The integration process of migrants and refugees in Germany

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The Integration Processes of Migrants and Refugees in Germany

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Abstract: In this PhD thesis, I look at different aspects of the integration process of migrants and refugees, and how their presence affects the protest activity and attitudes of natives. The four chapters build on and aim to extend the existing literature along several dimensions. The first and second chapters improve our understanding of the causal effect of home country conditions on migrants' return intentions and labour market outcomes at destination and of the effect of traumatizing experiences along the journey on refugees' labour market integration. The third chapter explores the socio-cultural integration of family migrants in a country that was not their primary choice, while the fourth chapter examines the effect of the presence of foreigners on the protest activity of a particular group of natives and how these protests affect worries about xenophobia and intolerance at the national level. The findings in the dissertation aim to inform policymakers on the potential side-effect of migration policies and to provide empirical evidence that help improve existing policies and better design future ones.

Abstrakt: In dieser Dissertation untersuche ich verschiedene Aspekte des Integrationsprozesses von Migranten und Flüchtlingen, insbesondere wie sich ihre Präsenz auf die Protestaktivitäten und Einstellungen von Einheimischen auswirkt. Die vier Kapitel bauen auf der bestehenden Literatur auf und zielen darauf ab, diese in mehreren Aspekten zu ergänzen. Das erste und das zweite Kapitel verbessern unser Verständnis der kausalen Auswirkungen der Bedingungen im Heimatland auf die Rückkehrabsichten und die Arbeitsmarktintegration von Migranten im Zielland sowie das Verständnis der Auswirkungen traumatischer Erfahrungen während der Flucht auf die Arbeitsmarktintegration von Flüchtlingen. Das dritte Kapitel befasst sich mit der soziokulturellen Integration von Familienmi- granten in einem Land, das nicht ihre erste Wahl darstellt. Das vierte Kapitel untersucht die Auswirkungen der Anwesenheit von Ausländern auf die Protestaktivität einer bestimmten Gruppe von Einheimischen und wie sich diese Proteste auf die Besorgnis über Fremdenfeindlichkeit und Intoleranz auf nationaler Ebene auswirken. Die Ergebnisse der Dissertation sollen die politischen Entscheidungsträger über die potenziellen Nebenwirkungen der Migrationspolitik informieren und empirische Erkenntnisse liefern, die zur Verbesserung bestehender und zur besseren Gestaltung künftiger politischer Maßnahmen beitragen.

Keywords— Return intentions, Victimization, Tied movers, Xenophobic protests

Summary

This PhD thesis studies migrants' integration processes and their consequences for host countries. The first chapter of this PhD thesis introduces and motivates my four empirical PhD chapters. The second chapter investigates whether changes in the socio-political conditions in the home country affect immigrants' return intentions and labour market outcomes. The results show that immigrants interviewed after a terrorist attack in their home country are 12 percentage points more likely to wish to remain in Germany permanently. Non-EEA immigrants who enter unemployment when a terrorist event hits their home countries have a shorter unemployment duration, while EEA immigrants are more likely to change occupation, switching to larger firms with fewer low-skilled workers.

The third chapter analyzes how victimization during asylum seekers' journeys affects their economic integration. The results show that refugees who were physically victimized during their journey to Germany have a higher propensity to join the labour force, and take up low-income employment rather than investing in host country human capital, compared to non-victimized refugees. These results are consistent with the idea that experiencing physical trauma in vulnerable situations leads to a loss of future orientation or increases impatience among victimized refugees, which leads them to discount future payoffs more heavily.

The fourth chapter examines the determinants of the migration status within households (tied or lead mover) and how it affects the ethnic identity of migrant spouses. The results show that women are 42.2 percentage points more likely to be tied movers than men and that the spouse with lower human capital is 22.7 percentage points more likely to be a tied mover. Overall, tied movers in Germany are more likely to be separated and less likely to be integrated and assimilated when compared to lead or equal movers. These findings suggest that, for tied movers, the benefits of investing in the host country's culture tend not to outweigh the costs.

The fifth chapter looks at how local or spontaneously organized far-right and xenophobic demonstrations affect concerns about hostility towards foreigners and worries about immigration in other districts in Germany. Using a regression discontinuity design, we find that right-wing demonstrations lead to a substantial increase in worries about hostility towards foreigners of about 13.70% of a standard deviation. In contrast, worries about immigration are not affected by the demonstrations, indicating that the demonstrations are not successful in swaying public opinion in their favour.

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Contents

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Chapter 1

Introduction

In recent decades, the flow and stock of migrants in Europe has increased to unprecedented levels. While the freedom of movement and the internationalization of educational degrees created incentives for young and high-skilled individuals to migrate, climate change and civil conflicts in less-developed countries have increased the number of internally and globally displaced persons. This trend is also reflected in a rise in the number of asylum applications in Europe, from 397,030 in 1990 to a peak of 1,322,850 in 2015 (Eurostat). The inflow of young migrants has created opportunities to counter-balance an ageing population, tackle skill shortages and foster entrepreneurship in Western countries. However, it has also created challenges as many of these countries' systems were not prepared to receive and support immigrants to integrate into a new society. Coupled with within-country regional inequalities and economic and cultural grievances, the large inflow of asylum seekers has increased political polarization and led to an immigration backlash among some groups of natives (Braun and Koopmans, 2009; Falk, Kuhn and Zweimüller, 2011; Jäckle and König, 2018; Rodrik, 2018; Entorf and Lange, 2019; Frey, 2020; Albarosa and Elsner, 2023).

These changes have prompted several governments to rethink their migration restrictions and policies to integrate immigrants, attract high-skilled immigrants and recall emigrants back home. Following the 2015 refugee crisis, countries such as Denmark, Sweden, France and Germany, among others, introduced measures such as stricter asylum eligibility criteria, facilitated deportations, short-term bans on family reunions and employment, welfare cuts or mobility restrictions. They have also introduced compulsory language and integration courses with the aim of fostering integration. To attract both young high-skilled expatriates and skilled natives living abroad, several European countries have designed preferential tax schemes. These have been introduced in countries such as Denmark, Portugal, and Italy, among others. Similarly, to attract highly skilled immigrants from third countries, some European countries have eased entry and residence regulations for this group. This includes both the EU-Blue card as well as country-specific policies such as Germany's Skilled Immigration Act.

For the design of effective policies, it is crucial to understand the determinants of migration and return migration, as well as the effects that migration has on migrants and natives. International economic migrants are usually defined as those who—driven by their own economic opportunities—willingly move across international boundaries. This definition contrasts with individuals who have been forcefully displaced by war, persecution, or natural disasters (asylum seekers and refugees) and those who move

because of the migration decision of another family member (tied movers). While economic reasons might have also influenced the overall decision to migrate or the choice of the destination country, this is not the primary force driving the migration decision among these other groups of migrants.

In this PhD thesis, I look at different aspects of the integration process of these three immigrant groups, and how their presence affects the protest activity and attitudes of natives. The four chapters build on and aim to extend the existing literature along several dimensions. The first and second chapters improve our understanding of the causal effect of home country conditions on migrants' return intentions and labour market outcomes at destination (Chapter 2) and of the effect of traumatizing experiences along the journey on refugees' labour market integration (Chapter 3). The third chapter explores the socio-cultural integration of family migrants in a country that was not their primary choice (Chapter 4), while the fourth chapter examines the effect of the presence of foreigners on the protest activity of a particular group of natives and how these protests affect worries about xenophobia and intolerance at the national level (Chapter 5). The findings in the dissertation aim to inform policymakers on the potential side-effect of migration policies and to provide empirical evidence that help improve existing policies and better design future ones.

Most immigrants arrive in a host country with an initial idea of how long they plan to stay. However, as personal circumstances and aggregate conditions in both their home and host countries change over time, migrants update their intended duration of stay. Revisions to the intended length of stay may lead to subsequent changes in the socio-economic behaviour of migrants and, hence, can have important implications for the host and home countries and the migrants themselves. Previous studies have shown that migrants planning to stay longer are more likely to invest in the host country's human capital, higher incomes and steeper career paths (Damelang and Kosyakova, 2021; Akay, Bargain and Elsayed, 2020; Bratsberg, Ragan and Nasir, 2002; Cortes, 2004; Dustmann, 1993; Dustmann, 1999).

This phenomenon is apparent in the recent wave of Ukrainian refugees who migrated to Europe after the Russian invasion in 2022. Many Ukrainians arrived in European countries expecting that they would stay for a short period of time. However, as the intensity of the conflict increased and more cities were attacked, Ukrainians revised their perception of security at home and delayed their return plans. The longer-term plans increased the incentives to invest in the host country's language and start bureaucratic processes of recognising foreign qualifications (Brücker et al., 2023; OECD, 2023).

Given the central importance of self-reported return intention, several studies have analyzed their individual-level determinants, such as the role of family ties, educational achievement and labour market conditions (Bijwaard and Wahba, 2014; Coulon, Radu and Steinhardt, 2016; Dustmann, 1993; Dustmann, 1997; Gibson and McKenzie, 2011; Nekby, 2006). However, as in the case of the Ukrainian refugees, the socio-political context at the country of origin and destination plays a major role in migrants' intentions to return to their home countries (Dustmann and Görlach, 2016). While some recent studies have shown that natives' anti-immigration attitudes and xenophobic violence increase return intentions among migrants (Steinhardt, 2018; Coulon, Radu and Steinhardt, 2016) and worsen their socio-economic outcomes (Gould and Klor, 2016; Elsayed and De Grip, 2018; Steinhardt, 2018; Schilling and Stillman, 2021), there is little empirical evidence on how changes in the socio-political

conditions in the home country affect these outcomes.

Together with Jacopo Bassetto (former PhD student at the University of Trento, now at University of Bologna), I explore the effect of these home country shocks in a paper entitled "Home country socio-political conditions, return intentions, and labour market outcomes" (Chapter 2). In the empirical analysis, we proxy changes in socio-political conditions in the home country with the occurrence of terrorist events. We choose terror events because these are largely unpredictable from the perspective of most individuals residing in their home country and abroad. Hence, to identify the causal effect of socio-political events on return intentions and labour market outcomes, we exploit the quasi-random occurrence of terrorist events in the home country relative to the dates of the survey interviews (where we measure return intentions) and unemployment registrations (where we measure labour market outcomes) in Germany. Our results show that immigrants interviewed after a terrorist attack in their home country are 12 percentage points more likely to wish to remain in Germany permanently. This translates to a faster entry to employment after an unemployment spell among non-EEA[1] migrants and higher job selectivity among EEA migrants.

There is an explicit consensus among EU member states that refugee protection is valued and that controlling the external border to control inflows of asylum seekers is necessary (Jeannet, Heidland and Ruhs, 2021). At the same time, EU and Schengen member states attempt to discourage irregular inflows by increasing the barriers to entry at the EU's external border (e.g. EU-Turkey statement, cooperation with the Libyan coast guard, and border fences at the Eastern border, among others). These restrictive measures expose migrants who still decide to embark on the journey to the EU to potentially traumatizing experiences that compound the trauma of violence and conflict many experienced in their home country (Reitano and Tinti, 2015; Arsenijević et al., 2017; Arsenijević et al., 2018). On the Balkan Route, there are several reports by international organizations accusing authorities of using violence against both male and female asylum seekers, which is consistently characterized by physical abuse through the use of batons and by hitting and kicking (International, 2015; International, 2016; HRW, 2016; HRW, 2018a; HRW, 2018b; Tondo, 2018).[2]

While EU member states have stepped up efforts to integrate new arrivals into their labour markets and societies swiftly (European Commission, 2019), EU member states risk that restrictive border policies undermine the economic integration of refugees. By increasing the chances of experiencing traumatizing events during the journey, restrictive border policies contribute to deteriorating refugees' physical and mental health, hope and time perspective. These, in turn, may affect the labour force participation, employability and human capital accumulation of refugees in the host country.

Together with Lars Ludolph (former PhD student at LSE, now at OECD), we explore this channel in a paper entitled "Barriers to humanitarian migration, victimization and integration outcomes: Evidence from Germany" (Chapter 3) using the IAB-BAMF-SOEP refugee survey. A concern in this project, is that an unobserved ability to navigate the journey could determine the likelihood of victimization and affect integration outcomes at the destination. To address this

[1] EEA refers to the European Economic Area

[2] Several accounts exist of migrants being stripped naked in freezing temperatures and beaten by local authorities in the different Balkan countries before being pushed back (International, 2019; Oxfam, Human Rights and Association, 2017; Tondo, 2018).

concern, we review qualitative evidence on the violent acts directed at asylum seekers along the main migration routes in our sample. We conclude that these acts are largely unpredictable for migrants who navigate unknown geographical territories and that the violence is generally directed at asylum seekers, with no consideration for individual observable characteristics. In line with this suggestive evidence, we show that individual-level characteristics, such as age, education, employment and economic situation before migration are poor predictors of the likelihood of falling victim to physical crime. Hence, in our preferred empirical specification, we compare victimized to non-victimized refugees with similar pre-migration and selected post-migration characteristics, who migrated from the same country in the same year-month, took the same migration route, are part of the same arrival cohort and live in the same German federal state. Our results show that despite being less likely to invest in education in Germany, physically victimized refugees are more likely to join the labour force and enter employment in the short run when compared to non-victimized and financially victimized migrants. We show that the higher employment among the physically victimized is driven by marginal and part-time employment, a type of work characterized by a lower income level. This finding suggests that experiencing physical trauma in vulnerable situations results in a "loss of future orientation" or "impatience" among victimized refugees, which leads them to discount future payoffs more heavily.

Despite the growing literature in economics on the social and cultural integration of migrants[3], there is little evidence on how migrating for economic reasons, or family reasons may differently affect the socio-cultural adjustment of migrants. A 'lead mover' is a family migrant for whom, even if single, the individual benefits from migration compensate for the costs, and hence it most closely resembles an economic migrant. In contrast, a 'tied mover' is a family migrant who, if single, would not have chosen to migrate (Mincer, 1978). Tied movers are, therefore, less likely to move to countries where they expect to integrate well into the labour market (Junge, Munk and Poutvaara, 2014; Luthra, Platt and Salamonska, 2018). Their migration motivation is intrinsically different: they moved to keep the family together, potentially maximizing joint household income, rather than to improve their individual wages or job. Even though some tied movers choose to work in the host country, some decide not to participate in the labour market. In such cases, the benefits of adopting the host country's culture might not compensate for the costs.[4]

In my third, solo-authored PhD Chapter, entitled "Migration motivation and ethnic identity of migrant couples in Germany: tied versus lead movers", I address a gap in the literature and look at the determinants of the migration position (tied versus lead or equal movers) among couples who migrated internationally[5] and evaluate quantitatively how the migration position affects the ethnic identity of migrant spouses at the destination. Using survey data, I show that women and the spouse with higher human capital are more likely to be tied movers and that tied

[3]See for instance, Constant and Zimmermann (2008), Bisin et al. (2008), Constant, Gataullina and Zimmermann (2009), Battu and Zenou (2010), Casey and Dustmann (2010), Manning and Roy (2010), Bisin et al. (2011), Georgiadis and Manning (2011), Drydakis (2013), Facchini, Patacchini and Steinhardt (2015) and Campbell (2019)

[4]These costs can be related to spending time and effort learning a new language and creating a network with natives, among others (Epstein and Heizler, 2015; Verdier and Zenou, 2017; Wang, 2018).

[5]Most studies looking at tied and lead movers look at couples who migrated internally.

migrants in Germany are more likely to be separated and less likely to be integrated and assimilated when compared to lead or equal migrants. While being descriptive, this study helps to understand the implications of migrating as a tied spouse on post-migration outcomes beyond the labour market integration.

Following the large inflow of migrants and refugees in the past decades, anti-foreigner protests and hate crimes increased dramatically in Western countries. Xenophobic protests and hate crimes can impact not only the integration effort of immigrants but also social cohesion by affecting the social relationships between migrants and natives and between natives of different political leanings (Gould and Klor, 2016; Deole, 2019; Steinhardt, 2018; Entorf and Lange, 2019; Albarosa and Elsner, 2023).

Many natives rallying against immigration live in economically deprived areas, hold nationalistic views or are motivated by prejudice towards minorities and foreigners (Entorf and Lange, 2019; Albarosa and Elsner, 2023). Irrespective of its underlying reasons, these protests allow citizens to express their opinions and stress issues that are important to them. Through protests, participants can 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; Larreboure and Gonzalez, 2021; Lagios, Méon and Tojerow, 2022). Yet, if turned disruptive or poorly organized, protests may reduce support for their cause (Wasow, 2020; Eady, Hjorth and Dinesen, 2021).

To understand the role xenophobic 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 in the district where the protests have occurred (e.g., Madestam et al., 2013; Enos, Kaufman and Sands, 2019; Klein Teeselink and Melios, 2021; Wasow, 2020; Larreboure and Gonzalez, 2021).[6] However, can local demonstrations affect the attitudes and party preferences of voters in other districts of a country?

Together with Christopher Prömel (PhD student at Freie Universität Berlin), in a paper entitled "Local far-right demonstrations and nationwide public attitudes", we look at the effect of local or spontaneously organized large right-wing xenophobic demonstrations in an administrative district on the attitudes and political preferences of respondents being interviewed in the rest of Germany (Chapter 5). We concentrate on spontaneous or locally organized demonstrations because it is unlikely that 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. Using a regression discontinuity design, we compare the attitudes of individuals interviewed in the days immediately before and after a large right-wing xenophobic demonstration. Our results show that right-wing demonstrations lead to a substantial increase in worries about hostility towards foreigners but do not affect worries about immigration. We also show that individuals become more politically active in response to protests, which mainly benefits left-wing parties. Our results indicate that the demonstrations are not successful in swaying public opinion in their favour.

The empirical chapters in this thesis have one common theme and institutional framework: the integration processes of migrants and refugees in Germany. Germany is an ideal country for studying the labour market and cultural integration of migrants as well as the impact of migrants in German society. From the guest workers' programs

[6]Four exceptions include Eady, Hjorth and Dinesen (2021), Reny and Newman (2021), Lagios, Méon and Tojerow (2022) and Brox and Krieger (2021)

in the early 1960s to the recent refugee crisis, Germany has received immigrants from all continents, contributing to the creation of a multicultural society. Germany collects and gives access to incredibly rich data, which allows researchers to answer different questions concerning the integration of migrants and the impact of migrants on natives. Since 1984 that Germany runs a yearly household survey, the German Socio- Economic Panel (GSOEP), which collects data on attitudes, return intentions, and cultural preferences, among others. This survey has allowed researchers to examine subjective outcomes unavailable in administrative datasets. Return intentions, worries about hostility towards foreigners and worries about immigra- tion are essential outcomes for the analysis in my first and fourth Phd chapters.

Two extensions to the GSOEP, the IAB-SOEP migration sample and the IAB-BAMF-SOEP refugee survey, have allowed me to identify victims of violence along the journey among refugees and tied movers among migrant couples. These variables are rare in most surveys and hence provide a valuable resource.

Overall, my PhD thesis explores different aspects of the integration processes of migrants and refugees in Germany. The findings in this dissertation aim to improve the knowledge in the field of migration and to inform policymakers on the potential side-effect of migration policies.

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Chapter 2

Home country socio-political conditions, return intentions, and labour market outcomes[1]

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Abstract: *Migration is often temporary, and the intended length of stay in the host country is an important determinant of immigrants' labour market behaviour, human capital investment, and socio-economic integration. In this paper, we investigate whether changes in the socio-political conditions in the home country affect immigrants' return intentions and labour market outcomes. We combine administrative and survey data with precise information on terrorist attacks worldwide. Our identification strategy exploits the quasi-random occurrence of terrorist attacks in the home country relative to the dates of the survey interviews and unemployment registrations in Germany. We show that immigrants interviewed after a terrorist attack in their home country are 12 percentage points more likely to wish to remain in Germany permanently. Economic theory tells us that revisions to the intended length of stay will lead to subsequent changes in the socioeconomic behaviour of migrants. Our second key result confirms this hypothesis by showing that non-EEA or non-Schengen area immigrants who enter unemployment when a terrorist event hits their home countries have a shorter unemployment duration than immigrants who enter unemployment in quiet times. EEA or Schengen area immigrants entering unemployment in the same month of a terrorist event in their home country are not more likely to re-enter employment faster but are more likely to change occupation and industry and to change to larger firms with fewer low-skilled workers.*

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2.1 Introduction

Many migration spells are temporary (OECD, 2019; OECD, 2008; Dustmann and Görlach, 2016)[2] as people who reside outside of their country of origin ultimately return. Although immigrants arrive in the host country with a planned intended duration of stay, most end up updating their expectations as a result of changes in their personal circumstances and aggregate conditions in both their home and host countries. Revisions to the intended length of stay may lead to subsequent changes in the socio-economic behaviour of migrants and hence can have important implications for the host and home countries and the migrants themselves. Previous research has shown that migrants who plan to stay longer are more likely to invest in the host country's human capital, which can lead to steeper earnings and career paths (Damelang and Kosyakova, 2021; Akay, Bargain and Elsayed, 2020; Bratsberg, Ragan and Nasir, 2002; Cortes, 2004; Dustmann, 1993; Dustmann, 1999).

This is apparent in the recent wave of Ukrainian refugees, following the 2022 Russian invasion, who migrate to European countries hoping to make it a short stay. However, with changes in the intensity of conflict in Ukraine and the perspective of a prolonged war, refugees regularly revise their return plans.[3] This initial short-term perspective and high uncertainty lower the incentives to invest in German-specific skills or start lengthy processes for the recognition of foreign qualifications, which are often associated with long-term integration (OECD, 2023).

Given the importance of temporary migrations, several studies have analyzed their individual determinants, including education, length of residency, and family ties (Bijwaard and Wahba, 2014; Coulon, Radu and Steinhardt, 2016; Dustmann, 1993; Dustmann, 1997; Gibson and McKenzie, 2011; Nekby, 2006). However, the socio-political context at the country of origin and destination can also act as push and pull factors that affect migrants' intentions to return migrate (Dustmann and Görlach, 2016). While some recent studies have looked at how changes in natives' attitudes towards immigrants and terror attacks by foreigners in the host country increase return intentions among migrants (Steinhardt, 2018; Coulon, Radu and Steinhardt, 2016) and worsen their socio-economic outcomes (Gould and Klor, 2016; Elsayed and De Grip, 2018; Steinhardt, 2018; Schilling and Stillman, 2021), there is little empirical evidence on how changes in the socio-political conditions in the home country affect these outcomes.

In this study, we investigate whether negative socio-political shocks in the home country affect return intentions and, in turn, the economic behaviour of immigrants in Germany. The underlying mechanism is that negative socio-political events in the home country affect the perception of security and hence work as shocks to migrants' location preferences by increasing the attractiveness of the destination country relative to the home country. Our results show that a negative shock to the socio-political conditions in the home country increases migrants' intention to remain in Germany permanently, which translates into lower unemployment duration among unemployed immigrants.[4]

[2]According to the OECD (2008) report on migration, around 20 to 50 per cent of immigrants in OECD countries leave the host country after five years after arrival

[3]According to a survey by Panchenko and Poutvaara (2022) in October 2022 asking "What are your thoughts on returning to Ukraine?" to a sample of Ukrainians refugees in Germany, around 49 per cent answered planned to return soon or when they feel safe in Ukraine, 30 per cent do not know and only 22 per cent said they would prefer o live outside of Ukraine.

[4]While return plans can change over the course of an individual migration spell and may

In the empirical analysis, we proxy changes in socio-political conditions in the home country with the occurrence of terrorist attacks. We choose terror events because these are largely unpredictable from the perspective of most individuals residing in their home country and abroad. Data on terrorist attacks come from the Global Terror Database (GTD), a large dataset containing information on almost 200,000 terrorist events worldwide from 1970 to 2018. Events are recorded daily, and the geographical location where the events took place is highly precise. Additionally, the data set includes events' characteristics, such as the number of killed and wounded, which allows us to investigate the effect of both occurrence and intensity of terrorist events.

Contrary to previous studies that consider the absolute number of casualties from terror events (see e.g., Akay, Bargain and Elsayed, 2020; Keita and Schewe, 2021; Sønderskov et al., 2021), we introduce a relative measure of terror that takes into account country-specific periods of the high and low incidence of terror events. This measure is based on the idea that individuals coming from countries with a high number of terrorist events in the recent past have a different reference point when compared to individuals coming from countries that have very rare terrorist attacks.

In the first part of the analysis, we combine the GTD data with the German Socio-Economic Panel (GSOEP) and investigate the effect of terrorist events in the home country on migrants' intention to remain in Germany. The GSOEP is a large-scale survey representative of the German population. It has been run yearly since 1984 and includes a wide variety of individual-level information. Crucial for our analysis, it also collects information on nationality, the year of migration, and the intention to stay permanently in Germany.

The identification strategy in this part relies on the quasi-random occurrence of the date of the event at origin relative to the timing of the GSOEP interviews and the characteristics of the respondent being interviewed. Our main results show that migrants interviewed within 90 days after terrorist events are 12.0 percentage points more likely to declare they want to stay in Germany permanently. The effect is particularly strong among immigrants who were less integrated before the terrorist event (e.g., scarce German knowledge) and have close family members in the home country. Risk-averse individuals are also more likely to revise their return intentions in the follow-up of a relevant terror event, while there is no difference between employed and unemployed individuals.

As one of the crucial identifying assumptions is that the occurrence of terrorist events in the home countries did not interfere with the implementation of the survey, we provide a series of balance tests as evidence in favour of our assumption. We also show that specific countries, survey years, or bandwidths around the events do not drive the main results. To ensure that we are not capturing some statistical artefact in the data, we provide two pieces of evidence. First, we assign random dates to the terror events (e.g., placebo events) and show that there are no effects on the intention to stay. Second, we look at the effect of terror events in the home country on placebo outcomes, such as worries about crime and the environment in Germany, and find no significant effect.

In the second part of the analysis, we look at the effect of terrorism on measurable labour market outcomes. A difficulty with this analysis is that if we focus on the most

deviate from the actual date of the return (Dustmann and Görlach, 2016; Chabé-Ferret, Machado and Wahba, 2018), in this study, we are interested in analyzing the effect on contemporaneous re-employment decisions which are based on current return plans.

common outcomes, such as investment in human capital, earnings, and career profiles, it is unlikely that we will see an immediate change in response to a shock to return intentions. The completion of an educational degree[5] or a change in the earnings path and career profile take time to materialize. Hence, it is empirically difficult to disentangle the true effect of terror events on these economic indicators. A measurable indicator of economic behaviour that reacts quickly to individual circumstances is job search activity and reservation wages among unemployed individuals. Since these two measures affect the length of unemployment, we take time to employment as our preferred economic indicator.

We argue that terror attacks in the home country can positively impact job search activity and negatively affect reservation wages among unemployed migrants in Germany. However, because terror events can affect these variables jointly, they will have ambiguous consequences for unemployment duration and accepted wages. First, if migrants benchmark their reservation wage in the host country with the wage they could get in the home country by lowering expected utility[6] in the home country terror attacks could lead to lower reservation wages at the destination. Because of lower reservation wages, we would expect terror events in the home country to lead to shorter unemployment duration and lower accepted wages. Second, terror events might create a sense of "fear", for instance, driven by the idea of being potentially obliged to return to the country of origin due to unsustainable economic conditions in the host country. This sense of "fear" might also operate in a way such that migrants feel more pressured and become more committed to ensuring a good career in Germany (e.g., they now intend to stay longer). The "fear" effect is expected to increase migrants' job search efforts while potentially making them more selective with respect to the type of career a job ensures. Hence, it can have an ambiguous effect on unemployment duration and a positive effect on accepted wages.

To accurately measure time to employment and the wages in the first job after unemployment, we rely on German administrative data (IEB), using the 10% of the immigrant population in the social security records between 2000 and 2018. The empirical strategy in this section is slightly different: we compare the labour market outcomes of immigrants entering unemployment when terrorist events occur in their home countries to those of immigrants that entered unemployment in times of stable home country conditions.

Our results show that non-EEA/Schengen[7] immigrants entering unemployment in the same month of a terrorist event in their home country re-enter employment 22 days earlier than non-EEA immigrants entering unemployment in times of stable home country conditions. On the other hand, EEA or Schengen area immigrants entering unemployment in the same month of a relevant terrorist event in their home country are not more likely to re-enter employment faster but are more likely to change occupation and industry and to change to larger firms with fewer low-skilled workers. This could signal that EEA or Schengen area immigrants become more committed to pursuing a long-term career in Germany, while non-EEA/Schengen immigrants are bound by visa or monetary constraints and hence re-enter employment

[5]Investments in human capital observed in the GSOEP, such as enrolling in further education or acquiring a university degree, are measured once individuals have started to attend them rather than when the decision to take them was taken - and there can be a considerable lag between the two.

[6]Terror events affect the perception of security in the home country

[7]EEA stands for European Economic Area

faster. These results are robust to placebo treatment assignments and alternative definitions of terrorism.

While this change in economic behaviour benefits the host country in the short term, it is unclear what are the long-run consequences of such a decision. These findings have important implications for sending countries, in parallel with other incentives such as tax incentives, ensuring socio-political stability might work well as a mechanism to attract emigrants back. Economic conditions go hand in hand with security conditions. This is relevant not only for countries experiencing intense internal conflict but more broadly to all sending countries experiencing large terror events.

We contribute to the literature in three ways. First, we provide empirical evidence on the link between return intentions and socio-political conditions in the home country. Given the importance of temporary migrations, several studies have analyzed the individual determinants of return intentions (see e.g., Bijwaard and Wahba, 2014; Dustmann and Görlach, 2016). Fewer studies studies look at the country-level determinants of return migration. Previous literature has shown that economic conditions in the home country matter for the well-being of immigrants abroad (Akay, Bargain and Zimmermann, 2017) and that they may determine both migration flows and the size of remittances (Gröger, 2021). However, the link between the home country's socio-political conditions and return intentions has only been theoretically hypothesized (Dustmann and Görlach, 2016). While Steinhardt (2018) empirically shows that xenophobic violence in Germany affects migrants' return intentions, we are the first to show that violence in the home country also affects return decisions.

Second, we contribute to the literature on the effects of external shocks on the labour market integration of immigrants. Previous studies have shown that terrorism in the host country affects immigrants' integration. For example, Gould and Klor (2016) shows that the 9/11 attacks had long-lasting effects on the integration of Muslim immigrants, while Brodeur and Wright (2019) shows that the same events also reduced asylum approval rates. Closest to our paper is Steinhardt (2018) which finds that xenophobic violence reduces immigrants' investments in language skills. We show that terrorist events at home do affect not only return intentions but also the labour market behaviour of immigrants. While we cannot directly link the effect of terror on return intentions to its effect on immigrants' labour market behaviour, we show that terror events that create a plausible shock to return intentions also have an effect on the search behaviour of immigrants. We also rule out alternative channels, such as the effect of terror events on remittances.

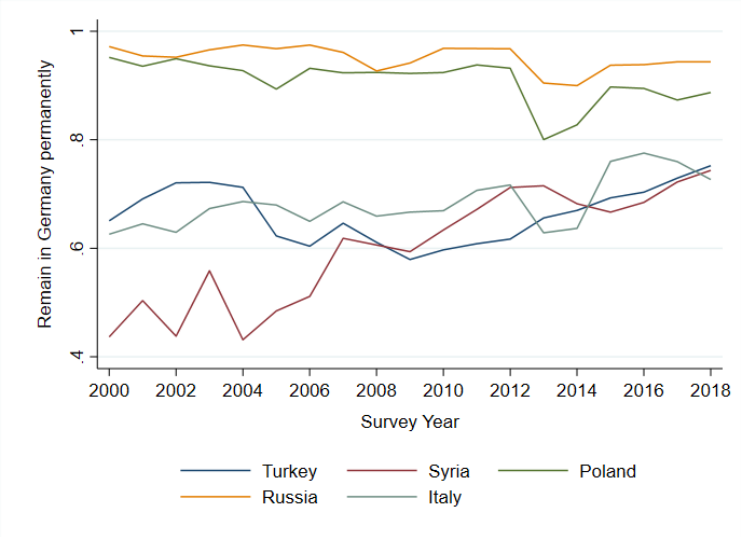
Third, despite using terrorism as a proxy for socio-political turmoil and violence in the home country, our paper is closely related to the literature on terrorism and its effect on well-being and mental health. A number of studies find that terrorism in the location of residency affects political opinions and voting behaviours (Peri, Rees and Smith, 2020), reduces the well-being of individuals (Akay, Bargain and Elsayed, 2020; Clark, Doyle and Stancanelli, 2020), and of immigrants from affected countries in particular (Sønderskov et al., 2021; Keita and Schewe, 2021). Using comparable research designs, we show that terrorism in the home country affects return intentions and labour market outcomes.

The rest of the paper is organized as follows. Section 2.2 describes the data. Section analyses terror and return intentions and Section 2.4 analyses terror and labor market behavior. Section 2.5 concludes.

2.2 Data

German Socio-Economic Panel: To analyse the impact of terror events on the intended length of stay in Germany, we use the full data set from the German Socio-Economic Panel (GSOEP) from 2000 to 2018. The GSOEP is a large-scale yearly household survey that is representative of the German population[8]. The dataset contains individual and family information on various topics, from education to work-life, to consumption, to more behavioural and attitudinal characteristics. Crucial to our analysis, a large number of immigrants are interviewed each year. If they have a migration background, respondents are asked migration-specific questions, such as their country of origin, the presence of family abroad, their German knowledge, and return intentions. The GSOEP has been widely used to study immigrants in German society, and specifically to study return migration intentions (see e.g. Dustmann and Görlach, 2016; Bauer and Sinning, 2011).

Figure 2.1 plots the share of immigrants that intend to remain in Germany permanently for the largest nationality groups in the GSOEP. While Eastern European immigrants (some of which are ethnic Germans) tend to have stable return migration intentions, for other nationality groups, the share of immigrants who want to settle permanently has increased over time[9].



Notes: Figure 2.1 displays the share of immigrants that intend to remain in Germany permanently. Shares are computed for each survey year (from 1984 to 2019) only for the 5 largest nationality groups.
Source: GSOEP

Figure 2.1: Remain in Germany permanently, main groups

In Table 2.B.1 in appendix 2.B.2, we show descriptive statistics of the migrant population in the GSOEP. A very high share of the migrants in Germany over the

[8]For a complete description of the data, please refer to Goebel et al. (2019)

[9]Part of the increase intentions to stay may be due to compositional changes and panel attrition. In Appendix 2.A.1, we show the share of migrants in the GSOEP over time and discuss the different migration waves to Germany in more detail.

period under analysis have only lower secondary education or below. While the mean of the full-time employed over the 2000-2018 period is only 0.34, these results are driven by the large inflows of refugees Germany has hosted over the years and by the low labour force participation among female migrants. Refugee employment over the first two to three years after migration is relatively low, but it then catches up with the rest of the migrant population. Finally, most migrants want to remain in Germany for many years.

Social Security Records: To analyse the effect of terror events on labour market outcomes, we rely on the social security records, *Integrated Employment Biographies* (IEB), for a random draw of 10% of the full population of immigrants in the German labor market. The Institute of Employment Research (IAB) of the German Federal Employment Agency provides the data.[10] The dataset includes detailed daily administrative longitudinal information on nationality, occupation, educational background, industry, employment status, and earnings records of all individuals subject to social security in Germany. Crucial for our empirical strategy, we have information on the precise date when immigrants enter unemployment, their occupation, and their wage. Given that the number of unemployed individuals in the GSOEP is relatively low and the questions regarding job search activity and participation in unemployment programs are missing for a large share of the unemployed, IEB administrative data are better suited for this part of the analysis.

Global Terror Database: The Global Terrorism Database (GTD) is an open-source database that provides detailed information on terrorist incidents worldwide (LaFree and Dugan, 2007). Data are collected daily using both human and machine intelligence.[11] The GTD team has developed a proprietary data management system that allows analysts to identify unique attacks, record the details of each event (e.g., date, location, the number killed), and update records for previously recorded events as new information becomes available (The Global Terrorism Database, 2019).

In Figure 2.B.1 in appendix 2.B.1, we present descriptive statistics on the terror events from the GTD database. The left-hand-side panels of figure 2.B.1 show monthly trends in terror events between 2000 and 2018 for the five countries of origin with the largest immigrant population in Germany: Turkey, Syria, Russia, Poland, and Kazakhstan. The number of events strongly varies over time and across countries. For example, Syria experienced a spike in terror events in the last five years, while these are more evenly distributed to other countries. Additionally, while Poland and Kazakhstan have only a few scattered events, Turkey has experienced frequent events from the 2000s up until nowadays.

Contrary to previous papers that consider the absolute number of casualties (see, e.g., Akay, Bargain and Elsayed, 2020; Keita and Schewe, 2021; Sønderskov et al., 2021), we introduce a relative measure of terror that takes into account country-specific periods of the high and low incidence of terror events. This measure is based on the idea that individuals coming from countries with a high number of

[10]For the description of a 2% random sample from the IEB, the *Sample of Integrated labor Market Biographies* (SIAB), see (Antoni et al., 2019).

[11]First, millions of articles from newspapers worldwide are processed daily to find and document all terrorist events. Natural language processing, named entity extraction, and machine learning models are used to identify and organize news articles that include information about terrorist attacks.

terrorist events in the recent past have a different reference point when compared to individuals coming from countries which have very rare terrorist attacks[12]. One terrorist event in a country such as France in 2016 is likely to create a bigger shock to the perception of security and a larger reaction among French migrants abroad than one terrorist event in Syria, for instance, which was experiencing a period of intense turmoil in 2016.

One difficulty with this approach is to know what individuals consider to be the "recent past." We consider different alternatives: if, in a given month, there was at least one more terror event than the past country-specific three-year average, four-year average, and five-year average. Our results do not change greatly with either definition and hence for most of our analysis, we will consider the past three-year average as the relevant "recent past."[13] We define one month as the treatment month (t = 0) if there is at least one more terror event in that month than the past three-year average number of monthly terror events. For our main results, we will also consider the intensity of these terror events, e.g., how many people were killed.

2.3 Socio-political conditions in the home country and return intentions

In this part of the analysis, we test whether a negative socio-political (e.g., a terrorist event) has a positive effect on immigrants' intention to stay in Germany. We hypothesize the following mechanism: a negative socio-political event in the home country works as a shock to immigrants' location preferences, increasing the attractiveness of the host country relative to the home country and therefore increasing the desire to remain permanently in the host country or to delay the timing of return migration.

2.3.1 Empirical Strategy

To estimate the effects of terrorist attacks on the intentions to remain, we exploit the variation induced by the timing of interviews in the SOEP and the timing of terror events in the home country[14]. We estimate the following model:

$$I_{i,o,y,m,f} = \sum_{t=-P}^{T} \beta_t Time_{t,o,y} + \delta X_{i,y} + \gamma_o + \eta_y + \mu_{o,y} + \phi_m + \rho_{m,y} + \lambda_f + \epsilon_{i,o,y,m,f} \quad (2.1)$$

[12]Individuals coming from countries with a high number of terrorist events might be more accustomed to this type of violence and hence one isolated terror attack might have little impact on their intentions to stay

[13]The right-hand side panels of figure 2.B.1 in appendix 2.B.1 shows the relevant events between 2000 and 2018 for the five countries of origin with the largest immigrant population in Germany: Turkey, Syria, Russia, Poland, and Kazakhstan. On the left-hand side are the graphs with all the terror events between 2000 and 2018 for the same set of countries.

[14]This design has been recently used to study also the effect of terrorism on well-being (Akay, Bargain and Elsayed, 2020; Clark, Doyle and Stancanelli, 2020) and political opinions (Peri, Rees and Smith, 2020), as well as the effect of football victories in international competitions on national identity sentiments (Depetris-Chauvin, Durante and Campante, 2020)

where $I_{i,o,y,t}$ measures the return intentions of individual i from country of origin o, interviewed in year y and month m and residing in federal state f. $Time_{t,o,y}$'s are dummies identifying periods around the event where t denotes weeks since the relevant terror event (e.g., $t = -2$ for those interviewed 2 weeks before the event). The coefficients $\beta_1, ..., \beta_T$ identify dynamic treatment effects, t= 0 is the baseline omitted period. γ_o are country-of-origin fixed effects, η_y are interview year fixed effects, ϕ_m are interview month fixed effects, $\mu_{o,y}$ are country of origin times year fixed effects, $\rho_{m,y}$ are interview month times year fixed effects and λ_f are federal state of residence fixed effects. $\delta X_{i,y}$ is a set of individual controls that includes age, gender years since migration, years since migration squared, marital status, children, and educational achievement.

To precisely estimate the effects of terror events, in our main specification, we include only immigrants interviewed within a 90 days bandwidth from the occurrence of the relevant terror event. In section 2.3.3, we show the results using smaller bandwidths, such as 30 and 60 days. Within each of these bandwidths, we select "isolated" relevant terror events. For instance, when using a 90 days bandwidth, we consider a relevant terror event to be isolated if individuals interviewed within the 90 days prior to the focal terror event have not experienced any relevant terror event in the past 90 days, and individuals interviewed within the 90 days after the focal terror event have not experienced any other relevant terror event. [15] This procedure ensures that the control group is not contaminated by any terror event within the relevant bandwidth. Table 2.B.2 in appendix 2.B.2 shows the number of relevant and isolated terror events per country, as well as the mean number of monthly terror attacks per relevant and isolated terror event.

The inclusion of country-of-origin times year fixed effects allows us to compare outcomes for immigrants from the same country of origin that are interviewed in the same year right before or right after the relevant and isolated terror event. The estimated coefficient is an average of the effects across countries of origin and terrorist events. The country-of-origin times month of interview fixed effects allows us to take into account seasonality in return intentions. Standard errors are clustered at the country-year-month level.
To summarize the average treatment effect over all periods, we also estimate:

$$I_{i,o,y,m,f} = \beta PostTerror_{i,o,y,m} + \delta X_{i,y} + \gamma_o + \eta_y + \mu_{o,y} + \phi_m + \rho_{m,y} + \lambda_f + \epsilon_{i,o,y,m,f} \tag{2.2}$$

where time dummies are substituted with the indicator $Post\text{-}Terror_{i,o,y,m}$, which takes the value of 1 if respondent i from the country of origin o is interviewed within 90 days after a relevant terror event, and 0 if a respondent is interviewed within 90 days before that same event.

Our identification strategy relies on the quasi-random occurrence of terror events relative to the precise time immigrants are interviewed. Therefore, our identifying assumption is that the occurrence of terror events in the home countries did not interfere with the implementation of the survey. While it is unlikely that the organization of the survey changes in response to terror events, it may happen that immigrants who are more attached to their home countries refuse to be interviewed after the event. This non-random selection may bias our results upward on the intentions to remain in Germany. To test our assumption, we first plot in Figure

[15] This procedure is similar to Graeber and Schikora (2021)

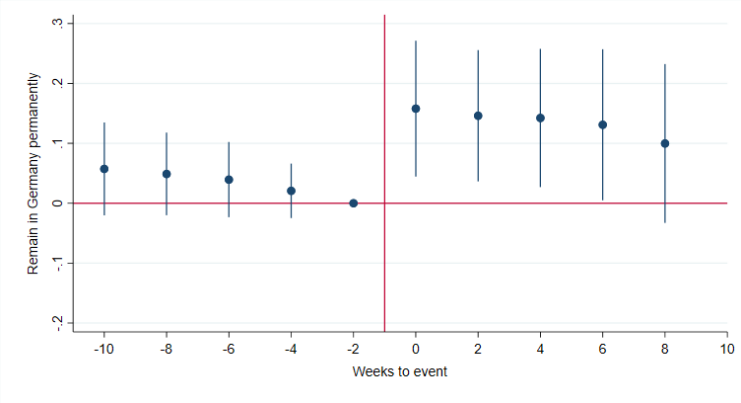
2.B.3 in appendix 2.B.1 the share of interviews around each country-specific event that we use in our main estimations[16]. Figure 2.B.3 shows that there is no evidence of a correlation between the implementation of the survey and the occurrence of events. As a second test, we show that the characteristics of the respondents do not depend on whether they were interviewed before or after a terror event. We regress each individual characteristic on the treatment status (i.e., interviewed after a terrorist event in the home country) and include year times country of origin fixed effects, year times month of interview fixed effects, and federal state of residency fixed effects. The results are presented in Table 2.B.3 in the appendix. For all included characteristics, there seems to be no difference between the treatment and control groups. In Figure 2.B.2 in appendix 2.B.1, we regress the treatment indicator on the full set of individual characteristics and find that none of these characteristics significantly predicts the treatment status. Nevertheless, we show our main results with and without the full set of individual characteristics.

2.3.2 Main Results

In this section, we present our main results for the effect of terrorism on intentions to remain in Germany. We first show graphical evidence of how intentions to remain in Germany evolved in the months around terror events using a 90-day bandwidth and considering an event to be relevant if the number of events in a given month is higher than the past three-year average. Figure 2.2 plots the event study coefficients, using the month before the event as a baseline. The plot shows that the coefficients for individuals interviewed before terror events are not statistically different from individuals interviewed in the month before the event, while coefficients are positive and statistically significant for immigrants interviewed after the event. Moreover, the plots show that the increase in intentions to remain lasts up to the fifth month after the attack.

In Table 2.1, we report the results based on Equation 2.2 using a bandwidth of 90 days around the event, and considering an event to be relevant if the number of events in a given month is higher than the past three-year average. Column (1) uses only the baseline fixed effects year times country of origin fixed effects, year times month of interview fixed effects, and state of residency fixed effects; columns (2) adds gender, age, years since migration, and years since migration squared to the controls in (1); column (3) adds marital status and the presence of children to the controls in (2); and column (4) adds educational achievement to the controls in (3). We estimate that a terror event in the home country leads to a 12.2 to 12.5 percentage point increase in the intention to remain in Germany. This corresponds to an increase of 10 per cent relative to the mean value of the outcome variable (0.81). Overall, the results suggest that the occurrence of terror events in the home country positively affects the intention to remain in the host country - Germany - permanently. In Section 2.4, we test whether changes in the intentions to remain in Germany affect the integration of immigrants in the labour market.

[16]For a given country-specific event, we consider: i) the total number of interviews in the 90 days before and after the event and; ii) the number of interviews at 90, 60, 30 days before and after the event and at 0. The ratio in the x-axis represents the number of interviews at each of these points relative to the total number of interviews, e.g. ii) / (i).



Notes: Figure 2.2 displays the event study plot from the estimation of Equation 2.1, where the outcome is "Remain permanently in Germany".The regression considers a 90 days bandwidth. Bars identify 95% confidence intervals.

Figure 2.2: Event study: intention to remain in Germany and terror events

	Higher than average of last 3 years			
	(1)	(2)	(3)	(4)
Post-Terror	0.122***	0.122***	0.125***	0.123***
	(0.030)	(0.030)	(0.030)	(0.030)
Observations	6604	6604	6604	6604
Origin country x Year FE	Yes	Yes	Yes	Yes
Month FE,	Yes	Yes	Yes	Yes
State of Residency FE	Yes	Yes	Yes	Yes
Indiv. Controls	No	Some	Some	Yes

Standard Errors in parenthesis clustered at the Country x Year x Month level, *p<.1; **p<.05; ***p<.01
Notes: Table 2.1 displays the coefficients from the estimation of Equation 2.2 where the outcome is "Remain permanently in Germany". All results consider a 90 days bandwidth. FE refers to fixed effects. Individual controls include age, gender, years since migration and its square, marital status, educational achievement, and children.

Table 2.1: Terror events and intentions to remain in Germany

In table 2.2, we explore whether differences in the intensity of the terror events matter for the intention to remain permanently in Germany. We interact the Post-Terror variable in Equation 2.2 with a dummy variable that equals 0 if no or less than k individuals were killed and equals 1 if k or more individuals were killed for k= 10, 30, 50. The results show that the effect of terror on return intentions gets stronger as the number of people killed increases.

	Higher than average of last 3 years		
	k=10	k=30	k=50
	(1)	(2)	(3)
Post-Terror	0.130***	0.111***	0.110***
	(0.032)	(0.032)	(0.032)
Post-Terror × (k or > than killed)	0.096**	0.197***	0.223***
	(0.039)	(0.057)	(0.061)
Observations	6604	6604	6604
Origin country x Year FE	Yes	Yes	Yes
Month FE,	Yes	Yes	Yes
State of Residency FE	Yes	Yes	Yes
Indiv. Controls	Yes	Yes	Yes

Standard Errors in parenthesis clustered at the Country x Year x Month level, *p<.1; **p<.05; ***p<.01
Notes: Table 2.2 displays the coefficients from the estimation of Equation 2.2 interacted with a dummy variable that equals 0 if no or less than k individuals were killed and equals 1 if k or more individuals were killed. k denotes the number of individuals killed. All results use a 90 days bandwidth. FE refers to fixed effects. Individual controls include age, gender, years since migration and its square, marital status, educational achievement, and children.

Table 2.2: Intensity of terror events and intentions to remain in Germany

One interesting question is if the response to terror events in the home country is the same for individuals from countries with a durable conflict and those from politically stable countries. Table 2.B.2 in the appendix shows that there is a significant variation in the mean number of terror attacks in a given month for it to be considered a month with a relevant and isolated event. Note that this table does not necessarily include all time periods with relevant events, but only those that occurred in isolated periods as explained in section 2.3.1. We can see that while in Belgium or Norway, 2 terror attacks in one month are enough for this month to be considered relevant, in Colombia, 17 attacks are necessary, and in Iraq, 285 attacks.

To study this question in more detail, we use the Political Stability Index from the World Bank[17] to rank countries based on their political stability. We consider the ranking in the year before the relevant and isolated event occurred and the mean ranking of the three years prior to the relevant and isolated event.[18] Based on these two measure countries of origin are categorized into: i) low political stability if the ranking below or equal to 25; ii) mid political stability if the ranking higher than 25 and below or equal to 75; and iii) high political stability if the ranking above 75.[19] The results are shown in table 2.3 column (1) and (2). The results exhibit no particular difference between countries with different political stability rankings. For instance, in column (2), individuals interviewed after a terror event coming from a country with low political stability are 13.4 percentage points more likely to wish to

[17]The Political Stability and Absence of Violence/Terrorism Index is built by the World Bank (Worldwide Governance Indicators) using information from different sources. The index measures perceptions of the likelihood of political instability and/or politically-motivated violence, including terrorism.

[18]This is to be consistent with the individual reference point used to consider an event as relevant: if, in a given month, there was at least one more terror event than the past country-specific three-year average

[19]The distribution of the index in our particular sample is displayed in table 2.B.4 in the appendix

remain in Germany permanently than individuals interviewed before the terror event. This compares to 11.5 percentage points and 13.5 percentage points for individuals interviewed after a terror event coming from a country with middle and high political stability, respectively.

As a second approach, we take the mean monthly number of terror attacks in the past three years used to classify terror events as relevant events. To compare with the previous analysis, we also use the mean monthly number of terror attacks in the past year. We categorize countries into: i) low stability if the mean monthly number of terror events is equal or above 12; ii) mid stability if the mean monthly number of terror events is above 0 and below or equal to 12; and iii) high stability if the mean monthly number of terror events is equal to 0.[20]. Using this approach, the effect of a relevant terror event on the intentions to remain permanently in Germany seems stronger for those coming from countries with low stability. This includes Algeria, Colombia, Thailand and Iraq, which experienced, on average 15, 17, 40 and 285 terror attacks in one single month, respectively. Nevertheless, the differences across groups are not stark.

	Political stability index		Mean monthly terror	
	Previous year	Mean prev. 3 years	Previous year	Mean prev. 3 years
	(1)	(2)	(3)	(4)
Post-Terror × Pol. Stab. <=25	0.146***	0.134***		
	(0.040)	(0.039)		
Post-Terror × Pol. Stab.]25-75]	0.109***	0.115***		
	(0.036)	(0.036)		
Post-Terror × Pol. Stab. > 75	0.136***	0.135***		
	(0.044)	(0.042)		
Post-Terror × >12 attacks month			0.190***	0.186***
			(0.045)	(0.044)
Post-Terror ×]0-12] attacks month			0.115***	0.106***
			(0.036)	(0.033)
Post-Terror × 0 attacks month			0.110**	0.138***
			(0.053)	(0.049)
Observations	6604	6604	6604	6604
Origin country x Year FE	Yes	Yes	Yes	Yes
Month FE,	Yes	Yes	Yes	Yes
State of Residency FE	Yes	Yes	Yes	Yes
Indiv. Controls	Yes	Yes	Yes	Yes

Standard Errors in parenthesis clustered at the Country x Year x Month level, *p<.1; **p<.05; ***p<.01
Notes: Table 2.3 displays the coefficients from the estimation of Equation 2.2 interacted with a dummy variable proxing for political stability. All results use a 90 days bandwidth. FE refers to fixed effects. Individual controls include age, gender, years since migration and its square, marital status, educational achievement, and children.

