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School of Business & Economics
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Local far-right demonstrations and nationwide public attitudes toward migration*

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Abstract

One of the primary objectives of protests and demonstrations is to bring social, political, or economic issues to the attention of politicians and the wider population. While protests can have a mobilizing and persuading effect, they may reduce support for their cause if they are perceived as a threat to public order. In this study, we look at how local or spontaneously organised xenophobic demonstrations affect concerns about hostility towards foreigners and worries about immigration among natives in Germany. We use a regression discontinuity design to compare the attitudes of individuals interviewed in the days immediately before a large far-right demonstration and individuals interviewed in the days immediately after that demonstration. Our results show that large right-wing demonstrations lead to a substantial increase in worries about hostility towards foreigners of 13.7% of a standard deviation. In contrast, worries about immigration are not affected by the demonstrations, indicating that the protesters are not successful in swaying public opinion in their favour. In the heterogeneity analyses, we uncover some polarisation in the population: While worries about hostility against foreigners increase and worries about immigration decrease in left-leaning regions, both types of worries increase in districts where centre-right parties are more successful. Lastly, we also show that people become more politically interested in response to protests, mainly benefiting left-wing parties, and are more likely to wish to donate money to help refugees.

JEL Code: D72, D74, D83, J15
Keywords: Protests, Right-wing Extremism, Xenophobia, Attitudes, Polarisation

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1 Introduction

Demonstrations and protests play a key role in the political arena, as they allow citizens to express their opinions and stress issues that are important to them. Through protests, participants are able to appeal to wider audiences and might be able to persuade or mobilise others for their cause (Madestam et al., 2013; Reny and Newman, 2021; Caprettini et al., 2021; Lagios et al., 2022). Yet, if turned disruptive or otherwise perceived as a threat to public order, protesters may reduce support for their cause (Wasow, 2020; Eady et al., 2023).

To understand the role protests play in shaping political attitudes and preferences, it is important to study not only the direction of their effect but also their geographical reach. Most of the literature in political science and economics looks at the effects of protests on political outcomes in the district where the protests have occurred (e.g., Madestam et al., 2013; Enos et al., 2019; Klein Teeselink and Melios, 2022; Wasow, 2020).[1] However, it is conceivable that local demonstrations affect the political preferences of voters at the national level. There is also little evidence on what are the underlying attitudes driving the changes in party preferences.

In this study, we focus on the effect of local or spontaneously organised large far-right xenophobic demonstrations[2] in an administrative district (*Nuts II*) on the attitudes towards migration of respondents being interviewed in the rest of Germany. More specifically, we look at concerns about hostility towards foreigners and worries about immigration in the native population in Germany between 2005 and 2020. Additionally, we also study the effect of local xenophobic demonstrations on interest in politics, party preferences, and pro-social behaviour towards migrants.

The effect of xenophobic demonstrations on our outcomes of interest is, a priori, ambiguous. On one hand, demonstrations can mobilise and persuade, raising support for the protesters' agenda. The issues and demands of the protesters might have strong resonance or mobilise cultural grievances linked to the presence or arrival of minority groups. They can also make certain issues more salient and push them to the

[1] Four exceptions are a study by Lagios et al. (2022), which considers spillover effects of demonstrations against the far right in France, a study by Eady et al. (2023) who show that the US Capitol insurrection led to de-identification with the Republican party nationwide, a study by Reny and Newman (2021) which finds that the George Floyd protests decreased favourability toward the police and increased perceived anti-Black discrimination and a study by Brox and Krieger (2021) which finds that the occurrence of large far-right rallies in Dresden reduced in-migration to the city from other German states.

[2] We interchangeably use other terms, such as "far-right", "xenophobic", "right-wing extremist", and "right-wing xenophobic" protests, to refer to "far-right xenophobic" demonstrations/protests.

public agenda. In this case, far-right demonstrations would strengthen xenophobic priors, and raise concerns about immigration.[3] Moreover, if the demonstrations resonate with the overall population, they may also influence political preferences, leading to a rise in support for (anti-immigrant) right-wing or far-right parties.

On the other hand, far-right protests may make xenophobia publicly visible or even threaten bystanders. The existence and salience of xenophobic groups may be increased, and the protesters' message can be perceived as a threat by others, including natives. In this situation, xenophobic protests could move public support against the protesters' agenda and possibly in support of parties with opposing policy platforms. In this case, we would expect far-right protests to increase worries about hostility towards foreigners.[4]

We rely on a dataset constructed by Kanol and Knoesel (2021), encompassing right-wing extremist demonstrations in Germany, to identify large right-wing xenophobic demonstrations. This dataset includes information on each protest's date, place, and number of participants. To measure public attitudes and opinions, we employ data from the German Socio-Economic Panel (SOEP), a longitudinal annual household panel. Our two primary questions of interest are those asking respondents to rate how worried they are about hostility towards foreigners and immigration. To understand how these changes in attitudes translate into changes in pro-social behaviour towards migrants and political preferences, we look at the effect of demonstrations on the intention to donate money or participate in initiatives to help refugees, interest in politics, and party preferences.

Using the Kanol and Knoesel (2021) dataset on right-wing demonstrations, we define our demonstrations of interest as those satisfying the following three criteria: 1) organised spontaneously and/or are of local nature, 2) larger than usual, and 3) isolated, i.e., there were no other large demonstrations taking place in the days before or afterwards. We concentrate on spontaneous or locally organised events because it is unlikely that *ex-ante* the organisation and planning of these right-wing xenophobic demonstrations in a specific district in Germany would have attracted or reached individuals residing in other districts of the country.[5] We focus on large

[3] The effects on concerns about xenophobia would be less clear, either decreasing or remaining flat.
[4] Concerns about immigration would remain unchanged or even decline.
[5] Alternatively, we exclude the entire state.

demonstrations so that *ex-post* people outside the demonstration's local district would likely be aware of them after their occurrence. In principle, we want to consider demonstrations with significantly more participants than the typical figures observed in xenophobic demonstrations such that these events stand out. In our preferred measure, we consider a demonstration large and salient if the number of participants is above the 99th percentile (1500 participants).[6] To ensure that the respondents were not recently exposed to other protests, we classify a demonstration as isolated if the individuals surveyed 30 days before and after the focal demonstration did not experience any other demonstration.[7]

Our empirical approach uses a regression discontinuity design (RDD) to compare the attitudes of individuals interviewed in the days immediately before a large right-wing xenophobic demonstration with those interviewed in the days immediately after that demonstration. To make the case of no anticipation stronger and to separate the spillover effect from the possible direct disruptive effect of large protests, we do not consider individuals residing in the district where the large protest took place.

Overall, we find that large xenophobic demonstrations significantly increase worries about hostility towards foreigners among native Germans. Our results show that within a 30-day bandwidth, right-wing demonstrations with more than 1500 participants lead to a substantial increase in worries about hostility towards foreigners of 13.7% of a standard deviation. Looking at our second outcome, we find that respondents' concerns about immigration remain unchanged. Since media reporting likely affects how respondents learn about protests, we examine how far-right demonstrations that received low versus high newspaper coverage affect our outcomes of interest. We find that the positive effect of xenophobic demonstrations on worries about hostility towards foreigners is mostly driven by the demonstrations that received high newspaper coverage. For worries about immigration, we see no significant difference.

In the heterogeneity analyses, we uncover some potential polarisation in the population: While worries about hostility against foreigners increase and worries

[6] As alternatives, we consider demonstrations where the number of participants is slightly below, at 1200, or above, at 2000.

[7] In the first step, we classify a demonstration as isolated (regardless of its nature) if the individuals surveyed 30 days before and after the focal demonstration did not experience any other demonstration during that period. In the second step, we identify the relevant and isolated events by excluding isolated demonstrations associated with annual events that are of national prominence. This procedure is further detailed in Section 3.1.

about immigration decrease in left-leaning regions, both types of worries increase in districts where right-of-centre parties are more successful. Moreover, at the individual level, we show that only respondents who place themselves left-of-centre on the political spectrum show significantly increased worries about hostility towards foreigners. When looking at how changes in attitudes translate into changes in economic and political behaviour, we find that following far-right demonstrations, individuals become more politically interested and engaged, mainly benefiting left-wing parties. Large xenophobic demonstrations also increase individual's intentions to donate money or goods to help refugees and to participate in initiatives to help refugees.

For the regression discontinuity design to be valid, we need to ensure that there is no selection on observables, no selective behaviour around the cutoff and no anticipation. To show that there is no evidence of selection on observables, we compare the characteristics of districts and individuals interviewed before the demonstrations (control group) with those interviewed after (treatment group). We also argue that selective behaviour around the cutoff is unlikely by showing that the empirical distribution of the number of observations is continuous at the cutoff. Additionally, we perform a qualitative media analysis, which suggests that newspaper reporting in the days leading up to demonstrations was fairly limited and usually only conducted by local or regional newspapers. To further strengthen the case of no anticipation, we assign a placebo treatment one week and two weeks before the true treatment day, and we find no effect on our outcomes of interest.

To assess the stability of our main results, we run a series of robustness checks. First, we show that our results hold when we use a binary instead of a continuous dependent variable. Second, we demonstrate that our results remain robust when adding time, geographical and individual controls and when choosing different specifications (e.g., bandwidth, weight, order of the polynomial). We also show that our conclusions hold when varying the cutoff for large demonstrations, excluding the entire state where the demonstration occurred (rather than the district) and excluding each demonstration at a time. To ensure that we are not capturing some randomness in the data, we randomly assign dates to each demonstration and show that they have no discernible effect on attitudes. We further examine the

impact of these demonstrations on other concerns reported in the SOEP that, in principle, should remain unaffected and find no effect. Lastly, we present our findings when employing a local randomisation RDD.[8] Overall, our main conclusions hold.

Our study contributes to several different strands of the literature. First, we add to existing research that analyses the effects of protests on attitudes and political preferences,[9] as we study the effects of far-right demonstrations on concerns about hostility towards foreigners, worries about immigration, interest in politics, party preference and intention to help refugees. Previous studies have examined the political effects of the 1932 Nazi marches (Caprettini et al., 2021), demonstrations against Le Pen (Lagios et al., 2022), US civil rights protests (Wasow, 2020), the Women's March (Larreboure and Gonzalez, 2021), the George Floyd protests (Reny and Newman, 2021), Black Lives Matter (Klein Teeselink and Melios, 2022) or the January 6th, 2021 capitol riots (Eady et al., 2023), among others. While some of these studies explore local variation in protest intensity to identify their effect on (aggregate) regional political outcomes, we can measure attitudes at the individual level and pin down how these change with respect to right-wing demonstrations. This allows us to study individual heterogeneity and understand the channels through which demonstrations affect individual attitudes. We focus particularly on worries about hostility towards foreigners and immigration since these are important determinants of political preferences and voting behaviour. Furthermore, by exploiting differences in the interview date within the same year in adjacent months, we avoid imposing strong assumptions on year-to-year variations in attitudes and decrease concerns regarding confounding factors.

A second significant contribution is that we show how local demonstrations (e.g., at the district level) can impact attitudes at the national level. This contrasts with most of the literature, which assumes that the effect of protests is mostly prevalent in the location where they took place and looks only at political outcomes (Madestam et al., 2013; Enos et al., 2019; Klein Teeselink and Melios, 2022; Wasow, 2020; Larreboure and Gonzalez, 2021). In this aspect, our work is closer to that of Eady et al. (2023), who show that the US Capitol insurrection led to de-identification

[8]The local randomisation RDD assumes that for a small window around the cutoff, the treatment status is assigned as it would have been in a randomised experiment

[9]Studies include Madestam et al. (2013); Enos et al. (2019); Wasow (2020); Eady et al. (2023); Larreboure and Gonzalez (2021); Reny and Newman (2021); Lagios et al. (2022).

with the Republican party nationwide, Reny and Newman (2021) who find that the George Floyd protests decreased favorability toward the police and increased perceived anti-Black discrimination, and Brox and Krieger (2021) who find that the occurrence of large far-right rallies in Dresden reduced in-migration to the city from other German states. In line with these studies, we argue that large protests may also impact attitudes on a national level as people learn about these protests from the news and other media.[10]

Our third contribution is that we focus on local or spontaneously organised right-wing xenophobic demonstrations. Many existing studies have primarily focused on the effect of left-wing protests (regarding issues like civil rights or women's rights) on public attitudes and voting behaviour (Mazumder, 2018; Enos et al., 2019; Wasow, 2020; Larreboure and Gonzalez, 2021; Reny and Newman, 2021; Klein Teeselink and Melios, 2022).[11] However, the effect of right-wing protests is not necessarily symmetric (Barker et al., 2021) since right-led protest differs from traditional left-led protests with regards to the underlying motive, ethnic and social composition of protesters (Eady et al., 2023; Manekin and Mitts, 2022). Most studies looking at right-wing demonstrations have focused on coordinated protests or party-sponsored demonstrations, which were organised to create a spectacle (Madestam et al., 2013; Caprettini et al., 2021). In contrast, we focus on local or spontaneously organised demonstrations, similar to the more left-wing demonstrations studied in the literature. Hence, our study broadens our understanding of the consequences of the different types of demonstrations.

Lastly, by looking at far-right protests, we contribute to the literature on the effects of xenophobia. Existing studies have focused on the effect of hate crimes or xenophobic policies on integration, return intentions, and mental health of immigrants (Friebel et al., 2013; Gould and Klor, 2015; Elsayed and de Grip, 2017; Steinhardt, 2018; Deole, 2019; Fouka, 2019; Graeber and Schikora, 2021; Abdelgadir and Fouka, 2020). Similar to this literature, the far-right demonstrations used in this study can be perceived as a xenophobic threat. Yet, while most studies examine the impact of xenophobic threats on migrants, we look at the effect on natives. Even though

[10] To look more deeply into that, we use information from the platform *genios.de* to show that most demonstrations were covered extensively in newspapers (Table B1).

[11] Some studies looking at the effect of right-wing protests and demonstrations include Madestam et al. (2013); Eady et al. (2023); Caprettini et al. (2021).

natives do not necessarily feel targeted by these protests, they may still be strongly opposed to xenophobia, instead preferring to live in an open and diverse society.

