The Determinants of Attitudes, Preferences, Perceptions, and Identities of Migrants and Natives

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Dedicated to my parents and my late aunt Gerlinde.

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Introduction

In recent decades, immigration to European and other Western countries like the United States has been on the rise. Although migration is not a new phenomenon but a constant staple of human civilization, both the scope and the mix of source countries have seen substantial changes. While up until the mid-1900s immigrants who came to Europe and North America were usually from other European countries, they nowadays come from a much more diverse set of source countries, with increased shares stemming from the MENA region, sub-Saharan Africa, and East Asia (Kerr and Kerr, 2011). Moreover, the reasons for migration have also changed as the share of arriving people fleeing violent conflicts, civil war, and political persecution has drastically increased, particularly after the start of the Syrian Civil War in 2012.

These new patterns of immigration not only bring unique challenges for host countries related to the integration of migrants, but also affect the political landscape. Right-wing populist and extremist parties and movements have been on the rise in many Western countries as they are able to harness fears and hostility toward migrants. Thereby, they have been able to raise the salience of their positions and see increased vote shares in elections (Rodrik, 2018).

One country that has been at the crosshairs of both of these dynamics is Germany. It has seen a rapid transformation in terms of immigration in recent years. Even though the country experienced substantial immigration from Southern Europe, former Yugoslavia, and Turkey due to guest worker programs starting in the mid-1950s, leading politicians for a long time refused to consider Germany to be an immigration country (Hell, 2005). This position also contributed to a reluctance to integrate those migrants, their family members, and their children even after it became clear that they would stay permanently, contributing to a large gap in labor market outcomes between migrants and natives (Brell et al., 2020).

Yet, in recent years, Germany has become much more open to immigration. For instance, it has received millions of refugees from Syria, Iraq, Afghanistan, and other countries during the European refugee crisis (ERC) between 2014 and 2015 and from Ukraine starting in 2022. Moreover, as the country is already affected by demographic change, it has also intensified its efforts to attract foreign talent from all over the world by liberalizing its migration policy. These dynamics have led to consistently large migration surpluses in recent years, leading Germany to have one of the highest shares of foreign born residents in Europe (OECD, 2023).

However, Germany has also experienced a substantial political realignment. For many decades after the second World War, the system was dominated by two major party blocks, namely the social democratic *SPD* and center-right *CDU/CSU*, with the smaller liberal *FDP* usually playing kingmaker. While the party system fragmented somewhat with the emergence of the Green Party, and later the left-wing *PDS/Die Linke*, far-right parties were marginalized on the national level for many decades. However, this changed in 2017, as the *Alternative für Deutschland (AfD)* was the first right-wing populist party to enter the *Bundestag* (German national parliament) after 1949 (Arzheimer and Berning, 2019). Originally founded as a Euro-skeptic party in 2013, it turned increasingly right-wing and anti-immigration, particularly from 2015 onwards. While consistently polling below five percent before the ERC, it thereafter became an outlet for people who were discontent with Germany's refugee policy, leading to a sharp uptick in support (Arzheimer and Berning, 2019).

This thesis deals with several aspects concerning immigration, far-right movements, and their interaction in Germany. In each chapter, this is reflected to a varying degree. Chapter 1 is mostly concerned with immigration, studying the effects of the inflow of hundreds of thousands asylum seekers on the ethnic identity of already-resident migrants. However, it also relates to the strengthening of far-right parties, as additional analyses uncover that some migrants became more attracted to them following the ERC. Chapter 2 mostly relates to right-wing extremist movements, as we study how demonstrations organized by the far-right affect public attitudes toward migration. Yet again, connections to immigration are present, as many of these protesters have strongly anti-immigrant motivations. Chapter 3 then deals with a factor which influences worries about immigration and support for far-right parties, namely bitterness (Poutvaara and Steinhardt, 2018), as we study how unemployment affects this outcome. In the fourth and final chapter, we look at perceived discrimination of immigrants in Germany, an outcome which is also plausibly related to the existence of far-right sentiments and actors.

Apart from these thematic relations, the chapters also share further similarities. First, all of them are based on empirical research that (at least partly) tries to elicit causal effects. This means that approaches share methodological similarities, with most of them (apart from Chapter 2) relying on difference-in-differences estimations. Moreover, in all papers some source of exogenous variation is exploited to reach causal effects, whether by making use of the placement of asylum seekers across German counties, the occurrence of local far-right protests, being affected by involuntary unemployment, or legal changes through citizenship reforms. Second, the empirical research is always based on panel data, more specifically data by the German socio-economic panel (SOEP, Goebel et al. (2019)). This data has the advantage of not only being extensive, as it consists of 30,000 annual interviews, but also being longitudinal. This means that individuals are interviewed repeatedly over time, allowing us to include individual fixed effects to control for time-constant heterogeneity. Lastly, the chapters also look at similar types of dependent variables, which are concerned with attitudes, preferences, perceptions, and identities of people. These outcomes are important to study as they may motivate economic, social, and political behaviors of individuals.

In the following, I will briefly summarize each chapter.

Chapter 1: Belonging or Estrangement – the European Refugee Crisis and its Effects on Immigrant Identity

In the first chapter, I examine the impact of the 2015 European refugee crisis on the ethnic identity of resident migrants in Germany. More specifically I study how the inflows of asylum seekers influenced migrants' attachment to Germany and to their home countries. I make use of the institutional setting in Germany, whereby refugees are allocated to different counties within Germany by state authorities without being able to choose their locations themselves. Instead, these decisions are usually rules-based, and differ across states. I then exploit the variation arising from counties being differently affected by asylum seeker inflows and study to what an extent my outcomes of interest are affected.

Thereby, I find that higher inflows of asylum seekers led to an increase in migrants' home country attachment, but no changes in their perceived belonging to Germany. Furthermore, I uncover substantial heterogeneities by origin region: While inflows led to decreased attachment to Germany and increased attachment to their home countries among Eastern Europeans, the opposite appears to be the case for Western migrants – even though effects are statistically insignificant for the latter. Moreover, migrants from Turkey and the MENA region also seem differently affected as higher inflows only led to a decrease in perceived belonging to Germany.

To examine the causes of these differing effects, I build on intergroup threat theory (Stephan et al., 2008, 2015). Hereby, I argue that migrants perceived threats from two directions, first, from refugees themselves, and, second, from hostile backlash by natives targeting all migrants. To investigate this further, I look at three factors: worries about xenophobia, experiences of discrimination, and consumption of foreign news media. Thereby, I find that higher refugee inflows led to increased worries about xenophobia. Moreover, effects were driven by migrants who previously experienced discrimination and those who consumed media from their countries of origin. These results indicate that migrants likely feared nativist backlash due to refugee inflows. However, I also argue that Eastern Europeans felt particularly threatened by refugees, as their home country media was often much more hostile towards refugees than Western media (Georgiou and Zaborowski, 2017).

In a last extension, I look at whether inflows also impacted political preferences, finding evidence for political polarization. While Western migrants started to lean more strongly towards moderately left-wing parties, Eastern European migrants became more favorable towards the AfD, and migrants from Turkey and the MENA region had increased preference for the left-wing *Die Linke*.

Chapter 2: Local Far-Right Demonstrations and Nationwide Public Attitudes toward Migration

The second chapter, which is co-authored by Teresa Freitas Monteiro, studies the short-term effects of far-right demonstrations on attitudes toward migration among natives in Germany, looking at their concerns about xenophobia and worries about immigration.

Thereby, we test two opposing theories: On one side, many studies have shown protests to be able to rally support in their favor (Madestam et al., 2013; Larreboure and Gonzalez, 2021). By being present on the streets, protesters may be able to mobilize and persuade bystanders, raising support for their agenda. This can happen if issues and demands of the protesters might have strong resonance or mobilise cultural grievances among the public. They can also make certain issues more salient and push them to the agenda. However, on the other hand, they may be perceived as a threat to public order, as – in our case – they could make xenophobia more publicly visible or even threaten bystanders. The existence and salience of xenophobic groups may be increased, and protests may backfire, lowering support for their causes.

To test these theories we perform a regression discontinuity design approach for our two outcomes of interest. Thereby we compare respondents interviewed in the days leading up to protests with individuals surveyed thereafter. Overall, we find that local protests led to an immediate short-term increase in concerns about xenophobia, but no changes in worries about immigration – indicating that protests backfired as they were perceived as a threat.

Looking more closely at the mechanisms behind the effects, we find they were mainly driven by media reporting, as only protests that were extensively covered in the news saw significant effects. Moreover, we also find considerable heterogeneities, suggesting that protests had polarizing effects. Specifically, we show that while worries about xenophobia increased both in regions where left-leaning and center-right parties were successful, concerns about immigration decreased in the former, but increased in the latter. Moreover, we also show that effects were mainly driven by politically left-leaning individuals.

Lastly, we also show that people became more politically interested in response to protests, mainly benefiting left-wing parties, and were more likely to wish to donate money to help refugees.

Chapter 3: The Bitter Taste of Unemployment – Evidence from Layoffs in Germany

In Chapter 3, which is co-authored by Max Steinhardt, we examine the impact of unemployment on bitterness, which describes a feeling of not having achieved what one deserves compared to others. This concept, which is distinct from other related psychological measures like life satisfaction and reciprocity, has been shown to be positively related to worries about immigration and higher support for far-right parties (Poutvaara and Steinhardt, 2018).

First, we illustrate descriptively that unemployed people appear much more bitter than those employed or out of the labor force. Thereafter, we perform pooled OLS regressions to determine whether this relation also holds when we condition it on a wide set of demographic and socioeconomic control variables as well as year and state fixed effects. Thereby, we find a very strong and highly significant positive relationship, which also holds when we include individual fixed effects that capture time-constant individual heterogeneity.

However, as this initial analysis is unable to control for all sources of endogeneity, we continue by estimating the causal effect of unemployment. To do so, we exploit variation from plant closures and firm layoffs. More specifically, we compare respondents who lost their jobs due to plant closures or dismissals and registered as unemployed in-between interviews (treatment group) with respondents who remained employed (control group). To control for both time-variant observable and time-invariant unobservable variation, we combine matching based on entropy balancing (Hainmueller, 2012) with difference-in-differences estimation. Our results show that unemployment leads to a substantial and significant increase in bitterness of one quarter of a standard deviation.

Further analyses reveal that job loss, unemployment, and unemployment duration all have separate positive and significant effects on bitterness. Moreover, we find evidence that effects persist over time for those who remain unemployed for over one year.

Chapter 4: Feeling Equal before the Law? The Impact of Naturalization and Legal Status on Perceived Discrimination

The last chapter of this dissertation, which is co-authored by Adriana Cardozo Silva, studies the effects of a change in legal status on migrants' perceived discrimination, which describes the impression that one has been treated unfairly due to some personal characteristic or group membership (Kaiser and Major, 2006).

In this study, we mostly focus on naturalization – the most impactful change in legal status – and estimate its effects on perceived discrimination of migrants in Germany. Hereby, we follow two main approaches. First, to elicit the direct effects of naturalization, we estimate a fixed effects model following Steinhardt (2012). As this method cannot fully account for all potential sources of endogeneity, we thereafter estimate a separate approach which makes use of two citizenship reforms in Germany in 1991 and 2000. These two reforms led to variation in residency requirements to be eligible to naturalize along two dimensions: age at and year of arrival. We exploit this exogenous variation to estimate intent-to-treat effects of a change in waiting periods on perceived discrimination.

Both approaches lead to similar findings in direction but not always in significance. Using our second and preferred approach, we find that a reduction in waiting periods of seven years – essentially the drop in residency requirements brought about by the German citizenship reform in 2000 for older migrants – led to a reduction in perceived discrimination of around 13 percent of a standard deviation. However, looking at heterogeneities, we find that these effects are largely driven by men and Eastern European migrants, while effects for other groups are insignificant.

Thereafter, we test whether there are similar patterns in a different setting, namely the EU enlargement between 2004 and 2013, in which mostly Eastern European countries started to become part of the EU. This offers us with a quasi-experiment as migrants from EU accession countries experienced an upgrade in their legal status in Germany, because they started to become covered by EU law (Tridimas, 2006). We leverage this variation to show that these citizens report significantly less discrimination after EU accession than non-EU immigrants.

Chapter 1

Belonging or Estrangement – the European Refugee Crisis and its Effects on Immigrant Identity

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1.1 Introduction

In the last decades, immigration has increasingly become a politically salient and hotly discussed topic in many Western countries. Not only has it galvanized voters in the 2016 US presidential election and the UK Brexit referendum, but it has also fueled populist movements in virtually all European countries (Inglehart and Norris 2016). One of the most impactful events in recent times was the European Refugee Crisis (ERC) in 2015. It polarized the political landscape in many European countries, with concern about the safety and welfare of refugees on one side and fear and worry about them on the other (Hangartner et al. 2018, Rodrik 2020). These dynamics were particularly pronounced in Germany, where over the span of only a few months close to a million asylum seekers arrived. At first, many Germans were accommodating, with a broad spectrum of society helping to provide immediate aid and support for the incoming. Yet over time, critical voices grew louder, leading to vocal anti-immigrant movements and culminating in far-right *Alternative für Deutschland* (AfD) entering into the *Bundestag* (German national parliament) in 2017 (Arzheimer and Berning 2019).

While public and scientific discourse was often mainly focused on either the integration of refugees or the concerns of the German population (Gehrsitz and Ungerer 2017, Aksoy et al. 2020), little attention has been paid to the reaction of migrants already living in Germany. Yet, migrants are an important and growing group in Germany, who may be distinctly affected by the newly arrived refugees, as they compete for the similar jobs and resources in society. Moreover, many European countries still struggle to integrate parts of their immigrant community, resulting in far worse labor market outcomes for migrants compared to natives (e.g., Dustmann et al. 2013). While researchers have studied which factors affect migrant integration, increased attention has been paid to the importance of identity in recent years, and more specifically, the effects of ethnic identity[1] (e.g., Battu and Zenou 2010, Casey and Dustmann 2010, Manning and Roy 2010). Adding to this literature, this study examines the effects the 2015 ERC had on already resident migrants, looking at how it affected their attachment to Germany and their original home country, respectively.

Theoretically, this study builds on a social psychological framework called intergroup threat theory (Stephan et al. 2008, Stephan et al. 2015), which states that members of ingroups can perceive threats from outgroups and may change their behavior accordingly. In the context of the ERC, migrants may perceive threats from two groups: either from refugees themselves or from natives, who in response to refugee inflows engage in xenophobic or discriminatory behavior against all migrants. Such threats can be realistic (affecting the ingroup's resources or welfare) or symbolic (affecting self-image, values and belief systems). Among other consequences, Stephan et al. (2015) argue that perceiving threats against the group can lead ingroup members to increase their group cohesion, which in the case of this study would translate to increased home country attachment.

[1]In the economics literature, sometimes the terms national identity or social identity are also used while sociologists and social psychologists often use more general terms such as group identification or belonging.

To test this theory, I examine the 2015 European Refugee Crisis, which offers a quasi-natural experiment in the form of an arguably exogenous migration shock. Starting on 5 September 2015, Germany allowed refugees stuck in other European countries to cross its border, leading to a sudden and very strong increase of asylum seekers in Germany of approximately 890,000 refugees until the end of 2015 (BAMF 2016b). After arrival, refugees were unable to choose their locations themselves, but were placed by the authorities to individual states, counties, and municipalities. The distribution to different states (*Bundesländer*) followed a pre-determined quota called the "Königstein Key" (*Königsteiner Schlüssel*), which is based on state population and tax revenue (Stips and Kis-Katos 2020). Within states, refugees were placed to counties according to rules set by each state. For example, nine of 16 states allocated refugees according to the population size of counties, while others had fixed and previously agreed upon quotas (Geis and Orth 2016). Importantly, residents in neighborhoods, where refugee reception facilities were established, had no influence on the allocation of asylum seekers. This was particularly true for migrants, who – by virtue of being a minority in society and oftentimes ineligible to vote – have little voice in these decisions.

In this study, I exploit the plausibly exogenous variation that arose from the placement of asylum seekers during the ERC, an approach which has previously been used in studies like Tomberg et al. (2021) and Torres (2022). Thereby, I am interested in how the inflow of a large outgroup, the refugees, affected the ethnic identity of resident migrants, which in the context of this study is the ingroup. In other words, I ask: How did the change in the local refugee share impact migrants' attachment to Germany and their connection to their or their parents' home country? To arrive at arguably causal estimates, I employ a variant of a difference-in-differences approach, regressing the two mentioned measures of ethnic identity on an interaction of the change in refugees over population per county (*Kreis*) between 2014 and 2015.

In my estimations, I use individual-level data from the German Socio-Economic Panel (SOEP), a representative longitudinal household survey, that provides time-varying information on migrants' identity measures. Due to its panel structure, it allows me to include individual fixed effects, which capture any time-constant differences across individuals. Attachment to Germany is measured through the question, to what extent migrants feel German, while the other outcome is captured by asking how connected migrants feel to their own or their parents' home country. For the main explanatory variable, I use administrative end-of-year data on the recipients of asylum seekers' benefits per county, which reflect actual refugee inflows very well.

To argue that the ERC offers an arguably exogenous migration shock requires the explanatory variable to be unaffected by the outcomes of interest and any confounding factor in the error term. There are three potential threats to the identification. First, migrants' identification with their home or host country itself could affect placement more or less directly. Testing whether my outcome variables influenced the placement of refugees directly, I find no effect of migrants' ethnic identity on refugee allocation. Second, refugees could be placed where immigrants generally integrate faster socially and economically. If better integrated migrants identify more with Germany and less with their home country, this could bias results. Nevertheless, I show that the placement of refugees per county was independent of a host of integration outcomes of migrants, including social, economic, and demographic measures. Lastly, there could be other confounding factors that are not controlled for in my main estimation equation. To assess this possibility, I include a myriad of further controls into my regression.

Overall, I find that increases in refugee concentration led to an increase in migrants' attachment to their home countries, while having no significant impact on their identification with Germany, on aggregate. Results imply that a mean increase in counties' asylum seeker share of .77 percentage points increased the number of respondents identifying strongly or very strongly with their home country by 2.18 percentage points. Using an event-study analysis, I can also show that there does not appear to be a pre-trend, suggesting that respondents in counties with higher inflows were not on a different trajectory than respondents in other counties.

My results are robust to a range of different specifications and possible objections, most importantly the regression method, scaling of the dependent variable, the exclusion of outliers, and sample selection, but also omitted variable bias, clustering of standard errors, and different specifications of the treatment variable.

In further analyses, I find substantial heterogeneities in these effects along migrants' country of origin. On one side, migrants from Eastern European countries, particularly those who are not ethnic Germans (*Aussiedler*), became significantly less attached to Germany and more attached to their home countries the more asylum seekers were placed in their county. On the other side, I observe opposite (albeit insignificant) effects for Western migrants, who increased their attachment to Germany while decreasing the connection to their home countries. Lastly, migrants from Turkey, the Middle East, and North Africa (TMENA) became less attached to Germany, with no change in their home country attachment in response to the treatment.

In this study, I argue that these results indicate that migrants perceived a threat from refugee placement, which differed between migrant groups. Examining the underlying causes of migrants' threat perceptions, I find that migrants' concerns about crime, job security, and immigration did not increase in counties that housed more asylum seekers. Rather, I identify two potential factors: First, worries about xenophobia increased in areas with more refugees and treatment effects only appear for migrants who had previous experiences of discrimination. Both indicate that migrants may have perceived an indirect threat from refugee placement, not coming from refugees but as a potential backlash from natives. Second, I find that only immigrants, who consumed foreign-language media, experienced significant treatment effects. On one side, Eastern European media, including social media (Sablina, 2021), was often much more critical of Germany's handling and more hostile towards refugees than Western media (Georgiou and Zaborowski 2017). This likely contributed to migrants feeling directly threatened by refugees. Media from TMENA countries, on the other side, was more empathetic towards refugees but often invoked narratives of state control (Sert and Danış 2021). This likely stood in contrast with experiences of viewers in Germany, who may have feared that the German government would be unable to protect them from xenophobia and discrimination.

In a last extension, I check whether the ERC also had an impact on other outcomes that could be associated with migrants' ethnic identity, as a number of studies have stressed its importance on the labor market (e.g., Battu and Zenou 2010), in school (e.g., Baysu et al. 2011) but also in shaping political preferences and voting behavior (Teney et al. 2010, Baysu and Swyngedouw 2020, Mayer et al. 2023). While the placement of refugees does not appear to have already affected labor market and educational outcomes of migrants, it seems to have had an impact on political preferences. First, migrants became more interested in politics in areas with higher relative inflows. Moreover, preferences for political parties increased in response to the treatment, too, with Western migrants leaning more strongly towards moderately left-wing parties, while Eastern European and TMENA migrants increasingly preferred the far right AfD and the socialist *Die Linke* (The Left), respectively, indicating some kind of political polarization. This shift in preferences would be in line with each migrant group's threat perception: Eastern European migrants particularly felt threatened by refugees themselves, which motivated some to favor the anti-refugee party. TMENA migrants felt more threatened by xenophobic actions from natives, therefore some started supporting a party more vocally opposed to anti-Muslim xenophobia.

This study contributes, first, to the evolving literature on identity (Akerlof and Kranton 2000, Shayo 2009) and, more specifically, ethnic identification in economics (Constant and Zimmermann 2008, Georgiadis and Manning 2013, Bisin et al. 2016). While group identification and measures of belonging have already been studied intensively in sociology and social psychology (Berry 1997, Ellemers et al. 2002), economists have more recently become interested in this topic. Although there are some extant studies that descriptively investigate the determinants of ethnic identity (e.g., Dustmann 1996, Manning and Roy 2010), we know only little about the causal factors determining why some migrants identify more or less with their home and host countries. This study tries to at least partly ameliorate that by exploiting the quasi-random setting of the ERC in Germany, examining whether the large-scale refugee inflows causally affected the ethnic identity of existing migrants in the short term. Second, this study adds to the literature on ethnic identity and the assimilation of migrants. Although its exact effects often depend on the specific circumstances, previous studies have found that ethnic identity affects labor market outcomes, with some finding evidence that host country identification relates positively to labor market outcomes (e.g., Nekby and Rödin 2010, Piracha et al. 2021), while the opposite is the case for home country attachment (e.g., Battu and Zenou 2010, Bisin et al. 2011, Monscheuer 2020).[2] Furthermore, it is transmitted across generations (Casey and Dustmann 2010), affecting second-generation educational, social, and labor market outcomes (Schüller 2015, Monscheuer 2020). As refugee inflows of the ERC impacted the ethnic identity of migrants, this may in extension influence labor market outcomes and overall assimilation in the long run. Third, my study adds to existing studies that exploit the dispersal policy of asylum seekers in Germany more generally (Glitz 2012, Jaschke et al. 2022), and more specifically for the case of the ERC[3], being one of the first to focus on the effects on already resident immigrants.[4] Fourthly, my study

[2]Newer studies on Australia (Piracha et al. 2021), Canada (Islam and Raschky 2015), China (Cai and Zimmermann 2020), Denmark (Gorinas 2014) and Italy (Carillo et al. 2021), that partly use instrumental variables to deal with endogeneity, also do not come up with clear patterns, either finding slightly positive (negative) or negligible effects of host country (home country) identification.

[3]Examples include the effects the ERC had on crime (Dehos 2017, Huang and Kvasnicka 2019), rental prices (Kürschner Rauck and Kvasnicka 2018), hate crimes against foreigners (Entorf and Lange 2019), attitudes toward immigration (Sola 2018, Torres 2022), support for right-wing parties (Schaub et al. 2021), and electoral outcomes (Gehrsitz and Ungerer 2017, Bredtmann 2022).

[4]Thematically, the most similar study to this one is Deole and Huang (2020), who, among other outcomes, study how the ERC affected the economic and social assimilation of immigrants from Turkey, the Middle East, and North Africa (TMENA). Using data from the German Socio-Economic Panel, they find no change in migrant identification with Germany, while the connection to their home country increased.

adds to the literature on the interactions between different minority groups in society. While some studies have looked at potentials of inter-group solidarity (Glasford and Calcagno 2012), others have focused on sources of tension, particularly between African Americans and immigrant groups in the US (Gay 2006, Fouka et al. 2022), but also different minority groups in Europe (Hindriks et al. 2014, Leidig 2019). Lastly, my findings also contribute to the literature on the determinants of immigrants' and other minorities' political preferences (e.g., Dancygier and Saunders 2006, Abrajano and Singh 2009, Bergh and Bjørklund 2011), including the literature on migrant groups preferring right-wing and far-right parties (Wüst 2004, Hansen and Olsen 2020).

The remaining parts of the paper are structured as follows. In section 1.2, I lay out some theoretical considerations motivated by intergroup threat theory, followed up by an overview of the ERC and the institutional background in Germany in section 1.3. I then describe the data used, introduce my methodological approach and provide evidence for the exogeneity of my empirical strategy (section 1.4). Section 1.5 presents my main results and shows that they are robust to a number of possible objections. In this section, I also examine the roles of several migrant concerns, past experiences of discrimination, and media consumption, and look at whether further outcomes were affected by the ERC. In the final section, I conclude my study.

1.2 Theoretical Considerations

To understand the mechanisms of the ERC affecting resident migrants in Germany, this study builds on the intergroup threat theory (Stephan et al., 2015). This framework argues from the basis that people, wherever they are, sort into groups, as this gives them a shared identity, self-esteem and social support as well as structure through shared values and norms (Tajfel and Turner 2004). Members of ingroups have a preference for their own group, as favoring it reinforces the positive benefits they receive from group membership. Yet, while this ingroup preference does not necessarily translate into hostility towards members of the outgroup (Brewer, 1999), it is frequently the case that ingroups and outgroups compete, particularly if they pursue the same outcomes, such as resources or power, or if they perceive threat (Amira et al. 2021, Jardina 2021).

In intergroup threat theory, groups can perceive two kinds of threat: realistic and symbolic threat. Realistic threats concern groups' material interests, i.e., their resources and position in society. This means that they might encounter more competition on the labor market, might be the victims of crime or that they might have less voice in political decision-making. In contrast, symbolic threats are less tangible as they pertain to groups' values, norms, religion, and esteem in society. In this case, groups' might fear that their way of life is threatened. Importantly, both types of threat need only be perceived, meaning they need not be carried out, to already lead to a reaction by ingroups. While the type of reaction to threats can differ depending on the circumstances, ranging from increased prejudice (Velasco González et al., 2008) to outright hostility or even dehumanization, ingroups may become more cohesive as members' attitudes toward it become more favorable (Stephan et al., 2015, p.270-271). While, theoretically, both realistic and symbolic threat can lead to this reaction, more studies have focused on the latter, showing that symbolic threat can lead to ingroup-affirming behavior (e.g., Wohl et al. 2010, Matthews and Levin 2012).

In the context of the ERC, I argue that some migrants perceived increasing refugee shares in their vicinity as a threat to their ingroup. On one hand, this threat comes from refugees directly. Particularly in the beginning, refugees received governmental support and resources in the form of housing, public utilities and transfers, which otherwise may have benefited migrants. Later, refugees might compete for the same jobs as migrants, especially lower-skilled ones. Moreover, migrants may fear rising crime or even terrorist events. These threats would be realistic. Yet, refugees may also pose a symbolic threat to migrants, as they come from different cultures, practice other religions and hold values that may differ from some migrant groups, in particular non-Muslim migrants. On the other hand, there may be an indirect threat, as accommodating asylum seekers can spur anti-immigrant hostilities from natives. These can take the form of protests, but also discrimination or even violence. While refugees might be primarily targeted by these hostilities, migrants would likely become victims of such aggressions themselves, which may pose a realistic and symbolic threat to migrants.

In consequence, both, the threat from refugees and the threat from natives, likely have an impact on migrants' group cohesion, leading to an increase in home country attachment. In the case of threat from natives, we may also observe decreased belonging to Germany, as migrants feel pushed out of the German society.

1.3 Institutional Background

1.3.1 Historical Background

The 2015 European Refugee Crisis was the culmination of several dynamics preceding this event. First, the Syrian Civil War starting in 2012 led to the spread of millions of Syrians fleeing war, hunger, and persecution, with parts of them heading to Europe. There, they were joined by other migrant groups, fleeing political and other forms of persecution, oppression, and lack of economic opportunity, from countries such as Afghanistan, Iraq, but also from Balkan countries such as Albania and Kosovo, African countries like Eritrea, and other Asian countries. Lastly, the European Union's system of registering and distributing asylum seekers across member states was already dysfunctional and subject of heated debate (Niemann and Zaun 2018). Unprepared and overwhelmed, the EU was unable to properly manage this groundswell of refugees, leading to thousands of people being stranded in countries such as Serbia and Hungary.

Faced with this situation, the German government headed by Chancellor Angela Merkel decided to suspend Dublin regulations and allow refugees stuck in Budapest to cross the border to Germany on 5 September 2015 (Herbert and Schönhagen, 2020). This led to the *de facto* removal of border controls and resulted in the arrival of hundreds of thousands more refugees seeking protection and opportunity in Germany. At the end of the year, a total of approximately 890,000 asylum seekers were received in Germany according to federal authorities (BAMF 2016b), with the vast majority arriving in the last few months of 2015 (BAMF 2015a).

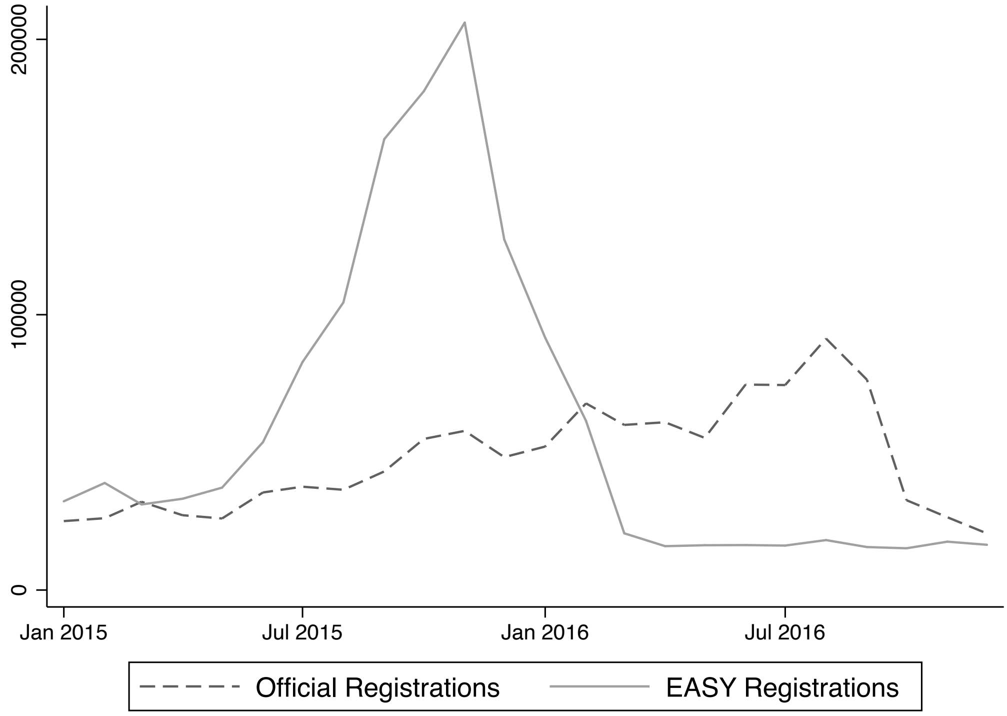
In the following, I briefly describe how asylum seekers are registered and distributed across counties in Germany.[5]

1.3.2 Registration of Asylum Seekers

Generally, when refugees arrive in Germany, they have to notify state authorities, which can happen either directly while crossing the border or later on at several state institutions, e.g., a refugee reception center or a local police station. There, they are initially recorded using the so-called EASY system (*"Erstverteilung der Asylbegehrenden"*). This declaration initiates the asylum process, whereby asylum seekers are provided with a proof of arrival, entitling them to reside in Germany and

[5] More extensive overviews of these processes can be found, e.g., in Geis and Orth (2016) or Huang and Kvasnicka (2019).

Figure 1.1 Registration of Asylum Seekers in Germany



Note: Official asylum seeker registration data over time compared to data captured through the EASY system ("Erstverteilung der Asylbegehrenden"). Data by BAMF (2015b) and BAMF (2016a).

receive asylum seekers' benefits. Later, refugees have to officially register and apply for asylum at the Federal Office for Migration and Refugees (*Bundesamt für Migration and Flüchtlinge*, BAMF). Under normal circumstances, the official registration mostly occurs relatively quickly. However, because of the large number of arriving refugees during the ERC, authorities struggled to register asylum seekers in due time, leading to delays of weeks and sometimes even months.

This discrepancy in arrival and registration can be seen in Figure 1.1. Official registrations (dashed line) suggest that refugees arrived gradually over time, barely exceeding 100,000 within a month and maintaining their inflow until the fall of 2016. In contrast, the EASY registration data (solid line) show that the inflow of refugees was actually much more sudden, reaching its peak in late 2015, and subsiding quickly thereafter. Comparing both lines clearly shows that official statistics severely lagged the EASY statistics, making its use inappropriate for my analysis.[6]

[6]It should be noted, that I am not working with the monthly EASY data, but with an end-of-year registry of recipients of asylum seekers' benefits. Nevertheless, the end-of-year registry builds upon the EASY data and reflects the sudden inflow of refugees presented in Figure 1.1 well.

1.3.3 Distribution of Asylum Seekers

After receiving their proofs of arrival, refugees are distributed to one of the 16 states in Germany according to the "Königstein Key" (*Königsteiner Schlüssel*). This is a predetermined quota based on tax revenues (with a weight of two thirds) and population size (weighted by one third) of each state (Geis and Orth 2016), which is supposed to ensure a fair and proportional allocation of asylum seekers across Germany.[7] While under normal circumstances, the quota is followed relatively closely, over time, as refugee inflows strained existing capacities of some states, the availability of vacant accommodations became an increasingly important concern during the ERC (Gehrsitz and Ungerer 2017).

After being allocated to a state, refugees were then further distributed to counties (*Kreise*) within the state according to state-specific rules. Each of the sixteen German states pursues its own allocation regime, which can range from a 1-Stage to a 3-Stage process. In a 1-Stage allocation process, asylum seekers are directly placed in accommodations by the state. In 2-Stage processes, they are usually first placed in a state reception center and then moved to each county or municipality. Lastly, in 3-Stage allocation regimes, refugees typically are moved from central reception facilities to a *Regierungsbezirk* (governmental district) and then moved to each county or municipality.[8] Table 1.A1 in the appendix gives an overview over the allocation regime in each state.

Moreover, the table also shows that each state followed its own within-state distribution quota of asylum seekers. In 2015, for nine of 16 states, decisions were based on county population size (Geis and Orth 2016)[9], meaning that counties received asylum seekers proportional to the number of residents. In other states, the distribution of asylum seekers mostly followed previously fixed, permanent quotas or took other factors like area size into account (Stips and Kis-Katos 2020).

Upon arrival in the allotted reception center, refugees had to stay there for at least six weeks and up to three months (later six months) (Stips and Kis-Katos 2020). Moreover, they also had to remain within a designated area, often within the borders of the county itself, in the first three months of the asylum process, severely restricting

[7] To determine how many refugees are allocated to each state, data collected through the initial asylum seeker declarations are used.

[8] Rules and mechanisms can differ depending on the state.

[9] Technically, in one of the nine states, Brandenburg, not only population size, but also the share of employed people subject to social security contributions by county influenced refugee allocation.

their freedom of movement. Restrictions were eventually lifted when an asylum seeker was permanently allowed, tolerated or permitted to stay in Germany for three months. However, residence restrictions could be placed on refugees relying on government aid, which was very often the case.

Figure 1.A2 in the appendix gives an impression of the spatial distribution of refugees in Germany. It depicts the share of asylum seekers in the 401 counties in Germany at the end of 2015.[10] Counties are sorted by decile, with darker colors indicating higher relative inflows. Generally, there is variation in the distribution of asylum seekers across counties, yet counties with higher refugee shares in excess of 1.5 percent of the county population remained the exception.

1.4 Data & Methodology

1.4.1 Data

In this study, I primarily work with two sources of data. First, to measure local refugee shares, I use administrative data on the recipients of asylum seekers' benefits. Second, to capture migrants' ethnic identity, I employ high-quality panel data from the German Socio-Economic Panel (SOEP).

I work with data on the recipients of asylum seekers' benefits due to severe distortions of official statistics on refugee numbers, as outlined in section 1.3.2. The statistics on asylum seekers' benefits are publicly available and provided by the statistical offices of the *Bundesländer* (states of Germany).[11] The data sets include the total number of asylum seekers in every German county, as well as their gender and age composition. In this study, I mainly use end-of-year data for 2014 and 2015 to construct a measure of change in the share of asylum seekers divided by total county population between both years. This is a conservative measure to gauge the actual inflows of asylum seekers during the ERC, as it eliminates the risk of double counting[12] at the expense of undercounting the actual refugee inflows.

[10]This is the number of counties in Germany at the end of 2018. There have been numerous territorial reforms of counties over time, with the last major one happening in 2021 in Thuringia, merging two counties. Therefore, there are 400 counties in Germany, as of 2023.

[11]The data can be accessed publicly and free of charge via *regionalstatistik.de*.

[12]In 2014, Asylum seekers could receive asylum seekers' benefits for up to 48 months (Wendel, 2014). After a reform in early 2015, this period has been shortened to 18 months. Therefore, it is possible for asylum seekers who arrived in Germany in 2014 to be counted in the total in 2015.

Table 1.1 Summary Statistics of Treatment Variable

	Mean	SD	Min	P25	Median	P75	Max	N	Counties
Ref_share 2015	1.20	0.73	0.02	0.94	1.11	1.33	11.05	5384	261
Ref_share 2014	0.43	0.16	0.05	0.34	0.42	0.50	2.67	5384	261
Δ Ref_share	0.77	0.69	-0.44	0.54	0.65	0.84	9.23	5384	261

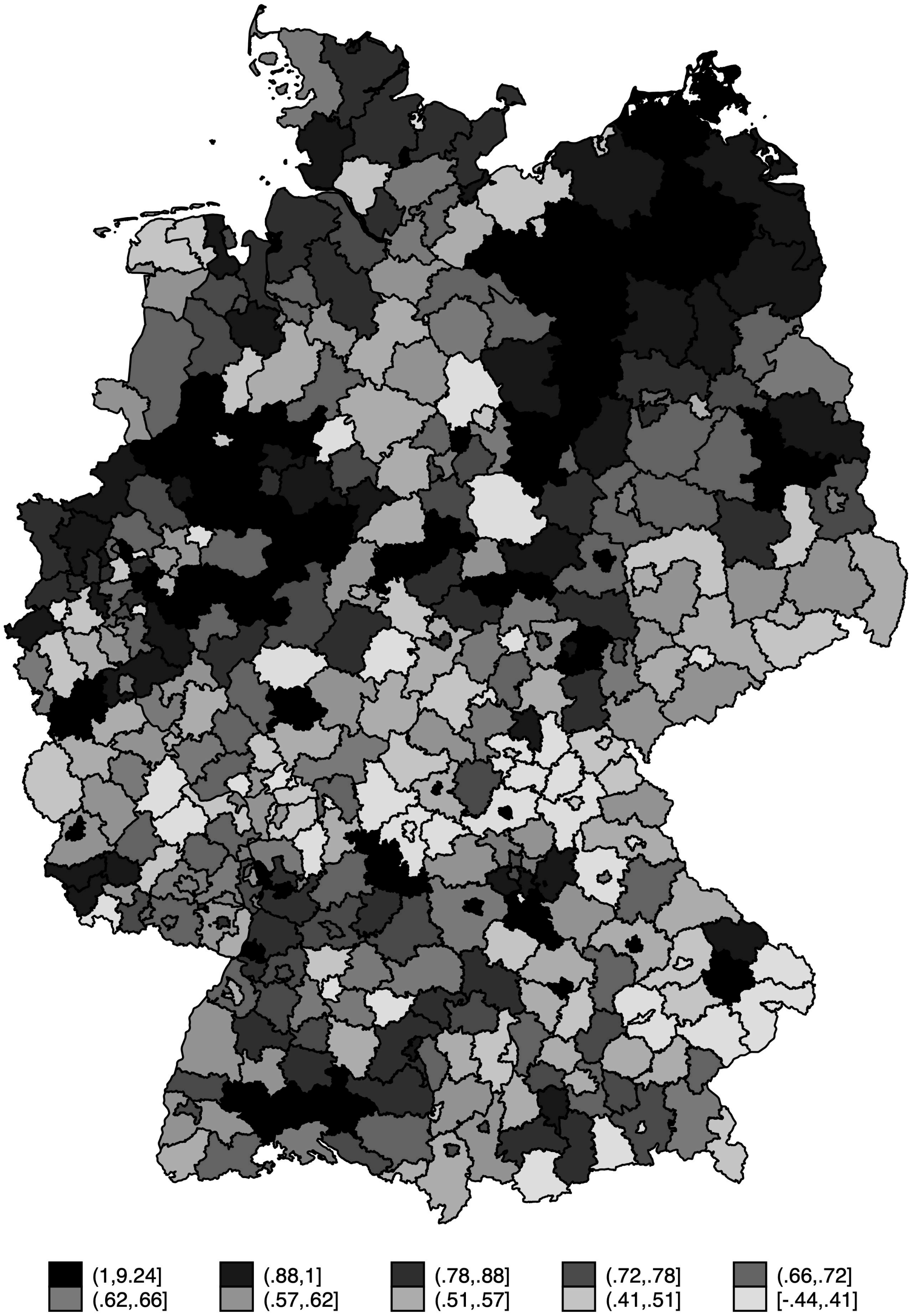
Note: Summary statistics of refugee share per county in 2015 (Ref_share 2015), in 2014 (Ref_share 2014) and the change in refugee share between 2014 and 2015 (Δ Ref_share) in percentage points in the sample: means, standard deviations, minimums, first, second, and third quartiles, maximums, numbers of observations, and numbers of counties included.

Table 1.1 provides summary statistics for this measure (Δ Ref_share) as well as for the asylum seeker shares per county in 2014 and 2015 for the sample used in the estimations. Generally, most counties saw similar increases in their refugee shares with first and third quartiles at .54 and .84, respectively, the median at .65 and the mean at .77. There are a number of outliers to the right, though, which can be better seen in Figure 1.A1 in the appendix. This figure plots the change in refugee shares by county against the size of the county, gauged by the number of observations in the sample. There are three counties that saw much larger increases of refugees shares than other counties with values of 9.2, 7.1, and 4.1 percentage points, respectively. To illustrate the variation of the refugee placement across Germany, Figure 1.2 maps the change in asylum seeker shares by county. The map is generally very similar to Figure 1.A2, which emphasizes the magnitude of the ERC.

Data from the SOEP provide information on the ethnic identity of migrants. The SOEP is a representative longitudinal household survey that is conducted annually since 1984. Because of the panel structure of the data set, I can include individual fixed effects in my estimations. These capture time-constant individual characteristics, thereby helping me to control for a lot of unobserved information. This is particularly advantageous when studying measures like ethnic identity, as it is probably interpreted differently between, but not within individuals over time.

In my analysis, I exclude native Germans without foreign-born parents, and solely look at respondents with either direct (born abroad) or indirect migration background (one or both parents born abroad). As main outcomes, I am interested in two variables, which are generally surveyed every two years: First, respondents with a migration background are asked: *"To what extent do you feel German?"* Second, to elicit respondents' identification with their original home country, they are also asked:

Figure 1.2 Change in the Spatial Distribution of Asylum Seekers



Note: County-level percentage change of recipients of asylum seekers' benefits between 2014 and 2015.
Source: Statistical offices of the *Bundesländer*.

“How connected do you feel to your country of origin?”[13] Both variables are ordinally-scaled from 0 (“not at all”) to 4 (“completely”/”very strong”).[14] In this study, both variables are used for the years 2012, 2014, 2016, and 2018.[15][16]

In addition to the main outcomes of interest, the SOEP data also provide a broad spectrum of further information, such as interview and household characteristics, and importantly, place of residence including county.[17]

In 2013 and 2015, the SOEP introduced two additional migration samples with the aim of acquiring more insights on migrants and improve their representation in empirical research. However, as this changed the sample composition and size drastically between 2012 and 2014, I have removed these two samples from my main estimations and constructed a balanced panel of 1,504 respondents for the years between 2012 and 2018. This panel includes only those respondents, who answered both of the questions stated above in 2012, 2014, 2016, and 2018.[18] To keep effects of the treatment consistent over time, respondents who moved between counties in those years are removed, limiting the number of respondents to 1,346 per year and 5,384 observations in total.

Table 1.2 displays the number of times each answer is given in each year to both questions. Moreover, the table also shows the total number of observations per year coupled with means and standard deviations. On aggregate, respondents feel more German over time, with increases for the two highest categories ("Completely" and "For the Most Part"), and decreases for the two lowest. This is also reflected by the yearly means, which steadily increase from around 2.7 in 2012 to 2.9 in 2018. Therefore, it seems as if migrants identified more with Germany, the longer they stayed in the studied time period, in line with the literature (Dustmann 1996, Manning and Roy 2010). On

[13]Before these questions are asked, the questionnaire states: "When we use the term “country of origin” below, we are referring to the country where you were born if you immigrated to Germany, as well as to the country where your parents or grandparents were born if you are the child or grandchild of immigrants to Germany."

[14]In the original data set both variables are scaled inversely from 1 (“completely”/”very strong”) to 5 (“not at all”). I rescaled them to make results more easily interpretable.

[15]Both outcome variables have been captured in some years before, however, there is a large gap between 2003 and 2010 and the sample size in 2010 is much smaller than the sample size of the following years. Therefore I exclude observations before 2012.

[16]The 2020-wave of the SOEP includes a question on both dimensions of ethnic identity. However, because the response options were altered from a five-point- to an eleven-point-scale, I unfortunately could not use this new information for this study.

[17]Because regional data including the respondent’s county of residence is sensitive and restricted in the SOEP, I have used the SOEPremote system to work with this information.

[18]The panel also includes respondents, who were asked both questions, but who declined to answer at least one of them in one or more years.

Table 1.2 Descriptive Statistics

		2012	2014	2016	2018	Total
Feel German	(-) *No Answer*	9	10	12	9	40
	(0) *Not at All*	75	53	39	35	202
	(1) *Barely*	126	106	91	81	404
	(2) *In Some Respects*	343	362	343	335	1383
	(3) *For the Most Part*	373	421	414	417	1625
	(4) *Completely*	420	394	447	469	1730
	Mean	2.701	2.746	2.854	2.901	2.800
	(SD)	(1.168)	(1.085)	(1.052)	(1.034)	(1.089)
Connect Home	(-) *No Answer*	9	12	10	12	43
	(0) *Not at All*	157	153	153	157	620
	(1) *Barely*	224	220	213	216	873
	(2) *In Some Respects*	440	424	447	451	1762
	(3) *Strong*	334	354	347	346	1381
	(4) *Very Strong*	182	183	176	164	705
	Mean	2.120	2.145	2.135	2.108	2.127
	(SD)	(1.191)	(1.191)	(1.178)	(1.172)	(1.183)
	N	1346	1346	1346	1346	5384

Note: Outcome frequencies, means, and standard deviations of the two main variables feeling German and attachment to home country for the years 2012, 2014, 2016, and 2018. Both outcomes are scaled from 0 to 4. Data source: German Socio-economic Panel.

the other side, there is hardly any dynamic visible for home country attachment, as means hover around 2.1.

In an extension of my main analysis, I also sort migrants by country of origin. To reach sufficiently large samples, I distinguish between three main groups: Western countries, Eastern Europe, and TMENA (Turkey, Middle East and North Africa). Moreover, there is an additional category for Balkan countries, which is relatively small.[19] Lastly, for further analyses, I split the Eastern European category in ethnic Germans, also called resettlers, (*Aussiedler*) and non-resettlers. Ethnic Germans are a large and important immigrant group, who predominantly lived in Poland, Romania and the former Soviet Union, before coming to Germany, particularly, after the Fall of the Berlin Wall in 1989. In appendix section 1.B, I explain how classifications into

[19] A relatively small number of respondents from countries not included in these categories are grouped into a very heterogeneous "Rest of World" category. It would be very hard to interpret results for this group meaningfully, therefore they are left out.

each group were made and which countries are included in each group. Moreover, I also provide descriptive statistics in Table 1.B2.

1.4.2 Empirical Strategy

In my main specification, I estimate a variant of a difference-in-differences regression of the following form:

$$y_{ict} = \beta_0 + \beta_1 Post_t + \beta_2 Post_t \cdot \Delta Ref\_share_{c14-15} + X'_{ict}\gamma + \rho_i + \tau_t + \epsilon_{ict}. \quad (1.1)$$

The outcome y_{ict} – which is either feeling German or the attachment to home country – for respondent i in county c at time t is regressed on the treatment dummy $Post_t$, which indicates the start of the treatment (which I define to be 5 September 2015, the date of chancellor Merkel's announcement mentioned in section 1.3.1), as well as the interaction of this dummy with the change in county refugee share $\Delta Ref\_share_{c14-15}$. The latter term is calculated by taking the difference in the number of refugees in 2014 and 2015 by county and dividing it by county population in 2012 ($\frac{\#Refugees_{c,2015} - \#Refugees_{c,2014}}{Population_{c,2012}}$).[20] I additionally include plausibly exogenous control variables (X_{ict}), as well as individual (ρ_i) and time fixed effects (τ_t). The main coefficient of interest is β_2, which measures the arguably causal effect of refugee inflows on the identification measures.

As events such as marriage or childbirth may affect respondents' ethnic identity, I include time-varying household controls, namely marriage status, a dummy indicating whether the respondent is the household head as well as the number of children and adults in the household. Moreover, to control for interview effects, dummies for month, weekday, and mode of the interview are also included. Following Abadie et al. (2017), standard errors are clustered at the county level – the level of the treatment. However, results are also robust to other forms of clustering.

1.4.3 Causal Identification

In order to estimate potentially causal effects, the relation of refugee placement and ethnic identity would have to be absent of any confounding factor not included in the regression that could be correlated with both the dependent and main independent variable. There are three potential risks to the identification.

[20] The population size is fixed at 2012 levels, i.e., clearly before the treatment started, to avoid issues of endogeneity.

The first issue may arise if immigrants' identification outcomes may have an effect on the placement of asylum seekers. While placement was rule-based in the beginning (see section 1.3.3), this became less tenable as the refugee crisis continued, leading to more discretionary allocation. Although it is unlikely that migrants' home- and host-country attachment would directly influence decision-makers, it could be correlated with cultural and social outcomes that are hard to capture statistically in the data. Therefore, state authorities might place refugees in accordance with the cultural assimilation of immigrants. Unfortunately, it is hard to test this possibility, as there are not any aggregate data for migrant ethnic identity by county for Germany. However, I can use the SOEP data to check whether any of the identification outcomes in the past is correlated with refugee allocation thereafter. In order to test this, I estimate six different regressions, using the change in host (home) country connection between 2010 and 2012, 2012 and 2014, and 2010 and 2014 (Δy_{ict}) as main regressors and the change in refugee concentration per county between 2014 and 2015 ($\Delta Ref\_share_{c14-15}$) as dependent variable. Using this approach, which is similar to the one used in Halla et al. (2017) and Dustmann et al. (2019), I arrive at the following first-differences regression equation:

$$\Delta Ref\_share_{c14-15} = a_0 + a_1 \Delta y_{ict} + X'_{ict} a_2 + e_{ict}. \tag{1.2}$$

The controls used (X_{ict}) are the same covariates as in equation 1.1. Moreover, in order to achieve sufficiently large sample sizes, I use unbalanced SOEP data.

