Immigrants' Return Intentions and Labor Market Behavior When the Home Country is Unsafe*

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October 13, 2024

Abstract

Migration is often temporary, and the intended length of stay in the host country is an important determinant of immigrants' integration. This paper investigates whether shocks to safety conditions in the home country affect immigrants' return intentions and labor market behavior. We combine administrative and survey data with precise information on violent events worldwide and exploit the quasi-random occurrence of violent events in the home country relative to the timing of interviews and job separations in Germany. We show that immigrants interviewed after a violent event in their home country are 12 percentage points more likely to wish to remain in Germany permanently. The effects are stronger if immigrants are less integrated in Germany and have close family members in their home country. Consistent with the prediction that revisions to the intended length of stay affect immigrants' labor market behavior, we show that immigrants who enter unemployment when a violent event hits their home country increase their job search effort and find employment faster. However, the same immigrants trade immediate job security for lower earnings, less stable jobs and less productive firms.

JEL Code: J15 J61 J64
Keywords: immigration, uncertainty, violence, return migration, unemployment

*A previous version of this paper circulated as "Home Country Socio-Political Conditions, Return Intentions, and Labour Market Outcomes". This work was supported by funding from the European Union's H2020 Research and Innovation Program under the Marie Skłodowska-Curie Grant agreement No. 765355. The authors are grateful for helpful suggestions and constructive comments from Achim Ahrens, Silke Anger, Marco Bertoni, Benjamin Elsner, Gloria Gennaro, and Nikolaj A. Harmon, Linea Hasager, Timo Hener, Enrico Rettore, Stephan Schneider and Alex Yarkin. We also thank the participants at the EEA 2023, EPCS 2023, CEMIR Workshop on Migration Research 2023, and Junior Workshop in Econ. of Migration, SEHO 2023, 12th ifo Dresden Workshop on Labor Econ. and Social Policy, CoLab seminar 2023 at U. Copenhagen, Labor and Public Policy Seminar 2023 at Aarhus U., AYEW Workshop on Migration 2023, Royal Econ. Society Junior Symposium 2022, ZPESS 2022 at ETH-UZH, Voluntary and Forced Migration Conference 2021, and the 2021 IAB Brown Bag Seminar. All errors and omissions remain our own.

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1 Introduction

Many migration spells are temporary (OECD, 2019, 2008; Dustmann and Görlach, 2016).[1] Although immigrants arrive in the host country with an intended duration of stay, they often revise their expectations in response to changes in their personal circumstances and aggregate conditions at origin and destination (Dustmann and Görlach, 2016). These revisions to the intended length of stay may lead to subsequent changes in immigrants' economic behavior (Adda et al., 2022; Cortes, 2004; Dustmann, 1993, 1999), with important implications for not only the host and home countries but also for the immigrants themselves (Barsbai et al., 2017; Colas and Sachs, 2023; Dustmann and Frattini, 2014; Foged and Peri, 2016; Mobarak et al., 2023).

As a result, previous research has studied extensively how individual characteristics (Bijwaard and Wahba, 2014; Dustmann, 1993, 1997; Gibson and McKenzie, 2011; Nekby, 2006) and host country economic and political conditions (de Coulon et al., 2016; Elsayed, 2018; Gould and Klor, 2016; Schilling and Stillman, 2024) affect immigrant's return intentions and labor market behavior. However, there is little evidence on the role of home country conditions. The few existing studies focused on economic shocks (Nekoei, 2013; Yang, 2006), but neglected the role of violence and safety conditions at origin. As the world experiences an upsurge in civil conflicts, wars and terrorism, understanding immigrants' behavioral responses to the unfolding of these events in their home countries is crucial.

In this paper, we study whether violent events in the home country affect return intentions and, in turn, the economic behavior of immigrants abroad. The underlying mechanism is that violent events in the home country affect the perception of security and hence work as shocks to immigrants' location preferences by increasing the attractiveness of the destination country relative to the home country. Our findings reveal that a negative shock to the safety conditions in the home country increases immigrants' desire to stay abroad permanently. For unemployed immigrants, the occurrence of violent events in their origin country shortens their unemployment duration but also worsens their employment outcomes.[2]

[1] According to an OECD (2008) report on migration, approximately 20 to 50 percent of immigrants in OECD countries leave the host country five years after their arrival.