This paper is organised as follows: in Section 2, we lay out some theoretical considerations on the effect of right-wing xenophobic protests on individual's attitudes, and in Section 3 we present the data and explain our procedure to select the demonstrations used in the empirical analysis. Section 4 explains our empirical strategy and shows some preliminary tests. We show all our main results, robustness checks, and heterogeneous analysis in Section 5. In Section 6, we extend our main results and show the effect of far-right demonstrations on interest in politics, party preference and intention to help refugees. Finally, Section 7 concludes.

2 Theoretical Considerations

The effect of right-wing xenophobic protests on public attitudes and political preferences toward migration is, *a priori*, ambiguous. This section considers two main channels: the "persuasion mechanism" and the "threat mechanism". Furthermore, because the effect of protests on attitudes can be heterogeneous across certain groups, we also discuss the role of media portrayal and polarisation.

Persuasion mechanism. Demonstrations and protests can help spread the protesters' message to a broader audience and increase public support (Madestam et al., 2013; Wasow, 2020; Larreboure and Gonzalez, 2021), as they can serve as platforms for participants to express their grievances, rally support, and engage in symbolic actions that may resonate with bystanders, among others.

Protesters could sway the public in their favour through several channels. First, they can have a persuasive effect (Wouters, 2019; Klein Teeselink and Melios, 2022). As the protests unfold, the visibility of the protesters' message may attract the attention of people close to the protest but may also extend to a broader audience that learns about the events through social networks or media coverage, affecting individuals' attitudes on a local and national scale (DellaVigna and Kaplan, 2007; Adena et al., 2015; Guriev et al., 2021; Melnikov, 2021). Second, protests may also help mobilise individuals who were previously politically inactive or disengaged (Madestam et al., 2013; Engist and Schafmeister, 2022). They provide a visible

and tangible outlet for individuals who share similar ideological views but have not been actively involved in political activities. These individuals may feel inspired and motivated to actively support the protesters and their cause. Third, salient protests covered in the media may also influence which topics are being discussed and change how they are framed in the public discourse (Dunivin et al., 2022). Fourth, protests could be crucial in facilitating coordination among the protesters themselves and setting the stage for forming local organisations and future mobilisation efforts (Madestam et al., 2013). This may help to sustain the momentum of the movement and increase the likelihood of future protests and demonstrations.

If protesters successfully spread their message and can persuade other people for their cause, we expect to see an increase in worries about immigration and no change or a decrease in worries about hostility towards foreigners among individuals interviewed after far-right demonstrations. Moreover, we might also observe an increase in the alignment of respondents with right-of-centre and far-right parties, whose policies are more restrictive with regard to immigration.

Threat mechanism. Political protests can backfire if they are perceived as threatening by the public (Wasow, 2020; Gutting, 2020; Eady et al., 2023; Brox and Krieger, 2021). The public's response to such protests is multifaceted, influenced by individual characteristics, societal context, and the specific actions and rhetoric employed during the protests. These protests often espouse exclusionary ideologies and target marginalised groups, creating an environment of hostility and fear. The perception of threat also arises from the potential consequences of the ideologies that protests propagate. They may foster intergroup tensions, increase social divisions, and erode social cohesion. The public's perception of these protests as threatening can lead to counter-mobilisation efforts, resistance against far-right ideologies, and strengthening support for alternative perspectives that promote inclusivity and social justice.

If protesters are unsuccessful in swaying public opinion in their favour, and xenophobic demonstrations are perceived as threatening, we expect to see an increase in worries about hostility towards foreigners and no change or a decrease in worries about immigration. In extension, there might be an increase in preferences for left-wing parties, who espouse more immigrant-friendly positions.

Media attention and polarisation. To what extent protesters are successful or unsuccessful in spreading their message depends in large part on two factors: i) audiences knowledge and perception of the demonstrations, which depends on media coverage and on how organised and coordinated protests are, and ii) how receptive potential audiences are to their message, which depends on individual ideology and economic situation, among other factors.

For a demonstration to successfully spread its message, it should have a wide public reach. Previous research has shown that events which receive high media coverage often have a stronger influence on public attitudes and political behaviour than those with lower media coverage (Oberholzer-Gee and Waldfogel, 2009; Gentzkow et al., 2011; Durante and Zhuravskaya, 2016; Mastrorocco and Minale, 2018; Benesch et al., 2019). Therefore, we would expect that demonstrations with higher media coverage will have a larger effect on our outcomes of interest.[12]

A number of studies have shown that pre-existing viewpoints and ideology are important mediators in how audiences perceive protesters, with conservatives more opposed to liberal protesters and vice versa (Gutting, 2020; Barker et al., 2021). Therefore, we would expect that more conservative individuals and those with higher initial levels of anti-immigrant attitudes might be more open to the messaging of far-right protesters, while the opposite might be true for more liberal individuals. If xenophobic demonstrations have such a polarizing effect we expect to see that following a far-right demonstration, worries about hostility towards foreigners increase more and worries about immigration change less for left-leaning respondents than centre-right and far-right respondents.

Similarly, by fostering a sense of relative deprivation among natives, economic inequality might impact national identification, anti-immigrant attitudes, and populist voting (Stoetzer et al., 2021; Riaz, 2023). Hence, people residing in economically deprived areas or who are facing harsher economic conditions might be more positively receptive to the position and rhetoric of far-right protesters. If this is the case, we expect to see that worries about hostility towards foreigners decrease (increase) and worries about immigration increase (do not change) for economically deprived

[12] In this analysis, we only look at the extent of newspaper coverage, but do not analyse how the media portrays the demonstrations. Nevertheless, the framing of reporting can also influence how protests are perceived by the public (DellaVigna and Kaplan, 2007; Adena et al., 2015; Guriev et al., 2021; Melnikov, 2021).

(non-economically deprived) respondents.

3 Data

3.1 Demonstrations Data and Selection

To study the effect of xenophobic demonstrations on attitudes, we rely on a dataset of right-wing extremist demonstrations that took place in Germany between 2005 and 2020. The dataset was constructed by Kanol and Knoesel (2021) using the German federal government's answers to "brief parliamentary questions" (*Kleine Anfragen*) by the left-wing party *Die Linke*. The dataset includes information on the location, date, number of participants, and the motto of the protests. The overall distribution of right-wing extremist demonstrations has a mean of 161 participants and a minimum and a maximum number of participants of 4 and 6500, respectively (Table 1, Panel B). The location of each demonstration is mapped into one of the 38 German government districts or *Regierungsbezirke*, which correspond to the Nuts II in European Unions' Nomenclature of Territorial Units for Statistics.

The Kanol and Knoesel (2021) dataset includes demonstrations that take place at dates that are prominent in the minds of many Germans (in the following discussion we refer to them as days of national knowledge), such as Labour Day or the bombing of Dresden, but also lists demonstrations that were spontaneously or locally organised, such as protests against asylum seeker centres or demonstrations following a local far-right rock festival. In this study, we are interested in right-wing xenophobic demonstrations that meet the following three criteria: 1) were organised spontaneously and/or are of local nature, 2) were larger than usual, and 3) were isolated.

We focus on demonstrations that were organised spontaneously and/or are of local nature such that it is likely that the organisation and planning of these right-wing xenophobic demonstrations in one given German district are unlikely to have drawn or reached people living in other German districts. Demonstrations related to annual events that are of national knowledge include protests on Labour Day, German unity day, landmark war days and demonstrations related to the anniversary of the bombings of Magdeburg, Dresden, and Chemnitz during WWII, which Neonazi

Table 1: Descriptive Statistics: Number of Participants

Panel A:	Percentiles Participants	Number
	1%	12
	5%	20
	10%	25
	25%	40
	50%	75
	75%	150
	90%	300
	95%	520
	99%	1500

Panel B:	Other statistics
Total numb. demonstrations	3,120
Mean numb. participants	161.1285
Std. Dev. numb. participants	347.7738
Min numb. participants	4
Max numb. participants	6500

Source: Kanol and Knoesel (2021), all protests and demonstrations between 2005-2020.

groups frequently instrumentalise. We exclude these events because one could argue that there might be anticipation effects at the national level. Moreover, protests on these days were usually accompanied by other major events. For example, in the case of the anniversary of the bombing of Dresden, there are usually large memorial events organised by a broad spectrum of civil society and politicians, as well as TV broadcasts that provide further information on the historical event. These simultaneous events likely also affect respondents' attitudes, biasing our estimates.

For the purpose of our analysis, we focus on relatively large protests so that *ex-post* people were likely to have read or heard about them after they took place - but not to have participated in them. To proxy for the scale and salience of the event, we use the estimated number of participants and consider different cutoffs. In principle, we want to consider events with a number of participants far above the typical number of participants in xenophobic demonstrations such that the event stands out. The distribution of the number of participants across all demonstrations in the Kanol and Knoesel (2021) dataset is shown in Table 1. In our preferred measure, we consider a demonstration large and salient if the number of participants is above the 99th percentile (1500). As alternatives, we consider demonstrations where the number of participants is slightly below, at 1200, or slightly above, at 2000.

To ensure that respondents in our analysis were not recently exposed to protests or treated more than once, we use only isolated large xenophobic demonstrations with a local or spontaneous character within a 30-day range.[13] First, we classify a large demonstration (irrespective of its nature) to be isolated if individuals interviewed

[13]This is similar to the design used in Graeber and Schikora (2021).

in the 30 days before and after the focal demonstration have not experienced any other large far-right demonstration during this time period. Second, we drop protests that are related to annual events that are of national knowledge, such as protests memorialising the bombing of Dresden.

Table B in Appendix B lists and summarises all protests that fulfil our criteria and which were included in our empirical analysis. In the first three columns, the table shows the date, location, and number of participants for each event. The smallest protests was in Jänkendorf in 2010, which had 2000 participants and the largest demonstrations were in Dresden in 2006 and in Chemnitz in 2018, with 6000 protesters.

To further ensure that the protests in our sample were not anticipated, we examined to what extent they were covered in national, regional, and local newspapers in the days leading up to them. Using the platform *genios.de*, which assembles and provides articles of several hundred national, regional, and local German newspapers, we construct a dataset that summarises which newspapers reported on our selected protests in the days before and after they took place. The dataset is also presented in Table B1 and described in more detail in Appendix B. Generally, most protests received only limited attention from newspapers in advance. For two protests, we found no mentions in the days leading up to the protests, while for most other protests, reporting was done by local or regional newspapers that serve readers in the same district or state where the protests took place. Even though a handful of protests did receive at least some coverage in newspapers from outside the state, in most cases, reporting was limited to only one or two articles and newspapers reporting were usually regional and from a neighboring state. The only protest that received meaningful national newspaper coverage leading up to the event was the first protest in our dataset (Berlin 2005). However, we show in Section 5.2 that excluding this event from our sample does not meaningfully alter the main results.[14]

For readability matters, we will refer to protests satisfying criteria 1) 2) and 3) simply as large right-wing demonstrations or xenophobic demonstrations.

[14]Using our dataset, we also show in Table B1 in Appendix B that, after they occurred, most demonstrations used in our analysis were covered extensively in national and regional newspapers.

3.2 Individual and Household Data

The Socio-Economic Panel (SOEP, Goebel et al. 2019) is a longitudinal annual household survey that is representative of the German population, for which every year approximately 30,000 people in around 15,000 households are interviewed. The dataset contains individual and household information on a wide range of topics related to work, education, family, consumption, preferences, and attitudes, among others. To match the demonstration dataset, we use the German Socio-Economic Panel (SOEP) from 2005 to 2020 and obtain access to the restricted-use SOEP data with identifiers for respondents' district of residence (*Nuts II*) such that we can link it with the location of the demonstration.

For our two main variables of interest, we rely on the SOEP questions which ask how concerned respondents are about "hostility towards foreigners or minorities in Germany" and "immigration", with the following available answers (1) "not concerned at all", (2) "somewhat concerned" and (3) "very concerned". For our baseline estimations, we use these variables in the continuous form (ranging from 1 to 3). Figure 1 shows the trajectory of outcome means over the sample period. Generally, both types of concerns declined in the years after 2005, then picked up sharply in the years of the refugee crisis, but again subsided somewhat afterwards. Interestingly, both our outcomes were generally decreasing in most years, with increases restricted to the few years between 2013 and 2016. Table A1 in the Appendix shows the basic statistics for the outcomes of interest for the sample used in the empirical analysis. Both outcome variables have means relatively close to 2.04, with worries about immigration being slightly lower at around 1.97 but with a higher standard deviation of about 0.76 in all specifications.

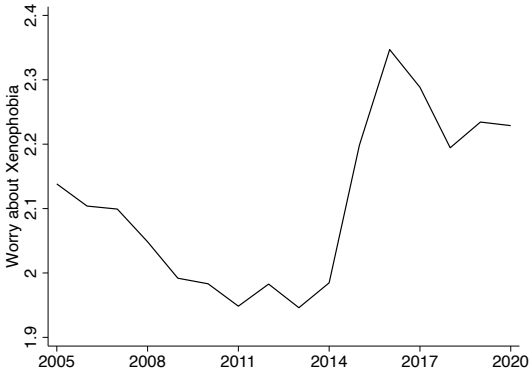
When looking at political behaviour, we focus on a variable reflecting interest in politics (1-4, where 4 is high interest)[15] and four dummy variables reflecting stated party preferences, i) no preference for any political party, ii) preference for a centre-left or left-wing party (SPD, Gruene, Die Linke, Piratenpartei), iii) preference for a centre-right party (CDU, CSU, FPD) and iv) preference for a far-right party (AfD, NPD, Republikaner, Die Rechte).[16]

To study changes in respondents' intentions to support migrants and refugees,

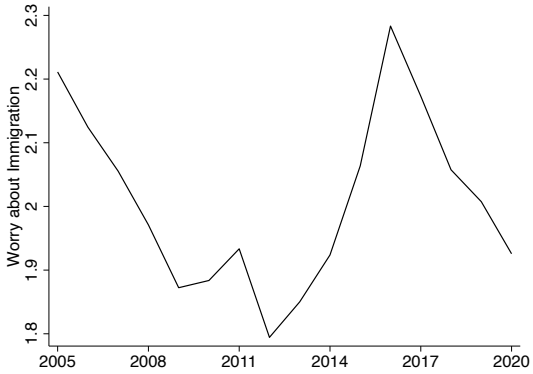
[15]This is based on the question "Generally speaking, how much are you interested in politics?"
[16]The construction of these variables are based on the question "Toward which party do you lean?"