Results are provided in Table 1.A2 in the appendix, with columns (1) to (3) (4 to 6) presenting results when using changes in feeling German (home country attachment). In all six columns, the coefficients of interest are insignificant, indicating that migrant host and home country attachment did not affect placement.

Another possible risk to identification is that the states may have allocated refugees to counties where migrants are generally better integrated or integrate faster. While integration and identification are not congruent concepts, it is likely that migrants who feel more attached to Germany are also better integrated. If the placement decisions were made according to these considerations, this could potentially distort estimates, likely biasing results upwards in the case of host- and downwards in the case of home-country attachment. To test whether this was the case up until the ERC, I run a host of fixed effects regressions of the following form:

$$Ref\_share_{ct} * 100 = \alpha_0 + \alpha_1 int\_measure_{ct} + \lambda_c + \pi_t + \eta_{ct}. \tag{1.3}$$

The regressions include asylum seeker concentration Ref_share_{ct} in county c in year t[21] as the outcome which is regressed separately on different integration measures ($int\_measure_{ct}$), as well as time (π_t) and county (λ_c) fixed effects. Standard errors are clustered at the county level. Integration measures include a number of social and economic integration outcomes. First, I introduce constructed measures for intermarriage and naturalization shares of migrants as well as their representation among *Gymnasium* students, Germany's academic secondary school track. Thereafter, I look at foreigner unemployment rate, their share in employment that is subject to social insurance contributions, as well as the shares of foreigners receiving different kinds of social security benefits. Lastly, I examine whether there were any differences in the demographic composition of counties, namely the foreigner share of the population and migrant nationality. The data, again, are from the statistical offices of the *Bundesländer*.

The results in Tables 1.A3 to 1.A5 (section 1.A) show that asylum seekers were not placed in areas where migrants had integrated faster. First, there is no indication that any of the social outcomes (intermarriage, naturalization, *Gymnasium* student representation) influenced placement, as all coefficients are insignificant. Second, there was no influence of migrants' economic integration, with estimates for both unemployment rate as well as employment share being insignificant. Third, it also does not appear as if asylum seekers were placed according to the demographic composition of migrants. Both the coefficient for foreign population share and those for all nationality groups are insignificant (Table 1.A4). The only category that jumps out is social security benefits in Table 1.A5, with the share of foreigners receiving *Mindestsicherung* (minimum income guaranteed), *Grundsicherung* (basic social security, mostly paid to low-income retirees) and *Hilfe bei besonderen Lebenslagen* (assistance for sick or disabled people or those facing social hardships) having a negative association with the share of refugees in a county. It is unclear, however, whether these coefficients actually capture differences in the integration of migrants, as differences in unemployment rates did not affect the distribution of refugees. Rather, it appears more likely that they are the result of fiscal considerations as some of the social benefits had to be paid by local governments. This would mean that refugees were placed less often in areas where social expenditures for foreigners were already growing.

So what determined the governments' allocation decisions, and thereby, the differences in refugee concentration by county? While Aksoy et al. (2020) found that, overall,

[21]With t representing the years 2010, 2011, 2012, 2013, 2014.

county population size was the main determinant in refugee placement, this cannot explain the variation observed here, as it is relative to population size. Rather, one major reason can probably be found when looking at the states' allocation regimes. As presented in section 1.3.3, each state had distinct allocation rules and quotas, leading to higher asylum seeker concentration in some counties compared to others. Moreover, it is likely that a notable number of refugees were still stuck between different allocation stages and housed in one of the initial reception facilities (*Erstaufnahmeeinrichtung*), which may have led to some additional variation. Moreover, as shown above, fiscal considerations may have also played some role, leading to a lower concentration of asylum seekers in areas that already experienced increases in social security expenses for foreigners. Lastly, another potential determinant may also be found in Table 1.A4, in column (7), namely the share of asylum seekers hosted in a county in the year before. Refugee allocation in the past appears to be highly decisive for allocation decisions in the future, with a percentage point increase in refugee shares in the past being associated with around half a percentage point increase in the future. This indicates that some counties may have existing structures and facilities to be better equipped to host larger numbers of refugees.

Overall, it appears as if refugees were not placed according to factors correlated with the identification or integration of migrants. Rather, determining factors in the allocation of refugees were legal frameworks and the organizational facilitation of the allocation in each state, as well as the existence of facilities and structures that are capable of handling the inflow of refugees. However, I cannot completely rule out that there are other possible confounders determining both refugee allocation and migrants' ethnic identity. Therefore, I include a number of individual and regional controls that are not strictly exogenous in further specifications of my main regressions. Rather, these covariates run at the risk of being "bad controls", meaning that they are also affected by the treatment (Angrist and Pischke 2008).

1.5 Results

1.5.1 Main Results

The main results for the effect of the treatment on the two outcome variables are shown in Table 1.3, with Panel A (Panel B) laying out results for feeling German (home country connection). Column (1) shows the results for the simplest specification, which only includes individual and time fixed effects and no further covariates. The other columns incrementally add controls. In column (2), only plausibly exogenous controls related to interview and household characteristics are added. This is the preferred specification, that is also used for most additional analyses. Further individual[22], and then regional controls[23] are gradually included in columns (3) and (4) to check whether there might be indications of some omitted variable bias.

Results for the first outcome variable, feeling German, are presented in Panel A. The coefficient *Post* is highly significant and positive at around 0.2 across the first three specifications, indicating that the attachment to Germany increased after 5 September 2015. These outcomes are along the lines of the descriptive statistics in Table 1.2 and the findings in previous research (e.g., Dustmann 1996, Manning and Roy 2010).[24]

The coefficient of interest is the interaction of Post and change in refugee concentration between 2014 and 2015 (Post * Δ Ref_share), shown in the second row. It is negative, close to zero and insignificant in all specifications. The estimate decreases somewhat after including more controls in (3) and (4), but still remains insignificant at conventional levels. This indicates that, on aggregate, there was no effect of the local presence of refugees on host-country identification.

In Panel B, estimates in the first row indicate no significant change of home country attachment after the treatment started. However, the coefficient in the second row, representing the main treatment effect, is positive and highly significant in all specifications. Values range from 0.05 without controls to around 0.0639 in column (3).

Due to the estimation approach employed – treating the ordinal dependent variable as if it was cardinally scaled – these coefficients are hard to interpret directly. Therefore, I estimated multiple logit fixed effects regressions in Table 1.4. Because of the scaling of the outcome variables, results for different cutoffs are shown. In column (1), I take the three most affirmative responses for both outcomes and code them as 1, with all other responses being 0. For the second (third) cutoff, I code the two (one) most affirmative response option(s) as 1, and the others as 0. As logit regressions with fixed effects are computationally intensive and usually require large sample sizes to work well, I estimate the two regressions without any controls, but only include individual and

[22]These include the respondent's logged individual labor income, annual hours worked, years of education and employment status.

[23]Regional controls include the county's GDP per capita, unemployment rate, population size, number of foreigners, and female population share.

[24]As a note of caution however, coefficients are likely inflated, as this panel exclusively consists of people who were present in Germany in all the observed periods between 2012 and 2018. Therefore, effects for other respondents, such as those who re-emigrated, would likely have been lower.

Table 1.3 Main Regression Results

	(1)	(2)	(3)	(4)
Panel A: Feel German				
Post	0.205***	0.202***	0.209***	0.105
	(0.036)	(0.038)	(0.037)	(0.085)
Post * Δ Ref_share	-0.009	-0.007	-0.013	-0.021
	(0.013)	(0.015)	(0.015)	(0.014)
Panel B: Connect Home				
Post	-0.051	-0.048	-0.042	-0.072
	(0.037)	(0.036)	(0.036)	(0.086)
Post * Δ Ref_share	0.050***	0.063***	0.064***	0.052***
	(0.018)	(0.018)	(0.018)	(0.019)
Basic Controls		Yes	Yes	Yes
Additional Indiv. Controls			Yes	Yes
Regional Controls				Yes
Individual FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Mean Feel German	2.72	2.72	2.72	2.72
Mean Connect Home	2.13	2.13	2.13	2.13
N	5384	5384	5384	5384

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. Column (1): Simple fixed effects regression without controls. Column (2): Also includes plausibly exogenous regressors, mentioned on page 12. Column (3): Adds further individual controls mentioned in footnote 22. Column (4): Adds regional controls mentioned in footnote 23. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

time fixed effects and clustered standard errors at the county level (which corresponds to the specification in Table 1.3, column 1). The results of the three logit regressions point in a similar direction as the previous results, showing insignificant values that are close to zero for all regressions in Panel A. Values for home country connection in Panel B, on the other hand, are positive and weakly significant for the medium and high cutoff. Taking the coefficient in column (2) implies that a mean increase in asylum seeker concentration of .77 percentage points raises the likelihood to report

Table 1.4 Binary Logit Regressions

	(1) Low Cutoff	(2) Medium Cutoff	(3) High Cutoff
Panel A: Feel German			
Post * Δ Ref_share	-0.016	0.006	0.010
	(0.020)	(0.009)	(0.027)
Panel B: Connect Home			
Post * Δ Ref_share	0.027	0.028*	0.022*
	(0.021)	(0.015)	(0.013)
Basic Controls			
Individual FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Mean Feel German	0.87	0.60	0.30
Mean Connect Home	0.72	0.40	0.14
N (Panel A)	1280	2216	1932
N (Panel B)	1892	2296	1424

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. Column (1): The three most affirmative responses ("In Some Respects", "For the Most Part"/"Strong", "Completely"/"Very Strong") are coded equal to 1, and all other options coded equal to 0. Column (2): The two most affirmative responses ("For the Most Part"/"Strong", "Completely"/"Very Strong") are coded equal to 1, and all other options coded equal to 0. Column (3): The most affirmative response ("Completely"/"Very Strong") is coded equal to 1, and all other options coded equal to 0. All logit regressions include individual and time fixed-effects. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of *Post* with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

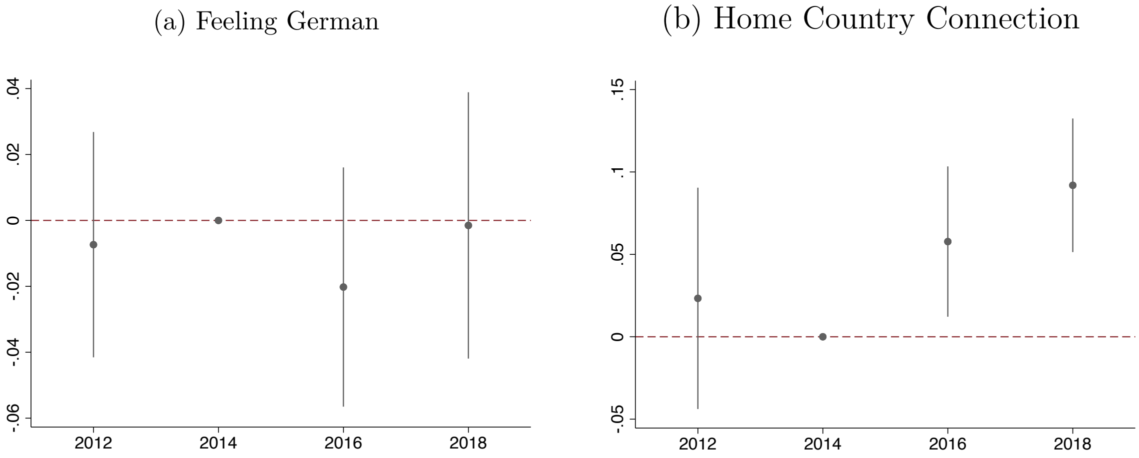
either a "strong" or "very strong" attachment to the home country by 2.18 percentage points or 5.5 percent[25], which is a noteworthy increase.

One important condition for the applicability of a difference-in-differences approach is the common trend assumption. To evaluate its validity, Figure 1.3 shows the treatment effects over time for feeling German (a) and home country connection (b) as an event study analysis. The graphs depict the coefficients and the 95% confidence intervals of the interaction of refugee share and year dummies, with base year 2014.[26]

[25]The share of respondents reporting either "strong" or "very strong" connection to their home country in 2012 is 39.89 percent of all respondents. Therefore, an increase of 2.18 percentage points raises the likelihood of having a "strong" or "very strong" home country attachment by (2.18/39.89 =) 5.5 percent.

[26]Regressions are similar to those in Table 1.3, column (2), as they include time- and individual fixed effects, as well as the control variables mentioned on page 12.

Figure 1.3 Event Study Analysis



Note: Effects of refugee placement on outcomes over time. Coefficients are for the interactions of refugee share and year dummies (base year 2014) in fixed-effects regressions including the regressors mentioned on page 12.

For both, feeling German and connection to home country, there is no significant difference between 2012 and 2014, indicating that counties that received more refugees were not on a different trajectory than those that received less. Furthermore, while coefficients for feeling German are never statistically different from the value in 2014, coefficients for connection to home country are both significantly different from 2014.[27] This indicates that home-country attachment became and remained significantly larger after time in areas with higher refugee concentration.

Overall, these results appear to support the idea that increased refugee inflows were seen as a threat by migrants. As a response, migrants' group cohesion was raised in the form of more attachment to the home country.

1.5.2 Robustness

In this section, I evaluate the robustness of my results, starting first with the sample selection and the presence of extreme values or outliers. Thereafter, I briefly touch on matters of scaling of the dependent variable, omitted variable bias, clustering of standard errors, and alternative treatment specification, which are all further discussed in appendix section 1.C.

[27]Taking 2012 as the base year, the difference to 2016 is insignificant. However, the coefficient for 2018 is significantly different at the 10% level.

Table 1.5 Sample Selection

	(1) West Germany	(2) Working Age Pop.	(3) +Moved Resp.	(4) Extended Sample
Panel A: Feel German				
Post	0.196***	0.217***	0.187***	0.165***
	(0.039)	(0.041)	(0.036)	(0.031)
Post * Δ Ref_share	-0.006	-0.015	0.001	-0.028
	(0.016)	(0.013)	(0.015)	(0.021)
Panel B: Connect Home				
Post	-0.037	-0.047	-0.062*	-0.085**
	(0.038)	(0.039)	(0.035)	(0.041)
Post * Δ Ref_share	0.064***	0.066***	0.079***	0.085**
	(0.020)	(0.020)	(0.024)	(0.039)
Basic Controls	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Mean Feel German	2.72	2.73	2.73	2.64
Mean Connect Home	2.13	2.13	2.10	2.17
N	5140	4868	6012	20058

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. All regressions are specified as those in Table 1.3, column (2). Column (1): Sample restricted to respondents from West Germany (incl. West-Berlin). Column (2): Sample restricted to respondents, who were of working age, meaning between 18 and 64, in 2012. Column (3): Sample including respondents who moved between 2012 and 2018. Treatment fixed by county, in which respondent lived in 2014. (4) Unbalanced sample including all respondents who did not move between 2012 and 2018. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

To check whether my results hold for different sample selections, I first test, whether results are driven by regions or particular states. In column (1) of Table 1.5, results remain largely the same when excluding East German counties. In an additional test to check whether individual states drive results (not shown), I estimate my regressions while selectively excluding one state at a time. Again, coefficients remain in line with previous results.[28] Next, as many labor economists are interested in the effects on

[28] When excluding Bavaria, the effect on home country connection becomes nearly twice as large.

working age adults, as they are the primary actors on the labor market, I exclude respondents, who were not of working age in 2012. Again, results change only little (2). Thereafter, I examine, whether results still hold, when I include all respondents, who moved between different counties after 2012. For that, I fix treatment effects for counties where respondents lived in 2014.[29] Results in column (3) show that including respondents who moved does not change coefficients a lot; if anything, the effect on home country attachment is even larger. Lastly, for my main regressions, I use a balanced panel of respondents who have regularly participated in the survey. This could lead to a selective sample, that might differ from the original sample. E.g., respondents dropping out over time might differ in important characteristics from those remaining, potentially biasing results. To check whether this could be a potential issue, I run the same regressions as in column (2) of Table 1.3 on the unbalanced sample. Results in Table 1.5, column (4) show that, while the coefficient for connection to home country loses a bit of its significance, it actually increases in size. Moreover, the coefficient for feeling German becomes more negative, while still remaining insignificant. Again, this supports the idea that the baseline results are robust.

Another concern regarding my estimation approach may be that I rely on a relatively small number of observations per county. Therefore, relatively large counties, i.e., those with many respondents, could have an outsized influence on the estimates. Table 1.6 shows, however, that this is not the case, as dropping those large counties (columns 2 and 3) barely affects estimates. Similarly, dropping very small counties (4 and 5), meaning those with few respondents, and constructing county averages for both outcomes (6) leads to similarly large and significant results as in the baseline specification.

Yet even though particularly large or small counties do not appear to have outsized influence on estimates, there may still be a problem that counties, which received disproportionately more asylum seekers, may drive results. As Figure 1.A1 illustrates, there are a few counties that received a much higher share of asylum seekers than others. Tables 1.C1 and 1.C2 reveal, however, that while the coefficients are somewhat sensitive to the trimming and winsorizing of these outliers, they remain positive and largely significant.

[29]This can be problematic, if respondents moved between 2014 and the start of the treatment. However, results barely change, when I look at the treatment effects for counties, where respondents lived in 2016.

Table 1.6 Dropping Large or Small Counties

	(1) Baseline	(2) <100 obs	(3) <75 obs	(4) >12 obs	(5) >20 obs	(6) County Mean
Panel A: Feel German						
Post	0.202***	0.205***	0.213***	0.207***	0.190***	0.204***
	(0.038)	(0.038)	(0.039)	(0.043)	(0.047)	(0.036)
Post * Δ Ref_share	-0.007	-0.006	-0.009	-0.015	-0.010	-0.008
	(0.015)	(0.015)	(0.014)	(0.017)	(0.019)	(0.014)
Panel B: Connect Home						
Post	-0.048	-0.041	-0.058	-0.030	-0.068	-0.041
	(0.036)	(0.041)	(0.040)	(0.043)	(0.046)	(0.036)
Post * Δ Ref_share	0.063***	0.059***	0.057***	0.072***	0.079***	0.054***
	(0.018)	(0.017)	(0.015)	(0.027)	(0.027)	(0.018)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Feel German	2.72	2.74	2.76	2.68	2.67	2.72
Mean Connect Home	2.13	2.12	2.10	2.15	2.15	2.13
N (Panel A)	5384	4700	4196	4308	3700	5344
N (Panel B)	5384	4700	4196	4308	3700	5341

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. Column (1): Baseline estimation as in Table 1.3, column (2). (2): Drops counties with at least 100 observations. (3): Drops counties with at least 75 observations. (4): Drops counties with 12 observations or less. (5): Drops counties with 20 observations or less. (6): Uses county means for feeling German and connection to home country as dependent variables. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In my main regressions, I treat my dependent variables as if they were cardinally scaled, which might be problematic. Although I already showed that the results generally hold when using binary logit methods, I additionally estimate the regressions using ordinal logit methods (Table 1.C3), finding similar results as in my main regressions.

Next, I address issues regarding omitted variable bias. While results in Table 1.3 already show that the inclusion of bad controls barely changes the main coefficients, I include a host of further potential confounders such as tragic events, regional migration and political outcomes in Table 1.C4 and 1.C5 and region-time fixed effects in Table 1.C6. Overall, results remain robust to the inclusion of these factors.

As described in section 1.4.1, I have constructed my treatment variable relatively conservatively, taking the change in asylum seeker share between 2014 and 2015.

To evaluate the robustness of using this treatment variable, I estimate the baseline regressions with a number of different treatment variables, namely the share of asylum seekers in 2015, the combined shares of asylum seekers in 2014 and 2015, the change in refugee share between 2013 and 2015, and, lastly, dividing the number of refugees by working age population (Table 1.C7). Results overall remain robust.

Lastly, results also remain robust to different clustering of standard errors, namely at the state, individual, and household level in Table 1.C8.

1.5.3 Heterogeneities

Taken together, the previous sections showed that migrants felt more connected to their home countries as a response to perceived threat emanating from higher refugee concentration. However, treating migrants as one homogeneous group could potentially overlook the diversity and variety of viewpoints in the immigrant community, which may impact their threat perception.

Therefore, to further investigate these potential heterogeneities, I look at the origin country of migrant respondents. Hereby, I face a trade-off between potentially matching dissimilar migrants into the same group and maintaining meaningfully large sample sizes. Trying to balance both objectives, I categorize most migrants into Westerners, Eastern Europeans – who can be split into resettlers and non-resettlers – migrants from Balkan countries, and migrants from Turkey, Middle East and North Africa (TMENA).

In the following, I employ the same estimation approach as before, but estimate separate regressions for each migrant group. As these regressions rely on smaller sample sizes, we should note, that coefficients are less precisely estimated and extreme values may have even more influence on the effect sizes. We should therefore be cautious in interpreting the estimated coefficients.

The results for the effects by origin group are displayed in Table 1.7, with Panel A presenting results for feeling German and Panel B those for home country connection. The regressions are specified as in Table 1.3, column (2). Looking at Panel A, we see that refugee concentration has a significantly negative impact on the outcome for migrants from the TMENA region (column 1). Using binary logit regressions[30] (not shown), this implies that a mean increase in refugee shares of .77 lowers attachment to Germany by up to 4 percentage points. This stands in contrast to Western migrants

[30]Again, due to the lack in statistical power, regressions are estimated without further controls apart from individual and time fixed effects.

Table 1.7 Effect on Feeling German by Country of Origin

	(1) TMENA	(2) Western	(3) E Europe	(4) EE + Balk.	(5) Aussiedler	(6) No Aussiedler
Panel A: Feel German						
Post	0.069	0.193***	0.244***	0.225***	0.184***	0.359***
	(0.094)	(0.071)	(0.053)	(0.047)	(0.063)	(0.088)
Post * Δ Ref_share	-0.072**	0.076	-0.044**	-0.038**	-0.020	-0.074***
	(0.029)	(0.055)	(0.021)	(0.018)	(0.027)	(0.026)
Panel B: Connect Home						
Post	-0.026	0.042	-0.026	-0.075	-0.039	0.016
	(0.099)	(0.078)	(0.053)	(0.048)	(0.067)	(0.107)
Post * Δ Ref_share	0.020	-0.055	0.095***	0.108***	0.081**	0.134***
	(0.037)	(0.046)	(0.025)	(0.026)	(0.040)	(0.042)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Feel German	2.40	2.53	3.02	2.80	3.16	2.63
Mean Connect Home	2.49	2.51	1.67	2.00	1.48	1.95
N	952	1376	2056	2680	1364	692

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. All regressions are specified as those in Table 1.3, column (2). Subsample regressions. Column (1): Respondents from Turkey, Middle East and North Africa. Column (2): Western Europe, USA, Canada, Australia, New Zealand. Column (3): Eastern Europe, meaning former Warsaw Pact countries. Column (4): Eastern Europe and Balkan countries. Column (5): Eastern Europe, only resettlers. Column (6): Eastern Europe, only non-resettlers. *Post* indicates years after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(2), who feel more German, albeit insignificantly, when surrounded by more asylum seekers. Results for Eastern Europeans in columns (3) to (6) are for the most part negative and significant, with coefficients of about -.04 for Eastern Europeans overall (3), implying that a mean increase in refugee shares lowers attachment to Germany by around 1.7 percentage points when using binary regressions. Including migrants from the Balkans in column (4) cuts the coefficient down only a bit. More interestingly, there appears to be a difference in the reaction of resettlers and non-resettlers (columns 5 and 6), with the former actually experiencing no effect overall, while the impact on the latter is negative and comparable in size to the one for TMENA migrants.

When looking at the second outcome, home country connection, in Panel B, the effects on Westerners (2) again point in the opposite direction compared to Eastern Europeans (3 to 6). While home country attachment for the former is (statistically insignificantly) decreasing in counties with higher refugee shares, the opposite is true for Eastern Europeans, where the negative effects are large and highly significant across the board. Coefficients are around .1 for Eastern Europeans overall and a little higher when including migrants from the Balkans. Using binary logit regressions, this implies that a mean increase in refugee shares raises home country belonging for this group by up to 6 percentage points. Again, there is a difference between non-resettlers and resettlers, with the effect for the former group being larger than for the latter. The only group that does not show any clear effect in either direction are the TMENA migrants (column 1).

Overall, these results support the idea that individual migrant groups were differently affected by refugee inflows. In particular, Eastern Europeans appear to be the most affected, suggesting that they felt the most threatened. This would be in line with previous findings, as, for example, a poll by the Boris Nemtsov Foundation in 2016 showed that more than 70 percent of Russian-Germans thought that there were terrorists among the refugees (Boris Nemtsov Foundation, 2016). Moreover, Sablina (2021) describes how anti-immigrant and Islamophobic statements and content were spreading on Russian-language social media platforms after 2015, lamenting the German immigration policy and arguing that refugees posed a risk to residents. These dynamics also were important in the infamous *Fall Lisa* (criminal case of Lisa): Therein, a Russian-German teenager falsely claimed to have been sexually abused by refugees. Promulgated by Russian media, this led to mass protests in Germany and even diplomatic disputes between Russia and Germany (Schmalz, 2019).

Western migrants did not show any significant treatment effects; if anything, the coefficients point in the other direction. This indicates that their level of perceived threat was presumably quite low, which might be explained by better labor market and cultural integration among Western migrants (Aleksynska and Algan 2010, Kogan 2011). The level of integration likely also played an important role, as Tables 1.A6 and 1.A7 show. Only migrants, who were foreign born and arrived as adults, who arrived later, and who had lower education experienced treatment effects. Lastly, effects for TMENA migrants are mixed, with only a decrease in host country belonging.[31] As I elaborate further in the following section, this might be explained by an indirect threat caused by refugee placement.

[31]The latter observation stands in contrast to the findings in Deole and Huang (2020), who find increased home country attachment for TMENA migrants, with no effects on feeling German.

1.5.4 Factors Shaping Threat Perception

In the previous section, I have laid out that different migrant groups were distinctly affected by the ERC, which was likely driven by differences in threat perceptions. Therefore, in this section, I examine which factors influenced this perception.

First, I check whether refugee inflows altered migrants' concerns in various domains, which are captured in the SOEP.[32] First, I check whether higher inflows made migrants more worried about crime and job security, two realistic threats. Then, I test whether worries about immigration changed, which may encompass both realistic and symbolic threat. Lastly, I examine worries about xenophobia, which would represent the sense of an indirect threat, not coming from refugees but from hostile natives.

Thereby, I use the same regression approach and sample as for the main analysis, but employ the respective worries as outcome variables. Table 1.A8 in the appendix (section 1.A) displays results, indicating that neither worries about crime (1) nor job security (2) increased the more refugees were accommodated in respondents' counties. Moreover, worries about immigration (3) were also unaffected. These results suggest that migrants may not have perceived a realistic threat coming directly from refugees. Interestingly though, the coefficient in (4) is positive and weakly significant, revealing that migrants had higher worries about xenophobia in counties with more refugees. Separate estimations by migrants' origin (not shown) indicate that migrants had more concerns about xenophobia regardless of their origin. This indicates that they may have perceived an indirect threat from natives (Gould and Klor 2015, Elsayed and de Grip 2017).

To examine this further, I look at whether respondents reported having experienced discrimination in the past, which may have informed these worries. Stephan et al. (2015) argue that discrimination makes people more alert to threat. Moreover, in social psychology, acts of discrimination are considered to not only threaten each individual, but also devalue and thereby threaten their social group (Branscombe et al., 1999). Such identity threat may have long-lasting effects and may also trigger a reaction from migrants during the ERC as they may fear future discrimination. I therefore split the sample of migrants depending on whether they experienced discrimination or

[32] All worries are captured on a 0 to 2 scale, with 0 indicating that the respondent is "not concerned at all", 1 that they are "somewhat concerned", and 2 that they are "very concerned".

Table 1.8 Experience of Discrimination

	(1) No Discrimination	(2) Experienced Discrimination
Panel A: Feel German		
Post	0.137***	0.329***
	(0.041)	(0.067)
Post * Δ Ref_share	0.027	-0.071**
	(0.017)	(0.029)
Panel B: Connect Home		
Post	-0.040	-0.055
	(0.044)	(0.065)
Post * Δ Ref_share	0.035	0.105**
	(0.024)	(0.041)
Basic Controls	Yes	Yes
Individual FE	Yes	Yes
Time FE	Yes	Yes
Mean Feel German	2.88	2.48
Mean Connect Home	2.08	2.23
N	3104	2052

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. All regressions are specified as those in Table 1.3, column (2). Subsample regressions. Column (1): Respondents reported in 2013 that they never felt disadvantaged in the last two years due to their ethnic origins. Column (2): Respondents reported in 2013 that they seldom or often felt disadvantaged in the last two years due to their ethnic origins. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

not, based on information in the SOEP from the 2013.[33] Overall, about 40 percent of immigrants state that they have experienced discrimination due to their origin in Germany, while the rest did not.

Table 1.8 displays that migrants, who reported discrimination, had significantly higher home country and lower host country attachment in counties with higher shares of refugees. Effects for non-discriminated migrants, on the other side, are

[33]The SOEP asked respondents with a migration background, how often they have felt disadvantaged due to their ethnic origin (never, rarely, often). This variable was also captured in 2017, however, as responses for 2017 were already affected by the treatment, I only use information for 2013.

small and insignificant. These patterns are also observable when I distinguish between country of origin in Table 1.A9. There, overall effects are visible for Eastern European and TMENA migrants.[34] However, while host (home) country attachment decreased (increased) for Eastern Europeans, who reported discrimination against them, TMENA migrants only became significantly less attached to Germany.

These results suggest that migrants likely perceived an indirect threat from refugee placement coming from natives. As accommodating refugees can provoke backlash in the form of protests by natives but also crime against migrants (Entorf and Lange 2019), migrants may have feared that they become the target of such violence. Such fears were seemingly much stronger among previously discriminated migrants, as they are probably more sensitive to these threats.

Another factor influencing threat perception may run through media consumption. Many immigrants, particularly those born and socialized abroad, often still consume television or newspapers from their countries of origin. In my sample, over sixty percent of respondents consumed at least some media from their country of origin. While media consumption is to a large extent a reflection of already held attitudes and beliefs (Gentzkow and Shapiro 2010), it still likely affects consumers' views (e.g., DellaVigna and Kaplan 2007).[35] This is probably even more the case for those, who trust foreign media more than domestic news outlets, which, e.g., is the case for about a third Russian-Germans (Boris Nemtsov Foundation, 2016). Georgiou and Zaborowski (2017) report strong differences in how mainstream media covered the European Refugee Crisis in different European countries. While the reporting in Western Europe often also incorporated sympathetic coverage, emphasizing the plight of the refugees, news media in Eastern Europe was generally much more sceptical, and often downright hostile. Media in Turkey and other MENA countries also differed in the way they covered the ERC. E.g., while Turkish media was generally empathetic with the refugees and their dire situation (Sunata and Yıldız 2018), images of state control were frequently invoked in the press (Sert and Danış 2021). Moreover, as outlined in the previous section, foreign-language social media use may have also influenced migrants. It is therefore likely that differences in media consumption between migrants have influenced their

[34] Too few Western migrants reported discrimination due to their origins to be able to run meaningful regressions.

[35] Because of strong issues of simultaneity and selection, research on the effects of media consumption on political views has for a long time been highly contested. However, some more recent studies, that exploit different natural experiments, generally find significant effects. Examples include Enikolopov et al. (2011), DellaVigna et al. (2014), Adena et al. (2015), and Durante et al. (2019).

Table 1.9 Language of Media Consumed

	(1) Only German Media	(2) At Least Some Foreign Media
Panel A: Feel German		
Post	0.167***	0.236***
	(0.047)	(0.048)
Post * Δ Ref_share	0.026	-0.029
	(0.023)	(0.020)
Panel B: Connect Home		
Post	-0.078	-0.038
	(0.064)	(0.045)
Post * Δ Ref_share	-0.019	0.111***
	(0.030)	(0.029)
Basic Controls	Yes	Yes
Individual FE	Yes	Yes
Time FE	Yes	Yes
Mean Feel German	3.26	2.42
Mean Connect Home	1.67	2.39
N	1844	3384

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. All regressions are specified as those in Table 1.3, column (2). Subsample regressions. Column (1): Respondents reported in 2014 that they only consumed news media in German. Column (2): Respondents reported in 2014 that they consumed at least some foreign media/media in the language of their country of origin. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

threat perception, particularly in areas where refugees are more present, as issues surrounding refugees are more visible in everyday life.

The SOEP data provide information about the language in which migrants consume news media for the year 2014.[36] In Table 1.9, I employ this information and compare migrants who exclusively consume German news coverage (column 1) with those who at least consume some foreign media (2). The results in Table 1.9 show strong differences

[36]Respondents are given five options, ranging from only consuming German media to only consuming media in the language of their home country. There is also a sixth option for those who do not consume news media at all. However, only very few respondents selected this option in 2014.

between the groups. While differences in media consumption do not lead to significant differences in attachment to Germany, they lead to striking differences in home country attachment. For those who consume no foreign news, the effect is virtually zero, while it is close to double the size of the baseline coefficient for those who consume at least some news in a foreign language.

A similar pattern can be observed for Eastern European and TMENA migrants. Even though sample sizes are quite small, Table 1.A10 shows that there was no treatment effect for members in both groups, who only consumed German media, while effects were large and significant for consumers of foreign media. The opposite of these effects can be observed for Western migrants. For this group, estimates for consumers of foreign media, while being insignificant, are large and point in the opposite direction, implying that they – if anything – became more attached to Germany and less attached to their home countries.

To make sure that these patterns are not solely due to differences in language use more generally, I run additional regressions, splitting the sample by the language spoken with family members and friends. In Table 1.A11, overall effects are not driven by respondents who predominantly talked with their family or their friends in the language of their home country. Rather, effects for this group are smaller than for other migrants and statistically insignificant.

These findings suggest that differences in news media consumption likely also played an important role in shaping migrants' threat perception. Migrants from Eastern Europe, who consumed foreign-language media, may have perceived a symbolic threat coming from refugees. This then led to a rise in their home country attachment and, to a smaller extent, a decrease in feeling German. The reaction of TMENA migrants, who consumed home country media, on the other side, is less clear and could be driven rather by a fear that the German state could not properly protect them against xenophobia and discrimination. As home country media stressed the importance of state control, this may have contrasted with TMENA migrants' worries about being the target of nativist hostility and discrimination, leading to an estrangement from Germany and stronger belonging to their home countries.

1.5.5 Related Outcomes

There have been a number of studies pointing to a connection between ethnic identity and labor market success (e.g., Battu and Zenou 2010), educational attainment (e.g., Baysu et al. 2011) or political preferences (e.g., Mayer et al. 2023). In this section, I therefore look at whether the placement of refugees has affected these outcomes, starting with the labor market and education.

We know that the arrival of a large amount of immigrants can lead to increased competition on the labor market – particularly for the lower-skilled. While this may result in lower wage growth and higher unemployment (Hunt 1992, Dustmann et al. 2013), labor market adjustments may also lead to opposite effects or no real changes (Kerr and Kerr 2011). Testing whether increased refugee inflows lead to changes in income, annual hours worked or employment, I find no changes overall (Table 1.A12). I additionally check for differential effects on education, finding no changes, either.

This is not particularly surprising, as asylum seekers in Germany are not allowed to work right away, but rather have to wait until they get a work permit. Moreover, many jobs require foreigners to provide language certificates, guaranteeing at least some knowledge of German. Refugees generally acquire these by visiting language classes for a substantial amount of time. In addition, due to the regulated nature of the German labor market, many jobs require apprenticeships or training, which usually last about three years. While we do not have data about the employment rates of asylum seekers specifically, employment rates[37] of migrants from the eight main source countries (such as Afghanistan and Syria) were still relatively low at 24.9 percent in January 2018 (Bundesagentur für Arbeit 2018), compared to foreigners (47.7 percent) and Germans (68.1 percent). As a further factor, it appears unlikely that changes in the ethnic identity of migrants would already make themselves visible in labor market outcomes in such a short amount of time. It is more likely, that, if they have consequences at all, they will manifest themselves over time.

Next, I look at the political preferences of migrants and how they were affected by refugee inflows. Previous studies have shown that perceived threat can impact voting behavior (Enos, 2016). Moreover, there have been a number of studies emphasizing the link between ethnic identity and voting behavior (Dancygier and Saunders 2006, Teney et al. 2010, Bergh and Bjørklund 2011, Baysu and Swyngedouw 2020). Motivated by the notion of a "linked fate" (Dawson, 1995), which leads migrants to integrate the interest of their ingroup in their decision-making, this can lead to group voting among migrants.

Historically, naturalized immigrants from Eastern Europe, in particular resettlers, voted mostly for the conservative CDU and CSU parties, while Western and TMENA

[37]These statistics only factor in employment subject to social security contributions.

migrants were predominantly left-leaning (Wüst, 2004). These preferences have diversified after the refugee crisis, though. The election study of immigrant voters by Goerres et al. (2018) showed that while the CDU/CSU still had plurality support among Russian-Germans (25 percent) and the SPD among Turkish-Germans (35 percent), the support was much lower than even a decade ago, when both parties routinely had majority support from the respective constituencies (Wüst, 2004). While Turkish Germans still predominantly favored left-wing parties, support for far-right AfD among Russian-Germans was already at 15 percent, higher than their support among natives.

As Hansen and Olsen (2020) showed that Russian-Germans favored the AfD because of hostility towards new refugees, this could also be interpreted as a reaction to perceived threat. Moreover, Mayer et al. (2023) expanded on this analysis and found that a strong Russian-German ethnic identity and lower levels of integration also made Russian-Germans more likely to prefer the AfD. Motivated by these recent findings, I check whether changes in ethnic identity were also accompanied by changes in political preferences.

Thereby, I use simple fixed effects linear probability models that are specified as in equation 1.1, testing whether the inflow of refugees leads to changes in political interest and preferences for political parties. To examine the first, which is scaled from 0 ("completely disinterested") to 3 ("very interested") in the SOEP, I transform this information into a binary variable, coded 1 if the respondent is at least "moderately" interested, and 0 if else. For party preference, the SOEP asks two questions, first, whether respondents lean towards any party at all, and second, which party that is.

Overall, the political interest of migrants is relatively low, with only a third being at least moderately interested. This is also reflected by the political preferences, as 70 percent of migrants had no preference for any party before 2015. This can at least partly be explained by the fact that over 40 percent of immigrants in the sample do not have a German citizenship, which precludes them from voting in most elections.[38]

It appears, however, that the inflow of refugees led to an increase in political interest and party preference. As Table 1.10 shows, immigrants in counties, which received relatively more refugees, experienced clear increases in both outcomes. While political interest rises by close to 2 percentage points for every percentage point increase in the share of refugees (column 1), stating a preference went up by 1.5 percentage points (2). These increases are noticeable, considering the overall low levels of political engagement.

[38]Citizens of EU countries are allowed to participate in European and local elections, but not federal and state elections. All other foreigners are ineligible to vote.

Table 1.10 Effects on Party Preferences

	(1) Pol. Int.	(2) Pref.	(3) CDU/CSU	(4) SPD	(5) Grüne	(6) FDP	(7) Linke	(8) AfD
Post	-0.001	0.005	0.011	-0.018	-0.012*	0.011***	0.001	0.017***
	(0.020)	(0.018)	(0.013)	(0.014)	(0.007)	(0.003)	(0.007)	(0.006)
Post * Δ Ref_share	0.018**	0.015**	-0.020***	0.004	0.006**	-0.002	0.016***	0.010**
	(0.008)	(0.007)	(0.006)	(0.010)	(0.002)	(0.001)	(0.006)	(0.004)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Mean	0.34	0.29	0.11	0.09	0.04	0.01	0.02	0.00
N	4885	5384	5384	5384	5384	5384	5384	5384

Note: Apart from dependent variable, all regressions are linear probability models specified as those in Table 1.3, column (2). Column (1): Outcome is whether respondent is moderately or very interested in politics (=1) or not (=0). Column (2): Outcome is whether respondent has a preference for a political party (=1) or not (=0). Column (3): Outcome is whether respondent prefers *CDU* or *CSU*. Column (4): Outcome is whether respondent prefers *SPD*. Column (5): Outcome is whether respondent prefers *Bündnis 90/Die Grünen*. Column (6): Outcome is whether respondent prefers *FDP*. Column (7): Outcome is whether respondent prefers *Die Linke*. Column (8): Outcome is whether respondent prefers *AfD*. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for each outcome for the years 2012 and 2014. All estimations conducted with Stata 15.1.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Looking at the major German parties in question, we can see that those parties, that had either very pro- (*Grüne*, *Linke*) or anti-refugee (*AfD*) stances, benefited most in counties with more refugee inflows. Support for more moderate parties did not change (*SPD*, *FDP*) or even decreased (*CDU/CSU*). Although we should be careful in interpreting the coefficients, considering the low number of respondents actually reporting a preference, the effects appear very large. Take the party *Die Linke* as an example: While before, only about two percent of all respondents had a preference for this party (about seven percent of respondents with a preference), raising the share of refugees by .77 percentage points increased the preference for them by 1.2 percentage points.

Lastly, I check how political interest and preferences changed by country of origin in Table 1.A13. For that, I group moderately conservative (*CDU/CSU* and *FDP*) and moderately left-wing (*SPD* and *Grüne*) parties together, but keep the most left- (*Die Linke*) and right-wing (*AfD*) parties separate.

Overall, the increase in political interest and preference appears to be predominantly driven by TMENA migrants. Hereby, it appears as if the threat TMENA migrants perceived coming from natives may have motivated them to actually become more politically engaged (Miller and Krosnick, 2004). Furthermore, we can see that the three groups react very differently to the refugee inflows. Westerners, who experience – if anything – more attachment toward Germany, show increases in preferences for moderately left-wing parties. In contrast, party preferences of Eastern Europeans and TMENA migrants show strong polarization: While the former increasingly leans towards right-wing *AfD* at the expense of moderately conservative parties, the latter exhibits increasing preference for the left-wing *Die Linke*, moving away from more moderate left-wing parties, in response to the treatment.

These results suggest that both Eastern Europeans and TMENA migrants perceived a threat from refugee placement. Yet, because the reason why both groups perceived threat was different, this led them to different changes in party preferences. On one side, Eastern Europeans, mostly perceived a symbolic threat from refugees. These feelings may have been amplified by foreign-language news coverage and social media, moving those voters towards the AfD, who supplied anti-immigrant positions that played to these fears. On the other side, those TMENA migrants, who felt more of indirect threat from refugee placement, as they feared increased hostility and discrimination from natives, might look for a party with policies strongly and vocally opposed to (anti-Arab and anti-Muslim) xenophobia. While other left-wing parties also supply such policies, previous studies have shown that Turkish migrants moved towards more left-wing parties, when they felt disappointed with the status quo (Aktürk, 2010), which the more moderate left-wing parties represent.

Nevertheless, these findings still provide ample ground for future research, looking at the effects of the European Refugee Crisis on political attitudes and intentions of migrants from different origins.

1.6 Conclusion

This study explores how the 2015 European Refugee Crisis – which led to a sudden and strong increase in asylum seekers in late 2015 – affected host- and home-country attachment of resident migrants in Germany. Using administrative and longitudinal survey data, I examine whether migrants in counties with higher increases in the share of asylum seekers had stronger changes in these outcomes. In order to arrive at arguably causal estimates, I exploit the quasi-experimental setting whereby refugees who arrived in Germany were allocated to counties by state authorities according to fixed quotas and rules.

In this study, I build on intergroup threat theory (Stephan et al., 2015), which argues that ingroups, in this case migrants, can feel threatened by an outgroup, which may increase their group cohesion. Hereby, I test whether migrants perceived such a threat from increased accommodation of refugees in their counties. My findings support intergroup threat theory, finding that migrants' attachment to their home country increased due to refugee inflows, while host country belonging was unaffected.

Additional analyses uncover strong heterogeneities by country of origin, which are likely driven by differences in perceived threat. Estimates suggest that while Western migrants became insignificantly more attached to Germany and less to their home countries when surrounded by more refugees in their county, the opposite was true for Eastern Europeans. Effects for migrants from Turkey, the Middle East, and North Africa (TMENA) were somewhere in-between, with decreases in their perceived belonging to Germany, but no changes in home country connection.

Investigating the nature of perceived threat, I find that migrants' worries about xenophobia increased in areas with more refugees, while worries about crime, job security, and immigration were unchanged, likely indicating that migrants did not perceive realistic threats from refugees. Rather, they were worried by an indirect threat, not from refugees themselves but a potential backlash from natives. This is supported by further analyses, uncovering that overall treatment effects only showed for migrants who experienced discrimination in the past, which may have informed worries about future discrimination. Moreover, foreign-language media likely also played a role, as only migrants who consumed non-German media experienced treatment effects. As hostile media portrayal of refugees was common in Eastern European media, this likely contributed to a symbolic threat for some migrants.

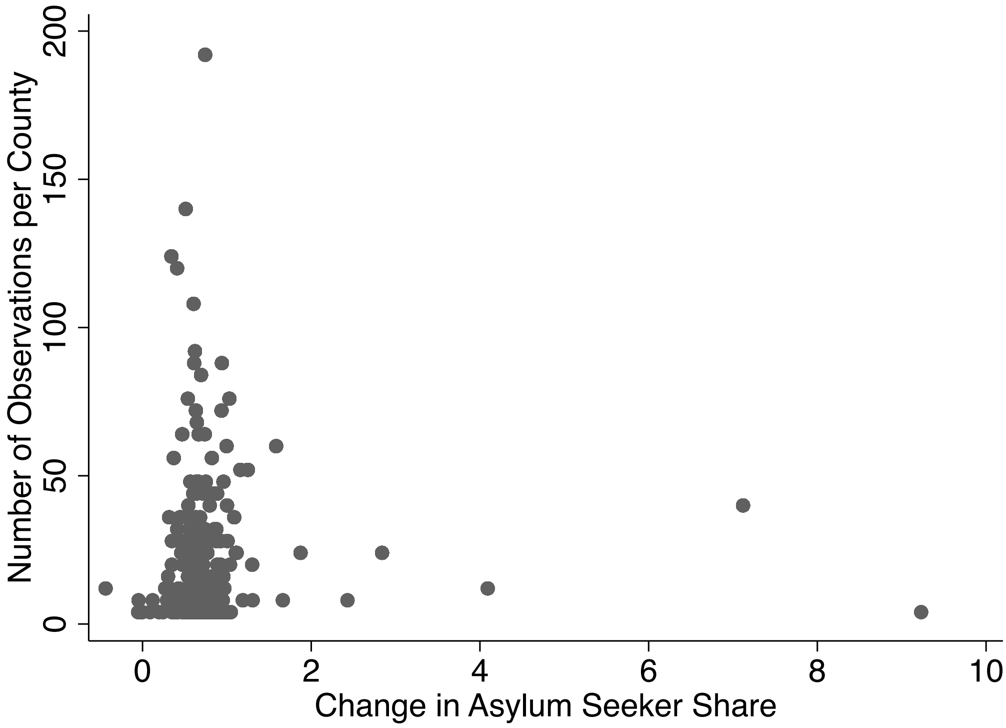
Further analyses show that the differential placement of refugees also affected the political preferences of migrants, making Eastern Europeans more likely to lean toward the far right *AfD* and TMENA migrants more likely to prefer the left-wing populist *Die Linke*. These results point towards a possible polarization of the migrant electorate.

This study has provided evidence for the short-term effects of the ERC on migrants' ethnic identity in Germany. To what an extent these results are applicable to other countries and contexts, is *ex ante* not clear. As immigration to Western countries may become even more frequent in the future, it is possible that more entrenched ethnic minorities may perceive this as a threat, and react accordingly. However, it will depend on the individual circumstances whether they will actually perceive threat. This also becomes clear in the context of this study, as threat perceptions differed vastly between different migrant groups.

Long-term, it will be interesting to see how the presence of refugees will affect it in the future. Will migrants' ethnic identity change again after being in closer contact with refugees? Or will effects accumulate further, possibly alienating some migrants more from Germany? A second question for future research is how the 2015 ERC will affect migrant assimilation. While it appears possible that some groups will actually invest less in host country-specific capital, we will have to see whether changes in ethnic identity are going to lead to substantial changes in labor market outcomes or whether effects will be more ambiguous overall.

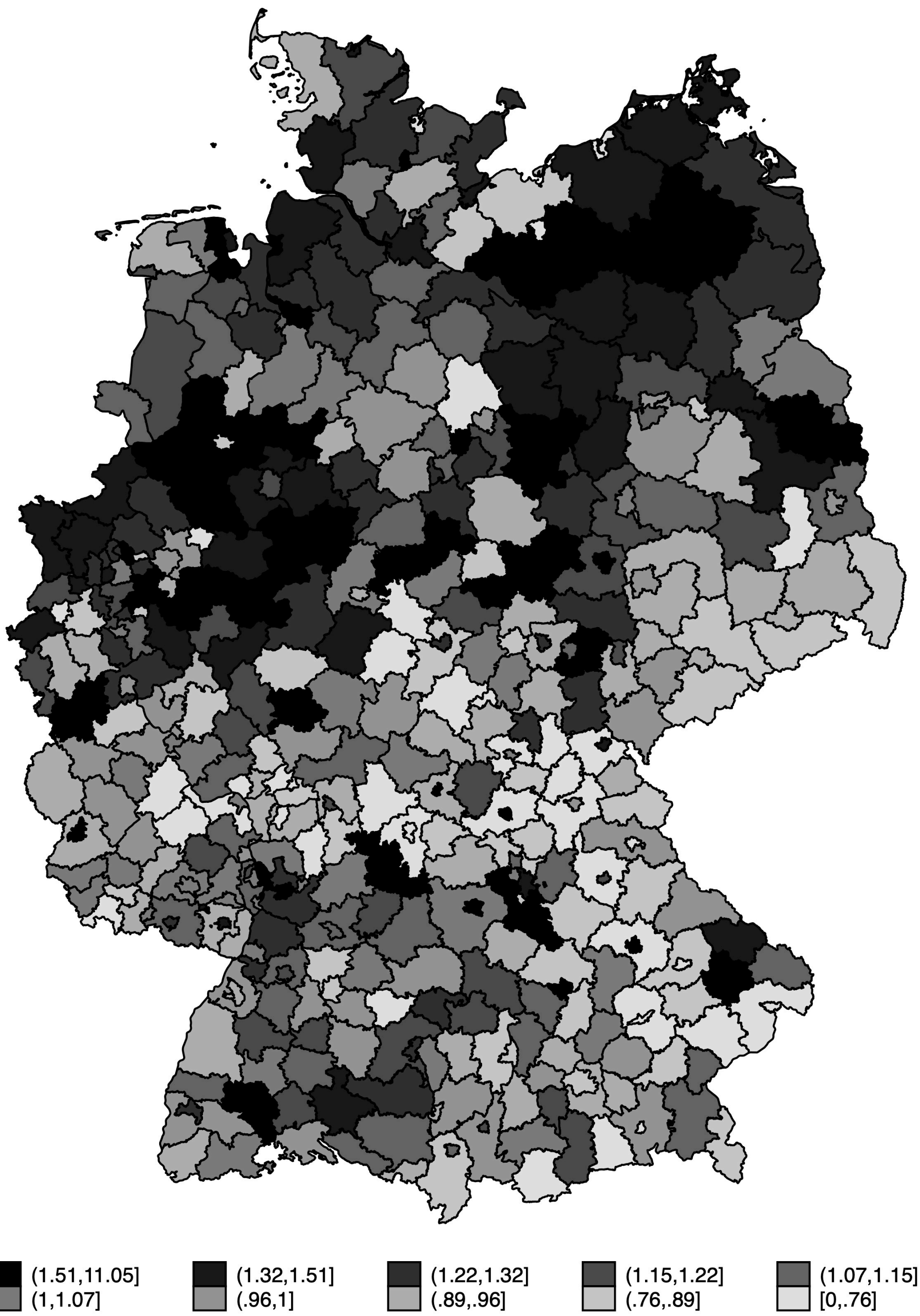
1.A Tables and Graphs (Appendix A)

Figure 1.A1 Change in Asylum Seeker Share by Number of Observations



Note: Scatterplot of change in asylum seeker share in percentage points against number of observations per county. Each dot represents a county, with N = 261.