[2] 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 et al., 2018), we are interested in the effect on contemporaneous re-employment decisions, which are based on current return plans.

In the empirical analysis, we proxy changes in safety conditions in the home country with the occurrence of terrorist events for three reasons. First, the frequency and size (e.g., damages and victims) of terrorist attacks are largely unpredictable. Second, previous research has shown that terrorist attacks are highly correlated with increased levels of uncertainty (Baker et al., 2023) and reduced feelings of safety (Brodeur, 2018). Third, terror events are likely to be perceived as a negative event in all aspects by the general population. Other types of violent events, such as armed conflicts, might be supported by individuals living at origin and abroad based on group identification (e.g., government-dominated ethnic group vs another ethnic group). Terror events, on the other hand, are supported by a small minority of extremists who are unlikely to be captured in nationally representative surveys.

The data on terrorist attacks come from the Global Terror Database (GTD), which is a large dataset containing information on almost 200,000 terrorist events that have occurred worldwide from 1970 to 2018. Events are recorded daily, and the geographical location where the events took place is highly precise.

In contrast to previous studies that have considered the absolute number of casualties from terror events (see e.g., Akay et al., 2020; Keita and Schewe, 2021; Sønderskov et al., 2021), we introduce a relative measure of violence that accounts for country-specific trends. This novel measure rests on the idea that individuals coming from countries with a high number of terrorist events in the recent past have a different reference point in comparison to individuals from countries where terrorist attacks rarely occur.

To estimate the effect of terrorist events on return intentions, we combine the GTD data with the German Socioeconomic Panel (GSOEP). The GSOEP is a large-scale survey representative of the German population. It has been conducted annually since 1984 and includes a wide variety of individual-level information. Crucial for our analysis, it also collects information on nationality, return intentions, and interview date.

The identification strategy relies on the quasi-random occurrence of the date of terrorist events at the origin relative to the timing of the GSOEP interviews and the characteristics of the respondents. We find that immigrants interviewed immediately after terrorist attacks are 12.0 percentage points more likely to declare that they

want to stay in Germany permanently. These effects persist up to 3 months after the event and are particularly strong among immigrants who are less integrated (e.g., scarce German knowledge) and have close family members in the home country. We find that employed and unemployed immigrants are equally likely to revise their return intentions following a terror event.

The key identifying assumption is that terrorist events in home countries did not interfere with the survey implementation. To validate our empirical design, we show that neither the number of interviews nor the characteristics of survey respondents differ around terrorist events. Our baseline estimates are not driven by any specific country or survey year and are robust to alternative definitions of terror events and bandwidths. Finally, we provide two pieces of evidence to ensure that we are not capturing any statistical artifacts in the data. First, we assign random dates to terror events (placebo events) and observe no impact on the intention to stay. Second, we show that home country terror events do not affect placebo outcomes, such as concerns about crime and the environment in Germany.

In the second part of the analysis, we look at the effect of terrorist events on labor market outcomes. As changes to investments in human capital and career choices take time to materialize, we focus on the job search behavior of unemployed immigrants, which is an outcome that can react quickly to changes in individual circumstances. We proxy job search behavior with unemployment duration.

We argue that, by leading to an update in immigrants' return intentions, violent events in the home country may affect job search and reservation wages among unemployed immigrants. However, because violent events can affect these variables jointly, they will have ambiguous consequences for unemployment duration. On the one hand, unemployed immigrants may fear having to return to their home country; thus, they increase their job search activity and lower their reservation wages to find employment faster, thereby potentially trading off longer-term job stability and quality. On the other hand, terror events in the home country may increase the length of unemployment if immigrants become more selective with respect to the jobs they are willing to accept, or if the occurrence of terror events at origin negatively affects their mental health.