Figure 1: Means of Outcome Variables over Time

(a) Worries about hostility towards foreigners



(b) Worries about immigration



Note: Panel (a) shows a plot of the variables "Worried about hostility towards foreigners or minorities in Germany" and Panel(b) "Worried about immigration" over time. Both variables are measured on a 1-3 scale, where (1) "not concerned at all", (2) "somewhat concerned" and (3) "very concerned".

we rely on three SOEP waves (2016, 2018, and 2020) which asked respondents, "Which of the following activities relating to refugee issues do you plan to engage in the future?", individuals could reply "yes" or "no" to the following three statements "Donating money or goods to help refugees", "Working with refugees directly (e.g., accompanying them to government agencies, providing support in language learning)", and "Going to demonstrations or collecting signatures for initiatives to help refugees". We code these three variables as dummies where 0 is for no and 1 is for yes. Since our dataset only has a few protests for these three years, we are left with a small sample size. Hence, our results should be viewed as complementary evidence.

4 Empirical Strategy and Identification

4.1 Regression Discontinuity Design (RDD)

Our empirical approach compares the attitudes of individuals interviewed in the days immediately before a large right-wing xenophobic demonstration (control group) with those of individuals interviewed immediately after that demonstration (treatment group). To make the case of no-anticipation stronger and separate spillover effects from the possible direct disruptive effect of large protests, we do not consider individuals residing in the district where the large demonstration occurred (l) in our estimations.

A local or spontaneously organised demonstration $j \in \{1, ..., J\}$ occurs on date

c_j (the demonstration-specific cutoff) and district l.[17] An individual $i \in \{1, ..., N_j\}$ living in district $k \neq l$ is interviewed on date d^*_{ij} (the score), which is scheduled many months in advance. We normalise the score $d_{ij} = d^*_{ij} - c_j$ such that treatment assignment is determined by a unique cutoff that is equal to zero in all demonstrations: $T_{ij} = \mathbb{1}\{d_{ij} > 0\}$. We then pool all observations around this unique cutoff and estimate a single regression discontinuity design (RDD) for all demonstrations.[18] Given that some individuals were interviewed on the day of the focal demonstration (approximately 1%), but we have no information on the time of the interview or demonstration, we do not include them. In Section 5.2, we show that our results do not depend on their inclusion.

Our local linear[19] polynomial estimation is the following:

$$Y_i = \alpha + \beta T_{ij} + \mu_1 d_{ij} + \mu_2 T_{ij} d_{ij} + \epsilon_i \tag{1}$$

In equation (1), Y_i is either worries about hostility towards foreigners or worries about immigration. β is our parameter of interest, which can be interpreted as the intent-to-treat estimator or as the causal effect of being interviewed after a local or spontaneously organised demonstration occurred. We use a triangular kernel to give more weight to the observations closer to the cutoff and heteroskedasticity-robust standard errors (Lee and Lemieux, 2010).[20]

In our main results, we consider different bandwidths around the demonstrations: b =15, 20, 30, and the mean squared error optimal bandwidth from Calonico et al. (2019).[21] For expositional clarity, we use the 30-day time window as our preferred bandwidth. We chose this bandwidth because i) we consider isolated demonstrations (described in Section 3.1) using a 30-day criterion, which ensures that the attitudes of individuals interviewed before and after the focal event have not been affected by any other demonstration, ii) we want to make our RDD estimates comparable across

[17] Note that each demonstration takes place in a different day-month-year. Therefore, each cutoff value occurs only once.

[18] This procedure is similar to the "Normalizing-and-Pooling" described in Cattaneo et al. (2016) and Fort et al. (2022) and used in applied work by Black et al. (2007) and Cohodes and Goodman (2014) for instance.

[19] The current consensus in the literature is to use a local linear specification (Cattaneo et al., 2020; Gelman and Imbens, 2019). In Section 5.2, we show our results using a second-order polynomial.

[20] In Section 5.2 we check if our results are sensitive to the choice of kernel by using a uniform kernel instead of a triangular one. We also confirm that our results are unlikely to be affected by potential outliers close to the cutoff by excluding observations in a one-day window around the demonstration in a "donut hole" specification as suggested by Cattaneo et al. (2020).

[21] For most of our analysis, we use the Stata package *rdrobust* (Calonico et al., 2017).

different specifications and iii) to maintain meaningful sample sizes when looking at heterogeneous effects for which we rely on a subset of the original sample. Table 2 in Section 5 shows that our conclusions are robust to different bandwidths.

In Section 5.2, we augment the local polynomial model to include predetermined covariates such as the day of the week, month and year month of the interview, residential district, gender, age, number of children, marital status, educational background and employment status.[22] We show in Section 4.2 that indidivudal characteristics are balanced at the cutoff and in Section 5.2 that their inclusion does not meaningfully change our point estimates.

4.2 Validity of the Regression Discontinuity Design

In this section, we address three potential threats to our regression discontinuity design: 1) selective behaviour around the cutoff, 2) anticipation and 3) selection on observables. 1) and 3) could happen if individuals can precisely manipulate their interview dates (the score). If individuals cannot precisely manipulate the score value they receive, we should not observe any systematic differences in observables between individuals interviewed just before and after the demonstration date (cutoff). Similarly, if there is no precise manipulation, random chance would allocate a similar number of observations to both sides of the cutoff such that the number of interviews is continuously distributed at the cutoff.

1) No selective behaviour at the cutoff A potential threat to the RDD design is if there is selection into or out of treatment based on expected gains. In our setting, there is no clear gain from selecting into or out of treatment, and individuals cannot easily manipulate their treatment assignment since the SOEP interviews are scheduled well in advance. However, it is still possible that individuals are more or less willing to reply to the SOEP survey questions right after a demonstration.

More formally, we employ a density test where the null hypothesis is that the empirical distribution of the number of observations is continuous at the cutoff.[23]

[22]The predetermined covariates are included in a linear and additive-separable way. For local polynomial methods to accommodate covariates without invoking parametric assumptions or redefining the parameter of interest, the covariates must be balanced at the cutoff (Cattaneo et al., 2020). If predetermined covariates were to be imbalanced at the cutoff, this would call into question the continuity assumption and including them as controls would not "fix" the RD design (Cattaneo et al., 2020).

[23]We use the *rdensity* package from Cattaneo et al. (2018) for the density test.

The value of the statistic is 0.4851, and the associated p-value is 0.6276. Hence, under the continuity-based approach, we fail to reject the null hypothesis of no difference in the density of treated and control observations around the cutoff. Figure A1 in the appendix shows a histogram of the number of interviews and confirms the results of the density test that there is no abrupt change in the number of observations at the cutoff.

2) No anticipation As mentioned in the data section, we focus on demonstrations that were organised spontaneously or are of local nature[24] and are larger than usual such that it is reasonable to assume that their date and scale was unlikely to be anticipated by individuals residing outside of the district where the demonstration took place. In Section 5.2, we show that our results are robust when excluding the entire state where the demonstrations occurred. We also show in Section 5.2, that our results remain robust when we exclude the observations near the cutoff, which helps to mitigate concerns about short-run anticipation effects.

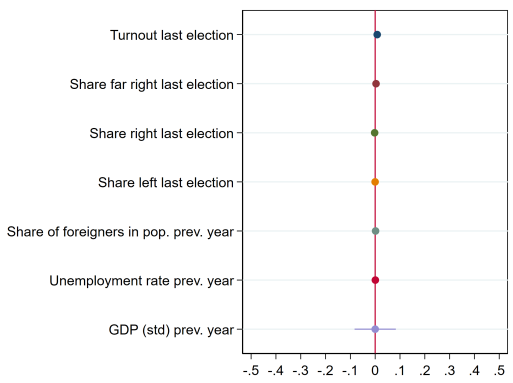
The qualitative media analysis outlined in Section 3.1 and described in detail in Appendix B also shows that there was little reporting in the newspapers before the demonstrations. Nevertheless, one or two local newspapers reported on the demonstrations in the week before they took place. Even though these newspapers are mostly regional and have low national circulation, we test if newspaper coverage potentially affected our outcomes of interest before the actual demonstration took place. To do this, we fix our sample in the pre-period - 30 days before a demonstration takes place - and assign a placebo newspaper treatment 7 days before the true demonstration. The idea behind this strategy is to compare the outcomes between those interviewed before and after the potential newspaper reporting on the demonstration. We also assign a newspaper treatment 5 days before to get closer to the actual demonstration date. The results of this exercise are displayed in Table A2 in Appendix A and show no significant effect of the placebo newspaper treatments on our outcomes of interest.

[24]The demonstrations considered in the RDD are those satisfying the criteria 1), 2), and 3) established in Section 3.1.

Figure 2: Continuity Tests

(a) Individual characteristics

(b) District characteristics



Note: Panel (a) and (b) display the coefficients from the estimation of Equation (1) on the individual characteristics and district characteristics listed on the y-axis, respectively. All regressions consider demonstrations with more than 1500 participants and use a 30-day bandwidth. 95 per cent confidence intervals are shown.

3) The continuity assumption holds Our identification strategy relies on the assumption that the individuals interviewed before a focal demonstration (control group) are similar to those interviewed after that focal demonstration (treatment group), constituting a credible counterfactual. We provide evidence that the continuity assumption holds by estimating equation (1) using predetermined individual and district characteristics as an outcome. Since the demonstration should not affect the predetermined covariates, the null hypothesis of no treatment effect should not be rejected if the RD design is valid. For individual characteristics, we consider gender, age group, marital status, if the respondent has a child, employment status and educational achievement at the time of the survey. For the characteristics of the districts, we use the one-year lag of the unemployment rate, share of foreigners, standardised GDP,[25] election turnout, share of the far-right, centre-right and left-wing vote in the last elections in the Nuts II region where the respondent resided at the time of the survey.[26]

In Figure 2, we show that the characteristics of the districts and of the respondents do not depend on whether they were interviewed before or after a demonstration. Across specifications, the treatment and control groups have very similar characteristics, with only mild differences in the share with vocational training.

[25] We standardise so that the scale fits with the other variables.

[26] Elections took place in 2005, 2009, 2013 and 2017. Individuals interviewed in 2015, for instance, will be assigned the turnout and vote shares of 2013.

5 Results

5.1 Main Results

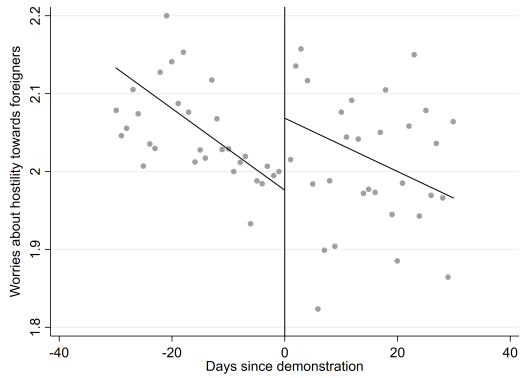
Figure 3 shows a regression discontinuity design plot for worries about hostility towards foreigners (Panel (a)) and worries about immigration (Panel (b)) using a local linear trend with a 30-day bandwidth, triangular kernel and mimicking variance evenly-spaced bins.

The plot in Panel (a) shows a discontinuity at the cutoff, suggesting that large right-wing demonstrations increase the worries about hostility towards foreigners. In Panel (b), we see no such suggestive evidence for the worries about immigration.

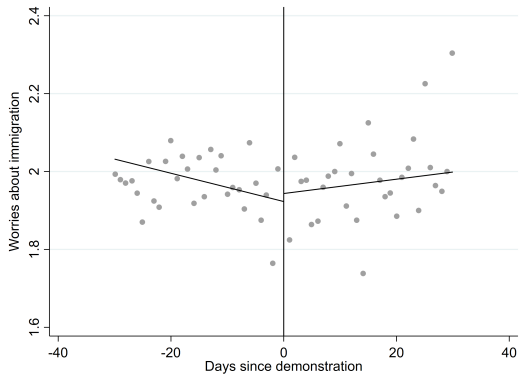
The main results of our analysis, using equation (1), are displayed in Table 2 below. They show the effects of large right-wing demonstrations on respondents' attitudes at the national level for time windows of 9 or 10 days (optimal bandwidth), 15 days, 20 days, and 30 days around the date of the demonstrations and excluding respondents from the district where each protest took place. In line with the graphical evidence, the coefficients in Panel A of Table 2 indicate that natives' concerns about intolerance increased markedly and significantly in response to large xenophobic demonstrations. Using a 30-day bandwidth, we see that a large, isolated and local or spontaneously organised protest led to a 0.0924 increase in worries about hostility towards foreigners, which represents an increase of 4.50% relative to the baseline or

Figure 3: Main RDD Plots

(a) Worries about hostility towards foreigners



(b) Worries about Immigration



Note: Figure 3 shows a regression discontinuity design plot for "Worries about hostility towards foreigners or minorities in Germany" (Panel (a)) and "Worries about immigration" (Panel (b)) using a local linear trend with a 30-day bandwidth, triangular kernel and mimicking variance evenly-spaced bins. Both variables are measured on a 1-3 scale. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded.

Table 2: Main RDD Results

Bandwidth:	Optimal: 9 days	15 days	20 days	30 days
	(1)	(2)	(3)	(4)
RD Estimate	0.1437**	0.1257***	0.1131***	0.0924***
	(0.0644)	(0.0430)	(0.0369)	(0.0300)
Baseline	2.0535	2.0192	2.0426	2.0535
Observations	2498	5206	7238	10902
Panel B: Worries about immigration				
RD Estimate	0.0588	0.0625	0.0539	0.0206
	(0.0648)	(0.0491)	(0.0422)	(0.0342)
Baseline	1.9715	1.9658	1.9779	1.9715
Observations	2867	5206	7238	10902

Note: Table 2 displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in Panel B and worries about immigration in Panel B for varying bandwidths. Both outcome variables range from 1-3, with *Baseline* indicating mean values within each bandwidth. All regressions consider a demonstration to be relevant if it has more than 1500 participants, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded. Heteroskedasticity-robust standard errors in parentheses.
*p<.1; **p<.05; ***p<.01

13.70% of a standard deviation. The RDD estimate does not vary greatly across time windows. As mentioned in Section 4, we use the 30-day bandwidth in most of our analysis because the procedure used to identify isolated demonstrations uses 30-day criteria and because we want to make our RDD estimates comparable across different subgroups and specifications. The results in Panel B of Table 2 show that respondents did not become more worried about immigration. While positive, the effect of demonstrations on worries about immigration remains insignificant.