Figure 1.A2 Spatial Distribution of Asylum Seekers



Note: Distribution of recipients of asylum seekers' benefits at the end of 2015.
Source: Statistical offices of the *Bundesländer*.

Table 1.A1 Refugee Allocation Rules by State

Bundesland	Allocation Regime	Quota based on
Baden-Württemberg	3-Stage	Population Size
Bayern	3-Stage	Legal Decree
Berlin	1-Stage	Local Authorities + Non-State Actors
Brandenburg	2-Stage	Population Size + Number of Employees
Bremen	2-Stage	State Law
Hamburg	1-Stage	State Agency
Hessen	2-Stage	Population Size
Mecklenburg-Vorpommern	2-Stage	Population Size
Niedersachsen	2-Stage	Population Size
Nordrhein-Westfalen	2-Stage	Population Size + Area Size
Rheinland-Pfalz	2-Stage	Population Size
Saarland	2-Stage	Population Size
Sachsen	2-Stage	Population Size
Sachsen-Anhalt	2-Stage	Population Size
Schleswig-Holstein	3-Stage	Legal Decree
Thüringen	2-Stage	Legal Decree

Source: Geis and Orth (2016), Wendel (2014)

Table 1.A2 Effects of Migrant Identification on Refugee Placement

	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta_{2012-2014}$ Feel German	-0.00760					
	(0.0123)					
$\Delta_{2010-2012}$ Feel German		-0.0111				
		(0.0120)				
$\Delta_{2010-2014}$ Feel German			0.00501			
			(0.0120)			
$\Delta_{2010-2014}$ Connect Home				-0.0114		
				(0.0114)		
$\Delta_{2012-2014}$ Connect Home					-0.00816	
					(0.0129)	
$\Delta_{2010-2012}$ Connect Home						-0.00848
						(0.0127)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	2691	869	897	2691	869	897

Note: Dependent variable is always the change in refugee share between 2014 and 2015. Column (1): Regressed on change in feeling German between 2012 and 2014. Column (2): Regressed on change in feeling German between 2010 and 2012. Column (3): Regressed on change in feeling German between 2010 and 2014. Column (4): Regressed on change in home country attachment between 2012 and 2014. Column (5): Regressed on change in home country attachment between 2010 and 2012. Column (6): Regressed on change in home country attachment between 2010 and 2014. All regressions include plausibly exogenous regressors, mentioned on page 12. Unbalanced sample is used in estimations. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.A3 Effects of Migrant Social and Economic Integration on Refugee Placement

	(1)	(2)	(3)	(4)	(5)
Naturalizations	-0.288				
	(0.926)				
Intermarriages		0.0597			
		(0.0828)			
Foreign. Gym. Rep.			-0.000245		
			(0.000445)		
Foreig. Unemp. Rate				-0.00185	
				(0.00154)	
Foreign. SSC Empl. Rate					-0.00156
					(0.00126)
County FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
N	1600	1869	1995	1978	1995

Note: Dependent variable is always the refugee share in county c at time t. Column (1): Regressed on naturalizations over foreign population. Column (2): Regressed on intermarriages over all marriages with at least one foreign spouse. Column (3): Regressed on measure of migrant representation among *Gymnasium* students, with 100 indicating equal representation compared to Germans. Column (4): Regressed on foreign unemployment rate. Column (5): Regressed on rate of foreigner in employment subject to social security contributions. All regressions contain observations for all years between 2010 and 2014 and include county and time fixed effects. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.A4 Effects of Migrant Demographics on Refugee Placement

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Africa %	1.253						
	(0.875)						
Asia %		0.409					
		(0.307)					
Europe %			-0.461				
			(0.299)				
TMENA %				0.948			
				(0.879)			
America %					-1.034		
					(0.849)		
Foreign Pop %						0.864	
						(0.635)	
Asylum Seeker % in t-1							52.17***
							(12.19)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1963	1963	1963	1963	1963	1995	1995

Note: Dependent variable is always the refugee share in county c at time t. Column (1): Regressed on share of Africans over total foreign population. Column (2): Regressed on share of Asians over total foreign population. Column (3): Regressed on share of Europeans over total foreign population. Column (4): Regressed on share of migrants from Turkey, Middle East and North Africa over total foreign population. Column (5): Regressed on share of Americans over total foreign population. Column (6): Regressed on foreign population share. Column (7): Regressed on share of asylum seekers in period before. All regressions contain observations for all years between 2010 and 2014 and include county and time fixed effects. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.A5 Effects of Migrant Social Security Reception on Refugee Placement

	(1)	(2)	(3)	(4)
Hilfe zum Lebensunterhalt	-1.217			
	(1.736)			
Hilfe bei bes. Lebensl.		-3.275***		
		(0.869)		
Grundsicherung			-2.996***	
			(0.964)	
Mindestsicherung				-0.424**
				(0.149)
County FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
N	1925	1936	1971	1989

Note: Dependent variable is always the refugee share in county c at time t. Regressed on share of foreigners receiving... Column (1): *Hilfe zum Lebensunterhalt*, assistance for some people who are unable to work. Column (2): *Hilfe bei besonderen Lebenslagen*, assistance for sick or disabled people or those facing special social hardships. Column (3): *Grundsicherung*, basic social security, mostly paid to low-income retirees. Column (4): *Mindestsicherung*, a minimum guaranteed income. All regressions contain observations for all years between 2010 and 2014 and include county and time fixed effects. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.A6 Effect by Country of Birth and Arrival Characteristics

	(1) German-born	(2) Foreign-born	(3) Minor at Arrival	(4) Adult at Arrival	(5) Arrived before 1990	(6) Arrived 1990 or later
Panel A: Feel German						
Post	0.074	0.235***	0.178**	0.280***	0.053	0.346***
	(0.068)	(0.045)	(0.077)	(0.061)	(0.054)	(0.070)
Post * Δ Ref_share	0.002	-0.009	0.011	-0.027	0.107**	-0.045**
	(0.046)	(0.017)	(0.056)	(0.034)	(0.047)	(0.019)
Panel B: Connect Home						
Post	-0.115	-0.012	-0.065	-0.015	0.030	-0.003
	(0.093)	(0.040)	(0.070)	(0.048)	(0.058)	(0.062)
Post * Δ Ref_share	-0.099	0.074***	0.055	0.099***	-0.084*	0.124***
	(0.070)	(0.017)	(0.040)	(0.021)	(0.049)	(0.042)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Feel German	3.13	2.61	2.90	2.45	2.71	2.51
Mean Connect Home	2.11	2.14	1.90	2.27	2.18	2.13
N	1180	4204	1304	2816	1780	2340

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. All regressions are specified as those in Table 1.3, column (2). Subsample regressions. Column (1): Respondents are German-born. Column (2): Foreign-born migrants. Column (3): Foreign-born migrants who came to Germany before they turned 18. Column (4): Foreign-born migrants who came to Germany as adults. Column (5): Foreign-born migrants who came to Germany before 1990. Column (6): Foreign-born migrants who came to Germany in 1990 or later. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.A7 Effect by Education

	(1) Low	(2) Medium	(3) High
Panel A: Feel German			
Post	0.209**	0.182***	0.306***
	(0.084)	(0.043)	(0.062)
Post * Δ Ref_share	-0.047	-0.004	-0.004
	(0.035)	(0.022)	(0.029)
Panel B: Connect Home			
Post	-0.003	-0.167***	0.029
	(0.078)	(0.051)	(0.068)
Post * Δ Ref_share	0.142***	0.054	0.023
	(0.031)	(0.038)	(0.024)
Basic Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Mean Feel German	2.52	2.87	2.67
Mean Connect Home	2.21	2.05	2.19
N	1376	2504	1408

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. All regressions are specified as those in Table 1.3, column (2). Subsample regressions. Column (1): Respondents have low educational attainment, i.e., they attained a degree from the intermediate secondary school (*Realschule*) or lower or did not graduate. Column (2): Respondents have medium educational attainment, i.e., an upper secondary school degree giving access to university studies (*Abitur*), a certificate of aptitude for specialized short-course higher education (*Fachhochschulreife*), finished an apprenticeship (*Lehre* or attained a degree from a specialized vocational school (*Berufsfachschule*). Column (3): Respondents have high educational attainment, i.e., a university degree or similar. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.A8 Worries

	(1) Crime	(2) Job Security	(3) Immigration	(4) Xenophobia
Post	0.180***	-0.163***	0.419***	0.114***
	(0.035)	(0.020)	(0.031)	(0.033)
Post * Δ Refugee %	0.009	0.009	-0.004	0.035*
	(0.018)	(0.016)	(0.015)	(0.020)
Basic Controls	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Outcome Means	1.10	0.70	0.80	1.01
N	5384	5384	5384	5384

Note: Apart from dependent variable, all regressions are specified as those in Table 1.3, column (2). The respective outcome is ... Column (1): worries about crime. (2): worries about job security. (3): worries about immigration. (4): worries about xenophobia. All outcomes are scaled from 0 (not concerned) to 2 (very concerned). *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for each outcome for the years 2012 and 2014. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.A9 Experience of Discrimination by Origin

	(1) E European: No Discrim.	(2) E European: Exp. Discrim.	(3) TMENA: No Discrim.	(4) TMENA: Exp. Discrim.
Panel A: Feel German				
Post	0.200***	0.403***	-0.093	0.150
	(0.065)	(0.095)	(0.142)	(0.134)
Post * Δ Ref_share	-0.008	-0.090***	-0.018	-0.120***
	(0.026)	(0.031)	(0.062)	(0.040)
Panel B: Connect Home				
Post	-0.002	-0.080	-0.048	0.050
	(0.060)	(0.106)	(0.223)	(0.119)
Post * Δ Ref_share	0.032	0.172***	0.116	-0.015
	(0.035)	(0.056)	(0.110)	(0.032)
Basic Controls	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Mean Feel German	3.22	2.68	2.41	2.40
Mean Connect Home	1.53	1.94	2.46	2.49
N	1200	768	348	576

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. All regressions are specified as those in Table 1.3, column (2). Subsample regressions. Column (1): Respondents were Eastern Europeans and reported in 2013 that they never felt disadvantaged in the last two years due to their ethnic origins. Column (2): Respondents were Eastern Europeans and reported in 2013 that they seldom or often felt disadvantaged in the last two years due to their ethnic origins. Column (3): As in column (1), but respondents were TMENA migrants. Column (4): As in column (2), but respondents were TMENA migrants. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.A10 Language of Media Consumed by Origin

	(1) Western: Only German	(2) Western: At Least Some Foreign	(3) E Europe: Only German	(4) E Europe: At Least Some Foreign	(5) TMENA: Only German	(6) TMENA: At Least Some Foreign
Panel A: Feel German						
Post	0.188*	0.125	0.160**	0.314***	-0.036	0.163
	(0.096)	(0.120)	(0.061)	(0.075)	(0.172)	(0.113)
Post * Δ Ref_share	0.046	0.188	-0.018	-0.061*	-0.082	-0.185***
	(0.042)	(0.142)	(0.024)	(0.031)	(0.061)	(0.050)
Panel B: Connect Home						
Post	-0.074	0.089	-0.028	-0.021	0.074	-0.061
	(0.135)	(0.108)	(0.074)	(0.078)	(0.360)	(0.110)
Post * Δ Ref_share	-0.047	-0.102	0.021	0.129***	-0.079	0.130*
	(0.052)	(0.108)	(0.048)	(0.042)	(0.049)	(0.066)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Feel German	3.14	2.17	3.44	2.72	3.15	2.26
Mean Connect Home	2.09	2.75	1.23	1.97	1.81	2.62
N	480	864	800	1196	144	780

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. All regressions are specified as those in Table 1.3, column (2). Subsample regressions. Column (1): Respondents were of Western origin and reported in 2014 that they only consumed news media in German. Column (2): Respondents were of Western origin and reported in 2014 that they only consumed at least some foreign news media. Column (3): As in column (1), but for Eastern European migrants. Column (4): As in column (2), but for Eastern European migrants. Column (5): As in column (1), but for TMENA migrants. Column (6): As in column (2), but for TMENA migrants. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.A11 Language with Family and Friends

	(1) German w/ Family	(2) HC Language w/ Family	(3) German w/ Friends	(4) HC Language w/ Friends
Panel A: Feel German				
Post	0.228***	0.153	0.223***	0.126
	(0.038)	(0.096)	(0.035)	(0.140)
Post * Δ Ref_share	-0.019	-0.017	-0.015	0.026
	(0.020)	(0.049)	(0.017)	(0.132)
Panel B: Connect Home				
Post	-0.064	0.062	-0.083**	0.171
	(0.041)	(0.078)	(0.038)	(0.114)
Post * Δ Ref_share	0.066***	0.041	0.067***	-0.018
	(0.018)	(0.044)	(0.016)	(0.122)
Basic Controls	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Mean Feel German	2.90	2.06	2.85	2.07
Mean Connect Home	2.01	2.57	2.05	2.52
N	4176	1084	4472	788

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. All regressions are specified as those in Table 1.3, column (2). Subsample regressions. Column (1): Respondents reported in 2015 that they mainly spoke German with family members. Column (2): Respondents reported in 2015 that they mainly spoke with family members in the language of their country of origin. Column (3): Respondents reported in 2015 that they mainly spoke German with friends. Column (4): Respondents reported in 2015 that they mainly spoke with friends in the language of their country of origin. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.A12 Effects on Labor Market Outcomes

	(1) Log Income	(2) Annual Hours	(3) Unemployment	(4) Education in Years
Post	0.440***	0.049*	-0.054***	0.162***
	(0.156)	(0.028)	(0.011)	(0.051)
Post * Δ Ref_share	0.028	-0.008	-0.004	-0.010
	(0.125)	(0.014)	(0.004)	(0.018)
Basic Controls	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Outcome Mean	6.77	1.18	0.10	11.02
N	5384	5384	5384	5384

Note: Apart from dependent variable, all regressions are specified as those in Table 1.3, column (2). Column (1): Outcome is individual income (logged). Column (2): Outcome is annual hours worked. Column (3): Outcome is whether respondent is unemployed (linear probability model). Column (4): Outcome is years of education. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for each outcome for the years 2012 and 2014. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.A13 Effects on Party Preferences by Country of Origin

	(1) Pol. Int.	(2) Pref.	(3) Mod. Cons.	(4) Mod. Left	(5) Linke	(6) AfD
Panel A: Western						
Post	0.009	-0.008	-0.001	-0.072**	0.036**	0.025***
	(0.033)	(0.036)	(0.017)	(0.029)	(0.016)	(0.009)
Post * Δ Ref_share	0.010	0.030*	-0.008	0.051***	-0.003	-0.004
	(0.018)	(0.018)	(0.008)	(0.014)	(0.005)	(0.004)
Panel B: E European						
Post	0.047	0.022	0.013	-0.035*	0.006	0.024*
	(0.032)	(0.026)	(0.022)	(0.019)	(0.007)	(0.013)
Post * Δ Ref_share	0.007	0.001	-0.034***	0.016	0.001	0.018**
	(0.018)	(0.017)	(0.011)	(0.014)	(0.003)	(0.007)
Panel C: TMENA						
Post	-0.059	-0.053	0.013	-0.012	-0.047*	0.008
	(0.040)	(0.052)	(0.025)	(0.044)	(0.024)	(0.007)
Post * Δ Ref_share	0.050***	0.043***	-0.003	-0.023**	0.059***	0.0001
	(0.012)	(0.014)	(0.006)	(0.010)	(0.016)	(0.0003)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Mean (Panel A)	0.40	0.30	0.09	0.19	0.01	0.00
Outcome Mean (Panel B)	0.29	0.30	0.20	0.07	0.02	0.00
Outcome Mean (Panel C)	0.33	0.28	0.03	0.18	0.04	0.00
N (Panel A)	1276	1376	1376	1376	1376	1376
N (Panel B)	1874	2056	2056	2056	2056	2056
N (Panel C)	847	952	952	952	952	952

Note: Panel A: Sample consists of Western migrants. Panel B: Sample consists of Eastern Europeans. Panel C: Sample consists of TMENA migrants. Apart from dependent variable, all regressions are linear probability models specified as those in Table 1.3, column (2). Column (1): Outcome is whether respondent is moderately or very interested in politics (=1) or not (=0). Column (2): Outcome is whether respondent has a preference for a political party (=1) or not (=0). Column (3): Outcome is whether respondent prefers a moderately conservative party (*CDU*, *CSU* or *FDP*). Column (4): Outcome is whether respondent prefers a moderately left-wing party (*SPD* or *Bündnis 90/Die Grünen*). Column (5): Outcome is whether respondent prefers *Die Linke*. Column (6): Outcome is whether respondent prefers *AfD*. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for each outcome for the years 2012 and 2014. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.B Classification into Migrant Groups (Appendix B)

Generally, I split migrants into three main groups: Western, Eastern European, and TMENA migrants (migrants from Turkey, the Middle East, and North Africa). Additionally, there are two smaller groups: migrants from Balkan countries and the residual "rest of world" category. Moreover, I split Eastern Europeans into resettlers and non-resettlers to examine the effects on the large and important migrant group of ethnic Germans.

For the most part, classifications are made using data about the respondents' country of origin and first nationality. Thus, e.g., if a respondent was born in France, I categorize them as 'Western'. Moreover, I do not make a judgement call when a respondent is part of two or more groups. Hence, e.g., if a respondent is a French citizen but born in Algeria, they are categorized as both a Western and a TMENA migrant. If a respondent is German-born with German citizenship, I additionally look at second and past nationality, as well as the father's and mother's nationality and country of origin. Still, 20 respondents could not be matched to one of the groups, as they were reported to be German citizens born in Germany without further information. These were excluded from the subsample analysis, but not the main analysis. To give readers an impression of the countries included in each group and how large each origin group is, Table 1.B1 displays the number of migrants by country of origin. This, of course, only gives an imperfect overview, as it does not include nationality and other indicators of origin of the migrants. While there is a wide range of countries included, the largest groups of people (after Germany) are from Italy and Austria, Turkey, Kazakhstan, Russia, and Poland.

Table 1.B2 shows descriptive statistics of the main origin groups mentioned above for host country (Panel A) and home country connection (Panel B). Eastern Europeans are the largest group with somewhat over 2,000 observations in total, and also the group that on average feels the most German at around 3.1 (looking at the mean for all years). Two thirds of them are resettlers and one third non-resettlers with mean values of 3.2 and 2.8, respectively. Eastern Europeans are followed by Balkan area migrants (2.9), Westerners at 2.6, and TMENA migrants (2.4). Moreover, the latter are the only that did not show any real upward trajectory while all other groups reported increased attachment. In Panel B, the differences between groups are basically the opposite of

what can be observed in the panel above, with groups who feel a strong belonging to Germany exerting only sparse home country connection, and vice versa. Eastern Europeans show the lowest home country attachment at 1.7 (with mean values of 1.5 for resettlers and 2.0 for non-resettlers), followed by migrants from the Balkans (1.9). The other two groups are somewhat farther apart at around 2.5. Remarkably, there is very little change within most of the groups over time (apart from Balkans), with values stagnating between 2012 and 2018.

Table 1.B1 Migrants by Country of Origin

	N	%	Group
Germany	1180	21.92	-
Italy	204	3.79	Western
Austria	108	2.01	Western
Greece	88	1.63	Western
The Netherlands	72	1.34	Western
USA	68	1.26	Western
Spain	64	1.19	Western
France	60	1.11	Western
Great Britain	48	0.89	Western
Switzerland	32	0.59	Western
Portugal	24	0.45	Western
Denmark	20	0.37	Western
Finland	16	0.30	Western
Belgium	16	0.30	Western
Ireland	12	0.22	Western
Sweden	8	0.15	Western
Luxembourg	4	0.07	Western
Turkey	468	8.69	TMENA
Iran	48	0.89	TMENA
Morocco	32	0.59	TMENA
Lebanon	20	0.37	TMENA
Iraq	12	0.22	TMENA
Syria	12	0.22	TMENA
Tunisia	12	0.22	TMENA
Palestine	8	0.15	TMENA
Algeria	4	0.07	TMENA
UAE	4	0.07	TMENA
Kazakhstan	480	8.92	Eastern Europe
Russia	444	8.25	Eastern Europe
Poland	412	7.65	Eastern Europe
Romania	192	3.57	Eastern Europe
Ukraine	92	1.71	Eastern Europe
Czech Republic	44	0.82	Eastern Europe
Kyrgyzstan	44	0.82	Eastern Europe
Hungary	40	0.74	Eastern Europe
Bulgaria	24	0.45	Eastern Europe
Slovakia	24	0.45	Eastern Europe
Tajikistan	16	0.30	Eastern Europe
Azerbaijan	16	0.30	Eastern Europe
Belarus	16	0.30	Eastern Europe
Uzbekistan	8	0.15	Eastern Europe
Estonia	8	0.15	Eastern Europe
Latvia	8	0.15	Eastern Europe
Lithuania	8	0.15	Eastern Europe
Georgia	4	0.07	Eastern Europe
Serbia	124	2.30	Balkan
Kosovo-Albania	112	2.08	Balkan
Bosnia-Herzegovina	88	1.63	Balkan
Croatia	76	1.41	Balkan
Ex-Yugoslavia	28	0.52	Balkan
Macedonia	28	0.52	Balkan
Albania	20	0.37	Balkan
Slovenia	20	0.37	Balkan
Montenegro	4	0.07	Balkan

Table 1.B1: Migrants by Country of Origin (cont.)

	N	%	Group
Thailand	32	0.59	Rest of World
Philippines	28	0.52	Rest of World
Sri Lanka	28	0.52	Rest of World
Peru	20	0.37	Rest of World
Afghanistan	16	0.30	Rest of World
Argentina	16	0.30	Rest of World
Bangladesh	12	0.22	Rest of World
Brazil	12	0.22	Rest of World
China	12	0.22	Rest of World
Columbia	12	0.22	Rest of World
Cuba	12	0.22	Rest of World
Japan	12	0.22	Rest of World
Cameroon	8	0.15	Rest of World
Ethiopia	8	0.15	Rest of World
Ghana	8	0.15	Rest of World
India	8	0.15	Rest of World
Indonesia	8	0.15	Rest of World
Jamaica	8	0.15	Rest of World
Mexico	8	0.15	Rest of World
Nigeria	8	0.15	Rest of World
Pakistan	8	0.15	Rest of World
Togo	8	0.15	Rest of World
Vietnam	8	0.15	Rest of World
Angola	4	0.07	Rest of World
Cambodia	4	0.07	Rest of World
Dominican Republic	4	0.07	Rest of World
El Salvador	4	0.07	Rest of World
Gambia	4	0.07	Rest of World
Israel	4	0.07	Rest of World
Kenya	4	0.07	Rest of World
Korea	4	0.07	Rest of World
Mauritius	4	0.07	Rest of World
Mozambique	4	0.07	Rest of World
South Africa	4	0.07	Rest of World
Surinam	4	0.07	Rest of World
Uruguay	4	0.07	Rest of World
Venezuela	4	0.07	Rest of World
Zimbabwe	4	0.07	Rest of World
Total	5,384	100.00	

Note: List of countries of origin included in the balanced sample with information on total number of observations, share overall, and classification in group.

Table 1.B2 Descriptive Statistics by Country of Origin

Panel A: Feel German		2012	2014	2016	2018	Total
	Mean	2.493	2.572	2.643	2.767	2.619
Western	(SD)	(1.232)	(1.165)	(1.161)	(1.080)	(1.164)
	N	344	344	344	344	1376
	Mean	2.980	3.063	3.172	3.191	3.102
Eastern Europe	(SD)	(1.097)	(0.998)	(0.888)	(0.893)	(0.976)
	N	514	514	514	514	2056
	Mean	2.853	2.741	2.941	2.982	2.879
Balkan	(SD)	(1.070)	(1.039)	(0.953)	(0.970)	(1.011)
	N	170	170	170	170	680
	Mean	2.426	2.369	2.487	2.441	2.431
TMENA	(SD)	(1.131)	(1.033)	(1.070)	(1.104)	(1.084)
	N	238	238	238	238	952
	Mean	2.628	2.737	2.947	2.965	2.819
Non-Resettlers	(SD)	(1.224)	(1.220)	(0.978)	(0.996)	(1.118)
	N	173	173	173	173	692
	Mean	3.159	3.226	3.284	3.306	3.244
Resettlers	(SD)	(0.981)	(0.819)	(0.818)	(0.813)	(0.862)
	N	341	341	341	341	1364
Panel B: Connect Home		**2012**	**2014**	**2016**	**2018**	**Total**
	Mean	2.523	2.496	2.507	2.506	2.508
Western	(SD)	(1.044)	(1.044)	(1.022)	(1.020)	(1.031)
	N	344	344	344	344	1376
	Mean	1.637	1.705	1.725	1.655	1.681
Eastern Europe	(SD)	(1.110)	(1.155)	(1.161)	(1.143)	(1.142)
	N	514	514	514	514	2056
	Mean	1.970	2.035	1.894	1.835	1.934
Balkan	(SD)	(1.245)	(1.206)	(1.157)	(1.139)	(1.187)
	N	170	170	170	170	680
	Mean	2.415	2.572	2.489	2.473	2.487
TMENA	(SD)	(1.147)	(1.095)	(1.095)	(1.114)	(1.113)
	N	238	238	238	238	952
	Mean	1.953	1.988	2.012	1.935	1.972
Non-Resettlers	(SD)	(1.251)	(1.188)	(1.223)	(1.126)	(1.196)
	N	173	173	173	173	692
	Mean	1.478	1.562	1.581	1.515	1.534
Resettlers	(SD)	(0.996)	(1.113)	(1.102)	(1.127)	(1.085)
	N	341	341	341	341	1364

Note: Means, standard errors, and observations of feeling German (Panel A) and home country connection (Panel B) for the years 2012, 2014, 2016, and 2018 by country of origin. The outcome is scaled from 0 to 4. Data source: German Socio-economic Panel.

1.C Robustness (Appendix C)

In this section, I discuss the other robustness checks of my estimations in more detail, focusing on outliers, omitted variable bias, clustering of standard errors, and alternative treatment definitions.

As Figure 1.A1 shows, there are a few counties that received disproportionately more refugees than others. In particular, three counties saw much larger increases in refugee accommodation with 9.2, 7.1, and 4.1 percentage points. Respondents in those counties could have an outsized influence in driving the main results. One possible approach to deal with such outliers is dropping them, even though this is problematic as it introduces statistical bias in the case that the outliers are genuine or at least relatively close to the true values (Ghosh and Vogt, 2012). This is particularly the case in instances where there is not a lot of variation in the explanatory variable, as in the case of refugee placement.

Nevertheless, columns (2) to (4) of Table 1.C1 show results when I gradually drop outliers. In (2), I drop two largest-inflow counties, and in (3), I additionally drop the third largest-inflow county. In the last two columns, I additionally drop counties using rule-of-thumb thresholds. In (4), counties with increases in refugee shares above $mean + 3 * SD (= 2.826)$ are dropped, in (5), counties with shares higher than $mean + 2 * SD (= 2.141)$ are removed. Generally, dropping high-inflow counties has two effects: On one side, the statistical significance declines. This decrease is caused as dropping the outlying observations curtails the variation in the treatment variable considerably. Nevertheless, results remain significant at a 10 percent level. On the other side, the magnitude of the coefficients increases markedly. Thus, as coefficients react rather sensitively to the exclusion of these outliers, they appear to get higher. While we should be careful and not draw too many conclusions from that, it is possible that the treatment effects are not linear but decreasing for higher-inflow counties. This could suggest that dynamics change if refugees are a larger group in the local society and contact between migrants and refugees becomes more likely.

Another possible, though again imperfect, way to deal with outliers is to winsorize them, meaning to cap outliers at a certain level. Table 1.C2 shows results when outliers are winsorized at the 1% (2), 2% (3), and 5% (4) upper level of the distribution. Again, coefficients increase in magnitude and lose statistical significance the lower the cap.

In my main regression, I treat my outcomes as if they were cardinally-scaled. This may be problematic, as I assume that the degree of difference between the proposed options is identical in the eyes of the respondents, which might not be the case. Although I already showed that the results generally hold when using binary logit methods, it might be prudent to additionally estimate the regressions using ordinal logit methods. Due to the limited sample sizes, I again estimate the two baseline regressions without any controls, and only include individual and time fixed effects.[39] Standard errors, again, are clustered at the county level. The results can be seen in Table 1.C3. Generally, they are in line with the results in Tables 1.3 and 1.4, showing insignificant effects for host country and significantly positive effects for the most affirmative response options regarding home country attachment. At the sample average, respondents became significantly more likely to report either strong or very strong home country connection in counties with higher proportions of asylum seekers.

Next, I discuss further robustness checks, assessing whether omitted variables might be a problem. While I already provide some evidence against it in Table 1.3, columns (3) and (4), there might be further potential confounders not controlled for. Therefore, I run additional regressions controlling for further potential influences. Results in Table 1.C4 illustrate that neither the inclusion of interviewer fixed effects, the inclusion of tragic or potentially traumatizing events such as deaths of relatives or separation from partner nor controls for movement within counties meaningfully alter the main coefficients.

To control for selective migration and political climate, Table 1.C5 shows regressions including information about cross-county in- and out-migration of Germans and foreigners and voting behavior and turnout by county for federal elections. Again the main results barely move, suggesting that the results are generally robust to the inclusion of these potential confounders.

Next, I check, whether results might be driven by region-specific shocks. For that, I include state-year fixed effects in the regressions in column (2) of Table 1.C6. Overall, this does hardly change the effects for home country attachment.

Going on, I check, whether results also hold when I employ different treatment variables. As mentioned in section 1.4.1, I constructed the treatment variable conservatively, taking the change in asylum seeker share between 2014 and 2015 – and not the total number of asylum seekers in 2015 – due to potential double counting. This, however, may lead to a bias in my treatment, if I systematically undercount asylum seekers in some counties. Therefore, I check whether results are robust, when I, first,

[39]Estimations are conducted with Stata 15.1 using the *feologit* command created by Baetschmann et al. (2020).

include the total number of asylum seekers in 2015 (1) and, second, the combined number of asylum seekers in 2014 and 2015 (2). The first two columns of Table 1.C7 show that results remain robust. On a similar note, it might be problematic to look at the change in refugee share between 2014 and 2015 if some counties have already received a lot of refugees between 2013 and 2014, thereby limiting the number of additional refugees that could be housed in the following years. Therefore, I estimate the main regression equations using the change in refugee share between 2013 and 2015. Results in Table 1.C7, column (3), show that this only affects results slightly, lowering coefficients for home country attachment. In column (4), I divide the number of asylum seekers not by the overall population, but only by the working age population. Again, this hardly changes estimates, only mechanically reducing them with the rate by which changes in refugee concentration are increased due to the smaller denominator of the treatment variable. Overall, results appear robust to using a different treatment variable.

A last issue with my main estimations may lie in the clustering of standard errors I have chosen. While I have followed Abadie et al. (2017) in clustering my standard errors at the level of the treatment before, it might be prudent to examine, whether results hold with other levels of clustering. First, I check whether results change when I employ a more conservative clustering at the state level in Table 1.C8, column (1). Then I check whether results hold when I cluster at the individual level (2), as in my main estimations, I employ individual fixed effects. Lastly, in columns (3) and (4), I check whether my results still hold when I cluster at the level where the sampling by the SOEP took place, namely the household. Unfortunately, household IDs change whenever respondents move or switch households within counties. Therefore, I first cluster at the level of original household, meaning where the household respondents lived in when they were first surveyed by the SOEP (3). As this may not be adequate for respondents, who have switched households thereafter, I cluster standard errors at the level of the household in which respondents lived in 2014, the year before the treatment started (4). Results are virtually identical when clustering at the level of the household in which respondents lived in 2012, 2016, and 2018 (not shown). Results in Table 1.C8 show, that, while the significance of the effects is a bit smaller, effects in Panel B are still significant at conventional levels in all columns.

Table 1.C1 Dropping Outliers

	(1) Baseline	(2) $\Delta < 5$	(3) $\Delta < 4$	(4) Threshold 1	(5) Threshold 2
Panel A: Feel German					
Post	0.202***	0.180***	0.177***	0.183***	0.168**
	(0.038)	(0.050)	(0.057)	(0.066)	(0.067)
Post * Δ Ref_share	-0.007	0.023	0.027	0.017	0.043
	(0.015)	(0.050)	(0.065)	(0.082)	(0.083)
Panel B: Connect Home					
Post	-0.048	-0.095**	-0.110**	-0.114*	-0.114*
	(0.036)	(0.044)	(0.049)	(0.060)	(0.063)
Post * Δ Ref_share	0.063***	0.132**	0.152**	0.157*	0.156*
	(0.018)	(0.052)	(0.066)	(0.086)	(0.092)
Basic Controls	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Mean Feel German	2.72	2.72	2.72	2.73	2.73
Mean Connect Home	2.13	2.13	2.13	2.13	2.13
N	5384	5340	5328	5304	5296

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. Column (1): Baseline estimation as in Table 1.3, column (2). (2): Drops counties with increase in asylum seekers of at least 5 percentage points. (3): Drops counties with increase in asylum seekers of at least 4 percentage points. (4): Drops counties with increase in asylum seekers of $mean + 3 * SD = .771 + 3 * .685 = 2.826$. (5): Drops counties with increase in asylum seekers of $mean + 2 * SD = .771 + 2 * .685 = 2.141$. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.C2 Winsorizing Outliers

	(1) Baseline	(2) Top 1%	(3) Top 2%	(4) Top 5%
Panel A: Feel German				
Post	0.202***	0.198***	0.185***	0.193***
	(0.038)	(0.043)	(0.059)	(0.071)
Post * Δ Ref_share	-0.007			
	(0.015)			
Post * Winsor_Top_1%		-0.002		
		(0.032)		
Post * Winsor_Top_2%			0.016	
			(0.066)	
Post * Winsor_Top_5%				0.005
				(0.090)
Panel B: Connect Home				
Post	-0.048	-0.081**	-0.126**	-0.144**
	(0.036)	(0.038)	(0.052)	(0.071)
Post * Δ Ref_share	0.063***			
	(0.018)			
Post * Winsor_Top_1%		0.110***		
		(0.030)		
Post * Winsor_Top_2%			0.176***	
			(0.068)	
Post * Winsor_Top_5%				0.207*
				(0.106)
Basic Controls	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Mean Feel German	2.72	2.72	2.72	2.72
Mean Connect Home	2.13	2.13	2.13	2.13
N	5384	5384	5384	5384

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. Column (1): Baseline estimation as in Table 1.3, column (2). (2): Winsorizes top 1% highest inflow counties. (3): Winsorizes top 2% highest inflow counties. (4): Winsorizes top 5% highest inflow counties. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.C3 Ordered Logit Regressions

	(1) Feel German	(2) Connect Home
No Answer	-0.0001	-0.0021***
	(0.0008)	(0.0007)
Not at All	-0.0005	-0.0208***
	(0.0037)	(0.0073)
Barely	-0.0009	-0.0250***
	(0.0059)	(0.0088)
In Some Respects	-0.0013	-0.0081***
	(0.0089)	(0.0028)
For the Most Part / Strong	0.0008	0.0289***
	(0.0057)	(0.0102)
Completely / Very Strong	0.0020	0.0270***
	(0.0135)	(0.0095)
Basic Controls		
Individual FE	Yes	Yes
Time FE	Yes	Yes
N	3900	4352

Note: Dependent variables are ordered and concern feeling German in column (1), and home country attachment in (2). Coefficients indicate marginal effects of the interaction *Post* * Δ *Ref_share* for the average respondent, *c.p.*, for the respective level of the dependent variable. All ordered logit regressions include individual and time fixed effects. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. All estimations conducted with Stata 15.1.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.C4 Adding Potential Confounders I

	(1)	(2)	(3)	(4)	(5)
Panel A: Feel German					
Post	0.202***	0.184***	0.202***	0.201***	0.201***
	(0.038)	(0.044)	(0.038)	(0.038)	(0.038)
Post * Δ Ref_share	-0.007	-0.004	-0.008	-0.008	-0.007
	(0.015)	(0.017)	(0.015)	(0.015)	(0.015)
Panel B: Connect Home					
Post	-0.048	-0.053	-0.058	-0.052	-0.048
	(0.036)	(0.042)	(0.036)	(0.036)	(0.036)
Post * Δ Ref_share	0.063***	0.058***	0.064***	0.064***	0.063***
	(0.018)	(0.017)	(0.018)	(0.018)	(0.018)
Basic Controls	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Interviewer FE		Yes			
Death of Relative Controls			Yes	Yes	
Separation Control				Yes	
Move Controls					Yes
Mean Feel German	2.72	2.72	2.72	2.72	2.72
Mean Connect Home	2.13	2.13	2.13	2.13	2.13
N	5384	5384	5384	5384	5384

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. Column (1): Baseline estimation as in Table 1.3, column (2). The following columns include additional potential confounders. (2): Regressions include interviewer fixed effects. (3): Regressions include dummies indicating death of partner, child, mother, father and other household member in last 2 years. (4): Regressions including controls as in (2) and dummy indicating separation from partner in last 2 years. (5): Regressions include dummy indicating if respondent left household and moved within county. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.C5 Adding Potential Confounders II

	(1)	(2)	(3)	(4)	(5)
Panel A: Feel German					
Post	0.202***	0.200***	0.204***	0.152	0.132
	(0.038)	(0.041)	(0.042)	(0.123)	(0.122)
Post * Δ Ref_share	-0.007	-0.007	-0.008	-0.006	-0.009
	(0.015)	(0.016)	(0.018)	(0.015)	(0.013)
Panel B: Connect Home					
Post	-0.048	-0.041	-0.041	0.073	0.084
	(0.036)	(0.040)	(0.040)	(0.135)	(0.136)
Post * Δ Ref_share	0.063***	0.063***	0.062***	0.066***	0.068***
	(0.018)	(0.018)	(0.018)	(0.016)	(0.017)
Basic Controls	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
German Mig. Controls		Yes	Yes		
Foreigner Mig. Controls			Yes		
Voting				Yes	Yes
Turnout					Yes
Mean Feel German	2.72	2.72	2.72	2.72	2.72
Mean Connect Home	2.13	2.13	2.13	2.13	2.13
N	5384	5384	5384	5372	5372

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. Column (1): Baseline estimation as in Table 1.3, column (2). The following columns include additional potential confounders. (2): Regressions include the number of Germans migrating into and migrating out of the county as a share of German county population. (3): Regressions as in (2), but also including the number of foreigners migrating into and migrating out of the county as a share of foreign county population. (4): Regressions include vote shares for the major parties (CDU/CSU, SPD, Grüne, FDP, Linke, AfD) in the most recent federal election, respectively. (5): As in (4), but also including turnout rates in the most recent federal election. *Post* indicates time after September 5 2015, *Post* * Δ *Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.C6 Adding Region-Specific Fixed Effects

	(1) Baseline	(2) State-Year FE
Panel A: Feel German		
Post * Δ Ref_share	-0.007	0.002
	(0.015)	(0.014)
Panel B: Connect Home		
Post * Δ Ref_share	0.063***	0.057***
	(0.018)	(0.016)
Basic Controls	Yes	Yes
Individual FE	Yes	Yes
Time FE	Yes	Yes
Year x State FE		Yes
Mean Feel German	2.72	2.72
Mean Connect Home	2.13	2.13
N	5384	5384

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. Column (1): Baseline estimation as in Table 1.3, column (2). Column (2): Regressions also includes state-year fixed effects. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.C7 Alternative Treatment Specifications

	(1)	(2)	(3)	(4)
Panel A: Feel German				
Post	0.209***	0.216***	0.202***	0.202***
	(0.042)	(0.046)	(0.039)	(0.038)
Post * Ref_share (2015)	-0.011			
	(0.015)			
Post * Ref_share (2014 + 2015)		-0.012		
		(0.015)		
Post * $\Delta_{13,15}$ Ref_share			-0.007	
			(0.015)	
Post * Δ Ref_share (WAP)				-0.005
				(0.009)
Panel B: Connect Home				
Post	-0.067*	-0.076*	-0.054	-0.047
	(0.039)	(0.043)	(0.037)	(0.036)
Post * Ref_share (2015)	0.056***			
	(0.017)			
Post * Ref_share (2014 + 2015)		0.046***		
		(0.016)		
Post * $\Delta_{13,15}$ Ref_share			0.058***	
			(0.018)	
Post * Δ Ref_share (WAP)				0.039***
				(0.012)
Basic Controls	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Mean Feel German	2.72	2.72	2.72	2.72
Mean Connect Home	2.13	2.13	2.13	2.13
N	5384	5384	5384	5384

Note:. In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. Apart from using different treatment variables, all regressions are specified as those in Table 1.3, column (2). Column (1): Treatment variable (TV) is the share of asylum seekers in a county in 2015 (Ref_share (2015)). (2): TV is the share of asylum seekers in a county in 2014 plus 2015 (Ref_share (2014 + 2015)). (3): TV is change in asylum seekers over population between 2013 and 2015 (Post * $\Delta_{13,15}$ Ref_share). Column (4): TV is change in asylum seekers over working age population (between 18 and 64) between 2014 and 2015 (Post * Δ Ref_share (WAP)). *Post* indicates time after September 5 2015. *Post* is interacted with the respective TV. Population size is fixed at 2012 levels. Standard errors (in parentheses) are clustered at the county (*Kreis*) level. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.C8 Alternative Clustering of Standard Errors

	(1) State	(2) Respondent ID	(3) Original HH ID	(4) HH ID in 2014
Panel A: Feel German				
Post	0.202***	0.202***	0.202***	0.202***
	(0.040)	(0.033)	(0.035)	(0.036)
Post * Δ Ref_share	-0.007	-0.007	-0.007	-0.007
	(0.010)	(0.021)	(0.022)	(0.022)
Panel B: Connect Home				
Post	-0.048	-0.048	-0.048	-0.048
	(0.041)	(0.037)	(0.039)	(0.040)
Post * Δ Ref_share	0.063**	0.063**	0.063**	0.063**
	(0.024)	(0.028)	(0.031)	(0.031)
Basic Controls	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Mean Feel German	2.72	2.72	2.72	2.72
Mean Connect Home	2.13	2.13	2.13	2.13
N	5384	5384	5384	5384

Note: In Panel A, the dependent variable is to what extent respondent feels German. In Panel B, the dependent variable is to what extent respondent feels connected to home country. Apart from clustering, all regressions are specified as those in Table 1.3, column (2). Column (1): Standard errors clustered at state (*Bundesland*) level. Column (2): Standard errors clustered by respondent ID. Column (3): Standard errors clustered by original household ID. Column (4): Standard errors clustered by household ID in 2014. *Post* indicates time after September 5 2015, *Post * Δ Ref_share* is the interaction of Post with the change in asylum seekers over population between 2014 and 2015. Population size is fixed at 2012 levels. Outcome means are the averages for both outcomes for the years 2012 and 2014. All estimations conducted with Stata 15.1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Chapter 2

Local Far-Right Demonstrations and Nationwide Public Attitudes toward Migration

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2.1 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 mobilize 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 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 behavior towards migrants.

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

[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 favorability 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.

[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.

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 to changes in pro-social behavior 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 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 organization 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 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

[5]Alternatively, we exclude the entire state.

[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 2.3.1.

xenophobic demonstration with those interviewed in the days immediately afterwards. 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 polarization 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 right-of-center parties are more successful. Moreover, at the individual level, we show that only respondents who place themselves left-of-center on the political spectrum show significantly increased worries about hostility towards foreigners. When looking at how changes in attitudes translate to changes in economic and political behavior, we find that following far-right demonstrations, individuals become more politically interested and have stronger party preferences, mainly benefiting left-wing parties. Large xenophobic demonstrations also increase people'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 behavior 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 behavior 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., regarding bandwidth, weights, 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 randomization RDD.[8] Overall, our main conclusions hold.

Our study contributes to several different strands of the literature. First, we add to existing research that analyzes 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

[8]The local randomization 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).

political preferences and voting behavior. 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; Wasow, 2020; Larreboure and Gonzalez, 2021; Klein Teeselink and Melios, 2022). 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 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 the 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 behavior (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 protests differs from traditional left-led protests with regards to the underlying motive, and ethnic and social composition of protesters (Manekin and Mitts, 2022; Eady et al., 2023). 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.

[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 2.B1).

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

Fourthly, we add to extant studies on the determinants of attitudes toward migration. This literature is already very extensive, looking at the influence of various factors, including labor market characteristics, skill composition, bitterness, inequality, and media consumption (Mayda, 2006; Markaki, 2012; Mocan and Raschke, 2016; Poutvaara and Steinhardt, 2018; Benesch et al., 2019; Riaz, 2023). Most of these studies look at attitudes toward immigration more generally or the determinants of xenophobia more specifically. We complement these studies by looking at an outcome that has received only scarce attention thus far, namely concerns about hostility towards foreigners, and are able to put this variable in relation to respondents' worries about immigration.

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; Abdelgadir and Fouka, 2020; Graeber and Schikora, 2021). Similar to this literature, the far-right demonstrations examined 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 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 2.2, we lay out some theoretical considerations on the effect of right-wing xenophobic protests on people's attitudes, and in Section 2.3 we present the data and explain our procedure to select the demonstrations used in the empirical analysis. Section 2.4 explains our empirical strategy and shows some preliminary tests. We show all our main results, robustness checks, and heterogeneity analyses in Section 2.5. In Section 2.6, 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 2.7 concludes.

2.2 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 polarization.

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 favor 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 mobilize 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 organizations and future mobilization 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-center 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 marginalized 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-mobilization 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 favor, 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 polarization. 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 behavior 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

[12] In this analysis, we only look at the extent of newspaper coverage, but do not analyze 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).

a far-right demonstration, worries about hostility towards foreigners increase more and worries about immigration change less for left-leaning respondents than center-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 (non-economically deprived) respondents.

2.3 Data

2.3.1 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 2.1, 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 Labor Day or the bombing of Dresden, but also lists demonstrations that were spontaneously or locally organised, such as protests against asylum seeker centers 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: they 1) were organised spontaneously and/or were of local nature, 2) were larger than usual, and 3) were isolated.

We focus on demonstrations that were organised spontaneously and/or were of local nature such that it is likely that the organization and planning of these right-wing xenophobic demonstrations in one given German district were 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 Labor 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 groups frequently instrumentalize. 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 2.1. 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 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 memorializing the bombing of Dresden.

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

Table 2.1 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.

Table 2.B1 in the appendix 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 examine 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 2.B1 and described in more detail in Appendix 2.B. Generally, most protests received only limited attention from newspapers in advance. For one protest, 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 2.5.2 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.

2.3.2 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 2.1 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 mostly restricted to the few years between 2013 and 2016. Table 2.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, with worries about immigration being slightly lower at 1.97 but with a higher standard deviation of 0.76.

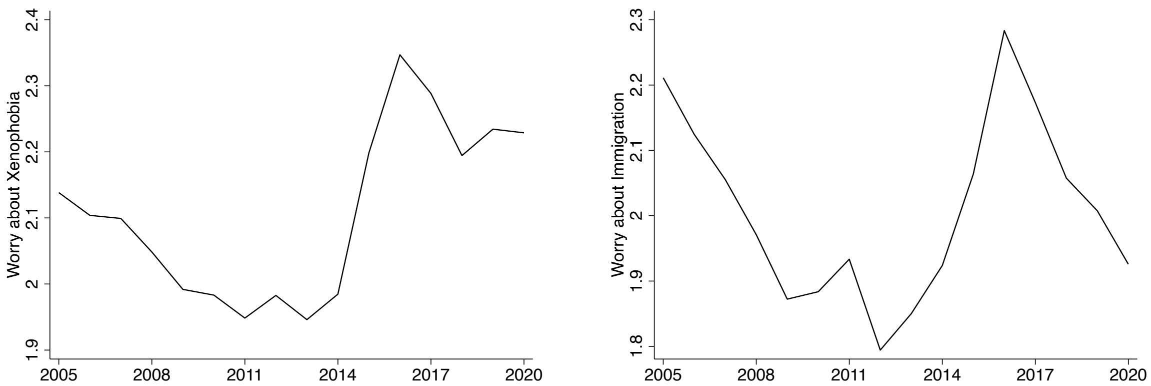
When looking at political behavior, we focus on a variable reflecting interest in politics (1-4, where 4 is high interest)[15] and four dummy variables reflecting stated party preferences, i) no preference for any political party, ii) preference for a center-left or left-wing party (SPD, Gruene, Die Linke, Piratenpartei), iii) preference for a

[14] Using our dataset, we also show in Table 2.B1 in the appendix that, after they occurred, most demonstrations used in our analysis were covered extensively in national and regional newspapers.

[15] This is based on the question "Generally speaking, how much are you interested in politics?"

Figure 2.1 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".

center-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, 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.

2.4 Empirical Strategy and Identification

2.4.1 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

[16]The construction of these variables are based on the question "Toward which party do you lean?"

those of individuals interviewed immediately afterwards (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 normalize 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 2.5.2, we show that our results do not depend on their inclusion.

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

$$Y_i = \alpha + \beta T_{ij} + \mu_1 d_{ij} + \mu_2 T_{ij} d_{ij} + \epsilon_i \tag{2.1}$$

In Equation 2.1, 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.

[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 2.5.2, we show our results using a second-order polynomial.

[20] In Section 2.5.2 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).

(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 2.3.1) 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 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 2.2 in Section 2.5 shows that our conclusions are robust to different bandwidths.

