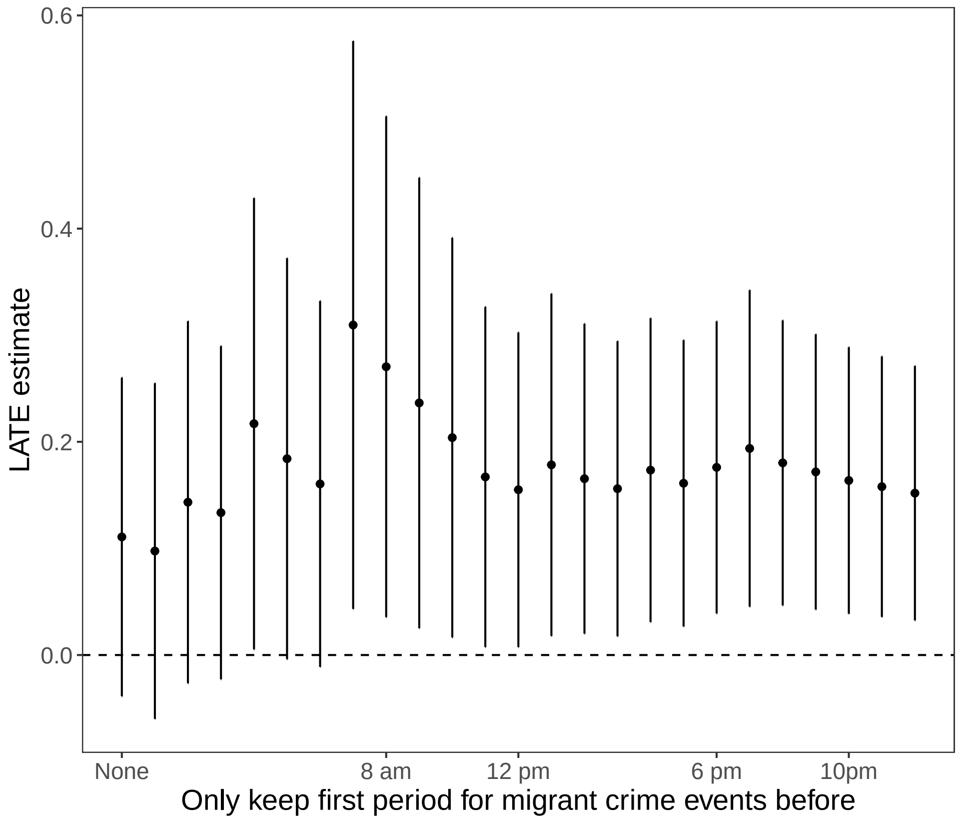
Note: Local average treatment effect on the treated estimates from RDiT models. We estimate the same models as for our main results with one single difference: for each period, $Y_{i,t,c} = 1$ if a hate crime event occurred in the closest neighboring county. We hence estimate the effect of immigrant crime events on hate crimes in adjacent counties.

Figure SI 9: RD plot: effect of hate crimes on migrant crime



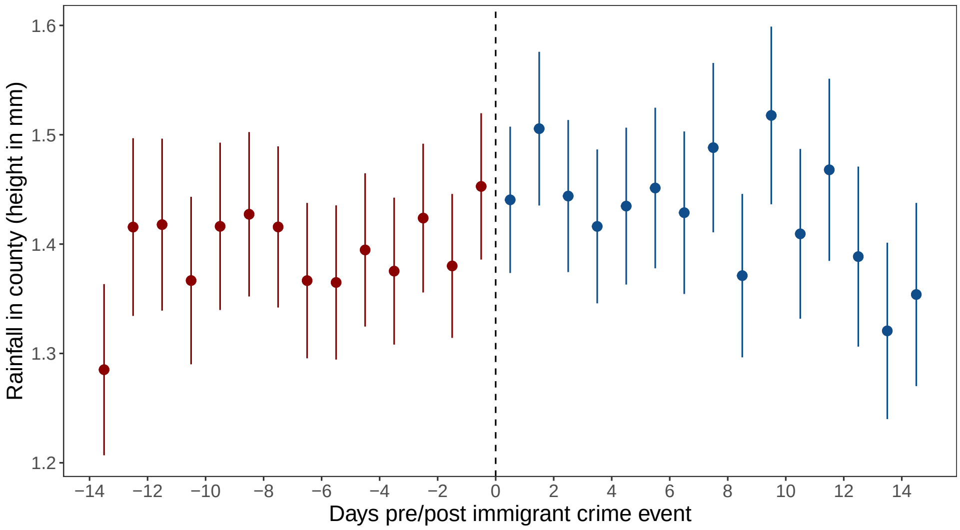
Note: RD plot where the outcome and treatment variable are reversed. We repeat exactly the same analysis as for our main results but treat hate crimes as the treatment event and the occurrence immigrant crimes as the outcome variable.