Taken together, these findings indicate large xenophobic demonstrations were unsuccessful in swaying the public's opinion in their favour nationwide, as concerns about hostility towards foreigners increased, while worries about immigration remained essentially flat. These results suggest that residents nationwide perceived far-right protesters as a threat.

5.2 Robustness Checks

In this section, we present a series of robustness checks using our preferred measure of large and salient demonstrations (number of participants above the 99th per centile at 1500 individuals) with a 30-day bandwidth. We start by demonstrating that our results are robust to using a binary instead of a continuous outcome variable, including control variables and choosing different empirical specifications. Secondly,

we show that our conclusions hold when varying the cutoff for large demonstrations, excluding the entire state where the demonstration occurred (rather than the Nuts II only) and excluding a specific demonstration from the analysis. We also demonstrate that when we assign a random date to each xenophobic demonstration, their effect on attitudes is null on average and that the effect of the true demonstrations on worries that should not be affected by far-right protests is also null. Finally, we show our results when using local randomisation RDD.

Dichotomous dependent variables As a first robustness test, we transform our dependent variables such that worries about hostility towards foreigners (immigration) equals one if the respondent replied to be "somewhat concerned" or "very concerned" about hostility towards foreigners (immigration) and zero if the respondent replied "not concerned at all". Columns (1) and (3) in Table A3 in the Appendix show the results when using the dependent variables in the continuous form, on a 1-3 scale, and columns (2) and (4) when dichotomizing the dependent variable. The results are qualitatively similar.

Controlling for individual characteristics, time and location factors As a second robustness test, we augment the local polynomial model to include predetermined covariates in a linear and additive-separable way. As shown in Figure 2 the assignment to the right or left side of the cutoff does not depend on individual or district characteristics. Nevertheless, Table A4 in the Appendix shows the results when adding different sets of controls. Column (1) shows the baseline results as in Table 2, column (2) adds the Nuts II region where the individuals being interviewed reside, column (3) the year of the interview, column (4) the month of the interview and column (5) the day of the week. Column (6) shows the main results when adding the individual characteristics used in the balance tests, and column (7) adds all controls combined. Our results do not change.

Alternative specifications This sub-section shows that our results are robust to different and more flexible specifications. Panel (a) in Figure 4 shows the robustness checks for the worries about hostility towards foreigners, and Panel (b) for worries about immigration. The first line in both panels displays the baseline effect reported

in column (4) of Table 2.

In the second line of Figure 4, we show that the dynamics of the European Refugee Crisis are unlikely to confound our analysis. The increased inflow of asylum seekers into Germany, which peaked in 2014-2015, led to potential monthly variations in the inflow of refugees to a given district. This could confound our pre-and-post demonstration analysis even when using a 30-day time window.[27] However, the results in Figure 4 show that our main coefficient of interest changes little when we exclude post-2013 demonstrations.

In our main specification, we excluded individuals interviewed on the day the focal demonstration took place because we have no information on the hours of the demonstration. Line 3 of Figure 4 shows that our results do not change when we add to the treatment group people interviewed on the day of the demonstration.

In line 4 of Figure 4, we investigate the sensitivity of the results to the response of the individuals interviewed very close to the cutoff. If there was a systematic manipulation of score values, individuals interviewed very close to the cutoff are those most likely to have engaged in manipulation. To test for this, we exclude individuals interviewed at -1 and 1 (the "donut hole" approach) (Cattaneo et al., 2020).[28] The results in line 4 show that the conclusions from the analysis are robust to excluding observations with $|d_{ij}| < 1$.

We excluded individuals residing in the district where the large protest took place to strengthen the case of no-anticipation and to separate the spillover effect from the possible direct disruptive effect of large demonstrations. In line 5 of Figure 4, we show that our results are robust to the inclusion of these individuals.

In our main specification, we have followed the recent consensus in the literature (Gelman and Imbens, 2019; Cattaneo et al., 2020) and used a local linear specification[29] and triangular kernel function which gives more weight to the observations closer to the cutoff. In line 6 of Figure 4, we show that our point estimates become larger when we include a second-order polynomial but do not change our study's

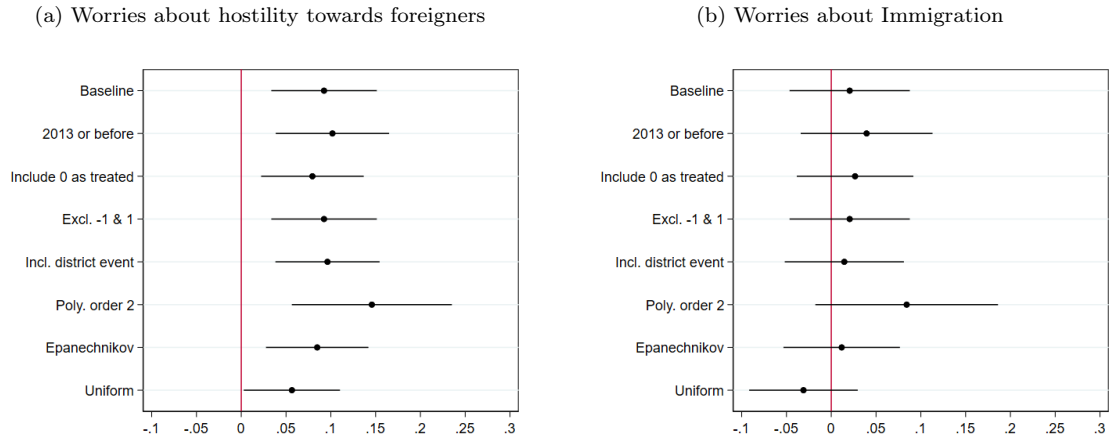
[27] At the same time, these dynamics made monthly protests more recurrent (Gattinari et al., 2021)

[28] Furthermore, this test also allows evaluating the sensitivity of the results to the extrapolation intrinsic to the local polynomial estimation, where the few individuals interviewed close to the demonstration are likely to be the most influential when fitting the local polynomials.

[29] The reason to do so is that higher-order polynomials increase the chances that we are giving high weights to observations which are further away from the cutoff; this tends to produce overfitting of the data and lead to unreliable results near boundary points (Cattaneo et al., 2020). Cattaneo et al. (2020) notes that in most situations, incorporating higher-order terms will reduce the approximation error and lead to changes in the estimated effect. However, the relevant question is whether such changes alter the study's conclusions.

conclusions. In lines 7 and 8 of Figure 4, we display our results when using an Epanechnikov kernel, which gives a quadratic decaying weight, and a uniform kernel, which gives equal weight to all observations whose scores are within the selected bandwidth. Although using a uniform kernel slightly changes the magnitude of the coefficients, the main conclusions remain unchanged.

Figure 4: Robustness: Testing Different Specifications



Note: Figure 4 displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in Panel (a) and worries about immigration in Panel (b). Both variables are measured on a 1-3 scale. All regressions consider a demonstration to be relevant if it has more than 1500 participants, use a 30-day bandwidth and heteroskedasticity-robust standard errors. 95 per cent confidence intervals are shown. The different methods and choices are listed on the y-axis. The baseline estimation uses a triangular kernel, a polynomial or order one and excludes the Nuts II and the day of the demonstrations.

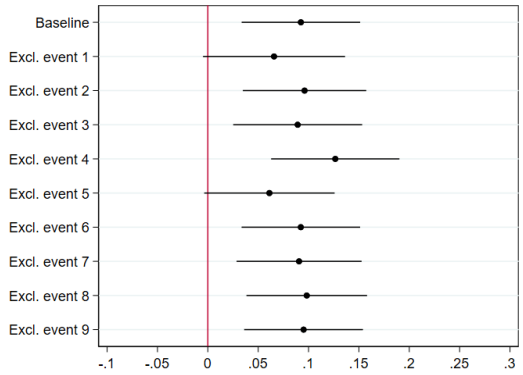
Varying the definition of a large demonstration We have considered a demonstration large if it is above the 99th percentile at 1500 participants (9 demonstrations). Since the boundary choice for a demonstration to be large carries a degree of arbitrariness, in this subsection, we check the sensitivity of our results to changes in this boundary. As alternatives, we consider demonstrations where the number of participants is slightly below, at 1200 (12 demonstrations), or slightly above, at 2000 (7 demonstrations). The results are displayed in Table A5 in the Appendix and show that our conclusions are robust to variations around the definition of a large event.

Exclude all districts in the state where the demonstration took place To further reduce any concerns about anticipation effects, we exclude respondents from the entire state (instead of the district) where the actual demonstration occurred. Table A6 in the Appendix shows the results for this exercise - the point estimates are close to those in our main results in Table 2.

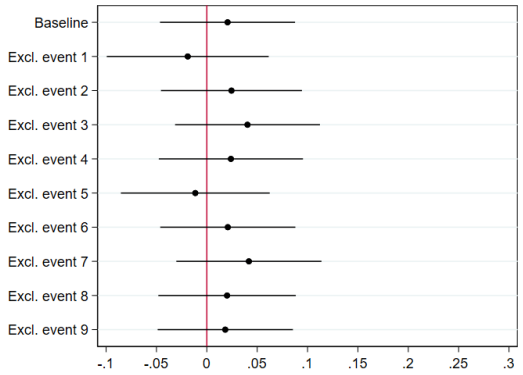
Exclude one event at a time To assess the importance of a particular demonstration to our estimation, Figure 5 shows our main results when we exclude one of the nine demonstrations at the time. Generally, our estimates remain stable and robust to the exclusion of these events. While excluding events 1 and 5 slightly reduces the coefficient on the worries about hostility towards foreigners, it remains statistically different from zero at the 10 per cent level. The coefficients in the worries about immigration regression are always statistically indistinguishable from zero.

Figure 5: Robustness: Exclude One Event at a Time

(a) Worries about hostility towards foreigners



(b) Worries about immigration

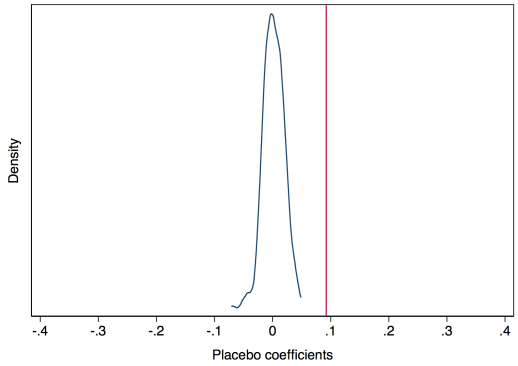


Note: Figure 5, displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in Panel (a) and worries about immigration in Panel (b). Both variables are measured on a 1-3 scale. All regressions consider a demonstration to be relevant if it has more than 1500 participants, use a 30-day bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. 95 per cent confidence intervals are shown. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded.

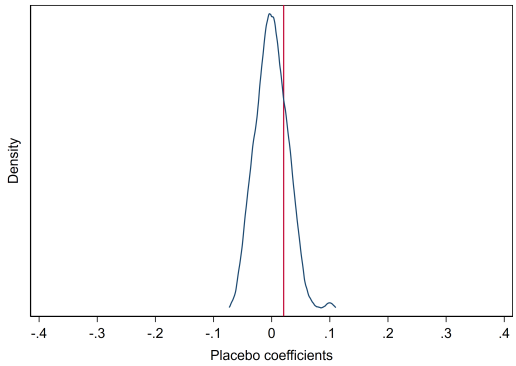
Placebo treatment date As a placebo test, we assign a random date to each relevant and isolated demonstration, estimate equation (1), and repeat this procedure 300 times. The distribution of the coefficients is shown in Figure 6 and is concentrated around zero. In Panel (a) "Worries about hostility towards foreigners", the red vertical line represents the true effect of 0.0924 estimated in our baseline regression in Table 2 and is far away from the distribution of the coefficients when using random dates. This indicates that our results are likely due to the xenophobic protests and not some statistical artefact. In Panel (b), "Worries about immigration", the true effect is 0.0206 and is close to the zero mean of the distribution of the coefficients when using random dates.

Figure 6: Robustness: Use Placebo Treatment Date

(a) Worries about hostility towards foreigners



(b) Worries about immigration



Note: Figure 6, displays the distribution of the coefficients from estimating 300 times equation (1) on worries about hostility towards foreigners in Panel (a) and worries about immigration in Panel (b). Both variables are measured on a 1-3 scale. All regressions consider a random demonstration date, use a 30-day bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. The Nuts II and the day of the random demonstrations are excluded.

Placebo outcomes As a second placebo test, we consider the treatment effects on worries which, in principle, should not be affected by far-right demonstrations. These worries are captured in the SOEP data and relate to own health, own economic situation, and global terrorism. Table A7 in the Appendix shows the coefficients when estimating equation (1) using these alternative outcomes. As expected, xenophobic demonstrations did not affect these worries, as all coefficients remain insignificant.

Local randomisation RDD The regression discontinuity framework used throughout this study is based on continuity assumptions. Although this approach is the most commonly used in practice (Cattaneo et al., 2020), we employ another framework based on local randomisation assumptions in this sub-section. We do so because our running variable, the interview day, is not truly continuous (we do not measure one-third of a day) and can be considered a discrete variable. When the running variable is discrete, the local randomisation approach can be employed because it does not impose assumptions as strong as when the running variable is truly continuous.