In Section 2.5.2, we augment the local polynomial model to include predetermined covariates such as the day of the week, month and year of the interview, residential district, gender, age, number of children, marital status, educational background and employment status.[22]

2.4.2 Validity of the Regression Discontinuity Design

In this section, we address three potential threats to our regression discontinuity design: 1) selective behavior around the cutoff, 2) anticipation and 3) selection on observables. 1) and 3) could happen if individuals can manipulate their interview dates (the score). If individuals cannot 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 behavior 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.

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

[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).

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] 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 2.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 were unlikely to be anticipated by individuals residing outside of the district where the demonstration took place. In Section 2.5.2, we show that our results are robust when excluding the entire state where the demonstrations occurred. We also show in Section 2.5.2, 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 2.3.1 and described in detail in Appendix 2.B also shows that there was little reporting in the newspapers leading up to 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 2.A2 in the appendix and show no significant effect of the placebo newspaper treatments on our outcomes of interest.

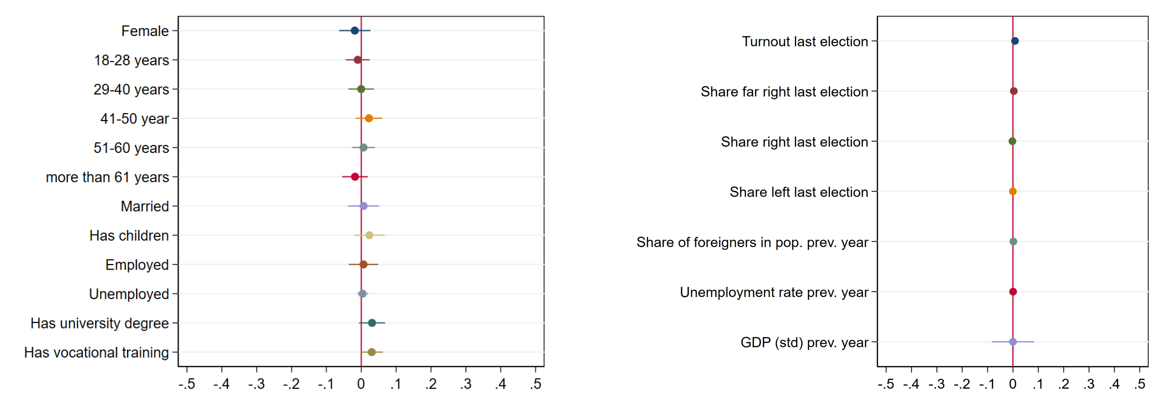
3) The continuity assumption holds Our identification strategy relies on the assumption that the individuals interviewed before a focal demonstration (control group) are similar to those interviewed after that focal demonstration (treatment

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

[24] The demonstrations considered in the RDD are those satisfying the criteria 1), 2), and 3) established in Section 2.3.1.

Figure 2.2 Continuity Tests

(a) Individual characteristics (b) District characteristics



Note: Panel (a) and (b) display the coefficients from the estimation of Equation 2.1 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 percent confidence intervals are shown.

group), constituting a credible counterfactual. We provide evidence that the continuity assumption holds by estimating Equation 2.1 using predetermined individual and district characteristics as outcomes. 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, center-right and left-wing vote at the last federal election in the Nuts II region where the respondent resided at the time of the survey.[26]

In Figure 2.2, 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, are assigned the turnout and vote shares of 2013.

2.5 Results

2.5.1 Main Results

Figure 2.3 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.

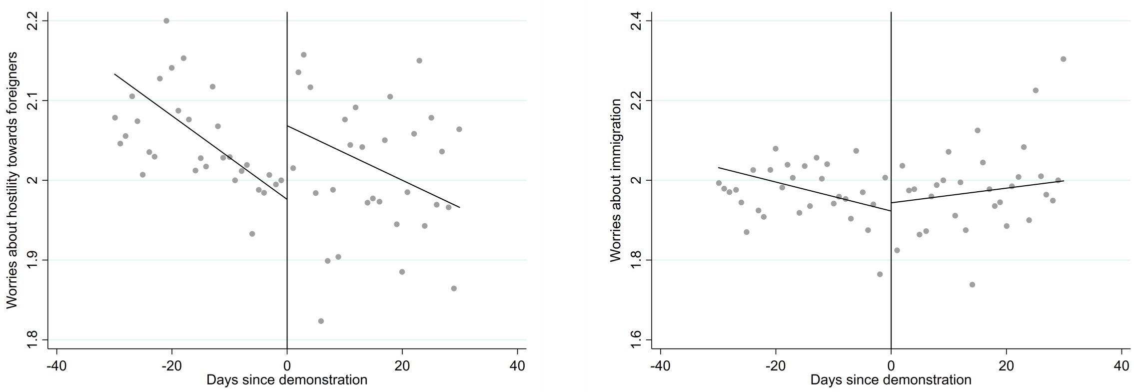
The plot in Panel (a) shows a discontinuity at the cutoff, suggesting that large right-wing demonstrations increase the worries about hostility towards foreigners. In Panel (b), we see no such suggestive evidence for the worries about immigration.

The main results of our analysis, using Equation 2.1, are displayed in Table 2.2 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 2.2 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

Figure 2.3 Main RDD Plots

(a) Worries about hostility towards foreigners

(b) Worries about Immigration



Note: Figure 2.3 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 2.2 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 2.2 displays the coefficients from the estimation of Equation 2.1 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

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 13.70% of a standard deviation. The RDD estimate does not vary greatly across time windows. As mentioned in Section 2.4, 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 2.2 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 favor 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.

2.5.2 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 percentile 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 randomization 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 2.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 2.2 the assignment to the right or left side of the cutoff does not depend on individual or district characteristics. Nevertheless, Table 2.A4 in the appendix shows the results when adding different sets of controls. Column (1) shows the baseline results as in Table 2.2, 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 2.4 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 2.2.

In the second line of Figure 2.4, 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 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 2.4 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 2.4 shows that our results do not change when we add people interviewed on the day of the demonstration to the treatment group.

In line 4 of Figure 2.4, 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 these observations.

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 2.4, 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]

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

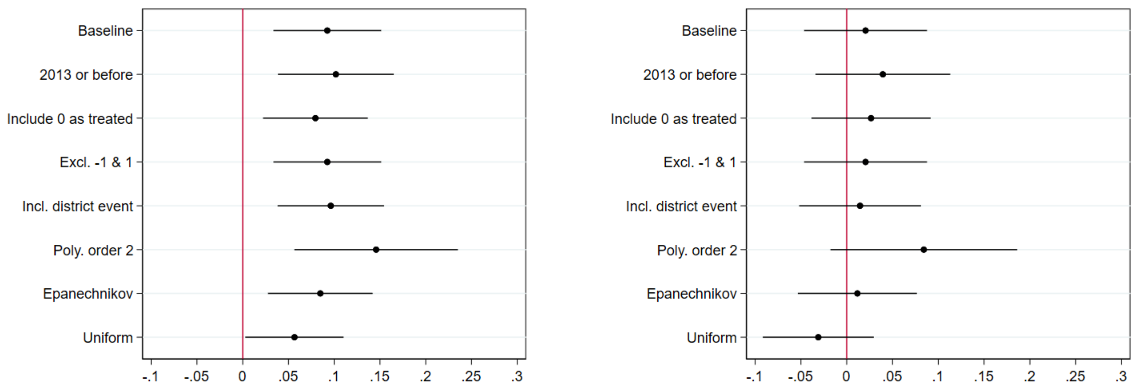
[28] Furthermore, this test also allows evaluating the sensitivity of the results to the extrapolation intrinsic to the local polynomial estimation, where the few individuals interviewed close to the demonstration are likely to be the most influential when fitting the local polynomials.

[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

and triangular kernel function which gives more weight to the observations closer to the cutoff. In line 6 of Figure 2.4, we show that our point estimates become larger when we include a second-order polynomial but do not change our study's conclusions. In lines 7 and 8 of Figure 2.4, 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 2.4 Robustness: Testing Different Specifications

(a) Worries about hostility towards foreigners (b) Worries about Immigration



Note: Figure 2.4 displays the coefficients from the estimation of Equation 2.1 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 percent 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

error and lead to changes in the estimated effect. However, the relevant question is whether such changes alter the study's conclusions.

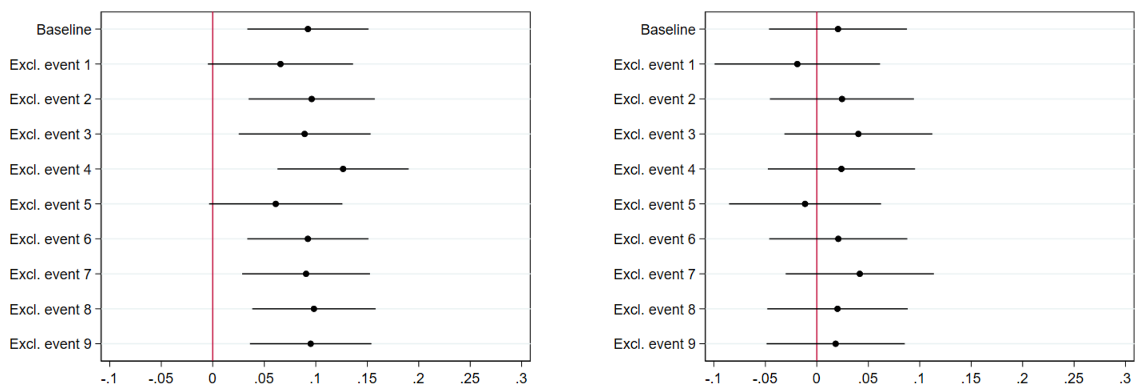
(7 demonstrations). The results are displayed in Table 2.A5 in the appendix and show that our conclusions are robust to variations around the definition of a large event.

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 2.A6 in the appendix shows the results for this exercise - the point estimates are close to those in our main results in Table 2.2.

Exclude one event at a time To assess the importance of a particular demonstration to our estimation, Figure 2.5 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 significantly different from zero at the 10 percent level. The coefficients in the worries about immigration regression are always statistically indistinguishable from zero.

Figure 2.5 Robustness: Exclude One Event at a Time

(a) Worries about hostility towards foreigners (b) Worries about immigration



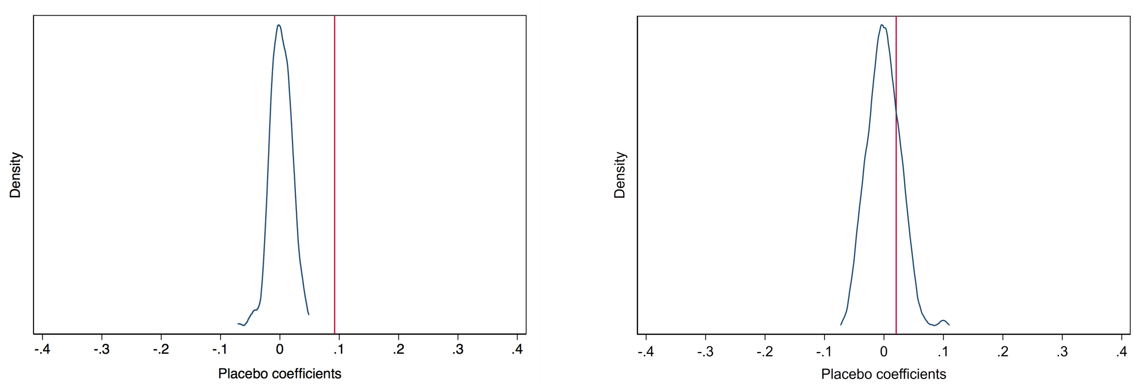
Note: Figure 2.5, displays the coefficients from the estimation of Equation 2.1 on worries about hostility towards foreigners in Panel (a) and worries about immigration in Panel (b). Both variables are measured on a 1-3 scale. All regressions consider a demonstration to be relevant if it has more than 1500 participants, use a 30-day bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. 95 percent 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 2.1, and repeat this procedure 300 times. The distribution of the coefficients is shown in Figure 2.6 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 2.2 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 2.6 Robustness: Use Placebo Treatment Date

(a) Worries about hostility towards foreigners

(b) Worries about immigration



Note: Figure 2.6, displays the distribution of the coefficients from estimating 300 times Equation 2.1 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 2.A7 in the appendix shows the coefficients when estimating Equation 2.1 using these alternative outcomes. As expected, xenophobic demonstrations did not affect these worries, as all coefficients remain insignificant.

Local randomization 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 randomization 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 randomization 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 randomization 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]

Table 2.3 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 2.3 displays the coefficients from the estimation of Equation 2.1 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 randomization approach is the window W, where the local randomization assumption is invoked. To choose this window, we follow Cattaneo et al. (2015, 2016) and use a procedure based on balance tests for regression discontinuity (RD) designs under local randomization. We use the *rdrandinf* package

[30]While in the continuity-based RDD the average potential outcomes are non-constant functions of the score, in the local randomization RDD, the functions are constant in the entire region where the score is randomly assigned.

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 randomization approach with a one-week window are displayed in Table 2.3. The point estimates are slightly smaller, but the overall results are robust and consistent with the continuity approach.

2.5.3 Newspaper Coverage

As laid out in Section 2.2, media and news reporting might play a role in how people learn about demonstrations and how they perceive them. In this section, we conduct a short media analysis and present suggestive evidence that newspaper reports are mediating 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. Table 2.B1 summarises and presents to what extent newspapers covered protests. The construction of this dataset is discussed in more detail in Appendix 2.B.

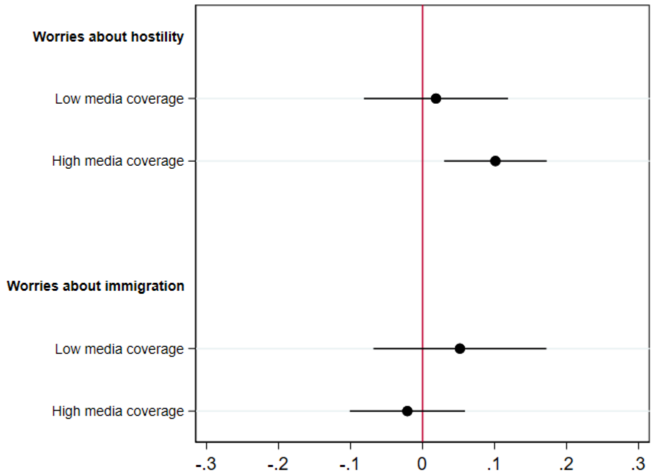
The data reveal several interesting insights: First, we see that there was reporting on all but one demonstration (Jänkendorf 2010) in the first three days afterwards, with all other events being covered by newspapers inside and outside of the state where they took place. Moreover, all but two protests 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

[31] Analysing the sentiment of the newspaper's reporting and/or other media is outside this study's scope.

[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.

Figure 2.7 RDD Results By Extent of Media Coverage



Note: The y-axis in Figure 2.7, displays the coefficients from the estimation of Equation 2.1 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 percent 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.

protest took place is usually fairly limited, and apart from one 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., 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 2.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 2.7 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.

[33]Not shown in Table 2.B1, as there was very little if any coverage of most protests four or more days after they took place.

For worries about immigration, we see no significant difference, as the coefficients are not statistically significant.

2.5.4 Heterogeneity Analysis

In the previous sections, we analyzed 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 analyze to what extent results may differ when we distinguish individuals by labor market, demographic, and attitudinal characteristics. We split the sample into different groups and run Equation 2.1 separately. As in the previous section, we present all our results using large demonstrations, with more than 1500 participants, and using a 30-day 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 not.[34] We then take a one-year lag of each of these measures relative to the year of the interview.[35]

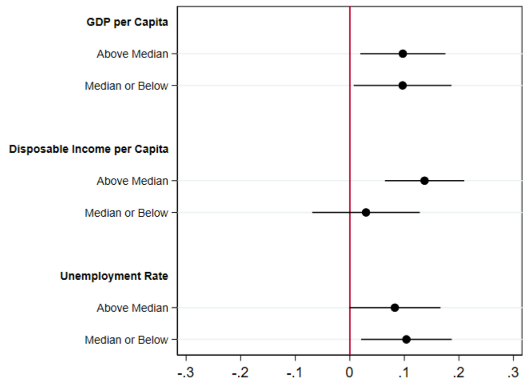
Generally, there is no clear indication that respondents in economically weaker regions react differently (Figure 2.8). Looking at GDP per capita and the unemployment rate, there is hardly any difference in estimates for both worries about hostility towards foreigners and worries about immigration. We 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 polarization, 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,

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

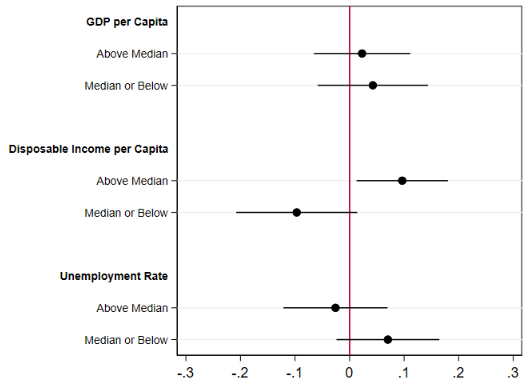
[35] This is done to avoid the issue that our treatment may directly affect those regional characteristics.

Figure 2.8 Heterogeneity Analysis: By Regional Economic Situation

(a) Worries about hostility towards foreigners



(b) Worries about immigration



Note: Figure 2.8, displays the coefficients from the estimation of Equation 2.1 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 percent 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.

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-center, and right-of-center parties[36] in the last federal election relative to the interview date.[37] In contrast to economic factors, Figure 2.9 displays that political factors appear to influence respondents' reactions to the protests.

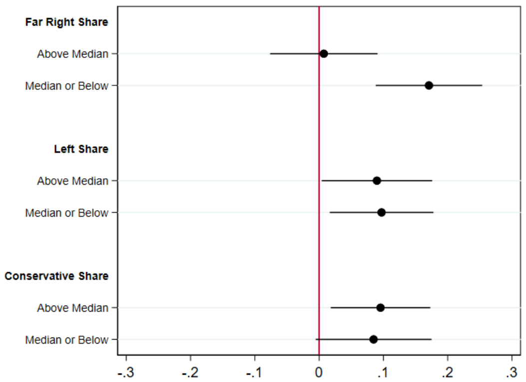
The estimates in Figure 2.9 (a) show that individuals who live in NUTS II regions with a higher share of far-right voting do not experience an increase in their concerns about hostility towards foreigners after protests take place, while respondents in other regions see a considerable increase. In contrast, when splitting the sample along the election vote share of left-wing and moderate conservative parties shows no statistically significant differences.

[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-center parties we include the SPD, Bündnis 90/Die Grünen, PDS/Die Linke, and Piratenpartei. Right-of-center parties are CDU, CSU, and FDP.

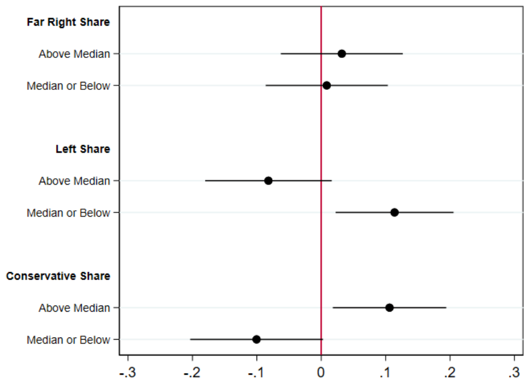
[37]There were federal elections in 2002, 2005, 2009, 2013 and 2017.

Figure 2.9 Heterogeneity Analysis: By Regional Political Environment

(a) Worries about hostility towards foreigners



(b) Worries about immigration



Note: Figure 2.9 displays the coefficients from the estimation of Equation 2.1 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 percent 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 2.9 (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-center 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-center 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 immigration. In right-leaning areas, on the other hand, there appears to be more of an ambivalent, potentially even polarized 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 2.A2 in the appendix looks at some additional heterogeneities at the district level. Most noteworthy here is that both types of concerns remain unchanged in eastern

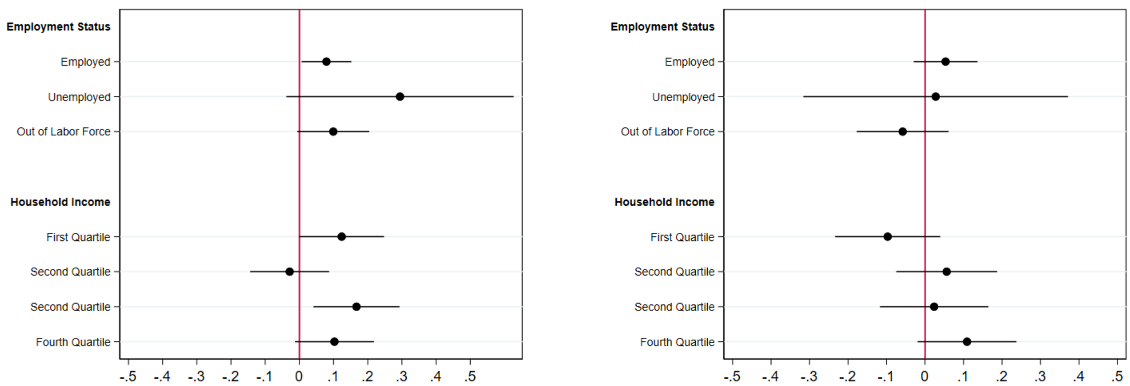
Germany. Moreover, worries about immigration increase significantly in districts with fewer foreigners, while the increase is only borderline significant in rural areas.

Individual characteristics Using information from the SOEP, we distinguish respondents by their labor market status and household income quartiles. The coefficients in Figure 2.10 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. These results are in line with the estimates on the regional level in Figure 2.8, suggesting that economic factors do not play much of a role in determining respondents' reactions to large far-right demonstrations.

In addition, Figure 2.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.

Figure 2.10 Heterogeneity Analysis: By Individual Labor Market Situation

(a) Worries about hostility towards foreigners (b) Worries about immigration



Note: Figure 2.10 displays the coefficients from the estimation of Equation 2.1 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 percent 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.

Individual political attitudes To look at heterogeneities by political attitudes, we rely on the panel structure of the SOEP. 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-center (from 0 to 3), center (4 to 6) and right-of-center (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.[38] 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.

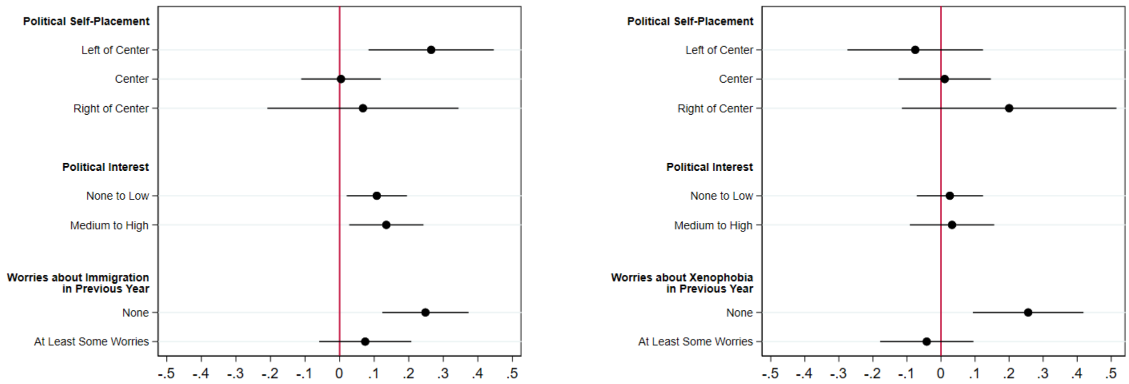
In contrast to economic characteristics, heterogeneities based on political attitudes seem much more striking. The heterogeneity by self-placement on the political spectrum in Figure 2.11 (a) shows an interesting picture; only those respondents who place themselves left-of-center had significantly increased concerns about hostility towards foreigners. On the other hand, Figure 2.11 (b) shows that the point estimate for worries about immigration is the highest for respondents who place themselves right-of-center, 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. The coefficients of the heterogeneity analysis by lagged worries suggest that existing political or social attitudes are major drivers in how people perceive and react to protests. While the effects in Figure 2.11 (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 polarization in the population in response to the protests, which would align with studies such as Caprettini et al. (2021).

[38]Using this approach, the sample size is reduced to 3,659 observations from the 10,902 observations reported in Table 2.2

Figure 2.11 Heterogeneity Analysis: By Political and Social Attitudes

(a) Worries about hostility towards foreigners (b) Worries about immigration



Note: Figure 2.11, displays the coefficients from the estimation of Equation 2.1 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 percent 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.

2.6 Changes in Political Preferences and Other Outcomes

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 behavior towards migrants. In this section, we present suggestive evidence of the effects of protests on these outcomes. We do not claim that the effect on political preferences 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 2.4 shows the results of estimating Equation 2.1 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 2.4 suggest two main effects: respondents become more politically interested 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

Table 2.4 Extension: Political Interest and Party Preferences

	Interest in politics (1)	No preference for any pol. party (2)	Preference left-wing party (3)	Preference right-wing party (4)	Preference far-right party (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 2.4 displays the coefficients from estimating Equation 2.1 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

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 percentage points. At the same time, there is no significant increase in the propensity to favor right-of-center 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 2.5, 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.

2.7 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

Table 2.5 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 2.5 displays the coefficients from the estimation of Equation 2.1. 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

demonstrations can have a mobilizing and persuading effect, if turned violent or disruptive, they may reduce support for their cause.

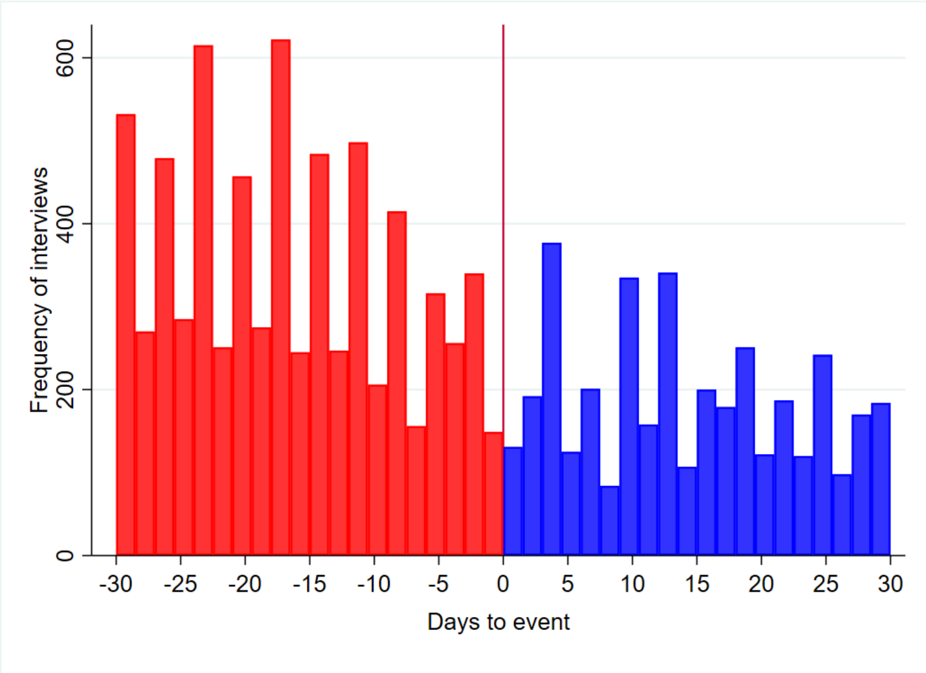
In this study, we use a regression discontinuity design to analyze how large right-wing xenophobic demonstrations affect concerns about hostility towards foreigners and worries about immigration among natives in Germany. Our results show that local xenophobic demonstrations lead to a significant short-term 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 favor. 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.

2.A Additional Tables and Figures (Appendix A)

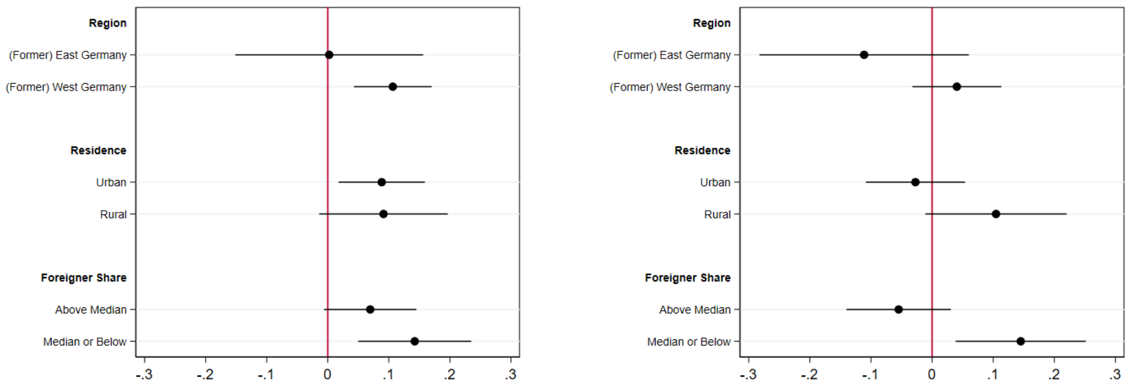
Figure 2.A1 Density Test: Frequency of Interviews



Note: The y-axis in Figure 2.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 2.A2 Heterogeneity Analysis: By Regional Characteristics

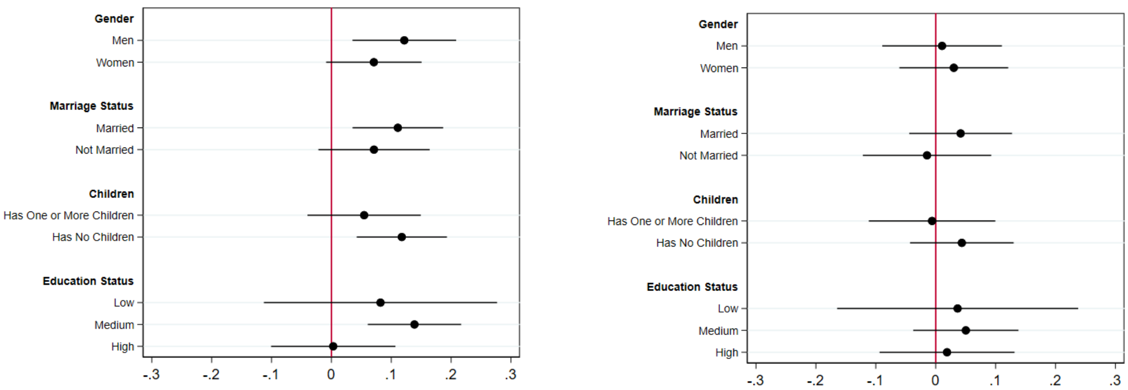
(a) Worries about hostility towards foreigners (b) Worries about Immigration



Note: Figure 2.A2, displays the coefficients from the estimation of Equation 2.1 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 percent 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 2.A3 Heterogeneity Analysis: By Individual Demographic Characteristics

(a) Worries about hostility towards foreigners (b) Worries about Immigration



Note: Figure 2.A3, displays the coefficients from the estimation of Equation 2.1 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 percent 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 2.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 2.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 2.A2 displays the coefficients from the estimation of Equation 2.1 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 2.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 2.A3 displays the coefficients from the estimation of Equation 2.1 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 2.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 2.A4 displays the coefficients from the estimation of Equation 2.1 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 2.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 2.A5 displays the coefficients from the estimation of Equation 2.1 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 2.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 2.A6 displays the coefficients from the estimation of Equation 2.1 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 2.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 2.A7 displays the coefficients from the estimation of Equation 2.1 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

2.B Media Analysis (Appendix B)

In this section, we describe the data used for our media analysis. Table 2.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.[39] We assembled our dataset by looking up various search terms – which are presented for each entry in the Table 2.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 2.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 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.

[39]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.

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 2.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 2.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

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
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.

Chapter 3

The Bitter Taste of Unemployment – Evidence from Layoffs in Germany

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3.1 Introduction

Unemployment is a pervasive and multifaceted characteristic of modern labor markets which has been shown to have profound impacts on individuals, economies, and societies at large. While the literature has long focused on economic outcomes, economists have increasingly shown interest in studying the implications of unemployment on individual well-being beyond labor markets. As a result, numerous studies evolved demonstrating that unemployment can have a detrimental impact on various non-monetary outcomes such as life satisfaction (Frijters et al., 2004), physical health (Schmitz, 2011), mental health (Marcus, 2013), and social exclusion (Pohlan, 2019). One so far unexplored effect is the one on bitterness.

Bitterness is a complex emotion, which already appeared in the Nicomachean Ethics of Aristotle, and is defined as a feeling of not having achieved in life what one deserves compared to others. It is also referred to as embitterment in the psychological literature, where it describes a state of feeling let down by fate, feeling helpless but also wanting to fight back (Alexander, 1960; Linden and Maercker, 2011). Bitterness is a unique concept that differs from other related psychological constructs and metrics, such as life satisfaction and reciprocity (Poutvaara and Steinhardt, 2018).[1] Beside its individual dimension bitterness is socially relevant, as Poutvaara and Steinhardt (2018) have demonstrated that more bitter people have significantly more worries about immigration than non-bitter people. Moreover, bitterness is associated with increased support for extreme right-wing parties. While there are some proposed determinants of bitterness in the psychological literature (Smith, 1985; Muschalla and Linden, 2011; Znoj, 2011), there has up to this point been no rigorous quantitative analysis on the causes of bitterness. This is where our paper steps in to examine the extent to which unemployment affects people's levels of bitterness.

For this purpose, we use four waves (2005, 2010, 2015, and 2020) of the German socio-economic panel, which capture our measure of bitterness by asking respondents to rate the following statement on a 7-point Likert scale: "Compared to other people, I have not achieved what I deserve." We use this information to identify the relationship between unemployment and bitterness in two steps.

First, we graphically compare the self-reported bitterness of people, who are employed, unemployed, and outside of the labor market. Doing so, we find that unemployed individuals are much more likely to be very bitter and much less likely to report low bitterness compared to the other two groups. Thereafter, we perform pooled OLS regressions to analyze whether this relationship still holds once we condition it on a wide set of demographic and socioeconomic control variables as well as year and state fixed effects. Thereby, we find a very strong and highly significant positive relationship, with unemployment being associated with an increase in bitterness of about a third of a standard deviation. Moreover, we show that the effect remains significant after accounting for long-term and involuntary unemployment. This analysis is then followed up by introducing individual fixed effects in our estimations, which remove all time-invariant unobserved heterogeneity. Hereby, we find that even though the coefficient is reduced, the effect of unemployment remains positive and highly significant, suggesting that moving into unemployment increases one's bitterness by around one eighth of a standard deviation.

Building on this initial analysis, we examine the causal impact of unemployment on bitterness. For this purpose, we exploit variation from plant closures and firm dismissals. Moreover, we combine matching based on entropy balancing (Hainmueller, 2012) to control for observable differences between respondents with difference-in-differences estimation to eliminate unobserved time-invariant heterogeneity. In this setting, we

[1]We will address this aspect explicitly in our analysis. See Section 3.4.3.

compare our treatment group, which consists of respondents who lost their jobs due to plant closures or dismissals and registered as unemployed in-between interviews with respondents who remained employed at the same time. Our results reveal that involuntary unemployment leads to a significant and substantial increase in bitterness of nearly 25 percent of a standard deviation.

We follow up this analysis by conducting a number of further tests and robustness checks, which uncover several noteworthy findings. First, we show that results are virtually identical when only looking at cases of job loss due to plant closures, which underlines that our approach is equivalent to the more established method of solely exploiting variation from plant closures (Kassenboehmer and Haisken-DeNew, 2009; Marcus, 2013). Second, we test to what extent our results still hold when we relax certain restrictions regarding our treatment group. Among others, we remove the condition that treated individuals necessarily also register as unemployed after being laid off, finding that our results still remain positive and highly significant. Third, we introduce measures of concepts potentially related to bitterness in the regression. Our estimations show that the effect of unemployment on bitterness remains largely unchanged and highly significant once measures of life satisfaction, positive reciprocity, negative reciprocity, and social deprivation are taken into account, which further supports that bitterness is a distinct concept. Fourth, we perform a number of additional robustness checks, showing that our results hold after modifying the treatment and control group, introducing a set of variables measuring job- and income-related worries and satisfactions as conditioning variables, rerunning regressions with a binary coding of our outcome variable, employing different clustering of standard errors, and using inverse probability weighting instead of entropy balancing. Finally, we document that those who remain unemployed for longer also become more bitter and that for some this effect persists over time.

Our study contributes to a number of different fields in economics. First, we contribute to the literature that analyzes the causal effects of unemployment on health and social outcomes (see, among others, Schmitz, 2011; Stauder, 2019; Marcus, 2013; Strandh et al., 2014; Pohlan, 2019), and specifically to those studies that estimate effects by exploiting variation from plant closures (Kunze and Suppa, 2017; Chadi and Hetschko, 2018). In particular, we highlight how unemployment can cause increases in bitterness. In doing so, we also contribute to the literature on bitterness (Poutvaara and Steinhardt, 2018) and other related concepts like life satisfaction (Frijters et al., 2004; Kassenboehmer and Haisken-DeNew, 2009), and positive and negative reciprocity

(Caliendo et al., 2012). We specifically provide the initial causal evidence for a significant contributor to bitterness, which is unemployment. Finally, our paper is indirectly related to the literature studying the drivers of attitudes towards immigration, right-wing attitudes, and electoral support for far-right parties (see, among others, Mayda, 2006; Otto and Steinhardt, 2014; Dehdari, 2018; Cantoni et al., 2019; Edo et al., 2019; Kratz, 2021; Margaryan et al., 2021), as bitterness is closely linked to concerns about immigration and support for the extreme right (Poutvaara and Steinhardt, 2018).

The remaining parts of our paper are structured as follows. In section 3.2, we describe our data and present descriptive statistics. This is followed up by using pooled OLS and fixed effects regressions to study the relationship between unemployment and bitterness in section 3.3. The causal analysis on the effect of unemployment is provided in section 3.4, which also includes robustness checks and heterogeneity analyses. Section 3.5 concludes our study.

3.2 Data and Descriptive Statistics

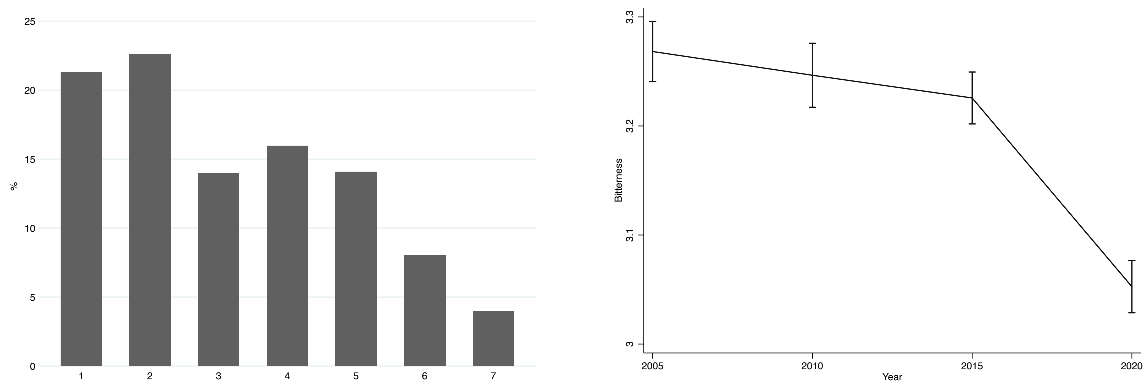
In this study, we primarily use data from the German socio-economic panel (SOEP, Goebel et al. 2019). This is a longitudinal household survey, which annually interviews around 30,000 respondents from about 15,000 households. Apart from capturing social, economic, and demographic characteristics, it also surveys a wide range of respondents' attitudes and opinions, including their level of bitterness. More specifically, every five years, they are asked to rate to what extent they agree with the following statement: "Compared to other people, I have not achieved what I deserve." This variable is scaled from 1 (completely disagree) to 7 (fully agree) and captured in 2005, 2010, 2015, and 2020.[2]

As shown by Poutvaara and Steinhardt (2018) bitterness is a distinct concept which stands apart from other related psychological or sociological constructs and measures such as life satisfaction, positive and/or negative reciprocity, (the opposite of) altruism, relative deprivation, or locus of control. While bitterness shares some similarities with these concepts, it represents a separate emotional state characterized by a sense of having been let down and a feeling of being a loser, a desire to fight back and, at the same time, feeling helpless.

[2]This variable was already included in the 1999 wave. However, because the scaling was different, namely from 1 to 4, we exclude this wave from our sample.

Figure 3.1 Distribution and Time Trend of Bitterness

(a) Distribution (b) Time Trend

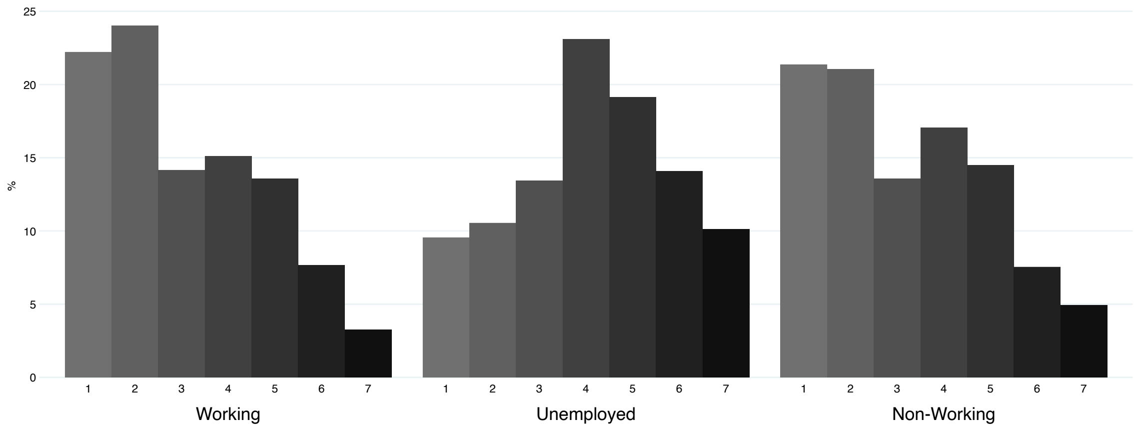


Note: Figure (a) displays the distribution of answers given for the sample years from 2005 to 2020, with 1 indicating complete disagreement and 7 full agreement. Figure (b) shows mean bitterness over time.

In our study, we employ our bitterness measure as our main dependent variable and include all individuals who provide an answer to this question. Hereby, we restrict our sample to respondents who are of working age, i.e., between 18 and 65, which results in a sample size of just under 72,000. Figure 3.1 graphically displays how bitterness is distributed in the sample (a) and how its average has developed over time (b). Hereby, it shows that a majority of people have low levels of bitterness, with more than 20 percent stating the lowest two response options each. Still, a little more than a quarter of the sample respond that they feel at least partially bitter with values of no less than 5, with the two highest options being chosen by around 8 and 4 percent of respondents each. Thus overall, a substantial minority feels that they did not achieve what they have deserved in life compared to others. Looking at Figure 3.1 (b), however, shows that the average level of bitterness has consistently declined over the years from just under 3.3 to around 3.05, with smaller drops between 2005 and 2015, and a large drop in 2020.[3] This is also illustrated in Table 3.A1 in the appendix, which shows how response

[3]One possible explanation for this drop in 2020 might be that the German economy improved, which may have decreased average bitterness. However, as the economy was also growing on a similar pace before, we suspect that this might be a one-time Covid effect. Most interviews in 2020 were conducted between February and May, thus in the period when the risks of the pandemic were most salient. People may have started comparing themselves less in terms of economic outcomes and more with regards to health. In this extraordinary time, as media reports stressed the health risks of the pandemic, they may have been more conscious of the value of being healthy. Regardless, to account for these changes over time, in later regressions we include year fixed effects.

Figure 3.2 Distribution of Bitterness by Employment Status



Note: Figure 3.2 displays the distribution of responses by employment status, with 1 indicating complete disagreement and 7 full agreement.

frequencies have developed over time, revealing a decline in bitterness. In addition, Figure 3.A1 shows the spatial distribution of mean bitterness across German states (*Bundesländer*) for the pooled sample. This map reveals a striking divide between the former East German states and its Western counterparts, with higher levels of bitterness in the former than the latter. In addition, it also uncovers a North-South gradient, and lower levels of bitterness in the city-states Berlin, Hamburg, and Bremen relative to their neighbors.

To examine the relationship between unemployment and bitterness, we make use of the information in the SOEP regarding employment and labor market status of individuals. Hereby, the dataset captures in every year, whether respondents are employed (full-time, part-time, or irregularly), not on the labor market (e.g., because they are in school or already retired), or unemployed. Table 3.A2 in the appendix shows that around 80 percent of the respondents in the sample are on the labor market, with 65 percent working either full-time or part-time, and only 6 percent being unemployed.

Figure 3.2 shows that the distribution of stated levels of bitterness differs significantly between unemployed respondents and those who work or are not active in the labor market. In particular, unemployed people are much less likely to state very low and much more likely to report very high bitterness. To be more specific, only around 10 percent of unemployed respondents stated each of the two lowest levels of bitterness, while more than 40 percent of respondents reported their bitterness levels to be at least 5, with nearly 15 percent and 10 percent reporting levels of 6 and 7, respectively. While, of course, this distribution could also be heavily driven by other non-examined factors and selection into unemployment, the differences between the groups are remarkable.

To look further into the matter, we make use of the breadth of information captured in the SOEP in terms of social, economic, and demographic individual characteristics. Apart from employment and labor market status, Table 3.A2 in the appendix also lists those variables which are used in the descriptive analysis as control variables, and shows their means and standard deviations. These include social and demographic characteristics – such as gender, age, migration background, and marriage status –, educational attainment[4], labor income[5], and health[6].

3.3 Pooled OLS and Fixed Effects Regression Analysis

3.3.1 Empirical Approach

In the first part of this study, we conduct various regression estimations to examine whether the relationship between unemployment and bitterness displayed in Figure 3.2 also holds once we condition this relation on other socio-economic and demographic individual characteristics. Hereby, we first perform pooled OLS regressions with a wide set of explanatory variables. Our regression equation thereby takes the following form:

$$bitterness_{it} = \beta_0 + \beta_1 unemployed_{it} + \beta_2 X_{it} + S + \tau_t + \epsilon_{it}. \tag{3.1}$$

Hence, we regress our main outcome of interest, bitterness ($bitterness_{it}$), on a dummy variable indicating whether the respondent is unemployed and a number of individual-level controls presented in Table 3.A2 (X_{it}), while including state (S) and year fixed effects (τ_t). Standard errors are clustered by individual. This approach, however,

[4]Hereby, we also distinguish between different secondary school degrees. In Germany, there are broadly three types of secondary school diplomas corresponding to the three secondary school types: *Hauptschulabschluss*, which is the lowest ranked school degree, *Realschulabschluss* or *Mittlerer Schulabschluss* (MSA), which is the mid-level school degree, and *Allgemeine Hochschulreife* or *Abitur*, which is the highest ranked school degree and generally required to attend university.

[5]This variable is, of course, only available for those who have some kind of gainful employment. To include respondents without employment in our regressions, we include a dummy variable which is equal to one if labor income is missing, and recode labor income as zero for those respondents.

[6]To measure this, we include a variable indicating whether respondents report to have poor health (*sick*) and a dummy variable indicating whether they have been officially assessed to have some kind of disability.

only gives us a rather rough estimate, as it cannot take potentially important unobserved heterogeneity into account. Moreover, the dummy variable $unemployed_{it}$ captures a very broad measure of unemployment which does not distinguish between long- and short-term unemployment or whether the respondent lost their job involuntarily or not. To try to amend for that, we introduce two additional variables in our regression: First, to capture long-term unemployment, we construct a dummy which is one if the respondent has been unemployed in the previous year.[7] Second, we include a dummy, which indicates whether the unemployed person reported that they lost their previous employment involuntarily or not. Hereby, we define a job loss to be involuntary if the respondent was laid off due to a plant closure or a dismissal by their firm.[8]

In the next step, to reduce issues due to endogeneity, we include individual fixed effects (ρ_i):

$$bitterness_{it} = \beta_0 + \beta_1 unemployed_{it} + \beta_2 X_{it} + S + \tau_t + \rho_i + \epsilon_{it}. \quad (3.2)$$

While this estimation strategy cannot eliminate all endogenous variation, it removes all time-invariant individual heterogeneity. This allows us to get closer to the true effect of unemployment as it examines changes in bitterness after respondents move in or out of unemployment.

3.3.2 Pooled OLS Results

The results of the pooled OLS regression of bitterness on unemployment without individual fixed effects but with state and time fixed effects are presented in Table 3.1. Importantly, apart from the dummy variable indicating whether a respondent is currently unemployed, all regressions also include dummy variables reflecting whether the person has a different labor market status, which can take the following expressions: part-time employment, irregular employment, pursuing further education – e.g., in school or university –, retirement, or lastly, otherwise not active on the labor market, with the base variable being full-time employment. This means that the effect of unemployment is measured against the counterfactual state of full-time employment. Column (1) displays results using the most basic specification, including only those labor

[7]This variable is somewhat imperfect as it relies on information of the interview in the previous year. Therefore, it is not able to capture the long-term unemployment of individuals, who have not been surveyed in the previous wave of the SOEP.

[8]This is a very strict definition, as, of course, other forms of job loss can also be to some extent involuntary, e.g., if a contract is not extended.

Table 3.1 Pooled OLS Regression Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
unemployed	0.5669***	0.5590***	0.5427***	0.4316***	0.3666***	0.3319***	0.2827***	0.2570***
	(0.0165)	(0.0166)	(0.0167)	(0.0168)	(0.0266)	(0.0266)	(0.0296)	(0.0299)
long-term unemployed							0.1120***	0.1335***
							(0.0296)	(0.0298)
involuntary job loss								0.1280***
								(0.0225)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Labor Market Status	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marriage Status			Yes	Yes	Yes	Yes	Yes	Yes
Education				Yes	Yes	Yes	Yes	Yes
Labor Income					Yes	Yes	Yes	Yes
Health						Yes	Yes	Yes
N	71951	71951	71951	71951	71951	71951	71951	71951
r2	0.030	0.044	0.049	0.078	0.085	0.092	0.092	0.093

Note: The table reports OLS estimates of equation 3.1, gradually adding more regressors, which are described in Table 3.A2 and the text. For coefficients of these additional covariates, see Table 3.A3. The outcome variable is standardized. Standard errors (in parentheses) are clustered at the person level.
* $p < .10$, ** $p < .05$, *** $p < .01$

market dummy variables as well as year and state fixed effects. The following columns thereon show estimates after different individual characteristics are incrementally added as control variables into the regression. While column (2) introduces demographic characteristics like gender, age, and migration background, subsequent columns add marriage status and children in household (3), educational attainment (4), labor income (5), and health variables (6). To ease interpretation, we standardize our outcome variable bitterness.

Across all regression specifications, there is a large positive and highly significant relationship between unemployment and bitterness. While estimates in columns (1) to (3) suggest that unemployment is associated with an increase in bitterness of more than 50 percent of a standard deviation, the coefficient is cut down by more than a third to .33 of a standard deviation once education, labor income, and health are taken into account.