Figure SI 10: Excluding the day of migrant crime event



Note: The figure shows the LATE effect estimates from the regression discontinuity in time analsysis. We repeat the same analysis as for our main results but now exclude the day of the migrant crime event itself (period zero) for migrant crime events that occurred before a certain time of the day. We also exclude this period for those migrant crime events for which we do not observe the exact time at which they ocurred. Error-bars indicate 90% confidence intervals.

Figure SI 11: RD plot: rainfall



Note: RD plot where the outcome variable is the precipitation height in millimeters observed by county-day. We obtained data from all weather stations in Germany from the Climate Data Center of Germany's National Meteorological Service. Our raw data can be found under the variable code *RS-MN006*. We aggregated this data to the county level by geocoding all weather stations and calculating the median precipitation measured on all stations located within a given county by day. We exclude extreme outliers above the 97.5 percentile of the rainfall distribution.

SI.4 Heterogeneity by regional characteristics

The galvanizing effect of local events may not be the same everywhere. Previous research emphasizes that various contextual factors might drive hate crimes – such as local attitudes and norms (Dancygier and Green 2010), elite discourse in the media (Karapin 2002; Jäckle and König 2018; Dancygier et al. 2019), economic conditions (Olzak 1990; Green, Glaser, and Rich 1998; Green, Strolovitch, and Wong 1998; Krueger 2007), demographic changes such as migrant influxes (Hopkins 2010; Green, Strolovitch, and Wong 1998; Marbach and Ropers 2018), and social cohesion measured usually via political trust (Lyons 2007).

We collected data to test each one of these five mechanisms. More specifically, we collected information on the increase in the foreign-born population between 2011 and 2019, the share of foreigners among crime suspects, electoral support for the far-right party AfD in the 2017 federal elections, turnout in the 2017 federal election, and club density (Franzen and Botzen 2011) at the county level.