The main difference of the local randomisation approach is that instead of relying upon continuity and differentiability assumptions, it assumes that for a small window around the cutoff, the treatment status is assigned as it would have been in a randomised experiment. The day an individual is interviewed can be considered a randomly generated number unrelated to the average potential outcomes.[30]

[30]While in the continuity-based RDD the average potential outcomes are non-constant functions of the score, in

Table 3: Robustness: Local Randomization

	Worries about hostility towards foreigners (1)	Worries about immigration (2)
Local Randomization Estimate	0.0594**	0.0197
Power vs Local Pol.	0.999	0.460
Baseline	1.9893	1.9227
Observations	2243	2243

Note: Table 3 displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in column (1) and worries about immigration in column (2) using a local randomization approach. Both variables are measured on a 1-3 scale. All regressions consider a demonstration to be relevant if it has more than 1500 participants and use a 30-day criteria to identify isolated demonstrations. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded. Heteroskedasticity-robust standard errors in parentheses.
*p<.1; **p<.05; ***p<.01

A crucial component of the local randomisation approach is the window W, where the local randomisation assumption is invoked. To choose this window, we follow on Cattaneo et al. (2015, 2016) and use a procedure based on balance tests for regression discontinuity (RD) designs under local randomisation. We use the *rdrandinf* package developed by Cattaneo et al. (2016) and consider the following individual characteristics: gender, age, marital status, presence of children, employment status and education. Using this criterion, the optimal window W is one week. The results using the local randomisation approach with a one-week window are displayed in Table 3. The point estimates are slightly smaller, but the overall results are robust and consistent with the continuity approach.

5.3 Newspaper Coverage

As laid out in Section 2, media and news reporting might play a role in how people learn about the demonstrations and how they perceive them. In this section, we conduct a short media analysis and present suggestive evidence that newspaper reporting are to mediate the effect of xenophobic demonstrations on migration attitudes.[31]

We conduct our media analysis based on data by *genios.de*, which assembles and provides articles from several hundred German newspapers in Germany. Apart from studying whether there has been anticipation of protests in newspapers, we also use this data to examine the extent of reporting after the protests have taken place.

the local randomisation RDD, the functions are constant in the entire region where the score is randomly assigned.
[31]Analysing the sentiment of the newspaper's reporting and/or other media is outside this study's scope.

Table B1 summarises and presents to what extent newspapers covered protests. The construction of this dataset is discussed in more detail in Appendix B.

The data reveal several interesting insights: First, we see that there was reporting on all but one demonstration (Jänkendorf 2010) within the first three days, with all other events being covered by newspapers inside and outside of the state where they took place. Moreover, all but two protests were reported by national newspapers within 3 days. This indicates that for most events, there was considerable attention from newspaper media.[32]

Second, most reporting occurs relatively close to the event date and then subsides. While for the protests between 2006 and 2015, there is often some lag in reporting as many newspapers only start reporting two days after the event took place, recent protests are covered much more quickly. Moreover, reporting three days after the protest took place is usually fairly limited, and apart from the Chemnitz event (2018a), reporting wanes afterwards.[33]

Third, the volume of reporting differs quite substantially between the different protests. While some protests (e.g., Berlin 2005, Chemnitz 2018a, 2018b, Dresden 2019) received a lot of reporting in newspapers, others (e.g., Leipzig 2009, Jänkendorf 2010, Jänkendorf 2011) received much less attention from news outlets.

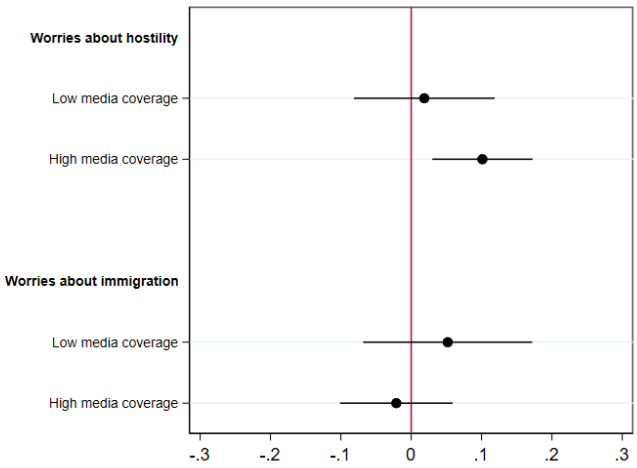
We make use of this variation and construct a dummy variable, which indicates whether media coverage was low or high ("Salience" in Table B1). Hereby, we consider the number of reporting newspapers distributed outside the district, for how many days coverage took place, and whether national newspapers covered the protest.

The results of this analysis are presented in Figure 7 and show that the intensity of newspaper reporting appears to play a role. While we see significant and large effects on worries about hostility towards foreigners for highly-covered protests, the coefficient is not statistically significant for those protests that received little newspapers coverage. For worries about immigration, we see no significant difference, as the coefficients are not statistically significant.

[32] A potential shortcoming of our analysis could be that we can not examine reporting on TV or other media sources which may have also played an important role in our period under analysis. However, we belief that the newspaper coverage summarized in our dataset is representative of media reporting more generally.

[33] Not shown in Appendix B, as there was very little if any coverage of most protests four or more days after they took place.

Figure 7: RDD Results By Extent of Media Coverage



Note: The y-axis in Figure 7, displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in Panel (a) and worries about immigration in Panel (b) when we distinguish protests by their level of media coverage (or salience). Both variables are measured on a 1-3 scale. All regressions consider a demonstration to be relevant if it has more than 1500 participants, use a 30-day bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. 95 per cent confidence intervals are shown. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded.

5.4 Heterogeneity Analysis

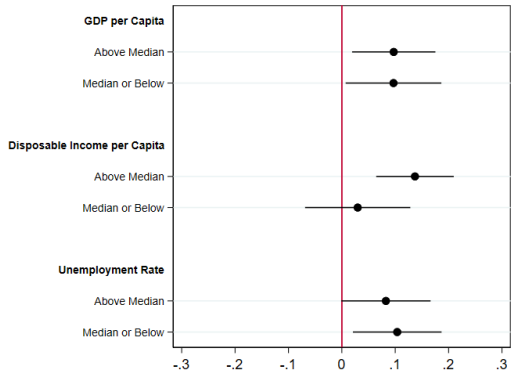
In the previous sections, we analysed the effects of far-right demonstrations on the attitudes of the native population. However, our estimates could obscure potential heterogeneities both in terms of the location where respondents reside as well as individuals' characteristics and previous political and social attitudes. Studying these heterogeneities can help us explain who actually reacted in which way in response to far-right protests.

In this section, we perform multiple separate regressions in which we evaluate the impact of economic, political, and structural factors at the regional level and analyse to what extent results may differ when we distinguish individuals by labour market, demographic, and attitudinal characteristics. We split the sample into different groups and run equation (1) on concerns about hostility towards foreigners and immigration. As in the previous section, we present all our results using large demonstrations, with more than 1500 participants, and using a 30 days bandwidth.

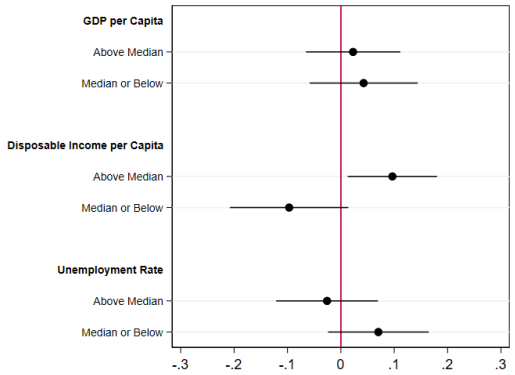
Regional economic characteristics For the heterogeneity analysis based on district economic characteristics, we take the yearly median GDP per capita, disposable income per capita, and the unemployment rate at the NUTS II level and classify each district-year as being above the yearly median in each of this characteristics or

Figure 8: Heterogeneity Analysis: By Regional Economic Situation

(a) Worries about hostility towards foreigners



(b) Worries about immigration



Note: Figure 8, displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in Panel (a) and worries about immigration in Panel (b), restricting the sample to the group listed on the y-axis. Both variables are measured on a 1-3 scale. All regressions consider a demonstration to be relevant if it has more than 1500 participants, use a 30-day bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. 95 per cent confidence intervals are shown. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded.

not.[34] We then take a one-year lag of each of these measures relative to the year of the interview.[35]

Generally, there is no clear indication that respondents in economically weaker regions react differently. Looking at GDP per capita and the unemployment rate, there is hardly any difference in estimates for both worries about hostility towards foreigners and worries about immigration. We see a difference only when we compare respondents by regional disposable income. However, there is no clear pattern here either, as individuals in regions with above-median income experience an increase in both types of concerns, possibly indicating some polarisation, while for the other group, neither coefficient is statistically different from 0. If anything, worries about immigration appear to decrease for respondents in the lower-income regions. Overall, we find no clear evidence that people residing in more economically deprived areas react differently than those in more prosperous regions.

Regional political characteristics In this sub-section, the sample is split by the NUTS II regional voting share of far-right, left-of-centre, and right-of-centre parties[36] in the

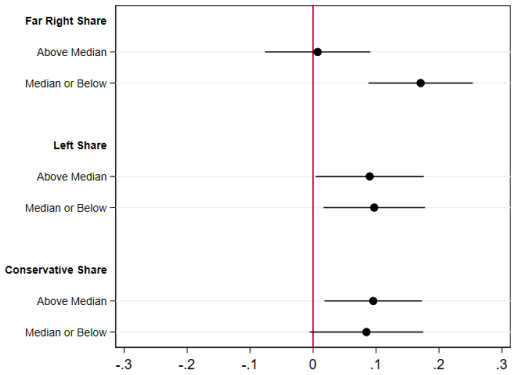
[34]The regional data is provided by the statistical offices of the German states (*Statistische Landesämter*) and can be accessed publicly via regionalstatistik.de.

[35]This is done to avoid the issue that our treatment may directly affect those regional characteristics.

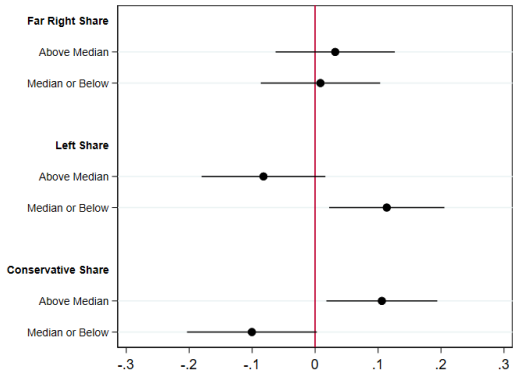
[36]For far-right parties, we look at the vote share of the following parties: NPD, Republikaner, DVU, AfD, Pro Deutschland, die Rechte, and Schill-Partei/Offensive D. For left-of-centre parties we include the SPD, Bündnis 90/Die Grünen, PDS/Die Linke, and Piratenpartei. Right-of-centre parties are CDU, CSU, and FDP.

Figure 9: Heterogeneity Analysis: By Regional Political Environment

(a) Worries about hostility towards foreigners



(b) Worries about immigration



Note: Figure 9 displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in Panel (a) and worries about immigration in Panel (b), restricting the sample to the group listed on the y-axis. Both variables are measured on a 1-3 scale. All regressions consider a demonstration to be relevant if it has more than 1500 participants, use a 30-day bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. 95 per cent confidence intervals are shown. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded.

last federal election relative to the interview date.[37] In contrast to economic factors, Figure 9 displays that political factors appear to influence respondents' reactions to the protests.

The estimates in Figure 9 (a) show that individuals who live in NUTS II regions with a higher share of far-right voting do not experience an increase in their concerns about hostility towards foreigners after protests take place, while respondents in other regions see a considerable increase. In contrast, when splitting the sample along the election vote share of left-wing and moderate conservative parties shows no statistically significant differences.

Figure 8 (b) shows the results for worries about immigration. While the estimates are virtually the same in regions where far-right parties are more or less successful, there is a marked difference when we split the sample by the vote share of left-of-centre and moderate right-leaning parties. While worries decrease (increase) in areas where left-wing parties are more (less) successful, the opposite is true for right-of-centre parties.

This sets up an interesting picture, whereby respondents in relatively left-leaning areas appear to show a reasonably consistent reaction to far-right demonstrations, which runs counter to the interests of the protesters, as they both increase their concerns about hostility towards foreigners and become less worried about immi-

[37]There were federal elections in 2002, 2005, 2009, 2013 and 2017.

gration. In right-leaning areas, on the other hand, there appears to be more of an ambivalent, potentially even polarizing reaction, with increases in both types of concerns. This indicates that the political environment might affect how respondents perceive protests. However, one should be careful not to draw strong conclusions, particularly with regard to the far-right vote share, as it was often still rather low, even in areas where they were relatively more successful.

Figure A2 in Appendix A looks at some additional heterogeneities at the district level. Most noteworthy here is that both types of concerns remain unchanged in eastern Germany. Moreover, worries about immigration increase significantly in districts with fewer foreigners, while the increase is only borderline significant in rural areas.

Individual characteristics Using information from the SOEP, we distinguish respondents by their labour market status and household income quartiles. The coefficients in Figure 10 show that there is not much of a difference across groups, as individuals react for the most part similarly to protests, both in terms of their concerns about hostility towards foreigners and immigration. Nevertheless, it seems that unemployed respondents or those with the lowest incomes do react more negatively. These results are in line with the estimates on the regional level in Figure 8, suggesting that economic factors do not play much of a role in determining respondents' reactions to local or spontaneously organised large far-right demonstrations.

In addition, Figure A3 in the appendix distinguishes along several demographic characteristics. While the differences across demographic groups are not very strong, the effects of the demonstrations on worries about hostility towards foreigners are more pronounced for men, married people, childless individuals, and respondents with medium education. The coefficients are virtually the same across demographic groups when looking at concerns about immigration. Overall, heterogeneities along demographic lines appear fairly limited.

Individual political attitudes To look at heterogeneity analysis by political attitudes, we rely on the panel structure of the SOEP. Since not all individuals are interviewed yearly, our sample size is reduced to 3,659 observations from the 10,902 observations reported in Table 2.

First, we consider SOEP interviewees' self-placement on the political spectrum - respondents can place themselves on a 0-10 scale from extremely left-wing (0) to extremely right-wing (10). We then group individuals into a left-of-centre (from 0 to 3), centre (4 to 6) and right-of-centre (7 to 10) category. Because this self-assessment only takes place every four to five years, we use the last known lagged value to ensure that it is not affected by the protests themselves. Second, we consider individual one-year-lagged political interests and create two categories: none-to-low political interest and medium-to-high political interest. Lastly, we split the sample by reported worries about hostility towards foreigners and worries about immigration in the previous interview.