Table 3.A3 in the appendix also documents the coefficients of the control variables in each of the six regressions. Apart from the effect of unemployment, results indicate several noteworthy statistical relationships: Men appear to be more bitter than women, migrants more bitter than natives, and foreign born migrants more bitter than those born in Germany. While divorced respondents show higher bitterness than the baseline group of otherwise single people, the opposite is the case for married individuals.

Moreover, respondents appear to become more bitter with every year they grow older, although this effect is decreasing with age. The effects that stand out the most, however, are the effects of education, labor income, and health, with bitterness being lower for those with more advanced school and university degrees, higher labor income, and better health.

Lastly, we also examine to what extent results may differ for long-term unemployed respondents, i.e., those who have been unemployed in at least two consecutive interviews, and for those who lost their previous employment involuntarily. Results are displayed in the final two columns of Table 3.1. The estimates in columns (7) and (8) show that the long-term unemployed appear to be more bitter. Furthermore, those who lost their last job involuntarily also showcase larger levels of bitterness. Nevertheless the effect of unemployment, i.e., the coefficient that remains after controlling for long-term unemployment and involuntary job loss is still highly significant and fairly large at around a quarter of a standard deviation.

3.3.3 Fixed Effects Results

The OLS regressions in section 3.3.2 reveal that bitterness is highly correlated with unemployment, even when controlling for a multitude of individual characteristics and after accounting for long-term unemployment and involuntary job loss. However, the analysis is possibly biased due to issues of endogeneity, especially with regards to potential omitted variable bias and reverse causality. To mitigate these concerns, we introduce individual fixed effects in the following estimations, allowing us to remove time-invariant unobserved heterogeneity.

Results of these fixed effects regressions are displayed in Table 3.2. Again, different controls are gradually included in the regression, starting with a set of dummy variables capturing respondents' employment and labor market status (1). Column (2) adds demographic and household characteristics as well as educational attainment, while columns (3) and (4) introduce labor income and health variables, respectively. As in section 3.3.2, the outcome is standardized and the effect of unemployment is measured against the base case of being full-time employed.

Across these four specifications, the coefficient for unemployment is remarkably stable with estimates remaining positive and highly significant at just above one eighth of a standard deviation. This means that estimates are less than half the size of the coefficients for unemployment in the pooled OLS regressions. On one side, this

Table 3.2 Fixed Effects Regression Results

	(1)	(2)	(3)	(4)	(5)
unemployed	0.1304***	0.1314***	0.1379***	0.1342***	0.1265***
	(0.0279)	(0.0281)	(0.0443)	(0.0443)	(0.0446)
involuntary job loss					0.0479
					(0.0327)
Indiv. FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Labor Market Status	Yes	Yes	Yes	Yes	Yes
Demographic Charact.		Yes	Yes	Yes	Yes
Household Charact.		Yes	Yes	Yes	Yes
Education		Yes	Yes	Yes	Yes
Labor Income			Yes	Yes	Yes
Health				Yes	Yes
N	71951	71951	71951	71951	71951
r2	0.002	0.003	0.004	0.005	0.005
r2_b	0.011	0.004	0.005	0.005	0.005
r2_o	0.009	0.003	0.004	0.004	0.004

Note: The table reports fixed effects regression estimates of equation 3.2, gradually adding more regressors, which are described in Table 3.A2 and the text. For coefficients of these additional covariates, see Table 3.A4. The outcome variable is standardized. Standard errors (in parentheses) are clustered at the person level.
* $p < .10$, ** $p < .05$, *** $p < .01$

could reflect that the pooled OLS estimates are biased upwards as they might be plagued more severely by problems of selection into unemployment. On the other side, fixed effects regression estimates present results of respondents moving in and out of unemployment, which may give us a flawed impression of how unemployment itself may affect bitterness over time. Moreover, the fixed effects estimations in columns (1) to (4) do not distinguish between voluntary and involuntary job loss. Therefore, in column (5), we include a variable indicating whether unemployed respondents lost their last job involuntarily – following the same approach as in Table 3.1, column (8). Interestingly, this coefficient is positive but insignificantly different from zero, which would imply that the causes leading to unemployment do not appear to play much of a role in affecting bitterness.

Table 3.A4 in the appendix, again, displays estimates of the other covariates in the fixed effects regressions. Generally, most variables lose their significance once individual fixed effects are included in the regression. While most demographic variables

already drop out in advance as they are time-invariant, the effects of nearly all social and educational variables are insignificantly different from zero. Only attaining a university degree has a slightly negative effect. We should note, however, that there is limited variation especially w.r.t. educational variables as most respondents are adults and only few pursue further educational qualifications, in particular secondary school degrees. The variables that stand out with highly significant effects are – apart from unemployment – labor income, self-assessed health and, interestingly, being in education, i.e., going to school or university or doing an apprenticeship.

Going back to the estimates of unemployment, we look further into the size and relevance of our effects. While the standardization of our dependent variable helps to make our coefficients more easily comparable, it still leaves their interpretation somewhat vague. Therefore, to get a better sense of the effect sizes of each coefficient, Table 3.A5 presents the results for selected coefficients of linear probability models with individual fixed effects, whereby the outcome variable has a binary coding. In column (1), values of 5, 6 or 7 are coded as one, otherwise they are zero. In column (2), values of 6 and 7 are coded as one, and in column (3), only the highest value of 7 is coded as one, with all else being coded equal to zero. In all specifications, the coefficient for unemployment is significant and large. This is particularly the case for the specifications in which only very large expressions of bitterness are coded as one, i.e., in column (2) and (3), with becoming unemployed being associated with a 5 and a 2.8 percentage point increase in bitterness, respectively. These effects are large, especially considering that, on average, only about 12 and 4 percent of all respondents have such high levels of bitterness. Yet, their size is reasonable considering that nearly 25 percent of unemployed respondents reported bitterness levels of 6 or 7 as seen in Figure 3.2.

Lastly, Tables 3.A6 and 3.A7 present fixed effects regressions using the same specifications as in Table 3.2 but splitting the sample by different characteristics, namely gender, migration background, and place of residence. Thereby, it shows that the effect of unemployment varies widely between different groups. While Table 3.A6 reveals that the effects are insignificant for men, but large and significant for women, Table 3.A7 shows large differences between residents of different areas in Germany. While respondents who become unemployed living in West Germany and in urban regions showcase large positive and significant effects, the coefficients for those living in East Germany and rural areas are not significantly different from zero.

3.4 Causal Effects of Unemployment

In the previous sections, we have examined the impact of unemployment on bitterness using pooled OLS regressions and fixed effects methods. Hereby, both approaches reveal positive and highly significant estimates, suggesting that unemployment leads to a marked increase in people's levels of bitterness. However, these results are likely still distorted due to issues of omitted variable bias as fixed effects regressions cannot capture all unobserved heterogeneity. Therefore, we proceed in this section by identifying the causal effect of unemployment on individuals' levels of bitterness.

In the following, we present our empirical approach, exploiting involuntary job loss and combining matching based on entropy balancing with difference-in-differences estimation.

3.4.1 Empirical Approach

When studying the causal effects of unemployment, researchers face the problem that unemployment does not occur randomly but is instead often related to the individual characteristics of workers, which may include unobserved factors such as talent, motivation, or disposition. In our case, this can lead to both issues of omitted variable bias, as omitted factors may affect both the probability of becoming unemployed and the level of bitterness, as well as reverse causality, because the level of bitterness may affect the chance of becoming unemployed if it, e.g., impacts work performance.

One approach frequently used in the existing literature on the causal effects of unemployment is to focus on cases in which workers lose their employment due to a plant closure (existing studies include, e.g., Kunze and Suppa (2017) and Chadi and Hetschko (2018)). Hereby, it is argued that plant closures occur quasi-randomly, are out of the control of individual employees, and importantly, affect all workers at the same respective plant. Thus, only cases in which unemployment is exogenous to the main outcome of interest and independent of workers' characteristics are studied.

In this study, we modify this approach to reach causal estimates. While we do look at instances of unemployment due to plant closures, we additionally examine those workers, who were dismissed by their firms. While firm dismissals likely are dependent on individual characteristics, we argue that by controlling for observable individual characteristics and unobserved time-invariant heterogeneity, we are able to account for this potential bias. Testing the validity of our approach in section 3.4.3, we are able to show that our approach leads to virtually the same estimates compared to only looking at unemployment due to plant closures while having the added benefit of increasing the sample size of treated individuals by a factor of more than four.

The construction of the treatment and control group is summarized in Table 3.3. Both groups consist of individuals, whose bitterness was captured in at least two separate interviews. Moreover, members in both groups were employed (full- or part-time) in t, when their level of bitterness was initially surveyed. Yet, while respondents in the control group remained employed with the same firm throughout the period in-between interviews, the treatment group consists of people who lost their job due to a plant closure or a dismissal and registered as unemployed. Furthermore, at the point of the next interview in which bitterness was captured ($t + 5$), members of the treatment group were registered as unemployed while those in the control group were still employed with the same firm. Overall, this leaves us with 285 observations in the treatment group and just under 11,000 observations in the control group.

Table 3.3 Construction of Treatment and Control Group

Treatment Group	Control Group
Respondents who...	Respondents who...
1. were employed in t (full-time or part-time)	1. were employed in t (full-time or part-time)
2. were interviewed in t and t + 5 (e.g., 2005 and 2010) and gave a response to the bitterness question	2. were interviewed in t and t + 5 (e.g., 2005 and 2010) and gave a response to the bitterness question
3. were affected by layoff due to plant closure or a dismissal by their firm between t and t+5 and registered as unemployed	**3. remained employed (full-time or part-time) with the same firm in all years between t and t + 5**
4. were registered as unemployed in t + 5	**4. were employed in t + 5 (full-time or part-time)**

To make treatment and control group comparable along observable features, we perform matching based on entropy balancing (Hainmueller, 2012). Hereby, we balance treatment and control group using a large set of conditioning variables.[9] The set of conditioning variables is listed in Table 3.A8. Through reweighting of the control group, we can make sure that both groups have very similar means and standard deviations. This is displayed in Table 3.A9 in the appendix, which compares means and standard deviations of the conditioning variables of the treatment with those of the control group

[9]For that, we use the *ebalance* command for Stata by Hainmueller and Xu (2013).

before and after matching using the whole set of conditioning variables.[10] Although treatment and control group differ substantially in their characteristics, means and standard deviations of all conditioning variables are very similar after the matching is done.

Lastly, to control for unobserved time-invariant variation, we perform a difference-in-differences estimation that includes the weights from entropy balancing with the following equation:[11]

$$\Delta bitterness_{it} = \beta_0 + \beta_1 treat_{it} + Z'_{it}\gamma + S + \tau_t + \epsilon_{it}. \tag{3.3}$$

Hereby, we look at the change in bitterness between t and $t+5$ as our dependent variable ($\Delta bitterness_{it}$). The main explanatory variable $treat_{it}$ is one if a respondent is in the treatment group and zero if they are in the control group. The conditioning variables (Z_{it}), which are captured in t, are used both for matching and as control variables in the subsequent estimations.[12] All other elements are defined as in the regressions above. The coefficient β_1 reveals the causal effect of unemployment on bitterness.

3.4.2 Main Results

The effects of involuntary unemployment due to plant closure or dismissal on bitterness are displayed in Table 3.4. To make them more easily comparable, the coefficients in the table are standardized. For each column, the set of conditioning variables used in the matching procedure is gradually altered. In column (1), only the basic conditioning variables are used, which include respondents' socio-demographic, labor market, and educational characteristics (as categorized in Table 3.A8). In the following two columns, we incrementally add a number of health-related variables (2), and a set of extended

[10] For our main results in Table 3.4, we estimate coefficients for a gradually increasing set of conditioning variables. The matching of control and treatment group displayed in Table 3.A9 therefore corresponds with the specification in Table 3.4, column (3). Nevertheless, the matching also works very well for the other specifications.

[11] We are aware of the ongoing debate in economics on two-way fixed effects regressions (De Chaisemartin and d'Haultfoeuille, 2020, 2023; Callaway et al., 2024). In our case, there could be a problem that treated individuals later join the control group. However, due to our data and the setup of our methodology, this would only be possible for individuals treated between 2005 and 2010, who may be included in the control group between 2015 and 2020. This group is negligibly small (11 observations), making up far below one percent of the control group and should therefore not impose any meaningful bias.

[12] While the latter has no effect on the size of the coefficient, it reduces the standard errors somewhat.

labor market-related conditioning variables (3). In column (4), we include state fixed effects instead of using a dummy indicating whether respondents live in a former East German state. Column (5) adds statewide GDP per capita and unemployment rates.

Table 3.4 Causal Effects of Unemployment on Bitterness: Main Results

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.235***	0.221***	0.241***	0.236***	0.233***	0.241***
	(0.0485)	(0.0483)	(0.0481)	(0.0482)	(0.0482)	(0.0669)
Treated N	285	285	285	285	285	285
Control N	10922	10922	10922	10922	10922	10922
R2	0.421	0.426	0.444	0.443	0.443	0.0130
Basic Cond. Var.	Yes	Yes	Yes	Yes	Yes	Yes
Health Cond. Var.		Yes	Yes	Yes	Yes	Yes
Ext. Labor Cond. Var.			Yes	Yes	Yes	Yes
State FE				Yes	Yes	
State Cond. Var.					Yes	

Note: This table reports the main results of the causal analysis of the effects of involuntary unemployment on bitterness. The outcome variable is standardized. Each column gradually expands the set of conditioning variables listed in Table 3.A8. (1): Basic conditioning variables, including demographic, educational, and labor market characteristics. (2): Additionally includes a set of health-related conditioning variables. (3): Additionally includes a set of extended labor market-related conditioning variables. (4): Uses state fixed effects instead of a dummy variable for the former Eastern states. (5): Additionally includes statewide unemployment rates and GDP per capita. (6): Like (3), but uses conditional variables only for matching, not as explanatory variables in the subsequent regression. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$

Across all specifications the effect of unemployment is large, highly significant, and fairly stable. In column (1), where we include the most basic set of conditioning variables, unemployment increases bitterness by .235 of a standard deviation. Reassuringly, the coefficient barely changes once we include further conditioning variables in columns (2) and (3), when we employ state fixed effects (4), and when we control for regional characteristics (5) – remaining just under a quarter of a standard deviation. This suggests that our result is not being driven by unobservable factors correlated with the controls we add (Oster, 2019). In column (6), we further check what happens, when we only use the conditioning variables in column (3) – our preferred specification – for matching without including them in the subsequent regression. While the standard error increases sightly, the coefficient still remains highly significant.

3.4.3 Robustness

In this section, we test the validity and robustness of our results, showing that our results hold when we modify the treatment and control group, include potentially related concepts, worries, and satisfactions as conditioning variables, use binary outcome variables, employ different clustering of standard errors, and use a different weighting technique.

At first, we compare our baseline estimates with results if we only include individuals in our treatment group who were laid off because of a plant closure, which – as outlined above – is the more commonly used approach in the literature. Results can be seen in Table 3.5. While column (1) shows our baseline estimates (from Table 3.4, column (3)), column (2) displays results after narrowing our treatment group to respondents who became jobless due to a plant closure. Even though the sample size of our treatment group in column (2) is cut down greatly to only 61 (compared to 285 in the main regression), the coefficient remains virtually identical.

In the following columns of Table 3.5, we then go on to check to what an extent our results are determined by the construction of our treatment group and whether overall effects are driven by unemployment, job loss, or both. In our baseline estimations, the treatment group has relatively narrow restrictions. We only include respondents, who were dismissed by their employer or laid off due to a plant closure, who registered as unemployed in the same period, and who were unemployed in the next period, when bitterness was captured again. In columns (3) to (5), we therefore test what happens, when we loosen some of these restrictions. Column (3) displays results when we do not require respondents to be unemployed in $t + 5$. This means that we also include individuals, who found a job again after becoming unemployed.[13] Even though this more than doubles the number of treated individuals compared to column (1), the coefficient is only a little lower at .2 of a standard deviation, and remains highly significant. In column (4), we even go a step further, as we also relax the assumption that laid off individuals register as unemployed in the same period. This means that we include those individuals who immediately found a new position after losing their jobs due to dismissal or plant closure. This expands our treatment group to more than five times the original sample size of treated respondents, indicating that only a minority of respondents affected by dismissal or plant closure actually become unemployed.

[13] Of course, this sample also includes individuals who leave the labor market altogether, e.g., because they retire or they want to pursue further education.

Table 3.5 Robustness: Modify Treatment Group

	(1)	(2)	(3)	(4)	(5)
Treatment Var. 1	0.241***				
	(0.0481)				
Treatment Var. 2		0.237***			
		(0.0770)			
Treatment Var. 3			0.199***		
			(0.0346)		
Treatment Var. 4				0.128***	
				(0.0245)	
Treatment Var. 5					0.0737**
					(0.0300)
Treated N	285	61	688	1548	860
Control N	10922	10922	10922	10922	10922
R2	0.444	0.491	0.429	0.412	0.410
Basic Cond. Var.	Yes	Yes	Yes	Yes	Yes
Health Cond. Var.	Yes	Yes	Yes	Yes	Yes
Ext. Labor Cond. Var.	Yes	Yes	Yes	Yes	Yes

Note: This table reports robustness checks for the main analysis in Table 3.4 by changing the definitions of the treatment group. The outcome variable bitterness is standardized and the regressions are otherwise specified as in Table 3.4, column (3). Column (1): Baseline result. (2): Only includes respondents in treatment group affected by plant closure, who registered as unemployed and were unemployed in $t+5$. (3): Relaxes the assumption of unemployment in $t+5$ for treated individuals. (4): Includes all individuals in treatment group affected by plant closure, regardless of unemployment. (5): Includes all individuals in treatment group affected by plant closure or dismissal, who did not become unemployed. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$

Nevertheless, the estimate remains highly significant at around an eighth of a standard deviation, and just over one half of our baseline coefficient. In column (5), we even go one step further and examine what happens, when we only look at those respondents, who were affected by dismissal or plant closure but did not become unemployed – e.g., because they immediately found a new position at a different firm. Notably, the coefficient remains positive and statistically significant at 7 percent of a standard deviation.

The estimates in columns (3) to (5) indicate two major points: First, the effect of unemployment on bitterness appears to hold even after respondents leave unemployment, indicating that bitterness may have long-term effects. Second, they show that just losing a job makes a person more bitter even if the individual does not become unemployed. This means that both, the experience of job loss and the state and length of unemployment have an effect on one's level of bitterness. The latter is analyzed in greater detail in section 3.4.4, where we look at the effects of unemployment duration.

Table 3.A10 in the appendix additionally checks whether our results hold for further modifications of the treatment and the control group. In column (1), we require individuals to have remained unemployed in the periods after initially becoming laid off until their level of bitterness is measured again in $t+5$, therefore excluding individuals who temporarily leave unemployment – either because they found a new job or they left the labor market. In column (2), instead of just focusing on full-time or part-time employed respondents, we also include individuals who were either in irregular employment or pursued further education in t. In column (3), we expand the control group by including all individuals, who have remained employed, regardless of whether they stayed with the same firm or not, while in column (4), we only look at individuals of whom we know that they were employed in every month between t and $t+5$. Across specifications, the estimates are very similar to baseline results in size and significance, supporting the robustness of our overall results.

Table 3.6 Robustness: Include Related Concepts as Conditioning Variables

	(1) Life Sat.	(2) Pos. Recipr.	(3) Neg. Recipr.	(4) Depr. 1	(5) Depr. 2	(6) Depr. 3
Treatment	0.185***	0.234***	0.242***	0.226***	0.241***	0.249***
	(0.0508)	(0.0485)	(0.0475)	(0.0504)	(0.0506)	(0.0512)
Treated N	283	281	281	264	264	264
Control N	10902	10855	10826	10189	10189	10189
R2	0.441	0.442	0.455	0.457	0.454	0.455
Basic Cond. Var.	Yes	Yes	Yes	Yes	Yes	Yes
Health Cond. Var.	Yes	Yes	Yes	Yes	Yes	Yes
Ext. Labor Cond. Var.	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table reports robustness checks for the main analysis in Table 3.4 by including measures of related concepts in t and $t+5$ as conditioning variables. The outcome variable bitterness is standardized and the regressions are otherwise specified as in Table 3.4, column (3). Column (1): Includes measure of life satisfaction. (2): Index for positive reciprocity. (3): Index for negative reciprocity. (4): First measure of relative deprivation: Household income is below reference income. (5): Second measure of relative deprivation: Household income is below reference income minus one standard deviation of reference income. (6): Third measure of relative deprivation: Household income is below 0.6 of the median household income. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$

Another potential concern might relate to whether our results actually display the impact of unemployment on bitterness or whether they reflect the effects of similar but separate concepts, namely life satisfaction, positive reciprocity, negative reciprocity, and social deprivation. We therefore test in Table 3.6 whether our results hold when we additionally include measures of these concepts in our regressions. To capture the change in these attitudinal expressions over time, we include the information in t and $t + 5$ as conditioning variables. E.g., in column (1) we additionally include a measure of life satisfaction[14] in t and $t + 5$ as conditioning variables, while in columns (2) and (3), we do so for positive and negative reciprocity[15], respectively. In columns (4) to (6), we employ information on three distinct measures of social deprivation.[16]

The estimates in Table 3.6 show that, overall, even after controlling for these separate measures, the coefficient remains fairly stable and highly significant. While life satisfaction cuts down our coefficient a little bit, it remains high and significant. Moreover, including the other measures leaves the estimate virtually unchanged. This indicates that unemployment affects bitterness separately from life satisfaction and the other related concepts, further supporting that bitterness is a distinct concept.

In our baseline regressions in Table 3.4, we already include a large set of conditioning variables to show that our results remain fairly robust to the selection of those. To evaluate this further, Table 3.A11 in the appendix shows estimates when we additionally include various measures of concerns and satisfactions as conditioning variables (columns 1-7), among them respondents' worries about job security and their own economic situation, and their satisfaction with their personal income. Results show that the coefficients remain remarkably stable to their inclusion. In column (8), we additionally test, whether our results hold, when we exclude the initial level of bitterness as a conditioning variable. While this cuts down the coefficient, it remains positive and significant.

[14]Life satisfaction is directly surveyed in the SOEP through the question "How satisfied are you with your life, all things considered?", which is scaled from 0 (lowest) to 10 (highest).

[15]To construct our measures of positive and negative reciprocity, we follow Caliendo et al. (2012). For positive reciprocity, we employ three questions in SOEP which are captured on a 1 to 7 Likert scale, namely: (1) "If someone does me a favor, I am prepared to return it"; (2) "I go out of my way to help somebody who has been kind to me before"; (3) "I am ready to undergo personal costs to help somebody who helped me before". The information is then combined to construct an index, which is then introduced in the regression. For negative reciprocity we do the same by employing information from the following three statements: (1) "If I suffer a serious wrong, I will take revenge as soon as possible, no matter what the cost"; (2) "If somebody puts me in a difficult position, I will do the same to him/her"; (3) "If somebody offends me, I will offend him/her back". All questions were asked in 2005, 2010, 2015, and 2020, i.e., in the same waves as the bitterness question.

[16]The first measure categorizes respondents as socially deprived, if their household income is below the reference household income, which is calculated based on each respondent's sex, age, education, and state of residence. The second measure is one if the household income is below the reference income minus one standard deviation of reference income. The third measure is one, if the household income is below 0.6 of the median household income.

In our main estimations, we exploit the full range of our outcome variable, treating it as if it was cardinally-scaled. In Table 3.A12 in the appendix, results are shown when using recoded binary outcome variables. In column (1), we define bitterness to be equal to 1, if respondents report at least a 5 in their level of bitterness, with lower values being coded as 0. In columns (2) and (3), we increase this cutoff value to 6 and 7, respectively, with lower values coded as 0. Using these specifications, coefficients remain positive and highly significant. Moreover, it becomes clear, that unemployment appears to be one of the main drivers of extreme levels of bitterness. While around 10 percent of respondents in the treatment group have an initial bitterness of 6 or 7, this share increases by more 80 percent after becoming unemployed. The relative increase is even larger for respondents with the highest level of bitterness, whose share increases by more than 100 percent.

Lastly, we do two final checks. First, Table 3.A13 in the appendix shows that our results are robust to the clustering of standard errors at different levels. While we use robust standard errors for our baseline estimations, the significance of our estimates remains very similar when we cluster standard errors at the individual, household, and state level. Second, results remain very much in line with our baseline results when we employ inverse probability weighting instead of entropy balancing as Table 3.A14 in the appendix shows.

3.4.4 Unemployment Duration, Heterogeneities, and Persistence

In this section, we perform various heterogeneity analyses of our treatment, and evaluate to what extent our main effects appear to hold long-term.

First, we test to what extent one's unemployment duration may impact bitterness. For this purpose, we split our original treatment sample into three groups: First, we look at respondents who were laid off fairly early, namely within one or two years ($t+1$ or $t+2$) after their initial interview and then remained unemployed until the next interview where one's level of bitterness was elicited ($t+5$). Second, we group those, who became unemployed in the third or fourth year after the interview ($t+3$ or $t+4$) and who were still unemployed in $t+5$. Third, we look at those respondents who lost their jobs in the fifth year ($t+5$), i.e., just before their bitterness was captured again.

Table 3.7 shows that respondents with a longer unemployment duration were substantially more bitter than those who became jobless more recently. Column (1)

Table 3.7 Heterogeneity Analysis: By Length of Unemployment

	Treated Became Unemployed		
	(1)	(2)	(3)
	In t+1 or t+2	In t+3 or t+4	In t+5
Treatment Var. 1a	0.476***		
	(0.0902)		
Treatment Var. 1b		0.317***	
		(0.0699)	
Treatment Var. 1c			0.185***
			(0.0673)
Treated N	27	82	112
Control N	10914	10914	10914
R2	0.513	0.534	0.436
Basic Cond. Var.	Yes	Yes	Yes
Health Cond. Var.	Yes	Yes	Yes
Ext. Labor Cond. Var.	Yes	Yes	Yes

Note: This table reports results of heterogeneity analyses by length of unemployment for the treatment group. The outcome variable bitterness is standardized and the regressions are specified as in Table 3.4, column (3). Column(1): Treatment group consists of those who became unemployed in $t+1$ or $t+2$. (2): Only those who became unemployed in $t+3$ or $t+4$. (3): Only those who became unemployed in $t+5$. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$

shows that respondents who lost their employment within two years after the initial interview had an increase in bitterness of nearly half of a standard deviation, while bitterness was only 18.5 percent of a standard deviation higher for those who lost their jobs in the year leading up to the interview in $t+5$. Hence, our estimates signal a striking relationship between length of unemployment and bitterness.

Second, we perform a number of heterogeneity analyses on the individual level in Tables 3.A15, 3.A16, and 3.A17. In Table 3.A15, we split the sample by different demographic characteristics. While results do not differ meaningfully by gender and age, the coefficient when restricting our sample to migrants is insignificant and fairly close to zero, suggesting that unemployment affects natives and migrants differently in terms of bitterness. In Table 3.A16, we distinguish respondents by initial labor market earnings and history of unemployment. Hereby, we see that the coefficients are smaller for respondents with lower incomes and for those who have been unemployed before. While the former could be explained by the smaller drop in income, status, and living standards, the reasons for the latter are more unclear. People who have been unemployed before often have lower incomes (Mroz and Savage, 2006), so this could just be a reflection of the effects when distinguishing by income. Another possible reason might be that this group is more familiar with the experience of unemployment and have a different reaction because of that (Knabe and Rätzel, 2011). Lastly, Table 3.A17 shows results when splitting the sample by initial bitterness and health. The estimates in columns (1) and (2) do not reveal major differences in effects by initial bitterness. In contrast, the coefficient is smaller for individuals, who report that their health is not good. While the reasons for that are not clear, these effects, again, could be a reflection of labor market differences, as people with poorer health often have lower incomes and are more likely to become unemployed (Currie and Madrian, 1999; Contoyannis and Rice, 2001; Cai, 2009).

Lastly, we test the persistence of our effects in Table 3.8. We do so by looking at the change in bitterness between t and $t + 10$, i.e., ten years after the initial interview, and considerably past the original treatment. In column (1), our treatment sample consists of the respondents from the baseline estimations – i.e., those who were initially employed in t, then affected by plant closure or layoff, who registered as unemployed in the same period, and were still unemployed in $t + 5$ –, whose bitterness level was elicited in $t + 10$. This means that we do not impose any further restrictions on this group after $t + 5$. The treatment group in column (2) is analogous to the one in Table 3.5, column (3), meaning that we do not require them to be unemployed in $t + 5$. In column (3), we additionally do not require individuals to register as unemployed after becoming laid off – analogous to Table 3.5, column (4). The control group in all three specifications consists of individuals who were employed in all years between t and $t + 10$.

The results on the long-term persistence of effects are somewhat mixed. In column (1), the coefficient is highly significant and large at 19 percent of a standard deviation, which would indicate that the effect of unemployment largely persists across time. In contrast, the estimates in columns (2) and (3) are markedly smaller and insignificant. One possible explanation for these results could be that longer periods of unemployment have more persistent effects on individuals' bitterness. This is supported by the results in columns (4) to (6), in which the treatment group consists of individuals who remain unemployed for at least two, three, and four years after being treated, respectively. Estimates grow in size and significance, the longer treated individuals remain unemployed.

Table 3.8 Extension: Long-Term Effects

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Var. 1	0.190***					
	(0.0733)					
Treatment Var. 3		0.0912				
		(0.0584)				
Treatment Var. 4			0.0650			
			(0.0428)			
Treatment Var. 1d				0.138*		
				(0.0774)		
Treatment Var. 1e					0.223**	
					(0.0896)	
Treatment Var. 1f						0.316***
						(0.0850)
Treated N	137	317	636	122	51	28
Control N	3091	3091	3091	3091	3091	3091
R2	0.498	0.469	0.433	0.535	0.590	0.693
Basic Cond. Var.	Yes	Yes	Yes	Yes	Yes	Yes
Health Cond. Var.	Yes	Yes	Yes	Yes	Yes	Yes
Ext. Labor Cond. Var.	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table examines the long-term effects of involuntary unemployment on bitterness, by studying outcomes in $t + 10$. The outcome variable bitterness is standardized and the regressions are otherwise specified as in Table 3.4, column (3). Column (1): Includes only respondents as in baseline, i.e., those who registered as unemployed after being laid off by firm and were unemployed in $t + 5$. (2): Relaxes the assumption of unemployment in $t + 5$ for treated individuals. (3): Includes all individuals in treatment group affected by plant closure, regardless of unemployment. (4): Only those respondents are treated who remained unemployed for at least two years after job loss. (5): Treated are those who remained unemployed for at least three years. (6): Treated are those who remained unemployed for at least four years. The control group in all specifications consists of individuals who remained employed throughout all years until $t + 10$. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$

Overall, these results suggest that the positive effect of unemployment on bitterness only persists in cases where respondents remain unemployed for longer periods of time, while the effect dissipates for short-term unemployment.

3.5 Conclusion

This study examines the impact of unemployment on bitterness, which describes the notion of feeling like one has not achieved what one deserves compared to others. Hereby, we first perform pooled OLS and fixed effects regressions, finding consistently large and significant positive effects of unemployment, even after controlling for a large set of demographic, social, educational, economic, and health-related covariates. Moreover, results also hold when taking involuntary and long-term unemployment into account.

To identify the causal effects of unemployment on bitterness, we exploit plausibly exogenous variation from plant closures and firm layoffs and combine matching based on entropy balancing with difference-in-differences estimation, thereby controlling for observable characteristics and unobserved time-invariant heterogeneity. Doing so, we find that unemployment leads to large and significant increases in respondents' levels of bitterness. Coefficients indicate that it raises bitterness by around 25 percent of a standard deviation. Our results remain robust to modifications of the treatment and control group, and reveal that bitterness increases even when only examining job loss, regardless of whether the person affected becomes unemployed afterwards. Reassuringly, our results are are also robust to including related measures of various related concepts, namely life satisfaction, negative and positive reciprocity, and social deprivation into our regressions. The same holds true when we introduce variables measuring job- and income-related worries and satisfactions as conditioning variables, rerun regressions with a binary coding of our outcome variable, employ different clustering of standard errors, and use inverse probability weighting instead of entropy balancing.

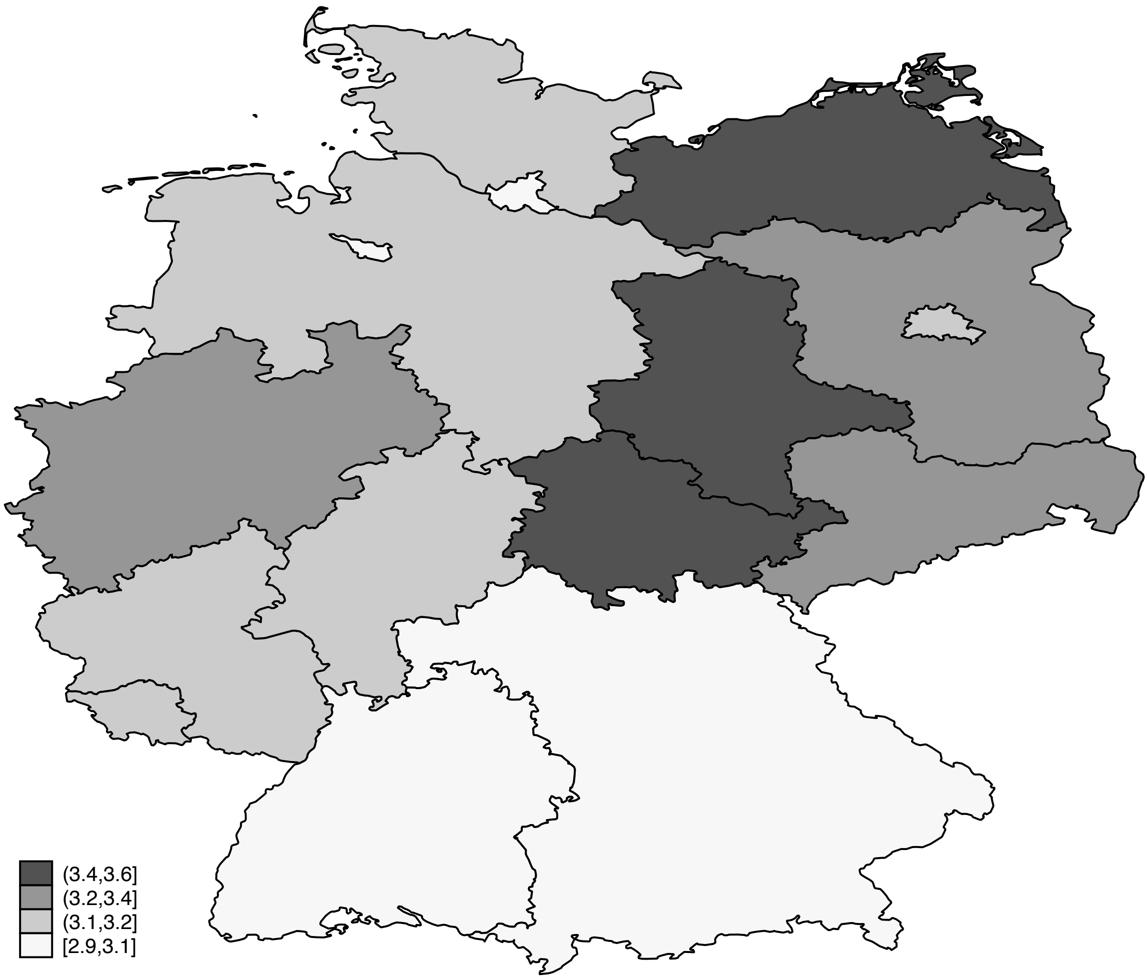
Lastly, we also examine to what extent unemployment duration impacts bitterness, and whether overall effects persist over time. Our results reveal a steady increase in bitterness the longer respondents remain unemployed. Furthermore, we find some evidence that the effects of unemployment last over time; yet, this only appears to be the case for people who remain unemployed for over one year. This implies that bitterness could potentially contribute to a downward spiral leading to prolonged unemployment.

Our study is the first to rigorously examine the impact of unemployment on bitterness in a quantitative, causal analysis, contributing to the literature on bitterness (or embitterment) (Linden and Maercker, 2011; Poutvaara and Steinhardt, 2018) and similar concepts like life satisfaction or positive and negative reciprocity (Caliendo et al., 2012). Moreover, our study adds to previous findings on the detrimental effects of unemployment on a wide range of factors like health (Schmitz, 2011), including mental health (Marcus, 2013; Strandh et al., 2014), social integration (Pohlan, 2019), and life satisfaction (Kassenboehmer and Haisken-DeNew, 2009; Luechinger et al., 2010).

With respect to policy, our paper highlights that unemployment, in particular long-term unemployment, can induce bitterness among citizens. This may result in growing worries about immigration and support for right-wing populist parties (Poutvaara and Steinhardt, 2018), increasing the likelihood of a political backlash against open societies. Hence, our paper furnishes additional evidence that unemployment constitutes more than just personal economic distress, but can yield severe repercussions for society as a whole. This makes it important to design unemployment policies that help to mitigate the psychosocial toll of unemployment. In particular, our paper stresses the importance for initiatives that prevent the development of frustration and anger among the unemployed, instead fostering a positive and resilient outlook on life.

3.A Additional Tables and Figures (Appendix)

Figure 3.A1 Variation in Mean Bitterness across States



Note: Spatial distribution of average bitterness in different German states (*Bundesländer*) when pooling all sample years. Brighter colors indicate lower and darker colors higher average bitterness.

Table 3.A1 Descriptive Statistics: Bitterness

	2005	%	2010	%	2015	%	2020	%	Total	%
1	3417	20.94	2811	20.10	4360	20.46	4725	23.23	15313	21.28
2	3397	20.82	3091	22.11	4849	22.75	4938	24.28	16275	22.62
3	2126	13.03	2090	14.95	2994	14.05	2866	14.09	10076	14.00
4	2964	18.16	2164	15.48	3367	15.80	2999	14.75	11494	15.97
5	2346	14.38	2042	14.60	3094	14.52	2656	13.06	10138	14.09
6	1323	8.11	1228	8.78	1772	8.31	1461	7.18	5784	8.04
7	746	4.57	557	3.98	877	4.11	691	3.40	2871	3.99
Mean	3.27	-	3.25	-	3.23	-	3.05	-	3.19	-
(SD)	(1.79)	-	(1.77)	-	(1.77)	-	(1.74)	-	(1.77)	-
Total	16319	100.00	13983	100.00	21313	100.00	20336	100.00	71951	100.00

Note: This table reports the response frequencies, means, and standard deviations of the main outcome variable, bitterness, in the SOEP for the sample years from 2005 to 2020. Response options are scaled from 1 (full disagreement) to 7 (full agreement).

Table 3.A2 Descriptive Statistics of Explanatory Variables

	Obs.	Mean	Std. Dev.
unemployed	71,951	0.06	0.24
full time	71,951	0.49	0.50
part time	71,951	0.16	0.37
irregular work	71,951	0.09	0.29
in education	71,951	0.07	0.26
non-working	71,951	0.12	0.32
male	71,944	0.47	0.50
age	71,951	41.98	12.95
migrant	71,951	0.25	0.44
foreign born	71,951	0.19	0.39
married	71,695	0.56	0.50
divorced	71,695	0.09	0.28
has children	71,780	0.43	0.49
has msa	71,951	0.30	0.46
has abitur	71,951	0.30	0.46
university degree	71,951	0.24	0.43
vocational training	71,951	0.61	0.49
labor income	54,198	2812.69	2746.52
sick	71,853	0.14	0.34
disabled	71,717	0.08	0.28

Note: The table reports means and standard deviations of all explanatory variables used in the OLS regressions, apart from state of residence and year. In the regressions, missing values are accounted for using dummy variables and recoded as zero.

Table 3.A3 OLS Regression Showing Coefficients of Control Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
unemployed	0.5669***	0.5590***	0.5427***	0.4316***	0.3666***	0.3319***	0.2827***	0.2570***
	(0.0165)	(0.0166)	(0.0167)	(0.0168)	(0.0266)	(0.0266)	(0.0296)	(0.0299)
part-time	-0.0225**	0.0529***	0.0719***	0.0529***	-0.0288**	-0.0317**	-0.0317**	-0.0319**
	(0.0114)	(0.0121)	(0.0122)	(0.0120)	(0.0128)	(0.0127)	(0.0127)	(0.0127)
irregular work	0.0520***	0.1209***	0.1339***	0.0815***	-0.0954***	-0.1028***	-0.1032***	-0.1039***
	(0.0138)	(0.0145)	(0.0146)	(0.0146)	(0.0190)	(0.0189)	(0.0189)	(0.0189)
in education	-0.0753***	0.0270	0.0289	-0.0282	-0.1176***	-0.1257***	-0.1274***	-0.1252***
	(0.0146)	(0.0181)	(0.0181)	(0.0184)	(0.0221)	(0.0220)	(0.0220)	(0.0220)
non working	0.1795***	0.2092***	0.2383***	0.1621***	0.0880***	0.0499**	0.0504**	0.0522**
	(0.0133)	(0.0141)	(0.0142)	(0.0141)	(0.0253)	(0.0253)	(0.0253)	(0.0253)
2010	-0.0013	0.0003	-0.0057	0.0020	-0.0060	-0.0082	-0.0077	-0.0076
	(0.0103)	(0.0103)	(0.0103)	(0.0102)	(0.0102)	(0.0102)	(0.0102)	(0.0102)
2015	-0.0109	-0.0443***	-0.0474***	-0.0338***	-0.0420***	-0.0463***	-0.0455***	-0.0446***
	(0.0099)	(0.0100)	(0.0100)	(0.0099)	(0.0099)	(0.0099)	(0.0099)	(0.0099)
2020	-0.0941***	-0.1309***	-0.1395***	-0.1113***	-0.1101***	-0.1118***	-0.1112***	-0.1113***
	(0.0102)	(0.0103)	(0.0103)	(0.0102)	(0.0102)	(0.0102)	(0.0102)	(0.0102)
male		0.1303***	0.1426***	0.1220***	0.1404***	0.1375***	0.1375***	0.1353***
		(0.0092)	(0.0092)	(0.0091)	(0.0091)	(0.0090)	(0.0090)	(0.0090)
age		0.0072***	0.0180***	0.0300***	0.0310***	0.0275***	0.0272***	0.0267***
		(0.0023)	(0.0026)	(0.0026)	(0.0026)	(0.0026)	(0.0026)	(0.0026)
age2		-0.0000*	-0.0002***	-0.0003***	-0.0003***	-0.0003***	-0.0003***	-0.0003***
		(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
migrant		0.1273***	0.1318***	0.0966***	0.0915***	0.0920***	0.0920***	0.0919***
		(0.0173)	(0.0173)	(0.0170)	(0.0169)	(0.0168)	(0.0168)	(0.0168)
foreign born		0.1359***	0.1495***	0.0431**	0.0353*	0.0434**	0.0442**	0.0443**
		(0.0190)	(0.0191)	(0.0194)	(0.0193)	(0.0192)	(0.0192)	(0.0191)
divorced			0.0852***	0.0422**	0.0449***	0.0393**	0.0386**	0.0383**
			(0.0178)	(0.0175)	(0.0173)	(0.0172)	(0.0172)	(0.0172)
married			-0.1100***	-0.1158***	-0.1098***	-0.1012***	-0.1005***	-0.1002***
			(0.0116)	(0.0114)	(0.0113)	(0.0113)	(0.0113)	(0.0113)
has child(ren)			-0.0400***	-0.0396***	-0.0325***	-0.0253***	-0.0254***	-0.0243***
			(0.0093)	(0.0091)	(0.0091)	(0.0090)	(0.0090)	(0.0090)
has msa				-0.1785***	-0.1699***	-0.1615***	-0.1610***	-0.1611***
				(0.0113)	(0.0113)	(0.0112)	(0.0112)	(0.0112)
has abitur				-0.3151***	-0.2932***	-0.2808***	-0.2803***	-0.2793***
				(0.0120)	(0.0120)	(0.0119)	(0.0119)	(0.0119)
university degree				-0.2195***	-0.1865***	-0.1773***	-0.1766***	-0.1766***
				(0.0121)	(0.0122)	(0.0122)	(0.0122)	(0.0122)
vocational training				-0.0321***	-0.0324***	-0.0313***	-0.0310***	-0.0320***
				(0.0107)	(0.0106)	(0.0106)	(0.0106)	(0.0106)
log labor income					-0.1132***	-0.1081***	-0.1081***	-0.1070***
					(0.0062)	(0.0062)	(0.0062)	(0.0062)
sick						0.2401***	0.2391***	0.2390***
						(0.0118)	(0.0118)	(0.0118)
disabled						0.0669***	0.0667***	0.0683***
						(0.0160)	(0.0160)	(0.0160)
long-term unemployed							0.1120***	0.1335***
							(0.0296)	(0.0298)
involuntary job loss								0.1280***
								(0.0225)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	71951	71951	71951	71951	71951	71951	71951	71951
r2	0.030	0.044	0.049	0.078	0.085	0.092	0.092	0.093

Note: The table reports OLS estimates of equation 3.1, gradually adding more regressors. The outcome variable is standardized. Standard errors (in parentheses) are clustered at the person level.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 3.A4 Fixed Effects Regression Showing Coefficients of Control Variables

	(1)	(2)	(3)	(4)	(5)
unemployed	0.1304***	0.1314***	0.1379***	0.1342***	0.1265***
	(0.0279)	(0.0281)	(0.0443)	(0.0443)	(0.0446)
part-time	0.0211	0.0197	0.0048	0.0058	0.0050
	(0.0195)	(0.0198)	(0.0204)	(0.0204)	(0.0204)
irregular work	0.0230	0.0224	-0.0086	-0.0080	-0.0094
	(0.0224)	(0.0240)	(0.0287)	(0.0287)	(0.0287)
in education	-0.0835***	-0.0844**	-0.0942**	-0.0933**	-0.0928**
	(0.0292)	(0.0347)	(0.0394)	(0.0394)	(0.0394)
non working	-0.0170	-0.0094	-0.0057	-0.0073	-0.0080
	(0.0233)	(0.0243)	(0.0421)	(0.0422)	(0.0422)
age		0.1583	0.1534	0.1597	0.1637
		(0.2305)	(0.2305)	(0.2305)	(0.2308)
age2		-0.0001*	-0.0001**	-0.0001**	-0.0001**
		(0.0001)	(0.0001)	(0.0001)	(0.0001)
divorced		0.0202	0.0216	0.0234	0.0240
		(0.0361)	(0.0362)	(0.0362)	(0.0362)
married		-0.0394*	-0.0385	-0.0380	-0.0378
		(0.0235)	(0.0235)	(0.0234)	(0.0234)
has child(ren)		0.0128	0.0127	0.0136	0.0141
		(0.0161)	(0.0161)	(0.0161)	(0.0161)
has msa		-0.0052	-0.0228	-0.0220	-0.0235
		(0.1029)	(0.1036)	(0.1038)	(0.1039)
has abitur		-0.0474	-0.0681	-0.0658	-0.0664
		(0.0576)	(0.0593)	(0.0593)	(0.0593)
university degree		-0.0711*	-0.0710*	-0.0709*	-0.0716*
		(0.0377)	(0.0381)	(0.0380)	(0.0380)
vocational training		-0.0068	0.0017	0.0025	0.0021
		(0.0417)	(0.0423)	(0.0422)	(0.0422)
log labor income			-0.0295***	-0.0286***	-0.0282***
			(0.0091)	(0.0091)	(0.0091)
sick				0.0841***	0.0841***
				(0.0176)	(0.0176)
disabled				0.0264	0.0270
				(0.0307)	(0.0307)
involuntary job loss					0.0479
					(0.0327)
Indiv. FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
N	71951	71951	71951	71951	71951
r2	0.002	0.003	0.004	0.005	0.005
r2_b	0.011	0.004	0.005	0.005	0.005
r2_o	0.009	0.003	0.004	0.004	0.004

Note: The table reports fixed effects regression estimates of equation 3.2, gradually adding more regressors. The outcome variable is standardized. Standard errors (in parentheses) are clustered at the person level.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 3.A5 Fixed Effects Regression with Binary Outcome Variable

	(1)	(2)	(3)
unemployed	0.0410*	0.0493***	0.0280***
	(0.0223)	(0.0173)	(0.0108)
part-time	-0.0002	0.0023	0.0050
	(0.0099)	(0.0073)	(0.0042)
irregular work	-0.0004	0.0022	0.0104*
	(0.0138)	(0.0106)	(0.0062)
in education	-0.0329*	-0.0164	0.0154*
	(0.0190)	(0.0139)	(0.0083)
non working	0.0079	0.0025	0.0176*
	(0.0206)	(0.0153)	(0.0091)
log labor income	-0.0087**	-0.0035	0.0004
	(0.0043)	(0.0034)	(0.0022)
sick	0.0315***	0.0166**	0.0073
	(0.0090)	(0.0071)	(0.0045)
disabled	0.0106	0.0083	0.0037
	(0.0154)	(0.0123)	(0.0079)
Other Exp. Variables	Yes	Yes	Yes
Indiv. FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	71951	71951	71951
Mean	0.261	0.120	0.040
r2	0.004	0.003	0.003
r2_b	0.005	0.003	0.001
r2_o	0.004	0.002	0.001

Note: The table reports fixed effects regression estimates of equation 3.2 using a binary outcome variable. Column (1): Outcome is coded 1, if respondents report 5, 6 or 7 on the 7-point Likert scale in their level of bitterness, and lower values coded as 0. Column (2): Outcome is coded 1, if respondents report 6 or 7, otherwise it is equal to 0. Column (3): Outcome is coded 1, if respondents report 7, otherwise it is equal to 0. Regressions otherwise specified as in Table 3.2, column (4). Standard errors (in parentheses) are clustered at the person level.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 3.A6 FE Regression: Heterogeneity by Gender and Migration Background

	(1) Male	(2) Female	(3) Native	(4) Migrant
unemployed	0.0813	0.1880***	0.1300***	0.1918*
	(0.0662)	(0.0597)	(0.0491)	(0.1023)
part-time	-0.0127	0.0124	-0.0132	0.0892*
	(0.0439)	(0.0244)	(0.0221)	(0.0513)
irregular work	-0.0303	0.0131	-0.0456	0.1486**
	(0.0480)	(0.0364)	(0.0315)	(0.0692)
in education	-0.1300**	-0.0540	-0.1154***	-0.0049
	(0.0595)	(0.0535)	(0.0443)	(0.0869)
non working	-0.0252	0.0431	-0.0453	0.1380
	(0.0706)	(0.0542)	(0.0469)	(0.0974)
age	0.1774	0.1355	0.2231	-0.0194
	(0.3612)	(0.2856)	(0.2497)	(0.5866)
age2	-0.0001	-0.0001	-0.0001**	0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0002)
divorced	0.0414	0.0153	0.0101	0.0867
	(0.0555)	(0.0476)	(0.0383)	(0.1006)
married	0.0158	-0.0712**	-0.0290	-0.0818
	(0.0353)	(0.0314)	(0.0255)	(0.0592)
has child(ren)	-0.0014	0.0261	0.0052	0.0548
	(0.0236)	(0.0224)	(0.0176)	(0.0396)
has msa	0.1289	-0.1708	-0.0395	0.0646
	(0.1535)	(0.1329)	(0.1166)	(0.2241)
has abitur	0.1217	-0.2343***	-0.0549	-0.0764
	(0.0882)	(0.0779)	(0.0681)	(0.1209)
university degree	0.0590	-0.1808***	-0.0680*	-0.0752
	(0.0523)	(0.0545)	(0.0409)	(0.1055)
vocational training	0.0172	-0.0018	-0.0120	0.0363
	(0.0645)	(0.0558)	(0.0469)	(0.0947)
log labor income	-0.0448***	-0.0153	-0.0370***	0.0118
	(0.0133)	(0.0125)	(0.0100)	(0.0214)
sick	0.0710***	0.0953***	0.0723***	0.1402***
	(0.0273)	(0.0231)	(0.0191)	(0.0450)
disabled	0.0275	0.0212	0.0249	0.0412
	(0.0449)	(0.0420)	(0.0330)	(0.0830)
Indiv. FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	33806	38145	53627	18324
r2	0.007	0.007	0.006	0.009
r2_b	0.002	0.010	0.007	0.001
r2_o	0.001	0.008	0.005	0.001

Note: The table reports fixed effects regression estimates of equation 3.2 by subgroup for the full set of independent variables. Column (1): Only men. (2): Only women. (3): Only natives. (4): Only people with direct (foreign born) or indirect (at least one parent foreign born) migration background. Standard errors (in parentheses) are clustered at the person level.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 3.A7 Fixed Effects Regression: Heterogeneity by Place of Residence

	(1) West	(2) East	(3) Urban	(4) Rural
unemployed	0.1565***	0.0796	0.1837***	0.0731
	(0.0538)	(0.0848)	(0.0567)	(0.0752)
part-time	0.0068	-0.0097	0.0217	-0.0112
	(0.0238)	(0.0412)	(0.0257)	(0.0349)
irregular work	-0.0191	0.0170	0.0132	-0.0169
	(0.0332)	(0.0625)	(0.0365)	(0.0491)
in education	-0.1020**	-0.0591	-0.0681	-0.1072
	(0.0448)	(0.0878)	(0.0508)	(0.0680)
non working	0.0222	-0.0859	0.0403	-0.0523
	(0.0488)	(0.0889)	(0.0527)	(0.0745)
age	0.0320	0.5026	0.0392	0.3968
	(0.2510)	(0.6046)	(0.2837)	(0.4265)
age2	-0.0001	-0.0002*	-0.0001*	-0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
divorced	-0.0052	0.1287*	0.0254	0.0453
	(0.0418)	(0.0745)	(0.0460)	(0.0625)
married	-0.0542**	-0.0030	-0.0519*	-0.0075
	(0.0272)	(0.0491)	(0.0291)	(0.0423)
has child(ren)	0.0111	0.0284	0.0024	0.0262
	(0.0184)	(0.0345)	(0.0200)	(0.0286)
has msa	-0.0396	0.0328	-0.1528	0.0825
	(0.1178)	(0.2295)	(0.1266)	(0.2009)
has abitur	-0.1090*	0.1383	-0.0920	-0.0480
	(0.0648)	(0.1660)	(0.0720)	(0.1220)
university degree	-0.0694	-0.1297	-0.0883*	-0.0594
	(0.0436)	(0.0899)	(0.0470)	(0.0775)
vocational training	0.0166	-0.0765	0.0356	-0.0222
	(0.0484)	(0.0913)	(0.0529)	(0.0776)
log labor income	-0.0224**	-0.0477***	-0.0202*	-0.0443***
	(0.0110)	(0.0172)	(0.0114)	(0.0159)
sick	0.0902***	0.0651*	0.0665***	0.1143***
	(0.0202)	(0.0369)	(0.0218)	(0.0310)
disabled	0.0536	-0.0271	0.0453	-0.0056
	(0.0355)	(0.0613)	(0.0380)	(0.0524)
Indiv. FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	56713	15238	47578	24373
r2	0.004	0.011	0.005	0.008
r2_b	0.014	0.016	0.011	0.008
r2_o	0.013	0.011	0.010	0.006

Note: The table reports fixed effects regression estimates of equation 3.2 by subgroup for the full set of independent variables. Column (1): Only residents in formerly West German states. (2): Only residents in formerly East German states. (3): Only residents in urban areas. (4): Only residents in rural areas. Standard errors (in parentheses) are clustered at the person level.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 3.A8 Conditioning Variables and their Categorization

variable	category
bitterness	main
east germany	basic
rural	basic
year = 2005	basic
year = 2010	basic
interview month: jan/feb	basic
interview month: march	basic
interview month: april	basic
interview month: may	basic
interview month: june	basic
age*	basic (socio-demographic)
female	basic (socio-demographic)
migration background	basic (socio-demographic)
foreign born	basic (socio-demographic)
married**	basic (socio-demographic)
divorced or separated**	basic (socio-demographic)
widowed**	basic (socio-demographic)
has children**	basic (socio-demographic)
real labor income*,**	basic (labor)
never unemployed**	basic (labor)
works full time	basic (labor)
work experience*,**	basic (labor)
school degree: Hauptschule	basic (education)
school degree: Realschule	basic (education)
school degree: Fachabitur	basic (education)
school degree: Abitur	basic (education)
university degree	basic (education)
vocational training	basic (education)
has bad health	health
has medium health	health
has good health	health
disabled**	health
tenure*,**	extended labor
firm size: <20	extended labor
firm size: >=20 & <200	extended labor
firm size: >=200 & <2000	extended labor
firm size: >=2000	extended labor
firm size: self-employed	extended labor
industry: primary sector	extended labor
industry: manufacturing	extended labor
industry: construction	extended labor
industry: wholesale & retail	extended labor
industry: hotel & restaurants	extended labor
industry: transport	extended labor
industry: banking & insurance	extended labor
industry: health services	extended labor
industry: other services	extended labor
industry: missing	extended labor

Note: The table reports the set of conditioning variables used in the matching procedure for the causal analysis as well as their categorization in the subsequent regressions. *: Also includes squared term. **: Also includes variable for missing information.