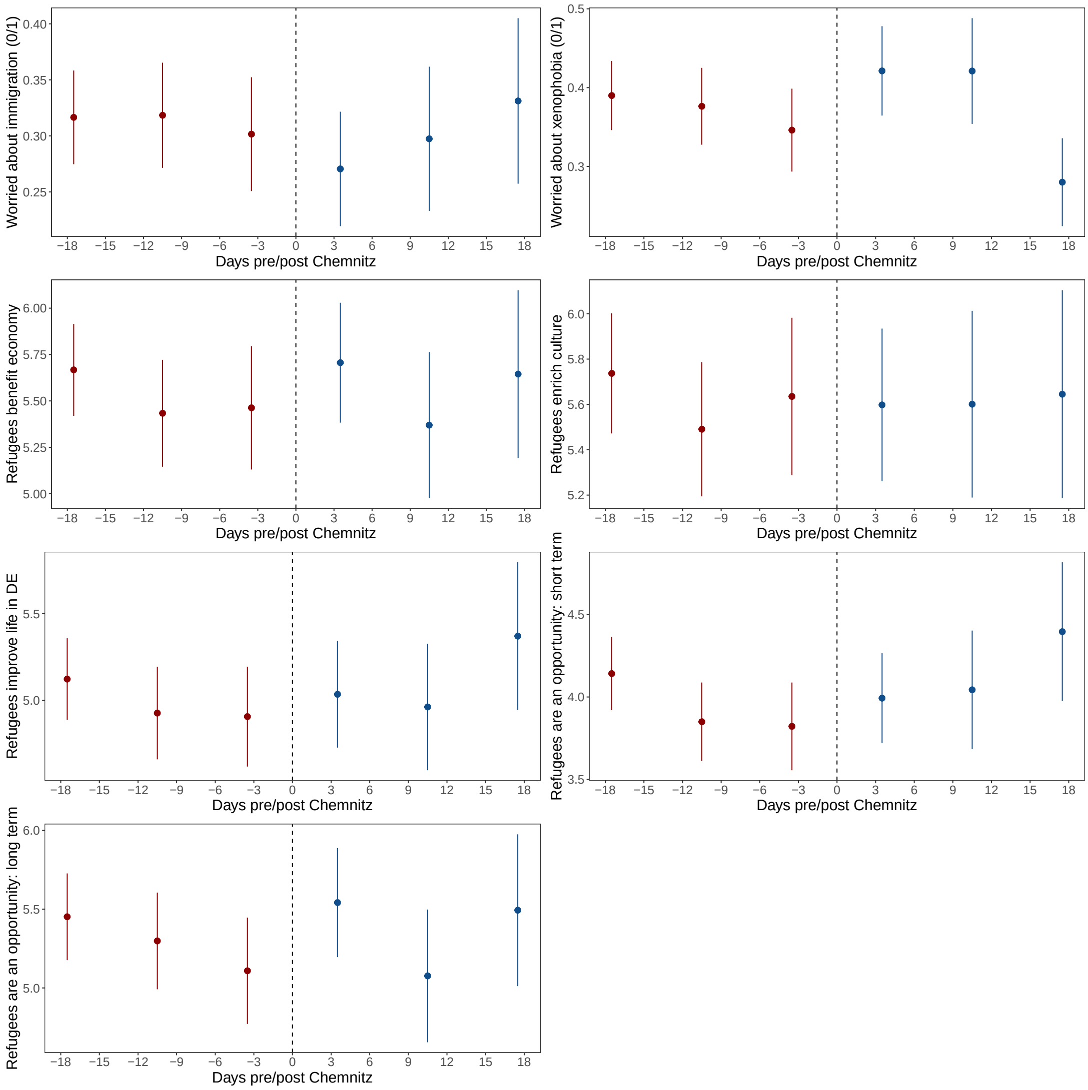
We dichotomize each of these variables into binary indicator variables that equal one if a county observation falls above the median of that variable. We then split the full sample on each variable and estimate the same model as described in section 4.2 for each subset. This way, we test for example whether we find stronger local average treatment effect estimates in the sub-sample of counties where electoral support for the far-right party AfD is strong. We present the results in Table SI 5.

Table SI 5: RDiT results for different subsets. The outcome variable is the probability of a hate crime in a county on a given day in percentage points. We use optimal bandwidth selection and bias-corrected robust standard errors for all estimated models. Subsets are based characteristics of the counties in which these events occur. We split the data based on the median value of the moderator variable. The number of observations that fall within the bandwidth, i.e. the sample size, is shown in the last column.

Sample	$\hat{\tau}$	$\hat{\sigma_\tau}$	**p-value**	$h_{\mathbf{MSE}}$	n
High increase in foreigner share 2011 - 2019 (county-level)	0.13	0.101	0.198	4	29971
Low increase in foreigner share 2011 - 2019 (county-level)	0.201 *	0.109	0.067	3	21234
High AfD voteshare 2017 (county-level)	0.161	0.104	0.122	4	30096
Low AfD voteshare 2017 (county-level)	0.142	0.089	0.110	3	22030
East Germany	0.308	0.212	0.147	5	12452
West Germany	0.125 **	0.063	0.046	4	47705