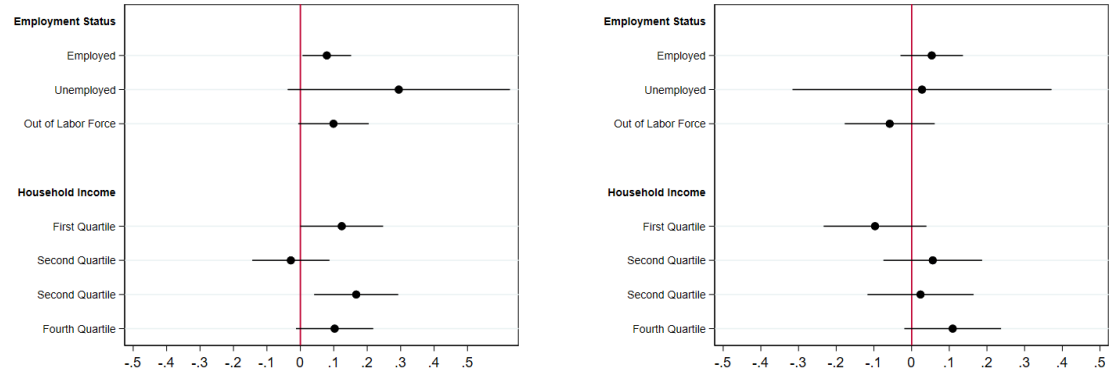
In contrast to economic characteristics, heterogeneities based on political attitudes seem much more striking. The heterogeneity by self-placement on the political spectrum in Figure 11 (a) shows an interesting picture; only those respondents who place themselves left-of-centre had significantly increased concerns about hostility towards foreigners. On the other hand, Figure 11 (b) shows that the point estimate for worries about immigration is the highest for respondents who place themselves right-of-centre, even though it is not significantly different from zero. Thus, previous political viewpoints appear to be key in individuals' receptiveness to protests.

When looking at the heterogeneous effects by lagged political interests, the estimates are virtually the same for those with higher and lower levels of interest.

Figure 10: Heterogeneity Analysis: By Individual Labour Market Situation

(a) Worries about hostility towards foreigners

(b) Worries about immigration

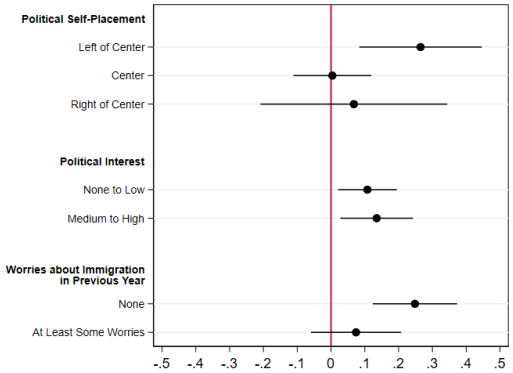


Note: Figure 10 displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in Panel (a) and worries about immigration in Panel (b), restricting the sample to the group listed on the y-axis. Both variables are measured on a 1-3 scale. All regressions consider a demonstration to be relevant if it has more than 1500 participants, use a 30-day bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. 95 per cent confidence intervals are shown. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded.

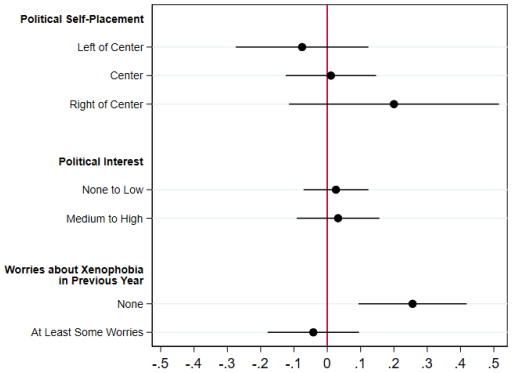
The coefficients of the heterogeneity analysis by lagged worries suggest that existing political or social attitudes are the major drivers in how people perceive and react to protests. While the effects in Figure 11 (a) seem solely driven by individuals who were previously unconcerned about immigration, respondents who were not concerned about hostility towards foreigners have significantly increased worries about immigration in response to far-right protests. These results suggest that there might be some polarisation in the population in response to the protests, which would align with studies such as Caprettini et al. (2021).

Figure 11: Heterogeneity Analysis: By Political and Social Attitudes

(a) Worries about hostility towards foreigners



(b) Worries about immigration



Note: Figure 11, displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in Panel (a) and worries about immigration in Panel (b), restricting the sample to the group listed on the y-axis. Both variables are measured on a 1-3 scale. All regressions consider a demonstration to be relevant if it has more than 1500 participants, use a 30-day bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. 95 per cent confidence intervals are shown. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded.

6 Changes in Political Preferences and Pro-Social Behaviour towards Migrants

In our main results, we focused on the effect of far-right protests on attitudes towards migration in the native population. However, it might be important for policymakers and politicians to know to what extent the changes in attitudes can lead to changes in interest in politics, party preferences, and pro-social behaviour towards migrants. In this section, we show suggestive evidence that by increasing the salience of immigrants and affecting public attitudes towards foreigners, large right-wing demonstrations can affect interests in politics, political preferences and intentions to help refugees. We do not claim that the effect on political preferences

Table 4: Extension: Political Interest and Party Preferences

	Interest in politics	No preference for any pol. party	Preference left-wing party	Preference right-wing party	Preference far-right party
	(1)	(2)	(3)	(4)	(5)
RD Estimate	0.0757**	-0.0686***	0.0453**	0.0221	0.0051
	(0.0372)	(0.0229)	(0.0202)	(0.0181)	(0.0051)
Baseline	2.3630	0.5349	0.2380	0.1961	0.0125
Observations	10886	10853	10680	10680	10680

Note: Table A7 displays the coefficients from estimating Equation (1) using the outcomes: interest in politics (1), having no party preference (2), and stated party preference for a left-wing (3), center-right (4), and far-right party (5). Political interest in scaled from 1 to 4. All other variables are binary, with *Baseline* indicating mean values for each outcome. All regressions consider a demonstration to be relevant if it has more than 1500 participants, use a 30-day bandwidth, a triangular kernel, a polynomial of order one. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded. Heteroskedasticity-robust standard errors in parentheses.
*p<.1; **p<.05; ***p<.01

Table 5: Extension: Pro-Social Behaviour toward Refugees

	Donate money or goods to help refugees	Work with refugees directly	Participate in initiatives to help refugees
	(1)	(2)	(3)
RD Estimate	0.1121**	-0.0182	0.0810**
	(0.0523)	(0.0290)	(0.0361)
Baseline	0.2286	0.0998	0.0633
Observations	1652	1652	1652

Note: Table 5 displays the coefficients from the estimation of Equation (1). All outcomes variables are binary, with *Baseline* indicating mean values for each outcome. All regressions consider a demonstration to be relevant if it has more than 1500 participants, use a 30-day bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded. Heteroskedasticity-robust standard errors in parentheses.
*p<.1; **p<.05; ***p<.01

stems directly from the demonstrations since there could be second-order effects, e.g., coming from the possible reaction of the different parties to some of these events.

Table 4 shows the results of estimating Equation (1) on interest in politics (1-4, where 4 is high interest) in column (1) and on four dummy variables reflecting party preferences in columns (2)-(5).

The estimates in Table 4 suggest two main effects: respondents become more politically engaged in response to the protests, and this shift mainly helps left-wing parties. The coefficients in columns (1) and (2) indicate both an increase in political interest and in expressing a preference for a political party. The estimates in the following columns (3) to (5) show us that preference for left-wing parties increases significantly by around 4.5 per centage points. At the same time, there is no significant increase in the propensity to favour right-of-centre or even far-right parties. While these coefficients do not perfectly inform us about the intentions of individuals, taken together, they imply that local or spontaneously organised large far-right demonstrations led to an adverse reaction in the population as people became more active in opposing the protesters.

In Table 5, we look at the effect of large right-wing demonstrations on the intentions to help refugees. We can see that following a large far-right demonstration, individuals are more likely to want to donate or participate in initiatives to help refugees in the future. However, they are not more likely to want to work directly with refugees in the future. These results also serve as complementary evidence that local and spontaneous large right-wing demonstrations did not sway the public's opinion against immigrants. Native Germans seem to wish to counterbalance the xenophobic rhetoric of these demonstrations by helping refugees.

7 Conclusion

One of the primary objectives of public demonstrations is to bring social, political, or economic issues to the attention of politicians and the wider population. Although demonstrations can have a mobilizing and persuading effect, if turned violent or disruptive, they may reduce support for their cause.

In this study, we use a regression discontinuity design to analyse how large right-wing xenophobic demonstrations affect concerns about hostility towards foreigners and worries about immigration among natives in Germany. Our results show that local xenophobic demonstrations lead to a significant short-term increase in worries about hostility towards foreigners at the national level, indicating that these demonstrations are perceived as a threat by Germans. On the other hand, worries about immigration are not affected by the demonstrations, indicating that the demonstrations are not successful in swaying public opinion in their favour. We also find that individuals become more politically interested following far-right demonstrations, mainly benefiting left-wing parties, and that they become more willing to help refugees. Our results are robust to a series of robustness checks.

The data and empirical design of this study have several advantages. Firstly, the SOEP individual data enables us to examine a larger range of outcome variables. We can focus on a set of variables that capture underlying individual attitudes and are not influenced by party affiliation: concerns about hostility towards foreigners, worries about immigration, intention of helping refugees and interest in politics. Secondly, we can estimate the immediate impact of the demonstrations. A typical

challenge in the protest literature is to understand whether protests cause political changes or reflect changes in the underlying political preferences. Since we compare the attitudes between 9 and 30 days before and after a demonstration, our estimation approach allows us to claim that the demonstrations and not other factors impact attitudes and party preferences. Thirdly, significant parts of the (quantitative) political science and economics literature is concerned with the local impacts of protests while overlooking national effects (e.g., Madestam et al., 2013; Enos et al., 2019; Klein Teeselink and Melios, 2022; Wasow, 2020; Larreboure and Gonzalez, 2021). However, we show that large-scale demonstrations also have an impact on national attitudes, especially in the time period when people learn about these demonstrations from news media.

This study broadens our understanding of the consequences of different types of demonstrations by showing how local or spontaneously organised right-wing demonstrations can impact attitudes at the national level. Yet, its conclusions are limited to protests that have a local or spontaneous nature. Therefore, future research is needed to understand the effects of protests that are organized at a national level or are concurrent with other major national events.

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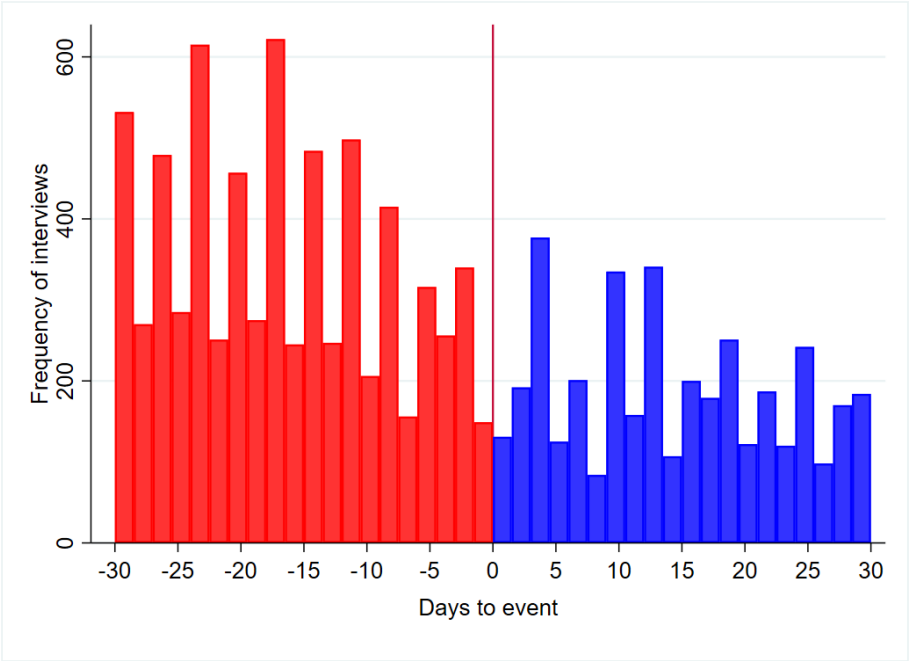
Wasow, O. (2020). Agenda seeding: How 1960s black protests moved elites, public opinion and voting. *American Pol. Science Rev. 114*(3), 638–659.

Wouters, R. (2019). The persuasive power of protest. How protest wins public support. *Social Forces 98*(1), 403–426.

A Additional Tables and Figures

Figure A1: Density Test: Frequency of Interviews

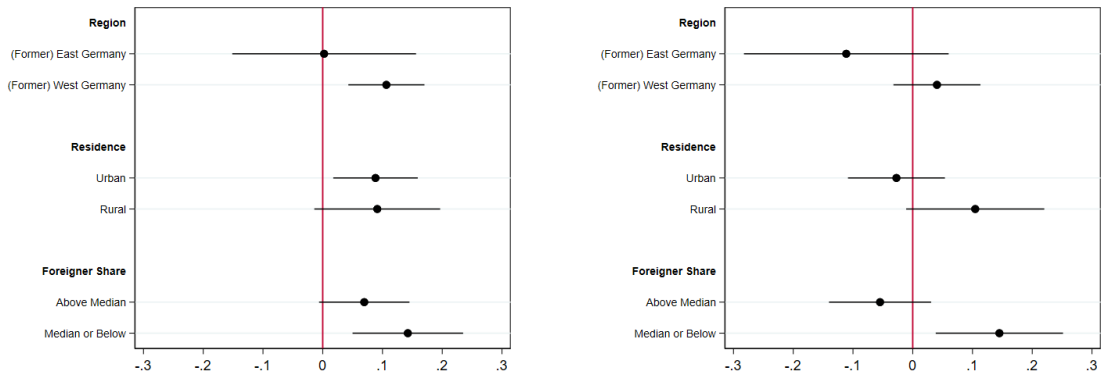


Note: The y-axis in Figure A1 displays the number of individual interviews used in the main analysis. The 0 at the x-axis represents the day a demonstration took place, to the left of the red vertical line are the days before the demonstration, to the right are the days after.