Table 2.3: Overall political stability, terror events and intentions to remain in Germany

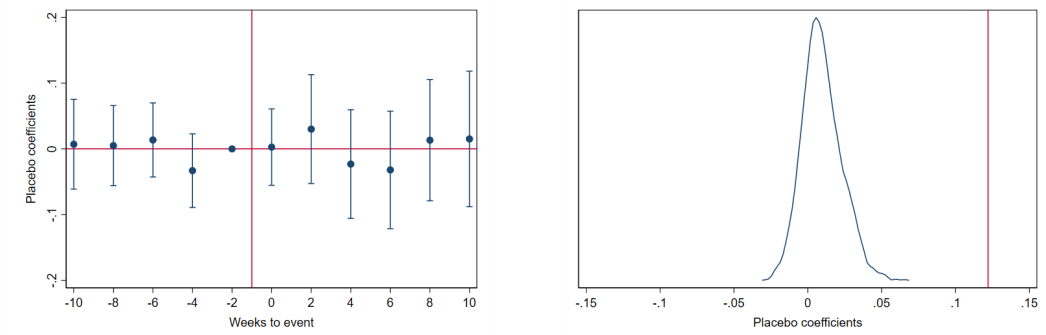
[20]The choice of cutoffs is fairly arbitrary, we chose 12 because it means that in one single month, there were more terror attacks than in the scenario of 1 event per month in an entire year. We considered different marginal cutoffs, and the results do not change greatly. The index distribution in our particular sample is displayed in table 2.B.4 in the appendix

2.3.3 Placebo Tests and Robustness Checks

In the previous section, we showed that terror events in the home countries positively impact the intentions to remain in Germany. In this section, we test the stability of our results using both placebo tests and robustness checks.

Changing bandwidth or reference point We start by testing whether the main results are sensitive to the bandwidth around the event or the average above which we consider a terror event to be relevant. In table 2.B.5 in appendix 2.B.2, we display the estimated coefficients when reducing the bandwidth from 90 days (i.e., the baseline bandwidth) to 60 days and 30 days around the terror event and when considering if, in a given month, there was at least one more terror event than the past country-specific three-year average (i.e., the baseline average), four-year average, or five-year average. The estimated coefficients remain positive and significant, and we see that the closer we get to the terror event, the larger the effect on the intention to remain permanently.

Placebo terror event date As a placebo test, for each country of origin, we assign a random date to each relevant terror event and estimate Equation 2.1. The event study resulting from this exercise is displayed in figure 2.3a and shows that there is no effect of the placebo terror events on the intention to remain in Germany permanently. We replicate this procedure 300 times and estimate Equation 2.2 to obtain the coefficients of the placebo terror events. The distribution of the coefficients is shown in figure 2.3b and is concentrated around zero, well of the 0.12 we estimated in table 2.1 using the true date of the relevant terror events.



(a) Event study, 90 days Bandwidth

(b) Coefficient distribution, 90 days Bandwidth

Notes: Panel 2.3a displays the coefficients from the estimation of Equation 2.1 using placebo terror events. Panel 2.3b displays the distribution of the coefficients from the 300 estimations of Equation 2.2 using placebo terror events with different random dates. All regressions consider an event as relevant if the number of terror events in a month is above the past three-year average. Bars identify 95% confidence intervals.

Figure 2.3: Placebo Tests using random terror dates

Placebo outcomes As a second placebo test, we consider the effect of relevant terror events in the home country on outcomes that, in principle, should not be

affected by such events. These outcomes include worries about the future of the European Union, crime in Germany, economic development, and the environment[21]. As some of these variables rely on questions that are not asked in all survey waves, our sample size differs with the outcome. Table 2.4 shows the coefficients of estimating Equation 2.2 using these alternative outcomes. We see no significant effect of relevant terror events in the home country on these outcomes.

Worries about	Higher than average of last 3 years			
	Future of EU	Crime in Ger.	Econ. Develop.	Environment
	(1)	(2)	(3)	(4)
Post-Terror	0.067	0.056	0.018	-0.044
	(0.104)	(0.060)	(0.056)	(0.068)
Observations	908	5097	5334	5085
Origin country x Year FE	Yes	Yes	Yes	Yes
Month FE,	Yes	Yes	Yes	Yes
State of Residency FE	Yes	Yes	Yes	Yes
Indiv. Controls	Yes	Yes	Yes	Yes

*p<.1; **p<.05; ***p<.01
Standard Errors in parenthesis clustered at the Country x Year x Month level
Notes: Table 2.4 displays the coefficients from the estimation of Equation 2.2 where the outcome is "Remain permanently in Germany". FE refers to fixed effects. All results consider a 90 days bandwidth. Individual controls include age, gender, years since migration and its square, marital status, educational achievement and children.

Table 2.4: Terror events and placebo outcomes, 90 days bandwidth

Excluding a year or a country Next, we test whether our results are driven by specific countries or survey years. We run the baseline regression excluding one survey year at a time and repeat the same procedure excluding countries of origin. Figure 2.B.4 in appendix 2.B.1 panel a) shows the estimated coefficients for each regression in which a survey year is excluded, while panel b) shows the estimated coefficients for each regression in which a country of origin is excluded. The y-axis displays the excluded survey year or country of origin. Overall, our results are stable throughout these robustness tests.

2.3.4 Heterogeneous Effects

In this sub-section, we investigate if the effect of terror events on the intention to remain in Germany varies with the level of integration, employment status, years since migration, location of close family at the time of the event and risk aversion and immigration group. First, we test the hypothesis that the level of integration in Germany mediates the importance of terror events in the home countries in determining the willingness to remain in Germany. If immigrants are highly integrated into German society is less likely that they pay attention to events occurring in their home countries.
We proxy the level of integration by the self-reported level of oral German knowledge and the language of the newspaper read by the respondent. For each of these variables, we run separate regressions for each level and display the coefficients in Figure 2.4.

[21]For each of these worries, we create a dummy variable that equals one if the respondent replied to be "very worried" or "worried" and zero otherwise

The results in panel 2.4a show that for immigrants with a very good level of Germany (i.e., highly integrated in Germany), the effect of terror events on the intention to stay is virtually zero. On the contrary, for immigrants with good or poor German knowledge, the effect is similar to our baseline results. Being interviewed after a terror event increases the intention to stay in Germany permanently by 11 percentage points.

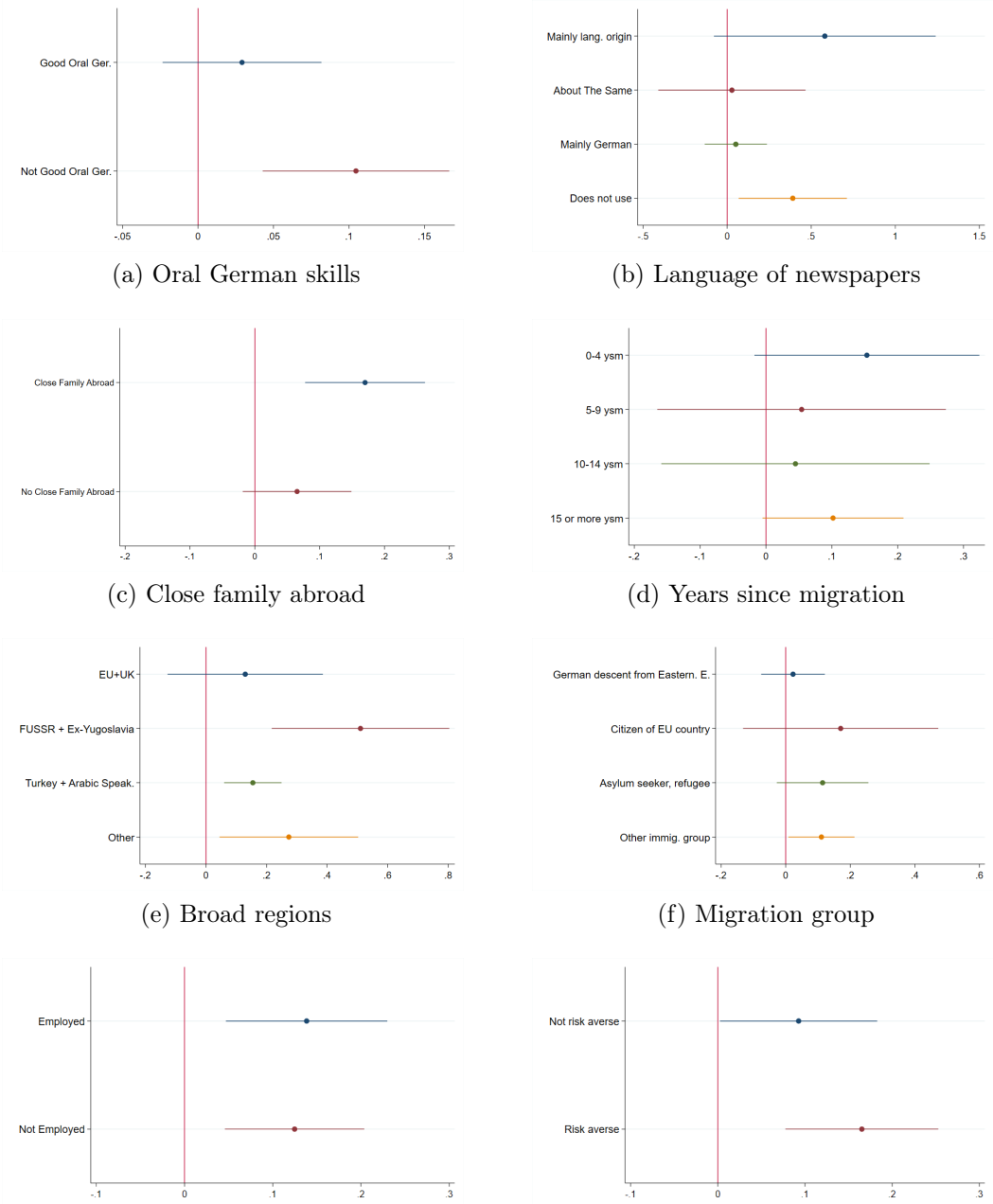
Similarly, when looking at the heterogeneous effects of the language of the newspaper read by the respondent in Figure 2.4b, we find that individuals who read newspapers in mainly the language of their country of origin are more likely to be affected by terror events in their home country.

We also consider how terror events in the home country might affect individuals differently depending on the location of their closer family members (e.g., parents, spouse, children, grandparents and siblings). In principle, we expect that individuals with close family members in their home country are more likely to be affected by events in their home country. The reason is that these individuals were less likely to wish to remain in Germany permanently when compared to individuals who already have their family in Germany. After experiencing socio-political events in their home country, it is not only more likely that they intend to remain in Germany permanently, but they are also more likely to wish to bring their family to Germany. Indeed Figure 2.4c shows that individuals who have close family abroad are more likely to revise their intentions to remain in Germany than individuals who have close family in Germany. In 2.4d, we allocate individuals into groups based on the number of years since they arrived in Germany. We can see that the effect is more pronounced among the recent arrivals (0-4 years) who came with a possibly shorter intended length of stay and hence have a higher scope to revise it upwards. The effect is also slightly larger for migrants who have been living in Germany for 15 years or more. These could be individuals who are close to retirement and initially planned to return to their home countries, but who update their return intentions following a terror event in their country of origin.

In Figure 2.4e, we group individuals into broad regions of origin. The effect of terror events on return intentions is larger for individuals coming from the former USSR and ex-Yugoslavian areas, although the standard errors are also considerably larger. Figure 2.4f compares individuals based on their entry visas to Germany. There are no significant differences between individuals entering Germany as EU nationals, asylum seekers/refugees or another group. The effect is substantially smaller among those entering Germany as German descendants from Eastern Europe. Around 60% of this group arrived in Germany in the 1990s following the fall of the Berlin Wall and the changes in the political systems of the former USSR. Most of these Ethnic German resettlers arrived in Germany already with the intention of staying permanently.

Critical to our analysis in section 2.4, in Figure 2.4g, we look at the heterogeneous effects of terror events by employment status at the time of the interview. The results show that there is no significant difference between employed and non-employed individuals.

Finally, in Figure 2.4h, we look at the heterogeneous effects of terror events by risk aversion. As expected, more risk-averse individuals are more likely to put a higher value on physical security and hence react more to changes in the socio-political conditions in their home countries. An increase in the incidence of terror events in the home country, for which the exact location and timing are unpredictable, creates a state of uncertainty and decreases safety.



(a) Oral German skills

(b) Language of newspapers

(c) Close family abroad

(d) Years since migration

(e) Broad regions

(f) Migration group

(g) Employment status

(h) Risk aversion

Notes: Each panel displays the coefficients from the estimation of Equation 2.2 for each level of the variable in the graph title. All regressions consider an event as relevant if the number of terror events in a month is above the past three-year average and uses 90 days bandwidth.

Figure 2.4: Heterogeneity analysis

2.4 Socio-political conditions in the home country and labor market outcomes

In the previous section, we showed that by affecting the perception of security in the home country, terror events in the country of origin lead to an update in migrants'

return intentions. In this section, we investigate whether this update on intentions to stay translates into changes in the economic behaviour of migrants.

Previous research has shown that differences in the intended length of stay among immigrants can create different incentives to invest in human capital, which in turn lead to differences in earnings and career profiles (Adda, Dustmann and Görlach, 2022). However, if we focus on these outcomes, it is unlikely that we will see an immediate change in response to a shock to return intentions. The completion of an educational degree[22] or a change in the earnings path and career profile take time to materialize. Hence, it is empirically difficult to disentangle the true effect of terror events on these economic indicators. A measurable indicator of economic behaviour that reacts quickly to individual circumstances is job search activity and reservation wages among unemployed individuals. Since these two measures affect the length of unemployment, we take time to employment as our preferred economic indicator.

In this section, we leverage social security data from Germany and test whether a negative shock to return intentions, induced by terror events, has an effect on the labour market outcomes of immigrants entering unemployment when terror events occur in their home countries. Specifically, we compare this group of immigrants to immigrants that enter unemployment in times of stable home country conditions and look at differences in the length of unemployment and the wage at the first job. In section 2.4.4, we ruled out an alternative channel through which terror events could affect unemployment length and accepted wages - sending remittances to the home country.

The a priori effect of a negative event in the home country among unemployed migrants is less clear than the effect among employed migrants or recently arrived migrants who have some economic security. First, by lowering expected utility[23] in the home country, terror attacks could result in lower reservation wages at the destination if migrants benchmark their reservation wage in the host country with the wage they could get their country of ancestry. In this case, we expect migrants who experience a relevant terrorist event in their home country to have shorter unemployment spells and lower accepted wages in Germany. Second, terrorist events could instil "fear," motivated, for example, by the possibility of having to leave Germany owing to unsustainable economic conditions.[24] This feeling of "fear" may also lead migrants to feel under strain, making them more determined to pursue a long-term career in Germany. In this case, the present value of a job in Germany increases. Hence, the "fear" effect can lead to a higher search effort and more selectivity regarding future wage growth and non-wage job characteristics, leading to a positive impact on accepted wages and an ambiguous effect on unemployment duration.

As a note of caution, by using the length of unemployment as our primary economic outcome means that we will use a particular group of immigrants – those who have already been employed in Germany and have unemployment spells. These

[22]Investments in human capital observed in the GSOEP, such as enrolling in further education or acquiring a university degree, are measured once individuals have started to attend them rather than when the decision to take them was taken - and there can be a considerable lag between the two.

[23]Terror events affect the perception of security in the home country

[24]The amount of unemployment benefits an individual receives and the duration of those benefits depends on how long they have contributed and the salary they received before becoming unemployed. Furthermore, individuals who have mini-jobs are not obliged to contribute to unemployment insurance, and self-employed individuals contribute on a voluntary basis.

migrants might have already spent resources learning German or invested in other host-country-specific skills. Hence, their adjustment pattern is not directly comparable to recent arrivals.

2.4.1 Empirical Strategy

To investigate the effect of terror events on unemployed immigrants' labor market outcomes, we define immigrants from the same nationality who enter unemployment at the time of a terror event as treated and those who enter unemployment at the time of no events as controls. We, therefore, estimate the following model:

$$Y_{i,o,y,m} = \beta Terror_{o,y,m} + \delta X_{i,y} + \gamma_{o,s} + \eta_y + \phi_m + \rho_k + \epsilon_{i,o,y,m} \tag{2.3}$$

where $Y_{i,o,y,m}$ can be the time until employment in days, a dummy variable taking the value of one if there was a change in occupation (industry) between the last occupation before unemployment and the first occupation (industry) after unemployment, the percentage change in the last wage before unemployment and the first wage after unemployment and a dummy if the first job after unemployment is a full-time job. $Terror_{o,y,m}$ is an indicator that takes the value of 1 if a person from country of origin o entered unemployment in a month m and year y when terrorist events occurred in the country of origin o and 0 if a person entered unemployment in a month with no events. The terror events used in this section are exactly the same used in the survey section. We consider only individuals who entered unemployment in the exact same month that a relevant terror event occurred in the home country and individuals who entered unemployment in a month where there were no terror events in the 90 days before and 90 days after that month.[25] By not considering individuals who entered unemployment one, two or three months after the relevant terror event we are taking a conservative approach to ensure that these individuals did not enter to unemployment as a consequence of the relevant terror event.

We use country of origin fixed effects times state fixed effects ($\gamma_{o,s}$) to compare individuals from the same country of origin who reside in the same German state and got unemployment at different time periods. The year fixed effects (η_y), month fixed effects (ϕ_m) and local labour market fixed effects (ρ_k) control for time and geographical differences that could drive time to unemployment. X includes gender, education, age, years since entering the administrative data set (a proxy for years since migration), and its square, the log of the last wage before unemployment and the log of the firm size (in number of employees) before unemployment.

Our identifying assumption is that had the terror event not occurred, the difference in outcomes between unemployed who entered unemployment with and without an event would have been zero. While we can't directly test this assumption, we run a balance test between these two groups of unemployed, comparing a large set of characteristics at the time of unemployment registration. Results are reported in Table 2.B.6 in appendix 2.B.2, where the first column indicates the average values for the control group (i.e. those who entered unemployment in a month with no home country terror events[26]), and the other columns indicate the difference between the control and the different treatment groups defined based on terror intensity. While

[25] In Figure 2.B.6 we show the results when varying this bandwidth.

[26] This follows our definition of no affected people in a terror event

some of the characteristics are statistically different, the size of the differences is extremely small, and significance is given by the large sample size. For example, the female coefficient is always statistically significant. However, on average, individuals in the treatment group are 0.2 percentage points more likely to be females than the control group, a qualitative small difference.

2.4.2 Main Results

We now turn to our main results for the effect of terror on immigrants' labour market outcomes. The results are reported in Table 2.5 where column (1) shows the estimated coefficients from Equation 2.3, using the number of days in unemployment as an outcome; columns (2) and (3) use as an outcome a dummy variable that equals one if the individual changed occupation or industry; column (4) use a dummy variable that equals one if the first job after unemployment is full time, and column (5) the percentage change in the last wage before unemployment and the first wage after unemployment. All specifications include individual characteristics, month and year-fixed effects, country-of-origin times state fixed effects, and local labour market (Kreis) fixed effects. We use the same events as in the SOEP section for the three months above the average with a 90 days bandwidth.

For unemployment duration, we find that immigrants who enter unemployment in a month when there is a relevant terror event in the home country are more likely to have a shorter unemployment duration, of about 12 days, than individuals entering unemployment in times of stable home country conditions. There is no significant difference in wages and changes in occupation. However, we find significantly different results when we break down by migrants from the EEA or Schengen area (Panel C) and from outside the EEA or Schengen area (Panel B). We choose this breakdown because of the legal residency differences between EU and non-EU.

Non-EEA migrants who entered unemployment when a terror event occurred in their home country have significantly shorter unemployment durations. The effects on the accepted wages are not significant at conventional levels, although they point to a negative effect. For EEA or Schengen area migrants (Panel C), terror attacks in their home country do not rush them into finding a new job. However, they seem more likely to change occupations and industries. Wages are marginally higher, but the difference is not statistically significant at 10 per cent. These results could signal that EEA or Schengen area migrants get more selective concerning their careers in Germany.[27]

[27]Figure 2.B.5 in the appendix shows the effect on return intentions for EEA/Schengen and Non-EEA/Schengen breakdown. The magnitude of the coefficients is similar, although the standard errors for the EEA/Schengen group are larger since this groups represents about 30% of the SOEP sample

Panel A: All migrants	Unemp. durat.	Change occup.	Change industry	FT employ	% wage change
	(1)	(2)	(3)	(4)	(5)
Unemp. with terror	-12.029*	0.010	0.007	0.006	-0.030
	(7.115)	(0.007)	(0.007)	(0.005)	(0.154)
Observations	188521	187441	187441	187441	186675
Panel B: Non-EEA/Schen. mig.	(1)	(2)	(3)	(4)	(5)
Unemp. with terror	-21.790**	-0.010	-0.001	0.012	-0.021
	(9.890)	(0.009)	(0.009)	(0.007)	(0.124)
Observations	101052	100697	100697	100697	100250
Panel C: EEA/Schen. mig.	(1)	(2)	(3)	(4)	(5)
Unemp. with terror	9.990	0.043***	0.023**	0.001	0.252
	(10.596)	(0.010)	(0.010)	(0.009)	(0.179)
Observations	87444	86719	86719	86719	86400
Year FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
C. Origin x State FE	Yes	Yes	Yes	Yes	Yes
Indiv. charact.	Yes	Yes	Yes	Yes	Yes

Robust Standard Errors in parenthesis, *p<.1; **p<.05; ***p<.01
Notes: Figure 2.5 reports the estimated coefficients and robust standard errors in parenthesis for regressions of the outcome on the terror indicator. The terror indicator is defined based on different levels of affected individuals in the home country in the same month when immigrants register as unemployed. FE refers to fixed effects. Individual controls: education, age, gender, years since migration, and its square.

Table 2.5: Effects of terror events on unemployed immigrants' outcomes

We now turn to the type of firms and jobs migrants become employed. Even if there are no immediate wage gains, non-EEA/Schengen area migrants could switch to companies that offer more stable jobs, better career prospects, higher future wage growth or better amenities. Even though we cannot measure all these outcomes directly in the IEB data, we use some proxies. In Table 2.6, we regress equation 2.3 on: (1) a dummy variable that equals one if after unemployment the individual is employed in a larger firm[28] than before unemployment; (2) a dummy variable that equals one if after unemployment the individual is employed in a firm where the top wages (25th percentile) are above the top wages before unemployment; (3) a dummy variable that equals one if after unemployment the individual is employed in a firm with fewer low qualified workers than before unemployment; (4) a dummy variable that equals one if after unemployment the individual is employed in a firm with more foreigner workers than before unemployment; (5) a dummy variable that equals one if the individual changed from non-full-time employment to full-time employment; and (6) a dummy variable that equals one if the individual changed from full-time employment to non-full-time employment. For some firms, information is missing on

[28]According to Destatis classification, a micro firm is a firm with up to 9 employees, a small firm with up to 49 employees, a medium firm with up to 249 employees and a large firm is one with more than 249 employees.

the wage distribution, and hence the sample size for that outcome differs.

Non-EEA/Schengen area migrants entering unemployment in a month with a relevant terror event are significantly less likely to be employed in a high-pay firm than non-EEA/Schengen area migrants entering unemployment in stable home country conditions (column (2)). Although not significant at 10 per cent, non-EEA/Schengen area migrants are slightly less likely to be employed in large firms, which can proxy for job stability (column (1)). On the other hand, EEA or Schengen area migrants entering unemployment in a month with a relevant terror event are significantly more likely to be employed in a larger firm and a firm with fewer low-qualified workers than non-EEA/Schengen area migrants entering unemployment in stable home country conditions (column (1)). Although not significant at 10 per cent, EEA or Schengen area migrants are slightly more likely to move to a firm which offers high wages at the top 25th percentile of the firm wage distribution (column (2)). This signals that non-EEA/Schengen area migrants entering unemployment in a month with a relevant terror event might be more selective and enter firms offering better job prospects or higher job stability.

Panel A: All migrants	Change larger firm	Change high pay firm	Change fewer low skill firm	Change higher share foreigners	Change non-FTE bfu to FTE	Change FTE bfu to non-FTE
	(1)	(2)	(3)	(4)	(5)	(6)
Unemp. with terror	0.004	-0.004	0.012*	-0.002	0.000	0.001
	(0.006)	(0.008)	(0.007)	(0.006)	(0.003)	(0.005)
Observations	179917	150748	179917	179917	187441	187441
Panel B: Non-EEA/Schen.	(1)	(2)	(3)	(4)	(5)	(6)
Unemp. with terror	-0.001	-0.019*	0.005	-0.007	-0.001	0.001
	(0.008)	(0.010)	(0.009)	(0.008)	(0.005)	(0.007)
Observations	96322	81061	96322	96322	100697	100697
Panel C: EEA/Schen.	(1)	(2)	(3)	(4)	(5)	(6)
Unemp. with terror	0.016*	0.011	0.024**	0.005	-0.003	0.001
	(0.009)	(0.012)	(0.011)	(0.010)	(0.006)	(0.008)
Observations	83576	69672	83576	83576	86719	86719
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes	Yes
C. Origin x State FE	Yes	Yes	Yes	Yes	Yes	Yes
Indiv. charact.	Yes	Yes	Yes	Yes	Yes	Yes

Robust Standard Errors in parenthesis, *p<.1; **p<.05; ***p<.01

Notes: Figure 2.6 reports the estimated coefficients and robust standard errors in parenthesis for regressions of the outcome on the terror indicator. The terror indicator is defined based on different levels of affected individuals in the home country in the same month when immigrants register as unemployed. FE refers to fixed effects. Individual controls: education, age, gender, years since migration, and its square.

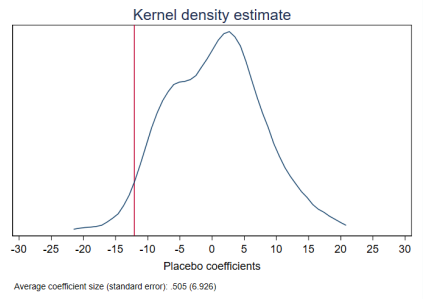
Table 2.6: Effects of terror events on unemployed immigrants' outcomes

2.4.3 Placebo Tests and Robustness Checks

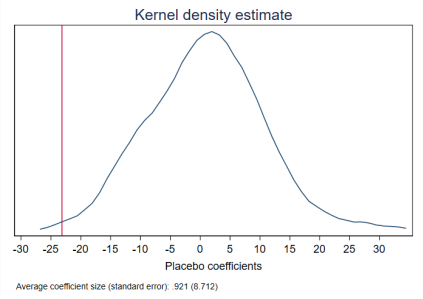
In the previous section, we showed that terror events in the home countries affect the labour market outcomes of immigrants entering unemployment in a month when a terror event occurs in their home country. In this section, we test the stability of our results using both placebo tests and robustness checks.

Changing bandwidth or reference point We start by testing whether the main results are sensitive to the bandwidth around the event or the average above which we consider a terror event to be relevant. Figure 2.B.6 in appendix 2.B.1 displays the estimated coefficients for the entire sample of immigrants when reducing the bandwidth from 90 days (i.e., the baseline bandwidth) to 60 days and 30 days around the terror event and when considering if, in a given month, there was at least one more terror event than the past country-specific three-year average (i.e., the baseline average), four-year average, or five-year average. Our main conclusions hold.

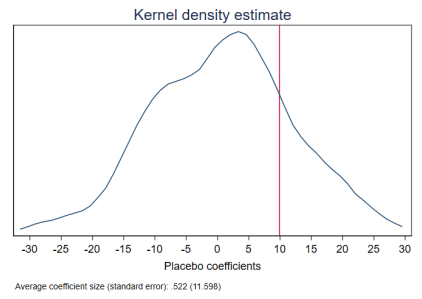
Placebo terror event date One concern is that other factors drive the effects on labour market outcomes, and we would observe the same pattern in the absence of the terrorist event. To address this issue, we randomly assign the binary treatment status 100 times across all observations. If there are x-treated and y-controls across all observations, the total number of treated and controls does not change, but x and y and reshuffled across observations. We then estimate the effect of placebo treatment status on unemployment duration. Figure 2.5 shows the distribution of the 200 estimated coefficients for the five outcomes of interest used in the main analysis in table 2.5. The red vertical lines indicate the point under the true treatment assignment (the same coefficients reported in Panel A of Table 2.5).



(a) All migrants: Unemp. dur.



(b) Non-EAA/Schengen: Unemp. dur.



(c) EAA/Schengen: Unemp. dur.

Notes: Figure 2.5 reports the estimated coefficients and confidence intervals in parenthesis for regressions of the outcome on the terror indicator. The outcomes and the specification are the same as those reported in Table 2.5. Unemp. dur. refers to unemployment duration

Figure 2.5: Robustness: terror events and labour market outcomes all migrants

In Table 2.5, we found a negative and significant effect of entering unemployment in a month with a terror event for all migrants. This effect was mostly driven by

non-EEA/Schengen immigrants. Figure 2.5 shows that assigning placebo treatment status to all migrants and non-EEA/Schengen immigrants who did not in reality experience a terrorist event has, on average, zero effects on their unemployment duration. This finding provides an important piece of evidence in favour of our baseline results.

Excluding specific groups We have chosen to break down countries by EEA/Schengen area and non-EEA/Schengen area because within the Schengen area, members from other Schengen countries have few work restrictions and generally do not need a permit to work (they also do not need a visa to enter). We test the sensibility of our results by considering only EU countries for the EEA/Schengen group and by excluding OECD members, which in principle are wealthier, and refugees, which in principle cannot return home, from the non-EEA/Schengen group. A drawback of the IEB data is that we cannot identify the workers' visas for the period 2000-2018; therefore, we cannot precisely identify refugees. We created a group of "potential" refugees by considering the ten largest refugees group each year in the Destatis. Nevertheless, many Eastern Europeans were entering Germany in the late 1990s and early 2000s both as refugees and as economic migrants, and hence we do not consider them refugees. The results are displayed in Figure 2.B.7 in the appendix and confirm that our main conclusions are robust to group specification.

2.4.4 Additional Results

In this section, we explore the effect of terror attacks on other outcomes that could potentially mediate the effect of terror on labour market behaviour - which we analyze in section 2.4. Namely, we look at the effect of terror events on remittances and self-reported health. It could be that the families of immigrants in the home country are directly affected by the terror events and hence some migrants will want to re-enter employment faster to be able to send money to their relatives. On the other hand, it could be that terror events affect the mental health of immigrants such that they find it difficult to re-enter employment. To proxy for remittances, we rely on a GSOEP question that asks respondents if they have sent money abroad.

The results are shown in table 2.7. Terror events have no significant effect on self-reported health satisfaction and have a negative and significant effect on sending money abroad. This negative effect could be driven by the fact that after a terror attack, migrants perceive their home country as being more financially insecure or that they expect the terror attacks to affect the financial markets.

If anything, wanting to spend less money abroad would have a negative effect on job search efforts and a positive effect on reservation wages - the opposite of the effect of the intention to stay permanently in Germany. Hence, there is the possibility that our results in section 2.4 are muted by the negative effect on remittances.

In column (3) of table 2.7, we also show that terror events might affect the reservation wage of GSOEP respondents who were unemployed at the time of the survey. About 60% of the sample of unemployed individuals around a terror event in the GSOEP are from non-EEA/Schengen area. Despite the small sample size, there is some suggestive evidence that by creating a feeling of insecurity in the home country, terror events lower the reservation by 364 euros in Germany. The negative effect of relevant terror events on reservation wages effect might be driven by the fact that migrants benchmark their reservation wage in Germany by the wage below

which they would prefer to go back to their home country. When comparing with results in Table 2.5, we do not find evidence that there is a pass-through from lower reservation wages to lower accepted wages but this could be driven by the fact that non-EEA/Schengen migrants earn very low wages to start with (close to minimum wage). Nevertheless, one should be cautious when drawing conclusions, given the small sample size.

	Higher than average of last 3 years		
	Send money abroad	Satisfaction with health	Reservation wage
	(1)	(2)	(3)
Post-Terror	-0.036	-0.041	-363.651**
	(0.028)	(0.181)	(179.130)
Observations	6555	6489	575
Origin country x Year FE	Yes	Yes	Yes
Month FE,	Yes	Yes	Yes
State of Residency FE	Yes	Yes	Yes
Indiv. Controls	Yes	Yes	Yes

Standard Errors in parenthesis clustered at the Country x Year x Month level, *p<.1; **p<.05; ***p<.01
Notes: Table 2.7 displays the coefficients from the estimation of Equation 2.2 where the outcome is "Remain permanently in Germany". FE refers to fixed effects. All results consider a 90 days bandwidth. Individual controls include age, gender, years since migration and its square, marital status, educational achievement and children.

Table 2.7: Terror events, 90 days bandwidth

2.5 Discussion and Conclusion

The economic and social behaviour of temporary migrants can sharply differ from that of permanent or long-term migrants. Previous research has shown that differences in the intended length of stay among immigrants can create different incentives to invest in human capital, leading to differences in earnings and career profiles (Adda, Dustmann and Görlach, 2022). It is, therefore, important to better understand the determinants of migrants' intended length of stay. In this paper, we contribute to this understanding. Specifically, we investigate whether the home country's socio-political conditions affect immigrants' return intentions and labour market behaviour in the host country. We focus on terrorist events in the home country and combine precise terror event data with survey and administrative data. Our paper is the first to empirically test the effect of changes in home country conditions on return intentions and labour market outcomes.

In this study, we provide evidence that terror events lead to an update in migrants' priors with respect to the level of security in the country of origin and hence affect the intended length of stay. While return plans can change over the course of an individual migration spell and may deviate from the actual date of the return (Dustmann and Görlach, 2016; Chabé-Ferret, Machado and Wahba, 2018), in this study, we are interested in analyzing the effect on contemporaneous re-employment decisions which are based on current return plans. We find that non-EEA/Schengen area migrants entering unemployment in Germany when a relevant terror event occurs in the home

country re-enter employment faster than migrants entering unemployment in stable times. While this change in economic behaviour benefits the host country in the short term, it is unclear what are the long-run consequences of such a decision since non-EEA/Schengen area migrants get employed in firms with lower top wages.

For EEA or Schengen area migrants, there are few Visa restrictions and their outside option in the home country is higher than that of non-EEA/Schengen migrants. We find strikingly different results for this group. Namely, EEA or Schengen area migrants entering unemployment in Germany when a relevant terror event occurs in the home country are more likely to change occupation and industry and to be employed in a larger firm with fewer low-skilled workers than migrants entering unemployment in stable times. This could signal that EEA or Schengen area migrants entering unemployment in Germany when a relevant terror event occurs in the home country become more committed to pursuing a long-term career in Germany.

Our results add an important and credible piece of evidence on the effect of home-country events on migrants' behaviour. With this study, we contribute to the understanding of migrants' intended duration of stay and its effect on economic behaviour in the host country. Our insights are policy-relevant for both host and home countries since they help host countries to understand what affects migrants' labour market outcomes and home countries how they might attract migrants back home.

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Appendix

2.A Immigrants in Germany

The current immigrant population in Germany essentially reflects three large immigration waves. The first wave started in the mid-1950s when, as a result of strong economic growth in (West-) Germany and a lack of available manpower, Germany started to actively recruit foreign workers abroad, predominantly in Turkey, Yugoslavia, Italy, Greece, and Spain. Following the recession in 1973/1974, this active recruitment of immigrants was abandoned. However, subsequent immigration of family members continued. The second and more recent immigration wave to Germany was triggered by the collapse of the former Soviet Union and the political changes in Eastern Europe in the late 1980s and early 1990s. The main immigrant groups of this period were, on the one hand, ethnic German immigrants (so-called Aussiedler), mostly from Poland and the former Soviet Union, and, on the other hand, refugees from the wars in former Yugoslavia. The third wave was in 2015-2016, when a new wave of asylum seekers arrived in Germany driven by the wars in Syria, Iraq, and Afghanistan.

In Table 2.A.1, we show the fifteen largest immigrant groups in the GSOEP survey across time, the last column shows the frequencies for the time period used in this study (we restrict to after 1999 to be compatible with the IEB). We can see that the share of migrants in the sample accompanies well the different migration waves[29]

[29]We discuss the migration samples within the GSOEP in appendix 2.A.1

	1985-1990	1991-2000	2001-2010	2011-2018	Total 1985-2018	Sample 2000-2018
Turkey	35.403	30.998	20.914	6.938	18.567	11.845
Italy	17.915	13.207	7.760	3.170	8.140	4.797
Greece	13.330	8.643	3.931	1.872	5.206	2.631
Spain	10.244	5.019	1.873	0.961	3.211	1.298
Ex-Yugoslavia	9.171	4.114	1.785	0.191	2.518	0.751
Croatia	4.601	5.105	3.029	1.018	2.751	1.735
Bosnia-Herzegovina	3.039	4.170	2.790	1.173	2.373	1.743
Poland	0.715	7.440	11.052	8.400	7.746	9.333
Kosovo-Albania	0.389	0.920	1.414	2.632	1.729	2.212
Romania	0.373	2.568	4.653	5.221	3.918	5.000
Russia	0.039	3.952	9.008	9.580	7.048	9.354
Kazakhstan	0.000	3.781	8.628	8.095	6.255	8.260
Syria	0.047	0.040	0.054	14.631	6.645	9.612
Iraq	0.000	0.020	0.171	4.575	2.110	3.058
Afghanistan	0.000	0.020	0.078	3.668	1.680	2.436

Notes: Table 2.A.1 reports the distribution of the largest nationalities in the GSOEP over time. Shares are computed across the sample of respondents in each decade. The last column reports the distribution of the largest nationality groups in the full sample. Source: GSOEP

Table 2.A.1: Largest migrant groups in the GSOEP data in %

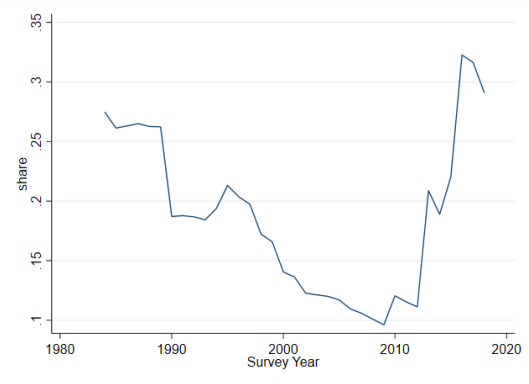
2.A.1 Migrants in the GSOEP

Figure 2.A.1 shows the share of migrants in the GSOEP sample. When the survey started, in 1984, migrants represented about 27 percent of the GSOEP sample. At this time, the main groups of foreigners were individuals from Turkey, Greece, Yugoslavia, Spain, and Italy (sample B). The share of migrants fell until 1994 when a boost sample (D1 and D2) of migrants who came to Germany after 1984 was added to take into account the flow of ethnic Germans from the former Soviet countries. After the boost sample was added in 1994-95 the share of migrants in the GSOEP fell steadily. To improve the representation of migrants living in Germany, two new samples (M1 and M2) were established in 2013, which covered individuals who immigrated to Germany after 1995 or second-generation immigrants[30]. Following, the Arab Spring and the war in Syria, a new refugee sample was added in 2016 (M3 and M4), with a subsequent booster in 2017 (M5). These samples covered households with individuals who arrived in Germany between January 2013 and December 2016 and had applied for asylum by June 2016 or were hosted as part of specific programs of the federal

[30]Sample M1 was added in 2013 with around 2,700 households and it includes individuals who immigrated to Germany after 1995 or second-generation immigrants. Sample M2 was added in 2015 with around 1,100 households and it includes individuals who immigrated to Germany between 2010 and 2013. The samples were drawn using register information from the German Federal Employment Agency and were the product of a cooperation between the Institute for Employment Research (IAB) in Nuremberg and the German Socio-Economic Panel (SOEP) at DIW Berlin. The first seven survey waves were carried out between 2013 and 2018.

states[31].

Figure 2.A.1: Share of migrants in the GSOEP

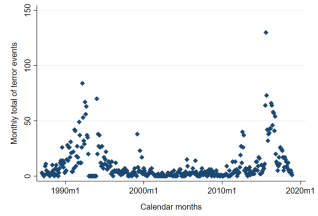


Notes: Figure 2.A.1 displays the share of immigrants in the sample of SOEP respondents in each survey wave. The y-axis refers to the share. The time window is 1984-2019. Source: GSOEP.

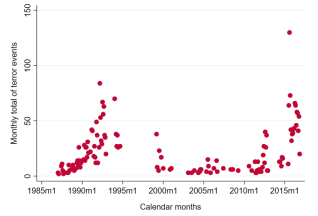
2.B Additional Tables and Figures

2.B.1 Additional Figures

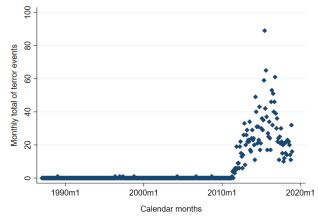
[31]The refugee samples are a joint project of the Institute for Employment Research (IAB), the Research Center of the Federal Office for Migration and Refugees (BAMF-FZ) and the Socio-Economic Panel (SOEP).



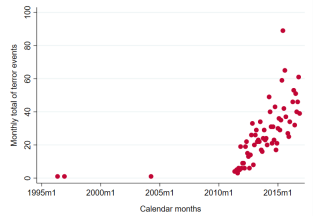
(a) Turkey: all terror events



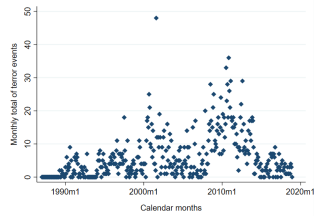
(b) Turkey: rel. terror ev.



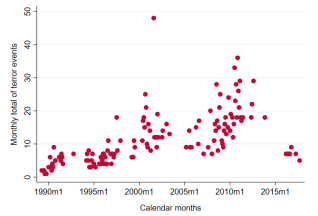
(c) Syria: all terror events



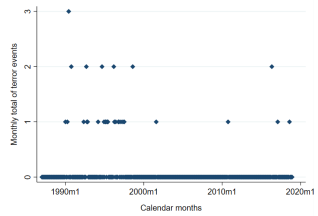
(d) Syria: rel. terror events



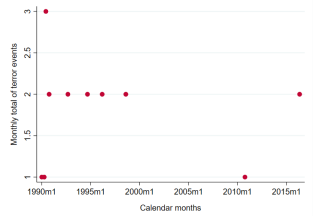
(e) Russia: all terror events



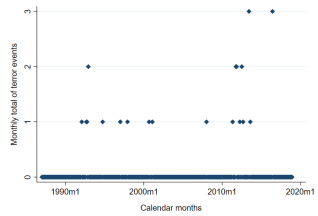
(f) Russia: rel. terror events



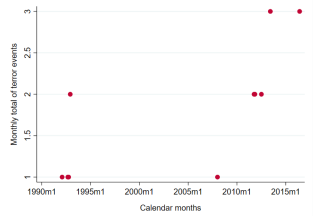
(g) Poland: all terror events



(h) Poland: rel. terror ev.



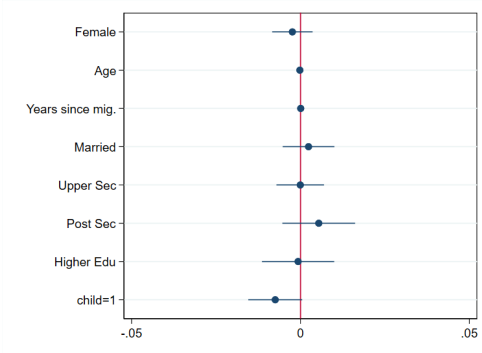
(i) Kazakhstan: all terror events



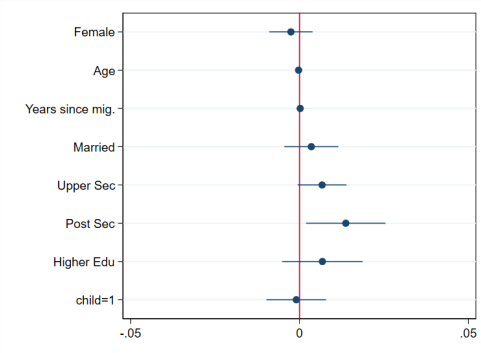
(j) Kazakhstan: rel. terror events

Notes: The left panel shows all terror attacks for each country between 2000-2018, as in the GTD data. The right panel shows the relevant events. An event is defined as relevant if, in a given, there is at least 1 more terror attack than the past country-specific 3-year monthly average number. Rel. refers to relevant and ev. to events

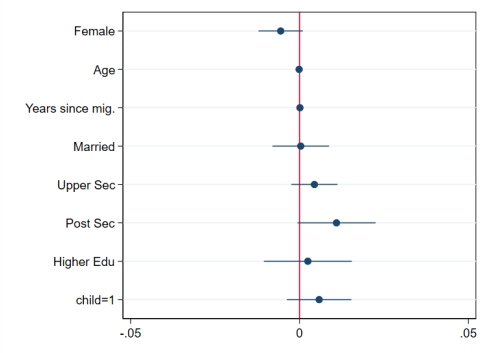
Figure 2.B.1: All terror events and relevant terror events



(a) Deviation to 3 year mean



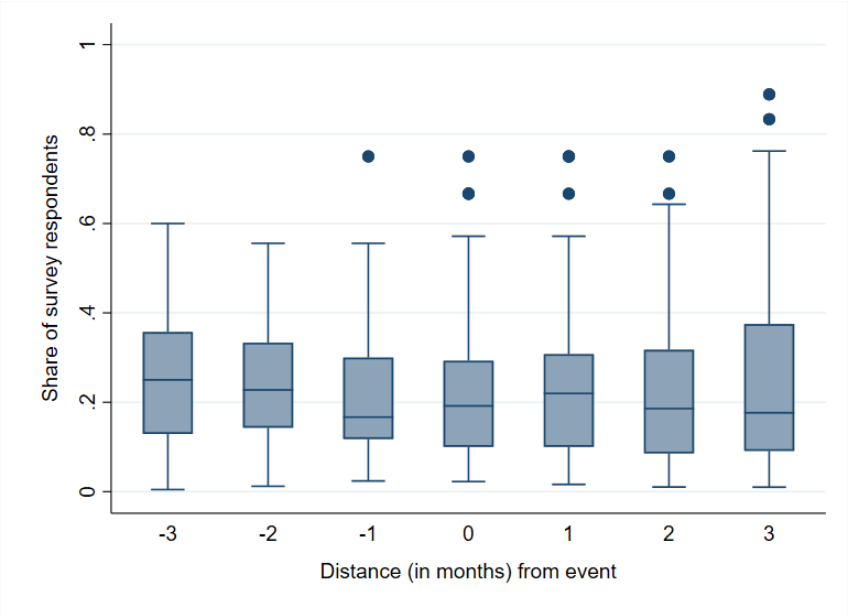
(b) Deviation to 4 year mean



(c) Deviation to 5 year mean

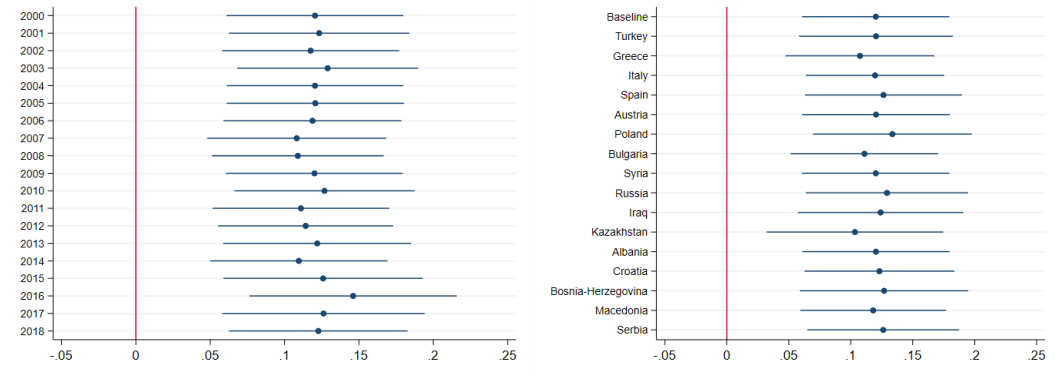
Notes: Panel 2.3a displays the coefficients from the estimation of Equation 2.1 using placebo terror events. Panel 2.3b displays the distribution of the coefficients from the 300 estimations of Equation 2.2 using placebo terror events with different random dates. All regressions consider an event as relevant if the number of terror events in a month is above the past three-year average and include country of origin fixed effects (FE), survey year FE, country of origin x survey year FE and month FE. Bars identify 95% confidence intervals.

Figure 2.B.2: Joint balance test



Notes: Figure 2.B.3 displays the share of interviews around each country-specific event that we use in our main estimations. For a given country-specific event, we consider: i) the total number of interviews in the 90 days before and after the event and; ii) the number of interviews at 90, 60, 30 days before and after the event and at 0. The ratio in the x-axis represents the number of interviews at each of these points relative to the total number of interviews, e.g. ii) / (i). The x-axis indicates the months around terror events and the red line at 0 indicates the time of the terror event.

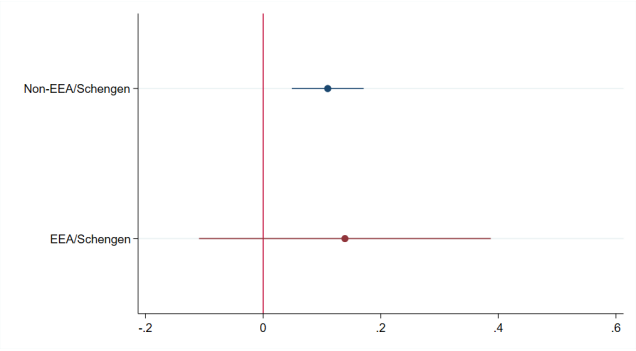
Figure 2.B.3: Density of interviews around terror events



a) Exclude one survey year, 90 days Bandwidth b) Exclude one country, 90 days Bandwidth

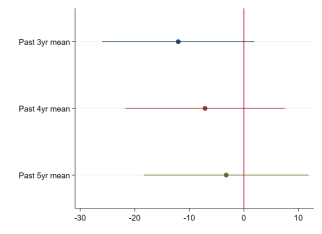
Notes: Panel a) and b) display point estimates and 95% confidence intervals for regressions that exclude one survey year and country at a time, respectively. The y-axis refers to the excluded survey year (country). The x-axis indicates the size of the estimated coefficients. All regressions consider an event as relevant if the number of terror events in a month is above the past three-year average and include the full set of fixed effects and individual controls as in the baseline estimation. Robust standard errors. 90 days bandwidth

Figure 2.B.4: Robustness: exclude one country and survey year at the time

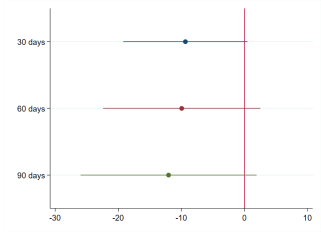


Notes: Figure 2.B.5 displays the coefficients from the estimation of Equation 2.2 for each level of the variable in y-axis. All regressions consider an event as relevant if the number of terror events in a month is above the past three-year average and uses 90 days bandwidth.

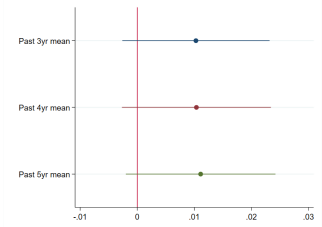
Figure 2.B.5: Return intentions Non-EEA/Schengen and EEA/Schengen



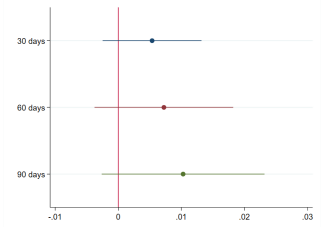
(a) Vary band.: U. dur.