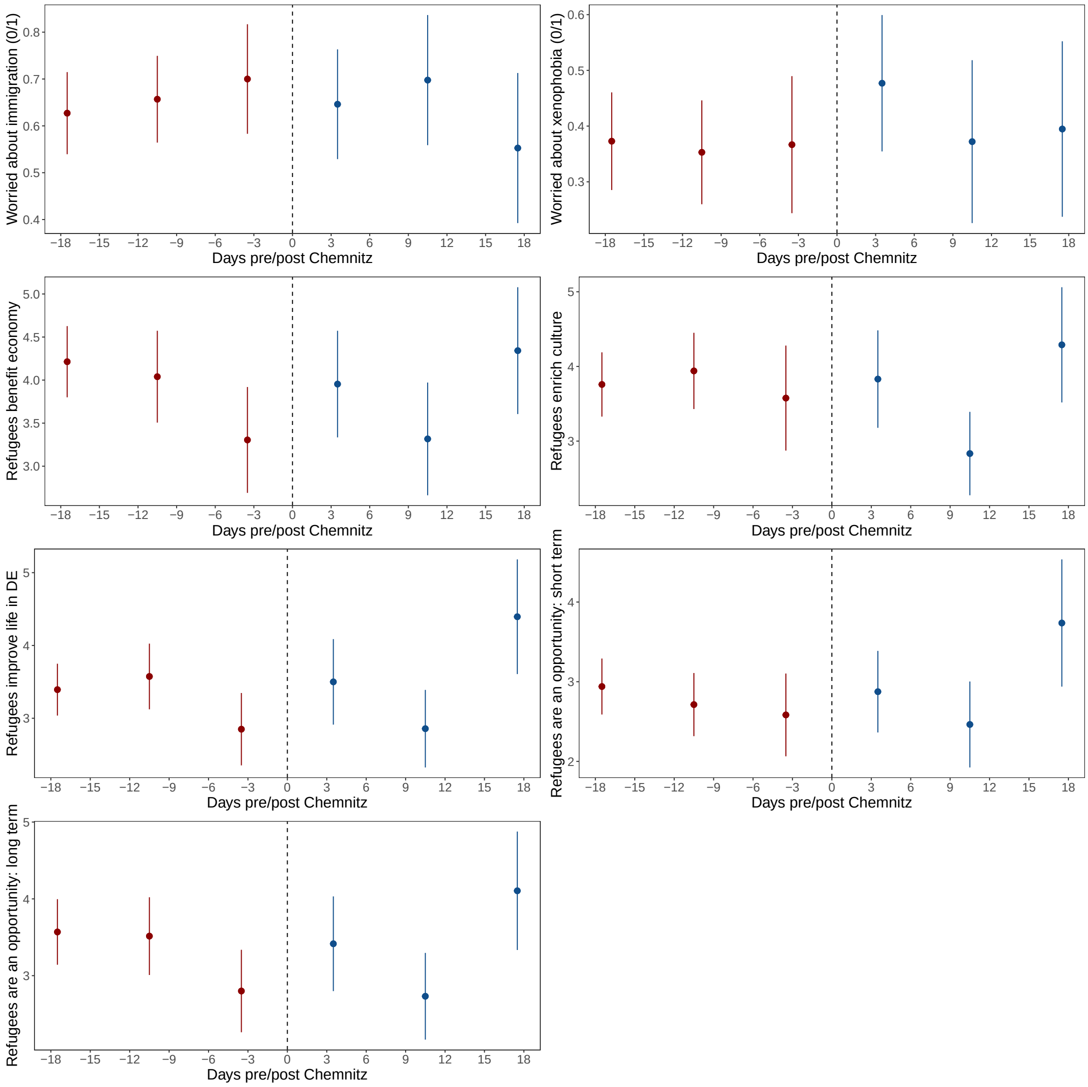
SI.5 Attitudes towards migration (SOEP/GLES)

Figure SI 12: Attitudes towards migration before and after Chemnitz (SOEP)