Figure A2: Heterogeneity Analysis: By Regional Characteristics

(a) Worries about hostility towards foreigners

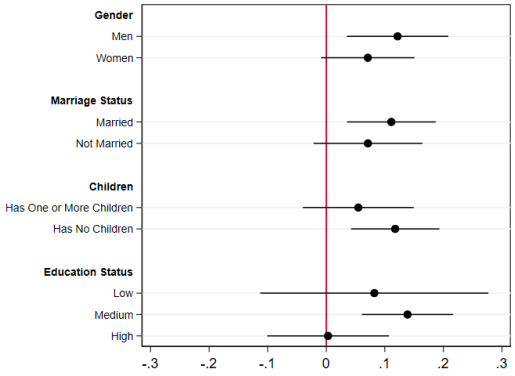
(b) Worries about Immigration



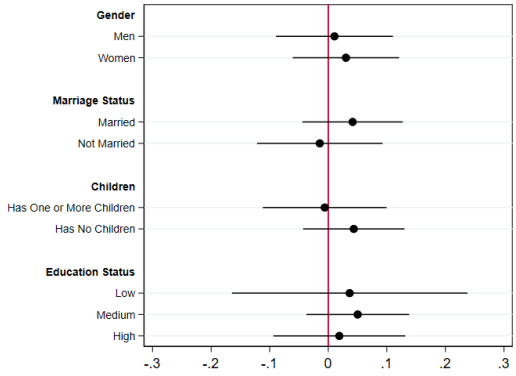
Note: Figure A2, displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in Panel (a) and worries about immigration in Panel (b), restricting the sample to the group listed on the y-axis. All regressions consider a demonstration to be relevant if it has more than 1500 participants, use a 30-day bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. 95 per cent confidence intervals are shown. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded.

Figure A3: Heterogeneity Analysis: By Individual Demographic Characteristics

(a) Worries about hostility towards foreigners



(b) Worries about Immigration



Note: Figure A3, displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in Panel (a) and worries about immigration in Panel (b), restricting the sample to the group listed on the y-axis. All regressions consider a demonstration to be relevant if it has more than 1500 participants, use a 30-day bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. 95 per cent confidence intervals are shown. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded.

Table A1: Descriptive Statistics: Outcomes

	count	mean	sd	min	max
Worries about hostility towards foreigners	10902	2.0440	0.6745	1	3
Worries about immigration	10902	1.9749	0.7615	1	3
Donate money or goods to help refugees	1662	0.2353	0.4243	0	1
Work with refugees directly	1661	0.0939	0.2918	0	1
Participate in initiatives to help refugees	1658	0.0730	0.2602	0	1
Interest in Politics	10902	2.3605	0.8130	0	4
No party preference	10902	0.5301	0.4991	0	1
Preference for a left-wing party	10902	0.2366	0.4250	0	1
Preference for a right-wing party	10902	0.1940	0.3954	0	1
Preference for an extreme right-wing party	10902	0.0119	0.1086	0	1
Worries about own health	10886	1.8008	0.6826	1	3
Worries about own economic situation	10890	1.9016	0.7032	1	3
Worries about global terrorism	5333	2.1378	0.6759	1	3

Note: Statistics of the raw outcomes used in the analysis. *p<.1; **p<.05; ***p<.01

Table A2: Testing No Anticipation: Placebo Regressions

	All demonstrations			Demonstrations with some reporting		
Placebo treatment at:	-5 days	- 7 days	-14 days	-5 days	- 7 days	-14 days
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Worries about hostility towards foreigners						
RD Estimate	-0.0453	-0.0630	-0.0338	-0.0377	-0.0593	-0.0397
	(0.0550)	(0.0502)	(0.0335)	(0.0551)	(0.0502)	(0.0338)
Observations	6949	6927	6846	6831	6809	6733
Panel B: Worries about immigration						
RD Estimate	-0.0571	-0.0179	0.0257	-0.0591	-0.0201	0.0254
	(0.0627)	(0.0550)	(0.0385)	(0.0628)	(0.0552)	(0.0389)
Observations	6949	6927	6846	6831	6809	6733

Note: Table A2 displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in Panel B and worries about immigration in Panel B. All regressions consider a demonstration to be relevant if it has more than 1500 participants, use a 15-day bandwidth, a triangular kernel, a polynomial of order one. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded. Heteroskedasticity-robust standard errors in parentheses.
*p<.1; **p<.05; ***p<.01

Table A3: Dichotomous vs. Continuous Dependent Variables

	Worries about hostility		Worries about immigration	
	Continuous	Dummy	Continuous	Dummy
	(1)	(2)	(3)	(4)
RD Estimate	0.0924***	0.0655***	0.0206	0.0243
	(0.0300)	(0.0182)	(0.0342)	(0.0212)
Baseline	2.0535	0.7990	1.9715	0.6930
Observations	10902	10902	10902	10902

Note: Table A3 displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in Panel B and worries about immigration in Panel B. Both variables are measured on a 1-3 scale, with *Baseline* indicating mean values for each outcome. All regressions consider a demonstration to be relevant if it has more than 1500 participants and use a 30-day bandwidth, a triangular kernel, a polynomial of order one. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded. Heteroskedasticity-robust standard errors in parentheses.
*p<.1; **p<.05; ***p<.01

Table A4: Robustness: Include Control Variables

	Base	Nuts II	Year	Month	Day week	Indiv. C.	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Worries about hostility towards foreigners							
RD Estimate	0.0924***	0.0925***	0.0969***	0.0939***	0.0921***	0.0922***	0.0977***
	(0.0300)	(0.0300)	(0.0299)	(0.0300)	(0.0300)	(0.0298)	(0.0297)
Baseline	2.0535	2.0535	2.0535	2.0535	2.0535	2.0535	2.0535
Observations	10902	10902	10902	10902	10902	10902	10902
Panel B: Worries about immigration							
RD Estimate	0.0206	0.0224	0.0140	0.0161	0.0196	0.0405	0.0355
	(0.0342)	(0.0342)	(0.0340)	(0.0339)	(0.0342)	(0.0325)	(0.0323)
Baseline	1.9715	1.9715	1.9715	1.9715	1.9715	1.9715	1.9715
Observations	10902	10902	10902	10902	10902	10902	10902
Nuts II	No	Yes	No	No	No	No	Yes
Year	No	No	Yes	No	No	No	Yes
Month	No	No	No	Yes	No	No	Yes
Day of week	No	No	No	No	Yes	No	Yes
Indiv. charact.	No	No	No	No	No	Yes	Yes

Note: Table A4 displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in Panel B and worries about immigration in Panel B. Both variables are measured on a 1-3 scale, with *Baseline* indicating mean values for each outcome. All regressions consider a demonstration to be relevant if it has more than 1500 participants and use a 30-day bandwidth, a triangular kernel, a polynomial of order one. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded. Heteroskedasticity-robust standard errors in parentheses.
*p<.1; **p<.05; ***p<.01

Table A5: Robustness: Use Varying Cutoffs for Large Protests

	Optimal bandwidth: 10d, 9d, 9d			Bandwidth: 30 days		
# Participants:	1200	1500	2000	1200	1500	2000
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Worries about hostility towards foreigners						
RD Estimate	0.1506***	0.1437**	0.1429***	0.0777***	0.0924***	0.1142***
	(0.0521)	(0.0644)	(0.0553)	(0.0269)	(0.0300)	(0.0312)
Baseline	2.0726	2.0535	2.0900	2.0726	2.0535	2.0900
Observations	3665	2498	2137	13460	10902	10151
Panel B: Worries about immigration						
RD Estimate	0.0891	0.0588	0.0545	0.0277	0.0206	0.0342
	(0.0734)	(0.0648)	(0.0663)	(0.0306)	(0.0342)	(0.0350)
Baseline	1.9859	1.9715	2744	1.9859	1.9715	2.0014
Observations	2874	2867	2681	13460	10902	10151

Note: Table A5 displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in Panel B and worries about immigration in Panel B. Both variables are measured on a 1-3 scale, with *Baseline* indicating mean values for each outcome. All regressions use a 30-day bandwidth, a triangular kernel, a polynomial of order one. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded. Heteroskedasticity-robust standard errors in parentheses.
*p<.1; **p<.05; ***p<.01

Table A6: Robustness: Excluding all Respondents from State of Demonstration

Bandwidth:	Optimal: 9 days	15 days	20 days	30 days
	(1)	(2)	(3)	(4)
Panel A: Worries about hostility towards foreigners				
RD Estimate	0.1497**	0.1241***	0.1140***	0.0949***
	(0.0647)	(0.0434)	(0.0373)	(0.0303)
Baseline	2.0527	2.0169	2.0417	2.0527
Observations	2457	5123	7104	10680
Panel B: Worries about immigration				
RD Estimate	0.0556	0.0560	0.0476	0.0175
	(0.0650)	(0.0495)	(0.0425)	(0.0345)
Baseline	1.9725	1.9678	1.9800	1.9725
Observations	3230	5123	7104	10680

Note: Table A6 displays the coefficients from the estimation of Equation (1) on worries about hostility towards foreigners in Panel B and worries about immigration in Panel B, with *Baseline* indicating mean values for each outcome. All regressions consider a demonstration to be relevant if it has more than 1500 participants, use a 30-day bandwidth, a triangular kernel, a polynomial of order one. The state and the day of the demonstrations are excluded. Heteroskedasticity-robust standard errors in parentheses.
*p<.1; **p<.05; ***p<.01

Table A7: Robustness: Use Placebo Worries as Outcomes

	Worry about:		
	Own health	Own economic situation	Global terrorism
	(1)	(2)	(3)
RD Estimate	-0.0273	0.0241	0.0024
	(0.0314)	(0.0323)	(0.0413)
Baseline	1.8100	1.8795	2.1250
Observations	10886	10890	5333

Note: Table A7 displays the coefficients from the estimation of Equation (1) on worries about own health, own economic situation and global terrorism. All outcome variables range from 1-3, with *Baseline* indicating mean values for each outcome. All regressions consider a demonstration to be relevant if it has more than 1500 participants, use a 30-day bandwidth, a triangular kernel, a polynomial of order one. Respondents who were interviewed in the Nuts II region of the protest and on the day of the demonstrations are excluded. Heteroskedasticity-robust standard errors in parentheses.
*p<.1; **p<.05; ***p<.01

B Media Analysis

In this section, we describe the data used for our media analysis. Table B1 presents to what extent German newspapers reported on each protest included in our study and shows which newspapers covered the protests. The tables are generated by manually looking up newspaper publications that covered the events using the platform *genios.de*, which assembles and provides articles from several hundred national, regional, and local German newspapers from 1994 until today.[38] We assembled our dataset by looking up various search terms – which are presented for each entry in the Table B1 – on the *genios* platform for the time period of two weeks before and after each protest. We then browsed through all the articles that showed up and manually collected those that reported on the protests. We used this information to construct our tables. As a note of caution: While the platform is relatively extensive, it is not fully comprehensive of all newspapers in Germany, as many smaller newspapers and online news outlets are not included. Moreover, it does not include information on other forms of news media, such as magazines, TV, radio, and, social media. Therefore, our dataset is likely not fully comprehensive of all reporting taking place in Germany. Nevertheless, we believe it to be fairly representative in terms of the salience of each protest.

Table B1 summarises when and to what extent newspapers covered each protest. Hereby, they present whether there has been any anticipation of the protests in different newspapers, which can be seen in columns "Anticipation" and "Anticipation: Sources". Generally, most protests received only limited attention from newspapers in advance. Overall, only two protests received considerable media attention in the days leading up to the protest (Berlin 2005 and Dresden 2019), with the one in Dresden being mostly covered by local newspapers. Most other protests received no attention or were only covered by local newspapers serving readers in the same district or state where the protests took place. There were a handful of protests which received at least some coverage in newspapers from outside the state. However, in most cases, there were only one or two articles and reporting newspapers were

[38] Even though each newspaper article can be purchased, in this study, we solely rely on the information given by the headline and first paragraph. This is done because we believe that this already captures most of the relevant information about each protest. Moreover, we believe that headline and the first paragraph of articles are the most salient and therefore the most impactful to readers.

usually from a neighbouring state. Therefore, apart from the first protest (Berlin 2005), we do not see any meaningful anticipation represented in newspapers in our data.

The tables also display which newspapers reported on each protest on the day of the protest and on the three days following the event. Generally, there is some variation in the reporting and, therefore the salience of events. While there has been a lot of coverage, e.g., for the protests in Berlin (2005), Chemnitz (2018a, 2018b), and Dresden (2019), some protests (e.g., Jänkendorf 2010, 2011) received relatively little attention. We use that to construct a simple indicator of coverage, which we call "Salience" and a dummy variable indicating whether a protest received a high or low level of reporting. We use this variable in our main study to show that those protests receiving a lot of reporting were driving our results.

Lastly, the data in Table B1 also displays that it usually takes some time for newspapers to report on the protests or demonstrations. Most of the protests only receive limited attention on the day of the protests, reflecting that physical newspapers are written the day before the publication. However, many newspapers only started reporting two days after the protest took place, which is the case for all protests between 2006 and 2015, indicating some lag. Interestingly, this is also displayed in our results on worries about hostility towards foreigners, as they only appear to increase around two days after protests have taken place.