To accurately measure the time to reemployment, we rely on German

administrative data (IEB), using a 40 percent random sample of the immigrant population in the social security records between 2000 and 2018. This empirical strategy mirrors, as closely as possible, the one used to estimate the effects of terror events on return intentions. A difference between the two settings is that return intentions are measured on a specific day, while unemployment duration can span several days. Hence, we define our main outcomes of interest as the likelihood of finding employment *within one month and within three months*. We then compare these labor market outcomes between immigrants who enter unemployment when terrorist events occur in their home countries (treated) and immigrants from the same home country who enter unemployment three months earlier when no terror event occurred (control).

Our results show that immigrants who enter unemployment in a month when terror events occurred in their home country are 1.7 percentage points more likely to be employed within three months than individuals from the same home country who entered unemployment three months before the relevant terror event. These results are robust to placebo treatment assignments and alternative definitions of terror events and treatment groups. For those immigrants who find a job within three months, we further look at the type of jobs and the firms to which they match. Immigrants who enter unemployment during unstable times in their home country have lower daily wages and are 5.1 percentage points less likely to be in full-time employment than immigrants who enter unemployment under stable conditions. We find no statistically significant difference in the likelihood of changing occupation or industry.

Turning to firm characteristics, immigrants who enter unemployment when a terror event occurs in their home country are 3.4 percentage points less likely to change to a firm with a wage premium above the median than immigrants who enter unemployment under stable conditions. These results suggest that immigrants who enter unemployment in a month with high levels of violence at home trade immediate job security for lower earnings, unstable jobs, and less-productive firms.

We contribute to the literature in three ways. First, we provide empirical evidence on the link between return intentions and safety conditions in the home country. Given the importance of temporary migrations, several studies have analyzed the individual

determinants of return intentions (see Bijwaard and Wahba, 2014; Dustmann and Görlach, 2016). Fewer studies have examined the country-level determinants of return migration, showing that economic conditions in the home country (e.g., GDP, exchange rates) matter for the well-being of immigrants abroad (Akay et al., 2017) and may determine migration flows and the amount of remittances (Nekoei, 2013; Yang, 2006). Closest to our study, Steinhardt (2018) shows that xenophobic violence in the host country affects immigrants' return intentions; Alrababah et al. (2023) use a conjoint experiment to show hypothetical scenarios to Syrian refugees in Lebanon and evaluates under which conditions they would return to the homeland; and Beaman et al. (2022) show descriptively that a reduction in conflict intensity in Syria increases the likelihood of refugees returning to Syria. We are the first to provide quasi-experimental evidence that violence in the home country has a causal effect on the return plans of immigrants.[3] Additionally, we also show that our results can be generalized to all origin countries and immigrants with diverse migration statuses.

Second, we contribute to the literature on the effects of external shocks on the labor market integration of immigrants. Previous studies have shown that terrorism in the host country affects immigrants' integration. For example, Gould and Klor (2016) find that the 9/11 attacks had long-lasting effects on the integration of Muslim immigrants, while Brodeur and Wright (2019) show that the same events also reduced asylum approval rates. Steinhardt (2018) finds that xenophobic violence by native Germans reduces Turkish immigrants' investments in language skills. We show that terrorist events in the origin country affect not only return intentions but also the labor market behavior of immigrants. While we cannot directly link the effect of terror on return intentions to its effect on immigrants' labor market behavior, we show that terror events that create a plausible shock to return intentions also affect the search behavior of immigrants.

Third, despite using terrorism as a proxy for sociopolitical turmoil and violence in the home country, our paper is related to the literature on the consequences of terrorism for individuals. Several studies have found that terrorism in the location of residency affects political opinions and voting behaviors (Peri et al., 2020) and

[3] In a recent working paper, Adema et al. (2024) use a longitudinal survey of recent Ukrainian refugees across Europe following Russia's full-scale invasion of Ukraine in February 2022 and find that an intensification of the ongoing conflict in Ukraine does not affect refugees' return intentions but it leads to an increase in national pride.

reduces the well-being of individuals (Akay et al., 2020; Clark et al., 2020). Close to our paper, Sønderskov et al. (2021) and Keita and Schewe (2021) show that terrorism in the origin country affects refugees' mental well-being in the host country. Using comparable research designs and a relative measure of terrorism intensity, we show that terrorist events in the home country affect immigrants' return plans and labor market behavior.