Table 3.A9 Characteristics of Treatment and Control Group Before and After Matching

	Treatment		Control		Matched Control	
	mean	variance	mean	variance	mean	variance
bitterness	3.786	3.197	3.023	2.955	**3.785**	**3.082**
age	44.2	105.9	45.32	78.29	**44.2**	**105.6**
age squared	2059	748178	2133	619767	**2059**	**826586**
female	.4526	.2486	.4511	.2476	**.4526**	**.2478**
migration background	.2211	.1728	.1257	.1099	**.221**	**.1722**
foreign born	.1754	.1452	.09284	.08423	**.1754**	**.1447**
married	.5649	.2467	.6683	.2217	**.5649**	**.2458**
divorced or separated	.1579	.1334	.129	.1124	**.1579**	**.133**
widowed	.03509	.03398	.01282	.01266	**.03508**	**.03385**
has children	.3719	.2344	.4452	.247	**.3719**	**.2336**
year = 2005	.5158	.2506	.3599	.2304	**.5158**	**.2498**
year = 2010	.2702	.1979	.3726	.2338	**.2702**	**.1972**
east Germany	.2807	.2026	.2276	.1758	**.2806**	**.2019**
rural	.3544	.2296	.352	.2281	**.3543**	**.2288**
interview month: jan/feb	.3088	.2142	.27	.1971	**.3088**	**.2134**
interview month: march	.2596	.1929	.3081	.2132	**.2597**	**.1923**
interview month: april	.1088	.09728	.1589	.1336	**.1088**	**.09696**
interview month: may	.07719	.07149	.07215	.06695	**.0772**	**.07125**
interview month: june	.1579	.1334	.1103	.09816	**.1579**	**.133**
info on marriage status missing	0	0	.002381	.002375	**.0000759**	**.0000759**
info on children missing	.003509	.003509	.0006409	.0006406	**.003507**	**.003495**
real labor income	2286	2409496	3449	6368306	**2287**	**2431148**
real labor income squared	7625001	3.01e+14	1.83e+07	2.81e+15	**7662029**	**2.16e+15**
works full-time	.7544	.1859	.7743	.1748	**.7544**	**.1853**
work experience full-time	17.74	125.8	18.28	111.4	**17.74**	**125.4**
work experience full-time squared	439.9	181090	445.7	177794	**440**	**210570**
never unemployed	.4	.2408	.6914	.2134	**.4002**	**.2401**
no info on employment history	.007018	.006993	.0006409	.0006406	**.007015**	**.006967**
school degree: Hauptschule	.3088	.2142	.2097	.1657	**.3087**	**.2134**
school degree: Realschule	.4877	.2507	.4231	.2441	**.4877**	**.2499**
school degree: Fachabitur	.04211	.04047	.07608	.0703	**.04212**	**.04035**
school degree: Abitur	.1439	.1236	.2842	.2034	**.144**	**.1232**
university degree	.1649	.1382	.3107	.2142	**.165**	**.1378**
vocational training	.7895	.1668	.7575	.1837	**.7894**	**.1663**
has bad health	.3965	.2401	.3169	.2165	**.3964**	**.2393**
has medium health	.4386	.2471	.5863	.2426	**.4386**	**.2463**
has good health	.0807	.07445	.05255	.0498	**.08069**	**.07419**
disabled	.003509	.003509	.001465	.001463	**.003534**	**.003521**
info on disability status missing	0	0	.002197	.002193	**.0000638**	**.0000638**
tenure	7.577	79.25	13.63	95.86	**7.58**	**79.02**
tenure squared	136.4	81799	281.6	121116	**136.5**	**112149**
info on tenure missing	0	0	.0007325	.000732	**.0000203**	**.0000203**

Table 3.A9 Characteristics of Treatment and Control Group Before and After Matching (cont.)

	Treatment		Control		Matched Control	
	mean	variance	mean	variance	mean	variance
firm size: <=20	.3509	.2286	.2187	.1709	**.3508**	**.2278**
firm size: >=20 & <200	.3298	.2218	.2613	.193	**.3297**	**.221**
firm size: >=200 & <2000	.1509	.1286	.2089	.1653	**.1509**	**.1281**
firm size: >=2000	.09474	.08606	.2519	.1885	**.09486**	**.08587**
firm size: self-employed	.02456	.02404	.0391	.03757	**.02457**	**.02397**
industry: primary sector	.02456	.02404	.01914	.01877	**.02455**	**.02395**
industry: manufacturing	.3053	.2128	.2294	.1768	**.3052**	**.2121**
industry: construction	.003509	.003509	.01099	.01087	**.003514**	**.003502**
industry: wholesale & retail	.08421	.07739	.05054	.04799	**.08418**	**.0771**
industry: hotel & restaurants	.1789	.1474	.1035	.09277	**.179**	**.1469**
industry: transport	.05965	.05629	.05063	.04807	**.05963**	**.05608**
industry: banking & insurance	.01404	.01389	.04624	.0441	**.01408**	**.01388**
industry: health services	.06316	.05938	.1292	.1125	**.06316**	**.05918**
industry: other services	.1579	.1334	.3233	.2188	**.158**	**.133**
industry: missing	.05263	.05004	.0228	.02228	**.05264**	**.04987**

Note: The table reports mean and variance of all conditioning variables for the treatment and control group before and after matching. Treatment and control group were matched based on the shown observable variables.

Table 3.A10 Robustness: Modify Treatment and Control Group

	(1)	(2)	(3)	(4)
Treatment Var. 6	0.238***			
	(0.0502)			
Treatment Var. 7		0.253***		
		(0.0450)		
Treatment Var. 8			0.240***	
			(0.0468)	
Treatment Var. 9				0.224***
				(0.0584)
Treated N	251	362	285	285
Control N	10922	11133	14983	6473
R2	0.439	0.447	0.437	0.436
Basic Cond. Var.	Yes	Yes	Yes	Yes
Health Cond. Var.	Yes	Yes	Yes	Yes
Ext. Labor Cond. Var.	Yes	Yes	Yes	Yes

Note: This table reports robustness checks for the main analysis in Table 3.4 by changing the definitions of treatment and control group. The outcome variable bitterness is standardized and the regressions are otherwise specified as in Table 3.4, column (3). (1): Only includes respondents in treatment group who remained unemployed after initially becoming unemployed due a dismissal or plant closure. (2): Relaxes the assumption of employment in t, also includes respondents, who were in school or in irregular employment. (3): Control group consists of respondents who were employed in all periods, but not necessarily with the same firm. (4): Limits control group to respondents for whom there is additional information that they were employed in all months between t and $t+5$. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 3.A11 Robustness: Include Worries and Satisfactions as Conditioning Vars.

	Concerns about				Satisfaction with			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Job Sec.	Own Ec. Sit.	Health	Economy	Work	HH Income	Pers. Income	No Init. Bit.
Treatment	0.227***	0.236***	0.240***	0.254***	0.253***	0.247***	0.228***	0.133**
	(0.0485)	(0.0481)	(0.0482)	(0.0483)	(0.0480)	(0.0477)	(0.0473)	(0.0611)
Treated N	285	285	284	275	272	277	285	285
Control N	10922	10912	10902	10867	10827	10794	10893	10922
R2	0.446	0.449	0.444	0.448	0.453	0.454	0.452	0.0868
Basic Cond. Var.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Health Cond. Var.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ext. Labor Cond. Var.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table reports robustness checks for the main analysis in Table 3.4 by including measures of worries and satisfactions in t as conditioning variables in columns (1)-(7). The outcome variable bitterness is standardized and the regressions are otherwise specified as in Table 3.4, column (3). Column (1): Includes concerns about job security. (2): Includes concerns about own economic situation. (3): Includes concerns about own health. (4): Includes concerns about the economy in general. (5): Includes satisfaction with work. (6): Includes satisfaction with household income. (7): Includes satisfaction with personal income. (8): Excludes measure for initial bitterness in t. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 3.A12 Robustness: Use Binary Outcome Variable

	(1)	(2)	(3)
	Cutoff b/w 4 and 5	Cutoff b/w 5 and 6	Cutoff b/w 6 and 7
Treatment	0.0774***	0.0846***	0.0325**
	(0.0275)	(0.0237)	(0.0146)
Treated N	285	285	285
Control N	10922	10922	10922
Pre-Treatment Mean	0.237	0.104	0.029
R2	0.447	0.493	0.572
Basic Cond. Var.	Yes	Yes	Yes
Health Cond. Var.	Yes	Yes	Yes
Ext. Labor Cond. Var.	Yes	Yes	Yes

Note: This table reports results of the causal analysis of the effects of involuntary unemployment on bitterness when using a binary outcome variable. Regressions are otherwise specified as in Table 3.4, column (3). Column (1): Dependent variable is coded 1, if respondents report at least a 5 on the 7-point Likert scale in their level of bitterness, and lower values coded as 0. (2): Respondents report at least a 6 on the 7-point Likert scale, 0 otherwise. (3): Respondents report a 7 on the 7-point Likert scale, 0 otherwise. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 3.A13 Robustness: Clustering of Standard Errors

	(1) Robust	(2) By Individual	(3) By Household	(4) By State
Treatment	0.241***	0.241***	0.241***	0.241***
	(0.0481)	(0.0481)	(0.0486)	(0.0338)
Treated N	285	285	285	285
Control N	10922	10922	10922	10922
R2	0.444	0.444	0.444	0.444
Basic Cond. Var.	Yes	Yes	Yes	Yes
Health Cond. Var.	Yes	Yes	Yes	Yes
Ext. Labor Cond. Var.	Yes	Yes	Yes	Yes

Note: This table reports robustness checks for the main analysis in Table 3.4 by using different clustering. Regressions are otherwise specified as in Table 3.4, column (3). Column (1): Baseline estimation with robust standard errors. (2): Standard errors clustered at the individual level. (3): Standard errors clustered at the level of the original household. (4): Standard errors clustered at the state level.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 3.A14 Robustness: Inverse Probability Weighting

	(1)	(2)	(3)	(4)	(5)
Treatment	0.234***	0.219***	0.237***	0.229***	0.227***
	(0.0653)	(0.0654)	(0.0670)	(0.0668)	(0.0671)
Treated N	285	285	285	285	285
Control N	10764	10764	10764	10764	10764
R2	0.00845	0.00740	0.00877	0.00819	0.00797
Basic Cond. Var.	Yes	Yes	Yes	Yes	Yes
Health Cond. Var.		Yes	Yes	Yes	Yes
Ext. Labor Cond. Var.			Yes	Yes	Yes
State FE				Yes	Yes
State Cond. Var.					Yes

Note: This table reports the results of the causal analysis of the effects of involuntary unemployment on bitterness when using inverse probability weighting instead of entropy balancing to match treatment and control group. The outcome variable is standardized. Each column gradually expands the set of conditioning variables listed in Table 3.A8. (1): Basic conditioning variables, including demographic, educational, and labor market characteristics. (2): Additionally includes a set of health-related conditioning variables. (3): Additionally includes a set of extended labor market-related conditioning variables. (4): Uses state fixed effects instead of a dummy variable for the former Eastern states. (5): Additionally includes statewide unemployment rates and GDP per capita. Robust standard errors in parentheses.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 3.A15 Heterogeneity Analysis: By Demographic Characteristics

	(1) Men	(2) Women	(3) Native	(4) Migrant	(5) Age: < 50	(6) Age: 50+
Treatment	0.254***	0.217***	0.282***	0.0959	0.236***	0.238***
	(0.0615)	(0.0696)	(0.0554)	(0.0843)	(0.0592)	(0.0735)
Treated N	156	129	222	63	179	106
Control N	5995	4927	9549	1373	7131	3791
R2	0.456	0.483	0.449	0.504	0.458	0.491
Basic Cond. Var.	Yes	Yes	Yes	Yes	Yes	Yes
Health Cond. Var.	Yes	Yes	Yes	Yes	Yes	Yes
Ext. Labor Cond. Var.	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table reports results of heterogeneity analyses. The outcome variable bitterness is standardized and the regressions are specified as in Table 3.4, column (3). Column(1): Sample consists only of male respondents. (2): Only female respondents. (3): Only native respondents. (4): Only respondents with migration background, i.e., they themselves or one of their parents immigrated to Germany. (5): Only respondents younger than 50. (6): Only respondents 50 and older. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 3.A16 Heterogeneity Analysis: By Labor Market Characteristics

	Labor Income		Ever Unemployed	
	(1) Low	(2) High	(3) Yes	(4) No
Treatment	0.167**	0.297***	0.191***	0.289***
	(0.0654)	(0.0648)	(0.0625)	(0.0698)
Treated N	142	143	171	114
Control N	2641	8281	3371	7551
R2	0.486	0.449	0.439	0.493
Basic Cond. Var.	Yes	Yes	Yes	Yes
Health Cond. Var.	Yes	Yes	Yes	Yes
Ext. Labor Cond. Var.	Yes	Yes	Yes	Yes

Note: This table reports results of heterogeneity analyses. The outcome variable bitterness is standardized and the regressions are specified as in Table 3.4, column (3). Column(1): Sample consists of respondents with monthly labor income of less than 2000 Euros (real income with base in 2016). (2): Only respondents with incomes of 2000 or more Euros. (3): Only respondents who have experienced unemployment before. (4): Only respondents who have never experienced unemployment before. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 3.A17 Heterogeneity Analysis: By Initial Bitterness and Health

	Initial Bitterness		Health	
	(1)	(2)	(3)	(4)
	Low	High	Not Good	Good
Treatment	0.246***	0.288***	0.194***	0.293***
	(0.0836)	(0.0655)	(0.0619)	(0.0695)
Treated N	117	168	160	125
Control N	6832	4090	4518	6404
R2	0.232	0.346	0.466	0.470
Basic Cond. Var.	Yes	Yes	Yes	Yes
Health Cond. Var.	Yes	Yes	Yes	Yes
Ext. Labor Cond. Var.	Yes	Yes	Yes	Yes

Note: This table reports results of heterogeneity analyses. The outcome variable bitterness is standardized and the regressions are specified as in Table 3.4, column (3). Column(1): Sample consists of respondents with low initial levels of bitterness, i.e., 3 or less on a 7-point Likert scale. (2): Only respondents with medium or high initial levels of bitterness, i.e., 4 or more on a 7-point Likert scale. (3): Only respondents who report that their health is either bad, poor or satisfactory. (4): Only respondents who report that their health is either good or very good. Robust standard errors in parentheses.

* $p < .10$, ** $p < .05$, *** $p < .01$

Chapter 4

Feeling Equal before the Law? The Impact of Naturalization and Legal Status on Perceived Discrimination

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4.1 Introduction

As many industrialized societies face the prospects of demographic change with aging populations, increased shares of retirees, and, as a consequence, strained social security systems (Börsch-Supan et al., 2014), they increasingly rely on immigrants to fill open positions on the labor market. Yet after arrival, immigrants often struggle to fully participate in the economic, social, and political spheres of their host country. Thereby, one factor possibly holding them back, which is often brought up by researchers, policymakers, and immigrants themselves (Liebig and del Carmen Huerta, 2024), are experiences and perceptions of discrimination.

Perceived discrimination, meaning the impression that one has been treated unfairly due to some personal characteristic or group membership (Kaiser and Major, 2006), has thus far received relatively scarce attention in economics. Instead, the field to a large degree has been focused on studying the extent and impact of discrimination in the labor market and in social life using laboratory or field experiments (Riach and Rich, 2002; Neumark, 2018). Yet, there is an extensive literature on perceived discrimination in other disciplines like social psychology, ethnic studies, and public health, looking at its impact on various outcomes such as health, migration decisions, and trust (Pascoe and Richman, 2009; Röder and Mühlau, 2011; Di Saint Pierre et al., 2015).

In this study, we focus on the perceived discrimination of migrants. We do so in the context of Germany, a country with one of the oldest populations in the world, which is already substantially affected by the consequences of demographic change. Even though nearly a quarter of the people currently living in Germany are foreign-born or the direct descendants of immigrants according to the German Federal Statistical Office, for a long time, the country, politically and culturally, has refused to be labeled an "immigration country" or "*Einwanderungsland*" (Hell, 2005). Even as the country has increasingly opened up to immigration to tackle labor shortages, surveys indicate that the country's attractiveness to immigrants, particularly high-skilled ones, appears average at best (Liebig and Ewald, 2023), as many of the newly arrived struggle to make German friends and feel left out (InterNations, 2023). In light of these dynamics, the German parliament has passed several reforms in recent years to raise Germany's attractiveness in the competition for global talent. Apart from making it easier for foreigners to come to Germany in the first place, a 2024 reform has also lowered residency requirements to acquire German citizenship.

As a number of researchers argue that improved access to citizenship can help to accelerate the integration of migrants (Hainmueller et al., 2017; Gathmann and Garbers, 2023), we want to analyze how changes in legal status impact perceived discrimination of immigrants in Germany – more specifically discrimination due to their ethnic background. In particular, we want to examine the effects of naturalization as possibly the most consequential change in legal status. Foreign nationals from non-EU countries face considerable legal and factual disadvantages on the labor market in Germany due to not having a German passport, as they are precluded from entering certain jobs, are costlier to employ for firms due to administrative obligations, and may face statistical discrimination (Steinhardt, 2012). Moreover, they are less able to participate politically, may only enjoy restricted mobility (particularly when trying to travel abroad), and may encounter steeper barriers when trying to bring family members to Germany. We therefore test whether the alleviation of these legal disadvantages after acquiring German citizenship leads to a decrease in feeling disadvantaged among immigrants.

To answer our research question, we use data from the German socio-economic panel, an extensive longitudinal household survey which annually interviews around

30,000 individuals. Apart from providing information on respondents' nationality, the dataset also asks first- and second-generation migrants about their experiences with discrimination due to their ethnic background.

We use this dataset in two separate approaches: First, we estimate the direct effects of naturalization on perceived discrimination largely following the methodology of Steinhardt (2012). Thereby, we regress our outcome of interest on a dummy variable indicating whether a migrant has become naturalized. In addition, we include individual fixed effects to eliminate all time-constant heterogeneity, and include state and year fixed effects as well as a wide set of individual characteristics to control for potential confounders that may vary over time.

While this first approach does control for many of the potential factors affecting our relationship, we cannot fully rule out potential endogeneity, as time-variant unobserved factors may impact changes in both our outcome of interest and main explanatory variable. We therefore employ a separate approach where we exploit changes in German citizenship laws in 1991 and 2000. More specifically, this method, which has been used by Gathmann and Keller (2018) and is our preferred approach, estimates intent-to-treat (ITT) effects making use of variation in residency requirements based on arrival year and age at arrival. We then use this variation to estimate whether a change in the years required to reside in Germany to acquire citizenship has an impact on perceived discrimination.

Overall, we find relatively similar results in the direction of effects, but not always in significance. Using our first approach, baseline estimates for the full sample reveal a negative, but insignificant effect of naturalization on perceived discrimination. In contrast, when using our second approach we find a weakly significant negative effect (p-value = 0.05). The coefficient indicates that a reduction in waiting times of seven years – essentially the drop in residency requirements for older migrants (15 years or older at arrival) brought about by the German citizenship reform in 2000 – translates to a reduction in perceived discrimination of around 13 percent of a standard deviation.

Looking at heterogeneities, using both approaches, we find that Eastern Europeans appear to benefit most from naturalization, as both the direct impact of naturalization and a reduction in waiting periods is associated with a decrease in perceived discrimination. Using the second approach, we find that a decrease in residency requirements of seven years reduces perceived discrimination among Eastern Europeans by around 30 percent of a standard deviation. In contrast, effects for Western and non-European immigrants are insignificant. While we argue that Western migrants usually already enjoy many of the benefits that naturalization brings by being citizens of EU or EEA countries – and therefore may not see a reduction in perceived discrimination – , it is less clear why results for Eastern European and non-European migrants are so different. One potential explanation could be that the nature of discrimination experienced may differ between these groups, as non-European migrants are more likely to be the target of discrimination based on personal features like skin color or religious attire than Eastern Europeans (Booth et al., 2012). Therefore, naturalization may not bring the same benefits to non-Europeans as it provides only little cover against this type of discrimination.

Looking at effects by gender, we find that a decrease in residency requirements of seven years reduces perceived discrimination among men by nearly 20 percent of a standard deviation. In contrast, perceptions of discrimination are unaffected for female migrants, which might be explained by increased labor market success after naturalization for men (Steinhardt, 2012).

We test the robustness of our results by making several adjustments to our estimation specifications. Apart from testing results with binary outcomes, we evaluate results when extending or restricting our samples, modifying control variables, and introducing additional covariates. Overall, results remain robust to these checks.

As a last extension, we evaluate the impact of an extension in rights and privileges for certain migrants by exploiting exogenous variation due to three phases of EU enlargement in 2004, 2007, and 2013. These events serve as a quasi-natural experiment, as immigrants from EU accession countries in Germany started to benefit from additional rights and opportunities granted by EU law. Moreover, as these later waves of EU enlargement near-exclusively benefited citizens from Eastern European countries, it serves as an additional check of our previous results.

We estimate a kind of staggered difference-in-differences model where we compare nationals from countries that became part of the EU with migrants from countries that have or had a plausible path to EU membership.[1] While the pool of treated individuals is rather small for the years before 2013, the estimates still broadly corroborate our previous findings. We find that becoming an EU citizen reduces perceived discrimination by up to 30 percent of a standard deviation. Moreover, effects are particularly pronounced for men, which again is in line with our previous findings showing stronger

[1] Hereby, we only include individuals without German nationality and whose nationality does not change over the observed time period.

reductions of perceived discrimination after naturalization for men. Effects remain significant and large even when we extend the control group.

Our study contributes to several strands of the literature. First, it contributes to the existent studies on the determinants of perceived discrimination. Even though it has been studied extensively in other disciplines like urban studies (Dill and Jirjahn, 2014), sociology (Diehl et al., 2021), ethnic studies (Yazdiha, 2019), and public health (Gil-González et al., 2013), the concept of perceived discrimination has thus far received only scant attention in economics. One notable exception is the recent study by Groeger et al. (2024), which examines perceived discrimination of Venezuelan immigrants in Peru.

Second, by studying perceived discrimination due to a person's ethnic background we add to the literature on ethnic and racial discrimination. The literature focusing on this type of discrimination is already very extensive (Riach and Rich, 2002; Rich, 2014; Bertrand and Duflo, 2017; Quillian et al., 2017; Neumark, 2018), studying not only its extent in various contexts and countries, but also examining whether the nature of discrimination is taste-based or statistical (Oreopoulos, 2011; Carlsson and Rooth, 2012; Zschirnt and Ruedin, 2016). However, given that most of these studies rely on laboratory or field experiments, they tend to be fairly restrictive methodologically. Studying perceived discrimination, on the other side, offers more flexibility, as it can be linked with survey and administrative data and examined both as an independent or dependent variable, – given that empirical researchers are aware of potential empirical pitfalls arising from endogeneity and selection.

Third, this study adds to the existent literature on the implications of legal status more broadly (Hall et al., 2010; Fasani, 2015; Mastrobuoni and Pinotti, 2015) and naturalization more specifically (Chiswick et al., 2009; Hainmueller et al., 2019; Govind, 2021; Gathmann and Garbers, 2023). While much research has examined the effects of naturalization on the labor market (Chiswick, 1978; Bratsberg et al., 2002; Devoretz and Pivnenko, 2005; Riphahn and Saif, 2019) and for social outcomes (Avitabile et al., 2013, 2014), by studying perceived discrimination, we add to this literature by focusing on a potentially intermediary factor, which helps explain social behaviors and the observed dynamics on the labor market.

The rest of this paper is structured as follows. In section 4.2, we describe perceived discrimination as a concept, lay out considerations how legal status and perceived discrimination might be linked, and discuss its potential implications for other outcomes. Thereafter, we present our data and provide descriptive statistics in section 4.3. This is followed up by presenting methodology, results, and robustness checks of the two main approaches in section 4.4 and 4.5. In section 4.6, we provide an extension to our main results by studying the natural experiment of EU enlargement. Section 4.7 concludes our study and discusses policy implications.

4.2 What is Perceived Discrimination?

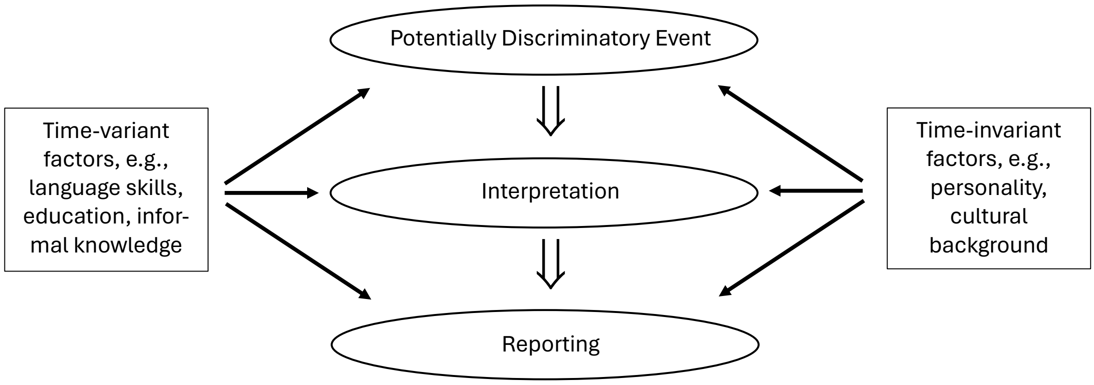
4.2.1 Concept

The concept of perceived discrimination usually refers to self-reports of having been treated unfairly due to some personal characteristic or group membership (Kaiser and Major, 2006). It captures whether individuals had any such experiences at all, but may also elicit how often people have faced such situations.[2] Perceived discrimination can be based on various personal characteristics. The most widely studied factors include gender, and – as in the case of this study – race or ethnic background (including related features like skin color, foreign names or accents). However, it can also extend to other aspects like age, religion, or sexual orientation (Almeida et al., 2009; Han and Richardson, 2015; Wu and Schimmele, 2021).

Importantly, perceived discrimination is not an objective or neutral measure of discrimination (Diehl et al., 2021), but depends on each affected individual; more specifically their experiences, how they interpret potentially discriminatory or otherwise negative situations, and how inclined they are to report them in an interview. In their study, Kaiser and Major (2006) lay out how perceived discrimination can theoretically under- or over-state actual discrimination. Under-reporting (also deemed minimization bias) may arise when affected individuals are not able to detect discrimination, e.g., because it is hidden or occurs in ambiguous circumstances, or when they deny its existence, e.g., to avoid psychological costs. In contrast, over-reporting (or vigilance bias) may result when individuals with a history of experiencing discrimination become more likely to attribute discrimination to ambiguous situations. Moreover, it may also occur if respondents blame negative events like job loss on discrimination to protect their self-worth.

[2]It can, however, also capture more general aspects, e.g., whether subjects believe themselves to be part of a discriminated group – thereby asking less about actual experienced discrimination and more about potential discrimination (Yazdiha, 2019).

Figure 4.1 Framework: Interpretation and Reporting of Discrimination



Note: This figure presents a simple graphical framework of the relation of experiences that were potentially discriminatory and the ensuing interpretation and reporting of these events in survey interviews. Own Illustration.

Figure 4.1 illustrates more broadly that discrimination and perceived discrimination are not necessarily directly linked. Rather, before being reported, potentially discriminatory events first have to be interpreted by each affected person, which determines whether individuals actually view events as discriminatory or not. How this interpretation actually plays out and which factors influence it has been the topic of many studies in social science research, particularly in the context of the so-called "integration paradox" (De Vroome et al., 2014; Steinmann, 2019; Schaeffer and Kas, 2023)[3]. It describes the phenomenon often found in cross-sectional studies, whereby better integrated migrants appear to experience more discrimination than less well integrated migrants.[4] There are several potential explanations: First, as migrants become more integrated – with higher educational attainment, better language skills and more host-country specific knowledge – they may also become more able to discern discrimination, increasing reporting (Van Doorn et al., 2013; De Vroome et al., 2014). Second, better integration may make one more likely to ascribe ill intent to negative events (Diehl et al., 2021). Third, higher-qualified migrants may be the target of more discrimination on the labor market than lower-qualified ones, as they compete for more exclusive and contested positions in firms (Dietz et al., 2015; Auer et al., 2019).

[3]It is sometimes also called "skill paradox" (Dietz et al., 2015).

[4]This stands in contrast to the more conventional thinking along the assimilation theory, which posits that experiences of discrimination decline when migrants become better integrated (Gordon, 1964).

Hence, the interpretation of events depends on various time-variant, but also time-constant factors, like personality or cultural background. As Figure 4.1 illustrates, these time-variant and time-invariant factors affect not only the interpretation, but also the occurrence of potentially discriminatory events, as, e.g., people with darker skin or stronger accents may not only face more discrimination, but may also interpret these situations differently. This makes it clear that empirical researchers are faced with various problems of endogeneity when studying perceived discrimination.

However, studying perceived discrimination also has several advantages compared to other established approaches which examine discrimination. While studies using field experiments (Bertrand and Mullainathan, 2004; Oreopoulos, 2011; Neumark, 2018) may give us a clearer idea of actual discrimination in general or in specific contexts, these approaches are oftentimes not very flexible, being usually restricted to certain setups and circumstances. Moreover, while field experiments can to some extent help us understand the determinants of discrimination, both approaches usually can tell us only little about how discrimination affects other outcome variables, e.g., how it impacts well-being or labor market behavior in the long run. In contrast, using perceived discrimination as a variable that can be easily plugged into regressions as both an outcome or a determinant makes it very flexible. Furthermore, as individual and household surveys frequently capture this variable, a lot of data is already available and can be used in combination with many other control variables in empirical analyses.

4.2.2 Legal Status and Perceived Discrimination

In this section, we want to briefly lay out how perceived discrimination may generally be dependent upon migrants' legal status, focusing in particular on the effects of naturalization.

Before doing so, it is important to mention, that the legal treatment of migrants in Germany is highly dependent on their nationality. On one side, there are migrants from EU countries who already enjoy very similar rights compared to natives due to EU law (Tridimas, 2006).[5] For these migrants, naturalization usually only gives very limited legal advantages. On the other side when looking at non-EU countries, conditions and opportunities for migrants can vary a lot depending on whether home countries have bilateral agreements with Germany or not (Steinhardt, 2012). Moreover,

[5] Similar privileges are also available for citizens of other EEA countries, i.e., Iceland, Liechtenstein, and Norway, outside the EU. Swiss citizens also benefit from certain additional privileges, but the legal setting is more complicated.

the type of residence permit also has an impact. Migrants with temporary residence permits usually face more restrictions, particularly in terms of mobility and on the labor market, but also higher uncertainty about their staying prospects. However, even immigrants with permanent residency face *de facto* legal disadvantages compared to natives.[6]

First, there are considerable constraints for migrants on the labor market.[7] While some jobs in the civil service are limited to German citizens, e.g., in the judicial system or in certain public administrative positions, many other jobs – such as doctors, lawyers, teachers, or pharmacists – are highly regulated, and require certain qualifications to enter (Gathmann and Garbers, 2023). Although migrants may already bring qualifications from their home countries, the recognition of certificates and degrees is usually very time-consuming, cumbersome, and not too rarely unsuccessful (Jacobsen, 2021; Sommer, 2021). Therefore, many immigrants are forced to pursue non-regulated jobs that usually are less well-paid or to go back to school to acquire the necessary certificates (Nikolov and Goodarzi, 2022). Moreover, employment chances of immigrants may also be reduced by other factors. First, hiring and employing foreign workers often is associated with additional administrative work and therefore more expensive for employers (Steinhardt, 2012). Second, employers may refrain from employing foreigners if they are unsure about their long-term staying prospects. This issue extends to the more general problem that migrants may become the target of statistical discrimination as employers only have incomplete information and may infer worker productivity based on wrong generalizations (Phelps, 1972; Hainmueller et al., 2019).

Second, non-Germans may also be less able to participate socially and politically. They have less access to public services, social welfare programs, it is harder for them to bring family members to Germany, and to apply for a credit at a bank – which for most people is a necessary step to purchase a home. Moreover, it is harder to partake in political activities like joining parties[8], their freedom of assembly is restricted, and, of course, they have neither active nor passive voting rights.

Lastly, there may be further disadvantages, which have been observed in the literature. E.g., there are studies showing that non-nationals get sentenced more harshly

[6]Unfortunately, we cannot look further into the consequences of having a temporary or permanent residence, as our dataset does not provide sufficient information on that.

[7]In his paper, Steinhardt (2012) describes several ways by which non-Germans may be disadvantaged on the German labor market. As these also can affect perceived discrimination, we reiterate some of these arguments in the following.

[8]While most parties allow non-citizens to join, this is not the case for some, e.g., the CDU, which just allows EU citizens to become a member.

(Light, 2016), and they may also experience disadvantages in school or university (Glock and Krolak-Schwerdt, 2013).

All of these factors can contribute to a sense of disadvantage that migrants may experience in the host country. Even though the mentioned examples of factual discrimination may not be based on characteristics like ethnicity or race, individuals may still interpret them to be due to their origins. There is the possibility that they might believe their experiences would have been different if they were born in a different country. Yet, to what extent one may feel discriminated because of these rules likely depends on each individual and their experiences and expectations.

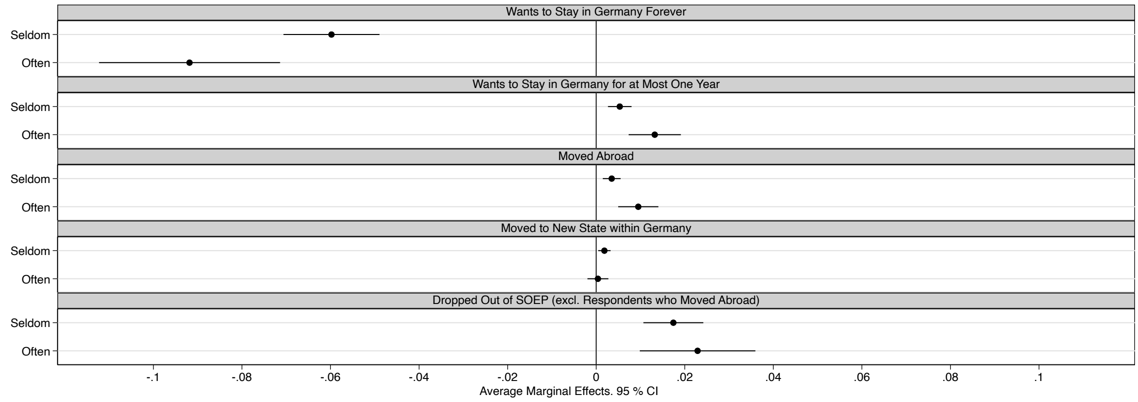
These disadvantages could be alleviated by acquiring German citizenship. While this process is fairly lengthy and takes effort, naturalization removes all legal barriers to the labor market, and grants migrants those rights and privileges mentioned above. Moreover, acquiring citizenship may also be perceived as a strong signal of ability and commitment to stay in Germany long-term by employers, thereby reducing potential statistical discrimination (Steinhardt, 2012). Thus, we would generally expect that migrants who naturalize report less discrimination than those who do not.

4.2.3 Implications

Before we start looking more closely at the impact of legal status on perceived discrimination, it may be worthwhile to first take a step back and examine the potential impact perceived discrimination may have on other outcomes.

There have been a number of studies from various fields looking at the implications of perceived discrimination. First, there is a broad literature on its health effects, finding a detrimental impact of perceived discrimination on both physical and mental health (Pascoe and Richman, 2009; Schmitt et al., 2014; Szaflarski and Bauldry, 2019). Moreover, some studies have shown that experiences of discrimination are a strong driver of return intentions and actual outmigration (Di Saint Pierre et al., 2015; Kunuroglu et al., 2018; Yilmaz Sener, 2019). In addition, further research has looked at the impact of perceived discrimination on other outcomes like national identification and ethnic identity (Martinovic and Verkuyten, 2012; De Vroome et al., 2014), political engagement (Fischer-Neumann, 2014), and trust in public institutions (Röder and Mühlau, 2011). These studies usually find negative effects on host country identification, while the impacts on institutional trust and political interest are more

Figure 4.2 Implications of Perceived Discrimination on Staying Intentions, Observed Migration, and Attrition



Note: This graph shows coefficients of OLS regressions (with 95% confidence intervals), with various outcomes regressed on the expressions of perceived discrimination. Outcome variables are binary. Regressions include all controls in Table 4.A2.

nuanced, and depend on factors like ethnicity, ethnic identity, and whether migrants are born abroad or in Germany.

Accompanying the main estimations of this study, we add to the existing research by running a quantitative analysis on the implications of perceived discrimination. This is described in greater detail in section 4.B. Thereby, we first run simple OLS regressions of various outcomes on perceived discrimination, while employing year and state of residence fixed effects, and controlling for a host of time-varying individual characteristics (listed in Table 4.A2 (Appendix)). Focusing on staying intentions and observed migration as outcomes, Figure 4.2 displays that perceived discrimination is negatively related to wanting to stay in Germany long-term. Looking at the other outcomes, it also reveals that respondents with higher perceived discrimination are more likely to leave Germany and to drop out of the dataset (even when not moving abroad). Moreover, effects are usually larger for respondents who report more discrimination. Hence, perceived discrimination appears to be a factor driving migrants out of Germany.

To examine the robustness of these relations, we extend our model to include individual fixed effects, which eliminate all time-constant heterogeneity – thereby largely accounting for people's personality and inclination to report perceived discrimination. This drastically reduces but not fully eliminates the potential bias in our estimations, as endogeneity due to omitted variable bias and, in some cases, reverse causality cannot be fully ruled out.

Overall, coefficients – while decreasing in size – remain significant and large, which is displayed in columns (6)-(10) in Table 4.B1. Moreover, the table also reveals that perceived discrimination appears to have a detrimental effect on other outcomes including individual well-being, and mental health. Furthermore, it also seems to change political preferences, with increases in political interest and a higher likelihood to prefer left-wing parties in Germany.

While we do not claim that results are causal, our findings nevertheless appear remarkably robust to the inclusion of further control variables (Table 4.B2). Thus, it appears that perceived discrimination has various negative effects on migrants' well-being and mental health, and, moreover, may also be detrimental to the German economy, as migrants who report discrimination also seem more likely to leave Germany.

4.3 Data & Descriptives

For our analyses, we employ data from the German socio-economic panel (SOEP, Goebel et al. 2019). This longitudinal household survey interviews around 30,000 respondents from about 15,000 households annually, capturing a wide range of social, economic, and demographic characteristics as well as attitudes and opinions. Importantly for our study, it also asks respondents with a migrant background (1st or 2nd generation): "How often in the last two years have you felt discriminated against here in Germany because of your ethnic origins?"[9] Response options are "never", "seldom", and "frequently". With this exact phrasing, the question was surveyed annually from 1996 to 2011 and every two years between 2011 and 2017.[10] Descriptive statistics of this variable are provided in Table 4.A1 (Appendix).

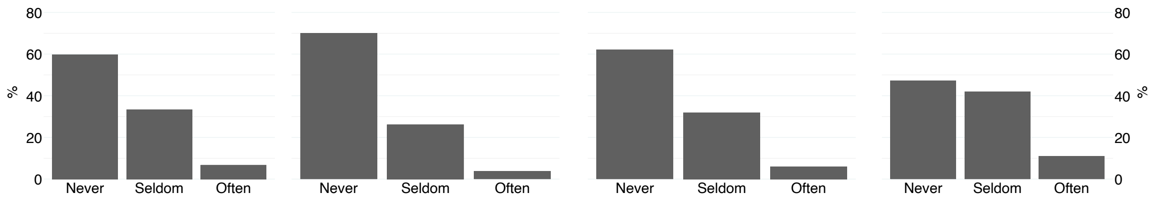
Furthermore, Figure 4.3 shows the distribution of responses averaged over time for the full sample (a) and for subsets based on region of origin (b-d). Hereby, Western migrants include respondents from Western Europe (e.g., France, Greece, Italy) or non-European "Western" countries (e.g., United States, Australia). Eastern Europeans include respondents from the former Warsaw Pact countries (e.g., Poland), post-Soviet nations (e.g., Russia, Kazakhstan), and from the Western Balkans (e.g., Serbia). Non-

[9] Hereby, we refer to the phrasing in the English questionnaires. In German, the term *Herkunft* is used, which is not necessarily congruent with ethnic origin, but can also describe more generally where someone comes from, referring to a location.

[10] In the following waves, the question on perceived discrimination was rephrased and response options were modified ("frequently", "sometimes", "rarely", "never"). For consistency we therefore include only data until 2017.

Figure 4.3 Distribution of Perceived Discrimination By Origin Groups

(a) Full Sample (b) Western (c) Eastern European (d) Non-European



Note: The figure presents the distribution of response options regarding perceived discrimination averaged over the observed time period from 1996 to 2017 for the full sample (a) and sub-samples by region of origin (b-d).

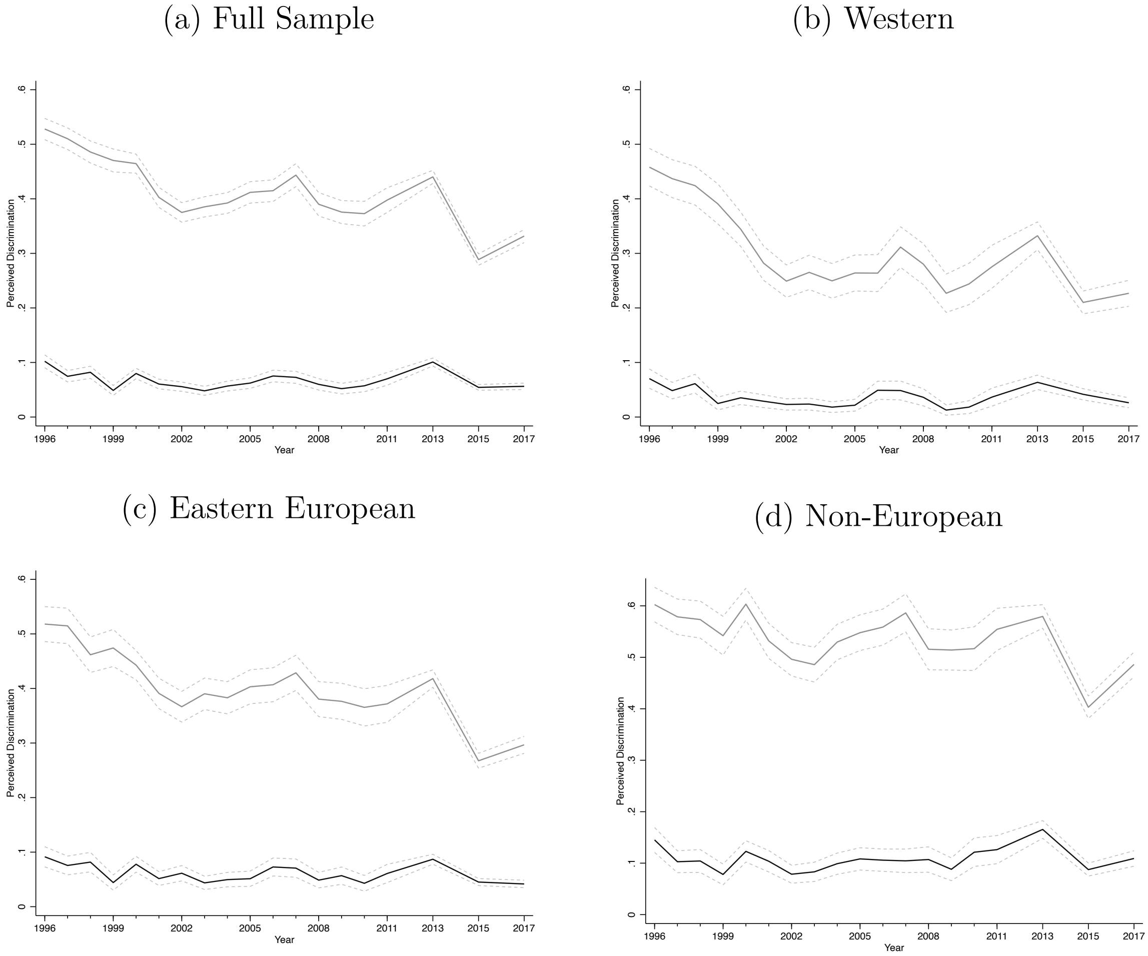
Europeans refer to respondents from non-European, non-Western states (e.g., Turkey) including those from the MENA region, Latin America, East Asia, and Africa.[11] Table 4.A3 (Appendix) reveals which region contains which countries. In Figure 4.3 we see that overall around 60 percent of respondents reported that they have never experienced discrimination in the previous two years, while less than 10 percent said to have felt disadvantaged often. Thus, while it is unclear what constitutes notable discrimination in the eyes of respondents, we can see that a clear majority of migrants in the sample reported to have faced no discrimination. However, there are strong differences between different groups. While Western migrants were less likely to report discrimination than migrants as a whole, more than 50 percent of non-Europeans reported at least some discrimination with Eastern Europeans in-between.