Table B1: Media Analysis

Date	Participants	Location	Anticipation	Anticipation: Sources	Sources Day of Protest	Sources 1 Day after Protest	Sources 2 Days after	Sources 3 Days after	Search Terms	Salience
08.05.05	Berlin	3300	yes	**One Day Before:** Hamburger Abendblatt, Frankfurter Rundschau, taz, Süddeutsche Zeitung, Tagesspiegel, WirtschaftsWoche online, Welt, Badische Zeitung, Aachener Zeitung, Nürnberger Zeitung, Stuttgarter Nachrichten, Berliner Zeitung, Berliner Morgenpost, Kölner Stadtanzeiger, Main-Post, Berliner Kurier, Sächsische Zeitung, Südkurier, Leipziger Volkszeitung + 5 other regional newspapers. **Two Days Before:** Welt, Tagesspiegel, Spiegel online, taz, Berliner Morgenpost, Lausitzer Rundschau, WirtschaftsWoche online, Leipziger Volkszeitung, Berliner Kurier, Financial Times Deutschland, Hamburger Abendblatt. **Three Days Before:** Nürnberger Nachrichten + 6 other smaller regional newspapers. **Four Days Before:** Lausitzer Rundschau, taz, Berliner Morgenpost, Ostthüringer Zeitung, Süddeutsche Zeitung. **Five Days Before:** taz, Berliner Kurier, BZ, Leipziger Volkszeitung, Rheinische Post, Berliner Zeitung	**National:** WirtschaftsWoche online, Handelsblatt online. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** Tagesspiegel, Berliner Morgenpost, Berliner Kurier.	**National:** FAZ, taz, Financial Times Deutschland, Süddeutsche Zeitung, Welt. **Regional/Local (outside of district):** Hamburger Abendblatt, Südkurier, Aar-Bote, Main-Spitze, Idsteiner Zeitung, Wormser Zeitung, Allgemeine Zeitung Mainz-Rheinhessen, Wiesbadner Tagblatt, Sächsische Zeitung/DRS Dresden, Hamburger Morgenpost, Rhein-Zeitung, Gelnhäuser Tageblatt, Kölnische Rundschau, Usinger Anzeiger, Nürnberger Zeitung, Kreis-Anzeiger, Lauterbacher Anzeiger, Gießener Anzeiger, Trierischer Volksfreund, Frankfurter Rundschau, Saarbrücker Zeitung, Lausitzer Rundschau, Stuttgarter Nachrichten, Main-Post, Aachener Nachrichten, Rhein-Zeitung, Badische Zeitung, Saarbrücker Zeitung, Thüringer Allgemeine, Stuttgarter Zeitung, Bonner General-Anzeiger, Wiesbadener Kurier, Main-Taunus-Kurier, Ostthüringer Zeitung. **Regional/Local (inside of district):** Tagesspiegel, Berliner Zeitung, Berliner Kurier, Berliner Morgenpost.	**National:** taz, Welt, Süddeutsche Zeitung. **Regional/Local (outside of district):** Neue Westfälische, Wiesbadener Kurier, Main-Taunus-Kurier, Saarbrücker Zeitung. **Regional/Local (inside of district):** Tagesspiegel, Berliner Kurier, Berliner Morgenpost.	**National:** —. **Regional/Local (outside of district):** Leipziger Volkszeitung. **Regional/Local (inside of district):** —.	"npd demo berlin"; "npd protest berlin"; "60 Jahre Befreiungslüge – Schluß mit dem Schuldkult"	high
05.08.06	Dresden	6000	only within state	**One Day Before:** Sächsische Zeitung, Leipziger Volkszeitung, Lausitzer Rundschau. **Two Days Before**: Sächsische Zeitung, Leipziger Volkszeitung. **Before That:** Sächsische Zeitung, Leipziger Volkszeitung.	**National:** Spiegel Online. **Regional/Local (outside of district):** Ostthüringer Zeitung. **Regional/Local (inside of district):** Sächsiche Zeitung, Leipziger Volkszeitung.	**National:** —. **Regional/Local (outside of district):** Tagesspiegel. **Regional/Local (inside of district):** —.	**National:** taz. **Regional/Local (outside of district):** Frankfurter Rundschau, Berliner Zeitung, Mitteldeutsche Zeitung, Frankfurter Neue Presse, Hamburger Morgenpost. **Regional/Local (inside of district):** Lausitzer Rundschau, Sächsische Zeitung, Leipziger Volkszeitung.	**National:** —. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** Sächsiche Zeitung.	"pressefest"	high
11.07.09	Gera	3900	mostly within state	**One Day Before:** Ostthüringer Zeitung, Thüringische Landeszeitung, Leipziger Volkszeitung. **Two Days Before:** Ostthüringer Zeitung, Thüringische Landeszeitung. **Before That:** Ostthüringer Zeitung, Thüringische Landeszeitung, Leipziger Volkszeitung, Thüringer Allgemeine.	**National:** —. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** Ostthüringer Zeitung.	**National:** —. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** —.	**National:** taz, Süddeutsche Zeitung. **Regional/Local (outside of district):** Frankfurter Rundschau, Leipziger Volkszeitung, Frankfurter Neue Presse, Trierischer Volksfreund, Berliner Zeitung, Sächsische Zeitung. **Regional/Local (inside of district):** Thüringer Allgemeine, Ostthüringer Zeitung, Thüringische Landeszeitung.	**National:** —. **Regional/Local (outside of district):** Leipziger Volkszeitung. **Regional/Local (inside of district):** Ostthüringer Zeitung, Thüringische Landeszeitung	"gera protest"; "gera demo"; "rock für deutschland"	high
07.08.10	Jänkendorf	2000	mostly within state	**One Day Before:** taz, Lausitzer Rundschau. **Before That: taz.**	**National:** —. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** —.	**National:** —. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** —.	**National:** —. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** —.	**National:** —. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** —.	"pressefest"; "npd pressefest"; "jänkendorf npd"	low
01.07.11	Jänkendorf	2100	only within state	**One Day Before:** Sächsische Zeitung. **Two Days Before:** Sächsische Zeitung. **Before That:** Sächsische Zeitung.	**National:** DAPD. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** Sächsische Zeitung.	**National:** DAPD. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** Lausitzer Rundschau, Leipziger Volkszeitung, Sächsische Zeitung.	**National:** DAPD. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** —.	**National:** —. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** Lausitzer Rundschau, Sächsische Zeitung, Leipziger Volkszeitung.	"npd pressefest"	low

Table B1: Media Analysis (cont.)

Date	Location	Participants	Anticipation	Anticipation: Sources	Sources Day of Protest	Sources 1 Day after Protest	Sources 2 Days after	Sources 3 Days after	Search Terms	Salience
27.08.18	Chemnitz	6000	yes, but very short-term	**One Day Before:** Spiegel online, Welt online, Handelsblatt online, FAZ.net, Süddeutsche.de.	**National:** Süddeutsche Zeitung, Welt Online, FAZ, Spiegel Online, Handelsblatt online, Zeit online. **Regional/Local (outside of district):** Mitteldeutsche Zeitung, Bonner General-Anzeiger, Tagesspiegel, Kölnische Rundschau, Rheinische Post, Münchner Merkur, Stuttgarter Nachrichten + 55 other smaller local/regional newspapers. **Regional/Local (inside of district):** Dresdner Neueste Nachrichten, Freie Presse, Dresdner Morgenpost, Chemnitzer Morgenpost, Osterländer Volkszeitung, Oschatzer Allgemeine Zeitung, Döbelner Allgemeine Zeitung.	**National:** Süddeutsche Zeitung, Welt Online, Handelsblatt online, Zeit online, Spiegel online, FAZ.net, dw.com, taz. **Regional/Local (outside of district):** Ostthüringer Zeitung, Mitteldeutsche Zeitung, Westdeutsche Zeitung, Müncher Merkur, Märkische Allgemeine, Frankfurter Rundschau, Rheinische Post, Express, Tagesspiegel, + around 150 other (smaller) local/regional newspapers. **Regional/Local (inside of district):** Dresdner Morgenpost, Chemnitzer Morgenpost, Freie Presse, Sächsische Zeitung, Leipziger Volkszeitung, Osterländer Volkszeitung, Oschatzer Allgemeine Zeitung, Döbelner Allgemeine Zeitung, Dresdner Neueste Nachrichten.	**National:** Süddeutsche Zeitung, Welt Online, Handelsblatt online, Zeit online, Spiegel online, FAZ.net, taz, dw.com. **Regional/Local (outside of district):** Rheinische Post, Tagespiegel, Frankfurter Rundschau, Stuttgarter Nachrichten, Hamburger Morgenpost, Westdeutsche Zeitung, Südkurier, Münchner Merkur, Westfalen-Blatt + around 150 other (smaller) local/regional newspapers. **Regional/Local (inside of district):** Chemnitzer Morgenpost, Leipziger Volkszeitung, Osterländer Volkszeitung, Oschatzer Allgemeine Zeitung, Döbelner Allgemeine Zeitung, Dresdner Neueste Nachrichten, Freie Presse.	**National:** Welt online, Spiegel online, FAZ.net, Zeit online Handelsblatt online, Süddeutsche Zeitung, taz, dw.com. **Regional/Local (outside of district):** Westdeutsche Zeitung, Tagesspiegel, Frankfurter Rundschau, Thüringische Landeszeitung, Rheinische Post, Hamburger Abendblatt, Berliner Morgenpost, Stuttgarter Zeitung, Westfalen-Blatt, Südkurier + around 60 other (smaller) local/regional newspapers. **Regional/Local (inside of district):** Sächsische Zeitung, Freie Presse, Dresdner Neueste Nachrichten, Leipziger Volkszeitung, Osterländer Volkszeitung, Oschatzer Allgemeine Zeitung, Döbelner Allgemeine Zeitung.	"sicherheit für chemnitz"; "chemnitz demo"; "chemnitz protest"	high
16.11.18	Chemnitz	2500	only within state	**One Day Before:** Freie Presse.	**National:** FAZ.net, Süddeutsche.de, Welt Online, Handelsblatt online, Spiegel online, Zeit online. **Regional/Local (outside of district):** Tagesspiegel, Frankfurter Rundschau, Münchner Merkur, Westdeutsche Zeitung, Nürnberger Nachrichten, Potsdamer Neueste Nachrichten, Ruhr Nachrichten, Wolfsburger Allgemeine Zeitung, Badische Zeitung + 14 other smaller local/regional newspapers. **Regional/Local (inside of district):** Freie Presse.	**National:** Süddeutsche Zeitung, FAZ. **Regional/Local (outside of district):** Hamburger Morgenpost, Frankfurter Rundschau, Nürnberger Zeitung, Frankfurter Neue Presse, BZ, Rheinische Post, Potsdamer Neueste Nachrichten, Westfalen-Blatt, Südkurier, Berliner Zeitung, Berliner Kurier, Express + 60 other smaller local/regional newspapers. **Regional/Local (inside of district):** Freie Presse, Osterländer Volkszeitung, Oschatzer Allgemeine Zeitung, Dresdner Neueste Nachrichten, Döbelner Allgemeine Zeitung, Sächsische Zeitung, Lausitzer Rundschau, Dresdner Morgenpost, Chemnitzer Morgenpost, Leipziger Volkszeitung.	**National:** —. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** —.	**National:** —. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** Freie Presse.	"sicherheit für chemnitz"; "chemnitz demo"; "chemnitz protest"	high
20.10.19	Dresden	3000	mostly within state	**One Day Before:** Sächsische Zeitung, taz, Dresdner Neueste Nachrichten. **Two Days Before:** Dresdner Morgenpost, Leipziger Volkszeitung, Lausitzer Rundschau, Oschatzer Allgemeine Zeitung, Döbelner Allgemeine Zeitung, Osterländer Volkszeitung, Dresdner Neueste Nachrichten, Sächsische Zeitung. **Three Days Before:** Welt online, Freie Presse, Dresdner Neueste Nachrichten, Sächsische Zeitung, Leipziger Volkszeitung, Lausitzer Rundschau, Dresdner Morgenpost, Oschatzer Allgemeine Zeitung, Döbelner Allgemeine Zeitung, Osterländer Volkszeitung, taz. **Before That:** Welt online, Dresdner Neueste Nachrichten.	**National:** Spiegel online. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** Dresdner Morgenpost.	**National:** Spiegel online. **Regional/Local (outside of district):** Märkische Zeitung, Mitteldeutsche Zeitung, Tagesspiegel, Westdeutsche Zeitung, Rheinische Post, Berliner Zeitung, Südkurier + 60 other smaller local/regional newspapers. **Regional/Local (inside of district):** Freie Presse, Sächsische Zeitung, Oschatzer Allgemeine Zeitung, Osterländer Volkszeitung, Döbelner Allgemeine Zeitung, Leipziger Volkszeitung, Lausitzer Rundschau, Dresden Neueste Nachrichten, Dresdner Morgenpost.	**National:** —. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** Sächsische Zeitung, Dresden Neueste Nachrichten.	**National:** —. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** —.	"pegida"	high
28.12.19	Aue/Bad Schl	2200	no		**National:** Welt online. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** Freie Presse.	**National:** —. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):**—.	**National:** FAZ.net. **Regional/Local (outside of district):** Der Prignitzer, Schweriner Volkszeitung, Norddeutsche Neueste Nachrichten, Badische Zeitung, Ems-Zeitung, Northeimer Neueste Nachrichten + 20 other smaller local/regional newspapers. **Regional/Local (inside of district):** Chemnitzer Morgenpost, Dresdner Morgenpost, Freie Presse, Dresdner Neueste Nachrichten, Leipziger Volkszeitung, Oschatzer Allgemeine Zeitung, Osterländer Volkszeitung, Döbelner Allgemeine Zeitung, Sächsische Zeitung.	**National:** —. **Regional/Local (outside of district):** —. **Regional/Local (inside of district):** —.	"aue demo"; "aue protest"	high

Note: This table reports to what extend each of the examined protests was covered in printed newspapers in Germany and which newspapers reported on the protests. This analysis is based on data by genios.de. Column "Anticipation" describes whether there has been any reporting on the protests in the days leading up to the protest. The following column then lists the newspapers that did report on protests by day. The four columns that follow then list all the newspapers that reported on each protest in the following order: Same day reporting, one day after, two days after, and three days after protests. Hereby, three types of newspapers are distinguished in each column: national newspapers, regional or local newspapers that serve areas strictly outside of the district where the protest took place, regional or local newspapers that at least in part serve the district where the protest took place but may also serve areas outside. The last two columns first list the search terms that were used to gather the data in the previous columns and second summarize whether there has been high or low coverage or salience on the protest.

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