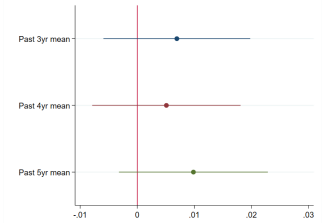
(b) Vary ref.p.: U. dur.



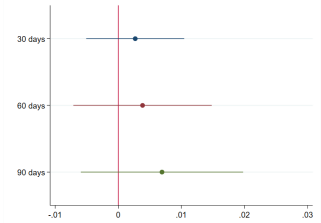
(c) Vary band.: Change occ.



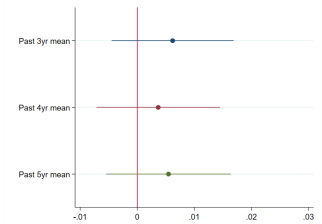
(d) Vary ref.p.: Change occ.



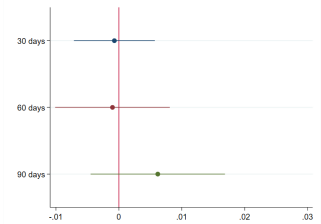
(e) Vary band.: Change ind.



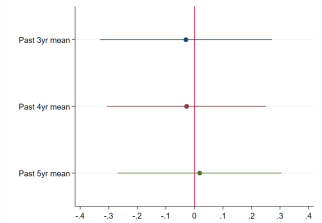
(f) Vary ref.p.: Change indu.



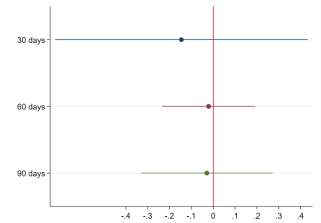
(g) Vary band.: FT employ.



(h) Vary ref.p.: FT employ.



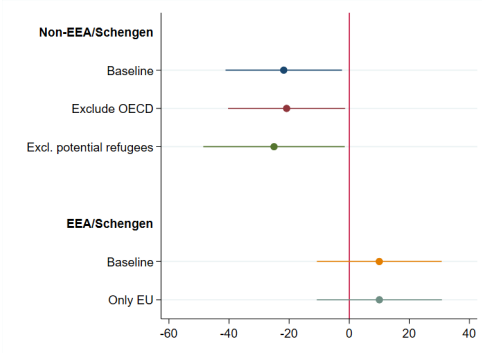
(i) Vary band.: % wage chg



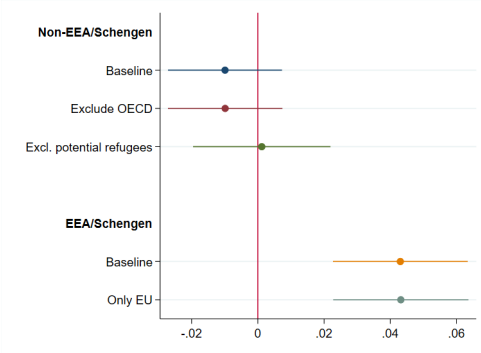
(j) Vary ref.p.: % wage chg

Notes: Figure 2.B.6 reports the estimated coefficients and confidence intervals in parenthesis for regressions of the outcome on the terror indicator. The outcomes and the specification are the same as those reported in Table 2.5. On the left-hand side, the bandwidth varies (baseline is 90 days). On the right-hand side, the reference point varies (baseline is past 3-year mean). U. dur. refers to unemployment duration, occ. to occupation, indu. to industry, FT employ. to full-time employment, chg. to change, ref.p to reference point and band to bandwidth.

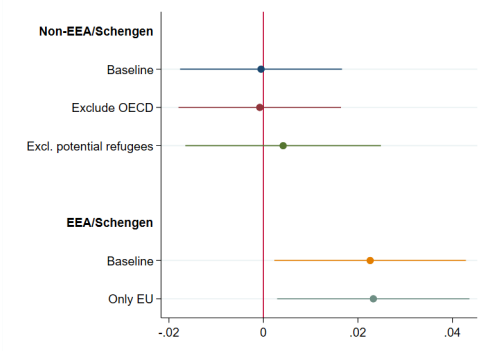
Figure 2.B.6: Robustness: terror events and labour market outcomes all migrants



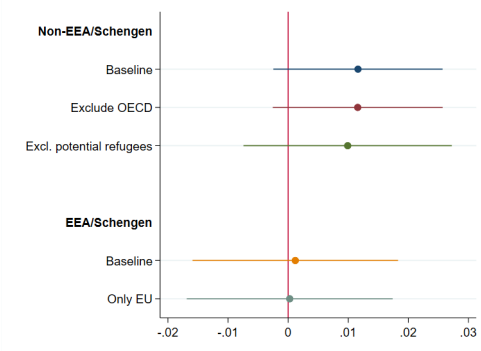
(a) Unemployment dur.



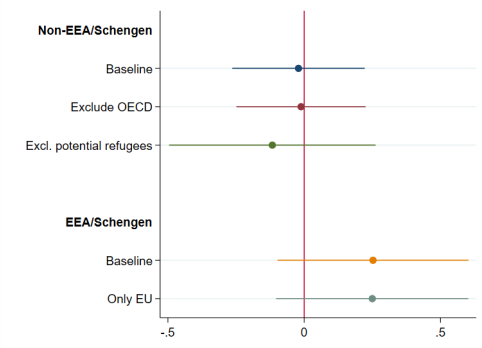
(b) Change occupation



(c) Change industry



(d) FT employment



(e) % wage change

Notes: Figure 2.5 reports the estimated coefficients and confidence intervals in parenthesis for regressions of the outcome on the terror indicator. The outcomes and the specification are the same as those reported in Table 2.5. Unemployment dur. refers to unemployment duration, FT employ. to full-time employment

Figure 2.B.7: Robustness: vary group definition

2.B.2 Additional Tables

	Entire sample 2000-18		Analysis sample 2000-18	
	Mean	SD	Mean	SD
Female	0.513	0.500	0.524	0.499
Age	42.606	14.344	43.986	14.418
Years since migration	17.049	12.885	20.031	12.404
Marital status	0.698	0.459	0.735	0.441
Has children	0.591	0.492	0.597	0.491
Low secondary or bellow educ.	0.348	0.476	0.347	0.476
Upper secondary educ.	0.322	0.467	0.354	0.478
Post-secondary educ.	0.133	0.340	0.135	0.342
Higher education	0.197	0.398	0.164	0.370
Full-time employed	0.338	0.473	0.360	0.480
Part-time employed	0.111	0.314	0.119	0.323
Other employed	0.079	0.270	0.082	0.274
Not employed	0.471	0.499	0.440	0.496
Remain in Germany permantly	0.835	0.371	0.812	0.391
Non-European	0.677	0.467	0.753	0.431
Observations	71059	71059	6604	6604

Notes: Table 2.B.1 reports the main characteristics of the full sample of immigrants in the GSOEP data (2000-2018). For each variable, we report the mean, standard deviation, and median value. The last row reports the total number of immigrants.
Source: GSOEP

Table 2.B.1: Summary characteristics of the migrant population in the GSOEP data

	Higher than average past 3 years, 90 days bandwidth	
	Number of rel. & isol. events	Mean number monthly of terror attacks
Algeria	2	15
Argentina	1	2
Austria	2	4
Belarus	1	1
Belgium	2	2
Bosnia-Herzegovina	4	3
Brazil	2	2
Bulgaria	1	2
Canada	2	2
China	3	4
Colombia	1	17
Congo	1	4
Croatia	1	2
Czech Republic	2	2
Denmark	1	2
Ecuador	1	3
Ethiopia	1	2
Ex-Yugoslavia	2	3
France	3	5
Georgia	1	3
Ghana	1	2
Great Britain	3	6
Greece	3	6
Hungary	1	2
Iran	1	3
Iraq	1	285
Ireland	1	2
Israel	1	9
Italy	2	3
Jamaica	1	1
Japan	1	6
Kazakhstan	3	3
Kosovo-Albania	8	4
Kyrgyzstan	1	2
Lithuania	1	1
Macedonia	4	2
Mexico	1	5
Montenegro	1	1
Morocco	2	1
Norway	1	2
Pakistan	1	9
Palestine	1	4
Peru	1	2
Philippines	1	7
Poland	2	2
Romania	1	1
Russia	4	10
Serbia	2	2
Spain	3	6
Sri Lanka	1	5
Sweden	2	5
Switzerland	1	2
Taiwan	1	2
Tajikistan	2	3
Thailand	3	40
The Netherlands	2	3
Tunisia	3	3
Turkey	5	6
USA	5	8
Ukraine	1	5
Uzbekistan	3	2
Vietnam	1	2

Notes: Table 2.B.2 reports the isolated and relevant events merged with the GSOEP. An event is defined as relevant if in a given there is at least 1 more terror attack than the past country-specific 3 year monthly average number. A relevant event is isolated if individuals interviewed within the 90 days prior to the focal relevant terror event have not experienced any relevant terror event in the past 90 day.

Table 2.B.2: Effective sample: Isolated and relevant terror events

	Higher than average of last 3 years				
	Gender	Age	YSM	Marital status	Child
	(1)	(2)	(3)	(4)	(5)
Post-Terror	-0.041	-0.502	-0.017	0.003	-0.070
	(0.048)	(1.180)	(0.979)	(0.043)	(0.045)

	Low sec. educ. or <	Upper sec. educ.	Post-sec. educ.	Higher educ.	
	(1)	(2)	(3)	(4)	
Post-Terror	-0.015	-0.009	0.039	-0.014	
	(0.041)	(0.045)	(0.043)	(0.045)	
Observations	6604	6604	6604	6604	6604
Origin C. x Year FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
State of Residency FE	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$
Standard Errors in parenthesis clustered at the Country x Year x Month level
Notes: Table 2.B.3 reports the estimated coefficients of a regression of each characteristic on the treatment status (i.e. interviewed after a terrorist event in the home country).

Table 2.B.3: Balance test (GSOEP), terror events, 90 days bandwidth

	Mean	Percentile						
		5	10	25	50	75	90	95
PSI prev. year	36.477	2.857	14.762	22.275	34.286	50.000	63.333	68.269
PSI mean prev. 3 years	37.728	2.857	14.603	20.063	30.490	57.203	75.661	77.648
Mean monthly terror prev. year	19.030	0.000	0.000	0.167	0.500	3.583	12.583	281.917
Mean monthly terror prev. 3 yrs	19.027	0.000	0.000	0.139	0.889	3.083	16.389	279.667

PSI refers to the Political Stability Index, which ranges from 0-100. Mean monthly terror refers to the mean number of terror attacks in one month

Table 2.B.4: Distribution of the political stability index and mean month terror events

Panel A: 30 days	Higher than average of last			Higher than average of last		
Bandwidth	5 years	4 years	3 years	5 years	4 years	3 years
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Terror	0.328***	0.322***	0.329***	0.322***	0.321***	0.324***
	(0.047)	(0.050)	(0.049)	(0.043)	(0.047)	(0.044)
Observations	1915	2056	2671	1915	2056	2671
Panel B: 60 days	Higher than average of last			Higher than average of last		
Bandwidth	5 years	4 years	3 years	5 years	4 years	3 years
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Terror	0.147**	0.202***	0.112***	0.146**	0.207***	0.118***
	(0.065)	(0.041)	(0.029)	(0.065)	(0.041)	(0.029)
Observations	3712	4078	4886	3712	4078	4886
Panel C: 90 days	Higher than average of last			Higher than average of last		
Bandwidth	5 years	4 years	3 years	5 years	4 years	3 years
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Terror	0.074**	0.083**	0.122***	0.068*	0.080**	0.123***
	(0.036)	(0.037)	(0.030)	(0.037)	(0.037)	(0.030)
Observations	5328	5790	6604	5328	5790	6604
Origin country x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE,	Yes	Yes	Yes	Yes	Yes	Yes
State of Residency FE	Yes	Yes	Yes	Yes	Yes	Yes
Indiv. Controls	No	No	No	Yes	Yes	Yes

Standard Errors in parenthesis clustered at the Country x Year x Month level, *p<.1; **p<.05; ***p<.01
Notes: Table 2.B.5 displays the coefficients from the estimation of Equation 2.2 where the outcome is "Remain permanently in Germany". FE refers to fixed effects. Individual controls include age, gender, years since migration and its square, marital status, educational achievement, and children.

Table 2.B.5: Terror events and intentions to remain in Germany using different bandwidths

	Treated mean	Control mean	Unemp. with terror Coef.
Middle education	0.289	0.341	0.001
			(0.001)
High education	0.060	0.092	0.002
			(0.001)
Age	36.748	37.401	-0.000***
			(0.000)
Female	1.361	1.431	0.002**
			(0.001)
Years since mig. at unemp.	12.915	9.548	0.001***
			(0.000)
Ln wage bfu	3.330	3.374	-0.000
			(0.000)
Ln firm size bfu	3.676	3.641	0.000
			(0.000)
Observations	15299	202439.00	217738
Year FE			Yes
Month FE			Yes
LLM FE			Yes
C. Origin x State FE			Yes

Robust Standard Errors in parenthesis, *p<.1; **p<.05; ***p<.01
Notes: Figure 2.B.6 reports the estimated coefficients and robust standard errors in parenthesis for regressions using entering unemployment with a terror event as an outcome. The terror indicator is defined based on different levels of affected individuals in the home country in the same month when immigrants register as unemployed. FE refers to fixed effects.

Table 2.B.6: Balance in covariates among unemployed immigrants

Chapter 3

Barriers to humanitarian migration, victimization and integration outcomes: Evidence from Germany[1]

Teresa Freitas Monteiro (HU Berlin and IAB)
Lars Ludolph (OECD)

Abstract: *Asylum seekers who migrate from developing countries to Europe frequently experience victimization events during their journey. The consequences of these events for their economic integration into destination countries are not yet well explored. In this paper, we analyze how victimization during asylum seekers' journey affects their economic integration into Germany by using survey data collected in the aftermath of the 2015 refugee crisis. Our data allow us to account for the exact timing and geography of migration such that samples of physically victimized and nonvictimized refugees are balanced along a wide range of group-level characteristics. We then show that, vis-'a-vis nonvictimized refugees, refugees who were physically victimized during their journey to Germany favour joining the labour force and taking up low-income employment rather than investing in host country human capital. After ruling out a range of alternative mechanisms, we place our findings into the psychology and experimental economic literature and provide evidence that experiencing physical trauma in vulnerable situations results in a "loss of future orientation" or "impatience" among victimized refugees, which leads them to discount future payoffs more heavily.*

[1]This chapter is also part of Lars Ludoplh's PhD Thesis at the London School of Economics. Teresa acknowledges support from the European Union's H2020 research and innovation programme funding under the Marie Skłodowska-Curie grant agreement No. 765355. The authors are grateful for helpful suggestions and constructive comments from Achim Ahrens, Cevat Aksoy, Herbert Brücker, Riccardo Crescenzi, Timo Hener, Nancy Holman, Yuliya Kosyakova, Angela Kunzler, Markus Nagler, and Olmo Silva. We also thank the participants at the Oxford V Workshop on Migration, Health and Well-being, UCL-ETH Workshop, the 11th Annual International Conference on Immigration in OECD Countries, the IAB-ECSR conference "Refugee Migration and Integration Revisited: Lessons from the Recent Past", the Migration, Health and Integration Symposium at Maastricht U., the EuHEA PhD Conference, the PhD seminar of the LSE and the seminars of Potsdam U. and the Maastricht School of Governance. All errors and omissions remain our own.

3.1 Introduction

One of the key features of humanitarian migration flows from developing to developed regions of the world is the significant risk that these journeys entail for individuals who embark on them. According to the International Organization for Migration's (IOM's) Missing Migrant database, approximately 15,000 migrants perished in the Mediterranean Sea alone while trying to reach the territory of European Union (EU) member states between 2015 and 2019. Asylum seekers who survive the perilous journey often do not make it to their destination unscathed; they are subjected to violent acts on their journey carried out by escape agents and border enforcement agencies, with detrimental consequences to their physical and mental health (Arsenijević et al., 2017; Albahari, 2018; Arsenijević et al., 2018). Against the backdrop of a subdued economic and societal integration of newly arriving humanitarian migrants in the EU (Brell, Dustmann and Preston, 2020), the potentially negative consequences of these victimization events for the future life trajectories of affected individuals – and, thus, the welfare of host countries – has increasingly found its way into the political debate.

In this paper, we analyze how victimization during asylum seekers' journey affects their economic integration into Germany, which is the main destination country of asylum seekers in the EU. To study this link, we deploy novel refugee survey data collected from 2016 to 2018, which follows the large inflows of asylum seekers into the country. We use these data to construct a physical and financial victimization indicator for each refugee based on detailed questions regarding the adverse events that these individuals experienced on the journey to Germany. We then study the effect of victimization events on refugees' economic activity, employment, wages and participation in host country education.

Our identification strategy relies on the plausible quasi-random nature of victimization events along the journey of asylum seekers. We identify the following four sources of omitted variable bias when linking victimization to economic integration outcomes: selection bias at the origin; survivor bias that we observe in the sample of arrivals; the unobserved ability to navigate the journey to safety that could determine both the likelihood of victimization and integration outcomes at the destination; and the misreporting of victimization events. To address the concerns about selection bias at the origin and survivor bias, we limit the variation in the data to narrowly defined fixed effects categories by using detailed information on the timing and geography of migration. We restrict the variation to narrowly defined migration route fixed effects and the month-year of departure fixed effects interacted with both country of origin fixed effects and the month-year of arrival fixed effects. In our setting, a further concern is that an unobserved ability to navigate the journey could determine the likelihood of victimization and affect integration outcomes at the destination. We address this concern in several ways. First, if cohorts that migrate during times of higher victimization risk do so due to their better (unobserved) ability to navigate the journey, by using time of departure times country of origin fixed effects, we control for different victimization risk levels. Second, we review qualitative evidence on the victimization events along the main migration routes used by the largest refugee groups in our sample (Syrians, Iraqis, Iranians, Afghanis and Pakistanis between 2013 and 2017). International organizations and local nongovernmental organizations document many violent acts targeted at asylum seekers along the

main migration routes[2] that are carried out by state authorities, criminal gangs and escape agents. The available evidence suggests that these acts of violence are largely unpredictable for migrants who navigate unknown geographical territory[3]. Third, we can condition all our estimates on a large set of individual-level characteristics that include pre-migration information on education, employment, wealth and knowledge of a foreign language (characteristics that capture pre-migration ability). We show that once the geography and the timing of migration are accounted for, there is little difference in observable characteristics between the physically victimized and the nonvictimized. Fourth, to further mitigate concerns related to unobserved ability bias, which our pre-migration controls might not be capturing, we use a coefficient stability test developed by Oster (2019) to provide an estimate of the relative importance of unobserved factors compared to the observed factors in our regressions. These tests show that the importance of unobserved factors would have to be multiple times higher than the large set of observables for the true estimated effects in our main regressions to be zero. Fifth, we deviate from the theory-based selection of control variables and deploy a postdouble selection LASSO, which is a data-driven machine learning technique, to select all potentially relevant survey information and to allow for all interactions and nonlinearities between individual characteristics and migration and geographical fixed effects. Finally, to rule out the misreporting of victimization events, we conduct tests on the sample of refugees who agreed to answer journey-related questions, which shows that neither the willingness to answer nor social desirability are likely to skew the obtained results.

Our results show that physically victimized refugees are less likely to invest in host country education but more likely to participate in the labor force and take up employment faster than nonvictimized refugees. This leads to the counterintuitive finding of a higher employment rate among physically victimized refugees vis-à-vis other refugees in the early years after arrival in the host country; this adjusted gap reaches 3.4 percentage points 31 months into refugees' stay in Germany. We show that the higher employment among the physically victimized relative to nonvictimized and financially victimized migrants is driven by marginal and part-time employment and, thus, jobs characterized by a relatively lower income level. These results are robust to different specifications and alternative constructions of the victimization indicators.

We draw on evidence from the sociology, psychology, and economics literature to conceptualize our findings. Evidence from sociology and psychology documents a "loss of future orientation" caused by potentially traumatic victimization events (Beiser, 1987; Hauff and Vaglum, 1993a; Hunkler and Khourshed, 2020; Sagbakken, Bregård and Varvin, 2020). As physically traumatized refugees adopt a more pessimistic outlook on life and discount their future more heavily, they tend to invest less in host country-specific education and are more likely to take up low-skilled employment soon after arrival. Similarly, the experimental economics literature shows that time preferences can be affected by extreme events linked to violence, which makes victimized individuals more impatient in their decision-making (Voors et al., 2012; Callen et al., 2014; Jakiela and Ozier, 2019; Brown et al., 2019). Although we cannot directly measure time preferences, we draw our conclusions based on two findings.

[2]Most refugees in our sample use the Eastern Mediterranean route followed by the Balkan route.

[3]This is true for the migration routes used by the wave of refugees that we analyze here. The situation for asylum seekers from the Horn of Africa is considerably different.

First, we can closely approximate time preferences through a self-reported "feeling under time pressure" variable that we construct based on the first time that individuals are observed in the sample such that reverse causality can be ruled out. Second, we rely on a revealed preferences argument and rule out several competing theories that could explain our main findings. These range from institutional mechanisms built into German asylum procedures to mechanisms related to financial hardship, an alteration in risk preferences and potential behavioral changes caused by victimization events.

Our study adds to the literature in a number of ways. We primarily contribute to the literature that links refugee victimization to their economic behavioral response in the host country (Couttenier et al., 2019; Hunkler and Khourshed, 2020; Hauff and Vaglum, 1993a). Unlike previous literature, our data allow us to explicitly focus on what asylum seekers endure during their journey as opposed to their country of origin, an important distinction for the design of asylum policies. Since the victimization of asylum seekers is interconnected with the choice of external border policies (Arsenijević et al., 2017; Arsenijević et al., 2018), we further contribute to the growing literature on how policies specific to asylum seekers shape their labor market integration (Damm, 2009; Battisti, Peri and Romiti, 2022; Hainmueller, Hangartner and Lawrence, 2016; Marbach, Hainmueller and Hangartner, 2018; Zwysen, 2019). One of the main takeaways of our study is that rapid labor market integration as a general success metric for integration outcomes should be treated with care; higher victimization rates may contribute to a relatively swift uptake of employment, but this nevertheless distorts long-run labor market outcomes. We further add to the recently developing stream of literature that links crime victimization to labor market outcomes more generally (Bindler and Ketel, 2019; Ornstein, 2017; Velamuri and Stillman, 2008). We show that this link is context-specific and depends on the stage of life of the victimized. Refugees face the decision to invest in host country education or join the labor force to take up low-income employment (Duleep and Regets, 1999; Cortes, 2004). Such a choice set most closely resembles that of adolescents and young adults. For these groups, exposure to violence has indeed been linked to lower educational investment (Stoddard et al., 2015) and a general loss of future orientation (Ramos et al., 2013; Monahan et al., 2015; Schmidt, Zimmerman and Stoddard, 2018). Finally, we add to the much broader literature on violence and human-capital investment decisions by providing further evidence that experiencing traumatic events lowers the willingness to invest in education (Blattman and Annan, 2010; Shemyakina, 2011; Leon, 2012; Akbulut-Yuksel, 2014; Koppensteiner and Menezes, 2021).

The remainder of this paper proceeds as follows. Section 3.2 discusses in more detail the conceptual framework that links victimization experiences to economic activity outcomes in the destination country. Section 3.3 identifies our data sources and provides the context of the empirical setting. Section 3.4 introduces the estimation strategy used and our approach to addressing econometric challenges in detail. Section 3.5 shows the main results, and in Section 3.6, we test alternative hypotheses that could explain our findings. Section 3.7 provides a concluding discussion.

3.2 Outline and Framework

Studies on the general population find negative consequences of victimization events, such as robbery or rape, on labor force participation, employment, earned income and increased welfare dependency among those affected (Bindler and Ketel, 2019; Ornstein, 2017; Velamuri and Stillman, 2008). A large body of research, primarily

conducted in developing countries, further documents distortions to human capital investment decisions following potentially traumatic events in conflict-related or high-crime settings (Blattman and Annan, 2010; Shemyakina, 2011; Leon, 2012; Akbulut-Yuksel, 2014; Koppensteiner and Menezes, 2021). The decline in health and mental well-being following traumatic events (Dolan et al., 2005; Mahuteau and Zhu, 2016; Johnston, Shields and Suziedelyte, 2018) is one likely mechanism behind the link between victimization and economic choices.

However, the situation of refugees who arrive in their host country is not easily comparable to that of the general population. Forcibly displaced migrants start their economic activity trajectory at zero in their host country, and most originate from less developed countries. Educational attainment is not regarded as equivalent to education obtained in economically advanced countries (Ludolph, 2023), and in many cases, refugees lack proof of their formal degrees or previous work experience. Once refugee status is received in the host country, refugees face the decision to either i) join the labor force immediately, accept a discount on their human capital and take up low-skilled employment or ii) invest in host country-specific human capital to have access to better-paid employment in the future (Duleep and Regets, 1999; Cortes, 2004). In the general population, such a choice set resembles that of adolescents and young adults. Within this group, exposure to violence is indeed associated with lower educational investment (Stoddard et al., 2015) and a general loss of future orientation (Ramos et al., 2013; Monahan et al., 2015; Schmidt, Zimmerman and Stoddard, 2018).

Evidence from the sociology and psychology literature supports the view that victimization experiences have similar effects on future-oriented planning among refugees. Refugees victimized during the journey have worse mental health (Hauff and Vaglum, 1993b) and are equally or more likely to be in the labor force (Hauff and Vaglum, 1993a; Hunkler and Khourshed, 2020), but they tend to invest less in host country-specific education (Hauff and Vaglum, 1993a) than nonvictimized refugees. A potential explanation for favoring early employment over long-term educational investment is that refugees who went through extreme events while fleeing their country have a shortened sense of their future (Beiser, 1987).

These findings that relate traumatic events to a lack of future-oriented planning among the affected find further support in the economics literature. Although classic economic models assume stable preferences over time (Stigler and Becker, 1977), recent experimental studies suggest that individuals' risk aversion and time preferences can indeed be affected by extreme events linked to violence (Voors et al., 2012; Callen et al., 2014; Jakiela and Ozier, 2019; Brown et al., 2019) and health shocks (Decker and Schmitz, 2016), among others.[4] Time preferences have, in turn, been found to affect human capital acquisition among younger individuals (Sutter et al., 2013; Cadena and Keys, 2015; Kemptner and Tolan, 2018).[5]

[4]Recent work has also found that time and risk preferences can be affected by natural disasters (Eckel, El-Gamal and Wilson, 2009; Page, Savage and Torgler, 2014; Callen, 2015; Cameron and Shah, 2015; Cassar, Healy and Kessler, 2017; Hanaoka, Shigeoka and Watanabe, 2018; Beine et al., 2020) and financial and macroeconomic shocks (Guiso et al., 2018; Jetter, Magnusson and Roth, 2020; Kettlewell, 2019).

[5]Our analysis relates to this literature by indirectly measuring the time preferences of victimized versus nonvictimized individuals. This interpretation assumes that individuals reveal their time preferences by engaging in certain activities (DellaVigna and Paserman, 2005). Individuals who attach more value to long-term rewards are more likely to pursue activities that entail an immediate cost (such as investing in human capital) but that have

Based on the reviewed literature, we expect asylum seekers victimized during their journey to Europe to have a shortened sense of future compared to nonvictimized asylum seekers. We also expect victimized asylum seekers to experience lower mental health upon arrival. Taken together, the expected effect of victimization on labor market outcomes is a priori ambiguous. On the one hand, due to a "loss of future orientation" victimization during the journey may lead refugees to favor joining the labor force and taking up low-income employment rather than investing in the host country's human capital compared to nonvictimized refugees.[6] On the other hand, due to their relatively poorer mental health, victimized refugees are expected to be less attached to the labor market and invest less in education than nonvictimized refugees.

Since both mechanisms may be at play simultaneously, we posit that the direction of the estimated effect of victimization on short-term labor market outcomes will capture whichever channel is stronger. If the "loss of future orientation" effect is stronger, then we expect to find higher labor force participation rates, increased uptake of lower-quality employment, and a lower propensity to invest in host country education among victimized refugees. If the negative effect of victimization events on mental health is stronger, then we expect to see lower labor force participation among victimized refugees.

Our empirical approach, therefore, proceeds by first looking at the effect of victimization on labor market outcomes, and investment in host country education. We then test the competing mechanisms - altered time preferences and a decline in mental health - as potential explanations for our findings. We note that our approach to testing the proposed mechanisms has two limitations. The first limitation pertains to the measurement of time preferences, which we do not observe directly in the data. We address this limitation in two ways. First, in Subsection 3.6.1, we approximate time preferences closely through a self-reported "feeling under time pressure" variable.[7] We measure this variable shortly after arrival such that reverse causality can be ruled out. Second, we rely on a revealed preferences argument and posit that by making certain economic choices, individuals indirectly reveal their time preferences (DellaVigna and Paserman, 2005). In this way, we test a number of alternative hypotheses that conceivably link refugees' victimization during their journey to early integration outcomes similar to an alteration of time preferences. These hypotheses relate to potentially altered risk preferences, the institutional setting in Germany, potential differences between the victimized and nonvictimized in financial hardship and refugees' intentions to stay in Germany over the long term. We discuss these in detail in Section 3.6. The second limitation pertains to the detail with which our approach unveils the underlying causal chain. Our data do not allow

delayed payoffs (access to higher-quality employment in the future). On the other hand, impatient individuals are more likely to engage in activities with immediate benefits (such as low-income employment) and delayed costs (a lack of access to higher quality employment in the future).

[6]Impatient individuals are more likely to engage in activities with immediate benefits, such as low-income employment, and delayed costs, such as a lack of access to higher-quality employment in the future.

[7]This approximation of time preferences is based on contributions in the psychology literature that have shown a link between time preferences and individuals' perceptions of time. Impatient individuals who discount future payoffs tend to experience a slower passage of time, are less comfortable waiting and tend to overestimate their waiting time (Wittmann and Paulus, 2008; Wittmann et al., 2015; Jokic, Zakay and Wittmann, 2018).

us to unambiguously infer if the decline in mental health causes a potential loss of future orientation or if the potentially traumatic experience directly changes time preferences.[8]

3.3 Data, definitions and background

3.3.1 Data and definitions

IAB-BAMF-SOEP refugee survey

The primary data source for our analyses is the Institute for Employment Research (IAB)-Federal Office for Migration and Refugees (BAMF)-German Socio-Economic Panel (SOEP) refugee survey.[9] The IAB-BAMF-SOEP refugee survey is an extension of the established German Socio-Economic Panel (GSOEP) and is designed for the population of asylum seekers and refugees in Germany. The sample was drawn from the German Central Register of Foreign Nationals (AZR), which makes the survey representative of asylum seekers arriving in Germany since 2013, that is, in the aftermath of the surge of refugee migration to Europe. The survey has a panel structure with interviews conducted in three waves in 2016, 2017 and 2018 with a total of 6,763 individuals. For details on the survey's design, methodology, and response rate, see Kroh et al. (2017).

The survey provides a wide range of pre- and postmigration information and detailed individual and household characteristics. Most importantly, the first-time respondents are interviewed (on average 18 months after migrating), they are asked detailed questions about the experiences that they went through during the journey from their country of origin to Germany. A total of 3,742 individuals, 55.2% of the total sample, agreed to provide information on these experiences (we address this issue in section 3.3.2). We have all relevant information, including all necessary control variables and additional outcomes, for our effective working sample that consists of 3,004 individuals aged between 18 and 65 years.

Among the questions posed, our main interest lies in the survey question 'During your journey or escape, did you experience one or more of the following?' which allows respondents to choose one or more answers from a list of negative experiences. Based on their responses, we create a binary physical victimization indicator that takes the value of one if an individual was subjected to sexual abuse, physical attacks, incarceration or a shipwreck (or any combination of these). We further create a binary financial victimization indicator that takes the value of one if an individual was subjected to financial fraud, extortion, robbery or blackmail (or any combination of these). The reason we split the victimization indicator into one that captures the experience of more severe physical harm and one that captures financial harm is twofold. First, recent evidence on the link between crime victimization and labor market outcomes shows that the physical victimization experience has stronger adverse labor market consequences for the affected (Bindler and Ketel, 2019). Second, unlike

[8]Throughout the paper, we use "loss of future orientation" and "impatience" interchangeably. The term "time preferences" serves as a neutral term.

[9]This study uses the factual and anonymous data of the IAB-BAMF-SOEP Survey of Refugees, waves 1-3. Data access was provided via a Scientific Use File supplied by the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB). DOI: 10.5684/soep.iab-bamf-soep-mig.2017.

physical victimization, financial victimization during asylum seekers' flight to safety may affect labor market outcomes through the need to recover financial losses once the destination is reached. We are able to capture this mechanism in our data. Table 3.3.1 shows the summary statistics for the two victimization indicators.

Variable	Mean	SD
Exper. robbery	0.133	0.340
Exper. extortion	0.151	0.358
Exper. fraud	0.282	0.450
Financial victimization	0.384	0.486
Observations	3004	

Variable	Mean	SD
Exper. sexual harassment	0.018	0.132
Exper. a shipwreck	0.137	0.344
Exper. a physical attack	0.148	0.356
Exper. incarceration	0.198	0.398
Physical victimization	0.363	0.481
Observations	3004	

Table 3.3.1: Physical and Financial Victimization indicator

We note that individuals may experience both financial and physical trauma by design. Reassuringly, the correlation between these two (r=0.326) is sufficiently low to not be a cause for concern in our regression analyses. We further note that some migrants experienced more than one victimization event, but we nevertheless modeled our preferred indicators as binary for two main reasons. First, the majority of migrants experienced one victimization event. Only 12.% of all individuals in our sample experienced more than one physical victimization event, and 15.4% experienced more than one financial victimization event. Second, there is no clear guidance in the literature on the correct functional form of the relation between our outcomes of interest and multiple victimization events that individuals experienced on their journeys, which lasted 42 days on average. We explore different constructions of the victimization indicators in Appendix 3.O, where we consider both a discrete and a continuous measure of the number of victimization events.[10]

For the economic integration outcomes, our main interest lies in the three measures of labor force participation, education and training, and employment. We complement our main analyses with a more detailed analysis of employment, which we split into full-time, part-time and marginal employment, and net monthly income.[11] The economic integration outcomes are measured in the last interview, which is 31 months after arrival on average.

One of this study's shortcomings is that we cannot directly measure time preferences. Nevertheless, we can provide evidence in favor of the time preference channel by analyzing an additional outcome related to individuals' *perceptions* of time. Contributions in the psychology literature have shown that time preferences affect how individuals perceive time itself. The link is intuitive; impatient individuals who discount payoffs in the future tend to experience a slower passage of time, are less comfortable waiting and tend to overestimate their waiting time (Wittmann and Paulus, 2008; Wittmann et al., 2015; Jokic, Zakay and Wittmann, 2018). Wittmann et al. (2015) further shows that the feeling of being under time pressure is directly linked to frequently thinking about adverse events that were experienced in the past. We measure time perception through the survey question, "How often in the last four weeks did you feel rushed or under time pressure?" We invert the original scale such

[10] In Table 3.F.3 in Appendix 3.F, we provide statistics of the victimization rates across the different migration cohorts, main countries of origin and main migration routes.

[11] Summary statistics for these measures are shown in Table 3.A.1 of Appendix 3.A for the last observation available for each individual in the panel.

that 1 corresponds to "Never" and 5 corresponds to "Always".[12]

Drawing on the psychology and health economics literature reviewed in Section 3.2, we use life satisfaction and self-assessed health measured on a scale from 1 to 10 (with 10 being the highest value) as our primary outcomes of the mental health effect of victimization (Johnston, Shields and Suziedelyte, 2018).[13] [14] We measure time pressure, mental health and life satisfaction in the first interview.

IAB integrated employment biographies

We use the IAB integrated employment biographies (IEB) to complement the survey employment questions with more reliable individual administrative records. The IEB data consist of all individuals in Germany who are characterized by at least one of the following employment statuses: employment subject to social security; a marginal part-time job; benefit recipient; officially registered as job-seeking; or (planned) participation in programs of active labor market policies[15]. The IEB data form a comprehensive dataset with daily precision and very little attrition.

The IEB data can be linked to only 66% of our original sample[16], and we therefore rely on the employment outcomes from the survey as our primary data source. Nevertheless, the more precise IEB job market data allow us to add some further suggestive evidence.[17]

Further data sources

We further link the IAB-BAMF-SOEP survey data to the Uppsala Conflict Data Program and Syrian Shuhada Martyr Revolution database at the province-month level. We use these datasets to construct a measure of conflict intensity before migration. An asylum seeker is considered to have migrated from a province with "no conflict," "low conflict intensity," or "high conflict intensity" based on the relationship between conflict-related fatalities twelve months before departure from the province (within country) of origin and the median conflict intensity across all provinces (Aksoy and Poutvaara, 2021). This measure of conflict intensity is calculated based on within-country conflict variation over time. Further details about the calculation of the conflict intensity measure and its summary statistics are shown in Appendix 3.B.

[12]The full scale is as follows: 1) Never; 2) Almost never; 3) Sometimes; 4) Often; and 5) Always.

[13]We complement this measure with a mental component score (MCS) and a physical component score (PCS) detailed in Section 3.D of the Appendix.

[14]The health measures are summarized in Table 3.A.1 of Appendix 3.A for the first observation available of each individual, together with the backward-reported measures of premigration life satisfaction and premigration self-reported health.

[15]The employer determines the social security notifications for each employment relationship

[16]Individuals must give written consent to be linked; exploring who gives consent is beyond the scope of this study.

[17]First, the IEB data provide us with the exact dates of the first formal jobs that refugees took up in Germany, which allows us to address in greater detail the question of the timing of employment uptake. Second, the linkage enables us to follow refugees even when they leave the survey, which mitigates attrition concerns. Finally, the IEB data allow us to obtain information on refugees' presurvey (un)employment histories.

3.3.2 Reliability of self-reported victimization

One issue when using sensitive survey data on victimization concerns the reliability of the responses. This subsection summarizes our concerns regarding four particular sources of bias and the tests that we conduct to rule them out. We outline these concerns, empirical specifications and results in greater detail in Appendix 3.E. Reassuringly, all our results point toward reliable self-reported victimization responses.

The first concern relates to a potential link between the employment status of respondents and the willingness to answer questions on victimization events. We note that victimization events were part of the survey only in the first interview, when the average time since migration was seventeen months and only 9.3% of the refugees in our sample were employed (in the last interview, 20.9% were employed). Thus, employment status is unlikely to significantly affect the willingness to answer the victimization question. To further mitigate this concern, we use our full sample of respondents who agreed and did not agree to answer the victimization-related questions[18] (5543 individuals) and show in Panel A of Table 3.E.1 in Appendix 3.E that employment status at the first interview has no significant effect on the willingness to answer the victimization questions.[19]

The second potential source of bias relates to a potential systematic misreporting of victimization events. As we explain in Section 3.E in the Appendix, the structure of the survey largely alleviates this concern. To further strengthen our argument, we show in Panel B of Table 3.E.1 in Appendix 3.E that the willingness to respond to journey-related questions in the first survey wave is not a significant predictor of individuals' employment status in the last available survey wave.

A third and related potential problem could apply if only the least traumatized individuals agreed to reply to the journey-related questions. In Panel A of Table 3.E.1 in Appendix 3.E, we show that the level of mental health in the first interview does not affect the willingness to reply to the journey-related questions.

Finally, in Appendix 3.P.1, we further address the concern that some respondents may have provided answers that they deemed favorable regarding their chances of receiving protection by showing that our results hold for Syrian refugees. Syrian refugees received protection with near 100% certainty and are therefore unlikely to have misreported their victimization experiences in an attempt to evoke sympathy.

3.3.3 Context: Victimization along the main refugee routes

For the purposes of our study, we are interested in the arbitrariness of physical and financial victimization events along the main migration routes with respect to observable individual-level characteristics once we account for geographical and time factors. Victimization along migration routes differs from victimization events observed in cities.[20] Although a fair share of crimes committed in a city deliberately target a specific individual or property, victimization events along migration routes are unlikely to be premeditated with respect to a specific person. The paths that

[18] And for whom the full set of controls is available.

[19] The empirical specification is outlined in Section 3.3.4 and considers a series of pre- and postmigration characteristics and time and geographic controls.

[20] City crime has been studied in more detail in the economics literature.(Bindler and Ketel, 2019; Mahuteau and Zhu, 2016; Ornstein, 2017; Velamuri and Stillman, 2008).

asylum seekers take are not a day-to-day activity that they perform routinely. Indeed, most asylum seekers take a migration route only once and are navigating unknown territory. As we will explain below, border patrols along the Balkan route are actively seeking to catch refugees attempting to cross borders - irrespective of the individual characteristics of refugees.

Several reports from local nongovernmental organizations (NGOs), investigative journalists, the United Nations High Commissioner for Refugees (UNHCR) and Human Rights Watch (HRW) document widespread violence along migration routes to Europe, with violent acts carried out by state authorities, criminal gangs and sometimes smugglers. These reports use qualitative and quantitative data, and provide valuable information regarding asylum seekers' journeys.

To narrow the scope of interest, we focus on the main cohort, countries of origin and migration routes taken by asylum seekers to reach Germany.[21] In our IAB-BAMF-SOEP working sample, the largest arrival cohort reached Germany in 2015 (65% of the sample), with fewer refugees arriving between 2012 and 2014 (20%) and from 2016 to 2017 (14%). These numbers accurately reflect the official statistics on the entry of asylum seekers into Germany found on Eurostat. The first and largest group of asylum seekers are Syrians (63% of the sample), followed by Iraqis and Iranians (16%) and Afghanis and Pakistanis (10%).[22]

Asylum seekers from Syria, Iraq and Iran typically cross land borders into Turkey[23] and from there, follow the Eastern Mediterranean route (EMR), which runs from Turkey to Bulgaria or Greece either through the mainland or by boat. Afghanis and Pakistanis reach Turkey either through Iran or Lebanon (Crawley et al., 2016).[24].

Consistent with UNHCR data (UNHCR, 2017), approximately 69% of Syrians in our sample took the EMR (either by sea or land). This compares to 74% of Iraqis and Iranians and 64% of Afghanis and Pakistanis (Table 3.F.2 in Appendix 3.F). Once in Greece or Bulgaria, the most frequently used route by asylum seekers to reach Western and Northern Europe is the so-called Balkan route.[25] (IOM, 2015; UNHCR, 2017).

[21]We provide basic statistics regarding our groups of interest in Appendix 3.F.

[22]To facilitate our analysis, we aggregate the countries of origin into these three main groups, which cover 89% of our sample.

[23]Syria, Iraq and Iran border Turkey

[24]According to a UNHCR report that used data collected from 2015 to 2016 (UNHCR, 2017), the primary groups that use the EMR are Syrians, Afghanis, Iraqis, Pakistanis and Iranians. These comprise 94% of the total arrivals, which is similar to our IAB-BAMF-SOEP refugee survey sample (89%). A different survey project, MEDMIG, finds that the three main nationality groups that use the EMR are Syrians, Afghans and Iraqis and that 40% of the respondents experienced violence (Crawley et al., 2016)

[25]The Balkan route runs from North Macedonia through Serbia or Bosnia Herzegovina and then crosses into Hungary or Croatia. With the construction of border fences in these countries from 2015 to 2016, refugees later transited through other counties, such as Albania, Montenegro, Romania and Slovenia (UNHCR, 2018).

Source: UNHCR Desperate Journeys, 2016

The UNHCR, HRW and Amnesty International have documented widespread chain pushback[26] in Greece, Bulgaria, and along the Balkan route and a series of unlawful detentions within these countries (Redden, 2015; International, 2016; Balla, 2016; Banich et al., 2016; UNHCR, 2017; UNHCR, 2018). These detentions and pushbacks have been characterized by the frequent and arbitrary use of violence and by the appropriation of asylum seekers' financial resources by local authorities (Redden, 2015; International, 2016; Balla, 2016; Oxfam, Human Rights and Association, 2017; UNHCR, 2017). Authorities have used violence against both male and female asylum seekers, which is consistently characterized by physical abuse through the use of batons and by hitting and kicking (International, 2015; International, 2016; HRW, 2016; HRW, 2018a; HRW, 2018b; Tondo, 2018).[27]

As reported by InfoMigrants, a news and information site co-financed by the EU[28], local NGOs have "consistently documented the mass proliferation of torture and inhuman treatment during pushbacks at Europe's borders" and "the overwhelming and indisputable evidence shows how forced undressing, inhuman detention conditions and lengthy physical assaults are now so commonplace that it is hard to distinguish it from an official policy." (InfoMigrants, 2021) [29]

Despite the increasing amount of violence and pushback found in Greece and

[26]Migrants are dropped across the border or even obliged to walk back under the supervision of local police.

[27]Several accounts exist of migrants being stripped naked in freezing temperatures and beaten by local authorities in the different Balkan countries before being pushed back (International, 2019; Oxfam, Human Rights and Association, 2017; Tondo, 2018). In the Balkan countries, cases of sexual abuse and the use of electric shocks, pepper spray and the release of dogs on asylum seekers have been documented (HRW, 2016; Oxfam, Human Rights and Association, 2017). HRW has also documented a practice by the Hungarian police of placing plastic handcuffs on asylum seekers and forcing them through holes in razor wire fence, which creates several wounds (HRW, 2016).

[28]InfoMigrants is a collaboration led by three major European media sources: France Médias Monde, German public broadcaster Deutsche Welle, and Italian press agency ANSA

[29]The stance taken against refugees in some Balkan countries, such as Hungary, seems to be driven by a generalized xenophobia toward refugees and migrants (Crawley et al., 2016;

along the Balkan route, most asylum seekers still considered the route through Turkey and Greece to be less dangerous than traveling through Libya (Crawley et al., 2016). Indeed, according to UNHCR reports, until the end of 2016, most Syrians, Afghanis and Iraqis used the EMR. This has contributed to sustaining the flow of migrants through the Balkan route, which decreased only after the 2016 EU-Turkey deal.

Overall, the reports and news reviewed in this section document a widespread use of violence against asylum seekers, and there is no evidence that local authorities or criminal gangs target specific asylum seekers. Refugees who manage to cross borders seem to have done so by using similar means and routes as others.

3.3.4 Balance tests

The quantitative and qualitative evidence presented in Section 3.3.3 supports the idea that border patrols and local gangs in Greece and along the Balkan route target refugees who attempt to cross the border, irrespective of individual characteristics.

In Table 3.H.1 in the Appendix, we show a conditional balance test under the hypothesis that individual-level characteristics do not predict victimization events, conditional on migration timing and geography. To test this hypothesis, the physical and financial victimization indicators are regressed on a set of backward reported premigration indicators, conditional on their geographical origin, the time of migration (and their interaction term), and the migration route. Physical victimization is additionally conditioned on experiencing financial victimization, and vice versa. The outcomes of these regressions are displayed in Columns (1) and (2).

The results show a balanced sample for the physically victimized (Column (1)). We note that, on average, individuals with a university degree and those speaking German before migration are slightly less likely to experience physical victimization. Our estimations routinely control for these variables. Experiencing financial victimization correlates with the economic situation and employment levels before migration. Finally, health satisfaction before migration and willingness to take risks are further significant predictors of financial victimization, albeit in opposing directions. Accordingly, we conclude that although the sample of physically victimized migrants is balanced along a wide range of individual-level premigration characteristics once conditioned on geography and migration timing, financial victimization events seem to occur less randomly. Our data allow us to control for a large set of potentially confounding variables to mitigate this problem. We further deploy several additional tests, which are outlined in Section 3.4.3, to study the significance of unobserved factors that could bias our results.

3.4 Empirical strategy

The identification strategy in this study relies on the plausibility of the assumption that conditional on individual characteristics and migration and geographical factors, victimization is a quasi-random event. We start by describing our main specification in Section 3.4.1 to estimate the effect of the victimization of asylum seekers along the route to Germany on their economic integration outcomes. Sections 3.4.2 and 3.4.3 explain the extensions.

Assembly, April 2016; Deardorff Miller, 2018; Rankin, 21 May 2019). We provide some further details in Section 3.G.

3.4.1 Main specification

To identify the effects of potentially traumatic events that occur during the migration journey on economic integration outcomes, we start by estimating the following empirical model:

$$\begin{aligned} Y_{i,c,a,t,\mu,f} =& \gamma_1 PhysicalVictim_i + \gamma_2 FinancialVictim_i \\ &+ \zeta Baseline_{i,t} + \eta PreMig_{i,\mu} + \varphi PostMig_{i,t} \\ &+ Route_i + ConflictIntensity_{i,\mu} + \delta_f + \beta_a + \kappa_{c,\mu} + \epsilon_{i,c,t,\mu,f} \end{aligned} \tag{3.1}$$

where $Y_{i,c,t,\mu,f}$ captures the outcome of interest for individual i from country of origin c, interviewed at time t, who left the country of origin at time μ, arrived in year a and resides in German federal state f. Both γ_1 and γ_2 are the coefficients that capture the effect of physical and financial victimization on the outcome.

$Baseline_{i,t}$ is a vector of individual-level characteristics. It includes the age, age squared, and the gender of respondents. One concern in specification 3.1 is that γ_1 and γ_2 may be biased by an intrinsic ability to navigate difficult situations that could also affect the ability to succeed in the destination country. While the qualitative and quantitative evidence presented in Sections 3.3.3 and 3.3.4 supports the random nature of victimization events in the specific context we are studying, we further mitigate this concern by conditioning on a proxy for intrinsic ability using a vector of individual time constant premigration information, $PreMig_{i,\mu}$. These include information on the economic conditions of a respondent's household, knowledge of the German language, employment experience, education, and backwards reported measures of health and life satisfaction before migration. $PostMig_{i,t}$ is a vector of individual postmigration information that may affect the propensity to integrate into the destination country. It includes the number of months that a refugee has spent in Germany and its squared term, the asylum status measured at the time of the survey t, and whether an individual arrived in Germany alone or accompanied. For the labor market outcomes, we include a set of variables related to the residence of the spouse and the location of children.[30]

The categorical variable $Route_i$ indicates the migratory route taken, namely, the Eastern Mediterranean Sea, Central Mediterranean, Western Mediterranean, Eastern Mediterranean land, Eastern Land border, and traveling directly to Germany by plane routes; a final option is if no route information is available.[31] [32] $ConflictIntensity_{i,\mu}$ measures the conflict intensity in the province (within country) of origin around the time of migration as explained in Section 3.3.1 and Appendix 3.B. Controlling for conflict intensity accounts for selection into migration at different levels of push factors at the origin (Aksoy and Poutvaara, 2021; Guichard, 2020) and for the possibility that

[30]Premigration and postmigration variables are described in detail in Appendix 3.I. When the outcome is the (log of) income, we include an estimated Heckman correction term. We exclude the spouse's residence and the children's location to identify the first stage of the Heckman correction. Standard errors are obtained by using delete-cluster jackknife methods in these specifications.

[31]The procedure for the construction of this variable is detailed in Appendix 3.C.

[32]In theory, conditioning victimization estimates on the migratory route could constitute a bad control if the route taken was understood as a choice. This is unlikely to be the case because routes are distinguished only on a very high level, largely determined by the geography of the country of origin and partly determined by the time of forced displacement. Nevertheless, we show in Appendix 3.Q that all our main results do not depend on this choice.

the individual-specific response to victimization might depend on previous traumatic experiences (Yehuda, 2002; Breslau, Peterson and Schultz, 2008). δ_f is a fixed effects term that captures the German federal state where the refugee resides at the time of the survey, and β_a refers to the year of arrival fixed effects

An additional econometric concern in specification 3.1 is that the coefficients γ_1 and γ_2 are biased because of potential selection effects at the country of origin. In our setting, the selection at origin is related to the time-varying expected risk of victimization that may affect the decision to migrate.[33] For instance, cohorts that migrate during times of higher victimization risk may do so due to their better (unobserved) ability to navigate the journey. Victimization events occurring within such cohorts may then capture ability, while in other cohorts, victimization events may capture the lack of such ability.