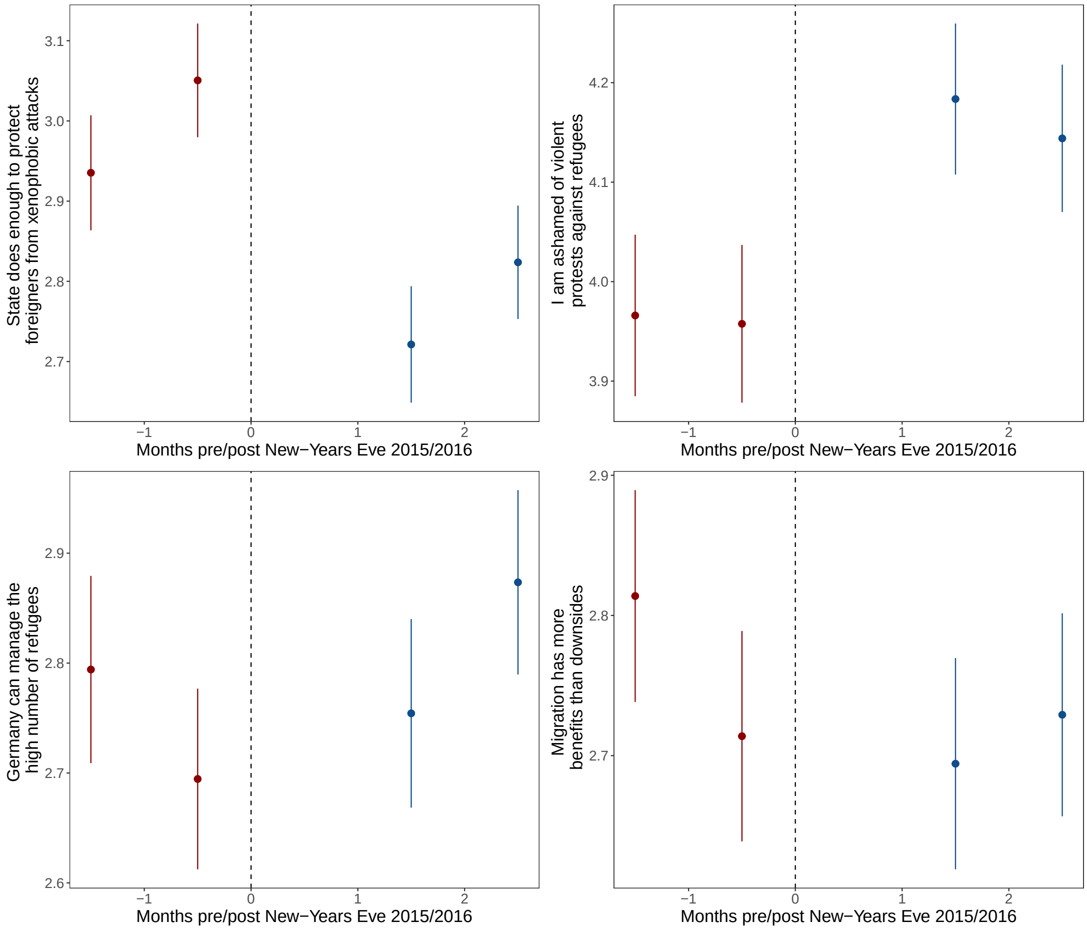
Note: The figure shows attitudes towards migration among a representative sample of Germans in the three-week period before and after the Chemnitz incident on August 25, 2018. We compare respondents interviewed right before and after the event ($n = 1216$). We aggregate the responses to seven different outcome variables measuring attitudes towards migration in weekly bins. We use data from the German Socio-Economic Panel (SOEP) 2018 survey wave.

Figure SI 13: Attitudes towards migration before and after Chemnitz: subset of anti-immigrant respondents (SOEP)



Note: The figure shows attitudes towards migration among a representative sample of Germans in the three-week period before and after the Chemnitz incident on August 25, 2018. The analysis is analogous to the results shown in figure SI 12 with one difference: here we show the results for the subset of respondents who in 2017, prior to the Chemnitz events, already expressed anti-immigrant views. Specifically, we subset to respondents who indicated that they are ‘somewhat’ or ‘very’ concerned about immigration.

Figure SI 14: Attitudes towards migration before and after New Year's eve 2015/2016 (GLES)



Note: The figure shows attitudes towards migration among a representative sample of Germans in the months before and after the sexual assaults on New-Year's eve 2015-2016 in Cologne. We use data from the 2015 and 2016 online survey waves of the German Longitudinal Election Study (GLES). This data is available under the study code *ZA6832* in the *GESIS* data archive.