Looking at perceived discrimination over time in Figure 4.4, it shows that reported discrimination has declined over the years for the sample as a whole, even though frequent discrimination has remained basically the same. In 1996, overall a majority of respondents said that they have faced at least some discrimination, while in 2017, the share was around 35 percent. Looking at the subgroups, we see that there were considerable decreases in perceived discrimination over time among Western migrants and Eastern Europeans. In contrast, dynamics among non-Europeans were basically flat until 2013, with a sudden decrease in 2015 and an uptick in 2017.

The SOEP also provides information on respondents' origin. Importantly for our research question, it captures their (first and second) nationality, which we make use of to see whether respondents became naturalized. Figure 4.5 reveals the share of migrants who attained German citizenship. While in the full sample, around 30 percent

[11]Figure 4.A1 (Appendix) shows the distribution of perceived discrimination for smaller subsets of origin region and by gender.

Figure 4.4 Time Trend of Perceived Discrimination By Origin Groups



Note: Figure presents time trends of perceived discrimination including 95 percent confidence intervals for the full sample (a) and sub-samples by region of origin (b-d). Grey line: Has experienced discrimination at least sometimes. Black line: Has experienced discrimination often.

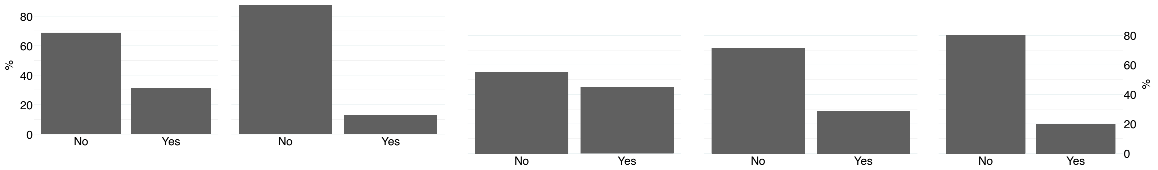
of migrants are German, there are considerable heterogeneities by origin group. While Western migrants only rarely hold German citizenship (around 15 percent), the share is much larger for Eastern Europeans (around 45 percent), and even when we remove resettlers – a group of ethnic Germans who arrived in Germany particularly after the Fall of the Berlin Wall from Eastern European countries like Russia, Poland, and Romania – the share is at 30 percent. Non-Europeans are at 20 percent.[12]

Lastly, the dataset also offers very broad and extensive information on individual and household characteristics, which we use as control variables. Apart from information

[12]Figure 4.A3 (Appendix) shows the shares of naturalized migrants in our sample for smaller subsets of origin region and by gender.

Figure 4.5 Share of Migrants with German Citizenship By Origin Groups

(a) Full Sample (b) Western (c) East. European (d) EE, No Resettlers (e) Non-European



Note: The figure displays the share of migrants with German citizenship averaged over the observed time period from 1996 to 2017 for the full sample (a) and sub-samples by region of origin (b-e).

on the exact dates of interviews and respondents' state of residence, we employ information on demographic (e.g., age, gender, region of origin[13]), social (e.g., marriage status, number of children), educational (e.g., type of school degree, post-secondary education), economic (e.g., labor income, employment and labor market status), and health characteristics from the dataset. Descriptive statistics of these variables are provided in Table 4.A2 (Appendix) for the full sample and by whether respondents experienced at least some discrimination or not.[14] Moreover, we include t-tests to check whether differences between these groups are significant. Generally, respondents who report having experienced discrimination appear different compared to those who do not. E.g., they are younger, more likely to be male and to be foreign-born, and more likely to come from East Asia, Africa, Turkey and the MENA region. In addition, they are less likely to have a German school degree, an upper secondary degree, have worse language proficiency and lower real labor incomes and are unemployed more often.

[13]For that, we construct several dummy variables for the following regions: Latin American, East Asia, Africa, Turkey and the MENA region, Western countries, Eastern Europe (excl. countries in the Western Balkans), and countries in the Western Balkans. For the categorization, we use various information based on the following characteristics: country of origin, first and second nationality, past nationality, and the country of origin and nationality of the respondents' parents. Hereby, we allow respondents to have multiple regions of origin: E.g., a respondent with a French father and a Polish mother would be classified as both Western and Eastern European. We additionally create dummies indicating whether a respondent is ethnic German, whether they are a recognized refugee, and whether they come from a country that is part of the EU at the time of the interview.

[14]In order to not lose too many observations due to missing values of control variables, we recoded missing values as zero and included additional dummy variables into our regressions, which indicate whether values were missing.

4.4 Approach 1: Fixed Effects Model

To test to what extent naturalization affects perceived discrimination in Germany, we first employ a fixed effects model, following the approach by Steinhardt (2012). In his paper, the author uses data of the employment sample of the IAB to examine how naturalization affects labor market outcomes. Thereby, he employs fixed effects methods to eliminate all time-invariant unobserved individual variation and finds that acquiring German citizenship leads to an increase in wages for men but not for women. Moreover, results show that this increase is not instantaneous, but rather builds up over time in the years after naturalization.

For our purposes, we can (with slight deviations) follow this approach, but use perceived discrimination as our dependent variable instead of wages. While this approach enables us to measure the direct effect of acquiring German citizenship on perceived discrimination, we cannot claim that estimates are causal. This is because selection into citizenship could be endogenous to changes in perceived discrimination. E.g., respondents who start to experience and/or report discrimination could be less likely to naturalize as they may become less attached to Germany. On the other side, they could also become more likely to naturalize, if they, e.g., may hope that this could decrease the likelihood of future discrimination. Nevertheless, as the extent and direction of the potential bias is not clear, this approach may still offer a good starting point for our analysis.

4.4.1 Sample Selection and Methodology

To start, we construct the sample as following: First, we restrict our sample to respondents with a migrant background who in their first interview in the SOEP have a non-German citizenship. This implicitly excludes all those migrants who acquired German citizenship at birth or during adolescence. We do so to be able to observe actual changes in citizenship, which is not possible for respondents who already naturalized before their first interview. Second, we also remove ethnic Germans (resettlers). As they have a claim to German citizenship through ancestry, they could apply for German citizenship much faster and much more easily than other migrants; thus, their experience is fairly unrepresentative for the average migrant. Lastly, we also exclude respondents in retirement age as we would expect them to benefit less from naturalization, given that they are usually no longer active on the labor market.

Using our sample, Figure 4.A4 (Appendix) displays the share of respondents who become naturalized over time. It starts out at zero (as we only start observing individuals in 1996), and then mostly increases in the years thereafter, with a sharp increase in 2002. The fall in 2013 is due to the inclusion of the newly added migrant samples in the SOEP (Brücker et al., 2014), which were introduced in 2013, and therefore not observed before.

We employ this dataset and estimate a model, where we regress our measure of perceived discrimination, which is standardized, on a dummy variable indicating whether a respondent is naturalized. We start out with a simple pooled OLS regression where we only control for time and state of residence fixed effects. We then gradually introduce more complexity in the relation, first including more control variables, and then adding individual fixed effects. The regression of our most extensive model then looks as follows:

$$PD_{it} = \alpha_0 + \alpha_1 N_{it} + \beta X_{it} + S_{it} + \tau_t + \rho_i + \epsilon_{it}. \tag{4.1}$$

Hereby, the dependent variable PD_{it} represents our standardized measure of perceived discrimination of respondent i interviewed in t, which is regressed on the dummy variable N_{it}, which turns one once a respondent acquires German citizenship and zero if a person is not (yet) naturalized. We then also include individual fixed effects (ρ_i), which eliminate all time-invariant unobservable heterogeneity, including the inclination to interpret situations as discriminatory. Moreover, we include state of residence fixed effects (S_{it}), which control for state-specific institutional, cultural, and demographic characteristics, which may have an effect on both perceived discrimination and the likelihood and ease to naturalize. Year fixed effects (τ_t) are included to control for changes over time that may affect all migrants at the same time such as institutional changes on the federal level or other political, cultural or social events (e.g., terror attacks or large demonstrations). Lastly, to limit the potential for omitted variable bias, we control for a wide range of individual characteristics (X_{it}), which are shown in Table 4.A2. Thereby, we distinguish between variables which are largely exogenous to the research question, namely demographic characteristics and education[15], and – to go even further – those that could be endogenous to them, namely language, health, social, and labor market characteristics, as they could be influenced by both changes

[15]While educational upgrading could potentially be impacted by discrimination, in the dataset most people's educational decisions were already made before they were sampled.

Table 4.1 Direct Effects of Naturalization: Main

	OLS Regressions			FE Regressions		
	(1)	(2)	(3)	(4)	(5)	(6)
Naturalized	0.1338***	0.0306	0.0452	-0.0387	-0.0423	-0.0447
	(0.0390)	(0.0379)	(0.0374)	(0.0338)	(0.0343)	(0.0343)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Indiv. FE				Yes	Yes	Yes
Exog. Contr.		Yes	Yes		Yes	Yes
End. Contr.			Yes			Yes
Mean	0.514	0.514	0.514	0.514	0.514	0.514
N	35296	35296	35296	35296	35296	35296

Note: The table reports results after estimating equation 4.1, in which the standardized outcome variable perceived discrimination is regressed on a dummy indicating whether the respondent is naturalized. All regressions include state of residence and year fixed effects. Results in columns (1-3) report OLS estimates without, with exogenous and with all control variables, respectively. Results in columns (4-6) are equivalent but for fixed effects regressions. Standard errors (in parentheses) are clustered at the person level. * $p < .10$, ** $p < .05$, *** $p < .01$

in perceived discrimination and in citizenship. Standard errors are clustered at the individual level.

4.4.2 Main Results

Our main results are shown in Table 4.1. Column (1) shows the estimate for the OLS regression, where we only control for state of residence and year. Column (2) then adds the plausibly exogenous control variables, and column (3) introduces all control variables in Table 4.A2. In the following three columns, we then include individual fixed effects, first without (4), then with exogenous covariates (5), and then with all controls (6). Interestingly, estimates in column (1) indicate that naturalized respondents report discrimination more often than non-Germans. This relationship vanishes, however, once we control for individual characteristics in (2) and (3). Moreover, when we include individual fixed effects – thereby looking at changes in perceived discrimination of the same individual – the sign of the coefficient reverses, but the overall effect remains insignificant. This also remains the case after controlling for further characteristics in (5) and (6). Thus overall, it appears that naturalization does not lead to a significant change – also meaning no reduction – in perceived discrimination. We should note, however, that we cannot rule out that this estimate is distorted as the relation may still suffer from omitted variable bias.

Table 4.2 Direct Effects of Naturalization: Heterogeneity

	(1) Men	(2) Women	(3) West	(4) Eastern Europe (incl. Balkan)	(5) Non-Western Non-Europe
Naturalized	-0.0780	-0.0055	0.1128	-0.1462***	-0.0405
	(0.0527)	(0.0446)	(0.0827)	(0.0560)	(0.0476)
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Indiv. FE	Yes	Yes	Yes	Yes	Yes
Exog. Contr.	Yes	Yes	Yes	Yes	Yes
Mean	0.541	0.488	0.375	0.494	0.654
N	16991	18305	11616	11092	13526

Note: The table shows results after estimating equation 4.1 but splitting the sample by gender or region of origin. Regressions otherwise specified as in column (5) of Table 4.1. The outcome variable is standardized. Standard errors (in parentheses) are clustered at the person level. * $p < .10$, ** $p < .05$, *** $p < .01$

4.4.3 Heterogeneity, Robustness Checks, and Extension

We follow up our main analysis by splitting our sample by gender and region of origin in Table 4.2, and find some noteworthy heterogeneities. Hereby, we use the same specification as in Table 4.1, column (5), for the five subsample estimations.

First, in column (1), the coefficient for men is negative at -0.078, but misses conventional levels of statistical significance[16], while the one for women is near zero (column 2).

However, estimates show striking differences when we distinguish by region of origin (columns 3 to 5). While effects are insignificant for Western and non-European migrants, respondents with an Eastern European background experience a large and highly significant decline in perceived discrimination of 15 percent of a standard deviation.

These results seem fairly surprising. On one side, we see a decline in perceived discrimination among Eastern Europeans. Possible reasons for that are rather straightforward: As laid out in section 4.2.2, naturalization removes most legal and factual forms of discrimination that foreigners may encounter in Germany, guaranteeing unrestricted access to the labor market, mobility within EU countries, and granting further rights and privileges, e.g., making it possible to fully participate in the democratic

[16]Including further potentially endogenous controls in the regression decreases the coefficient a little, making it weakly significant.

processes in Germany. Moreover, it may also make statistical discrimination less likely, as acquiring citizenship may be interpreted as a strong signal of ability and commitment to stay in Germany by employers. We would expect that all these are benefits that Eastern Europeans would enjoy after naturalizing, which may reduce feelings of exclusion and discrimination.[17] However, why would the other groups not experience the same effects?

First, Western migrants have only limited benefits from the additional legal privileges of naturalization, as this group is mostly composed of EU and EEA migrants who already enjoy most of them. In particular, there are very few additional labor market benefits EU migrants have from naturalization and EU law already guarantees unrestricted mobility (Tridimas, 2006). Therefore, it is not surprising that there are little changes for Western migrants.

In contrast, however, it is not clear why effects are insignificant for non-European migrants, as we would expect them to benefit from the rights and privileges of German citizenship mentioned above. Moreover, while the group of non-Europeans is heterogeneous, including migrants from very different regions like Latin America, Sub-Saharan Africa, and East Asia, results are likely not due to missing statistical power, as the sample size is even larger than for Eastern Europeans and the point coefficient is much closer to zero.

Rather, one potential explanation might be that the legal disadvantages of having a foreign citizenship in Germany are not as salient for non-European migrants. They may simply accept them as the "rules of the game" and would not consider them to be discriminatory. In contrast, discrimination based on one's ethnic origin – e.g., because of a different skin color, appearance, religion, accent or else – may be much more salient and impactful for one's experience in Germany (Vernby and Dancygier, 2019). Non-Europeans are more likely to experience these forms of discrimination than Europeans (Booth et al., 2012) – in part because cultural and genetic distances are larger (Spolaore and Wacziarg, 2016) – and naturalization might do little to dampen these forms of discrimination (Vernby and Dancygier, 2019).

To test the robustness of our results, we perform several checks. First, in Table 4.A4 (Appendix) we dichotomize our outcome variable. Therein, the dependent variable is one if respondents experienced at least some discrimination and zero else. The estimates

[17] Additional regressions (not shown for brevity) reveal that estimates are insignificant when only looking at Eastern European migrants, whose home countries were part of the EU at the point of the interview. Thus, Eastern Europeans only seem to benefit from naturalization if they do not already have additional rights due to EU law.

are in line with results of the baseline estimation. They indicate that naturalization leads to a reduction in experienced discrimination for Eastern Europeans but not for other groups, and effects for the full sample are insignificant.

Second, we make several modifications of our specification in Table 4.A5 (Appendix): using linear and quadratic time trends instead of year fixed effects (Panel A), including seniors in the regression (Panel B), excluding respondents with less than eight interviews (Panel C), and excluding respondents still in school, university, or training, as they do not yet benefit from the labor market advantages of naturalization (Panel D). Overall, results appear fairly stable. While coefficients are somewhat smaller when including seniors, only including respondents with a lot of interviews and excluding respondents in education even pushes the coefficient for men to weak significance.

Lastly, it may be interesting to examine to what extent perceived discrimination declines instantly after naturalization or whether effects take time to evolve. If our assumption is correct, that the reduction in perceived discrimination is mostly because of the elimination of factual legal discrimination and the granting of rights and privileges, we would expect to see an immediate change in perceived discrimination. We test this by performing an additional extension of our model, which was also done in the original paper by Steinhardt (2012) and follows Bratsberg et al. (2002). Hereby, we extend the base model to include two additional terms, which enable us to test the effects of naturalization over time. The model then takes the following form[18]:

$$\begin{aligned} PD_{it} &= \alpha_0 + \alpha_1 N_{it} + \alpha_2 N_{it} * (Age_{it} - Age_{iN}) + \alpha_3 CA_i * Age_{it} \\ &\quad + \beta Z_{it} + S_{it} + \tau_t + \rho_i + \epsilon_{it}. \end{aligned} \tag{4.2}$$

This regression equation includes two new terms: First, $N_{it} * (Age_{it} - Age_{iN})$ is an interaction of the dummy variable N_{it} – indicating whether person i is naturalized in year t – with the difference between the age at the interview and the age at naturalization. This term is either zero if a person is not (yet) naturalized or naturalized in the year of the interview or positive if the respondent naturalized in the past (e.g., it takes the value 1 one year after naturalization, the value 2 two years after, etc.).

This variable helps us to observe whether the effects of naturalization on perceived discrimination build up over time. Second, the term $CA_i * Age_{it}$ describes the interaction of age at interview and a time-constant dummy variable, which is always 1 for respondents who at some point are granted German citizenship and 0 for those who

[18]Standard errors are again clustered at the individual level.

Table 4.3 Direct Effects of Naturalization: Extension

	(1) All	(2) Men	(3) Women	(4) West	(5) Eastern Europe (incl. Balkan)	(6) Non-Western Non-Europe
Panel A: Without Controls						
Naturalized	-0.0245	-0.0466	-0.0046	-0.0121	-0.1613**	0.0612
	(0.0420)	(0.0633)	(0.0565)	(0.1080)	(0.0671)	(0.0603)
Post Naturalization	0.0013	-0.0066	0.0108	0.0204	0.0144	-0.0006
	(0.0089)	(0.0115)	(0.0135)	(0.0188)	(0.0157)	(0.0144)
Prior Naturalization	-0.0028	-0.0016	-0.0041	0.0064	-0.0010	-0.0162
	(0.0065)	(0.0084)	(0.0096)	(0.0111)	(0.0107)	(0.0121)
Panel B: With Exog. Controls						
Naturalized	-0.0271	-0.0461	-0.0074	0.0157	-0.1702**	0.0556
	(0.0422)	(0.0633)	(0.0569)	(0.1126)	(0.0672)	(0.0605)
Post Naturalization	0.0012	-0.0071	0.0113	0.0195	0.0143	-0.0010
	(0.0088)	(0.0115)	(0.0134)	(0.0191)	(0.0152)	(0.0144)
Prior Naturalization	-0.0028	-0.0017	-0.0043	0.0062	-0.0013	-0.0154
	(0.0065)	(0.0085)	(0.0096)	(0.0112)	(0.0106)	(0.0122)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Indiv. FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean	0.513	0.541	0.487	0.375	0.494	0.653
N	35249	16969	18280	11616	11087	13484

Note: The table reports results after estimating equation 4.2. Column (1) shows results for the full sample, while the following columns show results after splitting the sample by gender or region of origin. Panel A displays results without additional controls, while Panel B shows estimates with exogenous controls (which are listed in Table 4.A2). Standard errors (in parentheses) are clustered at the person level. * $p < .10$, ** $p < .05$, *** $p < .01$

did not become German citizens in the observed time period of the dataset. This allows us to discern whether perceived discrimination evolves differently for naturalized respondents even before naturalization, essentially serving as a pre-trend analysis.

The values of the different coefficients α_1, α_2, and α_3 allow us to examine how naturalization affects perceived discrimination over time: α_1 describes the immediate effect of naturalization, α_2 measures how perceived discrimination evolves after naturalization, and α_3 indicates whether there are differences beforehand.

Results of this extension are shown in Table 4.3. Panel A shows results without additional control variables, while Panel B presents estimates when exogenous controls are included. Column (1) uses the whole sample of respondents – excluding those for whom we have no information on their age or year of naturalization[19] – while

[19]For some respondents there are larger gaps between the years in the dataset, as they discontinued doing interviews for one or more years, but then joined again later on. In these years when interviews

columns (2) to (6) show subsample regressions based on gender and region of origin. Regarding the whole sample in (1), we see that perceived discrimination is neither immediately affected by naturalization nor is there any significant change over time. Furthermore, there are also no differing patterns before naturalization, ruling out any potential pre-trends. These results also hold when we include additional control variables. Regarding the subsamples, there are also no significant effects for any of them apart from Eastern Europeans, for whom we can see that naturalization reduces their perceived discrimination instantly at the point of acquiring German citizenship. These estimates are in line with our previously laid-out thinking that reductions in perceived discrimination are mostly due to the elimination of factual legal discrimination of foreigners.

4.5 Approach 2: Exploiting Variation from Citizenship Reforms

In the previous section, we have found that naturalization does not have a significant effect on perceived discrimination when looking at all migrants in Germany taken together. However, we did find a significant negative effect on migrants from Eastern Europe. While using this approach has the advantage of measuring the direct effect of becoming a German citizen, we cannot fully rule out potential endogeneity, as changes in unobserved factors could be correlated with naturalization and changes in our outcome.

In this section, we therefore pursue an alternative approach based on the methods used in the study by Gathmann and Keller (2018), which exploits variation in waiting times for naturalization from two citizenship reforms in Germany. In their paper, the authors use German Microcensus data to evaluate whether naturalization impacts labor market outcomes of female and male workers in Germany – looking at employment and individual labor income. Hereby, they exploit the exogenous variation from the citizenship reform to arrive at intent-to-treat (ITT) effects that show that longer waiting periods lead to a lower likelihood of naturalization, and, moreover, also lower

were skipped, respondents were not observed, so we needed to adjust for that. If respondents skipped only one year and became German citizens some time during the unobserved period, we assign the first year they were surveyed again as the year of naturalization. If they skipped more than one year and became German in this unobserved period, they were excluded from the estimations.

employment and labor incomes for women, but not men – somewhat opposed to the findings of Steinhardt (2012).

4.5.1 Citizenship Reforms

This approach makes use of two different citizenship reforms, the first in 1991, and the second in 2000. Before 1991, citizenship in Germany was generally based on ancestry (*jus sanguinis*). This means that migrants without German ancestors had no entitlement to become German even if they had been living in Germany for many years, were without criminal conviction, and economically self-sufficient. Instead, citizenship could be granted through discretionary decisions by public authorities. However, applications could also be denied. This legal setting had the consequence that the total annual numbers of naturalization were generally very low in Germany, not exceeding 20,000 per year (excluding ethnic Germans) before 1990 (see Gathmann and Keller, 2018).

This was changed with the passage of the Alien Act (*Ausländergesetz* (AuslG)), which was enacted on 1 January 1991. The reform established clear and explicit criteria to acquire German citizenship for non-ethnic Germans, removing discretionary leeway. Among other criteria[20], the law established minimum waiting periods for migrants based on their age at arrival. Migrants who arrived in Germany, when they were seven years or younger, had to wait until they were 16 years old to acquire German citizenship. Those, who were between 8 and 14 years at arrival had to reside in Germany for eight years, while the residency requirement for older migrants (15 years or older) was 15 years.

These criteria were amended somewhat through the passage of the Citizenship Act (*Staatsangehörigkeitsgesetz* (StAG)), which was enacted on 1 January 2000. Apart

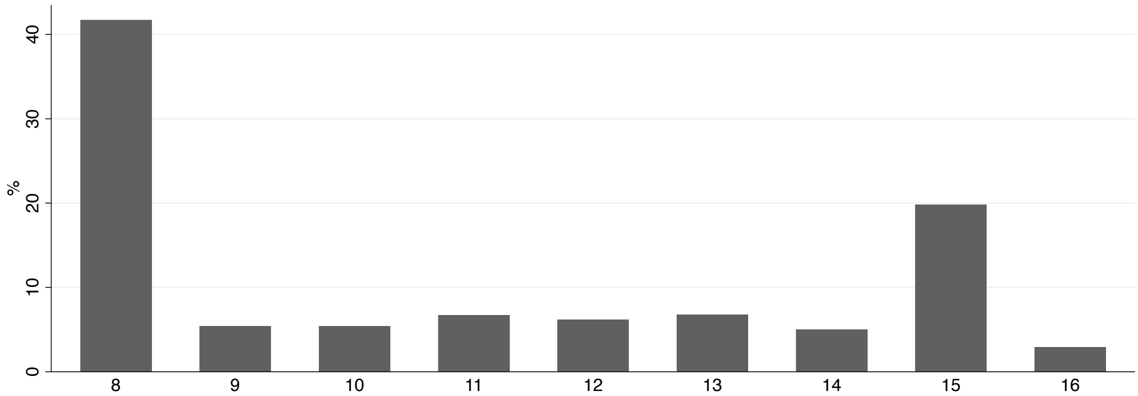
[20]There are several other criteria defined in the law: Migrants had to give up their previous citizenship upon naturalization. There were some exceptions to this: E.g., citizens from other EU countries or countries where renunciation of citizenship was not possible were allowed to keep their old citizenship. Moreover, they had to have no prior criminal convictions, could demonstrate their economic self-sufficiency (for older immigrants, i.e., those who arrived at age 15 or older) – meaning that they were able to provide for themselves and dependent family members without having to rely on welfare benefits or unemployment assistance –, had completed a minimum number of years of schooling in Germany (for younger immigrants), and declared their loyalty to the German constitution.

Table 4.4 Residency Requirements among Different Migrant Groups

Group	Age of arrival in Germany	Residency requirement for citizenship	Access to citizenship at age	% in the sample
Child immigrant	Ages 0–7	9–16 years (possibly longer for arrival cohorts 1975–82)	Age 16 (older for arrival cohorts 1975–82)	22.64
Younger immigrant	Ages 8–14	8 years (9–15 years for arrival cohorts 1975–82)	Ages 16–22 (older for arrival cohorts 1975–82)	24.21
Older immigrant	Ages 15–22	15 years (9–14 years for arrival cohorts 1986–91) 8 years (arrival cohorts 1992–2000)	Ages 30–38 (younger for arrival cohorts 1986–91) Ages 23–30 (arrival cohorts 1992–2000)	53.15

Note: Table from Gathmann & Keller (2018) which describes variation in waiting times by arrival cohort and age at arrival. Share in sample based on own calculations using SOEP data.

Figure 4.6 Distribution of Waiting Times



Note: Figure displays the distribution of how many years foreign citizens had to reside in Germany to be eligible to naturalize in our sample.

from adding language requirements, it reduced the residency requirements for migrants who were 15 or older at arrival to eight years, while keeping all other criteria in place.[21]

These two reforms led to variations in waiting times along two dimensions. First, the laws set up different waiting times by age at arrival. Migrants, who arrived in Germany when they were between zero and seven years old had to wait until they were 16 years old – or in other words: between 9 and 16 years. Migrants who were between eight and 14 years old at arrival had to wait 8 years, while older immigrants had to

[21]Moreover, the reform made it possible for children born in Germany to foreign parents to attain German citizenship if at least one parent had been living legally in Germany for at least eight years and had a permanent residence permit for at least three years.

wait 15 years (8 years since 2000). Second, migrants had different waiting times based on the timing of the reforms in combination with their arrival years. For instance, migrants who arrived in 1975 had to wait 16 years to naturalize regardless of age, as the reform was passed and enacted 16 years later, while waiting times were shorter for younger migrants of later cohorts. Moreover, there is additional variation because of the 2000 reform. Older immigrants (i.e., those who arrived at age 15 or older) who arrived in the years between 1986 and 1991 had to wait 9 to 14 years depending on the exact arrival date. This variation is summarized in Table 4.4, which is taken from the original paper by Gathmann and Keller (2018).[22] It also shows that just over one half of our sample (which is described in more detail further down below) consists of older immigrants who arrived when they were 15 or older, while the rest were younger at arrival.

Figure 4.6 additionally presents the distribution of waiting times to be eligible to naturalize. While around 40 percent of the sample had to wait for only eight years, 20 percent had to wait 15 years. The rest of the sample is spread relatively evenly among the other time periods.

4.5.2 Sample Selection and Empirical Methodology

In their paper, Gathmann and Keller (2018) perform certain sample restrictions in their paper, which we follow. First, we only study migrants who were born outside of Germany – thereby excluding second-generation migrants. Second, we remove ethnic Germans from the sample, as it was much easier and took less time for them to acquire German citizenship. Third, we only look at migrants who arrived in Germany between the years 1975 and 2002 – meaning those cohorts most affected by the reform – and who became eligible for citizenship between 1991 and 2010. The latter part implies that migrants who became German before 1991 were also excluded, as they were not affected by the reforms. Fourth, we also exclude migrants who were older than 22 at arrival.

For our estimations, we use the survey waves from 2002 until 2017. While, as outlined above, perceived discrimination was already captured in the years before, we want to make sure that respondents were already affected by both reforms at the time

[22]The last column has different values than in the original table as the data source and, hence, the sample we use is different.

of the interview. Moreover, as the question asks about experiences of discrimination in the prior two years, we add two years to our cutoff.[23]

Overall, this leaves us with a sample of 1,322 migrants and a total of 8,181 observations for this period, with 1,065 migrants who answered the question about perceived discrimination at least once and 6,134 observations in total.

This dataset is then used to estimate the following model:

$$\begin{aligned} PD_{iabt} = \beta Wait_{ab} + \lambda D(B_b) + \mu D(Coh_a) + \nu_t + \gamma_1 YSM_{at} \\ + \gamma_2 YSM_{at}^2 + \pi_1 Age_{bt} + \pi_2 Age_{bt}^2 + \delta' X_{it} + \epsilon_{iabt} \end{aligned} \tag{4.3}$$

Hereby, the outcome is (standardized) perceived discrimination (PD_{iabt}) of migrant i, who was born in year b, arrived in Germany in year a, and was interviewed in year t. The main explanatory variable is $Wait_{ab}$, the years a person has to wait until becoming eligible to acquire German citizenship, which depends on the year of arrival and the year of birth. We also follow Gathmann and Keller (2018) in our choice of controls. First, we include year of birth fixed effects $D(B_b)$ and cohort of arrival fixed effects $D(Coh_a)$ to control for potential differences in the likelihood and inclination to report discrimination among different birth and arrival cohorts. We also include year fixed effects ν_t to control for macro changes affecting all migrants, which may change respondents' likelihood to report discrimination. Moreover, we include years since arrival (YSM_{at}) and age (Age_{bt}) and their quadratic terms in the regression to control for the effects of assimilation and aging. Lastly, the model also includes several further control variables (X_{it}), namely gender, region of origin dummies[24], state fixed effects and state-specific time trends. Thereby, we are able to control for differences in terms of gender – as men and women may be differently affected – and origin – as respondents from different origin countries or regions face and process discrimination differently. Moreover, state fixed effects and state-specific time trends capture differences by state

[23] However, as a robustness check, we make sure that results also hold when we use other cutoff years.

[24] Here, we slightly deviate from the approach of the original paper. Therein, the authors use country of origin fixed effects for ten larger groups: EU-15, EU-12 (EU Accession countries), Turkey, MENA, former Yugoslavia, post-Soviet countries, Africa, North America, South America, and Asia. We instead use the dummy variables we also employed in the previous section 4.4, namely for Latin America, East Asia, Africa, Turkey + MENA, Western countries, Eastern Europe (excl. countries in the Western Balkans), and countries in the Western Balkans, allowing respondents to have multiple regions of origin. We additionally include a dummy indicating whether a person is from an EU country and whether the person came to Germany as a refugee. (As there are no ethnic Germans in the sample, we do not control for that.)

Table 4.5 ITT Effects of Citizenship Reforms: Main

	Naturalized		Perceived Discrimination	
	(1)	(2)	(3)	(4)
Residency Req.	-0.0062***	-0.0076***	0.0184*	0.0205**
	(0.0022)	(0.0022)	(0.0094)	(0.0094)
Years since Arrival	0.0154	0.0125	0.1378	0.1505
	(0.0293)	(0.0296)	(0.1557)	(0.1549)
Years since Arrival sq.	-0.0001**	-0.0001**	0.0004*	0.0004
	(0.0001)	(0.0001)	(0.0002)	(0.0002)
Age	0.0079**	0.0075*	0.0389**	0.0308*
	(0.0039)	(0.0040)	(0.0165)	(0.0172)
Age sq.	-0.0001**	-0.0001**	-0.0004	-0.0003
	(0.0001)	(0.0001)	(0.0002)	(0.0002)
Female	-0.0050	-0.0016	-0.0924***	-0.0964***
	(0.0067)	(0.0066)	(0.0255)	(0.0257)
Medium-skilled		0.0389***		-0.0594**
		(0.0078)		(0.0302)
High-skilled		0.0362***		-0.0385
		(0.0092)		(0.0319)
In School or Training		0.0145		-0.0952*
		(0.0176)		(0.0547)
Cohort of Arrival FE	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes
Region of Origin Dummies	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
State-Specific Linear Trends	Yes	Yes	Yes	Yes
Mean	0.531	0.531	0.533	0.533
N	8181	8181	6134	6134

Note: The table reports results estimating equation 4.3. Column (1)/(2): Dependent variable is whether respondent naturalized. (3)/(4): Dependent variable is perceived discrimination (standardized). Standard errors are clustered by age times year of arrival. * $p < .10$, ** $p < .05$, *** $p < .01$

of residence and changes over time within these states. Lastly, standard errors are clustered by age times year of arrival.

4.5.3 Main Results

The results of estimating equation 4.3 can be seen in Table 4.5. In columns (1) and (2), we first test whether waiting periods affect respondents' probability to naturalize – which essentially serves as a first stage. Therefore, instead of using perceived discrimination our dependent variable is whether a person is naturalized in year t.[25] The estimate in column (1) indicates that increasing one's waiting time by one year decreases the person's likelihood to naturalize by 0.6 percentage points. This means that a reduction in residency requirements of seven years – the reduction brought about by the 2000 reform – increases one's probability to naturalize by around 4 percentage points. Once we control for educational attainment in column (2), the coefficient rises a bit.[26]

Columns (3) and (4) display the reduced-form estimates of residency requirements on perceived discrimination. The coefficient in column (3) is positive and (weakly) significant (p-value = 0.05), indicating that each additional year a person has to wait longer increases perceived discrimination by 1.84 percent of a standard deviation. Phrased differently, reducing the waiting period by seven years decreases perceived discrimination by around 13 percent of a standard deviation ($0.0184 \cdot 7 = 12.88$). Again, the coefficient is slightly larger once we control for educational attainment (4).

4.5.4 Robustness Checks and Heterogeneity

To evaluate the robustness of our main findings, we perform a number of additional checks. As a baseline, we use the specification in Table 4.5, column (3).

First, in Table 4.A6 in the appendix, we modify the control variables used in our main model. Hereby, column (1) shows the baseline estimation. In column (2), instead of using year of arrival and year of birth fixed effects, we assign respondents to larger groups with five-year intervals for year of arrival and year of birth.[27] In column (3), we use country of origin dummies as originally employed in the study by Gathmann and Keller (2018). In column (4) we only use an East-West dummy instead of state fixed effects, while in column (5), we include state fixed effects, but treat the former East German states as if they were just one state – because the number of migrants in East Germany is very small (around 3 percent). Lastly, in column (6), we use state-year

[25]The samples are slightly smaller as perceived discrimination was not surveyed in 2012, 2014, 2016 and after 2017).

[26]It is noteworthy, that compared to the estimates in Gathmann and Keller (2018), the coefficients are only around half as large. One reason for that could be that the sampling of migrants in the SOEP differs from the micro-census, as it for a long time over-represented migrants from Turkey, Southern Europe, and former Yugoslavia (as well as ethnic Germans from Eastern Europe), but usually only had few migrants from other countries. While this problem was alleviated somewhat by the inclusion of the recent migrant samples starting in 2013, results displayed are to a large extent driven by the mentioned groups.

[27]E.g., a person born in 1972 is put into the bracket of those born between 1971 and 1975.

fixed effects instead of linear trends. Across specifications, our main coefficient remains fairly stable, never deviating strongly from the baseline estimate in terms of size or significance.

Second, in Table 4.A7 in the appendix, we include additional control variables to evaluate whether our estimates are sensitive to the inclusion of potential confounders. Hereby, we gradually add information on whether respondents live in an urban area (1), marriage status and number of children (2), language proficiency (3), personal labor income and labor market status (4), and health status (5).[28] Column (6) shows results with all additional controls. Estimates show that our main coefficient is barely affected by the introduction of these control variables.

Third, we evaluate whether our results are affected by our choice of the cutoff year. As mentioned previously, our estimations include the survey waves starting in 2002, as we want respondents to be affected by both reforms and to account for the phrasing of the survey question on perceived discrimination. Table 4.A8 in the appendix reveals, however, that results do remain positive and weakly significant even when choosing a different cutoff.

Fourthly, we use binary outcome measures in Table 4.A9 (Appendix). In Panel A, the outcome is one if respondents experienced at least some discrimination, and zero else. In column (1) we see that a decrease in waiting periods did not reduce experiences of discrimination for the full sample. We therefore check whether there is a change in frequently experienced discrimination in Panel B by constructing our dependent variable to be one only if discrimination was experienced often. The coefficient in column (1) is negative and significant. Thus, it appears that lowering residence requirements particularly leads to a decline in the perception of frequent discrimination.

Lastly, we again check for potential heterogeneities by gender and region of origin. Therefore, we conduct subsample regressions in Table 4.6. Interestingly, our results are very much in line with the findings in the previous section, where we estimated the direct effects of naturalization. Again, we find a highly significant effect on Eastern European migrants.[29] Reducing waiting period by seven years decreases perceived discrimination

[28]Considering the relatively small sample size and the already rather extensive amount of control variables in the baseline model, we had to be careful in our choice of covariates. Therefore, the additional control variables used in this analysis are a little less extensive compared to those used in the specification in Table 4.1, column (6). E.g., instead of including different expressions of marriage status (married, widowed, divorced), we just include a dummy for whether the respondent is married.

[29]Distinguishing between Eastern Europeans from the Western Balkans and outside, we run separate regressions, which are not shown for brevity. We find that the effects are largely driven by the former group. However, these results could be due to sampling, as many Eastern Europeans were

Table 4.6 ITT Effects of Citizenship Reforms: Heterogeneity

	(1) Men	(2) Women	(3) West	(4) Eastern Europe (incl. Balkan)	(5) Non-Western Non-Europe
Residency Req.	0.0272*	0.0112	0.0057	0.0437**	0.0088
	(0.0142)	(0.0125)	(0.0225)	(0.0194)	(0.0142)
Cohort of Arrival FE	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes
Region of Origin Dummies	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
State-Specific Linear Trends	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Mean	0.566	0.507	0.318	0.469	0.660
N	2738	3396	1177	2058	3080

Note: The table reports results estimating equation 4.3 but splitting the sample by gender or region of origin. Regressions otherwise specified as in column (3) of Table 4.5. The outcome variable is standardized. Standard errors (in parentheses) are clustered at the age times arrival year level. * $p < .10$, ** $p < .05$, *** $p < .01$

by around 30 percent of a standard deviation ($0.047 \cdot 7 = 30.59$), which is a large effect. Moreover, the coefficient for men is also (weakly) significant, suggesting a drop in perceived discrimination of nearly 20 percent of a standard deviation ($2.72 \cdot 7 = 19.04$).[30] Columns (2)-(6) in Table 4.A9 also look at heterogeneities by gender and region of origin, but use binary outcomes. Thereby, it shows that lower residence requirements reduced Eastern Europeans' experiences of infrequent discrimination, while it led to decreases of frequent discrimination among men.

Thus, overall, even though the approach used in this section differs quite substantially from the approach in section 4.4, results are not too different. In both cases, estimates indicate that naturalization leads to a reduction in perceived discrimination for Eastern European migrants, while other groups are not affected. Moreover, while the estimate for men is weakly significant only when using approach 2, the directions of the coefficients are consistent.

only included in the SOEP starting in 2013, i.e., after many of them were already EU citizens. In contrast, the sample size on migrants from the Western Balkans was already quite large.

[30]This result is interesting insofar as Gathmann and Keller (2018) found that that quicker access to citizenship mostly benefited women and not men. Thus, if we were to assume that our effects were substantially driven by improvements on the labor market, our results would be in contrast to these findings.

4.6 Natural Experiment: EU Enlargement

In the previous two sections, we employ two different approaches to evaluate the effects of naturalization on migrants' perceived discrimination, thereby testing the effects of a change in legal status. However, respondents may benefit from very similar rights and privileges compared to Germans if they are citizens of other EU countries. In extension, migrants in Germany would experience a change in legal status once their home countries become part of the EU.

To evaluate this we can make use of the exogenous variation created by different phases of EU enlargement over the years – which, in essence, presents us with a natural experiment. Moreover, this serves as an additional test of our main results regarding Eastern Europeans as mostly Eastern Europeans countries acceded the EU in recent years. We hereby exploit three waves of EU accessions after 1996:

- In 2004: Cyprus, Czechia, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia, and Slovenia.
- In 2007: Bulgaria, and Romania.
- In 2013: Croatia.

We can make use of the timing of these enlargements and estimate a kind of staggered difference-in-differences estimation, where we regress perceived discrimination on a dummy variable indicating whether a respondent is part of an EU country. Respondents are therefore treated once their home country joins the EU, with the treatment variable being zero beforehand.

For our estimations, we restrict our sample to respondents with a stable non-German nationality – meaning that they did not experience a change in citizenship over the observed time period. Additionally, we exclude all respondents whose home country was already part of the EU in 1996. Additionally, we exclude nationals from Iceland, Liechtenstein, Norway, and Switzerland as member states of the European Free Trade Agreement (EFTA). Lastly, we only keep respondents from countries that either became part of the EU after 1996 or those whose home countries have or had a plausible path to EU membership, e.g., because they are candidate countries. For further specifications, we extend our sample to first include citizens from post-Soviet countries, and then those from all non-EU, non-EFTA countries. The construction of these samples is summarized in Table 4.A10 (Appendix).

Unfortunately for us, the SOEP surveyed only few respondents from the EU accession countries in earlier waves. This is evident in Table 4.A11 (Appendix). There you can see that for 2004, the sample only consists of 35 individuals from the ten countries that joined the EU, with numbers remaining low even after the accession of Romania and Bulgaria in 2007, indicating that there are few individuals from these countries in the dataset. The sample size of treated people only starts to become substantial from 2013 onwards, when the SOEP started to include additional and specific migrant samples (Brücker et al., 2014). Because of this sample composition, we only have very little variation to work with. We therefore caution that the results presented below may not be very robust, but should rather be seen as complementary to our previous analyses.

Nevertheless, we use this sample to perform a staggered difference-in-differences estimation with the following regression equation:

$$PD_{it} = \beta_0 + \beta_1 T_{it} + \gamma X_{it} + S_{it} + \tau_t + \rho_i + \epsilon_{it}. \tag{4.4}$$

Our dependent variable (standardized) perceived discrimination (PD_{it}) of respondent i, interviewed in t, is regressed on the treatment variable T_{it}, which is a dummy variable indicating whether a respondent is citizen of an EU country. In addition, we also include individual, state, and year fixed effects. Lastly, we again introduce exogenous and thereafter also potentially endogenous control variables (X_{it}) in the regression, which are listed in Table 4.A2. Standard errors are clustered by individual.[31]

The results are shown in Table 4.7 with Panel A showing estimates without further controls, Panel B including plausibly exogenous control variables, and Panel C employing all controls together. Columns (1) to (3) show results when we use the preferred sample of EU accession countries and countries with a plausible path to the EU, where the full sample is used in column (1), and subsets by gender are employed in the following two columns. Columns (4) and (5) then show results for the extended samples.

Across specifications, estimates indicate that perceived discrimination decreased for respondents after their home countries joined the EU. While coefficients are somewhat

[31]We are aware of the ongoing debate in the literature on two-way fixed effects regressions (De Chaisemartin and d'Haultfoeuille, 2020, 2023; Callaway et al., 2024). However, given the restricted nature of our data, it was not possible for us to use newer estimators like the one from Callaway and Sant'Anna (2021). We, however, again would recommend to interpret the results not by themselves but together with our findings in the earlier sections.

Table 4.7 EU-Enlargement

	Sample 1				
	(1) All	(2) Men	(3) Women	(4) Sample 2	(5) Sample 3
Panel A: Without Controls					
Treated	-0.220**	-0.409***	-0.166	-0.192**	-0.178**
	(0.088)	(0.142)	(0.106)	(0.085)	(0.084)
Panel B: With Exog. Controls					
Treated	-0.298**	-0.494**	-0.247	-0.260**	-0.243**
	(0.122)	(0.203)	(0.152)	(0.108)	(0.106)
Panel C: With All Controls					
Treated	-0.304**	-0.486**	-0.257*	-0.262**	-0.244**
	(0.124)	(0.208)	(0.155)	(0.110)	(0.108)
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Indiv. FE	Yes	Yes	Yes	Yes	Yes
Mean	0.592	0.628	0.560	0.585	0.578
N	15258	7303	7955	16334	19367

Note: The table reports regression estimates of equation 4.4 using different control groups. Panel A displays results without additional controls, Panel B shows estimates with exogenous controls, and Panel C shows estimates with all controls (controls are listed in Table 4.A2). The outcome variable perceived discrimination is standardized. Standard errors (in parentheses) are clustered at the person level. * $p < .10$, ** $p < .05$, *** $p < .01$

smaller when we extend the sample, they remain negative, significant, and large. Moreover, they increase once we include further controls. Interestingly, the coefficient in column (1), Panel B, shows that becoming an EU citizen decreases perceived discrimination by around 30 percent of a standard deviation. This effect is basically identical to the effect of a reduction in waiting times of seven years among Eastern Europeans (which we found in section 4.5). In addition, we also find that the effects seem much larger for men than for women. While men's perceived discrimination decreases by nearly 50 percent of a standard deviation after becoming an EU citizen, effects are at most borderline significant for women, with coefficients only being about half as large.

Thus, findings from this exercise of exploiting EU accession of (mostly) Eastern European countries appear in line with the evidence from the previous sections. They suggest that men and Eastern Europeans benefit most from an improvement of their legal status, either because of naturalization or EU accession of their home countries.

4.7 Conclusion

In this study, we examine the impact of legal status on migrants' perceived discrimination, focusing in particular on the effects of naturalization. Hereby, we use data from the German socio-economic panel, in which perceived discrimination is elicited by asking respondents with a migrant background to what extent they have felt disadvantaged in the previous two years because of their ethnic origin. Before conducting our main analysis, we show that perceived discrimination is related to strong and significant decreases in well-being, mental health, lower staying intentions, higher probabilities to actually leave Germany, and to drop out of the panel as well as other variables. Moreover, these relationships hold even after controlling for time-invariant individual heterogeneity.

To estimate the effects of a change in legal status, we employ two different methods, by, first, estimating a fixed effects model (Steinhardt, 2012) and, second, exploiting variation from two citizenship reforms in Germany (Gathmann and Keller, 2018). While the first approach examines the direct impact of naturalization on perceived discrimination, the second approach exploits exogenous variation in residency requirements due to reforms of German citizenship laws in 1991 and 2000. Despite differences in methodology, results are similar, as both find that only Eastern European migrants benefit from (easier access to) naturalization. However, effects are only significant for the whole sample (and for men) when using the latter approach. In addition to these two approaches, we perform a further analysis exploiting variation from EU enlargement, which (apart from Cyprus and Malta) only affected Eastern European countries. Results show that (especially male) respondents whose home countries become part of the EU – leading to an improvement of those migrants' legal status – experienced a large and significant decline in perceived discrimination.

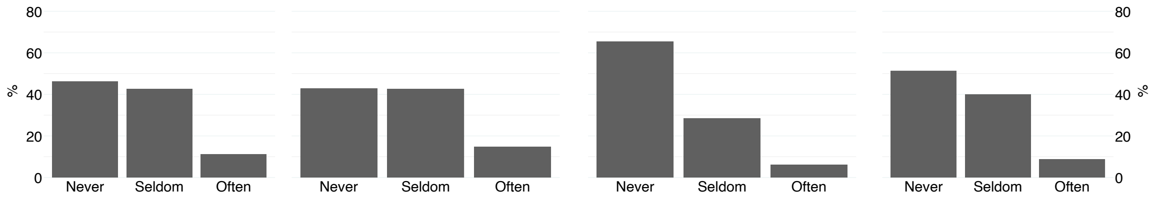
Our findings have numerous policy implications. First, our results indicate that (perceived) discrimination is not only detrimental to migrants who are affected, but also to the German economy, which increasingly relies on foreign workers. Second, we can show that acquiring host country citizenship leads to a decrease in perceived discrimination, which probably reflects that naturalized citizens gain privileges and rights and experience a reduction of barriers on the labor market. Observed effects, however, are not spread evenly across different migrant groups, but mainly seem to affect migrants from Eastern Europe. We hypothesize that this is due to the ethnic and cultural distance between natives and migrants (Spolaore and Wacziarg, 2016):

As Eastern Europeans appear closer in terms of appearance, customs, and religion to natives, they may experience less hostility and racist encounters in everyday life than other non-European migrant groups (such as those from the MENA region, sub-Saharan Africa or East Asia) (Booth et al., 2012). Instead issues like labor market access, extensions of temporary residence permits, issues on the housing market or family reunions – which may lead to a feeling of being disadvantaged – are probably more salient in their everyday lives. In contrast, although members of non-European migrant groups may also benefit from acquiring German citizenship, their experiences with discrimination are likely shaped much more by other factors (like skin color, religious clothing, accents) than by their nationality (Vernby and Dancygier, 2019). Our findings would therefore suggest that improving access to German citizenship (such as through the recent German citizenship reform in 2024) will likely lead to less of a reduction in perceived discrimination among non-European migrant groups. Hence, other measures would be needed to address this issue among these groups. Third, results regarding EU enlargement would suggest that decreases in perceived discrimination are not solely tied to acquiring citizenship, but may just as well be achieved through other improvements in legal status. This would suggest that policymakers could also pursue other measures to decrease perceived discrimination – e.g., by lowering barriers to the labor market.

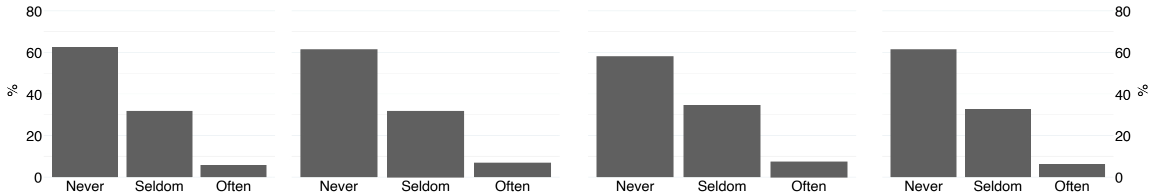
4.A Additional Tables and Figures (Appendix A)

Figure 4.A1 Distribution of Perceived Discrimination By Country of Origin and Gender

(a) TMENA (b) Sub-Sah. Africa (c) Latin America (d) East Asia



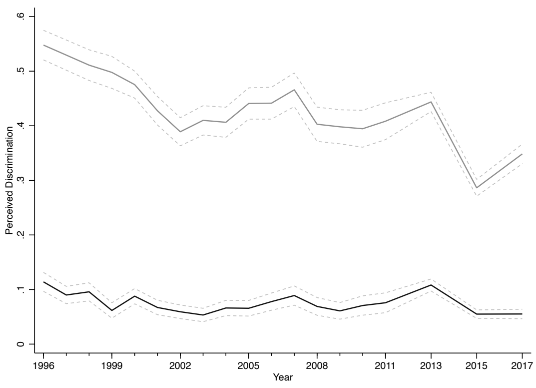
(e) Eastern Europe (f) Balkan Countries (g) Men (h) Women



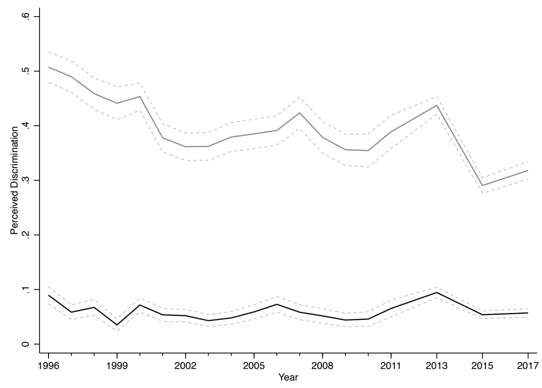
Note: The figure presents the distribution of response options regarding perceived discrimination averaged over the observed time period from 1996 to 2017 for various sub-samples by region of origin (a-f) and gender (g-h).