Despite controlling for the route travelled and the conflict intensity at the region of origin around the time of migration in our benchmark specification, the concerns around self-selection into migration at the origin can be better approached by using a large set of fixed effects related to the country of origin and the granular time of migration. This approach limits the variation in our variables of interest to groups of migrants who chose to migrate at the same time and originate from the same place. The detailed information from the IAB-BAFM-SOEP survey allows us to add country-of-origin by year-month-of-migration fixed effects, $\kappa_{c,\mu}$, to equation 3.1. $\epsilon_{i,c,t,\mu,f}$ constitutes the error term of this specification. We label this specification "Benchmark specification" in our regression tables and refer to it as our preferred specification throughout this paper.

Since the survey has a longitudinal dimension, but our variable of interest is not time-varying, we estimate equation 3.1 in two ways. First, we estimate the model as a cross-section. When studying the effects of victimization on outcomes related to (mental) health, well-being and time perception, we use the first observation available for each individual. We do this because when the refugees were interviewed for the first time, they had spent only 19 months in Germany on average. Thus, negative experiences during the journey to Germany could still affect their mental well-being outcomes. Using the first observation also reduces the potential reverse causality problem of mental well-being and employment (Brown, Roberts and Taylor, 2010; Kassenboehmer and Haisken-DeNew, 2009); only 9.3% of our sample were employed in the month before the interview when first surveyed. We then use the last observation in the sample to study the effect of victimization on economic integration outcomes. At this point, the individuals had spent an average of 31 months in the country, and 20.9% were employed. The average difference between each individual's first and last observation is only 12 months. Therefore, concerns about potential sample attrition due to selective return migration are minimized, while the additional variation that we gain in our outcomes of interest is considerable.

Second, we exploit the panel variation in the data and estimate a (individual

[33]We note that evidence on the self-selection of forced migrants in the country of origin at different expected journey risk levels has only recently started to emerge in the academic literature. Aksoy and Poutvaara (2021) provide suggestive evidence that intended destinations change when country-specific risk levels are altered through stricter migration policies, with potential consequences for the cohort composition. The authors further show that higher conflict intensity at the origin leads asylum seekers to positively self-select with respect to education, particularly among female migrants. It follows that at a higher expected level of journey risk, which can also be understood as a higher migration cost, positive self-selection may become even more salient.

i) random effects model under the assumption that $corr(\epsilon_{i,c,t,\mu,a,f}, X) = 0$.[34] A large number of time-constant variables in the model, including the set of fixed effects related to the time of migration and the origin of individuals, makes this key assumption of a random effects model plausible in our setting (Wooldridge, 2010). We note that since all asylum seekers naturally start their stay in Germany as economically inactive and the likelihood of engaging in economic activity then increases over time, the panel estimates that capture the average effects over time are not directly comparable to the cross-sectional estimates based on only the final observation of each individual. The results of the panel data estimations are shown in Appendix 3.Q.2.

3.4.2 Survivor bias

Not all forcibly displaced migrants who decide to embark on the journey to Germany make it to their preferred destination. Changes across time in the journey's difficulty may have nonrandom effects on arrival cohort composition, even when narrowly conditioning on the selection at the origin at different points in time.[35] We refer to this empirical issue as survivor bias. In theory, such a change in the composition of asylum seeker arrival cohorts can influence not only the probability of victimization but also their performance in the German labor market.

To the best of our knowledge, no previous research exists that could inform our empirical strategy regarding survivor bias and the extent to which it is a concern in our setting. Empirically, we partially address the issue of survivor bias by deploying a large set of dyadic fixed effects (departure-arrival-origin). Thus, in addition to our preferred specification shown in equation 3.1, we estimate a model that includes the year-month of the arrival fixed effects, which is interacted with the year-month of departure and region of origin, $\varrho_{c,\mu,a}$.[36]

3.4.3 Further methods to address omitted variable bias

The rich set of background information available from the IAB-BAMF-SOEP survey data allows us to control for a wide range of time and geographical factors and a set of pre- and postmigration characteristics, which allow us to mitigate potential unobserved variable bias.

To further test the sensitivity of our results to the modeling choice and improve our proxy for ability, we extend the benchmark specification 3.1 by allowing for all possible interactions and nonlinearities between individual characteristics and the country of origin, time of departure, conflict intensity, route and cohort of arrival fixed effects. In this more demanding specification, we also include further potentially relevant information drawn from the survey data that captures the following: willingness to

[34]Thus, this specification assumes that the individual-specific residual is uncorrelated with the explanatory variables.

[35]If the selection of asylum seekers who eventually reach their targeted destination were a random subset of the individuals who initially decide to migrate there, then selection during the journey would not be an empirical concern when studying the effect of victimization on integration outcomes as long as self-selection at the origin is accounted for.

[36]Due to the inevitable loss in degrees of freedom, we include broader categories of migrants' origins in these interactive fixed effects, in addition to the country of origin fixed effects. The regions are Syria, Iraq or Iran; Afghanistan or Pakistan; and Sub-Saharan Africa and other countries of origin.

take risks; the use of an escape agent; the cost of the escape agent; the means of financing the escape journey; the means of transportation used to reach Germany; the self-reported reason for migrating; having stayed in another country for three or more months before coming to Germany; and the duration of the journey in days.[37]

A drawback of including such large sets of fixed effects and interaction terms in the model is a loss of statistical precision. To balance this trade-off, we use a least absolute shrinkage and selection operator (LASSO) to select variables that sufficiently improve the model fit to justify the reduction in degrees of freedom.[38] We follow Belloni, Chernozhukov and Hansen (2014) and use a postdouble selection LASSO (PDS) to find predictors of the selection equation and outcome equation, where the selection equation refers to the predictors of physical and financial victimization. A more technical explanation of these selection methods is provided in Appendix 3.J.

Despite conditioning all estimates on relevant premigration and selected postmigration variables and accounting for selection effects at various stages of the migratory journey, omitted variable bias may nevertheless be a concern. To obtain an idea of the severity of such potential bias, we follow Oster (2019) and analyze the sensitivity of our coefficients of interest to their conditioning on observables. If the coefficients are stable after the inclusion of observed controls, then we can consider this suggestive evidence that omitted variable bias is limited.[39] We report the estimated δ for our preferred specification in Section 3.5.2.

3.5 Results

In this section, we discuss the main results, which explore the effect of asylum seekers' victimization during the journey to Germany on labor market outcomes. Since the specification that uses the dyadic time of arrival fixed effects decreases the sample size but does not change any of the derived conclusions, we refer to the benchmark specification as our preferred specification. We present the estimated coefficients for all control variables included in our preferred specification in Appendix 3.L. To ease readability, when we refer to individual controls in the tables, we are bundling $Baseline_{i,t}$ $PreMig_{i,\mu}$, $PostMig_{i,t}$, $Route_i$, $ConflictIntensity_{i,\mu}$ and δ_f.

[37]The means used to finance the journey include the sale of assets, borrowing, savings and others. The means of transportation used to reach Germany include boat, car, foot, train or airplane travel. Self-reported reasons for migrating include persecution, discrimination, economic factors and others.

[38]Although the main strength of supervised machine learning methods such as the LASSO is prediction, they can be used to select control variables to address omitted variable bias when many potential controls are available (Ahrens, Hansen and Schaffer, 2020).

[39]We implement this methodology in our preferred specification and define a value for the hypothetical R-squared value of a fully specified model, which includes all relevant observed control variables (R_{max}). We choose a conservative approach and set a value of $R_{max} = 1.5\hat{R}$, where $\hat{R}$ is the R-squared value obtained from the estimated model. Using this methodology, we compute δ, which informs us about the relative importance of omitted variables compared to the observed variables that we condition our estimates on. For instance, a value of $\delta = 1$ means that unobserved factors would have to be as important as those that are observed for γ_1 and γ_2 of equation 3.1 to equal zero.

3.5.1 Labor market outcomes

Table 3.5.1 Panel A reports the effects of physical and financial victimization on overall economic activity, which is defined as refugees in the labor force or those who pursue host country-specific education. Column (1) refers to a specification using only the most basic controls: baseline individual characteristics, country of origin, route, year of arrival, and federal state of residency. Columns (2)-(4) refer to the specifications described in Section 3.4. We do not find a negative effect of victimization during the journey on economic activity in the cross-sectional regressions (1) to (4). In our preferred specification under Column (2), physical victimization has a positive effect on being economically active ($p<.05$). The findings suggest that the gap in economic activity between physically and nonphysically victimized individuals already increases at least 19 months after arrival. No such effect can be found for the financially victimized.

To better understand the drivers of this finding, Table 3.5.1 Panel B shows the results of the regressions of labor force participation on our victimization measures. The coefficients in Panel B show a strong and positive effect of physical victimization on joining the labor force across all specifications. In our preferred specification under Column (2), this effect is 6.1 percentage points ($p<.01$). The effect remains stable across specifications, with a slightly smaller magnitude in Column (4). Taken together, the results suggest that physically victimized individuals indeed join the labor force sooner upon arrival. We do not find the same association between financial victimization and labor force participation, where the estimated effect is close to zero across all specifications.

Table 3.5.1 Panel C shows the effects of victimization on pursuing host country education and training. By design, the results complement those of Panels A and B. Physical victimization significantly decreases the propensity to pursue host country-specific education or training across all specifications. In our preferred specification under Column (2), this negative effect reaches 3.1 percentage points, which is a sizeable decrease considering that the total share of refugees in our sample who pursue education or training stands at 8.4 percentage points 31 months after arrival (see Table 3.A.1). The coefficients estimated on the financial victimization indicator show no effects across all specifications.

	Basic	Benchmark	Dyadic FE	PDS
Panel A: LFP or Education	(1)	(2)	(3)	(4)
Physical victim.	0.0509***	0.0424**	0.0360**	0.0254
	(0.0168)	(0.0167)	(0.0153)	(0.0161)
Financial victim.	0.0187	-0.0111	-0.0156	-0.0124
	(0.0168)	(0.0161)	(0.0151)	(0.0157)
R-squared	0.0045	0.3174		
Panel B: LFP	(1)	(2)	(3)	(4)
Physical victim.	0.0631***	0.0608***	0.0505***	0.0367**
	(0.0172)	(0.0170)	(0.0156)	(0.0164)
Financial victim.	0.0188	-0.0152	-0.0174	-0.0168
	(0.0172)	(0.0165)	(0.0154)	(0.0161)
R-squared	0.0061	0.3219		
Panel C: Education and Training	(1)	(2)	(3)	(4)
Physical victim.	-0.0197*	-0.0306***	-0.0253**	-0.0191*
	(0.0108)	(0.0117)	(0.0105)	(0.0111)
Financial victim.	0.0156	0.0090	0.0094	0.0111
	(0.0111)	(0.0114)	(0.0106)	(0.0109)
R-squared	0.0013	0.2054		
Observations	3004	3004	3004	3004
Individual Controls	Some	Yes	Yes	Some
Year of arrival FE	Yes	Yes	No	Some
Country of origin	Yes	No	No	No
C.origin*Departure FE	No	Yes	Yes	Some
R.origin*Departure*Arrival FE	No	No	Yes	Some

Huber-White standard errors; *p<.1; **p<.05; ***p<.01

Notes: The dependent variable is binary and takes a value 1 for individuals in the labor force or education (Panel A), individuals in the labor force (Panel B) or those pursuing host country education or training (Panel C). LFP stands for labor force participation. Columns (1) to (4) use observations that correspond to the last interview conducted, which is 31 months after arrival on average. The term FE indicates fixed effects. PDS refers to the post-double-selection LASSO. Departure FE signifies the year-month of displacement from the home country, and Arrival FE identifies the year-month of arrival in Germany. C.origin is the country of origin. R.origin is the wider region of origin (Syria, Iraq or Iran; Afghanistan or Pakistan; and Sub-Saharan Africa and other countries of origin). The dyadic FE regressions are also estimated using LASSO such that the coefficients on the interactions R.origin*Departure*Arrival FE for which there are no individuals are assigned zero. The PDS is estimated for the same sample as the fixed effects regression but drops singleton observations.

Table 3.5.1: Economic Activity

Accordingly, our findings indicate that a physical victimization event i) increases the propensity to join the labor force early on and ii) decreases the propensity to pursue host country education and training. The results in the three panels of Table 3.5.1 suggest that being physically victimized leads refugees to favor joining the labor force early on their integration path and that this effect dominates the more general well-being-related effects that would likely lead to lower labor force participation. We interpret these findings as supportive of the "loss of future orientation" effect.

Appendix sections 3.P.1 and 3.P.2 show heterogeneous effects by main country of origin and gender, respectively. Our results do not seem to be driven by any particular group. As an additional robustness check, we include in Appendix 3.Q the outcomes of regressions that exclude individuals with missing pre-migration information and do not include any control variables. Despite considering these variables in the PDS regression, we explicitly show the results when including in our preferred specification covariates that could potentially be considered outcomes of victimization events (a willingness to take risks and resilience), and adding extra control variables (using a smuggler and having contracted debt) for all our main outcomes. The results for our main variables of interest remain robust across the different specifications.

In Section 3.O in the Appendix, we also show that the main results shown in this section are robust to different definitions of the victimization indicators. We construct both a discrete and a continuous measure of the number of victimization events. We further consider the panel structure of the data in section 3.Q.2.

The findings reported in Table 3.5.1 strongly support the idea that physical victimization reduces future-oriented thinking among affected individuals. To test this hypothesis further, we now turn to the expected consequences of early labor force entry; if the "loss of future orientation" was indeed a relatively stronger driving force within the physically victimized group, then we would expect a lower reservation wage within this group and, thus, a relatively higher acceptance of readily available low-quality employment among those who experienced physical victimization events on their journey to Germany.

Table 3.5.2 shows the results with different types of employment rates as the dependent variable for the full sample of refugees. We report only the results for our benchmark specification in the cross-section in all subsequent analyses.

Employment	Any employment	Full-time	Part-time or marginal	Log of income
	(1)	(2)	(3)	(4)
Physical victim.	0.0336**	0.0115	0.0221*	-0.1401
	(0.0170)	(0.0133)	(0.0132)	(0.1162)
Financial victim.	-0.0038	-0.0020	-0.0018	-0.0362
	(0.0164)	(0.0127)	(0.0127)	(0.1145)
R-squared	0.2647	0.2383	0.1581	0.4085
Observations	3004	3004	3004	543
Individual Controls	Yes	Yes	Yes	Yes
Year of arrival FE	Yes	Yes	Yes	Yes
C.origin*Departure FE	Yes	Yes	Yes	Yes

Huber-White standard errors; *p<.1; **p<.05; ***p<.01

Notes: The dependent variable is binary and takes a value 1 for employed individuals, with the regressions showing employment outcomes for any form of employment (Column (1)), full-time employment (Column (2)) and part-time or marginal employment (Column (3)). When employment is divided, the other types of employment are set to zero. All samples use observations that correspond to the last interview conducted, which is 31 months after arrival on average. The dependent variable in Column (4) is the log of income among employed individuals. The term FE indicates fixed effects. The term departure refers to the year-month of forceful displacement from the home country. C.origin is the country of origin.

Table 3.5.2: Employment

The results in Table 3.5.2 suggest that the higher labor force participation among physically victimized individuals is driven by an increased uptake of part-time and marginal employment vis-à-vis the nonvictimized. Column (1) reports the employment rate of physically victimized refugees for the last observation available of each individual, at an average duration of stay in Germany of 31 months. The rate is 3.4 percentage points higher than the employment rate among the nonvictimized at this point. In line with the "loss of future orientation" hypothesis, early employment uptake is characterized by the poor-quality jobs available to refugees. Around two-thirds of the higher employment rate among the physically victimized is explained by employment in part-time and marginal jobs (Column (3)). Less than one-third of the effect is explained by full-time employment, which is a magnitude that is no longer statistically distinguishable from zero at conventional levels (Column (2)). Column (4) is estimated only for the sample of employed refugees. Although imprecisely estimated, the results provide suggestive evidence that 31 months after arrival, these differences result in a 14% wage gap between the nonphysically victimized and the physically victimized. We note that this difference is likely to increase in the future when the nonphysically victimized complete their training and education.

The IAB-BAMF-SOEP refugee sample contains further information on the training requirements for a subset of 569 of the 751 employed individuals in our sample, as shown in Table 3.K.1 in Appendix 3.M. The tabulation shows that physically victimized individuals take up jobs with unskilled or semiskilled task requirements at a higher frequency. Row 1 of Table 3.K.1 shows that the higher share of physically victimized vis-à-vis the nonphysically victimized stands at 8.6 percentage points. In contrast, the share of physically victimized employed in jobs with skilled, complex and highly complex tasks is smaller. The analysis of job skill requirements further supports the idea that the faster employment uptake among the physically victimized is characterized by low-skilled employment.[40]

To shed more light on the timing of first employment in Germany, we turn to the linked employment biography data, which contain information on the date of first employment. Figure 3.5.1 shows the unconditional Kaplan-Meier curve of time to first employment, where failure is defined as obtaining employment, and the x-axis indicates the number of months since arrival in Germany. The analysis is based on a subsample of 1,625 survey respondents who gave their consent to be linked to administrative employment records. Of these individuals, 751 obtained employment at some point over the observed time period; we note that this share is larger than the 21.8% in our cross-sectional regressions. The difference is explained by the IEB data that extend beyond the last available survey wave. The cross-sectional regressions that we present thus far therefore correspond to the 31-month point on the x-axis.

[40]Nevertheless, we note two limitations of this analysis. First, the sample size in most categories is small and should be interpreted with care. Second, since employment is measured at an early stage after arrival, it is likely that the returns to host country education are not yet fully captured and will pay off at a later stage.

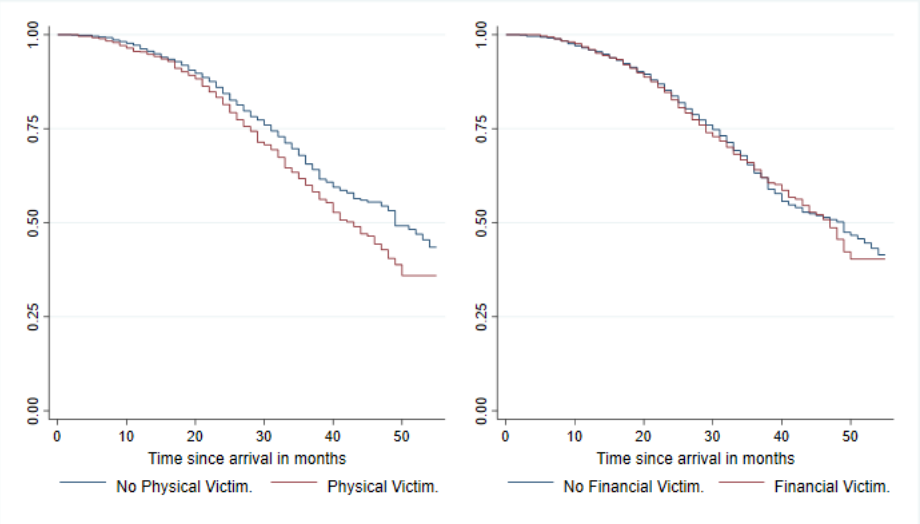


Figure 3.5.1: Kaplan-Meier estimates of time to first employment

The graph on the left in Figure 3.5.1 shows that compared to the nonvictimized refugees, physically victimized refugees obtain employment faster. The gap starts to expand approximately 18 months after arrival; we explicitly analyze this dynamic further in Section 3.6.4. The graph on the right shows the same comparison for the financially victimized, where we do not detect any effect. Table 3.M.1 in Appendix 3.M further reports the estimated output of the simple Cox proportional hazard model. The parameter estimates show an increase in the expected log of the relative hazard for the physical and financial victimization groups vis-à-vis the nonvictimized. Exponentiating the parameter estimates shows that the expected hazard, which is equal to finding employment, is 1.23 times higher for the physically victimized than for the nonvictimized on average. Although suggestive, these results support the interpretation that physical victimization events lead to a more present-oriented mindset that attaches more value to immediate payoffs.

3.5.2 Testing for the significance of unobserved confounding variables

In our main regressions, we control for a wide range of individual-level (premigration) socioeconomic characteristics, which should mitigate the risk of omitted variable bias. Nevertheless, to assess the relative importance of unobserved factors, we apply the coefficient stability test developed by Oster (2019) as discussed in Section 3.4.3.

Table 3.N.1 in Appendix 3.N shows the estimated δ values that correspond to the results of our preferred specification in Table 3.5.1 (Column (2)), and Table 3.5.2 (Columns (1)-(3)). All δ values indicate that the explanatory power of omitted variables would have to be very large compared to the variables included in the model for the estimated coefficients on physical victimization to be zero. For example, in Table 3.5.1 Panel C Column (2), we estimate that refugees who were physically victimized on their journey to Germany were 2.3 percentage points less likely to be in education or training 31 months after arrival compared to the nonphysically victimized. For the obtained coefficients to be zero instead, the unobserved variables would have to be 5.4 times larger than the control variables included in the model. The only value below the $\delta = 1$ threshold recommended by Oster (2019) is the obtained

coefficient found on full-time employment in Table 3.5.2 Column (2). However, the coefficient is not statistically significant from zero at any conventional level in our estimation. Accordingly, all test results suggest that the estimated effect of physical victimization on integration outcomes is highly robust to omitted variable bias.

3.6 Mechanisms

Our findings strongly support the hypothesis of a distortion around the decision to enter into marginal employment rather than investing in host-country-specific education or training. We interpret this as victimization leading to a "lack of future orientation" or impatience. The mental health channel discussed in Section 3.2 might still operate - it is not mutually exclusive - but the impatience channel seems to dominate. We base our claim on the assumption that individuals indirectly reveal their time preferences by engaging in certain types of activities (DellaVigna and Paserman, 2005).

One of the limitations of our study is that we cannot directly test the "loss of future orientation" as a mechanism. Our strategy, therefore, follows multiple steps. First, we analyze the effect of victimization events on individuals' self-assessed feelings of being under time pressure in Section 3.6.1, which is a measure that the psychology literature has linked to forward-looking planning, as discussed in Section 3.3.1. Second, for completeness of our analysis of deep preferences, we turn to the potential alternation of risk preferences in 3.6.2. Third, we proceed to show in Section 3.6.3 that our results suggest that well-being and health-related outcomes are negatively affected by victimization. Thus, any mechanism that explains the effect of victimization on labor market outcomes is stronger than the health-related mechanism. Fourth, we rule out all additional alternative mechanisms that could plausibly explain our main findings. We first examine the asylum procedure more closely in Section 3.6.4 and analyze whether our results could be mechanically driven by design features of the German asylum system. In Section 3.6.5, we test whether disproportional financial hardship among the physically victimized could explain their faster uptake of low-income employment. In Section 3.6.6, we test whether a negative experience during the journey could have an off-putting effect on victimized individuals' intention to stay in Germany shortly after arrival, which could, in turn, make an investment in host country-specific human capital less attractive. Finally, in Section 3.6.7 we turn to the question of whether larger expected returns to education could explain the relatively higher investment into host country-specific education among the physically victimized.

3.6.1 Altered time preferences

In this study, we argue that the main channel through which physical victimization events negatively affect refugees' investment in education is the impact of such traumatic events on the time preferences of the affected. The interpretation of a negative association between higher time discounting and educational attainment finds strong backing in the economics and psychology literature (Fersterer and Winter-Ebmer, 2003; Adelabu, 2008; Sutter et al., 2013; Cadena and Keys, 2015; Stoddard et al., 2015; Kemptner and Tolan, 2018). In this section, we provide evidence in favor of the time preference channel by analyzing an outcome that relates to individuals' *perception* of time.

Looking at time perception as an outcome offers one further benefit. Although we believe that the detailed individual information in the IAB-SOEP- -BAMF survey and our highly saturated specifications (e.g., a PDS LASSO regression) allow us to proxy for ability if one is still concerned that we are not capturing it, the time perception outcome can provide some reassurance - it is unlikely that ability directly affects time perception. In contrast, it is difficult to think of an omitted variable that positively affects victimization, labor force participation and time perception.

Table 3.6.1 shows the regression results based on our main specifications.[41]

	Feeling under time pressure	Willingness to take risks	Health Satisfaction	Life Satisfaction	Very worried finances	Intention to stay in GER
	(1)	(2)	(3)	(4)	(5)	(6)
Physical victim.	0.1427**	-0.2195	-0.2216**	-0.2039**	0.0204	-0.0113
	(0.0564)	(0.1506)	(0.1060)	(0.1000)	(0.0210)	(0.0117)
Financial victim.	0.0733	0.1694	-0.3136***	-0.2272**	0.0536***	-0.0032
	(0.0555)	(0.1446)	(0.1051)	(0.0971)	(0.0202)	(0.0111)
R-squared	0.1703	0.1801	0.2926	0.1787	0.1607	0.1169
Observations	2981	2901	2901	2901	2790	2790
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year of arrival FE	Yes	Yes	Yes	Yes	Yes	Yes
C.origin*Departure FE	Yes	Yes	Yes	Yes	Yes	Yes

Huber-White standard errors; *p<.1; **p<.05; ***p<.01

Notes: The dependent variable in Column (1) captures feeling time pressed in the past four weeks on a scale from 1 (never) to 5 (always). The dependent variable in Column (2) reflects willigness to take risks measured on a scale from 0 to 10. The dependent variable in Columns (3) and (4) captures self-reported life satisfaction or health on a scale from 1 to 10, with 10 being the highest score. The dependent variable in Column (5) is binary and takes a value of 1 for individuals who report being "very concerned about their finances" at the time of the interview. The dependent variable in Columns (6) is binary and takes a value of 1 for individuals who report that they "intend to stay in Germany permanently" at the time of the interview. Columns (1) to (5) use observations that correspond to the date of the first interview conducted, which is 19 months after arrival on average. The term FE indicates fixed effects. Departure FE refers to the year-month of displacement from the home country. C.origin is the country of origin.

Table 3.6.1: Mechanisms

Throughout the specifications, the results confirm that the physical victimization events that refugees experienced during their journey are positively associated with the self-reported feeling of being under time pressure. The results obtained from our preferred specification shown in Column (1) indicate that a physical victimization event increases the feeling of being under time pressure by 0.14 points on a 1 to 5 scale, which is an increase that corresponds to a 12% standard deviation of the measure (p<.05). The coefficients estimated on the financial victimization event are significantly smaller and are not statistically significant at conventional levels.

3.6.2 Altered risk preferences

Recent experimental studies suggest that extreme events linked to violence can affect individuals' risk aversion. Callen et al. (2014), Jakiela and Ozier (2019) and Brown et al. (2019) find that exposure to violence makes individuals more risk-averse, contradicting Voors et al. (2012) who find the opposite effect.

Evidence mostly links risk preferences to individual labor market outcomes through a positive association between the willingness to take risks and self-employment

[41]We lose 311 observations of the main working sample because of item nonresponse. However, our main results shown in Table 3.5.1 remain unchanged for the smaller sample.

(Schildberg-Hörisch, 2018). Other links are conceivable in the context of asylum-seeker victimization. For instance, a perilous journey could lead victimized refugees to become relatively more risk-averse in the search for their first job in the destination country, leading them to be less ambitious and settling for lower-paid positions.

We do not find compelling evidence favoring altered risk preferences among the victimized vis-à-vis the non-victimized. The results obtained from our preferred specification in Column (2) of Table 3.6.1 indicate the hypothesized negative association between physical victimization events and the willingness to take risks, but the coefficient is imprecisely estimated.

One could still think that willingness to take risks has a confounding effect on time preferences, affecting our results and proposed mechanism. In Appendix 3.Q, we show that our main results remain unchanged when controlling for willingness to take risks.

3.6.3 Health outcomes

Next, we turn to the results on the encompassing measure of mental well-being and life satisfaction in Columns (3)-(4) of Table 3.6.1. Column (3) shows that the effect of physical victimization on self-reported health at the time of arrival in Germany is negative. Using our preferred specification, the magnitude of the coefficient is 0.22 points ($p<.05$), which corresponds to a decrease of approximately 11% in the standard deviation of the measure. Financial victimization shows an even larger negative effect on self-reported health (0.31 points; $p<.01$).[42]

The life satisfaction outcomes in Table 3.6.1 show a similar pattern. The results in Column (4) show physical victimization decreases self-reported life satisfaction by 0.20 points ($p<.10$), which corresponds to approximately 7% in the standard deviation of the measure. The negative effect of financial victimization on life satisfaction is of similar magnitude and equally precisely estimated.

Accordingly, these two findings confirm the effect of victimization on the mental well-being and health of refugees established in previous studies on the general (nonrefugee) population (Dolan et al., 2005; Mahuteau and Zhu, 2016; Johnston, Shields and Suziedelyte, 2018).

The results of columns (1)-(4) further require a nuanced reflection on the simultaneous effect of victimization events on deep preferences and health outcomes. While health outcomes show a negative association with victimization events of any kind, our results suggest that only more severe physical victimization events may shift our proxy for time preferences. Our main results shown in Table 3.5.1 further suggest that the positive effect of altered future orientation on labour market outcomes dominates the negative health effect. In the following subsections, we further corroborate this interpretation by testing alternative mechanisms.

3.6.4 Institutional design: Asylum procedures

If asylum procedures take less (more) time for victimized individuals, then they will secure earlier (later) access to the labor market upon arrival. This would mechanically link victimization to faster (slower) labor market integration and potentially bias

[42]In Appendix 3.D, we split the health measure into physical and mental components and show that the overall results are driven by both, with the negative effect on mental health being slightly stronger.

the results. In our setting, it is conceivable that victimized individuals have a more legitimate claim for protection, and their refugee status could therefore allow them to integrate into the labor market in larger numbers by design.[43] We note here that asylum is granted based on reasons related to human rights violations and persecution that individuals face in the country of origin (rather than on the journey). However, asylum decisions are made based on the judgment of asylum officers, and the lower mental health of refugees victimized during the journey could make their asylum claims more convincing, which leads to faster procedures.

To test this, we compare the outcome and length of asylum procedures between victimized and nonvictimized individuals in Table 3.R.1 in the Appendix. There is no visible difference between financially victimized individuals and those who were not victimized regarding the share that ultimately received protection status. Among the physically victimized, the share is even slightly lower (71.6 %) than among the nonvictimized (78.2 %). We also note that the average unconditional duration of the asylum procedure is slightly longer among physically and financially victimized individuals compared to those who did not experience victimization during their journey.

The German asylum system has a second key institutional feature that could encourage rapid employment among specific segments of the asylum-seeking population. Despite employment options being very limited in scope, obtaining employment before asylum can improve the chances of receiving temporary protection status ("Duldung") in Germany (Brücker et al., 2019). Therefore, finding a job upon arrival might be particularly motivating for migrants with a low probability of receiving full protection status. Suppose that some individuals' migration decisions are motivated by economic reasons in addition to humanitarian reasons, and they take greater risks during their journeys. In this case, these asylum seekers could also be more motivated to increase their chances of being granted permission to stay by taking up employment before the end of their asylum procedure.

We test this possibility with the IEB employment biography data by mapping the employment rates between victimized and nonvictimized refugees for a) the time of arrival and the point in time when asylum was granted and b) after asylum was granted. The exercise of a pretrend and posttrend comparison allows us to see at what point employment rates start to diverge.

Figure 3.6.1 shows the results of this exercise, with the x-axis starting at the time of arrival and $t = 0$ indicating the month in which asylum was granted. We do not find any evidence that employment rates diverge before the end of the asylum procedure.

[43] We partly address this point by including a categorical variable that captures each individual's asylum status in our main specification.

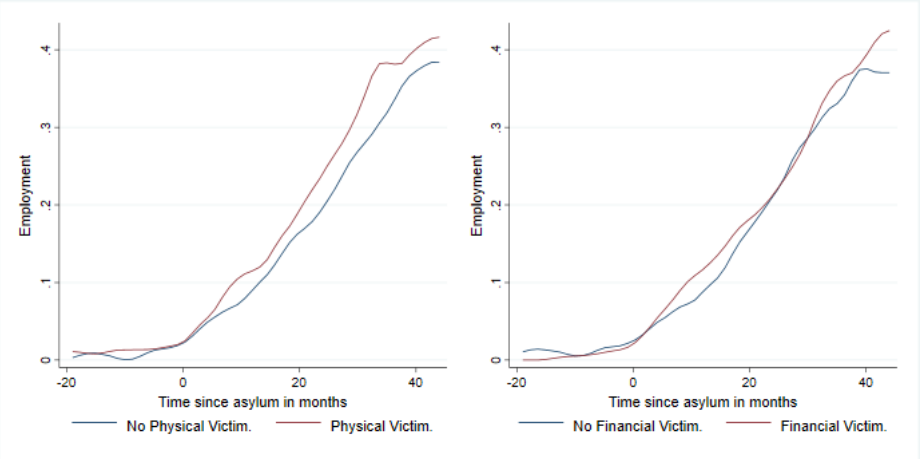


Figure 3.6.1: Pre- and postprotection trends in employment

3.6.5 Behavioral changes due to financial difficulties

People smugglers have been documented to be responsible for the abuse of asylum seekers during their journey and often charge high fees for their services Albahari (2018). Data from the IAB-BAMF-SOEP refugee survey suggest that the average unconditional amount physically and financially victimized asylum seekers paid to escape agents exceeded the amount that the nonvictimized paid by 1,420 Euros and 1,802 Euros, respectively[44]. An alternative hypothesis to explain our main results is that the faster labor market integration of victimized individuals is caused by an attempt to quickly recover the relatively high cost of the journey upon arrival. We note that since the reported average group-level differences in the amounts paid to people smugglers are unconditional, the opposite effect is also conceivable; victimization could reflect a lower ability to pay human smugglers, which could lead to violent retaliatory acts by agents who demand payment. Similar behavior could be carried out by border patrols or other intermediary agents in certain regions.

We partly address this concern in our main specifications by including the economic situation relative to other people in the country of origin before departure. Additionally, in our PDS specification, we include both the incomplete smuggler costs variable as a control and a dummy variable that equals 1 if the individual financed his or her trip through credit or borrowing.[45] Our data allow us to test the "financial hardship" hypothesis in a second way; we approximate the level of financial precariousness of refugees in Germany by the extent to which the survey respondents report being worried about their personal finances at the time of the first interview. The results of regressing a binary indicator that takes a value of 1 for individuals

[44]The average amount paid in Euros to a smuggler was 3,174.26 Euros among nonvictimized individuals, 4,597.98 Euros among the physically victimized and 4,976.13 Euros among the financially victimized

[45]In the additional specifications section in Appendix 3.Q, Table 3.Q.1 Column (6) shows the results for the main outcomes by using our preferred specification and by additionally controlling for variables "used a smuggler" and "financed escape by credit or borrowing." These variables do not change our coefficient of interest and are not significant themselves. We note that although most undocumented migrants before the 2015 refugee crisis accumulated debt with smugglers to be able to finance their journeys, this is not the case for the 2014-2016 wave of asylum seekers (which constitute the majority of our sample).

who state that they are "very concerned about their finances" (and 0 otherwise) by using our preferred specification are shown in Column (5) of Table 3.6.1. We use the first observation since, with time spent in Germany, the financial situation may become endogenous to our outcomes of interest.

We do not detect an effect of physical victimization on financial hardship at any conventional statistical level. Nevertheless, financially victimized refugees are less likely to voice concerns about their financial situation. In our preferred specification, the magnitude of the effect is 5.4 percentage points higher than that of other refugee groups.

3.6.6 Intention to remain in Germany

The hypotheses related to the intention to remain in Germany follow the classic human capital investment model for migrants. The model posits that when migrants intend to stay longer, they invest more in host country-specific education and are less likely to take up low-skilled employment in the early years after arrival (Cortes, 2004). There are two ways in which the victimization events that individuals experienced during their journey can conceivably be linked to the intended time of stay in Germany. First, the difficulty of the journey could disenchant the victimized, particularly if violent acts were carried out by official agents such as border police associated with the host country (or in the case of the EU, the hosting union). This would lead to an observed negative effect of victimization on the intention to stay in Germany and may explain why the victimized invest less in education and training in Germany. Second, victimized refugees might perceive their negative experience as an additional migration cost; therefore, the victimized may want to recover these costs by staying in Germany for as long as possible. In this case, the observed effect of victimization on the intention to stay in Germany would be positive, and our main results would underestimate the true effect of the hypothesized "loss of future orientation".

In Column (6) of Table 3.6.1, we test these competing hypotheses by analyzing the differences in refugees' stated intention to stay in Germany permanently upon arrival. The regression uses the first available observation collected after arrival because, with time spent in Germany, the intention to stay in Germany may permanently become endogenous to our outcomes of interest. If individuals do not manage to integrate economically, then they may have a lower propensity to continue staying in the host country. Using our preferred specification, Column (6) shows a small, statistically insignificant negative effect of physical victimization on the likelihood of wanting to stay in Germany. Thus, the results do not support the hypothesis that the main results can be explained by differences in the intention to stay in Germany between the victimized and the nonvictimized.

3.6.7 Returns to education

If most people who were victimized are older or if most refugees are generally older people, then the negative effect of victimization on education could be driven by the fact that older people have lower returns to education. Our analyses show that this is unlikely to be the case. We start by noticing that 90% of refugees in our sample are less than 45 years old, and 67% are less than 35 years old (table 3.F.1). Second, the conditional balance test results listed in Table 3.H show that the conditional age difference between the victimized and nonvictimized is precisely zero, and we control

for age and age squared in all regressions. Nevertheless, Table 3.6.2 shows the results of our main economic outcomes of interest obtained when progressively restricting the analysis to younger cohorts. The results are relatively stable across groups.

	LFP				**Education**			
	Full sample (18-65)	Less than 50 years	Less than 45 years	Less than 35 years	Full sample (18-65)	Less than 50 years	Less than 45 years	Less than 35 years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Physical victim.	0.0600***	0.0632***	0.0616***	0.0602***	-0.0302**	-0.0323***	-0.0306**	-0.0304*
	(0.0172)	(0.0174)	(0.0178)	(0.0210)	(0.0117)	(0.0122)	(0.0126)	(0.0161)
Financial victim.	-0.0151	-0.0114	-0.0098	0.0114	0.0090	0.0077	0.0071	0.0038
	(0.0165)	(0.0168)	(0.0174)	(0.0204)	(0.0114)	(0.0120)	(0.0124)	(0.0159)
R-squared	0.3126	0.3171	0.3168	0.3560	0.2015	0.2049	0.2095	0.2496
Observations	3004	2844	2708	1977	3004	2844	2708	1977
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of arrival FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
C.origin*Departure FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Huber-White standard errors; *p<.1; **p<.05; ***p<.01

Notes: The dependent variable is binary and takes a value of 1 for individuals in the labor force (left-hand side) or those pursuing host country education (right-hand side). LFP stands for labor force participation. Columns (1) to (8) use observations that correspond to the last interview conducted, which is 31 months after arrival on average. The term FE indicates fixed effects. The term departure refers to the year-month of forceful displacement from the home country. C.origin refers to the country of origin.

Table 3.6.2: LFP and In Education with Age Restrictions

3.7 Conclusion

One of the key features of humanitarian migration flows from the developing to developed regions of the world has been the extreme conditions under which these movements occur. We show in this paper that the physical victimization events that individuals endured during their journeys affect their economic integration outcomes in the destination country. Three years after arrival, refugees who were physically victimized during their journey are five percentage points more likely to have joined the labor force by taking up low-income employment and are three percentage points less likely to pursue host country-specific education or training compared to the nonvictimized refugee population. We do not find a similar effect for financially victimized refugees, which suggests that in line with the previous victimization literature, physical victimization has stronger effects on life trajectories.

We conceptualize our findings as a "loss of future orientation," a concept closely related to "impatience" (or higher time discounting rates) in the economics literature, where events of physical victimization lead to less forward- looking decision making. In the migrant-specific human capital investment model framework, this can be interpreted as a distortion of the trade-off that refugees face upon arrival to either invest in education to gain access to higher quality employment at a later stage or to take up lower quality employment shortly after arrival. Our findings therefore cast doubt on the notion of swift labor market integration as a general success metric for refugees. Although beneficial to determine the efficacy of supportive integration policies, we show that the aggregate speed of labor market integration also reflects unintended consequences of policies that serve entirely different purposes.

A more general lesson of this study relates to the contextual factors of victimization events. Past studies have documented that traumatic events experienced by adolescents and young adults may lower their human capital investment and lead to less future-oriented planning (Ramos et al., 2013; Monahan et al., 2015; Schmidt, Zimmerman and Stoddard, 2018; Stoddard et al., 2015). Our findings imply that these negative consequences of physical victimization events may indeed be measurable in

all victimized individuals who face important decisions that shape their economic trajectories, regardless of their age.

Further policy implications of our findings therefore relate to the potentially costly repercussions of restrictive migration policies for optimal labor market trajectories in the destination. The results strongly suggest that entry restrictions for asylum seekers have short- and possibly long-term welfare implications for destination countries beyond limiting the numbers of new arrivals. The victimization events reported by refugees in the surveys match those systematically measured around the EU's external borders, which suggests that at least some of the physical violence inflicted on asylum seekers is directly carried out by border agents (Arsenijević et al., 2017). Our findings imply that these deterrent measures have consequences for the mental well-being of asylum seekers that extend to their economic integration into the host country.

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Appendix

3.A Summary statistics of main outcomes

The outcome variables related to the labor market integration of refugees are summarised in table 3.A.1 panel A. These refer to the last time we observe the individual in the panel, corresponding to an average time spent in Germany of 31 months. The outcome variables related to mental well-being and health indicators are summarised in table 3.A.1 panel B. These refer to the first time we observe the individual in the panel, corresponding to an average time spent in Germany of 19 months. Panel C displays the summary statistics for other outcome measures used in the paper measured in the last time we observe the individual in the panel.

Variable	Mean	Std. Dev.	Max.	Min.	N
Panel A:					
Economically active	0.760	0.427	1	0	3004
Labor force participation	0.739	0.439	1	0	3004
Education or training	0.084	0.277	1	0	3004
Employed	0.209	0.406	1	0	3004
Full-time employed	0.108	0.311	1	0	3004
Part-time or marginally employed	0.101	0.301	1	0	3004
Net Income	873.537	559.144	3100	0	579
Panel B:					
Life Satisfaction BFM (1-10)	6.926	2.953	10	0	3004
Life Satisfaction after migration (1-10)	7.260	2.284	10	0	2901
Health Satisfaction BFM (1-10)	8.222	2.563	10	0	3004
Health Satisfaction after migration (1-10)	7.887	2.537	10	0	2901
PCS: Physical component scale	53.843	9.952	77.808	15.098	2831
MCS: Mental component scale	47.671	11.699	73.073	4.626	2831
Panel C:					
Feeling under time pressure	2.708	1.284	5	1	2982
Willingness to take risks	4.578	3.386	10	0	2901
Intention to stay in Germany	0.938	0.241	1	0	2790
Very worried about finances	0.290	0.454	1	0	2790

Table 3.A.1: Outcomes summary statistics

3.B Measure of conflict intensity: construction and summary statiscs

We link the IAB-BAMF-SOEP survey data to the Uppsala Conflict Data Program and the Syrian Shuhada Martyr Revolution database on the province- month level. These databases report the aggregate number of fatalities by province and month between 2011 and 2019 for all countries of origin found among the refugee population in Germany. We use these datasets to construct a measure of conflict intensity before migration to account for potential selection effects at the origin. We start by constructing a province-specific conflict-related death count in the province of origin, defined as the twelve-months rolling average of conflict-related fatalities before departure. As argued by Aksoy and Poutvaara (2021), simple province-level death counts may not adequately capture conflict intensity, as all variation in the variable may come from a few historically war-ridden countries with substantially different institutional settings. We follow their approach and calculate a measure of conflict intensity (CI) in the following way: $CI_{c,t-\mu} = \frac{\sum_{m=1}^{12} TotalDeaths_{c,t-\mu-m}}{12}$, where t denotes the survey year-month, μ the year-month of departure and m months before departure. For each country and month $t-\mu$, we then calculate the median conflict intensity, M, of all provinces and create three categories: "No conflict", for all individuals departing at $t-\mu$ for whom $CI_{c,t-\mu} = 0$; "Low conflict", for all individuals departing at $t-\mu$ for whom $CI_{c,t-\mu} < M$ and "High conflict", for all individuals departing at $t-\mu$ for whom $CI_{c,t-\mu} \geq M$.

Thus, the conflict intensity measure is calculated based on within-country conflict variation over time.[46] The calculated variables are summarized in table 3.B.1.

Variable	Mean	Std. Dev.	Min.	Max.	Obs
No conflict	0.111	0.314	1	0	3004
Low conflict	0.326	0.469	1	0	3004
High conflict	0.476	0.500	1	0	3004
No conflict info.	0.087	0.282	1	0	3004

Table 3.B.1: Conflict intensity

3.C Route approximation

From the second wave of the IAB-BAMF-SOEP survey onwards, individuals willing to answer questions regarding their escape journey were explicitly asked which route they took to reach their destination. We assign their answers to the five main migration routes: (1) The Eastern Mediterranean sea route, (2) the Central Mediterranean route, (3) the Western Mediterranean route, (4) the Eastern Mediterranean land route, (5) the Eastern Land border route and (6) travelling directly to Germany by plane[47]. Since the survey questions on the route taken are missing for some of the

[46] An alternative solution to the problem Aksoy and Poutvaara outline would be to use the death count measure in combination with country-by-year-of-departure fixed effects.

[47] (1) By boat or ship across the sea from Turkey to Greece; (2) By boat or ship across the sea from North Africa to Italy or Malta; (3) By boat or ship across the sea from North

individuals interviewed in the first wave of the survey[48], we impute the routes using an additional source of information. Since the first wave, individuals were invited to report on a virtual map all locations they passed through on their migratory journey from their country of origin to Germany. We use this data and apply the method developed by Guichard, Issifou and Keita (2021) to extract the geo-referenced points, infer the migration route and classify these to match the five first routes.

Guichard, Issifou and Keita (2021) start by assigning the geo-coded points to all countries and define a sequence of countries for each migration route. Secondly, they identify the last country before an individual entered the Schengen area and the first location in the Schengen zone. A path is assigned to (1) the Eastern Mediterranean sea route if the last non-Schengen country was Turkey and the first Schengen country was Greece; (2) the Central Mediterranean route if the last non-Schengen country was Egypt, Libya, Tunisia, or Turkey and the first Schengen country was Italy or Malta; (3) the Western Mediterranean route, if the last non-Schengen country was Morocco or Algeria and the first Schengen country was Spain or France; (4) the Eastern Mediterranean land route if the last non-Schengen country was Turkey and the first EU country was Bulgaria; (5) the Eastern Land border route if the last non-Schengen country was Romania, Ukraine, or Belarus, and the first Schengen country was Poland, Slovakia, or Hungary.

Applying these methods allows us to recover route information for 77 % of the sample. We assign the remaining 23 % to a seventh category (7), no route information available.

3.D Mental and physical health scores

The mental and physical health scores are constructed strictly following Jacobsen, Klikar and Schupp (2017), who describe all necessary calculations in detail (p.23-24). A higher score reflects better health. The mental health scale (MCS) is based on the following questions: "Did you feel in low spirits and melancholy?"; "Did you feel calm and balanced?"; "Did you feel full of energy?"; "Due to psychological or emotional problems, did you achieve less in your work or everyday activities than you actually intended?"; "Due to psychological problems or emotional problems, did you perform your work or everyday activities less carefully than usual?" and "Due to health or psychological problems, have you been restricted in terms of your social contact to for example friends, acquaintances or relatives?". The response scale for all questions related to the mental scale is 1 (Very often), 2 (Often), 3 (Sometimes), 4 (Almost never), 5 (Never).

The physical health scale (PCS) is based on the following questions: "How would you describe your current state of health (scale: 1 (Poor) to 5 (Very Well))?"; "If you have to climb stairs, i.e. walk up several floors: Does your state of health restrict you (scale: 1 (A lot), 2 (A little), 3 (Not at all))?"; "What about other strenuous activities in everyday life, e.g. when you have to lift something heavy or need to be mobile: Does your state of health restrict you a lot, a little or not at all (scale: 1 (A lot), 2 (A little), 3 (Not at all))?"; "How often in the last four weeks did you

Africa to Spain or France; (4) Through mainland from Turkey to Bulgaria or Greece; (5) Through mainland from Russian.

[48]Thanks to the longitudinal structure of the survey, individuals from the first wave who were followed up in the second or later waves, were asked about the route in these later waves

suffer from severe physical pain?"; "How often in the last four weeks, due to health problems of a physical nature, did you achieve less in your work or everyday activities than you actually intended?" and "How often in the last four weeks, due to health problems of a physical nature, have you been restricted in the type of tasks you can perform in your work or everyday activities?". The response scale for the last three questions is 1 (Very often), 2 (Often), 3 (Sometimes), 4 (Almost never), 5 (Never).

The regression results of the effect of victimization on these outcomes, using our preferred specification, are shown in table 3.D.1.

	PCS	MCS
	(1)	(2)
Physical victim.	-0.8773**	-1.4049***
	(0.4051)	(0.5152)
Financial victim.	-0.7804*	-2.1834***
	(0.4028)	(0.5027)
R-squared	0.3202	0.1897
Observations	2821	2821
Individual Controls	Yes	Yes
Year of arrival FE	Yes	Yes
C.origin*Departure FE	Yes	Yes

Huber-White standard errors; *p<.1; **p<.05; ***p<.01

Notes: The dependent variable in Column (1) is the physical health scale and in Column (2) is the menatl health scale. Columns (1) to (2) use observations that correspond to the date of the first interview conducted, which is 19 months after arrival on average. The term FE indicates fixed effects. Departure FE refers to the year-month of displacement from the home country. C.origin is the country of origin.

Table 3.D.1: Physical and mental health scale

3.E Reliability of self-reported victimization

One of the concerns when using sensitive survey data on victimization is the reliability of responses. In our setting, four particular sources of bias need to be ruled out, which our data allows us to do under weak assumptions.

The first concern relates to a potential link between the employment status of respondents and the willingness to answer questions on victimization events - which can be either positive or negative. First, it is conceivable that employed individuals feel more comfortable sharing their victimization experience if employment is indicative of a relatively more stable life that allows putting distance between the present and past experiences. We refer to this concern as "willingness to answer bias". Second, unemployed individuals could be more willing to answer the questions on victimization events to justify the difficulties they experience in integrating into the labor market. We refer to this concern as "social desirability bias" (Krumpal, 2013). The social desirability would bias the estimates in the opposite direction.

The panel structure of our data largely addresses the willingness to answer bias and social desirability bias. Questions on victimization events are only asked in the first interview when a large share of refugees had arrived recently. At the time of

the first interview, the average time since migration was one year and eight months. Only 9.3% of refugees in our sample were employed at that time, a number we verify using IAB administrative data for the sub-sample of refugees for which this information is available. Thus, employment status is unlikely to significantly affect the willingness to answer the victimization question. To further mitigate concerns around these sources of bias, we regress the willingness to answer the victimization questions on the employment status reported during the first interview, using our preferred specification as outlined in Section 3.4.[49] The result of this exercise is shown in Panel A, Column (1) of table 3.E.1. None of the estimated coefficients is significantly different from zero at any conventional level, leading us to accept the null hypotheses of no "willingness to answer" and no "social desirability" bias.

Panel A:	Replied journey questions (1)	Replied journey questions (2)
Employed 1st interview	0.0293	
	(0.0229)	
Health 1st interview		-0.0017
		(0.0029)
R-squared	0.2468	0.2466
Observations	5543	5543
Individual Controls	Yes	Yes
Year of arrival FE	Yes	Yes
C.origin*Departure FE	Yes	Yes

Panel B:	FT employ (1)	PT or marg. employ (2)	Health after migration (3)
Replied journey questions	0.0127	-0.0013	0.0643
	(0.0087)	(0.0088)	(0.0711)
R-squared	0.2046	0.1192	0.2506
Observations	5543	5543	5543
Individual Controls	Yes	Yes	Yes
Year of arrival FE	Yes	Yes	Yes
C.origin*Departure FE	Yes	Yes	Yes

Huber-White Standard Errors; *p<.1; **p<.05; ***p<.01
Notes: All estimated on the whole sample available. In Column (1), the dependent variable is a binary indicator taking the value 1 if the individual agreed to give information on experiences during the journey in their first interview, conducted 19 months after arrival on average. In Columns (2) and (3), the dependent variable is a binary indicator, taking the value 1 if the negative respondent engaged in full-time employment and part-time or marginal employment respectively in the month prior to the last interview, conducted 31 months after arrival on average. The term FE indicates fixed effects departure refers to the year-month of forceful displacement from the home country. C. of origin is the country of origin.

Table 3.E.1: Willing to answer journey questions/Social desirability

The second potential source of bias relates to a possible systematic misreporting of victimization events: We rely on the truthful reporting of victimization experiences among those who agreed to answer questions related to these negative experiences. Without further investigation, it is conceivable that individuals who reported victimization experiences are the ones who feel comfortable reporting these because the experienced events were not traumatic to them. On the other hand, those who did not report victimization could have been most severely affected by traumatic events. In this case, the victimization indicators would paradoxically capture the opposite of what they intend to measure.

We first note that the structure of the survey questions largely reduces this concern. Before any journey-related or victimization-related questions are asked, individuals are explicitly confronted with the following introductory question: "Next, we have a few questions about the experiences connected with your escape. Some of the questions will be about negative experiences. Would you like to answer questions about this subject or would you prefer not to answer these questions?" Thus, individuals are given the option not to answer questions related to their own victimization experiences. Once individuals agree to reply to this set of questions, the assumption that they will respond to these questions truthfully becomes plausible.

Based on respondents' answers regarding their willingness to reply to questions about negative experiences during the journey, we can further test the representativeness

[49]We use our full sample of respondents who agreed and did not agree to answer the victimization-related questions.

of the sub-sample of those who were willing to respond to these questions. Panel B of table 3.E.1 shows the results of regressing individuals' employment status in the last available survey wave on the willingness to respond to journey-related questions, again using our preferred specification. We do not find the willingness to respond to victimization-related questions to be a significant predictor of employment outcomes. Thus, we conclude that the underlying characteristics that predict whether or not an individual is willing to respond to sensitive survey questions in our setting are unrelated to our outcomes of interest or fully accounted for by the included covariates.

A third and related potential problem could occur if only the least traumatized individuals agree to reply to the journey related questions. As covered in section 3.2, the link between victimization and mental health is clear and well established: victimization leads to lower mental health. Hence, if the most traumatized individuals (e.g. with lower mental health) are the ones who do not agree to reply to the journey related questions in a systematic way, we expect mental health and non-reply to be negatively associated. To test this, we regress self-reported mental health in the first interview on the willingness to reply to questions about negative experiences during the journey. The results are shown in Panel A, column (2) of table 3.E.1 and suggest that those who agree to reply to the journey related questions are not more or less likely to have better mental health. Similarly, in Panel B of table 3.E.1 we test if the willingness to answer the journey-related questions affects the mental health in the last interview. If not agreeing to answer was in fact capturing more traumatized individuals, we would again expect a negative association. The results suggest that no such relation exists.

Finally, in appendix 3.P.1 we further address the concern that some respondents may give answers they deem favorable regarding their chances of receiving protection by showing that our results hold for Syrian refugees. Syrian refugees received protection with near 100% certainty and are therefore unlikely to misreport their victimization experiences in an attempt to evoke sympathy.

3.F Sample summary statistics

Before Migration	N	Share (%)
Region of Origin		
Syria	1889	62.883
Iraq and Iran	485	16.145
Afghanistan and Pakistan	286	9.521
Africa	118	3.928
All other countries	226	7.523
Gender		
Male	1961	65.280
Female	1043	34.720
Age group		
18-24	998	33.222
25-34	1020	33.955
35-44	661	22.004
45-54	271	9.021
more 55	54	1.798
Education		
No qualification	2126	70.772
Some qualification	878	29.228
No vocational training	2863	95.306
Vocational training	141	4.694
No university degree	2429	80.859
University degree	575	19.141
Employment experience		
Never been employed	929	30.925
Has been employed	2075	69.075
Economic situation		
Above Average	1485	49.434
Average	611	20.340
Bellow average	908	30.226
German		
Fair or low German	2961	98.569
Good German	43	1.431

During Journey	N	Share (%)
Route		
Eastern Mediterranean (sea)	1662	55.326
Central Mediterranean	191	6.358
Western Mediterranean	18	0.599
Eastern Mediterranean (land)	245	8.156
Eastern Land Borders	29	0.965
Plane directly to GER	126	4.194
No route information	733	24.401
Took loan or credit to finance escape		
No	2736	91.079
Yes	268	8.921
Cohort of arrival		
2012-2014	613	20.406
2015	1968	65.513
2016-2017	423	14.081
Arrived to Germany		
(*not mutually exclusive)		
Family*	2131	70.939
Friends*	248	8.256
With others*	95	3.162

Table 3.F.1: Home Country Characteristics and Journey Characteristics

	Syria		Iraq & Iran		Afgh. & Pakist.		Africa		All other		Total	
	N	Col %	N	Col	N	Col %	N	Col	N	Col %	N	Col%
Eastern Mediterranean (sea)	1191	63.049	278	57.320	161	56.294	3	2.542	29	12.832	1662	55.326
Eastern Mediterranean (land)	114	6.035	83	17.113	23	8.042	0	0.000	25	11.062	245	8.156
Central Mediterranean	84	4.447	1	0.206	2	0.699	84	71.186	20	8.850	191	6.358
Western Mediterranean	12	0.635	1	0.206	0	0.000	0	0.000	5	2.212	18	0.599
Eastern Land Borders	1	0.053	0	0.000	4	1.399	1	0.847	23	10.177	29	0.965
Plane directly to GER	93	4.923	6	1.237	11	3.846	1	0.847	15	6.637	126	4.194
No route information	394	20.858	116	23.918	85	29.720	29	24.576	109	48.230	733	24.401
Total	1889	100.000	485	100.000	286	100.000	118	100.000	226	100.000	3004	100.000

Notes: Medit. stands for Mediterranean and GER for Germany

Table 3.F.2: Route and Main Region of Origin

	No financial victim.		Financial victim.			No physical victim.		Physical victim.		
Panel A: Total	N	Row %	N	Row %	Total	N	Row %	N	Row %	Total
Eastern Medit. (sea)	1014	61.011	648	38.989	1662	1069	64.320	593	35.680	1662
Central Medit.	100	52.356	91	47.644	191	75	39.267	116	60.733	191
Western Medit.	9	50.000	9	50.000	18	14	77.778	4	22.222	18
Eastern Medit. (land)	154	62.857	91	37.143	245	167	68.163	78	31.837	245
Eastern Land Borders	26	89.655	3	10.345	29	24	82.759	5	17.241	29
Plane directly to GER	102	80.952	24	19.048	126	117	92.857	9	7.143	126
No route information	445	60.709	288	39.291	733	448	61.119	285	38.881	733
Total	1850	61.585	1154	38.415	3004	1914	63.715	1090	36.285	3004
Panel B: Syria	N	Row %	N	Row %	Total	N	Row %	N	Row %	Total
Eastern Medit. (sea)	728	61.125	463	38.875	1191	800	67.170	391	32.830	1191
Central Medit.	35	41.667	49	58.333	84	40	47.619	44	52.381	84
Western Medit.	6	50.000	6	50.000	12	11	91.667	1	8.333	12
Eastern Medit. (land)	59	51.754	55	48.246	114	72	63.158	42	36.842	114
Eastern Land Borders	1	100.000	0	0.000	1	1	100.000	0	0.000	1
Plane directly to GER	76	81.720	17	18.280	93	88	94.624	5	5.376	93
No route information	230	58.376	164	41.624	394	251	63.706	143	36.294	394
Total	1135	60.085	754	39.915	1889	1263	66.861	626	33.139	1889
Panel C: Iran & Iraq	N	Row %	N	Row %	Total	N	Row %	N	Row %	Total
Eastern Medit. (sea)	178	64.029	100	35.971	278	181	65.108	97	34.892	278
Central Medit.	1	100.000	0	0.000	1	1	100.000	0	0.000	1
Western Medit.	1	100.000	0	0.000	1	1	100.000	0	0.000	1
Eastern Medit. (land)	63	75.904	20	24.096	83	63	75.904	20	24.096	83
Plane directly to GER	4	66.667	2	33.333	6	5	83.333	1	16.667	6
No route information	69	59.483	47	40.517	116	71	61.207	45	38.793	116
Total	316	65.155	169	34.845	485	322	66.392	163	33.608	485
Panel D: Afgh. & Pakist.	N	Row %	N	Row %	Total	N	Row %	N	Row %	Total
Eastern Medit. (sea)	94	58.385	67	41.615	161	74	45.963	87	54.037	161
Central Medit.	2	100.000	0	0.000	2	2	100.000	0	0.000	2
Eastern Medit. (land)	13	56.522	10	43.478	23	10	43.478	13	56.522	23
Eastern Land Borders	3	75.000	1	25.000	4	2	50.000	2	50.000	4
Plane directly to GER	10	90.909	1	9.091	11	9	81.818	2	18.182	11
No route information	43	50.588	42	49.412	85	32	37.647	53	62.353	85
Total	165	57.692	121	42.308	286	129	45.105	157	54.895	286
Panel E: Cohort 2012-14	N	Row %	N	Row %	Total	N	Row %	N	Row %	Total
Eastern Medit. (sea)	49	48.039	53	51.961	102	53	51.961	49	48.039	102
Central Medit.	60	52.632	54	47.368	114	54	47.368	60	52.632	114
Western Medit.	4	80.000	1	20.000	5	4	80.000	1	20.000	5
Eastern Medit. (land)	51	57.303	38	42.697	89	66	74.157	23	25.843	89
Eastern Land Borders	18	85.714	3	14.286	21	18	85.714	3	14.286	21
Plane directly to GER	41	71.930	16	28.070	57	52	91.228	5	8.772	57
No route information	143	63.556	82	36.444	225	138	61.333	87	38.667	225
Total	366	59.706	247	40.294	613	385	62.806	228	37.194	613
Panel F: Cohort 2015	N	Row %	N	Row %	Total	N	Row %	N	Row %	Total
Eastern Medit. (sea)	758	60.302	499	39.698	1257	795	63.246	462	36.754	1257
Central Medit.	27	50.943	26	49.057	53	11	20.755	42	79.245	53
Western Medit.	5	38.462	8	61.538	13	10	76.923	3	23.077	13
Eastern Medit. (land)	78	63.415	45	36.585	123	78	63.415	45	36.585	123
Eastern Land Borders	5	100.000	0	0.000	5	4	80.000	1	20.000	5
Plane directly to GER	42	89.362	5	10.638	47	43	91.489	4	8.511	47
No route information	274	58.298	196	41.702	470	283	60.213	187	39.787	470
Total	1189	60.417	779	39.583	1968	1224	62.195	744	37.805	1968
Panel G: Cohort 2016-17	N	Row %	N	Row %	Total	N	Row %	N	Row %	Total
Eastern Medit. (sea)	207	68.317	96	31.683	303	221	72.937	82	27.063	303
Central Medit.	13	54.167	11	45.833	24	10	41.667	14	58.333	24
Eastern Medit. (land)	25	75.758	8	24.242	33	23	69.697	10	30.303	33
Eastern Land Borders	3	100.000	0	0.000	3	2	66.667	1	33.333	3
Plane directly to GER	19	86.364	3	13.636	22	22	100.000	0	0.000	22
No route information	28	73.684	10	26.316	38	27	71.053	11	28.947	38
Total	295	69.740	128	30.260	423	305	72.104	118	27.896	423

Notes: Medit. stands for Mediterranean and GER for Germany

Table 3.F.3: Route and Victimization

3.G Xenophobia along the Balkan Route

During the early stages of the war in Syria, countries such as Turkey or Bosnia Herzegovina were not hostile toward asylum seekers. Due to their geographical position, they then received large inflows of asylum seekers, putting intense pressure on often ill-prepared reception facilities and asylum processing centres (Oxfam,

Human Rights and Association, 2017). Consequently, conditions at reception, transit centres and refugee camps deteriorated, and asylum procedures became increasingly slow and restrictive (Oxfam, Human Rights and Association, 2017; UNHCR, 2018). The stance taken against refugees in some Balkan countries, such as Hungary, seems to be driven by a generalized xenophobic stance towards refugees and migrants (Assembly, April 2016; Deardorff Miller, 2018; Rankin, 21 May 2019). The Council of Europe's commissioner for human rights and several NGOs have accused the Hungarian government of using anti-migrant rhetoric and propaganda[50] aimed at fuelling fear and xenophobic attitudes among the local population[51]. AAccording to data from the Euro-barometer, in 2012, only 5 % of Slovenes, 8 % of Hungarians, 10 % of Bulgarians and 16 % of Greeks thought that immigration was among the top three challenges facing Europe. However, in 2015 about 40 % of Slovenes, 65 % of Hungarians, 50 % of Bulgarians and 40 % of Greeks reported immigration as one of the main challenges in Europe. These sharp increases reflect the growing concern with immigration in Europe. Additionally, in 2016 when asked if migratory flows from outside the EU are a good thing for the economy, between 50-60% of the population in the countries mentioned above tended to disagree, reflecting further the negative stance towards immigrants.

Despite the increasing violence and push-backs in Greece and along the Balkan route, most asylum seekers still considered the route through Turkey and Greece less dangerous than travelling through Libya (Crawley et al., 2016). Indeed, according to the UNHCR reports, until the end of 2016 most Syrians, Afghanis and Iraqis used the Eastern Mediterranean route. This has contributed to sustaining the flow of migrants through the Balkan route, which was reduced only after the 2016 EU-Turkish deal.

The Eastern Mediterranean migration route perpetrators are mostly local authorities and criminal gangs. The ability of migrants to avoid these perpetrators is limited since they tend to be armed. Local officers patrol borders and inner parts of these countries throughout the day and night, making it difficult for migrants to predict the best timing to cross the border.

3.H Conditional balance test

[50]This included, for instance, several billboards across the country linking asylum seekers to violent crime ("Did you know? Since the beginning of the immigration crisis, the number of sexual assaults on women has exponentially increased") and as threats to social cohesion ("if you come to Hungary, you can not take away the work places from Hungarians") (International, 2016; InfoMigrants, 2019).

[51]The rhetoric used by the government relies on the fear and distrust of the unknown and portrays the "others" as a threat to social cohesion and society. In a country with little immigration experience, such as Hungary, these fears gain prominence more easily

	Physical victimization		Financial victimization	
	Coef	SE	Coef	SE
Female	0.0036	(0.0215)	-0.0067	(0.0227)
Age	-0.0023	(0.0054)	-0.0037	(0.0055)
Age squared	-0.0000	(0.0001)	0.0000	(0.0001)
Arrived alone	0.0270	(0.0238)	-0.0101	(0.0239)
Has been employ. BFM	0.0277	(0.0233)	0.0589**	(0.0238)
No qualification BFM	-0.0062	(0.0202)	-0.0239	(0.0207)
University degree BFM	-0.0505**	(0.0230)	0.0426*	(0.0244)
Vocational tr. BFM	-0.0294	(0.0417)	-0.0145	(0.0445)
Good German BFM	-0.1407**	(0.0709)	-0.0627	(0.0699)
Econ. situation BFM below average	0.0070	(0.0248)	-0.0162	(0.0247)
Econ. situation BFM above average	0.0325	(0.0214)	0.0674***	(0.0223)
Low conflict intensity	0.0632	(0.0544)	0.0066	(0.0549)
High conflict intensity	0.0595	(0.0541)	0.0221	(0.0546)
No conflict intensity info.	0.0940	(0.0577)	0.0300	(0.0565)
Life satisf. BFM (1-10)	-0.0030	(0.0039)	0.0036	(0.0039)
Health satisf. BFM (1-10)	-0.0048	(0.0042)	-0.0117***	(0.0042)
Willingness to take risk	-0.0010	(0.0027)	0.0057**	(0.0028)
Missing BFM info.	0.0283	(0.0458)	-0.0539	(0.0494)
Observations	3004		3004	
R-squared	0.2540		0.2233	
C.origin*Departure FE	Yes		Yes	
Migration Route FE	Yes		Yes	
Year of arrival FE	Yes		Yes	

Huber-White SE; *p<.1; **p<.05; ***p<.01

Note: BFM stands for backward reported "before migration" information. The term FE indicates fixed effects. Willingness to take risk, Life satisfaction BFM and Health satisfaction BFM are measured on a scale from 1 (low) to 10 (very high). The baseline category for the education variables is some school qualification, no vocational training and no university degree. The baseline category for the dummy variable "German skills BFM: Good" is no or very limited German skills before migration. The baseline for the dummy variable "Economic Situation BFM < Avg" is "Economic Situation BFM > Avg" and refers to the economic situation of individuals compared to the population in their country of origin. "Arrived alone" is dummy variables capturing if migrants arrived alone in Germany.

Table 3.H.1: Conditional balance test

3.I Control variables definition

The backward reported measures of life satisfaction and health satisfaction before migration included in $PreMig_{i,\mu}$ are measured on a scale from 1 (low) to 10 (very high). Because compulsory education and its academic contents vary by country of origin, we use a dummy variable that equals one if the respondent left school without a qualification/certificate[52]. We then add a dummy variable that equals one if individual i acquired vocational training in the home county and another dummy variable that equals one if the respondent received a university degree. German skills equal one if an individual has good or very good spoken German, zero otherwise. The variable "economic situation before migration" refers to the economic situation of individuals compared to the population in their country of origin - it equals zero if above average and one if below average". Employment before migration equals one if individual i had working experience before migration, zero otherwise.

The residence of the spouse variable included in the $PostMig_{i,t}$ contains the following categories: Single; the spouse lives in the same household; the spouse lives in a different household in Germany; the spouse resides abroad. The variable related

[52]This is based in a question that asks respondents "What kind of certificate did you leave school with?", the possible answers include "Left school with no qualification", "Middle school leaving certificate", "Practical-based further education certificate", "General-based further education certificate" and "Certificate from a different school"

to children's location contains the following categories: No children; all children live in the same household; some children live in a different household. $AsylumStatus_{i,t}$ is a time-varying individual level characteristic, measured at the time of the survey t. It is a fixed effects term with four categories: "Asylum granted", "Temporary suspension of deportation", "Request to leave Germany" and "Decision pending". Only the first two give refugees unrestricted access to the labor market in Germany, an institutional feature we discuss in more detail in section 3.6.4. "Arrived alone" is a dummy variable that equals one if migrants arrived alone in Germany, and zero o

3.J Least absolute shrinkage and selection operators

While the main strength of supervised machine learning methods, such as the least absolute shrinkage and selection operators (LASSO) is prediction, they can be used to select control variables to address omitted variable bias when many potential controls are available (Tibshirani, 1996; Ahrens, Hansen and Schaffer, 2020). These methods also allow us to consider interactions and non-linearities that theory-driven specifications typically omit. Starting with a general model $y_i = x_i'\beta + \epsilon_i$, the LASSO minimization problem can be written as:

$$\frac{1}{n}\sum_{i=1}^{n}(y_i - x_i'\beta)^2 + \lambda\sum_{j=1}^{p} \mid \beta_j \mid, \tag{3.2}$$

with $i = 1, ..., n$ observations and $j = 1, ..., p$ regressors. There are up to $p = dim(\beta)$ potential regressors. Here p can be very large, potentially even $p > n$.

The second term of equation (3) represents the cost of including many regressors. λ is the penalization term[53]. The effect of the penalization is that LASSO sets the $\hat{\beta}_j$s of the variables that contribute little to the model fit to zero.

Belloni, Chernozhukov and Hansen (2014) developed a 'post-double selection' (PDS) method, in which the underlying idea is to estimate separate LASSO regressions to find predictors of the selection equation and the outcome equation using 'rigorous' penalization. The final equation then includes the union of the variables chosen as controls from the previous step.

In our setting, with two variables of interest, $PhysicalVictim$ (PT) and $FinancialVictim$ (FT), we apply this method to a post-triple selection. The first step in this procedure is to estimate the outcome equation (labor market outcomes) using LASSO, without including PT nor FT: $Y_{i,f,c,t} = x'_{i,f,c,t}\beta_j + \epsilon_{i,f,c,t}$, where we denote the set of LASSO-selected controls by A. The vector $x_{i,f,c,t}$ includes a large set of time constant and time varying individual characteristics, country of origin fixed effects, year-month fixed effects, year-quarter fixed effects, and the interaction between all these variables.

The second step is to estimate an equation using physical victimization as an outcome: $PT_{i,c,t} = x'_{i,c,t}\delta_j + \epsilon_{i,f,c,t}$, where we denote the set of LASSO-selected controls by B. The third step is to estimate an equation using financial victimization as an outcome: $FT_{i,c,t} = x'_{i,c,t}\eta_j + \epsilon_{i,f,c,t}$, where we denote the set of LASSO-selected controls by C. The final step is to use OLS to estimate $Y_{i,f,c,t} = \gamma_1 PT_i + \gamma_2 FT_i + w'_{i,f,c,t}\beta_j + \epsilon_{i,f,c,t}$, where $w_{i,f,c,t}$ is the union of the selected controls from steps 1,2 and 3 (e.g., $w_{i,f,c,t} = A \cup B \cup C$).

[53]There are three main approaches to choose λ: cross-validation (Chetverikov, Liao and Chernozhukov, 2019), 'rigorous' penalization (Belloni, Chernozhukov and Hansen, 2014) and information criteria (AIC, AICc, BIC or EBIC).

Belloni, Chernozhukov and Hansen (2014) argue that LASSO can be used to select controls because moderate model selection mistakes of the LASSO do not affect the asymptotic properties of the estimator of the low-dimensional parameters of interest. Hence, modelling the nuisance component of our structural model can be seen as a prediction problem (**AHLR08;AHS20**).

3.K Training

	No F.V.		F.V.		No P.V.		P.V.		Total	
	%	Obs	%	Obs	%	Obs	%	Obs	%	Obs
No Training Required	57.2	174	62.1	151	54.6	171	65.8	154	59.4	325
Professional Training	32.6	99	26.7	65	31.9	100	27.4	64	30.0	164
Technical college or University	10.2	31	11.1	27	13.4	42	6.8	16	10.6	58
Total	100	304	100	243	100	313	100	234	100.0	547

Note: F.V. refers to financial victimization and P.V. to physical victimization. Classification of task level according to IEB categories.

Table 3.K.1: Training required for job

3.L Full Results

Table 3.L.1 shows the main results as in section 3.5.1 showing the coefficients on all covariates included, estimated using our preferred specification.

	LFP& Educ	LFP	Educ	All emp.	FT emp.	PT,M emp.	Inc
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Physical victim.	0.0424**	0.0608***	-0.0306***	0.0336**	0.0115	0.0221*	-0.1415
	(0.0167)	(0.0170)	(0.0117)	(0.0170)	(0.0133)	(0.0132)	(0.1159)
Financial victim.	-0.0111	-0.0152	0.0090	-0.0038	-0.0020	-0.0018	-0.0285
	(0.0161)	(0.0165)	(0.0114)	(0.0164)	(0.0127)	(0.0127)	(0.1144)
Central Medit.	0.0185	0.0134	0.0055	-0.0124	0.0154	-0.0278	0.0720
	(0.0389)	(0.0393)	(0.0326)	(0.0444)	(0.0392)	(0.0329)	(0.2177)
Western Medit.	0.1853***	0.2147***	-0.0260	0.0208	0.0881	-0.0673*	0.1408
	(0.0554)	(0.0589)	(0.0556)	(0.0954)	(0.0939)	(0.0405)	(0.3944)
Eastern Medit. (land)	0.0252	0.0169	-0.0164	0.0085	0.0198	-0.0113	-0.0614
	(0.0270)	(0.0284)	(0.0200)	(0.0305)	(0.0249)	(0.0234)	(0.2015)
Eastern Land Borders	0.0214	0.0206	-0.0275	-0.0403	-0.0385	-0.0018	-0.2948
	(0.0911)	(0.0918)	(0.0392)	(0.0828)	(0.0472)	(0.0803)	(0.8379)
Plane directly to GER	0.0073	0.0110	0.0800**	-0.0110	-0.0217	0.0107	0.2183
	(0.0398)	(0.0411)	(0.0347)	(0.0432)	(0.0271)	(0.0380)	(0.2677)
No route information	-0.0295	-0.0387	0.0234	-0.0524**	-0.0137	-0.0387**	0.1095
	(0.0243)	(0.0247)	(0.0163)	(0.0232)	(0.0186)	(0.0176)	(0.1776)
Low conflict intensity	0.0091	0.0074	0.0373	0.0081	0.0481	-0.0400	0.1675
	(0.0516)	(0.0517)	(0.0267)	(0.0457)	(0.0328)	(0.0382)	(0.4260)
High conflict intensity	0.0086	-0.0036	0.0434*	0.0075	0.0499	-0.0424	0.1996
	(0.0509)	(0.0511)	(0.0262)	(0.0453)	(0.0324)	(0.0378)	(0.4259)
No conflict info.	-0.0157	-0.0379	0.0764***	-0.0188	0.0234	-0.0422	-0.1472
	(0.0512)	(0.0515)	(0.0281)	(0.0485)	(0.0364)	(0.0382)	(0.4378)
Female	-0.2329***	-0.2201***	-0.0374***	-0.1405***	-0.1028***	-0.0377**	-0.6057**
	(0.0205)	(0.0212)	(0.0138)	(0.0181)	(0.0124)	(0.0152)	(0.2768)
Age	0.0231***	0.0315***	-0.0160***	0.0203***	0.0136***	0.0067*	0.1044**
	(0.0057)	(0.0057)	(0.0035)	(0.0044)	(0.0032)	(0.0036)	(0.0489)
Age squared	-0.0003***	-0.0004***	0.0002***	-0.0003***	-0.0002***	-0.0001**	-0.0013*
	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0000)	(0.0000)	(0.0008)
Health satisf. BFM (1-10)	0.0036	0.0017	0.0008	0.0090***	0.0049**	0.0041	-0.0043
	(0.0035)	(0.0036)	(0.0021)	(0.0032)	(0.0025)	(0.0025)	(0.0287)
Life satisf. BFM (1-10)	-0.0036	-0.0013	0.0001	-0.0023	-0.0006	-0.0017	0.0018
	(0.0032)	(0.0033)	(0.0022)	(0.0033)	(0.0025)	(0.0027)	(0.0202)
No qualification BFM	-0.0388**	-0.0465***	-0.0023	-0.0131	-0.0087	-0.0044	0.0775
	(0.0174)	(0.0177)	(0.0110)	(0.0166)	(0.0129)	(0.0127)	(0.1197)
Vocational tr. BFM	0.0196	0.0346	-0.0172	0.0679*	0.0198	0.0481	0.0513
	(0.0328)	(0.0331)	(0.0216)	(0.0377)	(0.0315)	(0.0304)	(0.2382)
University degree BFM	0.0095	0.0113	0.0280*	0.0249	0.0116	0.0134	0.0454
	(0.0195)	(0.0201)	(0.0154)	(0.0208)	(0.0172)	(0.0164)	(0.1387)
Good German BFM	-0.0280	-0.0014	-0.0422	0.1140*	0.0215	0.0925	-0.2218
	(0.0586)	(0.0583)	(0.0430)	(0.0681)	(0.0477)	(0.0624)	(0.4412)
Has been employ. BFM	0.1199***	0.1524***	-0.0296*	0.0520***	0.0298**	0.0222	0.0158
	(0.0219)	(0.0226)	(0.0153)	(0.0185)	(0.0134)	(0.0152)	(0.1825)
Econ. situation BFM < avg.	0.0061	0.0096	-0.0221	-0.0156	-0.0032	-0.0124	0.0160
	(0.0204)	(0.0209)	(0.0136)	(0.0198)	(0.0149)	(0.0159)	(0.1469)
Econ. situation BFM > avg.	0.0407**	0.0429**	-0.0119	0.0165	0.0177	-0.0012	0.0675
	(0.0178)	(0.0182)	(0.0128)	(0.0181)	(0.0142)	(0.0143)	(0.1262)
Arrived alone	-0.0145	-0.0000	0.0012	0.0140	0.0202	-0.0062	0.0430
	(0.0179)	(0.0186)	(0.0160)	(0.0215)	(0.0175)	(0.0168)	(0.1245)
Time in Germany (in months)	0.0170***	0.0156***	0.0068***	0.0045	0.0018	0.0027	0.0236
	(0.0031)	(0.0032)	(0.0021)	(0.0029)	(0.0024)	(0.0023)	(0.0310)
Time in Germany squared	-0.0001**	-0.0001**	-0.0001*	0.0000	0.0000	-0.0000	-0.0001
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0004)
Temporary susp. of deportation	-0.0349	-0.0327	-0.0034	-0.0181	-0.0085	-0.0096	0.0182
	(0.0349)	(0.0364)	(0.0209)	(0.0329)	(0.0250)	(0.0260)	(0.3026)
Request to leave Germany	-0.0958**	-0.0839*	0.0249	-0.0551	-0.0587*	0.0036	0.5602
	(0.0459)	(0.0468)	(0.0300)	(0.0449)	(0.0347)	(0.0349)	(0.4101)
Decision for Asylum still open	-0.0412	-0.0345	-0.0044	-0.0523**	-0.0275	-0.0248	-0.0072
	(0.0380)	(0.0382)	(0.0201)	(0.0262)	(0.0190)	(0.0213)	(0.3726)
... continue				... continue			

Table 3.L.1: Fixed Effects Results, main outcomes

... continue				... continue			
Status unknown	-0.0339	-0.0365	-0.0184	-0.0465*	0.0036	-0.0501**	0.0368
	(0.0291)	(0.0301)	(0.0201)	(0.0282)	(0.0218)	(0.0215)	(0.2298)
Partner same HH	-0.0520**	-0.0281	-0.0611***	0.0321	0.0095	0.0226	
	(0.0238)	(0.0243)	(0.0157)	(0.0231)	(0.0174)	(0.0194)	
Partner diff. HH, in GER	0.0424	0.0438	-0.0140	0.0586	0.0358	0.0228	
	(0.0325)	(0.0343)	(0.0318)	(0.0386)	(0.0308)	(0.0308)	
Partner abroad	0.0568*	0.0733**	-0.0603***	0.1130***	0.0668**	0.0463	
	(0.0299)	(0.0309)	(0.0206)	(0.0343)	(0.0266)	(0.0296)	
No partner location info.	-0.1149	-0.1025	0.0189	-0.0910	0.0493	-0.1403*	
	(0.1522)	(0.1536)	(0.1237)	(0.0942)	(0.0910)	(0.0785)	
All children same HH	-0.0471*	-0.0495**	-0.0309**	-0.1082***	-0.0467**	-0.0615***	
	(0.0241)	(0.0244)	(0.0153)	(0.0233)	(0.0183)	(0.0188)	
Some children not same HH	-0.0476*	-0.0484*	-0.0304*	-0.0483	-0.0257	-0.0225	
	(0.0289)	(0.0292)	(0.0159)	(0.0304)	(0.0235)	(0.0240)	
Missing BFM info.	0.0516	0.0194	0.0529	0.0374	0.0540	-0.0166	0.2403
	(0.0426)	(0.0459)	(0.0392)	(0.0445)	(0.0351)	(0.0327)	(0.2398)
R-squared	0.3174	0.3219	0.2054	0.2647	0.2383	0.1581	0.4100
Observations	3004	3004	3004	3004	3004	3004	543
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of arrival FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
C.origin*Departure FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Huber–White standard errors; *p<.1; **p<.05; ***p<.01

Notes: The table replicates the results shown in table 3.5.1 (Panel A, B and C, Column (2)), and 3.5.2 (Columns (1)-(3)) of the main text but also shows all coefficients estimated on the control variables included in included in these regressions. The term FE indicates fixed effects. The term departure refers to the year-month of year-month of forceful displacement from the home country. C. of origin is the country of origin.

3.M IEB data results

	Time to employment
Physical victim.	0.211***
	(0.0742)
Financial victim.	-0.0435
	(0.0738)
Observations	1990

*p<.1; **p<.05; ***p<.01

Table 3.M.1: Cox proportional hazard model

3.N Oster Test

	LFP &. Educ.	LFP	Educ.	All employ.	FT employ.	PT or marg. employ.
	(1)	(2)	(3)	(4)	(5)	(6)
δ (Physical trauma)	2.407	3.611	-5.403	1.283	0.487	3.875
δ (Financial trauma)	-0.393	-0.461	5.995	-0.175	-0.0891	-0.292
R-squared	3,004	3,004	3,004	3,004	3,004	3,004
Observations	0.317	0.322	0.205	0.297	0.238	0.175

*p<.1; **p<.05; ***p<.01

Notes: The table shows the estimated δ values based on a test for the salience of unobserved confounders following Oster (2019), which show the relative importance of omitted variables compared to those variables we condition our estimates on. The R_{max}, the hypothetical R-squared value of a fully specified model, is set to $1.5\hat{R}$ where $\hat{R}$ is the R-squared value obtained from the respective estimated model. These $\hat{R}$ are obtained from our preferred specification in tables 3.5.1 (Panel A, B and C, Column (2)), and 3.5.2 (Columns (1)-(3)). LFP means labor force participation. Educ. means education. FT means full-time, PT part-time and marg. marginal.

Table 3.N.1: Oster Test

3.O Robustness to the construction of victimization variables

In this section, we show the results using alternative ways of aggregating the victimization experiences.

We start by showing that our results are robust to different physical and financial victimization specifications. In particular, one of our modelling choices in the analyses has been to code the victimization events as binary indicators. For our integration outcomes, this choice implicitly assumes that once individuals had to endure a physical or financial victimization event, additional victimization events do not alter their well-being and behaviour further. In this subsection, we relax this assumption and consider the number of victimization events individuals endured. Table 3.O.1 summarises the number of physical (financial) victimization events by the share of

individuals who endured them. The acronym P.V.E. denotes physical victimization events(s) and F.V.E. financial victimization events(s).

Physical victimization			Financial victimization		
Variable	**N**	**Mean (%)**	**Variable**	**N**	**Mean (%)**
None	1914	63.715	None	1850	61.585
1 P.V.E	747	24.867	1 F.V.E	712	23.702
2 P.V.E	278	9.254	2 F.V.E	338	11.252
3 P.V.E	59	1.964	3 F.V.E	104	3.462
4 P.V.E	6	0.200			
N	3004		N	3004	

Table 3.O.1: Summary statistics - number of physical and financial victimization events

In table 3.O.2 we first turn to the regression results of our preferred specification using a linear and a squared measure of the number of physical victimization events, which ranges from zero to a maximum of four, and of the number of financial victimization events, that ranges from zero to a maximum of three.

	LFP & Educ.	LFP	Educ.	All employ.	FT employ.	Part-Time or marginal
	(1)	(2)	(3)	(4)	(5)	(6)
Number of physical victim. experiences	0.0396*	0.0703**	-0.0433**	0.0622**	0.0233	0.0389**
	(0.0240)	(0.0249)	(0.0160)	(0.0242)	(0.0195)	(0.0186)
Number of physical victim. experiences squared	-0.0052	-0.0173*	0.0134**	-0.0229**	-0.0082	-0.0147**
	(0.0095)	(0.0100)	(0.0062)	(0.0099)	(0.0085)	(0.0074)
Number financial of victim. experiences	-0.0069	-0.0181	0.0138	0.0146	0.0074	0.0072
	(0.0248)	(0.0254)	(0.0179)	(0.0257)	(0.0200)	(0.0202)
Number of physical victim. experiences squared	-0.0023	0.0035	-0.0056	-0.0089	-0.0041	-0.0048
	(0.0101)	(0.0104)	(0.0074)	(0.0105)	(0.0084)	(0.0083)
Observation	3004	3004	3004	3004	3004	3004
R-squared	0.3175	0.3216	0.2056	0.2657	0.2386	0.1587
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year of arrival FE	Yes	Yes	Yes	Yes	Yes	Yes
C.origin*Departure FE	Yes	Yes	Yes	Yes	Yes	Yes

Huber–White standard errors; *p<.1; **p<.05; ***p<.01

Notes: The dependent variable is binary and takes the value 1 for individuals in employment/labor force participation/education, zero otherwise. The acronym P.V.E. denotes physical victimization events(s) and F.V.E. financial victimization events(s). LFP means labor force participation. Educ. means education. FT means full-time. Results are only shown for our preferred specification, corresponding to Column (2) in the main result tables. The term FE indicates fixed effects. The term departure refers to the year-month of forceful displacement from the home country. C.origin is the country of origin.

Table 3.O.2: Number of victimization experiences (continuous)

The results confirm our main results but their interpretation changes. Column (2) now shows that any *additional* physical victimization event increases the probability of joining the labor force by 5.2 percentage points, an effect again driven by the take up of marginal and part-time employment (Column (6)) and at the cost of not pursuing host-country education (Column (3)). We also note that the estimated coefficients on the squared number of victimization events are close to zero and not statistically significant at any conventional level. Adding polynomials that allow for a more flexible curvilinear relation between victimization events and integration outcomes makes no significant difference. Similar to our main results, we find no effect of financial victimization events on economic integration outcomes in Germany.

In table 3.O.3 we then turn to the results where the different numbers of victimization events enter as categorical variables against the base category of zero victimization events. Since very few individuals had more than two events, we aggregate individuals with two or more events into a single category.

	LFP & Educ.	LFP	Educ.	All employ.	FT employ.	Part-Time or marginal
	(1)	(2)	(3)	(4)	(5)	(6)
1 P.V.E.	0.0451**	0.0666***	-0.0281**	0.0408**	0.0100	0.0308**
	(0.0182)	(0.0186)	(0.0124)	(0.0185)	(0.0146)	(0.0146)
2 or more P.V.E.	0.0475*	0.0523**	-0.0303	0.0179	0.0131	0.0048
	(0.0244)	(0.0250)	(0.0188)	(0.0273)	(0.0224)	(0.0207)
1 F.V.E.	-0.0000	-0.0073	0.0154	0.0023	-0.0044	0.0067
	(0.0179)	(0.0185)	(0.0133)	(0.0189)	(0.0144)	(0.0148)
2 or more F.V.E.	-0.0334	-0.0274	-0.0030	-0.0105	0.0019	-0.0124
	(0.0228)	(0.0231)	(0.0152)	(0.0237)	(0.0194)	(0.0182)
Observations	3004	3004	3004	3004	3004	3004
R-squared	0.3179	0.3222	0.2057	0.2650	0.2383	0.1590
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year of arrival FE	Yes	Yes	Yes	Yes	Yes	Yes
C.origin*Departure FE	Yes	Yes	Yes	Yes	Yes	Yes

Huber–White standard errors; *p<.1; **p<.05; ***p<.01

Notes: The dependent variable is binary and takes the value 1 for individuals in employment/labor force participation/education, zero otherwise. The acronym P.V.E. denotes physical victimization events(s) and F.V.E. financial victimization events(s). LFP means labor force participation. Educ. means education. FT means full-time. Results are only shown for our preferred specification, corresponding to Column (2) in the main result tables. The term FE indicates fixed effects. The term departure refers to the year-month of forceful displacement from the home country. C.origin is the country of origin.

Table 3.O.3: Number of victimization experiences (discrete)

The results show that the main results are driven by individuals in both categories, those that experienced one and those that experienced multiple victimization events, with no apparent pattern emerging. The less precisely estimated coefficients on the multiple victimization event categories are likely a result of the smaller number of observations in this group. Overall, we interpret the results of these alternative victimization specifications as a confirmation of our main results and the modelling choice of victimization as a binary indicator.

3.P Heterogeneous effects

In this section we present a range of split-sample regressions to show our main results for specific groups of interest. Subsection 3.P.1 shows the main results split by major countries of origin and subsection 3.P.2 splits the sample by gender

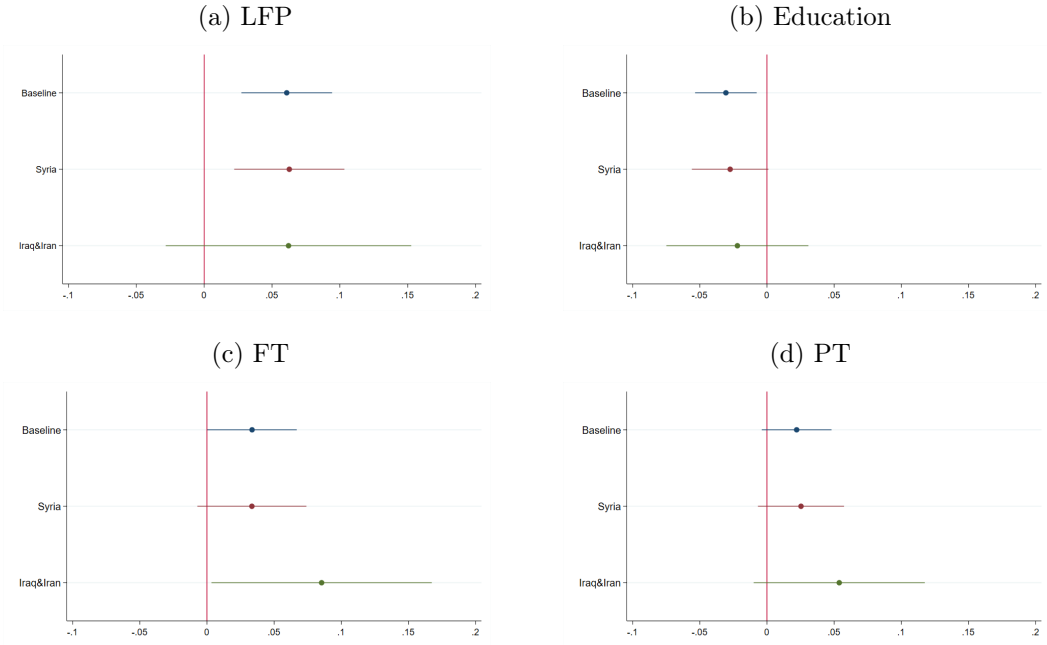
3.P.1 Heterogeneous effects by major countries of origin

Respondents could be inclined to over-report victimization if they think vulnerability is expected of them by the host community. Two institutional features alleviate this concern. First, interviewers make it clear to all respondents that the survey is conducted independent of the asylum procedure itself and information provided in the survey cannot be used against respondents. Second, asylum is granted based on individuals' safety in their home country, rather than during their journey. Nevertheless, some respondents may still give answers they deem favorable with regards to their chances of receiving protection, which could bias our unknown in an unknown manner. We address this concern by splitting up our our analysis by country of origin, exploiting the fact that Syrians who were displaced from Syria between 2014 and 2016 are particularly unlikely to give socially desired survey responses. Due to the war in Syria that spread across the entire country, the rate of Syrians who were granted protection in Germany was extremely high and stood at 97 percent over our observation period. In fact, the German government acknowledged the general need for protection of displaced Syrians and introduced so-called simplified asylum

procedures for Syrians already in November 2014. These allowed Syrian asylum seekers to get their asylum status granted by simply filling in a ten-page questionnaire and by proving that they were actually from Syria (Grote, 2018). [54]

Figure 3.P.1, shows the results for the countries of origin where more than 300 respondents are available, grouping Iraq and Iran.

Figure 3.P.1: Heterogeneity analysis



Notes: The dependent variable is binary and takes the value 1 for individuals in the labor force/education/employment/part-time or marginal employment, zero otherwise. Results are only shown for our preferred specification, corresponding to Column (2) in Table 3.5.1.

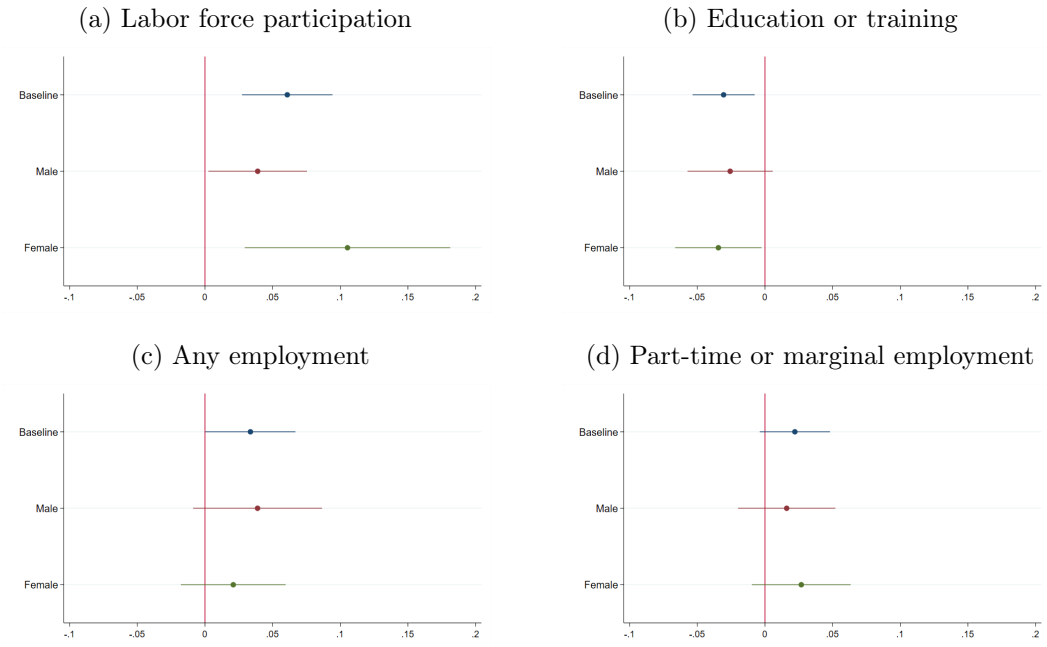
We conclude that the main results of physical victimization hold for the two largest groups in our sample, Syrians and Iraqis plus Iranians. Figures 3.P.1 and 3.P.1b show that physically victimized individuals are more likely to join the labor force rather than pursuing host-country specific education. The magnitue is very similar between these two groups although the standard errors are much larger for the Iraqis plus Iranians group (485 observations only). The association of physical victimization with part-time employment (Columns (7), (8) and (9)) on the other hand is driven primarily by Syrians, both in magnitude and statistical precision. We note that Syrians constitute by far the largest group and the more precisely estimated results are therefore not unexpected. Financial victimization shows no association with economic integration measures across the different estimations, adding further robustness to our main results.

[54]While at the time of the policy introduction there was public fear of abuse of these simplified procedures, a later assessment by the German Federal Office for Migration and Refugees found that 99.6 percent of applicants had filled in the questionnaires truthfully and were indeed Syrian Nationals (German Federal Office for Migration and Refugees, 2020).

3.P.2 Heterogeneous effects by gender

The asylum seekers entering into Germany between 2013 and 2017 mainly originated from countries where women have culturally different economic roles than men (Fuchs, Fan and Scheve, 2020). If individuals regress to a present-oriented mindset in response to victimization experiences, negative events occurring during the flight to Germany could have effects on the economic integration of refugees that differ between men and women. For example, if joining the labor force represents a bigger step for women than men, potentially traumatic events and their negative effect on mental well-being may discourage women relatively more from becoming economically active. Figure 3.P.2 therefore shows the main results of our preferred specification by gender. Recall that women constitute only 35% of our sample and hence the results for this group are more imprecisely estimated.

Figure 3.P.2: Heterogeneity analysis



Notes: The dependent variable is binary and takes the value 1 for individuals in the labor force/education/employment/part-time or marginal employment, zero otherwise. Results are only shown for our preferred specification, corresponding to Column (2) in Table 3.5.1.

The results indicate that both men and women are significantly and similarly affected by victimization events. While noting that the smaller samples lead to a loss in statistical precision, the estimated coefficients on physical victimization in figures 3.P.2 and 3.P.2b indicate that physical victimization events affect woman's decision to join the labor force instead of pursuing host-country education at an even larger magnitude than man's. For both men and women, the higher probability to join the labor force following physical victimization is mosty driven by an uptake in part-time employment (figure 3.P.2d). Financial victimization shows no association with economic integration outcomes when splitting the sample between men and women. We therefore conclude that our main results are not driven by any gender in

particular.

3.Q Main results using alternative specifications

3.Q.1 Using different sets of controls

In this section, we present the results for our main outcomes of interest using alternative specifications. These specifications differ from our baseline specification shown in equation 3.1 in the following way:

(1) Excluding individuals with missing BFM information: Only includes for whom pre-migration information, related to education and the knowledge of German, is not missing

(2) No controls: he we use our core sample of 3004 individuals, but include no controls

(3)-(5) Bad controls: we add the controls willingness to take risks and mental resilience to our preferred specification (Column (2), Table 3.5.1).[55] More resilient individuals might be better able to cope with distressing life events and willingness to take risks could account for self-selection into migration and because of its confounding effect on time preferences.

(6) Extra controls: Most undocumented migrants before the 2015 refugee crisis accumulated debt with smugglers to be able to finance their journey. While our data indicate that this is not the case for the more recent waves of asylum seekers (which constitute the majority of our sample), we explicitly control for debt with smugglers in this specification for the sub-sample of individuals for whom this information is available. We do so by including a variable that equals 1 if the individual used a smuggler and 2 if he or she chose not to report whether he or she used a smuggler or not. We further include a variable that equals 1 if the individual financed his/her trip through credit or borrowing. These variables are also considered in the post-double-selection LASSO (PDS) regressions.

The outcomes are shown in table 3.Q.1. The results all point into the same direction when compared to table 3.5.1.

[55]The mental resilience scale is based on the procedure suggested by Jacobsen, Klikar and Schupp (2017). The scale is based on the responses to four questions: "I try to think of how I can change difficult situations";"No matter what happens to me, I think I have my reactions under control"; "I think I can develop further if I deal with difficult situations" and "I actively seek ways to balance out the losses that have affected my life". The response scale ranges from 1 (disagree) to 7 (fully agree) and the resilience variable is the average of these responses. The willingness to take risk variable is based on the question "How do you rate yourself personally? In general, are you someone who is ready to take risks or do you try to avoid risks?" The response scale ranges from 0 (not prepared to take risks at all) to 10 (Prepared to take risk).

Panel A: Labor Force Participation	(1)	(2)	(3)	(4)	(5)	(6)
Physical victim.	0.0636***	0.0631***	0.0609***	0.0596***	0.0596***	0.0577***
	(0.0172)	(0.0172)	(0.0171)	(0.0179)	(0.0179)	(0.0171)
Financial victim.	-0.0137	0.0188	-0.0156	-0.0083	-0.0083	-0.0164
	(0.0167)	(0.0172)	(0.0165)	(0.0174)	(0.0174)	(0.0165)
R-squared	0.3247	0.0061	0.3220	0.3185	0.3185	0.3233
Panel B: Education or training	(1)	(2)	(3)	(4)	(5)	(6)
Physical victim.	-0.0339***	-0.0197*	-0.0304***	-0.0253**	-0.0254**	-0.0290**
	(0.0117)	(0.0108)	(0.0117)	(0.0121)	(0.0120)	(0.0118)
Financial victim.	0.0095	0.0156	0.0079	0.0003	-0.0006	0.0093
	(0.0115)	(0.0111)	(0.0113)	(0.0119)	(0.0118)	(0.0114)
R-squared	0.2051	0.0013	0.2075	0.2084	0.2106	0.2067
Panel C: Any employment	(1)	(2)	(3)	(4)	(5)	(6)
Physical victim.	0.0359**	0.0486***	0.0337**	0.0342*	0.0341*	0.0362**
	(0.0172)	(0.0165)	(0.0170)	(0.0178)	(0.0178)	(0.0171)
Financial victim.	0.0008	0.0338**	-0.0044	-0.0036	-0.0040	-0.0010
	(0.0167)	(0.0162)	(0.0164)	(0.0173)	(0.0174)	(0.0165)
R-squared	0.2619	0.0065	0.2650	0.2530	0.2533	0.2659
Panel D: Part-time or marginal employ.	(1)	(2)	(3)	(4)	(5)	(6)
Physical victim.	0.0236*	0.0127	0.0222*	0.0262*	0.0262*	0.0235*
	(0.0135)	(0.0120)	(0.0133)	(0.0138)	(0.0138)	(0.0135)
Financial victim.	-0.0014	0.0114	-0.0024	-0.0074	-0.0080	-0.0001
	(0.0130)	(0.0118)	(0.0128)	(0.0134)	(0.0134)	(0.0129)
R-squared	0.1590	0.0010	0.1587	0.1576	0.1584	0.1592
Observations	2906	3004	3004	2676	2676	3004
Individual Controls	Some	No	Yes	Yes	Yes	Yes
Year of arrival FE	Yes	No	Yes	Yes	Yes	Yes
C.origin*Departure FE	Yes	No	Yes	Yes	Yes	Yes
Willingness to take risks	No	No	Yes	No	Yes	No
Resilience	No	No	No	Yes	Yes	No
Used Smuggler	No	No	No	No	No	Yes
Credit/borrow to finance trip	No	No	No	No	No	Yes

Huber-White Standard Errors; *p<.1; **p<.05; ***p<.01

Notes: The dependent variable is binary and takes the value 1 for individuals in the labor force (Panel A), pursuing host-country education or training (Panel B), in any type of employment (Panel C) or in part-time or marginal employment (Panel D). The term FE indicates fixed effects. The term departure refers to the year-month of forceful displacement from the home country. C. of origin is the country of origin.