Figure 4.A2 Perceived Discrimination over Time by Gender

(a) Men

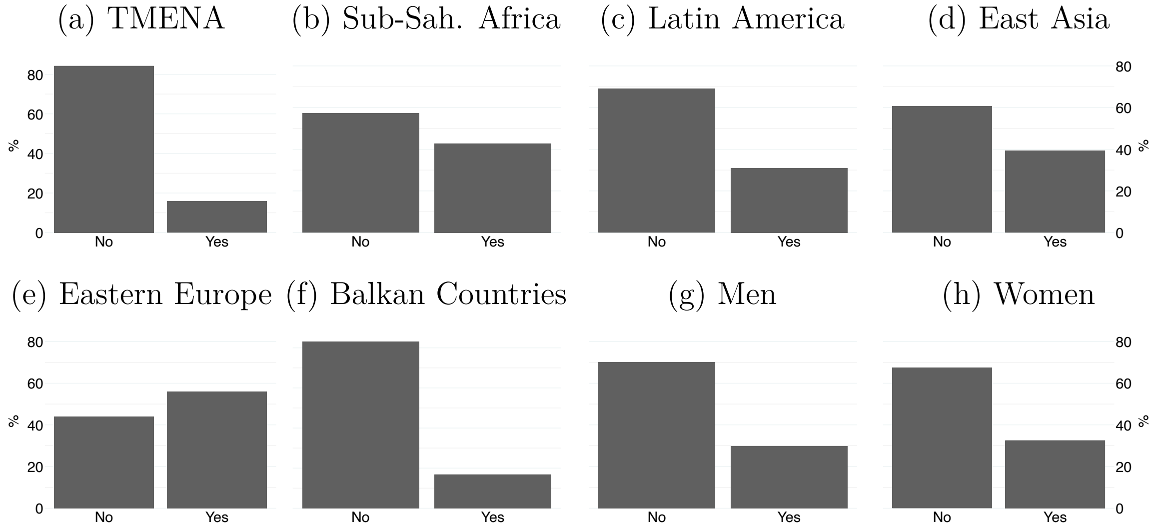


(b) Women



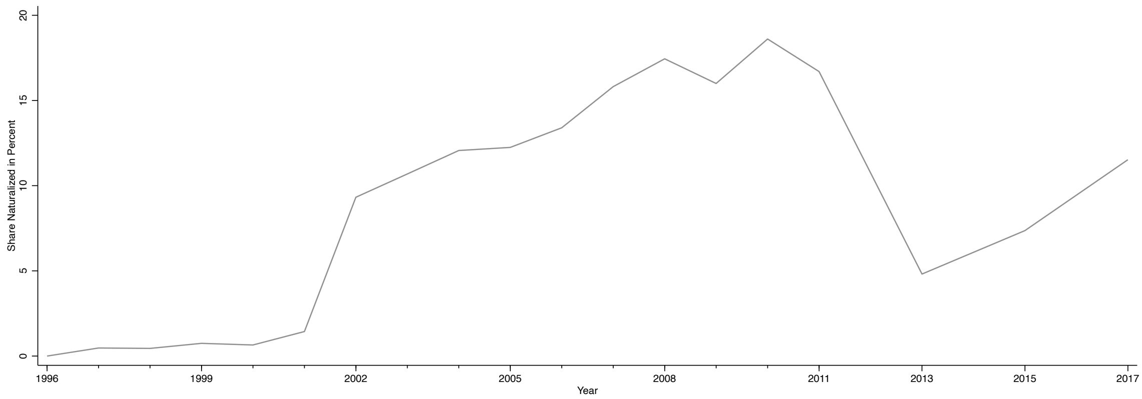
Note: Figure presents time trends of perceived discrimination including 95 percent confidence intervals for men (a) and women (b). Grey line: Has experienced discrimination at least sometimes. Black line: Has experienced discrimination often.

Figure 4.A3 Share of Migrants with German Citizenship By Origin Groups and Gender



Note: The figure displays the share of migrants with German citizenship averaged over the observed time period from 1996 to 2017 for various sub-samples by region of origin (a-f) and gender (g-h).

Figure 4.A4 Share of Naturalized Respondents over Time



Note: This figure presents the share of naturalized respondents in percent over time for the sample used in our first approach (section 4.4).

Table 4.A1 Descriptive Statistics of Perceived Discrimination Variable

Year	Never	%	Seldom	%	Often	%	Total	%	Mean	SD
1996	1197	47.20	1080	42.59	259	10.21	2536	100.00	0.63	0.66
1997	1201	48.98	1068	43.56	183	7.46	2452	100.00	0.58	0.63
1998	1216	51.42	955	40.38	194	8.20	2365	100.00	0.57	0.64
1999	1151	52.97	916	42.15	106	4.88	2173	100.00	0.52	0.59
2000	1677	53.54	1205	38.47	250	7.98	3132	100.00	0.54	0.64
2001	1651	59.73	946	34.23	167	6.04	2764	100.00	0.46	0.61
2002	1763	62.50	901	31.94	157	5.57	2821	100.00	0.43	0.60
2003	1638	61.46	899	33.73	128	4.80	2665	100.00	0.43	0.58
2004	1541	60.76	851	33.56	144	5.68	2536	100.00	0.45	0.60
2005	1418	58.81	843	34.96	150	6.22	2411	100.00	0.47	0.61
2006	1392	58.51	808	33.96	179	7.52	2379	100.00	0.49	0.63
2007	1201	55.65	800	37.07	157	7.28	2158	100.00	0.52	0.63
2008	1223	61.00	662	33.02	120	5.99	2005	100.00	0.45	0.61
2009	1273	62.43	660	32.37	106	5.20	2039	100.00	0.43	0.59
2010	1107	62.72	557	31.56	101	5.72	1765	100.00	0.43	0.60
2011	1092	60.23	594	32.76	127	7.00	1813	100.00	0.47	0.62
2013	3729	55.97	2261	33.94	672	10.09	6662	100.00	0.54	0.67
2015	5156	71.14	1698	23.43	394	5.44	7248	100.00	0.34	0.58
2017	4015	66.83	1655	27.55	338	5.63	6008	100.00	0.39	0.59
Total	34641	59.80	19359	33.42	3932	6.79	57932	100.00	0.47	0.62

Note: The table reports response frequencies on the question about perceived discrimination including percentages, means, and standard deviations for each year. Frequencies are based on the full sample in the SOEP without further restrictions.

Table 4.A2 Descriptive Statistics of Control Variables

	Full Sample					Never Disc.			Seldom/Often			Difference		
	N	Mean	SD	Min	Max	N	Mean	SD	N	Mean	SD	Coef	SE	P-Value
"Exogenous" Variables														
Age	57,931	41.664	15.096	15	100	34,641	42.569	15.699	23,290	40.319	14.044	1.791	0.123	0.000
Age sq.	57,931	1963.8	1391.2	225	10000	34,641	2058.5	1473.1	23,290	1822.8	1246.5	222.3	11.3	0.000
Female	57,932	0.523	0.499	0	1	34,641	0.537	0.499	23,291	0.502	0.500	0.029	0.004	0.000
Born In Germany	57,932	0.190	0.392	0	1	34,641	0.208	0.406	23,291	0.163	0.369	0.071	0.003	0.000
Refugee	57,932	0.058	0.234	0	1	34,641	0.050	0.217	23,291	0.071	0.257	-0.026	0.002	0.000
Year of Arrival	45,177	1986.9	13.7	1950	2017	26,301	1986.7	14.1	18,876	1987.2	13.1	-0.388	0.126	0.002
Latin America	57,932	0.012	0.108	0	1	34,641	0.013	0.113	23,291	0.010	0.100	0.003	0.001	0.001
East Asia	57,932	0.025	0.156	0	1	34,641	0.021	0.145	23,291	0.030	0.171	-0.008	0.001	0.000
Sub-Saharan Africa	57,932	0.010	0.102	0	1	34,641	0.007	0.086	23,291	0.015	0.121	-0.008	0.001	0.000
Turkey, MENA	57,932	0.254	0.435	0	1	34,641	0.196	0.397	23,291	0.340	0.474	-0.147	0.003	0.000
West	57,932	0.259	0.438	0	1	34,641	0.303	0.460	23,291	0.194	0.395	0.109	0.003	0.000
East Europe	57,932	0.325	0.468	0	1	34,641	0.339	0.473	23,291	0.303	0.460	0.039	0.004	0.000
Balkan Countries	57,932	0.135	0.342	0	1	34,641	0.138	0.345	23,291	0.130	0.336	0.006	0.003	0.029
Resettler	57,932	0.197	0.398	0	1	34,641	0.205	0.404	23,291	0.185	0.389	-0.000	0.003	0.897
EU Citizen	57,932	0.301	0.459	0	1	34,641	0.341	0.474	23,291	0.241	0.428	0.091	0.004	0.000
German School Degree	57,932	0.414	0.493	0	1	34,641	0.436	0.496	23,291	0.381	0.486	0.022	0.004	0.000
Foreign School Degree	57,932	0.440	0.496	0	1	34,641	0.429	0.495	23,291	0.458	0.498	-0.074	0.004	0.000
Upper 2nd School Degree	57,932	0.277	0.448	0	1	34,641	0.297	0.457	23,291	0.248	0.432	0.027	0.004	0.000
Uni Degree	57,932	0.153	0.360	0	1	34,641	0.160	0.366	23,291	0.143	0.351	0.005	0.003	0.054
Voc. Training	57,932	0.442	0.497	0	1	34,641	0.457	0.498	23,291	0.420	0.494	-0.010	0.004	0.014
"Endogenous" Variables														
Low Language Prof.	20,694	0.122	0.327	0	1	12,771	0.119	0.324	7,923	0.127	0.333	-0.006	0.004	0.114
Medium Lang. Prof.	20,694	0.230	0.421	0	1	12,771	0.216	0.412	7,923	0.253	0.435	-0.035	0.005	0.000
High Lang. Prof.	20,694	0.648	0.478	0	1	12,771	0.665	0.472	7,923	0.620	0.485	0.042	0.006	0.000
Married	57,932	0.664	0.472	0	1	34,641	0.649	0.477	23,291	0.686	0.464	-0.102	0.004	0.000
Divorced/Separated	57,932	0.086	0.280	0	1	34,641	0.086	0.281	23,291	0.085	0.279	-0.004	0.002	0.046
Widowed	57,932	0.026	0.160	0	1	34,641	0.031	0.174	23,291	0.019	0.137	0.008	0.001	0.000
Children	57,873	0.930	1.165	0	10	34,600	0.870	1.151	23,273	1.019	1.181	-0.093	0.010	0.000
Lives Alone	57,932	0.082	0.274	0	1	34,641	0.086	0.280	23,291	0.077	0.266	0.009	0.002	0.000
Urban Residence	57,932	0.819	0.385	0	1	34,641	0.814	0.389	23,291	0.827	0.378	-0.018	0.003	0.000
Real Labor Income	34,578	2.352	1.970	0	61	20,973	2.433	2.141	13,605	2.227	1.665	0.250	0.024	0.000
Works Full Time	57,930	0.404	0.491	0	1	34,641	0.407	0.491	23,289	0.398	0.489	-0.001	0.004	0.814
Works Part Time	57,930	0.107	0.309	0	1	34,641	0.112	0.315	23,289	0.099	0.299	0.012	0.003	0.000
Works Irregularly	57,930	0.079	0.270	0	1	34,641	0.078	0.268	23,289	0.081	0.272	-0.002	0.002	0.345
In Education	57,930	0.076	0.266	0	1	34,641	0.077	0.267	23,289	0.075	0.264	0.025	0.002	0.000
Unemployed	57,930	0.092	0.289	0	1	34,641	0.076	0.265	23,289	0.116	0.320	-0.042	0.002	0.000
Retired	57,930	0.069	0.253	0	1	34,641	0.083	0.276	23,289	0.047	0.211	0.033	0.002	0.000
Non Working	57,930	0.174	0.379	0	1	34,641	0.166	0.372	23,289	0.185	0.388	-0.025	0.003	0.000
Sick	57,862	0.167	0.373	0	1	34,606	0.159	0.365	23,256	0.180	0.384	-0.022	0.003	0.000
Disabled	57,765	0.075	0.263	0	1	34,543	0.075	0.263	23,222	0.075	0.263	-0.000	0.002	0.961

Note: The table reports means and standard deviations of explanatory variables used as additional control variables in regressions of sections 4.4 and 4.6, and 4.B, apart from state of residence and year. Descriptive statistics are based on the full sample in the SOEP without further restrictions, apart from requiring respondents to have also given an answer to the questions related to perceived discrimination in the respective interview.

Table 4.A3 Categorization of Countries into Regions of Origin

Region	Countries
Western	Australia, Austria. Belgium, Benelux, Canada, Cyprus, Denmark, Finland, France, Greece, Great Britain, Ireland, Israel, Italy, Luxembourg, New Zealand, Netherlands, Norway, Portugal, Sweden, Switzerland, Spain, USA
Eastern Europe	Armenia, Azerbaijan, Belarus, Bulgaria, Chechnya, Czechia, Eastern Europe, Estonia, Georgia, Hungary, Kabardino-Balkaria, Kazakhstan, Kyrgyzstan, Latvia, Lithuania, Moldova, Poland, Romania, Russia, Slovakia, Tajikistan, Turkmenistan, Ukraine, Uzbekistan
Balkan Countries	Albania, Bosnia and Herzegovina, Former Yugoslavia/Serbia and Montenegro, Kosovo, Kosovo/Albania, Croatia, (North) Macedonia, Montenegro, Serbia, Slovenia
Turkey, Middle East, North Africa	Algeria, Egypt, Iraq, Iran, Jordan, Kurdistan, Kuwait, Lebanon, Libya, Morocco, Palestine, Saudi-Arabia, Stateless, Syria, Tunesia, Turkey, United Arab Emirates, Yemen
Africa (excl. North Africa)	Africa, Angola, Benin, Botswana, Burkina Faso, Cameroon, Chad, Congo, Djibouti, Eritrea, Ethiopia, Gambia, Ghana, Guinea, Ivory Coast, Kenya, Lesotho, Liberia, Madagascar, Mali, Mauritius, Mozambique, Namibia, Niger, Nigeria, Rwanda, Senegal, Seychelles, Sierra Leone, Somalia, South Africa, Sudan, Tanzania, Togo, Uganda, Zambia, Zimbabwe
Latin America	Argentina, Bahamas, Bolivia, Brazil, Chile, Colombia, Costa Rica, Cuba, Dominican Republic, Ecuador, El Salvador, Guyana, Haiti, Honduras, Jamaica, Mexico, Nicaragua, Paraguay, Peru, St. Lucia, Suriname, Trinidad and Tobago, Uruguay, Venezuela
East Asia (incl. Oceania)	Afghanistan, Bangladesh, Cambodia, China, Hong Kong, India, Indonesia, Japan, Korea, Laos, Malaysia, Maldives, Mongolia, Myanmar, Nepal, Pakistan, Philippines, Samoa, Singapore, Sri Lanka, Taiwan, Thailand, Vietnam

Note: The table displays which countries were categorized as belonging to which regions. Country names are directly taken from the SOEP dataset. Therein, some of the entries do not represent actual countries, but either regions within countries (e.g., Chechnya) or broader regions capturing several countries (e.g., Benelux, Kosovo/Albania).

Table 4.A4 Direct Effects of Naturalization: Binary Outcome

	(1) All	(2) Men	(3) Women	(4) West	(5) Eastern Europe (incl. Balkan)	(6) Non-Western Non-Europe
Naturalized	-0.0183	-0.0329	-0.0028	0.0119	-0.0733**	-0.0115
	(0.0169)	(0.0250)	(0.0230)	(0.0388)	(0.0291)	(0.0237)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Indiv. FE	Yes	Yes	Yes	Yes	Yes	Yes
Exog. Contr.	Yes	Yes	Yes	Yes	Yes	Yes
Mean	0.436	0.455	0.419	0.332	0.418	0.543
N	35296	16991	18305	11616	11092	13526

Note: The table shows results after estimating equation 4.1 when using a binary outcome. The dependent variable is one if respondent experienced discrimination at least "seldom", and zero else. Regressions otherwise specified as in column (5) of the Table 4.1. The outcome variable is standardized. Standard errors (in parentheses) are clustered at the person level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 4.A5 Direct Effects of Naturalization: Robustness

	(1) All	(2) Men	(3) Women	(4) West	(5) Eastern Europe (incl. Balkan)	(6) Non-Western Non-Europe
Panel A: Linear and Quadratic Time Trend						
Naturalized	-0.0501	-0.0877	-0.0142	0.0868	-0.1584***	-0.0475
	(0.0344)	(0.0541)	(0.0437)	(0.0715)	(0.0549)	(0.0504)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE						
Indiv. FE	Yes	Yes	Yes	Yes	Yes	Yes
Exog. Contr.	Yes	Yes	Yes	Yes	Yes	Yes
Mean	0.514	0.541	0.488	0.375	0.494	0.654
N	35296	16991	18305	11616	11092	13526
Panel B: Include Seniors						
Naturalized	-0.0380	-0.0715	-0.0034	0.0833	-0.1211**	-0.0410
	(0.0334)	(0.0507)	(0.0442)	(0.0821)	(0.0551)	(0.0463)
Mean	0.503	0.527	0.481	0.363	0.485	0.651
N	38243	18667	19576	13110	11743	14388
Panel C: Excl. Respondents with <8 Obs.						
Naturalized	-0.0672	-0.1090*	-0.0306	0.0642	-0.1728**	-0.0659
	(0.0417)	(0.0654)	(0.0527)	(0.0763)	(0.0721)	(0.0603)
Mean	0.499	0.531	0.469	0.344	0.490	0.647
N	18853	9110	9743	6791	4786	7676
Panel D: Exclude Respondents in Education						
Naturalized	-0.0637*	-0.1066*	-0.0213	0.1005	-0.1616***	-0.0662
	(0.0377)	(0.0582)	(0.0487)	(0.0780)	(0.0597)	(0.0561)
Mean	0.514	0.540	0.490	0.373	0.493	0.659
N	32787	15742	17045	10936	10288	12398
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Indiv. FE	Yes	Yes	Yes	Yes	Yes	Yes
Exog. Contr.	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table shows robustness tests for estimated results of equation 4.1. Panel A: Regressions include linear and quadratic time trends instead of year fixed effects. Panel B: Regressions include respondents 65 or older. Panel C: Regressions only for respondents with at least eight observations. Panel D: Excludes respondents in school, university or training. Regressions otherwise specified as in column (5) of the Table 4.1. The outcome variable is standardized. Standard errors (in parentheses) are clustered at the person level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 4.A6 ITT Effects of Citizenship Reforms: Robustness I

	(1)	(2)	(3)	(4)	(5)	(6)
Residency Req.	0.0184*	0.0161*	0.0178*	0.0206**	0.0194**	0.0197**
	(0.0094)	(0.0089)	(0.0094)	(0.0093)	(0.0094)	(0.0095)
Cohort of Arrival FE	Yes		Yes	Yes	Yes	Yes
Year of Birth FE	Yes		Yes	Yes	Yes	Yes
Grouped Cohort of Arrival		Yes				
Grouped Year of Birth		Yes				
Region of Origin Dummies	Yes	Yes		Yes	Yes	Yes
Region of Origin FE			Yes			
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes			Yes
East Dummy				Yes		
State FE w/ East as One					Yes	
State-Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	
State-Year FE						Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean	0.533	0.533	0.533	0.533	0.533	0.533
N	6134	6134	6134	6134	6134	6134

Note: The table shows robustness tests for estimated results of equation 4.3 by modifying parts of the regression specification. Regressions otherwise specified as in column (3) of the Table 4.5. The outcome variable is standardized. Standard errors (in parentheses) are clustered at the age times arrival year level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 4.A7 ITT Effects of Citizenship Reforms: Robustness II

	(1)	(2)	(3)	(4)	(5)	(6)
Residency Req.	0.0207**	0.0194**	0.0190**	0.0204**	0.0204**	0.0188**
	(0.0094)	(0.0095)	(0.0095)	(0.0094)	(0.0094)	(0.0095)
Cohort of Arrival FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Region of Origin Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes
Urban Residence	Yes					Yes
Social Contr. Var.		Yes				Yes
Language Contr. Var.			Yes			Yes
Labor Contr. Var.				Yes		Yes
Health Contr. Var.					Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean	0.533	0.533	0.533	0.533	0.533	0.533
N	6134	6134	6134	6134	6134	6134

Note: The table shows robustness tests for estimated results of equation 4.3 by gradually including control variables: (1): Respondent lives in urban residence (0/1 dummy). (2): Respondent is married (0/1 dummy), has at least one kid (0/1 dummy). (3): Respondent has at least medium language proficiency (0/1 dummy). (4): Labor market income, respondent is working (0/1 dummy), unemployed (0/1 dummy). (5): Self-assessed health status. (6): All controls at once. Regressions otherwise specified as in column (3) of the Table 4.5. The outcome variable is standardized. Standard errors (in parentheses) are clustered at the age times arrival year level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 4.A8 ITT Effects of Citizenship Reforms: Robustness III

	(1) Baseline: 2002	(2) 2000	(3) 2001	(4) 2003
Residency Req.	0.0184*	0.0156*	0.0153*	0.0178*
	(0.0094)	(0.0087)	(0.0090)	(0.0097)
Cohort of Arrival FE	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes
Region of Origin Dummies	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
State-Specific Linear Trends	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Mean	0.533	0.539	0.536	0.534
N	6134	7037	6591	5681

Note: The table shows robustness tests for estimated results of equation 4.3 by changing the selection of the first year of survey data included in regressions. Regressions otherwise specified as in column (3) of the Table 4.5. The outcome variable is standardized. Standard errors (in parentheses) are clustered at the age times arrival year level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 4.A9 ITT Effects of Citizenship Reforms: Binary Outcome

	(1) All	(2) Men	(3) Women	(4) West	(5) Eastern Europe (incl. Balkan)	(6) Non-Western Non-Europe
Panel A: Cutoff b/w "Never" and "Seldom"						
Residency Req.	0.0060	0.0050	0.0064	0.0064	0.0212**	-0.0013
	(0.0047)	(0.0071)	(0.0062)	(0.0102)	(0.0095)	(0.0069)
Mean	0.453	0.475	0.435	0.276	0.401	0.552
Panel B: Cutoff b/w "Seldom" and "Frequently"						
Residency Req.	0.0058**	0.0128***	0.0006	-0.0032	0.0059	0.0072
	(0.0024)	(0.0042)	(0.0035)	(0.0049)	(0.0047)	(0.0046)
Mean	0.081	0.091	0.072	0.042	0.068	0.108
Cohort of Arrival FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Region of Origin Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes
N	6134	2738	3396	1177	2058	3080

Note: The table reports results estimating equation 4.3 but using a binary outcome. Panel A: The dependent variable is one if respondent experienced discrimination at least "seldom", and zero else. Panel B: The dependent is one if respondent experienced discrimination at least "frequently", and zero else. Regressions otherwise specified as in column (3) of the Table 4.5. Standard errors (in parentheses) are clustered at the age times arrival year level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 4.A10 EU-Enlargement: Construction of Treatment and Control Groups

Region	Countries
Treatment Group	Cyprus, Czechia, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia, Slovenia (starting in 2004), Bulgaria and Romania (starting in 2007), Croatia (starting in 2013).
Control Group 1	Albania, Bosnia & Hercegovina, Georgia, Kosovo, Kosovo-Albania, Macedonia, Moldova, Montenegro, Serbia, Turkey, Ukraine, Yugoslavia/Serbia & Montenegro.
Control Group 2	All countries in Control Group 1 + all other post-Soviet countries in the sample, in this case Armenia, Azerbaijan, Belarus, Kazakhstan, Kyrgyzstan, Russia, Tajikistan, Uzbekistan.
Control Group 3	All non-EU, non-EFTA countries including Control Group 2.

Note: The table displays which countries were categorized as belonging to the treatment and the control groups used for the estimations in Table 4.7. Country names are directly taken from the SOEP dataset.

Table 4.A11 EU-Enlargement: Distribution of Treated Individuals over Time

	Not Treated	Treated	Total
1996	817	0	817
1997	771	0	771
1998	745	0	745
1999	696	0	696
2000	961	0	961
2001	840	0	840
2002	822	0	822
2003	738	0	738
2004	677	35	712
2005	658	27	685
2006	607	33	640
2007	538	34	572
2008	463	31	494
2009	449	47	496
2010	346	43	389
2011	347	47	394
2013	914	524	1438
2015	763	1009	1772
2017	603	673	1276
Total	12755	2503	15258

Note: The table displays in which years how many respondents in our sample were affected by EU-enlargement. Sample consists of treatment group and control group 1.

4.B Implications of Perceived Discrimination (Appendix B)

As outlined in section 4.2.3, we add to the literature on the potential implications of perceived discrimination by looking at its effect on a number of outcome variables.

To do so, we again employ data from the German socio-economic panel, which has already been described in section 4.3. Hereby, we do not perform any sample restrictions. Rather the respective sample for each of the following regressions is purely determined by the number of observations for which we have information on our main explanatory variable perceived discrimination and the respective outcome variable. In our analysis, we look at the following outcomes: (1) Life satisfaction (scaled from 1 to 10); (2) mental health (based on the mental component score); (3) concerns about xenophobia (with available response options: “not concerned at all”, “somewhat concerned”, “very concerned"); (4) whether respondent is feeling German (scaled from 0 (“not at all”) to 4 (“completely”)); (5) perceived connection to home country (scaled from 0 (“not at all”) to 4 (”very strong”)); (6) intention to stay forever in Germany; (7) intention to stay in Germany for at most one year; (8) whether respondent has moved abroad; (9) whether respondent has moved to another state within Germany; (10) whether respondent has dropped out of the SOEP, excluding those who moved abroad; (11) political interest (scaled from 0 ("completely disinterested") to 3 ("very interested"); (12) whether respondent has a preference for a political party in Germany; (13) whether respondent has a preference for a left-wing party in Germany, namely SPD, Bündnis 90/Die Grünen, Die Linke or Piratenpartei; (14) whether respondent has a preference for a center-right party in Germany, namely CDU, CSU or FDP. Hereby, outcomes (1) to (5) and outcome (11) are standardized while all other outcomes are binary (0 = no; 1 = yes).

We use these outcomes as dependent variables in separate regressions, in which two of the three response options to the question on perceived discrimination are used as the main explanatory variables, namely, whether discrimination has occurred "frequently" or "seldom", with "never" being the base category. Hereby, we use the following regression model (with standard errors clustered by person):

$$y_{it} = \alpha_0 + \alpha_1 Seldom_{it} + \alpha_2 Frequently_{it} + \beta Z_{it} + S_{it} + \tau_t + \rho_i + \epsilon_{it}. \tag{4.5}$$

In this model, the respective outcome variable for individual i in year of interview t is regressed on the two dummy variables $Seldom_{it}$ and $Frequently_{it}$, which indicate whether the respondent has rarely or frequently experienced discrimination in the past two years. (If respondents did not experience discrimination, both dummy variables would be zero.) Additionally, we include state of residence (S_{it}) and year fixed effects (τ_t), and control for a number of demographic, social, educational, language, labor market, and health characteristics (Z_{it}). Adding these controls should already to a large extent account for time-varying factors that could affect the relation between perceived discrimination and the respective outcome. In particular, we are able to control for the general effects of integration and assimilation, which may allow migrants to better identify discrimination (Diehl et al., 2021). Moreover, we also add individual fixed effects to the regression (ρ_i). Doing so has the meaningful benefit of allowing us to estimate within-individual effects, or in other words: they eliminate all observable and unobservable time-constant heterogeneity. This means that we are able to account for differences in personality between individuals, which may influence how likely they are to attribute discrimination to certain situations and how willing they are to report discrimination – at least to the extent that these factors are time-constant.

Nevertheless, we caution against interpreting our estimates as causal as we cannot rule out that there is some remaining omitted variation coming from unobserved and time-variant factors, which may influence both our outcomes and perceived discrimination. Furthermore, for many of the outcome variables, we also cannot rule out reverse causality (or at least simultaneity), as, e.g., changes in life satisfaction could potentially lead to changes in how respondents report discrimination.

The results of our estimations can be found in Table 4.B1, with Panel A (Panel B) showing results without (with) individual fixed effects. Generally, we find very striking relationships between perceived discrimination and most of the examined outcomes, with significant effects for both frequent and infrequent discrimination. Thereby, the effects appear particularly large for the former, even after accounting for time-invariant heterogeneity. E.g., facing frequent discrimination is associated with a decrease in life satisfaction (column 1) and mental health (2) of around 25 percent of a standard deviation each. Rather unsurprisingly, there is also a very strong positive relationship with concerns about xenophobia (3). In addition, estimates also indicate that respondents who report discrimination appear to de-identify with Germany (4) and feel stronger attachment to their home countries (5). In addition, there is not only an effect on one's staying intentions, but also observed migration. Respondents are less likely to want to stay in Germany forever (6), more likely to want to leave Germany within one year (7), and, they are also more likely to actually leave Germany after the respective interview (8). Respondents who encountered discrimination were 0.6 percentage points more likely to move abroad, which – considering that the mean in the sample is only around one percent – is a noteworthy effect. Looking at movement between states within Germany, effects appear rather muted (9), with only a borderline significant effect for those reporting rare discrimination. However, the mean value of 0.4 percent is very low, which makes us suspect that many respondents who move residence inside of Germany may simply drop out of the panel. We therefore estimate the effects on panel attrition in (10), but exclude cases in which respondents moved abroad. Hereby, we do find a significant increase in panel attrition for those who rarely faced discrimination. These findings suggest that respondents who face infrequent discrimination may try to alleviate that by moving to another location within Germany while those who feel discriminated more often rather leave the country altogether. Estimates in the last four columns reveal that perceived discrimination may also go along with changes in political preferences. First we see positive coefficients for political interest (11) but also a higher preference for any political party (12). This increased preference for a party appears to benefit left-wing parties (13), while support for moderately conservative parties goes down (14). This is not too surprising, as left-wing parties are usually perceived as being more supportive of the causes of immigrants.

To test the robustness of our estimates and potentially minimize omitted variable bias, we modify our fixed effects regressions to include additional control variables. Results are shown in Table 4.B2. In Panel A, we include month and day of week fixed effects to account for potential seasonality and weekend effects in our estimates. In Panel B, regressions include state GDP per capita and unemployment rates, as changes in the local labor markets may affect the examined relation. In Panel C, we test, whether results may be driven by tragic events experienced by respondents, namely the deaths of relatives or close friends. In Panel D, we examine, whether effects could be caused by changes in worries and satisfactions more generally. Hereby, we include measures that should be completely unrelated to perceived discrimination and the relation in question, namely worries about the economy in general, satisfaction with one's own health, and satisfactions with housework. Panel E shows results when all control variables are inserted together in the regression equation. Overall, our main estimates remain remarkably stable.

Table 4.B1 Effects of Perceived Discrimination on Several Outcomes

	(1) Life Satisf.	(2) Mental Health	(3) Worry Xenophobia	(4) Feel German	(5) Connect Home C.	(6) Stay Forever	(7) Leave Soon	(8) Moved Abroad	(9) Moved to New State	(10) Left SOEP	(11) Polit. Interest	(12) Party Pref.	(13) Left-Wing	(14) Conserv.
Panel A: OLS Results														
Seldom	-0.1947***	-0.3020***	0.2993***	-0.1562***	0.1113***	-0.0598***	0.0053***	0.0035***	0.0019***	0.0174***	0.0395***	-0.0041	0.0161***	-0.0211***
	(0.0110)	(0.0224)	(0.0113)	(0.0147)	(0.0159)	(0.0055)	(0.0014)	(0.0010)	(0.0007)	(0.0034)	(0.0122)	(0.0055)	(0.0047)	(0.0036)
Often	-0.4446***	-0.5678***	0.6643***	-0.3309***	0.1269***	-0.0918***	0.0132***	0.0095***	0.0004	0.0229***	0.1430***	0.0357***	0.0512***	-0.0194***
	(0.0245)	(0.0502)	(0.0207)	(0.0298)	(0.0326)	(0.0104)	(0.0030)	(0.0023)	(0.0012)	(0.0067)	(0.0253)	(0.0103)	(0.0090)	(0.0057)
Panel B: FE Results														
Seldom	-0.0804***	-0.0740***	0.1628***	-0.0449**	0.0451**	-0.0208***	0.0025	0.0015	0.0015*	0.0099***	0.0245***	-0.0006	0.0057	-0.0061**
	(0.0095)	(0.0248)	(0.0107)	(0.0182)	(0.0195)	(0.0042)	(0.0017)	(0.0012)	(0.0009)	(0.0037)	(0.0091)	(0.0045)	(0.0036)	(0.0029)
Often	-0.2347***	-0.2562***	0.3929***	-0.2241***	0.1653***	-0.0776***	0.0132***	0.0057**	-0.0006	0.0092	0.0207	0.0190**	0.0182**	-0.0009
	(0.0217)	(0.0540)	(0.0226)	(0.0386)	(0.0403)	(0.0086)	(0.0040)	(0.0026)	(0.0015)	(0.0072)	(0.0187)	(0.0088)	(0.0077)	(0.0053)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean	7.378	51.071	0.931	2.308	2.325	0.796	0.016	0.010	0.004	0.132	0.965	0.270	0.141	0.121
N	57367	11127	57379	16219	16264	57598	57560	57932	57932	54624	57746	57456	57456	57456

Note: The table reports results when regressing various outcomes on perceived discrimination and additional control variables (listed in Table 4.A2) and state and year fixed effects. Panel A shows estimates for OLS regressions, while shows results after additionally including individual fixed effects. Examined outcomes are: (1) Life satisfaction (scaled from 1 to 10), but standardized; (2) Mental health (based on the mental component score), but standardized; (3) Concerns about xenophobia (response options: “not concerned at all” (1), “somewhat concerned” (2), “very concerned" (3)), but standardized; (4) Attachment to Germany, but standardized; (5) Attachment to home country (both scaled from 0 (“not at all”) to 4 (“completely”/”very strong”)), but standardized; (6) Intention to stay forever in Germany (either yes (1) or no (0); (7) Intention to stay in Germany for at most one year (either yes (1) or no (0); (8) Respondent has moved abroad (either yes (1) or no (0); (9) Respondent has moved to another state within Germany (either yes (1) or no (0); (10) Respondent has dropped out of the SOEP, excluding those who moved abroad (either yes (1) or no (0); (11) Political interest (scaled from 0 ("completely disinterested") to 3 ("very interested"), but standardized; (12) Preference for a political party in Germany (either yes (1) or no (0); (13) Preference for a left-wing party in Germany, namely SPD, Bündnis 90/Die Grünen, Die Linke or Piratenpartei (either yes (1) or no (0); (14) Preference for a center-right party in Germany, namely CDU, CSU or FDP (either yes (1) or no (0). Standard errors (in parentheses) are clustered at the person level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 4.B2 Effects of Perceived Discrimination on Several Outcomes: Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Life Satisf.	Mental Health	Worry Xenophobia	Feel German	Connect Home C.	Stay Forever	Leave Soon	Moved Abroad	Moved to New State	Left SOEP	Polit. Interest	Party Pref.	Left-Wing	Conserv.
Panel A: Month and Weekday FE														
Seldom	-0.0804***	-0.0749***	0.1629***	-0.0465**	0.0443**	-0.0206***	0.0025	0.0015	0.0015*	0.0094***	0.0246***	-0.0005	0.0058	-0.0059**
	(0.0095)	(0.0247)	(0.0107)	(0.0181)	(0.0196)	(0.0042)	(0.0017)	(0.0012)	(0.0009)	(0.0037)	(0.0091)	(0.0045)	(0.0036)	(0.0029)
Often	-0.2342***	-0.2588***	0.3935***	-0.2243***	0.1644***	-0.0776***	0.0133***	0.0056**	-0.0006	0.0085	0.0207	0.0194**	0.0183**	-0.0008
	(0.0217)	(0.0537)	(0.0227)	(0.0385)	(0.0403)	(0.0086)	(0.0040)	(0.0026)	(0.0015)	(0.0072)	(0.0188)	(0.0088)	(0.0077)	(0.0053)
Mean	7.378	51.071	0.931	2.308	2.325	0.796	0.016	0.010	0.004	0.132	0.965	0.270	0.141	0.121
N	57367	11127	57379	16219	16264	57598	57560	57932	57932	54624	57746	57456	57456	57456
Panel B: State GDP/Capita and Unemployment Rate														
Seldom	-0.0804***	-0.0741***	0.1625***	-0.0414**	0.0432**	-0.0210***	0.0026	0.0016	0.0015*	0.0099***	0.0242***	-0.0005	0.0059	-0.0061**
	(0.0095)	(0.0248)	(0.0107)	(0.0182)	(0.0195)	(0.0042)	(0.0017)	(0.0012)	(0.0009)	(0.0037)	(0.0091)	(0.0045)	(0.0036)	(0.0029)
Often	-0.2350***	-0.2570***	0.3924***	-0.2185***	0.1612***	-0.0780***	0.0133***	0.0058**	-0.0006	0.0092	0.0203	0.0193**	0.0185**	-0.0010
	(0.0217)	(0.0540)	(0.0226)	(0.0389)	(0.0404)	(0.0086)	(0.0040)	(0.0026)	(0.0015)	(0.0072)	(0.0188)	(0.0088)	(0.0077)	(0.0053)
Mean	7.378	51.071	0.931	2.308	2.325	0.796	0.016	0.010	0.004	0.132	0.965	0.270	0.141	0.121
N	57367	11127	57379	16219	16264	57598	57560	57932	57932	54624	57746	57456	57456	57456
Panel C: Death of Relative or Close Friend														
Seldom	-0.0807***	-0.0748***	0.1629***	-0.0460**	0.0455**	-0.0208***	0.0025	0.0015	0.0015*	0.0098***	0.0244***	-0.0007	0.0057	-0.0061**
	(0.0095)	(0.0248)	(0.0107)	(0.0182)	(0.0195)	(0.0042)	(0.0017)	(0.0012)	(0.0009)	(0.0037)	(0.0091)	(0.0045)	(0.0036)	(0.0029)
Often	-0.2352***	-0.2538***	0.3929***	-0.2237***	0.1656***	-0.0777***	0.0132***	0.0057**	-0.0006	0.0092	0.0206	0.0190**	0.0181**	-0.0010
	(0.0217)	(0.0537)	(0.0226)	(0.0386)	(0.0403)	(0.0086)	(0.0040)	(0.0026)	(0.0015)	(0.0072)	(0.0187)	(0.0088)	(0.0077)	(0.0053)
Mean	7.378	51.071	0.931	2.308	2.325	0.796	0.016	0.010	0.004	0.132	0.965	0.270	0.141	0.121
N	57367	11127	57379	16219	16264	57598	57560	57932	57932	54624	57746	57456	57456	57456
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indiv. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.6: Effects of Perceived Discrimination on Several Outcomes: Robustness Checks (cont.)

	(1) Life Satisf.	(2) Mental Health	(3) Worry Xenophobia	(4) Feel German	(5) Connect Home C.	(6) Stay Forever	(7) Leave Soon	(8) Moved Abroad	(9) Moved to New State	(10) Left SOEP	(11) Polit. Interest	(12) Party Pref.	(13) Left-Wing	(14) Conserv.
Panel D: Unrelated Worries & Satisfactions														
Seldom	-0.0655***	-0.0598**	0.1583***	-0.0428**	0.0432**	-0.0198***	0.0025	0.0015	0.0016*	0.0098***	0.0220**	-0.0011	0.0055	-0.0063**
	(0.0092)	(0.0248)	(0.0105)	(0.0182)	(0.0196)	(0.0042)	(0.0017)	(0.0012)	(0.0009)	(0.0037)	(0.0091)	(0.0045)	(0.0036)	(0.0029)
Often	-0.2093***	-0.2302***	0.3852***	-0.2227***	0.1619***	-0.0754***	0.0132***	0.0059**	-0.0005	0.0104	0.0131	0.0169*	0.0170**	-0.0019
	(0.0211)	(0.0523)	(0.0222)	(0.0388)	(0.0402)	(0.0086)	(0.0040)	(0.0026)	(0.0015)	(0.0072)	(0.0186)	(0.0088)	(0.0077)	(0.0053)
Mean	7.378	51.071	0.931	2.308	2.325	0.796	0.016	0.010	0.004	0.132	0.965	0.270	0.141	0.121
N	57367	11127	57379	16219	16264	57598	57560	57932	57932	54624	57746	57456	57456	57456
Panel E: All Confounders at Once														
Seldom	-0.0657***	-0.0622**	0.1583***	-0.0420**	0.0411**	-0.0198***	0.0027	0.0015	0.0015*	0.0094***	0.0217**	-0.0008	0.0057	-0.0062**
	(0.0092)	(0.0248)	(0.0105)	(0.0181)	(0.0196)	(0.0042)	(0.0017)	(0.0012)	(0.0009)	(0.0037)	(0.0091)	(0.0045)	(0.0036)	(0.0029)
Often	-0.2095***	-0.2326***	0.3852***	-0.2168***	0.1572***	-0.0757***	0.0134***	0.0060**	-0.0006	0.0097	0.0128	0.0175**	0.0174**	-0.0018
	(0.0211)	(0.0518)	(0.0222)	(0.0389)	(0.0404)	(0.0086)	(0.0040)	(0.0026)	(0.0015)	(0.0072)	(0.0186)	(0.0088)	(0.0077)	(0.0053)
Mean	7.378	51.071	0.931	2.308	2.325	0.796	0.016	0.010	0.004	0.132	0.965	0.270	0.141	0.121
N	57367	11127	57379	16219	16264	57598	57560	57932	57932	54624	57746	57456	57456	57456
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indiv. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reports robustness checks for the fixed effects estimations from Table 4.B1, Panel B, in which various outcomes (described in the table notes of Table 4.B1) are regressed on perceived discrimination, additional control variables (listed in Table 4.A2) and state and year fixed effects. Panel A: Estimations include month and day of week fixed effects. Panel B: Estimations include state GDP per capita and unemployment rates as controls. Panel C: Estimations controls for relatives or close friends. Panel D: Estimations include worries and satisfactions that should be unrelated to perceived discrimination, namely worries about the economy in general, satisfaction with own health, and satisfaction with housework. Panel E: Uses all control variables from Panels A-D combined. Standard errors (in parentheses) are clustered at the person level. * $p < .10$, ** $p < .05$, *** $p < .01$

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English Summary

This dissertation consists of four chapters which study how external events influence attitudes, preferences, perceptions, and identities of migrants and natives in different settings.

The first chapter examines the effects of the European Refugee Crisis between 2014 and 2015 on the ethnic identity of already resident migrants in Germany. Thereby, I exploit the quasi-experimental setting in Germany, by which refugees are allocated to different counties by state authorities without being able to choose their locations themselves. Doing so, I find that higher shares of refugees in a county increased migrants' attachment to their home countries, but not their perceived belonging to Germany. Further analyses uncover strong heterogeneities with respect to country of origin and suggest that concerns about xenophobia, experiences of discrimination, and the consumption of foreign media contributed to these effects. Lastly, I find that changes in ethnic identity coincide with the political polarization of migrants.

The second chapter looks at the effects of far-right protests in Germany on natives' attitudes toward migration nationwide. More specifically, we test whether protesters are able to raise support for their concerns, or whether they are perceived as a threat by the public. To do so, we perform a regression discontinuity design approach to estimate short-term effects on natives' worries about xenophobia and concerns about immigration. Results indicate that protesters were seen as a threat as worries about xenophobia increased while concerns about immigration remained flat after demonstrations took place. Further analyses indicate that media coverage was essential in driving results and that effects were highly dependent on people's preexisting political views, suggesting that protests had polarizing effects.

In the third chapter, we study how unemployment impacts bitterness, which describes a feeling of not having achieved what one deserves compared to others. After finding consistently positive effects using pooled OLS and fixed effects regressions, we identify the causal effect of unemployment on bitterness by exploiting variation from

plant closures and firm layoffs in Germany. Combining matching based on entropy balancing with difference-in-differences estimation, we show that unemployment leads to a substantial and significant increase in bitterness. Further analyses uncover evidence that the experience of job loss, the state of being unemployed, and the duration of unemployment contribute separately to overall effects. Lastly, we find some evidence that effects persist over time.

In Chapter 4, we analyze how changes in legal status affect perceived discrimination of migrants in Germany. Hereby, we follow two distinct approaches. First, studying the direct impact of naturalization, we estimate a fixed effects model. As this method cannot fully account for all potential sources of endogeneity, we thereafter exploit exogenous variation in residency requirements due to two citizenship reforms in Germany. Overall, we find that while naturalization does appear to reduce perceptions of discrimination overall, these effects are limited to men and immigrants from Eastern European countries. Extending the analysis, we exploit exogenous variation from EU enlargement to show that citizens from countries that became part of the EU experienced a significant reduction in discrimination compared to non-EU immigrants.

Deutsche Zusammenfassung

Diese Dissertation besteht aus vier Kapiteln, die in unterschiedlichen Kontexten untersuchen, wie externe Ereignisse Einstellungen, Präferenzen, Wahrnehmungen und Identitäten von Migranten und Einheimischen beeinflussen.

Das erste Kapitel betrachtet die Effekte, die die Europäische Flüchtlingskrise zwischen 2014 und 2015 auf die ethnische Identität von bereits in Deutschland lebenden Migranten hatte. Hierbei nutze ich die quasi-experimentelle Situation in Deutschland aus, wonach Geflüchtete ihren Wohnort nicht selbst wählen konnten, sondern durch staatliche Stellen auf unterschiedliche Kreise verteilt wurden. Meine Ergebnisse zeigen, dass sich die Zugehörigkeit zu ihrem Heimatland von Migranten in Kreisen mit höherem Flüchtlingsanteil erhöht hat, während die empfundene Zugehörigkeit zu Deutschland gleichblieb. In weiteren Analysen zeigen sich starke Heterogenitäten nach Heimatland der Migranten. Zudem wird deutlich, dass Sorgen über Fremdenfeindlichkeit, wahrgenommene Diskriminierung und Konsum von ausländischen Medien die Gesamteffekte beeinflusst haben. Zuletzt zeige ich, dass Veränderungen der ethnischen Identität von Migranten auch mit einer politischen Polarisierung einhergingen.

Das zweite Kapitel untersucht die Effekte von rechtsextremen Protesten in Deutschland auf die Einstellungen von Einheimischen bezüglich Migration. Es wird getestet, ob Protestierende in der Lage waren, die Unterstützung ihrer Interessen zu erhöhen oder ob sie als Bedrohung von der Öffentlichkeit wahrgenommen wurden. Hierbei verwenden wir die Methode der Regressions-Diskontinuitäts-Analyse, um kurzfristige Effekte von Protesten auf Sorgen über Fremdenfeindlichkeit und Immigration zu berechnen. Unsere Ergebnisse zeigen, dass Protestierende als Bedrohung betrachtet wurden, da die Sorgen über Fremdenfeindlichkeit anstiegen, während sich Sorgen über Einwanderung in den Tagen nach den Protesten nicht veränderten. Weitere Analysen deuten darauf hin, dass die Berichterstattung von Medien Ergebnisse wesentlich mitbeeinflusst hat. Zudem gibt es Anzeichen dafür, dass Proteste polarisierende Effekte hatten, da sich Resultate je nach bestehenden politischen Meinungen unterscheiden.

Im dritten Kapitel analysieren wir, wie Arbeitslosigkeit Verbitterung beeinflusst. Verbitterung beschreibt das Gefühl, im Vergleich zu anderen nicht das im Leben erreicht zu haben, was man verdient hat. Nachdem OLS- und Panel-Regressionen mit festen Effekten konsistent positive Effekte finden, identifizieren wir den kausalen Effekt von Arbeitslosigkeit auf Verbitterung, indem wir Variation von Betriebsschließungen und Entlassungen in Deutschland ausnutzen. Hierbei nutzen wir Matching basierend auf Entropy Balancing in Kombination mit einem Differenz-von-Differenzen-Ansatz. Dabei zeigen wir, dass Arbeitslosigkeit zu einem substanziellen und signifikanten Anstieg von Verbitterung führt. Weitere Analysen zeigen, dass Arbeitsplatzverlust, Arbeitslosigkeit und deren Dauer separat zu den Gesamteffekten beitragen. Zuletzt finden wir noch Evidenz für die Persistenz von Effekten.

In Kapitel 4 analysieren wir, wie sich Veränderungen des Rechtsstatus auf die wahrgenommene Diskriminierung von Migranten auswirken. Dabei folgen wir zwei unterschiedlichen Ansätzen. Zuerst untersuchen wir mit Hilfe eines Panel-Modells mit festen Effekten die direkten Auswirkungen einer Einbürgerung. Da wir mit dieser Methode jedoch nicht jede Form von Endogenität ausschließen können, nutzen wir daraufhin exogene Variation durch zwei Einbürgerungsreformen in Deutschland aus. Unsere Ergebnisse zeigen, dass Einbürgerungen die wahrgenommene Diskriminierung von Migranten zwar reduzieren, die Gesamteffekte aber nur von männlichen und osteuropäischen Migranten herrühren. In einer zusätzlichen Untersuchung nutzen wir im Anschluss noch exogene Variation aufgrund der EU-Erweiterung aus, um zu zeigen, dass die wahrgenommene Diskriminierung von Staatsbürgern aus Ländern, die Teil der EU wurden, signifikant zurückgegangen ist.

Ehrenwörtliche Erklärung

Erklärung gemäß §4 Abs. 2

Hiermit erkläre ich, dass ich mich noch keinem Promotionsverfahren unterzogen oder um Zulassung zu einem solchen beworben habe, und die Dissertation in der gleichen oder einer anderen Fassung bzw. Überarbeitung einer anderen Fakultät, einem Prüfungsausschuss oder einem Fachvertreter an einer anderen Hochschule nicht bereits zur Überprüfung vorgelegen hat.

Erklärung gemäß §10 Abs. 3

Hiermit erkläre ich, dass ich für die Dissertation folgende Hilfsmittel und Hilfen verwendet habe:

- Statistiken und Regressionen: Stata, Microsoft Excel
- Schriftsatz und Formatierung: LaTeX, Overleaf

Auf dieser Grundlage und soweit nicht anders vermerkt (siehe "Koautorenschaft und Publikationen") habe ich die Arbeit selbstständig verfasst.

Christopher Prömel
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