Table 3.Q.1: External and Internal Margin of Economic Activity - alternative specifications

3.Q.2 Using the panel structure of the data

In Table 3.Q.2, we exploit the panel variation in the data and estimate a (individual i) random effects model under the assumption that $corr(\epsilon_{i,c,t,\mu,a,f}, X) = 0$ using our preferred specification.[56] A large number of time-constant variables in the model, including the set of fixed effects related to the time of migration and the origin of individuals, makes this key assumption of a random effects model plausible in our setting (Wooldridge, 2010). We note that since all asylum seekers naturally start their stay in Germany as economically inactive and the likelihood of engaging in economic activity then increases over time, the panel estimates that capture the average effects over time are not directly comparable to the cross-sectional estimates based on only the final observation of each individual.

	LFP	Education	Any employment	Part-time or marg. emp.
	(1)	(2)	(3)	(4)
Physical victim.	0.0548***	-0.0124	0.0281**	0.0186**
	(0.0130)	(0.0084)	(0.0118)	(0.0093)
Financial victim.	-0.0017	0.0045	-0.0053	-0.0011
	(0.0127)	(0.0080)	(0.0112)	(0.0088)
Observations	6458	6458	6458	6458
Individual Controls	Yes	Yes	Yes	Yes
Year of arrival FE	Yes	Yes	Yes	Yes
C.origin*Departure FE	Yes	Yes	Yes	Yes

Huber-White Standard Errors; *p<.1; **p<.05; ***p<.01

Notes: The dependent variable is binary and takes the value 1 for individuals in the labor force (Column (1)), pursuing host-country education or training (Column (2)), in any type of employment (Column (3)) or in part-time or marginal employment (Column (4)). The term FE indicates fixed effects. The term departure refers to the -month of forceful displacement from the home country. C. of origin is the country of origin.

Table 3.Q.2: External and Internal Margin of Economic Activity - alternative specifications

The results from table 3.5.1 are confirmed in the panel regressions. Since these panel regressions include observations of the same individuals at an earlier point in time (when these individuals had spent 19 months in Germany on average) and the cross-sectional regressions do not include these observations, the stability of the coefficients when comparing the cross-sectional and panel results is noteworthy.

Two points are noteworthy. First, the panel regressions of Column (2) suggest that the effect of physical victimization on education becomes visible only after some time has been spent in the country. Second, the lower share of physically victimized refugees in education and training does not entirely close the gap to the higher labor force participation of the same group shown in Table 3.5.1 Panel B. These two observations suggest that the barriers to pursuing host country education are greater and that it takes more time to search for education opportunities than to join the labor force. Finally, the coefficients estimated on the financial victimization indicator show no effects across all specifications.

[56]Thus, this specification assumes that the individual-specific residual is uncorrelated with the explanatory variables.

3.R Testing alternative mechanisms

Variable	Mean	Std. Dev.	Max.	Min.	N
Everyone					
Protection status granted	0.748	0.434	1	0	3004
Length of asylum procedure in months	8.652	7.384	70	0	2111
Non-victimized					
Protection status granted	0.762	0.426	1	0	1408
Length of asylum procedure in months	8.603	7.573	51	0	978
Physically victimized					
Protection status granted	0.714	0.452	1	0	1090
Length of asylum procedure in months	8.969	7.284	39	0	747
Financially victimized					
Protection status granted	0.744	0.437	1	0	1154
Length of asylum procedure in months	8.327	7.100	70	0	825

Note: The variable measuring the length of the asylum procedure is not available for all asylum seekers. For the 2506 individuals in our sample for whom a decision regarding their protection status has been made, for only 2111 we have information
Our tests that the variable is not systematically missing are available upon request.

Table 3.R.1: Summary statistics asylum procedure

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Chapter 4

Migration motivation and ethnic identity of migrant couples in Germany: tied versus lead movers[1]

Teresa Freitas Monteiro (HU Berlin and IAB)

Abstract: *This study examines the determinants of the migration status within households (tied or lead mover) and how it affects the ethnic identity of migrant spouses. Tied and lead movers differ in their migration motivations, face different constraints and opportunities (e.g., social network through work). This is likely to be reflected in different investment strategies and adjustment patterns in the host country. To study the adjustment of tied and lead movers, I rely on the IAB-SOEP migration sample, which asks migrant spouses who was the main driver of the migration decision and measures several socio-economic outcomes in Germany. The results show that women are 42.2 percentage points more likely to be tied movers than men and that the spouse with lower human capital is 22.7 percentage points more likely to be a tied mover. Overall, tied movers in Germany are more likely to be separated and less likely to be integrated and assimilated when compared to lead or equal movers. These findings suggest that for tied movers, the benefits of investing in the host country's culture do not outweigh the costs.*

4.1 Introduction

The challenge migrants face regarding their commitment and sense of belonging to a culture and society (ethnic identity) only becomes salient after migration when pre- and post-migration cultures potentially clash (Constant, Gataullina and Zimmermann, 2009; Manning and Roy, 2010). Before migrating, most individuals identify with the culture they inherited from their parents in their country of origin. After migrating, individuals are exposed to a different culture and society, and feelings of belonging and commitment will develop. Particularly, individuals who migrated for family

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reasons might be more likely to experience a loss in the sense of belonging, social relations, and professional attainments.

Despite the growing literature in economics on the social and cultural integration of migrants,[2] there is little evidence on how migrating for economic reasons, or family reasons may differently affect the socio-cultural adjustment of migrants. A 'lead mover' is a family migrant for whom, even if single, the individual benefits from migration compensate for the costs, and hence it most closely resembles an economic migrant. In contrast, a 'tied mover' is a family migrant who, if single, would not have chosen to migrate (Mincer, 1978). Tied movers are, therefore, less likely to be selected on characteristics 'relevant' to the labour market where they migrated (Junge, Munk and Poutvaara, 2014; Luthra, Platt and Salamonska, 2018). Their migration motivation is intrinsically different: they moved to keep the family together rather than to improve their wages or job. Even though some tied movers choose to work in the host country, some will decide not to participate in the labour market. Particularly in such cases, the benefits of adopting the host country's culture might not compensate for the costs.[3]

Using data from the IAB-SOEP migration sample, a representative survey of the migrant population in Germany, figure 4.1.1 exhibits the raw difference between tied and lead or equal movers with regards to the two most prominent elements of ethnic identity - self-identification with respect to the country of origin (4.1.1a) and the host country (4.1.1b) - with years since migration.[4] There is a persistent gap between tied and lead movers, namely tied movers are more likely than lead movers to feel connected with the country of origin and less likely to feel German.

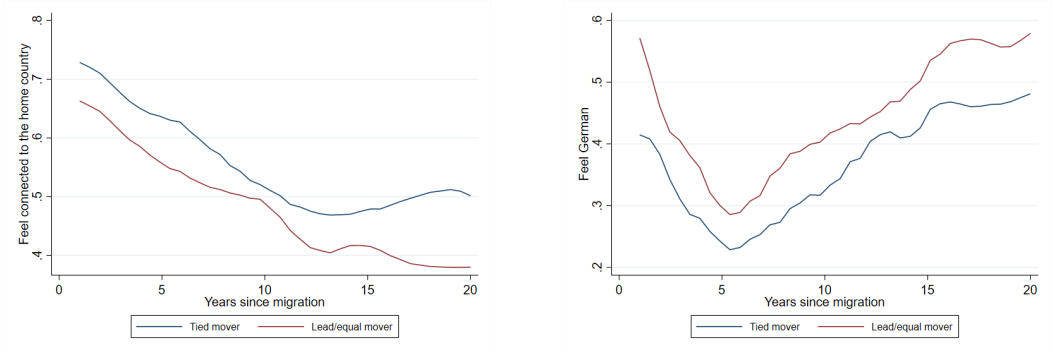
This study aims at addressing a gap in the literature by looking at the determinants of the migration position (tied versus lead or equal movers) among couples who migrated internationally[5] and by evaluating quantitatively how the migration position affects the ethnic identity of migrant spouses in Germany. The empirical analysis shows that gender and relative human capital are important predictors of who is a tied mover in a couple and that tied movers in Germany are more likely to be separated and less likely to be integrated and assimilated when compared to lead or equal movers.

[2]See for instance, Constant and Zimmermann (2008), Bisin et al. (2008), Constant, Gataullina and Zimmermann (2009), Battu and Zenou (2010), Casey and Dustmann (2010), Manning and Roy (2010), Bisin et al. (2011), Georgiadis and Manning (2011), Drydakis (2013), Facchini, Patacchini and Steinhardt (2015) and Campbell (2019)

[3]These costs can be related to spending time and effort learning a new language, creating a network with natives, among others (Epstein and Heizler, 2015; Verdier and Zenou, 2017; Wang, 2018)

[4]The sample has a panel structure but also considers individuals who are interviewed only once

[5]Most studies looking at tied and lead movers look at couples who migrated internally.



(a) Feel connected to the country of origin

(b) Feel German

Notes: "Feel connected to the country of origin" and "Feel German" are dummy variables that take the value of one if the respondent feels very strongly or strongly connected to the country, and zero otherwise

Figure 4.1.1: Self-identification

To study the determinants of the migration position among couples who migrated to Germany, I rely on a retrospective question from the IAB-SOEP migration sample, which allows identifying tied, lead, and equal migrants. Because I cannot compare movers with stayers, I look at which spouse was more likely to be a tied mover in a couple who migrated to Germany. The results suggest that gender remains a main determinant of who is a tied mover, and, in line with human capital theory, the spouse with higher (relative) human capital is more likely to be a tied mover.

After migrating, individuals decide on whether to adapt their identity to the host country by weighting the benefits, such as increasing prospects for integration, and the costs, such as spending time and effort learning a new language, creating a network with natives, among others (Epstein and Heizler, 2015; Verdier and Zenou, 2017; Wang, 2018). As tied and lead movers have different migration motivations (e.g., family versus work) and face different constraints (e.g., human capital) and opportunities (e.g., social network through work), they are likely to face different costs and benefits from investing in the host country's culture.

For evaluating the effect of the migration position on ethnic identity, I follow on Constant, Gataullina and Zimmermann (2009) and define ethnic identity as the balance between the commitment or self-identification with the culture and society of origin and the commitment or self-identification with the host culture and society, achieved by an individual after migration.[6] Ethnic identity is measured in the IAB-SOEP migration sample by bundling five elements: (i) language; (ii) future citizenship and locational plans; (iii) ethnic self-identification; (iv) ethnic interaction and (v) media consumption. In each element, individuals are classified into one of four states: assimilated, integrated, marginalized, and separated. The overall measure of assimilation, in terms of ethnic identity, counts on how many elements an individual is considered to be assimilated. The same logic is applied to the overall measure of the other three states.

Using this framework, I find that tied movers are more likely to be separated and

[6]Ethnic identity is different from the concept of ethnicity, which is a permanent characteristic related to the country of origin

less likely to be integrated or assimilated when compared to lead or equal movers. I find no difference in the likelihood of being marginalized. The results are robust to the exclusion of one element of the ethnic identity measure at the time, when looking at each element separately and when adding or excluding a series of control variables. I also show that tied movers are not more likely to be discriminated against than lead or equal movers; hence, higher antipathy from natives is unlikely to be driving my results. Because the migration position is self-reported, I confirm that a systematic miss-reporting is unlikely: individuals who are unemployed or unsatisfied with their life or health at the time of the survey are not more likely to report being tied movers. In the extensions section, I compare individuals who migrated as singles and lead or equal movers and tied movers and find that the adjustment of singles is not statistically different from that of lead or equal movers, while tied movers remain significantly different. Singles and lead or equal movers are more likely to have migrated for economic reasons and hence, everything else equal, are more likely to have similar socio-cultural integration patterns than tied movers and singles or tied movers and lead movers. Furthermore, I find a positive correlation between partners' ethnic identity states.

While being descriptive, the results in this study help to understand the implications of migrating as a tied spouse on post-migration outcomes beyond the labour market integration.

Studying the socio-cultural integration patterns of those who would not have come to Germany on their own (e.g., tied movers) is important because they are likely to be more resistant towards assimilation. Indeed, tied movers might be at risk of becoming disconnected from both the host and home country communities, which can lead to adverse spillover effects on individual outcomes and their family's outcomes. Understanding the ethnic identity of migrants is crucial since it influences their economic behaviour, return decisions, and life choices (Akerlof and Kranton, 2000). Furthermore, the ethnic identity of first-generation migrants also helps to understand the cultural integration of the second generation and the overall persistence of ethnic identity (Casey and Dustmann, 2010; Monscheuer, 2023).

This paper contributes to two streams of literature on ethnic identity and family migration. It contributes to the literature on the ethnic or national identity of migrants by showing how migrating for different motives affects the socio-cultural integration of migrants. There is a growing literature in economics on the ethnic or national identity of migrants (e.g., Constant and Zimmermann, 2008; Bisin et al., 2008; Constant, Gataullina and Zimmermann, 2009; Battu and Zenou, 2010; Casey and Dustmann, 2010; Manning and Roy, 2010; Georgiadis and Manning, 2011; Facchini, Patacchini and Steinhardt, 2015; Campbell, 2019) which finds that the original culture of immigrants is somehow resilient and although some groups adjust to the majority (natives) others display persistent differences even across generations. Most of these studies focus on the cultural adaptation of immigrants from different countries with different residency permits or citizenship rights. Nevertheless, there is little evidence on how migrating for economic or family reasons affects the socio-cultural adjustment of migrants.[7] Although these two groups benefit differently from adjusting their

[7]An exception is an UK study by Campbell (2019), who proxies the different time horizons with the original motive for migration. The author argues that refugees and family migrants are more likely to have larger time horizons and hence higher benefits from adopting the host-country national identity. Campbell's definition of family migrant considers children as well. However, the integration process of immigrant children who attended school in the UK

national identity.

This paper also contributes to the literature on family migration by analysing the driver of the migration decision in an international context and by studying a different aspect of integration that goes beyond the economic integration of spouses. Most empirical research on tied movers has focused on internal migration where pre-and post-migration characteristics and labour market outcomes are observable (Nivalainen, 2004; Juerges, 2006; Shauman, 2010; Rabe, 2011). Research on international family joint migration usually proxies tied movers by those who entered the host country with a family visa[8] (Cobb-Clark, Connolly and Worswick, 2005; Le, 2006; Adsera and Chiswick, 2007) or by relying on retrospective survey questions that ask who was the migration driver (Nikolka and Poutvaara, 2014; Krieger, 2019; Munk, Nikolka and Poutvaara, 2022). Overall, these studies find that tied movers tend to have worse labour market outcomes than primary movers even if they worked before migration (Le, 2006; Adsera and Chiswick, 2007; Krieger, 2019; Munk, Nikolka and Poutvaara, 2022) and some suggest that international family joint migration is not fully gender neutral (Junge, Munk and Poutvaara, 2014; Munk, Nikolka and Poutvaara, 2022; Krieger, 2019). Nevertheless, no empirical study in economics or sociology has looked into the sociocultural adaptation of spouses.

The psychological literature on female trailing spouses (Shaffer and Harrison, 2001; Jervis, 2011; Slobodin, 2018) documented how female trailing spouses often experience a sudden loss of sense of belonging, professional achievement, and social interactions that establish identities. However, these studies use small samples and focus on a specific group of skilled migrants.[9]

This paper is organized as follows: section 4.2 lays down the conceptual and empirical framework used in this study, and section 4.3 describes the data. Section 4.4 shows the main results, heterogeneous effects, and robustness checks. Section 4.5 considers the role of the spouses' ethnic identity and compares singles to lead or equal movers and tied movers. Lastly, section 4.6 concludes.

4.2 Conceptual and empirical framework

This section uses the two distinct literatures on tied movers and ethnic identity to formulate a hypothesis on how being a tied mover or a lead mover affects the socio-cultural adjustment at the destination country. I use the theoretical concepts and the empirical findings of two literatures to motivate my empirical specification.

is expected to differ from an individual who migrates as an adult. Furthermore, host-country national identity is only one element of the ethnic identity of individuals.

[8]While Visa categories can work as proxies for the migration motivation in countries like Australia or the US, they do not allow to identify tied movers in the context of intra-EU migration

[9]Called expatriates in business and psychological literature

4.2.1 The decision to migrate and the migration position

Following the seminal studies of Mincer (1978) and Sandell (1977) in economics,[10] and Shihadeh (1991) and Bielby and Bielby (1992) in sociology,[11],the family gains from migration can be written has $G_H = G_m + \alpha G_f$. Where $G_i = R_i - C$ are the individual $i = m, f$ net gains from migration, R_i the returns from migration[12] and C the monetary and psychological costs. $\alpha > 0$ is a relative weight assigned to the returns of the wife ($i = f$), which can depend on social norms or extra-environmental factors that are thought to affect the marriage market and hence the bargaining power of spouses[13] (e.g., divorce laws, sex ratios). For simplification, all potential destinations are aggregated into one, and it is assumed that the sign of G_m is independent of the sign of G_f and that divorce is not possible.

If single, individual i chooses to migrate if $G_i > 0$. As a household, the family will migrate if $G_H > 0$.A tied mover is an individual who, if single, would not have chosen to migrate but who migrates as part of a family, hence $G_i \leq 0$ and $G_H > 0$.[14] In such a case, the gains of the lead mover must be large enough to compensate for the losses of the tied mover.

Most empirical studies analysing couple migration decision look at the selection of tied movers and lead movers with regards to human capital and gender (Cooke, 2003; Nivalainen, 2004; Juerges, 2006; Shauman, 2010; Rabe, 2011). However, I only observe migrant couples, and I do not have the same level of information on couples who remained in their home country. As such, I can only look at couples who have already made the migration decision and analyse what determines which spouse within the couple took the role of a tied mover or a lead/equal mover.

Therefore, in this study, the probability of being a tied mover is defined as $P(TiedM_i) = P(G_H > 0 \cap G_i \leq 0)$. Following the insights from the tied mover literature, the probability of spouse i being a tied mover within a couple can be written as:

$$P(TiedM_i) = \alpha I_i S_i + \eta(G_j - G_i) + \varepsilon_{1i} \tag{4.1}$$

where I_i equals one if $i = f$ and zero otherwise. Although simpler, this specification allows for a more parsimonious empirical model and captures the essential features. The probability that individual i is a tied mover depends on how large their net gains are relative to the net gains of the spouse and depends on whether they have a 'penalty' or 'benefit', e.g. if the net gains of i are weighted differently from j.

S_i is specified as a function of social norms, and it considers the region of origin, migration cohort, religion, and the presence of a child under the age of seven. Gender

[10]These models were gender neutral in the sense that they considered how much each spouse contributes to the total family earnings, irrespective of gender. They relied on the human capital theory to explain how location decisions were made and argued that wives were more likely to be tied movers since they had a more discontinuous labour force participation and less earnings power - hence smaller gains from migration.

[11]Shihadeh (1991) and Bielby and Bielby (1992) argued that gender roles were an important explanation for the observed migration pattern of wives. Women were more likely to be tied movers not because of their lower human capital but because of their prescribed role within societies.

[12]One can think of these returns (R_i) as the difference in expected wages between origin and destination country, which depend on human capital and the distribution of wages.

[13]These weights are assumed to be exogenously given and the couple is still assumed to behave cooperatively, maximizing the weighted sum of spouse's utilities.

[14]If G_m and G_f have the same sign, there is no conflict between family members.

norms and the laws and regulations restricting women's economic opportunities differ across geographical regions and time. Because these norms and rules affect the relative bargaining strength of each spouse, S_i includes the region of origin and the year in which the migration decision was made (e.g., migration cohort).[15] Similarly, different religions have different views regarding the role of women in society, and mothers with children under the age of seven are expected to bear higher caring responsibilities.

G_i is defined as a function of human capital characteristics before migration (H) such that $G_j - G_i = f(H_j, H_i)$. Using the difference in spouses' human capital rather than the individual level is intuitive since I can only compare tied movers with lead/both movers and, hence, what matters is the human capital of an individual relative to their spouse. Consider a couple with spouse a and spouse b. If spouse a has only secondary education, this could seem predictive of being a tied mover. However, if spouse b has only primary education, then spouse a is more likely to be a lead mover, everything else equal[16]. $G_j - G_i$ includes a variable reflecting whether i is older than its spouse if i has no university degree or vocational training and the spouse has if i had better or worse oral German than its spouse and if i was not full time employed one year before migration and the spouse was. $P(TiedM_i)$ is estimated using ordinary least squares, and the standard errors are clustered at the household level.

4.2.2 After migration: ethnic identity and migration position

To define the ethnic identity of migrants, I follow on the work of Berry (1980), Berry (1997) and Berry (2006) in the psychology literature and Constant and Zimmermann (2008) and Constant, Gataullina and Zimmermann (2009) in the economics literature. According to Berry's framework, individuals can be categorized into four acculturation states which reflect the degree of devotion to the culture of origin and the culture of other groups. In the case of immigrants, an individual who strongly identifies with the host country's culture and norms but is only weakly devoted to the home country's culture is considered to have an assimilated identity. While an immigrant who exhibits strong identification with both the home and host country's culture and norms is said to have an integrated identity. On the other hand, an individual who is strongly committed to the culture of the country of ancestry but is distant from the majority culture is deemed separated. Lastly, an immigrant who is weakly connected to both the origin and host country's culture is considered to have a marginalised identity.

[15]To make the model more parsimonious, countries are grouped into regions of origin that have relatively more homogeneous gender norms: i) Central and North EU plus Switzerland and Norway (Reference), ii) South EU, iii) 2004 EU enlargement, iv) 2007-2013 EU enlargement, v) Russia and other former Soviet Union countries, vi) Former Yugoslavia and, vii) Turkey, viii) Arabic-speaking countries, ix) Central Asia, and x) others. Cohort includes before 1995 (reference), 1996-2000, 2001-05, 2006-10, and after 2011.

[16]Whether higher or lower educated have more to gain from migration depends on the distribution of wages in the home and host country. But in this case, the difference in gains between partners and the literature shows that the spouse with higher education is more likely to be a lead mover (Mincer, 1978; Cooke, 2003; Nivalainen, 2004; Juerges, 2006; Shauman, 2010; Rabe, 2011)

The ethnic identity of immigrants is associated with the degree of exposure to German society ($ExpGer_i$), exposure to home country society ($ExpHC_i$), background characteristics ($BackC_i$)[17] and social and family environment (Fam_i). Among other possible factors, the social and family environment considers the main variable of interest: being a tied mover.

The effect of being a tied mover on the different states of ethnic identity is ambiguous a priori. A key insight from the literature on the social and cultural integration of migrants[18] is that creating a new national identity may involve costs (effort in creating new social networks) and benefits (increasing prospects for integration), and these costs and benefits may vary by immigrant group. The different migration motives and expected earnings between lead movers and tied movers mean these two groups will have different incentives to invest in the host country's culture. As a simplification, the investment of migrants in the host (home) country culture can be thought of as an investment in natives (co-ethnic) network, where the cost of investing in the natives' network in terms of effort and time is higher than the cost of investing in migrant's network (Epstein and Heizler, 2015; Verdier and Zenou, 2017; Wang, 2018).[19]

As exposed in the introduction, tied movers are less likely to be selected on host country labour market 'relevant' characteristics (Junge, Munk and Poutvaara, 2014; Luthra, Platt and Salamonska, 2018). Their migration motivation is intrinsically different: they moved to keep the family together rather than to improve their wages or job. By definition, a tied mover is an individual who, if alone, would not have chosen to migrate: individual gains do not compensate for the costs.[20] While lead movers are those for whom benefits compensate the costs and whose gains are also likely to compensate for at least part of the losses of the spouse. Therefore, if the bargaining power of the lead mover is not disproportionally large, one possibility is that tied movers have lower potential earnings at entry to Germany than lead movers. By having lower expected benefits than lead movers, tied movers are less likely to invest in the natives' network. Furthermore, in the longer term, by shying away from the labour market,[21] tied movers are also less likely to be exposed to people from the host country, which leads them to have fewer opportunities to build social networks with natives.[22]

A second related possibility is that, for instance, couples with a lead and tied mover have decided to increase the family size such that it becomes an optimal

[17]Background characteristics are those acquired upon birth or that came with the migrant from the country of origin. These include factors such cultural distance (e.g., country of origin, religion) or characteristics that reflect the ability to create new social networks (e.g., age).

[18]See for example Dustmann (1996), Constant and Zimmermann (2008), Bisin et al. (2008), Constant, Gataullina and Zimmermann (2009), Battu and Zenou (2010), Casey and Dustmann (2010), Manning and Roy (2010), Bisin et al. (2011), Georgiadis and Manning (2011), Drydakis (2013), Masella (2013) and Campbell (2019)

[19]Alternatively one can think of it as the cost of identity formation or learning a new language or culture

[20]Tied movers did not expect to 'gain' in labour market terms from migration

[21]As documented in table 4.A.1 using the IAB-SOEP migration sample, tied movers are considerably less likely to be full-time employed (33.8 percent) when compared to lead or equal movers (45.7 percent). This has also been documented previously in the literature (Le, 2006; Adsera and Chiswick, 2007; Munk, Nikolka and Poutvaara, 2022; Krieger, 2019).

[22]While I cannot directly access the role of social networks, this is a possible mechanism

strategy to have one spouse focusing on the labour market (lead mover) and the other spouse concentrating on the family (tied mover).[23] In such a situation, tied movers are also less likely than lead movers to invest in the natives' network in Germany. A third possibility is that tied movers' dis-utility from spending time investing in the natives' network rather than being able to spend time with their children or taking care of household cores is higher than that of lead or equal movers. In these three cases, we expect to observe that being a tied mover is positively associated with separation or marginalization and negatively associated with integration and assimilation.

However, if the bargaining spouse of the lead mover is very large or if the difference in potential gains at entry to Germany is small, investing in creating a network and learning the German language might be worthwhile - there is no large difference in benefits or costs between tied and lead or equal movers. In these cases, we expect to observe that tied movers are as likely or less (more) likely to be separated or marginalized (integrated or assimilated) compared to lead movers. Ultimately, the direction of the effect of being a tied mover on ethnic identity is an empirical question.

The ethnic identity of migrant i interviewed at time t can be expressed as:

$$EIden_{it} = \lambda BackC_i + \gamma ExpGer_{it} + \rho ExpHC_i + \beta Fam_{it} + \varepsilon_{2i} \tag{4.2}$$

Where $EIden_i$ is a measure of ethnic identity $BackC_i$ includes gender, country of origin, and religion.[24] $ExpGer_{it}$ includes a dummy for whether vocational training was acquired in Germany (previous to the survey year), a dummy for university or school in Germany (previous to the survey year), age at immigration, age at immigration squared, years since migration, years since migration squared and survey year fixed effects. Because different states in Germany might have different institutions that help different types of migrants to integrate (e.g., associations, information centres), $ExpGer_{it}$ also includes the federal state of residency fixed effects. $ExpHC_i$ considers years of employment in the home country and years of education in the home country. Fam_{it} includes the number of children at survey year t, if there is a child in kindergarten at t and if there is a child in school at t. The main explanatory variable of interest, being a tied mover, is also included in Fam_{it}. $EIden_{it}$ is estimated using ordinary least squares, and standard errors are clustered at the household level.

4.3 Data

The empirical analysis relies on data from the IAB-SOEP Migration Sample,[25] a representative longitudinal survey[26] of migrants in Germany that started in 2013 and is conducted yearly. The sample targets individuals who migrated to Germany

[23]Although the decision to have kids is the most common reason, there can be other life-changing situations that could explain a change in the allocation of work in the family.

[24]The religion affiliations are: atheist, Islamic, Christian or other religious community

[25]I use anonymous data of the IAB-SOEP Migration Sample Survey Data, 2013-18. The IAB-SOEP Migration Sample is a joint project of the Institute for Employment Research (IAB) and the German Institute for Economic Research (DIW Berlin). Data access was provided via a Scientific Use File supplied by the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the IAB.

[26]The anchor persons were drawn from administrative data (Integrated Employment Biographies, IEB) of the Institute for Employment Research and are representative of the target population

between 1995 and 2010 and has a higher proportion of households containing migrants from the EU-New Member States and Southern European Countries. All persons living in the same household were interviewed. The first six survey waves were carried out between 2013 and 2018, with around 3,000-5,000 persons participating in each of them.

The strength of the IAB-SOEP Migration Sample relies on the battery of pre-and post-migration-specific questions that are rarely available in (general) population surveys or administrative datasets. Namely, it allows to identify if a couple was together before migration and who was the lead or tied mover. It also distinguishes between home and host country education and work experience, among others.

For the current study, I excluded individuals who migrated when they were 18 years old or younger and those who migrated at 64 years or older. Individuals entering Germany as asylum seekers were also excluded since their migration motivation tends to be very different from those whose main migration motive is either economic or family related.

4.3.1 Identifying tied movers

The tied mover analysis relies on three main questions regarding the relationship status before and after migration.

1. Were you in a serious relationship before moving to Germany?	Yes / No
2. Did this relationship continue after you moved to Germany?	Yes / No
3. What played the decisive role in your decision to move here - who was the driving force in that decision? I was / My partner / Both to an equal extent	

Table 4.3.1: Determining who is a tied mover

Only individuals who replied "Yes" to the two first questions are considered to have migrated in a couple. These individuals constitute the main sample used in this study. Combining these questions with the "driving force" question, I classify each individual who migrated as a couple as a lead mover ("I was"), equal mover ("Both to an equal extent"), or tied mover ("My partner")[27].

The final sample comprises 2,132 individuals who have reported migrating as tied movers (621), as lead movers (659), and as equal movers (852). For the analysis, I merged lead and equal movers since, in both cases, the individual wanted to move and is expected to have positive returns from moving. There are also few cases of equal movers. Both spouses are observed for most couples (89 percent), but in some cases, there is information on only one spouse (11 percent).

Table 4.A.1 in the appendix reports individual characteristics. Relevant pre-migration information is built using IAB-SOEP migration sample retrospective biographical questions, which ask individuals if they were studying or working from 15 years old until their current age, year by year. This allows me to construct a variable that indicates the years of education since the age of 15 in the home country and years of labour market experience in the home country. In some cases, pre-migration information and mainly partner pre-migration information are missing. To avoid

[27]Because this question was not asked in the first wave of the survey in 2013, some individuals didn't reply to this question. In these, if a reply from the spouse in later waves was available, I used this information.

decreasing the sample size, I allowed some of the questions to be coded as "missing pre-migration information." I show that this does not influence my results.

Around 69.6 percent of tied movers were female, while only 49.3 of lead or equal movers were female. Lead or equal movers were more likely to speak good German and to have a vocational degree than tied movers before migration. They were also more likely to be full-time employed in the year just before migration and to have more years of full-time employment experience before migration. However, around 21.0 percent of tied movers had a university degree before migration, compared to 18.9 percent among lead or equal movers. This pattern is driven by the fact that a higher share of females has a university degree from the home country (20.9 percent compared to 18.0 percent among men) and that a higher share of females is also a tied mover. The largest regions of origin are "Russia and other former Soviet Union states" and the "2004 EU enlargement"[28] with 19.1 percent and 16.2 percent, respectively. Around 54.0 percent of respondents consider themselves Christian, 24.5 percent of no religious denomination, 17.6 percent Islamic, and 3.9 percent belong to other religious communities.

4.3.2 Constructing the ethnosizer

Based on the theoretical framework described in section 4.2.2, Constant and Zimmermann (2008) and Constant, Gataullina and Zimmermann (2009) construct a measure of ethnic identity which they call the two-dimensional *ethnosizer*. Using data from the German Socio-Economic Panel (GSOEP) the authors construct the four measures of the two-dimensional ethnosizer by identifying pairs of questions in the GSOEP, which transmit information on individual commitment to the German culture and to the culture of origin.

Following on the work of Constant and Zimmermann (2008), I consider five elements: (i) language; (ii) future citizenship and locational plans; (iii) ethnic self-identification; (iv) ethnic interaction and (v) media consumption.[29] In each element, individuals are classified into one of the four states: assimilation, integration, marginalization, and separation. The overall measure of assimilation counts on how many elements an individual is considered to be assimilated (similarly for the other three states). If an individual is assimilated in all five elements, they receive a 5 in assimilation and a 0 in all other states.

Each element is constructed using the information on the commitment to the host and origin cultures. A variable reflecting devotion to German culture is paired with a similar variable characterizing the commitment to the home country's culture. To construct the first element (language), I rely on information about self-reported speaking proficiency in German and in the language of origin. For the future citizenship and locational plans element, I combine the questions on the intentions to apply for German citizenship with the one on the intention to return to the country of ancestry.[30] The ethnic self-identification elements are based on the questions asking

[28]The 2004 EU enlargement concerns the following countries: Cyprus, the Czech Republic, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia, and Slovenia

[29]The GSOEP data used by the authors differs from the one used in this study since it referred to a sample of migrants from the guest-worker population, which at the time was represented in the regular GSOEP. The IAB-SOEP migration sample, however, is the current sample representing the migrant population in Germany, and while it asks a set of additional questions, such as the tied mover one, it does not ask others.

[30]This variable is the inverse of a question which asks respondents if they wish to stay

how connected the respondent feels to the country of origin and to what extent they feel German. The ethnic interaction variable relies on questions that ask respondents if they have visited foreigners and if they have visited Germans in the past year, while media consumption relies on the question that asks respondents about the language used when consuming news.[31] Table 4.A.2 in the appendix provides basic statistics for each question.

An individual is classified as integrated in terms of ethnic identity if they feel 'very strongly' or 'strongly' connected to both Germany and the country of origin, while it is considered assimilated if it feels 'very strongly' or 'strongly' connected to Germany but 'in some respects', 'barely' or 'not at all' to the country of ancestry. Immigrants who answered that they feel 'very strongly' or 'strongly' connected to their country of origin and 'in some respects', 'barely', or 'not at all' to Germany are regarded as separated. Those answering that they feel connected 'in some respects', 'barely', or 'not at all' to both Germany and the country of origin are considered to be marginalized. The same rationale is applied to the other elements. Tables 4.A.2 and 4.A.3 in the appendix show how each element is constructed using the survey questions and answers.

The main empirical analysis in this study uses a repeated cross-section. There are several reasons why I choose to do so. First, the questions from the IAB-SOEP migration sample used to construct the ethnic identity indicators are not asked in every wave. Second, in such a short period, there is a relatively small variation in ethnic identity between waves. Third, since this study aims to evaluate the impact of being a tied mover (a time constant variable) on ethnic identity, using a fixed effects estimation would absorb the effect of this variable. Nevertheless, I will also present the results using the panel structure of data. For the cross-sectional sample, I prioritize the first-time individuals appear in a IAB-SOEP migration sample wave that asks the ethnic identity questions. This is when there is a higher response rate and when the pre-migration questions are asked.

Table 4.A.4 in the appendix reports the mean values for each element of the ethnosizer. A higher or relatively equal share of lead or equal movers is assimilated or integrated compared to tied movers.

The summary statistics of the individual characteristics used in the analysis are shown in table 4.A.1 in appendix 4.A. Overall, the proportion of lead or equal and tied movers acquiring education in Germany is low. This is not entirely surprising since individuals in this study migrated as part of a family formed in their home country and an average age of 32 years. Nevertheless, tied movers are more likely to have taken an apprenticeship, while lead or equal movers are more likely to have studied at a higher education institution. The mean years since migration for all individuals is ten years, and the largest migration cohort is 'after 2011'.

Beyond the ethnosizer, there is a growing literature in economics on the social and cultural integration of migrants, which has used different proxies for cultural or ethnic identity.[32] Most studies use one single variable as an indicator for cultural or ethnic

permanently in Germany

[31]The questions used for the language, migration history, and ethnic self-identification elements were asked in 2013, 2014, 2016, and 2018 waves of the IAB-SOEP-MIG. However, the media question was only asked in 2014, 2016, and 2018 and the questions on ethnic interaction in 2013, 2015, 2017, and 2018. For this reason, I interpolated some components between two waves so that I could measure them in the same year. Since I use only cross-section, this is not a significant problem.

[32]See for example Dustmann (1996), Constant and Zimmermann (2008), Bisin et al.

identity. For first-generation migrants, the most common measure is self-reported national identification but also friendship ties, use of native language, fertility, female employment, and children's choice of names, among others (Dustmann, 1996; Casey and Dustmann, 2010; Manning and Roy, 2010; Blau, Kahn and Papps, 2011; Drydakis, 2013; Facchini, Patacchini and Steinhardt, 2015). Constant and Zimmermann (2008) framework captures some of these measures succinctly and hence is my preferred measure, although I also show the results separately for each element.

4.4 Results

4.4.1 Main results

Table 4.4.1 shows the results corresponding to a linear estimation of the probability of being a tied mover versus lead or equal mover, as specified in equation 4.1.[33] The top panel displays the coefficients and standard errors associated with gender and espousal gaps. To ease readability, the bottom panel shows the F-tests and p-values of three joint tests that the coefficients on the interaction of gender with the three sets of fixed effects reflecting social norms are equal to zero. The coefficients and standard errors corresponding to each interaction in the bottom panel of table 4.4.1 are shown in table 4.B.1 in appendix 4.B.

The results in table 4.4.1 highlight the importance of gender in determining the migration position: females are 42.4 percentage points more likely to be tied movers than males. Consistent with the literature on internal migration,[34] relative human capital between spouses matter. Namely, an individual who before migration had lower education and worse knowledge of German than their spouse and who was not full-time employed when the partner was full-time employed, is more likely to be a tied mover. Based on the joint hypothesis testing, migration cohort and religion do not seem to contribute to explain the migration position. However, geography helps to explain gender differences in the likelihood of being a tied mover. By looking at table 4.B.1 in appendix 4.B, we can see that men from Russia and the former Soviet states and Central Asia are around 21 to 29 percentage points more likely to be tied movers, while women from Russia and the former Soviet states and Central Asia are 29 to 51 percentage points less likely to be tied movers. Women who migrated in couple in later migration cohorts were also less likely to be tied movers, potentially reflecting some changes in social norms. In section 4.4.4, I show that the results are robust to the exclusion of individuals with missing information and that it is unlikely that there are major issues with the self-reported measure of being a tied mover.

(2008), Constant, Gataullina and Zimmermann (2009), Battu and Zenou (2010), Casey and Dustmann (2010), Manning and Roy (2010), Bisin et al. (2011), Georgiadis and Manning (2011), Drydakis (2013), Masella (2013) and Campbell (2019)

[33]The control group for gender is male, for the espousal gap in German skills before migration is the same skills, for religion is no religious denomination, and for children is no children below age seven before migration. For education and employment in the year before migration, the control group is all other combinations (both have, both do not have, respondent has partner does not have).

[34]For instance Mincer (1978), Cooke (2003), Nivalainen (2004), Juerges (2006) and Shauman (2010) and Rabe (2011)

	Tied Mover	
	Coef.	SE
	(1)	(2)
Female	0.424***	(0.139)
Older than partner	-0.042	(0.027)
Not full-time employed BFM, partner full-time employed	0.056*	(0.033)
No voc. training, tech. college or uni. BFM, partner has	0.066**	(0.034)
Better German BFM than partner	-0.051*	(0.030)
Worse German BFM than partner	0.063*	(0.033)
Children bellow age 7 BFM	-0.026	(0.029)
Female × Children bellow age 7 BFM	0.046	(0.044)
Missing BFM information	-0.016	(0.021)
	F-test	PV
	(1)	(2)
Female x Religion FE	1.072	0.377
Female x Cohort FE	1.135	0.336
Female x Region of Origin FE	3.232	0.000
Observations	2132	

Standard errors in parentheses are clustered at the household level; *p<0.10, *p<0.05, *p<0.01
Notes: BFM denotes before migration, and FE refers to fixed effects. Voc. training refers to vocational training, tech. college to technical college and uni. to university. The control group for gender is male, for religion is no denomination, for the gap in German skills before migration is same skills, for education and employment in the year before migration, the control group are all other combinations (both have, both do not have, respondent has partner does not have). Religion considers no religious affiliation, Christian religion, Islamic religion, another religious community. Cohort are grouped into before 1995 (reference), 1996-2000, 2001-05, 2006-10 and after 2011. Region of origin considers the following grouping: i) Central and North EU plus Switzerland and Norway (Reference), ii) South EU, iii) 2004 EU enlargement, iv) 2007-2013 EU enlargement, v) Russia and other former Soviet Union countries, vi) Former Yugoslavia and, vii) Turkey, viii) Arabic-speaking countries, ix) Central Asia, and x) others. Cohort includes before 1995 (reference), 1996-2000, 2001-05, 2006-10, and after 2011.

Table 4.4.1: Probability of being tied mover

Table 4.4.2 shows the results for ethnic identity as measured by the ethnosizer. Besides focusing on the role of being a tied mover, I also consider the effect of gender in particular because gender was one of the key characteristics determining who is a tied mover. These findings thus demonstrate the impact of the migration position beyond gender. In panel A, columns (1)-(4) use only tied mover as an explanatory variable, and columns (5)-(8) only gender. In panel B, columns (1)-(4) consider both being tied mover and gender as explanatory variables, and columns (5)-(8) add country of origin fixed effects, survey year fixed effects, federal state fixed effects and the other individual controls as described in section 4.2.2. Tied movers score on average 0.2 points less in assimilation and 0.1 points less in integration than lead or equal movers, everything else equal. On the other hand, tied movers score on average 0.3 points more in separation than lead or equal movers. These results are significant at 0.01 percent. However, being a tied mover does not affect the strength of marginalization. This result is not entirely surprising since marginalized individuals are those who do not identify and do not have a sense of commitment to their home country. By living in a couple, both tied and lead or equal movers have the presence of a spouse and potentially of children, and hence are unlikely to feel completely disconnected from the home country.

While the direction and magnitude of the effect of being a tied mover on ethnic identity is fairly stable between panels A and B, this is not the case for gender. When looking at the effect of gender in columns (5)-(8) of panel A, we are led to think that

females might be less likely to be assimilated than men, although more likely to be integrated. However, once we control for being a tied mover, we see that females are not less likely to be assimilated than men and that they are, in fact, less likely to be separated. The coefficients on the assimilation and marginalization score in columns (5) and (7) of panel B are similar to those found by Constant and Zimmermann (2008) using an older cohort of migrants, although the coefficients on the integration and separation score in columns (6) and (8) of panel B are slightly larger.

The difference in the direction of the sign in the integration and separation scores between female and tied mover (panel B columns (4)-(8)) shows how it is the disadvantaged position in which tied movers come, rather than gender, that makes them less likely to be assimilated or integrated when compared to lead or equal movers.

Panel A:	**Tied mover only**				**Gender only**			
	Assi.	Integ.	Marg.	Separ.	Assi.	Integ.	Marg.	Separ.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tied Mover	-0.212***	-0.113**	0.031	0.295***				
	(0.041)	(0.049)	(0.035)	(0.058)				
Female					-0.081**	0.143***	0.007	-0.070
					(0.034)	(0.039)	(0.028)	(0.047)
Observations	2132	2132	2132	2132	2132	2132	2132	2132
C.of Origin FE	No	No	No	No	No	No	No	No
Survey year FE	No	No	No	No	No	No	No	No
Federal state FE	No	No	No	No	No	No	No	No
Individual controls	No	No	No	No	No	No	No	No
Panel B:	**Tied mover and gender only**				**All controls**			
	Assi.	Integ.	Marg.	Separ.	Assi.	Integ.	Marg.	Separ.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tied Mover	-0.203***	-0.147***	0.030	0.320***	-0.178***	-0.131***	0.024	0.285***
	(0.042)	(0.049)	(0.035)	(0.059)	(0.040)	(0.046)	(0.036)	(0.055)
Female	-0.046	0.168***	0.002	-0.124***	-0.026	0.128***	0.008	-0.110**
	(0.035)	(0.040)	(0.029)	(0.047)	(0.035)	(0.042)	(0.031)	(0.047)
Observations	2132	2132	2132	2132	2132	2132	2132	2132
C. of Origin FE	No	No	No	No	Yes	Yes	Yes	Yes
Survey year FE	No	No	No	No	Yes	Yes	Yes	Yes
Federal state FE	No	No	No	No	Yes	Yes	Yes	Yes
Individual controls	No	No	No	No	Yes	Yes	Yes	Yes

Standard errors in parentheses are clustered at the household level; *p<0.10, *p<0.05, *p<0.01
Notes: FE refers to fixed effects. Individual controls include age at immigration and its square, years since migration and its square, religious affiliation, education in the home country, training in Germany, University in Germany, employment years in the home country, number of children, if there is a child in school and if there is a child in kindergarten. The reference individual is male and a lead/equal mover.

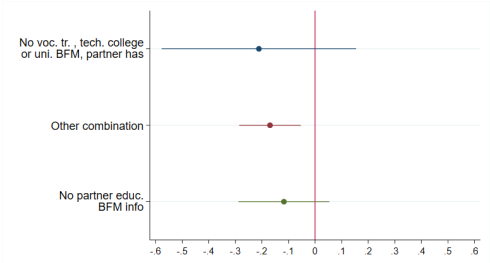
Table 4.4.2: Ethnic identity measured by the ethnosizer

The results in the table 4.B.2 in the appendix show the effect of being a tied mover on ethnic identity using the panel structure of the data. The model is estimated using a pooled OLS, and the coefficients' magnitude is close to those found using cross-sectional data.

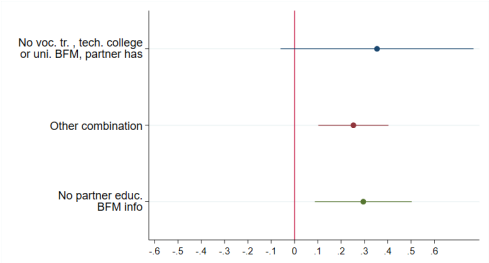
4.4.2 Heterogeneous effects

This section displays the heterogeneous effects of being a tied mover by the differences in human capital between spouses before migration and gender - the most relevant characteristics determining who is a tied mover. These pre-migration characteristics signal differences in the potential benefits of investing in the host country's culture. For ease of exposition, I only show the heterogeneous effects for the integration and

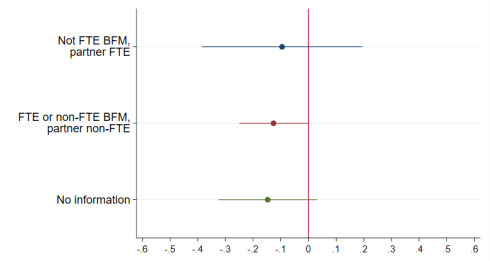
separation scores. Panel a) (b)) of figure 4.4.1 shows that the negative (positive) effect of being a tied mover on the integration (separation) score is slightly stronger in the cases where the lead mover has no vocational training, technical college or university, but the partner has one these degrees. There seems to be no particular difference on the effect of being a tied mover on the integration score by the differences in labour market status before migration between spouses (panel c)). However, tied movers who were not full-time employed in the year before migration but whose partner was, are more likely to be separated than tied movers who were full-time employed irrespective of the spouses' status (panel d)). These results suggest that tied movers with lower human capital or employment experience than the partner before migrating have generally lower incentives to invest in the host country's culture. Panel e) and f) show that the negative effect of being a tied mover on integration is more substantial for males than for females, while the positive effect of being a tied mover on separation is stronger for females than for males.



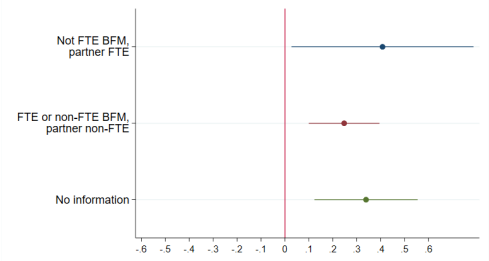
(a) Home country education: Integ.



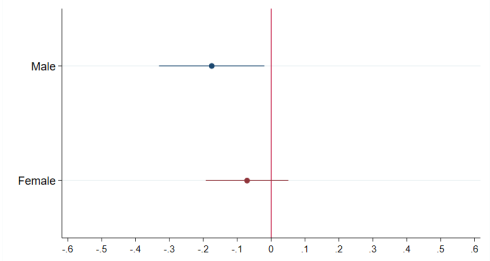
(b) Home country education: Separ.



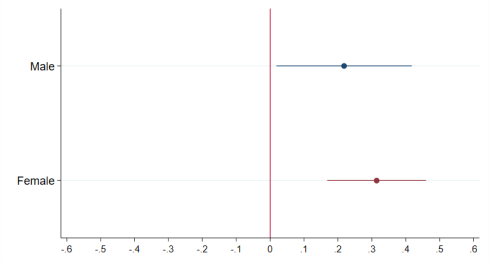
(c) Employment status BFM: Integ.



(d) Employment status BFM: Separ.



(e) Gender: Integ.



(f) Gender: Separ.

Notes: The plots in figure 4.4.1 display the coefficients on tied mover from the estimation of Equation 4.2 split by the pre-migration characteristic of the respective plot. The outcome on the left columns is the ethnosizers' integration score and on the right column is the ethnosizers' separation score. Bars identify 95% confidence intervals.

Figure 4.4.1: Heterogeneous effects of being a tied mover on integration and separation

4.4.3 Further suggestive evidence

As suggestive evidence that the choices regarding ethnic identity and employment and family preferences are linked, I look at the effect of being a tied mover on employment, wages and children born in Germany. Because of simultaneity bias between these outcomes and ethnic identity, I do not include them as controls in the ethnic identity regression but look separately at the effect of being a tied mover on each of them. MENTION KRIEGER STUDY

The results in table 4.4.3 column (1) and (2) show that tied movers are less likely to be employed than lead or equal movers, and those who are employed earn lower wages, everything else equal. As discussed in section 4.2.2, while this can reflect lower host country labour market 'relevant' characteristics among tied movers, it can also be that couples with a lead and tied mover have decided to increase the family size such that it becomes an optimal strategy to have one spouse focusing on the labour market (lead mover) and the other spouse concentrating on the family (tied mover). To analyse this possibility using the same set up, I check if females who migrated as tied movers are more likely to have a child born in Germany than females who migrated as lead or equal movers.[35] Column (3) of table 4.4.3 confirms that female tied movers are 5 percentage points more likely to have a child born in Germany.

	Employed	Ln Wage	A child born Germany
	(1)	(2)	(3)
Tied Mover	-0.050**	-0.082***	0.055**
	(0.022)	(0.030)	(0.026)
Female	-0.285***	-0.199***	
	(0.020)	(0.029)	
Observations	2132	1107	1159
C. of Origin FE	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes
Federal state FE	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes

Standard errors in parentheses are clustered at the household level; *p<0.10, *p<0.05, *p<0.01
Notes: FE refers to fixed effects. Individual controls include age at immigration and its square, years since migration and its square, religious affiliation, education in the home country, training in Germany, University in Germany, employment years in the home country, number of children, if there is a child in school and if there is a child in kindergarten. The reference individual is male and a lead/equal mover.

Table 4.4.3: Employment, wages and children born in Germany

4.4.4 Robustness checks

In this section, I perform a series of robustness checks to analyse the stability and credibility of my results. First, I test if there are issues with the self-reported measure of being a tied mover. Secondly, I analyse the stability of the results when excluding individuals with missing information, excluding potentially bad controls (education acquired in Germany), and adding other potentially bad controls (employment status in Germany). Thirdly, I show that my results are robust to different constructions of the ethnosizer. Finally, I look at potential alternative mechanisms. Overall, I can conclude that the main results remain stable.

[35] In the regression I substitute the total number of children and having a child in kindergarten or school with the number of children before migration

Self-reported migration position: One of the shortcomings in this study is that the migration position is self-reported. Hence, it could be that individuals who are not satisfied with their life in Germany or have serious health problems report being tied movers. Similarly, individuals who cannot find a job might be more likely to report having been tied movers as a mechanism to justify their labour market status. To check if these factors influence the reporting of the migration position, I look at the effect of not being satisfied with life, not being satisfied with health, and being registered as unemployed in Germany on the likelihood of being a tied mover using equation 4.1. These variables are measured after the migration decision is made and refer to life satisfaction, health satisfaction, and unemployment in Germany specifically. The results are reported in table 4.4.4 and show that individuals who are unemployed or not satisfied with their life or health at the time of the survey are not more likely to report being tied movers.

	Tied Mover		
	(1)	(2)	(3)
Not satisfied with life	0.040		
	(0.046)		
Not satisfied with health		0.007	
		(0.031)	
Registered unemployed			-0.014
			(0.024)
Observations	2132	2128	2131
Female x Religion FE	Yes	Yes	Yes
Female x Child bellow 7 BFM	Yes	Yes	Yes
Female x Cohort FE	Yes	Yes	Yes
Female x Region of Origin FE	Yes	Yes	Yes
Female dummy & espousal gaps in HC	Yes	Yes	Yes

Standard errors in parentheses are clustered at the household level; *p<0.10, *p<0.05, *p<0.01
Notes: FE refers to fixed effects. HC refers to human capital. Espousal gap in human capital includes the following dummy variables: whether the respondent is older than their spouse, if the respondent has no university degree or vocational training and the spouse has, if the respondent had better or worse knowledge of German (speaking) than its spouse and if the respondent was not full time employed one year before migration and the spouse was.

Table 4.4.4: Self-reported migration position

Excluding information and adding extra controls: I start by excluding individuals with missing pre-migration information or with missing partner pre-migration information (942 observations) in the tied mover regression. The results are displayed in table 4.C.1 in appendix 4.C and show that the main conclusions regarding the role of gender and human capital hold. Table 4.4.5 shows the results for the ethnosizer when excluding individuals with missing pre-migration information (panel A, columns (5)-(8)), excluding the potentially bad controls "having acquired vocational training in Germany" and "having attended university or school in Germany" (panel B, columns (1)-(4)), and when adding potentially bad control related to the labour market status in Germany (panel B, columns (5)-(8)). These changes do not impact the sign or magnitude of the coefficients on the main variables of interest. Consistent with the previous findings in the literature, non-employed individuals are less likely to be integrated and assimilated and more likely to be separated or marginalized than full-time employed individuals.

Panel A:	**Benchmark OLS**				**Excl. indiv. with missing information**			
	Assi.	Integ.	Marg.	Separ.	Assi.	Integ.	Marg.	Separ.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tied Mover	-0.178***	-0.131***	0.024	0.285***	-0.193***	-0.140***	0.047	0.285***
	(0.040)	(0.046)	(0.036)	(0.055)	(0.043)	(0.048)	(0.038)	(0.059)
Female	-0.026	0.128***	0.008	-0.110**	-0.018	0.181***	-0.019	-0.144***
	(0.035)	(0.042)	(0.031)	(0.047)	(0.037)	(0.044)	(0.033)	(0.051)
Observations	2132	2132	2132	2132	1874	1874	1874	1874
Panel B:	**Excl. education in Germany**				**Controlling for employment**			
	Assi.	Integ.	Marg.	Separ.	Assi.	Integ.	Marg.	Separ.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tied Mover	-0.176***	-0.134***	0.026	0.285***	-0.173***	-0.121***	0.017	0.278***
	(0.040)	(0.046)	(0.036)	(0.056)	(0.040)	(0.046)	(0.036)	(0.055)
Female	-0.023	0.127***	0.009	-0.113**	0.022	0.184***	-0.034	-0.172***
	(0.036)	(0.042)	(0.031)	(0.047)	(0.040)	(0.048)	(0.035)	(0.053)
Part-time					-0.085	0.017	0.050	0.018
					(0.062)	(0.068)	(0.055)	(0.077)
Not employed					-0.113**	-0.229***	0.078*	0.264***
					(0.051)	(0.057)	(0.045)	(0.066)
Other labour market status					-0.140**	-0.145*	0.221***	0.065
					(0.068)	(0.082)	(0.065)	(0.098)
Observations	2132	2132	2132	2132	2132	2132	2132	2132
C. of Origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Federal state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indiv. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses are clustered at the household level; *p<0.10, *p<0.05, *p<0.01
Notes: FE refers to fixed effects. Individual controls include age at immigration and its square, years since migration and its square, religious affiliation, education in the home country, training in Germany, University in Germany, employment years in the home country, number of children, if there is a child in school and if there is a child in kindergarten. The reference individual is male and a lead/equal mover. In columns (5)-(8) of panel B the reference individual is full-time employed.

Table 4.4.5: Ethnosizer: Excluding missing information or education in Germany and controlling for employment status in Germany

Excluding one element at the time and looking at individual components: Figure 4.C.1 in appendix 4.C compares the results of the effect of being a tied mover the ethnosizer when using all elements and when excluding one element at the time. We can see that the main results remain stable and that no particular element is driving the results. Table 4.C.2 in the appendix shows the results for each variable composing the ethnosizer using the same specification as in equation 4.2. These outcomes are not directly comparable as they cannot be analysed in terms of being assimilated, integrated, marginalized, or separated. The results in table 4.C.2 are consistent with the results using the ethnosizer and show that tied movers are more likely to feel connected with the country of origin and to consume media in the language of the country of origin. However, tied movers are less likely to have a good command of German, feel German, or intend to acquire German citizenship.

Alternative mechanisms: It could be that tied movers are less likely to be assimilated or integrated and more likely to be separated because they face higher discrimination from natives than lead or equal movers. This could happen if, for instance, natives perceive that tied movers are less likely to contribute to the local economy or that their visa access has less merit and depends entirely on their spouses' merit. To test this, I use a question in the IAB-SOEP migration sample that asks respondents if they felt discriminated against in their everyday life over the past two years. This question was only asked in 2013, 2015, and 2017, so the sample size is slightly reduced. The results in column (1) of table 4.C.3 in appendix 4.C suggest that tied movers had no more discriminatory experiences when compared to lead movers. Similarly, in columns (2)-(5) of table 4.C.3 in the appendix I confirm that the

effect of being a tied mover on the ethnosizer is not driven by a higher concern with regards to hostility towards foreigners, cohesion in society, own retirement pension or own health among tied movers.

This section provided some robustness checks to the findings that gender and human capital are important determinants of the espousal migration position and that migrating as a tied mover has a negative effect on being integrated or assimilated in Germany but a positive effect on being separated. Despite the effect of the tied mover variable on ethnic identity being robust to the inclusion of different control variables, I cannot rule out that there exist unobserved individual characteristics driving the migration position and the level of integration or assimilation in Germany. Hence, a causal interpretation cannot be given to these results. Designing a causal setup for studying post-migration outcomes of tied and lead movers would be difficult and largely unreliable. The counterfactual of a spouse taking the role of a tied mover would be to take the role of a lead or equal mover. However, in such a counterfactual, we would not observe this spouse and their family in Germany - by definition, a tied spouse is a family migrant who would not have chosen to migrate to the observed location. Nevertheless, we know very little about the consequences of migrating internationally as a tied mover on post-migration outcomes and this study helps to shed some light on the subject.

4.5 Extensions

So far, this study has focused on the migration position as the most interesting factor affecting ethnic identity. However, the commitment of other household members is another interesting and related social and family factor affecting the ethnic identity of migrant couples. In this section, I look at the role of the partner ethnic identity.

I also extend my analysis to include individuals who migrated as singles and see how these compare with tied and lead or equal movers. In principle, individuals who migrated without having to take the family into consideration are a very different group. Nevertheless, they might offer interesting insights since single, and lead or equal movers had more similar gains from coming to Germany than single and tied movers.

4.5.1 Spouse ethnic identity

The integration or assimilation of a spouse might also have an effect on an individual connectedness to the host country culture and society. As a couple, individuals are likely to share common experiences outside work, such as meeting friends or other social events. A spouse who feels closer to German society due to contacts through work, for instance, might be able to push his or her partner to attend events or proportionate contacts that are closer to the German culture. Furthermore, couples usually share their frustrations or sources of happiness. Feelings of empathy and care for a spouse are likely to influence an individual's happiness and feeling of belonging. Consider an individual whose spouse feels alienated from German society, who has great difficulty in learning the language, who misses other family and friends and therefore expresses strong desires to return back to the home country. Because people who live as a couple usually care for each other, this individual is more likely to also want to return to the home country for the sake of their spouse's well-being.

Hence, it would not be unexpected if the integration (separation) or assimilation (marginalization) of partners is positively correlated.

Table 4.5.1 panel A shows the results when adding the partner's ethnic identity. Migrants whose spouse is assimilated are more likely to be assimilated and less likely to be marginalized or separated. Similarly, migrants whose partner is integrated are less likely to be marginalized and more likely to be integrated. An individual with a spouse who is separated is more likely to be separated and less likely to be marginalized. These results are as expected; individuals in a couple are likely to benefit from each other knowledge and social connections. They are also more likely to share frustrations and decide on future plans together, exerting influence on each other.

Table 4.C.4 in the appendix shows the results for the difference in the ethnosizer between partners when information on both is available. Although not comparable in magnitude, the tied mover results are significant and in line with those in table 4.5.1.

4.5.2 Including married individuals who arrived as singles

A lead or an equal mover is a spouse who, if single, would still have chosen to migrate. Hence, both single movers and lead or equal movers are expected to gain individually from migration. One can therefore expect that the adjustment pattern of lead or equal movers is closer to that of single migrants than that of tied migrants.

In this section, I consider the ethnic identity of individuals who arrived as singles in Germany and who lived in a couple at the time of the survey. I choose individuals who live in a couple to make them more comparable to lead or equal movers and tied movers (who also live as a couple). 729 individuals migrated as singles and lived in a couple at the time of the survey. The baseline category remains a lead or equal mover. The results in table 4.5.1 show that single movers who, at the time of the survey live in a couple in Germany are not statistically different from lead or equal movers. The effect of being a tied mover remains fairly similar.

Panel A: Partner ethnic identity	Assi.	Integ.	Marg.	Separ.
	(1)	(2)	(3)	(4)
Tied mover	-0.195***	-0.145***	0.032	0.308***
	(0.040)	(0.046)	(0.036)	(0.056)
Female	-0.018	0.100**	0.012	-0.094*
	(0.039)	(0.045)	(0.033)	(0.052)
Partner separated	-0.055	-0.004	-0.398***	0.457***
	(0.086)	(0.102)	(0.101)	(0.130)
Partner integrated	0.051	0.493***	-0.361***	-0.183
	(0.084)	(0.104)	(0.098)	(0.117)
Partner assimilated	0.520***	0.145	-0.397***	-0.268**
	(0.110)	(0.111)	(0.108)	(0.129)
No partner information	0.039	0.508***	-0.374***	-0.173
	(0.083)	(0.098)	(0.095)	(0.114)
Observations	2132	2132	2132	2132
Panel B: Including singles	Assi.	Integ.	Marg.	Separ.
	(1)	(2)	(3)	(4)
Single mover	-0.026	0.006	0.025	-0.006
	(0.046)	(0.054)	(0.041)	(0.059)
Tied mover	-0.176***	-0.122***	0.021	0.278***
	(0.039)	(0.045)	(0.036)	(0.054)
Female	-0.060**	0.106***	0.052*	-0.098**
	(0.030)	(0.035)	(0.027)	(0.039)
Observations	2861	2861	2861	2861
Country of Origin FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Federal state FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes

Standard errors in parentheses are clustered at the household level; *p<0.10, *p<0.05, *p<0.01
Notes: FE refers to fixed effects. Individual controls include age at immigration and its square, years since migration and its square, religious affiliation, education in the home country, training in Germany, University in Germany, employment years in the home country, number of children, if there is a child in school and if there is a child in kindergarten. The reference individual is male and a lead/equal mover.

Table 4.5.1: Including partner ethnic identity and singles

4.6 Conclusion

This study examined the determinants of the migration position (tied mover or lead and equal mover) of spouses who migrated as a couple to Germany and the ethnic identity of first-generation migrant spouses depending on who was the migration driver (tied or lead mover). The results show that gender remains a main determinant of who is a tied mover within a couple, and, in line with human capital theory, the spouse with lower (relative) human capital is more likely to be a tied mover. When looking at the effect of the migration position on ethnic identity, this study finds that tied movers are more likely to be separated and less likely to be integrated and assimilated. These findings suggest that for tied movers, the psychological costs of distancing from the culture of their country of ancestry do not compensate the benefits of investing in the host country's culture. I have shown that these results are robust to a series of robustness checks and presented suggestive evidence that single migrants are not different from lead or equal migrants. This result is not entirely surprising, as both groups expected to gain individually from migration. As highlighted in the introduction, a causal interpretation cannot be given to these

results, but nevertheless, they help to understand the implications of migrating as a tied spouse on post-migration outcomes which go beyond the labour market integration.

Migration into Germany has grown substantially over the past decade. The degree of economic, political, and cultural integration of migrants became one of the most pressing topics in the German political debate. A good understanding of the different integration processes is thus essential to design effective integration policies. The findings in this study suggest that tied migrants are more likely to struggle to assimilate and integrate into German culture and society. Integrating entire families might have important consequences for retaining migrants in Germany and using their full labour market potential. Although not explicitly analysed in this study, the lower integration among tied movers can not only hinder even further their labour market prospects but it might also affect the integration of their children through inter-generational transmission of culture.

Appendix

4.A Statistics

	Lead/equal mover	Tied mover	Total	Obs.
Panel A: Time constant				
Female				
Male	50.695	30.435	44.794	955
Female	49.305	69.565	55.206	1,177
Region of origin				
Central & North EU+Switzerland + Norway	1.655	1.449	1.595	34
South EU	8.802	9.018	8.865	189
2004 EU enlargement	16.082	16.425	16.182	345
2007-2013 EU enlargement	15.156	14.171	14.869	317
Russia + other former Soviet Union	19.060	19.324	19.137	408
Former Yugoslavia	8.140	7.407	7.927	169
Turkey	5.162	9.179	6.332	135
Arab Countries	6.750	4.670	6.144	131
Central Asia	10.920	9.018	10.366	221
Others	8.273	9.340	8.583	183
Belongs to church/religious community				
No denomination	23.759	26.409	24.531	523
Islamic religion	17.207	18.519	17.589	375
Christian religion	54.732	52.174	53.987	1,151
Another religious comm.	4.302	2.899	3.893	83
Panel B: Pre-migration				
German Skills BFM				
Poor German	71.476	79.549	73.827	1,574
Fair German	14.494	11.111	13.508	288
Good German	13.236	8.857	11.961	255
No information	0.794	0.483	0.704	15
Vocational training in home country				
No vocational training	68.140	71.380	69.104	1,380
Vocational training	31.860	28.620	30.896	617
University degree in home country				
No university degree	81.041	78.956	80.421	1,606
University degree	18.959	21.044	19.579	391
Years of full-time employment BFM				
0-1 years	20.979	24.638	22.045	470
2-5 years	16.413	18.519	17.026	363
6-12 years	22.700	23.027	22.795	486
13 or more years	31.502	25.604	29.784	635
No information	8.405	8.213	8.349	178
Full-time employed in the year BFM				
Not full-time employed	34.613	41.546	36.632	781
Full-time employed	58.769	53.140	57.129	1,218
No information	6.618	5.314	6.238	133
Children bellow age 7 BFM				
No children bellow age 7 BFM	72.005	71.498	71.857	1,532
Children bellow age 7 BFM	27.995	28.502	28.143	600
Migration cohort				
before 1995	14.957	14.815	14.916	318
1996-2000	18.134	19.646	18.574	396
2001-2005	21.046	19.485	20.591	439
2006-2010	18.597	21.417	19.418	414
after 2011	27.267	24.638	26.501	565
Age at migration	31.922	31.403	31.777	2,132
Panel C: Post-migration				
Attended School in Germany				
No School	89.080	92.915	90.197	1,923
School	10.920	7.085	9.803	209
Apprent./vocational training in Germany				
No apprent./vocational training	90.073	89.694	89.962	1,918
Apprent./vocational training	9.927	10.306	10.038	214
University in Germany				
No university	98.412	99.356	98.687	2,104
University	1.588	0.644	1.313	28
Employment status in Germany				
Full-time employed	45.731	33.816	42.261	901
Part-time employed	12.972	14.654	13.462	287
Not employed	33.355	39.775	35.225	751
Other labour market status	7.942	11.755	9.053	193
Years since migration	10.119	10.082	10.108	2,132

Table 4.A.1: Individual Characteristics

	Lead/equal mover	Tied mover	Total	Obs.
	%	%	%	
1. Knowledge of the language from the country of origin				
1.1. Nod bad, bad or very bad	2.515	1.932	2.345	50
1.2. Good of very good	97.485	98.068	97.655	2,082
2. Knowledge of German language				
2.1. Nod bad, bad or very bad	42.952	51.047	45.310	966
2.2. Good of very good	57.048	48.953	54.690	1,166
3. Plans to return to country of origin*				
3.1. No	81.866	78.744	80.957	1,726
3.2. Yes	18.134	21.256	19.043	406
4. Plans to acquire German citizenship				
4.1. Improbable or definitely not	25.961	34.861	28.475	506
4.2. Has acquired, will definitely or probably acquire	74.039	65.139	71.525	1,271
5. Feel connected to the country of origin				
5.1. In some respects, hardly or not at all	51.423	41.385	48.499	1,034
5.2. Very strongly or strongly	48.577	58.615	51.501	1,098
6. Feel German				
6.1. In some respects, hardly or not at all	61.946	70.692	64.493	1,375
6.2. Completely or mostly	38.054	29.308	35.507	757
7. Visited foreigners in the previous year				
7.1. No	12.111	12.560	12.242	261
7.2. Yes	87.889	87.440	87.758	1,871
8. Visited Germans in the previous year				
8.1. No	23.958	26.087	24.578	524
8.2. Yes	76.042	73.913	75.422	1,608
9. News media consumption				
9.1. Exclusively or mostly lang. origin	36.341	41.365	37.789	653
9.2. Equally often German and lang. origin	49.350	44.378	47.917	828
9.3. Exclusively or mostly German	13.577	13.855	13.657	236
9.4. Does not apply, does not use	0.732	0.402	0.637	11

*This variable is the inverse of a question which asks respondents if they wish to stay permanently in Germany

Table 4.A.2: Ethnic identity components

An individual is considered to be: (the numbers correspond to the answer given to the questions in table 4.A.2)
Language
Assimilated if 1.1. and 2.2.
Integrated if 1.2. and 2.2.
Separated if 1.2. and 2.1.
Marginalized if 1.1. and 2.1.
Future citizenship and locational plans
Assimilated if 3.1. and 4.2.
Integrated if 3.2. and 4.2.
Separated if 3.2. and 4.1.
Marginalized if 3.1. and 4.1.
Ethnic self-identification
Assimilated if 5.1. and 6.2.
Integrated if 5.2. and 6.2.
Separated if 5.2. and 6.1.
Marginalized if 5.1. and 6.1.
Ethnic interaction
Assimilated if 7.1. and 8.2.
Integrated if 7.2. and 8.2.
Separated if 7.2. and 8.1.
Marginalized if 7.1. and 8.1.
Media consumption
Assimilated if 9.3.
Integrated if 9.2.
Separated if 9.1
Marginalized if 9.4.

Table 4.A.3: Construction of ethnic identity elements

	Lead/equal mover	Tied mover	Total
Language: Assi.	0.017	0.006	0.014
Language: Integ.	0.554	0.483	0.533
Language: Marg.	0.009	0.013	0.010
Language: Separ.	0.421	0.498	0.444
Future citizen. and loc. plans: Assi.	0.424	0.337	0.398
Future citizen. and loc. plans: Integ.	0.038	0.023	0.033
Future citizen. and loc. plans: Marg.	0.395	0.451	0.411
Future citizen. and loc. plans: Separ.	0.144	0.190	0.157
Self-identification: Assi.	0.234	0.158	0.212
Self-identification: Integ.	0.147	0.135	0.144
Self-identification: Marg.	0.281	0.256	0.273
Self-identification: Separ.	0.339	0.451	0.371
Ethnic interaction: Assi.	0.040	0.047	0.042
Ethnic interaction: Integ.	0.721	0.692	0.712
Ethnic interaction: Marg.	0.081	0.079	0.081
Ethnic interaction: Separ.	0.158	0.182	0.165
Media consumption: Assi.	0.402	0.356	0.389
Media consumption: Integ.	0.296	0.309	0.300
Media consumption: Marg.	0.006	0.003	0.005
Media consumption: Separ.	0.296	0.332	0.306
Observations	2132	2132	2132

Table 4.A.4: Ethnic identity and elements

4.B Main results

	Tied Mover	
	Coef.	SE
	(1)	(2)
Female	0.424***	(0.139)
Older than partner	-0.042	(0.027)
Not full-time employed BFM, partner full-time employed	0.056*	(0.033)
No voc. training, tech. college or university BFM, partner has	0.066*	(0.034)
Better German BFM than partner	-0.051*	(0.030)
Worse German BFM than partner	0.063*	(0.033)
Missing partner info.	-0.016	(0.021)
Children bellow age 7 BFM	-0.026	(0.029)
Children bellow age 7 BFM × Female	0.046	(0.044)
Islamic religion	-0.018	(0.048)
Christian religion	-0.030	(0.033)
Another religious comm.	-0.101*	(0.060)
Islamic religion × Female	-0.045	(0.077)
Christian religion × Female	0.009	(0.051)
Another religious comm. × Female	-0.014	(0.100)
1996-00	0.013	(0.044)
2001-05	0.035	(0.045)
2006-10	0.099**	(0.046)
aft 2011	0.046	(0.042)
1996-00 × Female	-0.026	(0.070)
2001-05 × Female	-0.100	(0.070)
2006-10 × Female	-0.125*	(0.074)
aft 2011 × Female	-0.117*	(0.068)
South EU	0.122	(0.089)
EU enlargement, 2004	0.124	(0.086)
EU enlargement, 2007-2013	0.109	(0.087)
Russia + other former Soviet Union	0.205**	(0.087)
Former Yugoslavia	0.148	(0.096)
Turkey	0.263**	(0.104)
Arab Countries	0.059	(0.090)
Central Asia	0.289***	(0.094)
Others	0.174*	(0.095)
South EU × Female	-0.108	(0.141)
EU enlargement, 2004 × Female	-0.148	(0.133)
EU enlargement, 2007-2013 × Female	-0.156	(0.133)
Russia + other former Soviet Union × Female	-0.292**	(0.135)
Former Yugoslavia+2 × Female	-0.182	(0.151)
Turkey × Female	-0.097	(0.167)
Arab Countries × Female	-0.082	(0.150)
Central Asia × Female	-0.516***	(0.143)
Others × Female	-0.166	(0.146)
Constant	0.045	(0.087)
Observations	2132	

Standard errors in parentheses are clustered at the household level; *p<0.10, *p<0.05, *p<0.01
Notes: BFM denotes before migration, and FE refers to fixed effects. Voc. training refers to vocational training, tech. college to technical college and uni. to university. The control group for gender is male, for religion is no denomination, for the gap in German skills before migration is same skills, for education and employment in the year before migration, the control group are all other combinations (both have, both do not have, respondent has partner does not have). Cohort includes before 1995 (reference), 1996-2000, 2001-05, 2006-10 and after 2011. The categories for the region of origin are i) Central and North EU plus Switzerland and Norway (Reference), ii) South EU, iii) 2004 EU enlargement, iv) 2007-2013 EU enlargement, v) Russia and other former Soviet Union countries, vi) Former Yugoslavia and, vii) Turkey, viii) Arabic-speaking countries, ix) Central Asia, and x) others.

Table 4.B.1: Probability of being tied mover

	Assi.	Integ.	Marg.	Separ.
	(1)	(2)	(3)	(4)
Tied mover	-0.175***	-0.102***	0.018	0.259***
	(0.033)	(0.037)	(0.029)	(0.045)
Female	-0.000	0.120***	-0.028	-0.092**
	(0.029)	(0.034)	(0.025)	(0.040)
Observations	10721	10721	10721	10721
Country of Origin FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Federal state FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes

Standard errors in parentheses are clustered at the household level; *p<0.10, *p<0.05, *p<0.01
Notes: FE refers to fixed effects. Individual controls include age at immigration and its square, years since migration and its square, religious affiliation, education in the home country, training in Germany, University in Germany, employment years in the home country, number of children, if there is a child in school and if there is a child in kindergarten. The reference individual is male and a lead/equal mover.

Table 4.B.2: Ethnic Identity: Panel data (pooled OLS)

4.C Robustness checks

Tables

	Tied Mover	
	Benchmark OLS	Excl. missings
	(1)	(2)
Female	0.424***	0.687***
	(0.139)	(0.188)
Older than partner	-0.042	-0.019
	(0.027)	(0.034)
Not full-time employed BFM, partner full-time employed	0.056*	0.072**
	(0.033)	(0.036)
No voc. training, tech. college or university BFM, partner has	0.066*	0.064*
	(0.034)	(0.037)
Better German BFM than partner	-0.051*	-0.086**
	(0.030)	(0.034)
Worse German BFM than partner	0.063*	0.044
	(0.033)	(0.038)
Missing BFM information	-0.016	(.)
	(0.021)	(.)
Observations	2132	1184
Female x Religion FE	Yes	Yes
Female x Child bellow 7 BFM	Yes	Yes
Female x Cohort FE	Yes	Yes
Female x Region of Origin FE	Yes	Yes

Standard errors in parentheses are clustered at the household level; *p<0.10, *p<0.05, *p<0.01
Notes: BFM denotes before migration, and FE refers to fixed effects. Voc. training refers to vocational training, tech. college to technical college and uni. to university. The reference group for gender is male, for religion is no denomination, for the gap in German skills before migration is same skills, for education and employment in the year before migration, the control group are all other combinations (both have, both do not have, respondent has partner does not have).

Table 4.C.1: Probability of being tied mover: Excluding missing information

	Lang. C. Origin	German Lang.	Return to C. Origin	Acquire Ger. Citizenship	Feel Conn. to C. Origin
	(1)	(2)	(3)	(4)	(5)
Tied Mover	0.002	-0.093***	0.007	-0.286***	0.197***
	(0.007)	(0.022)	(0.017)	(0.063)	(0.053)
Female	-0.003	0.083***	-0.014	-0.026	0.018
	(0.007)	(0.021)	(0.015)	(0.055)	(0.047)
	Feel German	Visited Foreigners	Visited Germans	Media in Lang. Orig.	Media in German
	(6)	(7)	(8)	(9)	(10)
Tied Mover	-0.168**	-0.004	-0.032	0.044**	0.009
	(0.066)	(0.016)	(0.020)	(0.021)	(0.021)
Female	-0.116**	0.021	0.024	-0.029	0.048***
	(0.058)	(0.013)	(0.016)	(0.018)	(0.018)
Observations	2132	2132	2132	2132	2132
Country of Origin FE	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes
Federal state FE	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses are clustered at the household level; *p<0.10, *p<0.05, *p<0.01
Notes: FE refers to fixed effects. Individual controls include age at immigration and its square, years since migration and its square, religious affiliation, education in the home country, training in Germany, University in Germany, employment years in the home country, number of children, if there is a child in school and if there is a child in kindergarten. The reference individual is male and a lead/equal mover.

Table 4.C.2: Individual components of the Ethnosizer

	Feel Discriminated	Worried About Hostility To Foreigners	Worried About Cohesion in Society	Worried About Own Retirement Pension	Worried About Own Health
	(1)	(2)	(3)	(4)	(5)
Tied Mover	0.015	-0.011	-0.022	-0.017	-0.035
	(0.024)	(0.031)	(0.033)	(0.036)	(0.034)
Female	-0.009	-0.042	-0.026	-0.100***	-0.104***
	(0.021)	(0.028)	(0.030)	(0.032)	(0.030)
Observations	2018	2131	1866	1879	2127
Country of Origin FE	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes
Federal state FE	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses are clustered at the household level; *p<0.10, *p<0.05, *p<0.01
Notes: FE refers to fixed effects. Individual controls include age at immigration and its square, years since migration and its square, religious affiliation, education in the home country, training in Germany, University in Germany, employment years in the home country, number of children, if there is a child in school and if there is a child in kindergarten. The reference individual is male and a lead/equal mover.

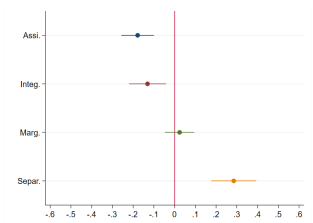
Table 4.C.3: Alternative mechanisms

	Assi.	Integ.	Marg.	Separ.
	(1)	(2)	(3)	(4)
Tied Mover	-0.342***	-0.367***	0.122**	0.587***
	(0.070)	(0.079)	(0.059)	(0.098)
Female	-0.108	0.074	0.035	-0.001
	(0.076)	(0.083)	(0.064)	(0.100)
Observations	1378	1378	1378	1378
Country of Origin FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Federal state FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes

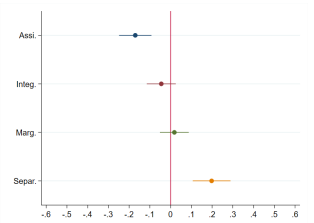
Standard errors in parentheses are clustered at the household level; *p<0.10, *p<0.05, *p<0.01
Notes: The dependent variable is the difference between individual i ethnosizer and her partner ethnosizer. FE refers to fixed effects. Individual controls include age at immigration and its square, years since migration and its square, religious affiliation, education in the home country, training in Germany, University in Germany, employment years in the home country, number of children, if there is a child in school and if there is a child in kindergarten. The reference individual is male and a lead/equal mover.

Table 4.C.4: Gap in Ethnic Identity Between Partners

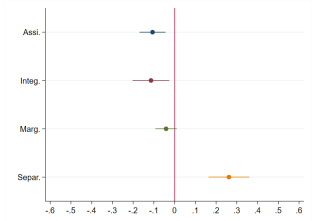
Figures



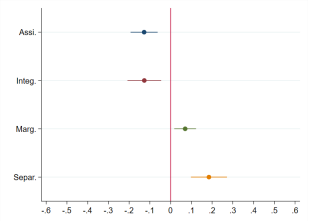
(a) All elements



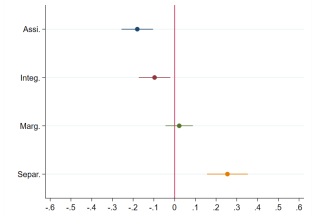
(b) Excl. language



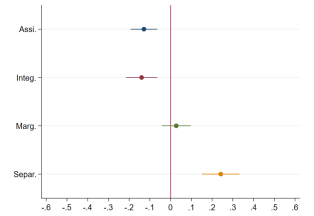
(c) Excl. future citizenship and location plans



(d) Excl. self-identification



(e) Excl. ethnic interaction



(f) Excl. media consumption

Notes: Bars identify 95% confidence intervals. Citizen. refers to citizenship and loc. to location.

Figure 4.C.1: Excluding one component at the time effects

Chapter 5

Local far-right demonstrations and nationwide public[1]

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Abstract: *One of the primary objectives of protests and demonstrations is to bring social, political, or economic issues to the attention of politicians and the wider population. While protests can have a mobilizing and persuading effect, they may reduce support for their cause if turned disruptive or disorganised. In this study, we look at how local or spontaneously organized far-right and xenophobic demonstrations affect concerns about hostility towards foreigners and worries about immigration in other districts in Germany. We use a regression discontinuity design to compare the attitudes of individuals interviewed in the days immediately before a large right-wing xenophobic demonstration and individuals interviewed in the days immediately after that demonstration. Our results show that within a 30-day bandwidth, right-wing demonstrations with 1500 or more participants lead to a substantial increase in worries about hostility towards foreigners of about 13.70% of a standard deviation. Additionally, we show that the effects of larger demonstrations are stronger, which suggests that the higher salience of demonstrations, the higher their potential threat. In contrast, worries about immigration are not affected by the demonstrations, indicating that the demonstrations are not successful in swaying public opinion in their favour. Lastly, we also show that individuals become more politically active in response to protests, which mainly benefits left-wing parties.*

5.1 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

[1]This chapter is also part of Christopher Prömel's PhD Thesis at the Freie Universität Berlin. The authors thank Achim Ahrens, Timo Hener and the participants of the IAB Brown Bag Seminar, the BeNa Summer Workshop 2022, and the ZEW Workshop on Immigration, Integration, and Attitudes 2022 for their comments and suggestions.

and Newman, 2021; Caprettini et al., 2021; Larreboure and Gonzalez, 2021; Lagios, Méon and Tojerow, 2022). Yet, if turned disruptive or poorly organized, protests may reduce support for their cause (Wasow, 2020; Eady, Hjorth and Dinesen, 2021).

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 in the district where the protests have occurred (e.g., Madestam et al., 2013; Enos, Kaufman and Sands, 2019; Klein Teeselink and Melios, 2021; Wasow, 2020; Larreboure and Gonzalez, 2021).[2] However, can local demonstrations affect the attitudes and party preferences of voters in other districts of a country? In this study, we focus on the effect of local or spontaneously organized large right-wing xenophobic demonstrations in an administrative district (*Nuts II*) on the attitudes 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.

The effect of xenophobic demonstrations on attitudes is, a priori, ambiguous. On the one hand, demonstrations can mobilise and persuade, raising support for the protesters' agenda. The issues and demands of the protest might have strong resonance or mobilize cultural grievances linked to the presence or arrival of migrants or other 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 we would observe that worries about hostility towards foreigners do not change or decrease, while concerns about immigration would potentially increase. Moreover, if the demonstrations resonate strongly with a country's overall population, they may also impact political preferences and lead to an increase in the preference for right-wing or even 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. In this situation, xenophobic protests could move public support against the protesters' agenda and possibly in support of parties with counter-agendas. Therefore, we would expect far-right protests to increase worries about hostility towards foreigners and have no effect on worries about immigration.

Similarly, the organization and coordination of demonstrations and the media coverage they receive can affect the public's perceptions differently. Previous research has shown that violent protests are less likely to raise support for their cause (Wasow, 2020; Eady, Hjorth and Dinesen, 2021), while well-organized and coordinated demonstrations can raise support for their agendas (Reny and Newman, 2021; Caprettini et al., 2021; Larreboure and Gonzalez, 2021; Lagios, Méon and Tojerow, 2022).

To identify large right-wing xenophobic demonstrations, we rely on a data

[2]Four exceptions are a study by Lagios, Méon and Tojerow (2022), which considers spillover effects of demonstrations against the far right in France, a study by Eady, Hjorth and Dinesen (2021) who show that the US Capitol insurrection led to deidentification 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 the city of Dresden reduced in-migration of Germans from other states.

set constructed by Kanol and Knoesel (2021), encompassing right-wing extremist demonstrations in Germany. This data set 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 with more than 30,000 observations. Our two primary questions of interest are those asking respondents to rate how worried they are about hostility towards foreigners and immigration on a three-point scale. As secondary outcomes, we also look at the intention to donate money or goods to help refugees, work with refugees directly, 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 criteria: 1) larger than "usual", 2) organized spontaneously and/or are of local nature, and 3) are not "surrounded" by other demonstrations, e.g. are isolated. We focus on large demonstrations so that 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).[3]

We concentrate on spontaneous or locally organized demonstrations because it is unlikely that 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.[4] To ensure that the respondents in our analysis were not recently exposed to events taking place on dates of national knowledge, we only consider "isolated" large xenophobic demonstrations that are locally or spontaneously organized within a 30-day time frame. 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 knowledge.[5]

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

Overall, we find that large xenophobic demonstrations significantly increase worries about hostility towards foreigners among native Germans. Our results show that within a 30-day bandwidth, right-wing demonstrations with 1500 or more participants lead to a substantial increase in worries about hostility towards foreigners of about 13.70% of a standard deviation. Additionally, larger demonstrations have a stronger impact on attitudes, which suggests that more salient protests also convey a higher potential threat. Looking at our second outcome, we find that protesters cannot

[3]As alternatives, we consider demonstrations where the number of participants is slightly below, at 1200, or above, at 1700.

[4]Alternatively, we exclude the entire state.

[5]This procedure is further detailed in section 5.3.1.

sway respondents' attitudes in their favour nationally, as respondents' concerns about immigration do not change significantly. In the heterogeneity analyses, we uncover some polarization in the population, the effects on worries about hostility towards foreigners are particularly strong in left-leaning districts. Lastly, we also show that following far-right demonstrations, individuals become more politically interested, mainly benefiting left-wing parties.

For the regression discontinuity design to be valid, we need to ensure that there is no selection on observables and no selective behaviour around the cutoff. In section 5.4.2, we show that there is no evidence of selection on observables by comparing the characteristics of districts and individuals interviewed before the demonstrations (control group) with those interviewed after the protests (treatment group). Using a density test where the null hypothesis is that the empirical distribution of the number of observations is continuous at the cutoff, we also show that selective behaviour around the cutoff is unlikely. In section 5.5.2, we present a series of robustness checks. We start by demonstrating that our results hold when adding time, geographical and individual controls and when choosing different 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 district) and excluding a specific demonstration or time period. Next, we demonstrate that, on average, when randomly assigning dates to each demonstration, they have no discernible effect on attitudes. To further ensure that we are not capturing some other randomness in the data, we examine the impact of these demonstrations on other concerns reported in the SOEP that, in principle, should remain unaffected (worries about own finances, worries about own health, worries about global terrorism). Lastly, we present our findings when employing a local randomization RDD which assumes that for a small window around the cutoff, the treatment status is assigned as it would have been in a randomized experiment. Overall, our main conclusions hold.

The data and empirical design used in this study have several advantages. First, individual-level data allows us to examine a more extensive set of outcome variables, particularly attitudinal variables. Both worries about hostility towards foreigners and immigration are important determinants of political preferences and voting behaviour. Second, we can estimate the immediate effect of the demonstrations. A typical challenge in the protest literature is understanding if demonstrations cause political change or reflect changes in underlying policy preferences. Because we compare attitudes 10 to 30 days before and after a set of demonstrations, our estimation approach allows us to claim that demonstrations, not other factors, drive the effect we find on attitudes and party preferences. Thirdly, most of the literature in political science and economics assumes that the effects of protests are primarily prevalent in the location where the protests have occurred (e.g., Madestam et al., 2013; Enos, Kaufman and Sands, 2019; Klein Teeselink and Melios, 2021; Wasow, 2020; Larreboure and Gonzalez, 2021).[6] However, we argue that large protests may also impact attitudes on a national level as people learn about these protests in the newspapers or on TV.[7] Two examples related to our work are a study by Eady, Hjorth and Dinesen (2021), which shows that the US Capitol insurrection led to deidentification with the Republican party beyond its immediate geographical context,

[6]Four exceptions are the studies mentioned in footnote 1.

[7]Using Lexis Nexis we show in table 5.A.2 that most demonstrations were covered in mainstream newspapers. We cannot check TV or social reporting, which are probably the most common places for reporting in the period under analysis 2005-2019.

and a study by Brox and Krieger (2021), which finds that the occurrence of large far-right rallies in the city of Dresden significantly reduced in-migration of young Germans from other German states.

Our study contributes to several different strands of the literature. First, we add to existing research that analyses the effects of protests on attitudes and political preferences,[8] as we study the effects of far-right demonstrations on concerns about hostility towards foreigners, worries about immigration, interest in politics and party preference. Previous studies have examined the political effects of the 1932 Nazi marches (Caprettini et al., 2021), demonstrating against Le Pen (Lagios, Méon and Tojerow, 2022), US civil rights protests (Wasow, 2020), the Women's March (Larreboure and Gonzalez, 2021), the George Floyd protests (Reny and Newman, 2021), the Black Lives Matter (Klein Teeselink and Melios, 2021) or the January 6th, 2021 capitol riots (Eady, Hjorth and Dinesen, 2021), among others. While some of these studies explore local variation in protest intensity to identify their effect on (aggregate) regional attitudes, 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. 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 decreasing 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 (Madestam et al., 2013; Enos, Kaufman and Sands, 2019; Wasow, 2020). In this aspect, our work is closer to that of Eady, Hjorth and Dinesen (2021) who show that the US Capitol insurrection led to deidentification with the Republican party nationwide, Reny and Newman (2021) who finds 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 the city of Dresden reduced in-migration of Germans from other states.

Our third contribution is that we focus on local or spontaneously organized right-wing xenophobic demonstrations. Many existing studies have primarily focused on the effect of left-wing protests (regarding issues like civil rights or women's rights) on public attitudes and voting behaviour (Mazumder, 2018; Enos, Kaufman and Sands, 2019; Wasow, 2020; Larreboure and Gonzalez, 2021; Reny and Newman, 2021; Klein Teeselink and Melios, 2021).[9] However, the effect of right-wing protests is not necessarily symmetric (Barker, Nalder and Newham, 2021) since right-led protest differs from traditional left-led protests with regards to the underlying motive, ethnic and social composition of protesters Eady, Hjorth and Dinesen (2021) and Manekin and Mitts (2022). Most studies looking at right-wing demonstrations have focused on coordinated protests or party-sponsored demonstrations, which were organized to create a spectacle Madestam et al. (2013) and Caprettini et al. (2021). In contrast,

[8]Madestam et al. (2013), Enos, Kaufman and Sands (2019), Wasow (2020), Eady, Hjorth and Dinesen (2021), Larreboure and Gonzalez (2021), Reny and Newman (2021) and Lagios, Méon and Tojerow (2022).

[9]Some studies looking at the effect of right-wing protests and demonstrations include Madestam et al. (2013), Eady, Hjorth and Dinesen (2021) and Caprettini et al. (2021).

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.

This paper is organized as follows, in Section 5.2, we lay out some theoretical considerations on the effect of right-wing xenophobic protests on other individual's attitudes and in section 5.3.1 we expose our data and explain our procedure to select demonstrations that are 1) larger than "usual", 2) organized spontaneously and/or are of local nature, and 3) "isolated". Section 5.4 explains our empirical strategy and shows some preliminary tests. We show all our main results, robustness checks and heterogeneous analysis in section 5.5. In section 5.6 we extend our main results and show the effect of far-right demonstrations on interest in politics and party preference. Finally, section 5.7 concludes.

5.2 Theoretical Considerations

The effects of right-wing xenophobic protests on other people's attitudes and political beliefs are *ex-ante* not clear and potentially ambiguous. We consider two main channels, the "persuasion mechanism" and the "threat mechanism".

Persuasion mechanism Demonstrations and protests can help spread the protesters' message to a broader audience and increase 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 or others. Protesters could sway the public in their favour through several channels. First, they can have a persuasive effect (Wouters, 2019; Klein Teeselink and Melios, 2021). 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, Melnikov and Zhuravskaya, 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 public discourse (Dunivin et al., 2022). Fourth, protests could play a crucial role 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 increases the likelihood of future protests and demonstrations

Threat mechanism Political protests can backfire if they are perceived as threatening by the public (Wasow, 2020; Gutting, 2020; Eady, Hjorth and Dinesen, 2021; 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 arises from the potential consequences of the ideologies these protests propagated. 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.

To what extent protesters are successful or unsuccessful depends in large part on two factors: i) how receptive potential audiences are to their message and ii) how audiences perceive the protester's message and engagement, which depends not only on how organized and coordinated protests are but also they are portrayed in the media.

A number of extant 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, Nalder and Newham, 2021). We would therefore expect that more conservative people 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 the case for more liberal individuals.

The second important factor in the success of protesters is how their protests appear to others. Some existing studies have shown that violent, disruptive, and disorderly protests usually are perceived negatively by the audience (Wasow, 2020; Eady, Hjorth and Dinesen, 2021), while peaceful and orderly protests can make protesters appear more sympathetic and can help shift attitudes and positions (Reny and Newman, 2021; Caprettini et al., 2021; Larreboure and Gonzalez, 2021; Lagios, Méon and Tojerow, 2022). Hereby, the role of media comes into play, as their portrayal can also frame how protests are being perceived (DellaVigna and Kaplan, 2007; Adena et al., 2015; Guriev, Melnikov and Zhuravskaya, 2021; Melnikov, 2021).

From these theoretical considerations, we can derive two hypotheses. Hypo- thesis one: protesters successfully spread their message and can persuade and mobilize other people to their cause. If this is the case, we would expect to see an increase in worries about immigration increase and no change or a decrease in worries about hostility towards foreigners among individuals interviewed after far-right demonstrations. Hypothesis two: protesters are unsuccessful, and their demonstrations are perceived as threatening. In this scenario, we would expect to see an increase in worries about hostility towards foreigners and no change or a decrease in worries about immigration among individuals interviewed after far-right demonstrations.

5.3 Data and Background

5.3.1 Demonstrations data and selection

To study the effect of xenophobic protests on attitudes, we rely on a data set 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 mottos 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.[10]

The Kanol and Knoesel (2021) dataset includes demonstrations that take place at key dates of national knowledge, such as labour day and the bombing of Dresden, and demonstrations that were spontaneously or locally organized, such as protests against asylum seeker centres or demonstrations following a local rock festival. In this study, we are interested in 1) right-wing xenophobic demonstrations that were larger than "usual", 2) demonstrations that were organized spontaneously and/or are of local nature, and 3) demonstrations that are "isolated".

For the purpose of our analysis, we focus on relatively large protests so that people from other districts besides the district where the protest occurred were likely to have read or heard about them after they took place[11] - but not to have participated in them. To proxy for the scale 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 this event stands out. The distribution of the number of participants across all demonstrations in the Kanol and Knoesel (2021) dataset is shown in table 5.3.1 bellow. 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 1700.

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 5.3.1: Distribution of the number of participants in all demonstrations

We focus on demonstrations that were organized spontaneously and/or are of local nature because the organization and planning of these right-wing xenophobic demonstrations in one given German district are unlikely to have drawn or reached people living in other German districts. Demonstrations related to annual events that are of national knowledge include protests on labour day, German unity day, landmark war days and demonstrations related to the anniversary of the bombings of Magdeburg, Dresden, and Chemnitz during WWII, which Neonazi groups frequently instrumentalise. We exclude these events because one could argue that there might be anticipation effects at the national level. Moreover, protests on these days were usually accompanied by other major events. E.g., in the case of the anniversary of

[10]These numbers are estimates.

[11]Using Lexis Nexis we show in table 5.A.2 that most demonstrations used in our analysis were covered in mainstream newspapers. We cannot validate againts TV or social reporting, which are probably the most common places for reporting in the period under analysis 2005-2019.

the bombing of Dresden, there are usually large memorial events organized 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.

To ensure that the respondents in our analysis were not recently exposed to events taking place on dates of national knowledge or were exposed to more than one demonstration, we use only "isolated" large xenophobic demonstrations with a local or spontaneous character within a 30-day range.[12] In the first step, 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 demonstration during this time period. In the second step, we select the relevant and isolated events by dropping the isolated demonstrations related to annual events that are of national knowledge.

The overall idea behind criteria 1) "larger than usual" and 2) "local or spontaneously organized" is to ensure that we are only using demonstrations for which it is reasonable to assume that their date and scale were unlikely to be anticipated by individuals residing in other German districts. In section 5.5.2, we provide some tests to support this assumption. The purpose of 3) "isolated" is to ensure that the attitudes of individuals interviewed before the focal event have not been affected by any previous demonstration (which could bias our results) and that the attitudes of individuals interviewed after the focal demonstration have not been affected by more than one demonstration. For readability matters we will refer to protests satisfying criteria 1) 2) and 3) simply as large right-wing demonstrations.

5.3.2 Individual and Household Data

The Socio-Economic Panel (SOEP) is a longitudinal annual household survey that is representative of the German population[13] where every year, approximately 30,000 people in around 15,000 households are interviewed. The dataset contains both 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 such that we can link it with the location of the demonstration.

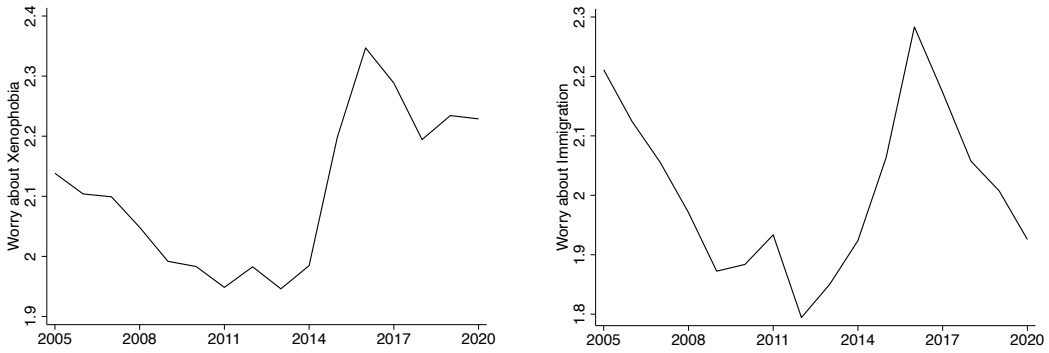
For our two main variables of interest, we rely on the SOEP questions, which ask how concerned respondents are about "hostility towards foreigners or minorities in Germany" and "immigration", with the following available answers (1) "Not concerned at all", (2) "somewhat concerned" and (3) "very concerned". For our baseline estimations, we use these variables in the continuous form. Figure 5.3.1 shows the trajectory of outcome means over the sample period. Generally, both types of concerns decline consistently after 2005. While concerns about hostility towards foreigners reached their low point in 2011, worries about immigration bottomed out in 2012 and picked up slightly after that. Table 5.A.1 in the appendix shows the basic statistics for the two outcomes for the sample used in the empirical analysis. Both outcome variables have means relatively close to 2.04, with worries about immigration

[12] Similar to the design in Graeber and Schikora (2021)

[13] For a complete description of the data, please refer to Goebel et al. (2019).

being slightly lower at around 1.97 but with a higher standard deviation of about 0.76 in all specifications.

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



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

Figure 5.3.1: Weighted Means of Outcome Variable

5.4 Empirical Strategy and Identification

5.4.1 Regression discontinuity design (RDD)

Our empirical approach compares the attitudes of individuals interviewed in the days immediately before a large right-wing xenophobic demonstration (control group) with those of individuals interviewed immediately after that demonstration (treatment group). To make the case of no-anticipation stronger and to separate the spillover effect from the possible direct disruptive effect of large protests, we do not consider individuals residing in either the district or the state where the large demonstration took place (l). The treatment assignment for individual i living in district/state $k \neq l$, T_{ik}, is a deterministic function of the date the individual was interviewed d_{ik} (the running variable) and is defined as $T_{ik} = \mathbb{1}\{d_{ik} > c\}$, where c is the cutoff date of a specific demonstration. $d_{ik} - c$ is the time in days between the cutoff date and the date individual i was interviewed. Given that some individuals were interviewed on the day of the focal demonstration (approximately 1%), but we have no information on the time of the interview or demonstration, we do not include them. In section 5.5.2, we show that our results do not depend on their inclusion.

Our local linear[14] polynomial estimation is the following:

$$Y_{ik} = \alpha + \beta T_{ik} + \mu_1(d_{ik} - c) + \mu_2 T_{ik}(d_{ik} - c) + \epsilon_{ik} \tag{5.1}$$

In equation (1), Y_{ik} is either worries about hostility towards foreigners or worries about immigration. β is the regression discontinuity design (RDD) estimate and captures the intention to treat effect of the demonstrations on Y_{ik}. We use a triangular kernel to give more weight to the observations closer to the cutoff and

[14]The current consensus in the literature is to use a local linear specification (Cattaneo, Idrobo and Titiunik, 2020; Gelman and Imbens, 2019). In section 5.5.2, we show our results using a second-order polynomial.

heteroskedasticity-robust standard errors (Lee and Lemieux, 2010).[15] In our main results, we consider different bandwidths around the demonstrations: b =15,20,30 and the mean squared error optimal bandwidth from Calonico, Cattaneo and Farrell (2019).[16] For expositional clarity, we use the 30-day bandwidth as our preferred bandwidth. We chose this bandwidth because i) we consider isolated demonstrations (described in section 5.3.1) 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. Table 5.5.1 in section 5.5 shows that our conclusions are robust to different bandwidths. Since we use more than one demonstration, we are stacking the RDD across multiple demonstrations

In section 5.5.2, we augment the local polynomial model to include predetermined covariates such as the day of the week and month of the interview, residential district, gender, age, number of children, marital status, educational background and employment status.[17] 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, Idrobo and Titiunik, 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, Idrobo and Titiunik, 2020). We show in section 5.4.2 that the covariates are balanced at the cutoff and in section 5.5.2 that they do not change drastically our point estimates.

5.4.2 Validity of the regression discontinuity design

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

1) No selective behaviour at the cutoff A potential threat to the RDD design is if there is selection into (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

[15] In section 5.5.2 we check if our results are sensitive to the choice of kernel by using a uniform kernel instead of the 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, Idrobo and Titiunik (2020).

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

[17] Including post-treatment or imbalanced covariates would change the parameter being estimated (Cattaneo, Idrobo and Titiunik, 2020). The covariates are included in a linear and additive-separable way

scheduled well in advance. However, it is still possible that individuals are less willing to reply to the SOEP survey questions right after a demonstration.

More formally, we employ a density test where the null hypothesis is that the empirical distribution of the number of observations is continuous at the cutoff.[18] 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 5.B.1 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.

Furthermore, as mentioned in the data section we focus on demonstrations that were organized spontaneously and/or are of local nature[19] such that it is reasonable to assume that their date and scale was unlikely to be anticipated by individuals residing in German districts other than the district where the demonstration took place. In section 5.5.2, we show that our results are also robust when excluding the entire state where the demonstrations took place.

2) The continuity assumption holds Our identification strategy relies on the assumption that the individuals interviewed before a focal demonstration (control group) are similar to those interviewed after that focal demonstration (treatment group), constituting a credible counterfactual.

We provide evidence that the continuity assumption holds by estimating equation (1) using predetermined individual and district characteristics as an outcome. Since the predetermined covariates should not be affected by the demonstration, 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 district we use the one-year lag of the unemployment rate, the share of foreigners and standardized GDP[20] and the turnout, share of the far-right, right and left vote in the last elections in the Nuts II where the respondent resided at the time of the survey.[21]

In Figure 5.4.1, 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 group and control group have very similar characteristics, with only mild differences in the share with vocational training.

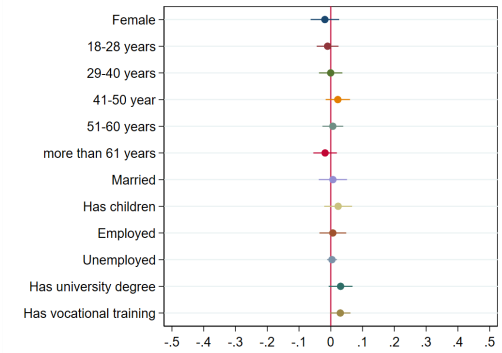
[18] We use the *rdensity* package from Cattaneo, Jansson and Ma (2018) for the density test.

[19] The demonstrations considered in the RDD are those satisfying the criteria 1), 2) and 3) established in section 5.3.1.

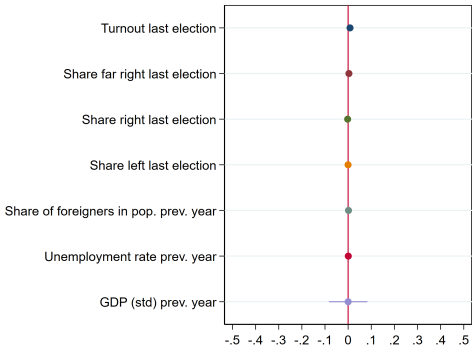
[20] We standardize so that the scale fits with the other variables.

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

(a) Individual characteristics



(b) District characteristics



Notes: Panel (a) and (b) display the coefficients from the estimation of Equation (1) on the individual characteristics and district characteristics listed on the y-axis, respectively. All regressions consider demonstrations with 1500 participants or more and use a 30-day bandwidth.

Figure 5.4.1: Continuity test, 30 days isolated demonstrations, > 1500 participants

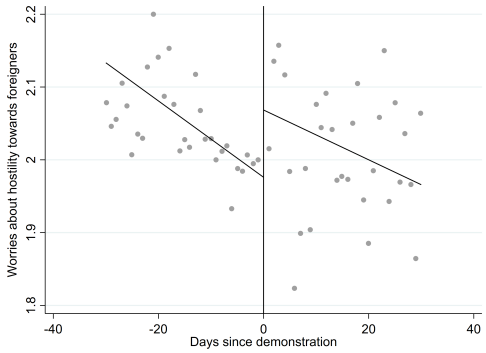
5.5 Results

5.5.1 Main results: demonstrations, worries about xenophobia and immigration

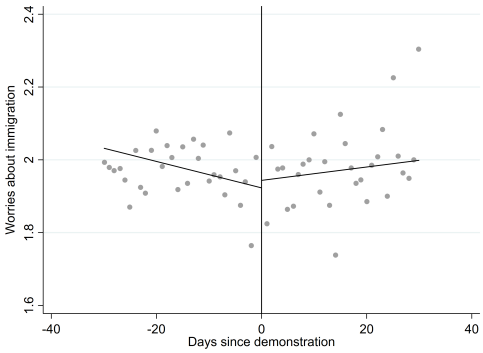
Figure 5.5.1 shows a regression discontinuity design plot for worries about hostility towards foreigners (panel (a)) and worries about immigration (panel (b)) using a local linear trend with a 30-day bandwidth, triangular kernel and mimicking variance evenly-spaced bins.

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.

(a) Worries about hostility towards foreigners



(b) Worries about Immigration



Notes: Figure 5.5.1 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 Nuts II and the day of the demonstrations are excluded.

Figure 5.5.1: RDD plots, 30 days isolated demonstrations, > 1500 participants

The main results of our analysis are displayed in table 5.5.1 below (using equation (1)). They show the effects of large right-wing extremist 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 5.5.1 indicate that natives' concerns about xenophobia increased markedly and significantly in response to large demonstrations. Using a 30-day bandwidth, we see that a large, isolated and local or spontaneously organized 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 5.4, we use the 30 days bandwidth in most of our analysis because the procedure used to identify isolated demonstrations (described in section 5.3.1) uses a 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 5.5.1 show that protesters could not make respondents more worried about immigration. While positive, the effect of demonstrations on worries about immigration remains insignificant.

Panel A	Worries about hostility towards foreigners			
	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			
	Optimal: 10 days	15 days	20 days	30 days
	(1)	(2)	(3)	(4)
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

Robust Standard Errors in parenthesis *p<.1; **p<.05; ***p<.01
Notes: Table 5.5.1 displays the coefficients from the estimation of Equation (1) on Worries about hostility towards foreigners in panel B and Worries about immigration in panel B. All regressions consider a demonstration to be relevant if it has 1500 or more participants, use a 30 days bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. The Nuts II and the day of the demonstrations are excluded.

Table 5.5.1: RDD results, 30 days isolated demonstrations, > 1500 participants

In the appendix, we show complementary evidence that these local and spontaneous large right-wing demonstrations did not sway the public's opinion against immigrants. In 2016, 2018 and 2020 the SOEP 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 large and isolated events in 2018, we are left with a small sample size and hence show these results as complementary evidence. Table 5.B.1 in the appendix shows the results of this exercise. 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.

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

5.5.2 Robustness checks

In this section, we present a series of robustness checks using our preferred measure of large and salient demonstration (number of participants above the 99th percentile at 1500 individuals) with a 30 days bandwidth. We start by demonstrating that our results are robust to the inclusion of control variables and when choosing different specifications. Secondly, we show that our conclusions hold when varying the cutoff for large demonstrations and when excluding the entire state where the demonstration took place (rather than the Nuts II only). Thirdly, we show that our results are invariant to the exclusion of a specific demonstration. Fourth, when assigning a random date to each xenophobic demonstration, we show that on average their effect on attitudes is null. Fifth, to ensure that we are not capturing some other random variation, we look at the effect of these demonstrations on other worries reported in the SOEP, that, in principle, should not be affected by far-right protests. Finally, we show our results when using local randomization RDD.

Controlling for individual characteristics, time and location factors
As a first robustness test, we augment the local polynomial model to include predetermined covariates in a linear and additive-separable way. As shown in Figure 5.4.1 the assignment to the right or left side of the cutoff does not depend on individual or district characteristics. Nevertheless, table 5.5.2 below shows the results when adding different sets of controls. Column (1) shows the baseline results as in table 5.5.1, column (2) adds the Nuts II where the individuals being interviewed reside, column (3) the month of the interview, column (4) the day of the week and column (5) adds together Nuts II and the month and day of the interview. Finally, columns (6) and (7) show the main results when adding individual and district characteristics as in figure 5.4.1, respectively. Our results do not change.

Panel A	Worries about hostility towards foreigners						
	Base	Nuts II	Month	Day week	(2)+(3)+(4)	Indiv. C.	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD_Estimate	0.0945***	0.0925***	0.0939***	0.0921***	0.0940***	0.0922***	0.0945***
	(0.0298)	(0.0300)	(0.0300)	(0.0300)	(0.0300)	(0.0298)	(0.0298)
Observations	10902	10902	10902	10902	10902	10902	10902
Panel B	Worries about immigration						
	Base	Nuts II	Month	Day week	(2)+(3)+(4)	Indiv. C.	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD_Estimate	0.0364	0.0224	0.0161	0.0196	0.0162	0.0405	0.0364
	(0.0323)	(0.0342)	(0.0339)	(0.0342)	(0.0339)	(0.0325)	(0.0323)
Observations	10902	10902	10902	10902	10902	10902	10902
Nuts II	No	Yes	No	No	Yes	No	Yes
Month	No	No	Yes	No	Yes	No	Yes
Day of week	No	No	No	Yes	Yes	No	Yes
Indiv. charact.	No	No	No	No	No	Yes	Yes

Robust Standard Errors in parenthesis *p<.1; **p<.05; ***p<.01
Notes: Table 5.5.2 displays the coefficients from the estimation of Equation (1) on Worries about hostility towards foreigners in panel B and Worries about immigration in panel B. All regressions consider a demonstration to be relevant if it has 1500 or more participants, and use a 30 days bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. The Nuts II and the day of the demonstrations are excluded.

Table 5.5.2: Controlling for individual characteristics, time and location factors, 30 day bandwidth

Alternative specifications This sub-section shows that our results are robust to different and more flexible specifications. Panel (a) in figure 5.5.2 shows the robustness checks for the worries about hostility towards foreigners, and panel (b) in figure 5.5.2 shows the robustness checks for the worries about immigration. The first in both panels displays the baseline effect reported in column (4) of table 5.5.1.

In the second line of figure 5.5.2, we show that the dynamics of the European Refugee Crisis are unlikely to confound our analysis. The increased monthly inflow of asylum seekers, which peaked in 2014-2015, led to concerns among some residents about Germany's ability to manage immigration and even hostility and xenophobia from others. Since there were 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. At the same time, these dynamics made monthly protests more recurrent (Castelli Gattinari, Froio and Pirro, 2021). The results in figure 5.5.2 show that our main coefficient of interest changes little when we exclude post-2013 events.

In our main specification, we excluded individuals interviewed on the day the focal demonstration took place because we cannot know if they were treated or not - we have no information on the hours of the demonstration. Line 3 of figure 5.5.2 shows that our results do not change when adding to the treatment group people interviewed on the day the focal demonstration occurred.

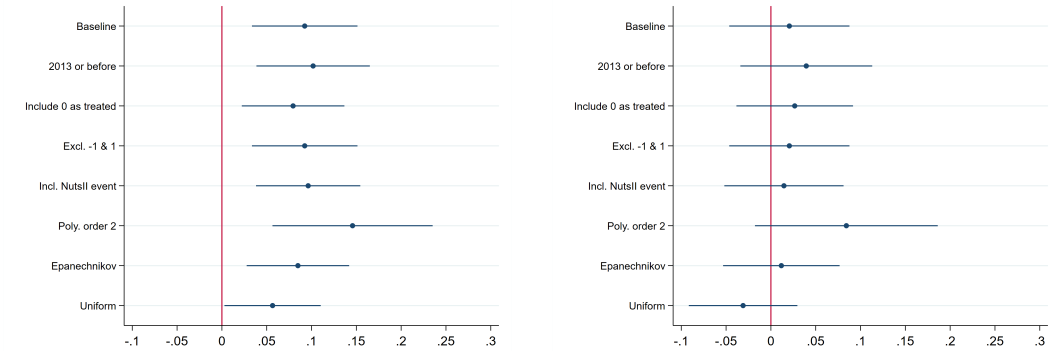
In line 4 of figure 5.5.2, we investigate the sensitivity of the results to the response of the individuals interviewed very close to the cutoff. If there was a systematic manipulation of score values, individuals interviewed very close to the cutoff are those most likely to have engaged in manipulation. To test for this, we exclude individuals interviewed at -1 and 1 (the "donut hole" approach) (Cattaneo, Idrobo and Titiunik, 2020). 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. The results in line 4 show that the conclusions from the analysis are robust to excluding observations with $|d_{ik}| < 1$.

We excluded individuals residing in the district where the large protest took place to strengthen the case of no-anticipation and to separate the spillover effect from the possible direct disruptive effect of large demonstrations. In line 5 of figure 5.5.2, 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, Idrobo and Titiunik, 2020) and used a local linear specification. 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, Idrobo and Titiunik, 2020). The results in line 6 of figure 5.5.2 show that our point estimated become larger when we include a second-order polynomial but do not change the conclusions of the study, e.g., demonstrations have a positive and significant effect in the worries about hostility towards foreigners and a non-statistically significant effect in the worries about immigration. Cattaneo, Idrobo and Titiunik (2020) notes that in most situations incorporating higher-order terms will reduce the approximation error and lead to changes in the estimated effect. However, the relevant question is if such changes alter the study's conclusions.

Finally, in our main specification, we have followed the literature on RDD (Cattaneo, Idrobo and Titiunik, 2020) and used the recommended choice of a triangular kernel function. A defining feature of the triangular kernel is that it gives more weight to the observations closer to the cutoff. In lines 7 and 8 of figure 5.5.2, we show 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.

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



Notes: Figure 5.5.2, displays the coefficients from the estimation of Equation (1) on Worries about hostility towards foreigners in panel (a) and Worries about immigration in panel (b). All regressions consider a demonstration to be relevant if it has 1500 or more participants, use a 30-day bandwidth and heteroskedasticity-robust standard errors. 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.

Figure 5.5.2: Different specifications

Varying the definition of a large demonstration We have considered a demonstration large if it is above the 99th percentile at 1500 participants. 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, or slightly above, at 1700. The results are displayed in table 5.5.3 and show that our results are robust to variations around the definition of a large event. Interestingly, larger demonstrations seem to have a stronger impact on attitudes, which suggests that more salient protests also convey a higher potential threat.

Panel A	Worries about hostility towards foreigners					
Bandwidth	Optimal: 10d, 9d, 9d			30 days		
Participants	1200	1500	1700	1200	1500	1700
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.1506***	0.1437**	0.1425**	0.0777***	0.0924***	0.0823***
	(0.0521)	(0.0644)	(0.0593)	(0.0269)	(0.0300)	(0.0291)
Observations	3665	2498	3077	13460	10902	11671
Panel B	Worries about immigration					
Bandwidth	Optimal: 8d, 10d, 9d			30 days		
Participants	1200	1500	1700	1200	1500	1700
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.0891	0.0588	0.0854	0.0277	0.0206	0.0306
	(0.0734)	(0.0648)	(0.0696)	(0.0306)	(0.0342)	(0.0330)
Observations	2874	2867	2681	13460	10902	11671

Robust Standard Errors in parenthesis *p<.1; **p<.05; ***p<.01
Notes: Table 5.5.3 displays the coefficients from the estimation of Equation (1) on Worries about hostility towards foreigners in panel B and Worries about immigration in panel B. All regressions use a 30 days bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. The Nuts II and the day of the demonstrations are excluded.

Table 5.5.3: 30 days isolated demonstrations, varying cutoff for large protests

Exclude all districts in the state where the demonstration took place To further reduce any concerns about anticipation effects, we restrict our analysis to consider only the effect on districts located in a different state from the district where the large right-wing demonstration took place. In this case, we exclude the entire state where the actual demonstration occurred. Table 5.5.4 shows the results for this exercise - the point estimates are close to those in our main results in table 5.5.1.

Panel A	Worries about hostility towards foreigners			
	Optimal: 9 days	15 days	20 days	30 days
	(1)	(2)	(3)	(4)
RD_Estimate	0.1497**	0.1241***	0.1140***	0.0949***
	(0.0647)	(0.0434)	(0.0373)	(0.0303)
Observations	2457	5123	7104	10680
Panel B	Worries about immigration			
	Optimal: 10 days	15 days	20 days	30 days
	(1)	(2)	(3)	(4)
RD_Estimate	0.0556	0.0560	0.0476	0.0175
	(0.0650)	(0.0495)	(0.0425)	(0.0345)
Observations	3230	5123	7104	10680

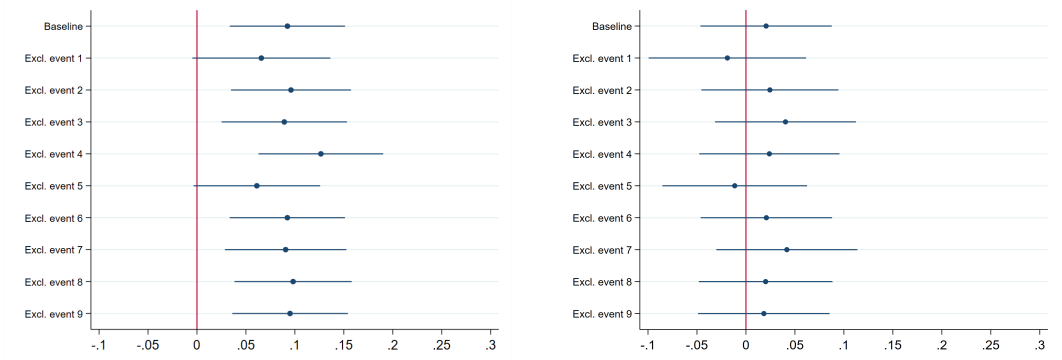
Robust Standard Errors in parenthesis clustered at the distance to the event in days. *p<.1; **p<.05; ***p<.01
Notes: Table 5.5.4 displays the coefficients from the estimation of Equation (1) on Worries about hostility towards foreigners in panel B and Worries about immigration in panel B. All regressions consider a demonstration to be relevant if it has 1500 or more participants, use a 30 days bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. The state and the day of the demonstrations are excluded.

Table 5.5.4: Excluding all districts in the state where the demonstration took place

Exclude one event at a time To assess the importance of a particular demonstration to our estimation, figure 5.5.3 shows our main results when we exclude one of the nine demonstrations at the time. Generally, our estimates remain very stable and robust to the exclusion of these events. While excluding event 5 slightly

reduces the coefficient on the worries about hostility towards foreigners, it remains statistically different from zero. The coefficients in the worries about immigration regression are always statistically indistinguishable from zero.

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

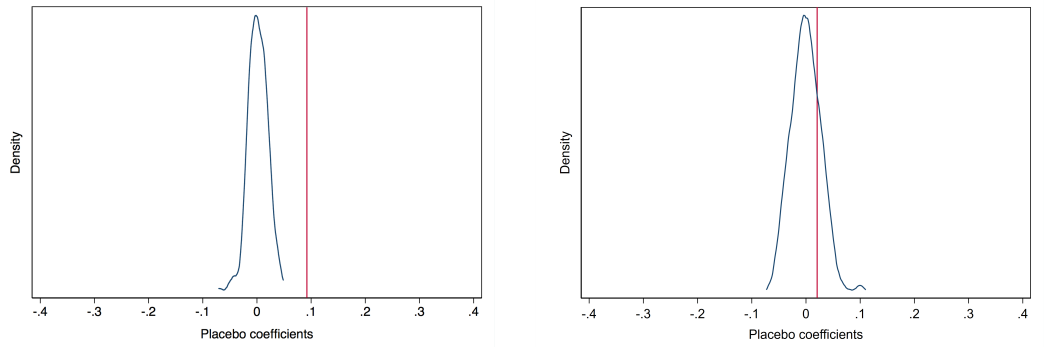


Notes: Figure 5.5.3, displays the coefficients from the estimation of Equation (1) on Worries about hostility towards foreigners in panel (a) and Worries about immigration in panel (b). All regressions consider a demonstration to be relevant if it has 1500 or more participants, use a 30 days bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. The Nuts II and the day of the demonstrations are excluded.

Figure 5.5.3: Exclude one year or one event at a time, 30 days isolated demonstrations, > 1500 participants

Placebo treatment date As a placebo test, we assign a random date to each relevant and isolated demonstration, estimate Equation (1), and repeat this procedure 300 times. The distribution of the coefficients is shown in figure 5.5.4 and is concentrated around zero. In panel (a), the red vertical line represents the true effect of 0.0924 estimated in our baseline regression in table 5.5.1 and is apart from the distribution of random coefficients. This indicates that our results are likely due to the xenophobic protests and not some statistical artefact.

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



Notes: Figure 5.5.4, displays the distribution of the coefficients from estimating 300 times Equation (1) on Worries about hostility towards foreigners in panel (a) and Worries about immigration in panel (b). All regressions consider a random demonstration date, use a 30 days 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.

Figure 5.5.4: Placebo treatment date, 30 days isolated demonstrations, > 1500 participants

Placebo outcomes As a second placebo test, we consider the effect of relevant and isolated demonstrations on worries which, in principle, should not be affected by these events. These worries are captured in the SOEP data and relate to the environment, economic development, and economic situation. Table 5.5.5 shows the coefficients when estimating equation (1) using these alternative outcomes. As expected, xenophobic demonstrations did not affect these worries, as all coefficients remain insignificant.

	Worry about:		
	Own health	Own econ. situation	Global terrorism
	(1)	(2)	(3)
RD_Estimate	-0.0273	0.0241	0.0024
	(0.0314)	(0.0323)	(0.0413)
Observations	10886	10890	5333

Robust Standard Errors in parenthesis clustered at the distance to the event in days. *p<.1; **p<.05; ***p<.01
Notes: Table 5.5.5 displays the coefficients from the estimation of Equation (1) on Worries about own health, own economic situation and global terrorism. All regressions consider a demonstration to be relevant if it has 1500 or more participants, use a 30 days bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. The Nuts II and the day of the demonstrations are excluded.

Table 5.5.5: Placebo worries, 30 days isolated demonstrations, > 1500 participants

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, Idrobo and Titiunik (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 as strong assumptions 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 randomized experiment. The day an individual is interviewed can be considered a randomly generated number unrelated to the average potential outcomes. 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.

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 on Cattaneo, Frandsen and Titiunik (2015) and Cattaneo, Titiunik and Vazquez-Bare (2016) and use a procedure based on balance tests for regression discontinuity (RD) designs under local randomization. We use the *rdrandinf* package developed by Cattaneo, Titiunik and Vazquez-Bare (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 5.5.6. The point estimated are slightly smaller, but the overall results are robust and consistent with the continuity approach.

Panel A	Worries about hostility towards foreigners		
	1200 part.	1500 part.	1700 part.
	(1)	(2)	(3)
Local Random. Estimate	0.0582**	0.0594**	0.0733***
Power vs Local Pol.	0.999	0.999	0.999
Observations	2874	2243	2144
Panel B	Worries about immigration		
	1200 part.	1500 part.	1700 part.
	(1)	(2)	(3)
Local Random. Estimate	0.0220	0.0197	0.0343
Power vs Local Pol.	0.889	0.460	0.753
Observations	2874	2243	2144

Robust Standard Errors in parenthesis clustered at the distance to the event in days. *p<.1; **p<.05; ***p<.01
Notes: Table 5.5.5 displays the coefficients from the estimation of Equation (1) on Worries about hostility towards foreigners in panel B and Worries about immigration in panel B. All regressions consider a demonstration to be relevant if it has 1500 or more participants and use a 30 days criteria to identify isolated demonstrations. The Nuts II and the day of the demonstrations are excluded.

Table 5.5.6: Local randomization, 1 week, > 1500 participants

5.5.3 Heterogeneity Analysis

In the previous section, 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. For example, people residing in more economically deprived areas might react differently than those in more prosperous regions, as people around them may shape how they perceive such protests. These heterogeneities, however, may 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 labour market, demographic, and attitudinal characteristics. We split the sample into different groups and run equation (1) on concerns about hostility towards foreigners and immigration. As in the previous section, we present all our results using large demonstrations, with more than 1500 participants, and using a 30 days bandwidth. While this means that we include more observations that are further away from the date of the protests, which possibly dilutes estimates, it maximizes the number of observations included, which is beneficial as the heterogeneity analysis considerably cuts down the size of the respective samples. Nevertheless, one should keep in mind that the estimates are less precisely estimated than the baseline estimates.

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 or below the yearly median in each of this characteristics.[22] We then take the lag of each of these measures relative to the year of the interview.[23]

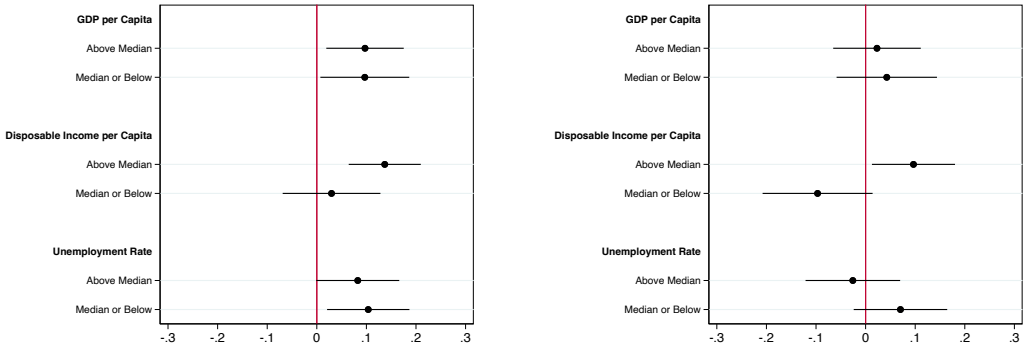
Generally, there is no clear indication that respondents in economically weaker

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

[23]To avoid the issue that our treatment may potentially affect those regional characteristics, we use regional data of the previous year.

regions react differently. Looking at GDP per capita and the unemployment rate, there is hardly any difference in estimates for both worries about hostility towards foreigners and worries about immigration. We see a difference only when we compare respondents by regional disposable income. However, there is no clear pattern here either, as individuals in regions with above-median income experience an increase in both types of concerns, possibly indicating some 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.

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



Notes: Figure 5.5.5, displays the coefficients from the estimation of Equation (1) on Worries about hostility towards foreigners in panel (a) and Worries about immigration in panel (b) restricting the sample to the group listed on the y-axis. All regressions consider a demonstration to be relevant if it has 1500 or more participants, use a 30 days bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. The Nuts II and the day of the demonstrations are excluded.

Figure 5.5.5: Heterogeneity analysis by regional economic situation

Regional political characteristics In this sub-section, the samples are split by the NUTS II regional voting share of far-right, left-of-centre, and right-of-centre parties[24] in the last election relative to the interview date,[25] allowing us to compare results in regions where each was relatively more or less successful. In contrast to economic factors, Figure 5.5.6 displays that political factors appear to influence respondents' reactions to the protests.

The estimates in Figure 5.5.6 (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, splitting the sample along the election vote share of left-wing and moderate conservative parties, there does not appear to be a difference.

Figure 5.5.5 (b) shows results for worries about immigration. While estimates are virtually the same in regions where far-right parties are more or less successful, there is a marked difference when we split the sample by the vote share of left-of-centre and moderate right-leaning parties. While worries decrease (increase) in areas where

[24]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. The left-of-centre parties only include the mainstream parties of SPD, Bündnis 90/Die Grünen, PDS/Die Linke, and Piratenpartei. Right-of-centre parties are CDU, CSU, and FDP.

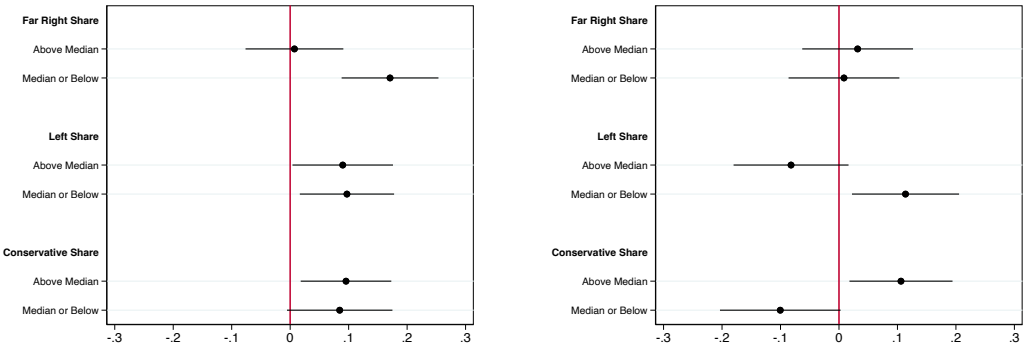
[25]There were elections in 2002, 2005, 2009, 2013 and 2017.

left-wing parties are more (less) successful, the opposite is true for right-of-centre parties.

This sets up an interesting picture, whereby respondents in relatively left-leaning areas appear to show a reasonably consistent reaction to far-right demonstrations, which runs counter to the interests of the protesters, as they both increase their concerns about xenophobia and become less worried about immigration. In right-leaning areas, on the other side, there appears to be more of an ambivalent, potentially even polarizing, reaction, with increases in both types of concerns. This indicates that the political environment might affect how respondents perceive protests. However, one should be careful not to draw strong conclusions, particularly with regard to the far-right vote share, as it was often still rather low, even in areas where they were relatively more successful.

Figure 5.C.1 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.

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



Notes: Figure 5.5.6, displays the coefficients from the estimation of Equation (1) on Worries about hostility towards foreigners in panel (a) and Worries about immigration in panel (b) restricting the sample to the group listed on the y-axis. All regressions consider a demonstration to be relevant if it has 1500 or more participants, use a 30 days bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. The Nuts II and the day of the demonstrations are excluded.

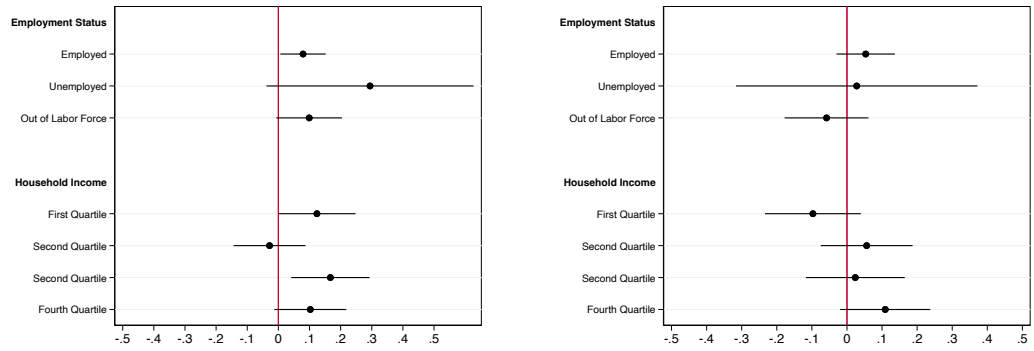
Figure 5.5.6: Heterogeneity analysis by regional political Environment

Individual characteristics Using information from the SOEP, we distinguish respondents by their labour market status (employed, unemployed, and out of the labour force) and household income quartiles. The coefficients in figure 5.5.7 show that there is not much of a difference across groups, as individuals react for the most part rather similarly to protests, both in terms of their concerns about hostility towards foreigners and immigration. In particular, it seems that unemployed respondents or those with the lowest incomes do react more negatively. These results are in line with the estimates on the regional level in Figure 5.5.5, suggesting that economic factors do not play much of a role in determining respondents' reactions to local or spontaneously organized large far-right demonstrations.

In addition, figure 5.C.2 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.

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



Notes: Figure 5.5.7, displays the coefficients from the estimation of Equation (1) on Worries about hostility towards foreigners in panel (a) and Worries about immigration in panel (b) restricting the sample to the group listed on the y-axis. All regressions consider a demonstration to be relevant if it has 1500 or more participants, use a 30 days bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. The Nuts II and the day of the demonstrations are excluded.

Figure 5.5.7: Heterogeneity analysis by individual labor market situation

Individual political attitudes To look at heterogeneity analysis by political attitudes, we rely on the panel structure of the SOEP. Since not all individuals are interviewed yearly, our sample size is reduced to 3,659 observations from the 10,902 observations reported in table 5.5.1.

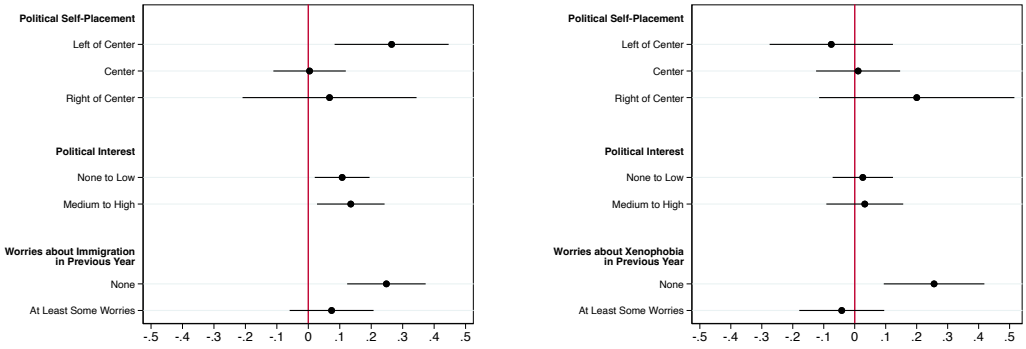
First, we consider SOEP interviewees' self-placement on the political spectrum - respondents can place themselves on a 0-10 scale from extremely left-wing (0) to extremely right-wing (10). Because this self-assessment only takes place every four to five years, we use the last known value, create an additional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

In contrast to economic characteristics, heterogeneities based on political attitudes, which are displayed in Figure 5.5.8, seem much more striking. The heterogeneity by self-placement on the political spectrum in figure 5.5.8 (a) shows an interesting picture; only those respondents who place themselves left-of-centre see an increase in concerns about hostility towards foreigners. On the other side, figure 5.5.8 (b) shows that the point coefficient for worries about immigration is the highest for respondents who place themselves right-of-centre, even though it is not significantly different from zero. Thus, previous political viewpoints appear to be key in individuals' receptiveness to protests.

When looking at the heterogeneous effects by "lagged" political interests, the estimates are virtually the same for those with higher and lower levels of interest. The coefficients of the heterogeneity analysis by "lagged" worries suggest that existing political or social attitudes are the major drivers in how people perceive and react to

protests. While the effects in 5.5.8 (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).

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



Notes: Figure 5.5.8, displays the coefficients from the estimation of Equation (1) on Worries about hostility towards foreigners in panel (a) and Worries about immigration in panel (b), restricting the sample to the group listed on the y-axis. All regressions consider a demonstration to be relevant if it has 1500 or more participants, use a 30 days bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. The Nuts II and the day of the demonstrations are excluded.

Figure 5.5.8: Heterogeneity analysis by political and social attitudes

5.6 Interests in politics and party preferences

In our main estimations, we have thus far only looked at the effect of far-right protests on attitudes in the native population. However, for policymakers and politicians, it might be important to know to what extent the changes in attitudes can lead to changes in party preference. In this section, we show suggestive evidence that by increasing the salience of immigrants and affecting public attitudes towards foreigners, large right-wing demonstrations can affect interests in politics and political preferences. 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 5.6.1 shows the results of estimating equation (1) on a continuous variable reflecting interest in politics (0-5, where 5 is high interest) in column (1) and four dummy variables reflecting party preferences in columns (2)-(5). In column (2), the dummy variable equals 1 if the individual does not have a preference for any political party, and in column (3) if the individual state a preference for a centre-left or left-wing party (SPD, Gruene, Die Linke, Piratenpartei), in column (4) if stated a preference for a centre-right or right-wing party (CDU, CSU, FPD) and in column (5) if stated a preference for a far-right party (AfD, NPD, Republikaner, Die Rechte).

The estimates in Table 5.6.1 suggest two main effects: respondents become more politically engaged in response to the protests, and this shift mainly helps left-wing parties. The coefficients in columns (1) and (2) indicate both an increase in political interest and in expressing a preference for a political party. The estimates in the following columns (3) to (5) show us that preference for left-wing parties increases

significantly by around 4.5 percentage points. At the same time, there is no significant increase in the propensity to favour right-of-centre or even far-right parties. While these coefficients do not perfectly inform us about the intentions of individuals, taken together, they imply that local or spontaneously organized large far-right demonstrations led to an adverse reaction in the population, as people became more active in opposing the protesters.

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

Robust Standard Errors in parenthesis. *p<.1; **p<.05; ***p<.01
Notes: Table 5.5.5 displays the coefficients from estimating Equation (1) using the outcomes: interest in politics and preferences for political parties in different spectrums. All regressions consider a demonstration to be relevant if it has 1500 or more participants, use a 30 days bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. The Nuts II and the day of the demonstrations are excluded.

Table 5.6.1: Interests in politics and party preferences

5.7 Conclusion

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

In this study, we use a regression discontinuity design to analyse how large and salient right-wing xenophobic demonstrations in one German district affect concerns about hostility towards foreigners and worries about immigration in the rest of Germany. Our results show that local xenophobic demonstrations lead to a significant short-term increase in worries about hostility towards foreigners at the national level, indicating that these demonstrations are perceived as a threat by Germans. On the other hand, worries about immigration are not affected by the demonstrations, indicating that the demonstrations are not successful in swaying public opinion in their favour. We also find that following far-right demonstrations, individuals become more politically interested, mainly benefiting left-wing parties. 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 enable us to examine a larger set 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 of 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 10 and 30 days before and after a given demonstration, our estimation approach allows us to claim that the demonstrations and not other factors have an impact on attitudes and party preferences. Thirdly, most of the political science and economics literature assumes that the effects of protests are primarily occurring in places where protests occurred (e.g., Madestam et al., 2013; Enos, Kaufman and Sands, 2019; Klein Teeselink and Melios, 2021; Wasow, 2020; Larreboure and

Gonzalez, 2021). However, we show that large-scale demonstrations also have an impact on national attitudes, especially in a time period when people can learn about these demonstrations in newspapers and television.

By showing how local or spontaneously organized right-wing demonstrations (e.g., at the district level) can impact attitudes at the national level, this study broadens our understanding of the consequences of the different types of demonstrations. Nevertheless, the conclusions of this study are precisely limited to protests that have a local or spontaneous nature. We cannot extrapolate to protests organized at the national level, where the paths, marches and slogans have undergone years of finance and design.

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Appendix

5.A Statistics

	count	mean	sd	min	max
Worry about Xenophobia	10902	2.0440	0.6745	1	3
Worry 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
Worry about health	10886	1.8008	0.6826	1	3
Worry about own economic situation	10890	1.9016	0.7032	1	3
Worry about global terrorism	5333	2.1378	0.6759	1	3
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

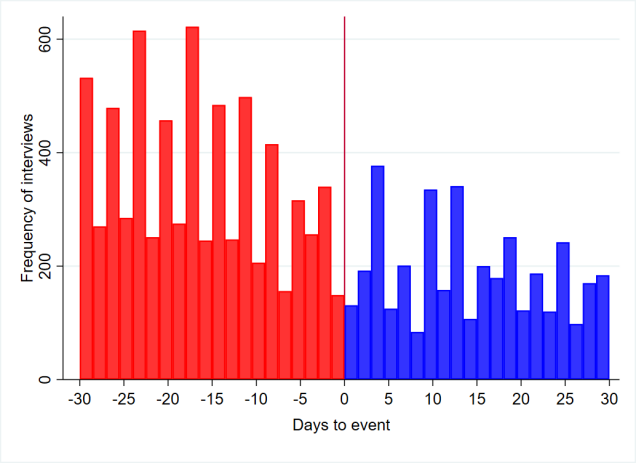
Statistics of the raw outcomes used in the analysis.

Table 5.A.1: Outcomes, 30 days isolated demonstrations, > 1500 participants

Date of Event	Location	Participants	Motto	Lexis Nexis Newspapers
08may2005	Berlin	3300	60 Jahre Befreiungslüge – Schluß mit dem Schuldkult	Spiegel Online, TAZ- Die Tages., General-Anzeiger (Bonn), Nürnberger Zeitung, DPA-AFX, Stuttgarter Nachrichten, Frankfurter Rundschau, NZZ, Agence France Presse
05aug2006	Dresden	6000	DS-Pressefest Meinungsfreiheit auch für Deutsche	TAZ- Die Tages., Der Tagesspiegel, Frankfurter Rundschau
08sep2007	Jena	1400	Fest der Völker– Für ein Europa der Vaterländer	Spiegel Online, Der Tagesspiegel, Rheinische Post, Frankfurter Rundschau
11jul2009	Gera	3900	Hier bleiben – anpacken – Rock für Deutschland	Frankfurter Rundschau, Berliner Zeitung
17oct2009	Leipzig	1350	Recht auf Zukunft	
07aug2010	Jänkendorf	2000	NPD Pressefest	TAZ- Die Tages.
01jul2011	Jänkendorf	2100	NPD Pressefest	TAZ- Die Tages.
19oct2013	Schneeberg	1500	"Schneeberg wehrt sich gegen Asylmissbrauch!"	TAZ- Die Tages., Stuttgarter Zeitung, Sächsische Zeitung Stamm. Dresden, Sächsische Zeitung Region.,
10oct2015	Gera	1500	gegen Asylmissbrauch!" "Schluss mit der verfehlten" Asylpolitik	Mitteldeutsche Zeitung, ZEIT-online Berliner Zeitung
27aug2018	Chemnitz	6000	"Sicherheit für Chemnitz"	Spiegel Online, Die Zeit, ZEIT – online, Welt Online, Der Tagesspiegel, Berliner Zeitung, Berliner Kurier, Sächsische Zeitung Stamm. Dresden, Sächsische Zeitung Region., Welt Kompact, Bilanz, Spaactor, Agence France Presse, Hamburger morgenpost, DPA-AFX, DPA RegioLine,
16nov2018	Chemnitz	2500	"Sicherheit für Chemnitz"	Der Tagesspiegel, Berliner Kurier, Sächsische Zeitung Region.
28oct2019	Dresden	3000	Pegida	TAZ- Die Tages., Die Zeit, BBC News, ZEIT - online, Sächsische Zeitung Stamm. Dresden, Sächsische Zeitung Region., Nürnberger Nachrichten, Spaactor, Bilanz
28dec2019	Aue/Bad Schl	2200	"Heimat bewahren – Überfremdung und deren Auswirkungen verhindern"	Sächsische Zeitung Stamm. Dresden, Sächsische Zeitung Region.

Table 5.A.2: Distribution of 30 days isolated demonstrations, $>$ 1200 participants

5.B Complementary evidence



Notes: The y-axis in Figure 5.B.1 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 5.B.1: Density test for protests with 1500 participants or more

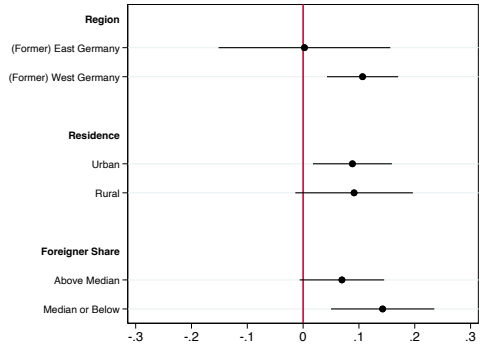
	Donate money or goods to help refugees	Work with refugees directly	Participate in initiatives to help refugees
	(1)	(2)	(3)
RD_Estimate	0.1121**	-0.0182	0.0810**
	(0.0523)	(0.0290)	(0.0361)
Observations	1652	1652	1652

Robust Standard Errors in parenthesis *p<.1; **p<.05; ***p<.01
Notes: Table 5.B.1 displays the coefficients from the estimation of Equation (1). All regressions consider a demonstration to be relevant if it has 1500 or more participants, use a 30 days bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. The Nuts II and the day of the demonstrations are excluded.

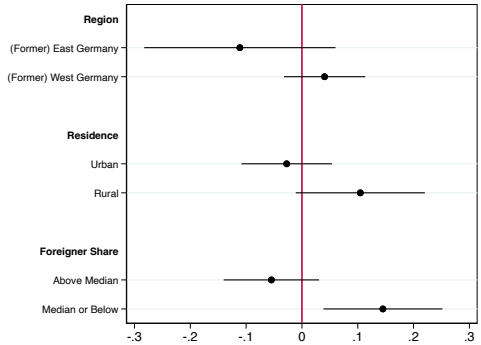
Table 5.B.1: Complementary evidence, 30 days isolated demonstrations, > 1500 participants

5.C Complementary heterogeneous effects

(a) Worries about hostility towards foreigners



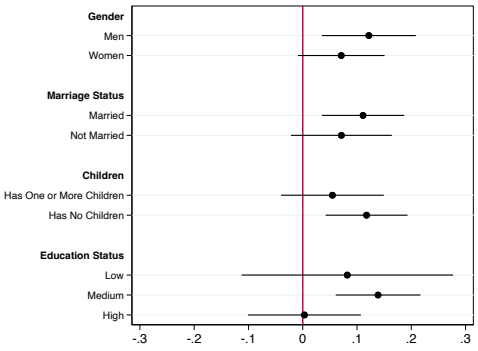
(b) Worries about Immigration



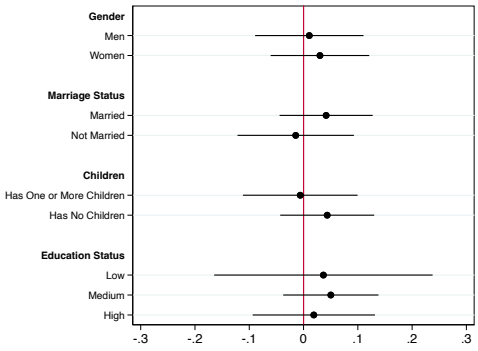
Notes: Figure 5.C.1, displays the coefficients from the estimation of Equation (1) on Worries about hostility towards foreigners in panel (a) and Worries about immigration in panel (b) restricting the sample to the group listed on the y-axis. All regressions consider a demonstration to be relevant if it has 1500 or more participants, use a 30 days bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. The Nuts II and the day of the demonstrations are excluded.

Figure 5.C.1: Heterogeneity analysis by regional Characteristics

(a) Worries about hostility towards foreigners



(b) Worries about Immigration



Notes: Figure 5.C.2, displays the coefficients from the estimation of Equation (1) on Worries about hostility towards foreigners in panel (a) and Worries about immigration in panel (b) restricting the sample to the group listed on the y-axis. All regressions consider a demonstration to be relevant if it has 1500 or more participants, use a 30 days bandwidth, a triangular kernel, a polynomial of order one, and heteroskedasticity-robust standard errors. The Nuts II and the day of the demonstrations are excluded.

Figure 5.C.2: Heterogeneity Analysis by individual demographic Characteristics

Chapter 6

Conclusion

In the past decade, European countries have reformed their migration policies to attract, retain and integrate migrants into their societies. At the same time, European countries have tried to balance migration and the responsibility of receiving asylum seekers with a surge in populist movements and migration scepticism among (some) natives.

This PhD thesis aims to improve the knowledge in the field of migration, while informing policymakers on the potential side-effect of migration policies. I utilize various empirical methods to study migrants' integration processes and their consequences for host countries using Germany as the host country of reference given its historical role in receiving migrants and refugees and due to the quality of available data. In Chapter 2, I provide causal evidence that terror events in the home country positively affect migrants' intentions to stay in Germany. This is reflected in shorter unemployment duration among non-EAA/Schengen migrants entering unemployment at the time of a terror event, and a bigger commitment to pursuing a long-term career in Germany among EAA/Schengen migrants entering unemployment at the time of a terror event. While this change in economic behaviour benefits Germany in the short term, the long-run consequences of such decisions are elusive since non-EEA/Schengen area migrants get employed in firms with lower top wages. The overall results in this chapter are policy-relevant for both host and home countries. The insights from this chapter help host countries in understanding the drivers of migrants' intention to stay and labour market outcomes, and inform home countries about the value of security and how improving security might attract migrants back home.

Chapter 3 shows that asylum seekers who were physically victimized during their journey to reach Germany are less likely to invest in human capital but are more likely to enter the labour force. However, the higher labour force participation is characterized by low-income and part-time employment. The mechanism behind this pattern is that victimized individuals are more likely to experience a loss in the sense of future orientation, leading them to engage in activities with immediate benefits (such as low-income employment) and delayed costs (a lack of access to higher-quality employment in the future). These findings cast doubt on the notion of swift labour market integration as a general success metric for refugee long-term integration. Furthermore, our results raise awareness of the potential repercussions of restrictive migration policies for optimal labour market trajectories at the destination. Finally, another important lesson from this study relates to the contextual factors of victimization events. The literature in psychology has documented that adolescents and young adults experiencing traumatic events have lower human capital investment

and less future-oriented planning, which affects their outcomes as adults (Ramos et al., 2013; Monahan et al., 2015; Schmidt, Zimmerman and Stoddard, 2018; Stoddard et al., 2015). Our findings imply that these negative consequences of physical victimization events can be extended to individuals who are at a point in life where they have to decide between investing in human capital or entering the labour market right away, regardless of their age.

I examine the socio-cultural integration of family migrants in a country that was not their primary choice in Chapter 4. The empirical analysis shows that gender and relative human capital are central predictors of who is a tied mover in a couple and that tied movers in Germany are more likely to be separated and less likely to be integrated and assimilated when compared to lead or equal movers. Understanding the ethnic identity of migrants is crucial since it influences their economic behaviour and life choices (Akerlof and Kranton, 2000). Most studies in economics have focused on the socio-cultural integration of economic migrants and refugees. However, studying the socio-cultural integration patterns of those who would not have come to Germany on their own (e.g., tied movers) is crucial because they might be at risk of becoming disconnected from both the host and home country communities, which can lead to adverse spillover effects on individual outcomes and their family's outcomes. The socio-cultural integration of an individual spouse is also likely to influence their partner's decision to stay in the destination country. Hence, improving the integration of accompanying spouses has vital consequences for retaining and attracting economic migrants. Furthermore, the ethnic identity of first-generation migrants also helps to understand the cultural integration of the second generation and the overall persistence of ethnic identity (Casey and Dustmann, 2010; Monscheuer, 2023).

Finally, in Chapter 5, I show that large local or spontaneously organized xenophobic demonstrations increase worries about hostility towards foreigners among native Germans. However, I find that protesters cannot sway respondents' attitudes in their favour nationally, as their concerns about immigration on aggregate do not change significantly. In line with previous studies, I uncover 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-centre parties are more successful. Moreover, at the individual level, I show that only respondents who place themselves left to the centre on the political spectrum show significantly increased worries about xenophobia. I also analyse to what extent the changes in attitudes lead to changes in party preference and show suggestive evidence that by increasing the salience of immigrants and affecting public attitudes towards foreigners, large right-wing demonstrations affect interests in politics and political engagement, mainly benefiting left-wing parties.

By showing how local or spontaneously organized right-wing demonstrations can impact attitudes at the national level, this chapter broadened our understanding of the consequences of the different types of demonstrations. Our results also show that exposure to incivility or poorly organized demonstrations could lead voters to distance themselves from the protester's agendas and turn to parties with counter agendas. This carries significant implications for the use of the democratic right to protest and provides practical implications for right-wing activist communities. Nevertheless, this study's conclusions are limited to protests that have a local or spontaneous nature. I cannot extrapolate to protests organized at the national level, where the paths, marches and slogans have undergone years of finance and design.

According to the previous literature, these demonstrations are far more successful in achieving their goals (Reny and Newman, 2021; Caprettini et al., 2021). A concern for the future is if local or spontaneously organized right-wing demonstrations only have an impact in the short run and fail to foster political interest in the long run.

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