The rest of the paper is organized as follows. Section 2 describes the data. Section analyses terror and return intentions and Section 4 analyses terror and labor market behavior. Section 5 concludes.

2 Data

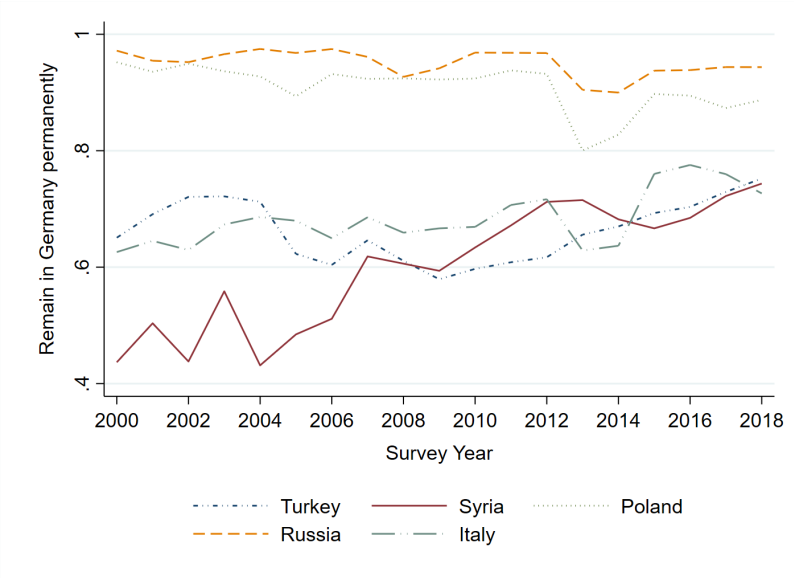
German Socio-Economic Panel: To analyze the impact of violent events on the intended length of stay in Germany, we use the full dataset from the German Socioeconomic Panel (GSOEP) for the years 2000 to 2018. The GSOEP is a large-scale yearly household survey and is representative of the German population.[4] The dataset contains individual and family information on various topics, including education, work-life balance, consumption and more behavioral and attitudinal characteristics. Crucial to our analysis, around 20% of respondents are immigrants. If they have a migration background, the respondents are asked migration-specific questions, such as their country of origin, presence of family abroad, German knowledge, and return intentions. The GSOEP has been widely used to study the socioeconomic integration of immigrants and, specifically, to investigate return migration intentions (see e.g. Dustmann and Görlach, 2016; Bauer and Sinning, 2011). In our setting, we focus on intentions to stay in Germany rather than actual return migration for two reasons. First, return intentions react immediately to violent events occurring in the home country, while actual returns may take time to materialize. Second, economic behavior in the host country is based on contemporaneous intentions to stay. Nonetheless, previous research has shown that return intentions are closely related to realized migration patterns (Adda et al., 2022; Sallam, 2023).

Figure 1 plots the share of immigrants who intend to remain in Germany permanently for the largest nationality groups contained in the GSOEP. While

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

Eastern European immigrants (some of who 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.[5]

Figure 1: Remain in Germany permanently, main groups



Notes: Figure 1 displays the share of immigrants who intend to remain in Germany permanently. Shares are computed for each survey year (from 2000 to 2018) only for the 5 largest immigrant nationality groups. Source: GSOEP

In Table 6.2 in Appendix 6.1, we show descriptive statistics of the migrant population in the GSOEP. A very high share of the immigrants in Germany over the period under analysis report having only a lower secondary education or below. While the mean number of full-time employees over the 2000-2018 period is only 0.34, these results are driven by the large inflows of refugees in Germany over the years and by the low labor force participation rate among female immigrants. Refugee employment over the first two to three years after migration is relatively low; however, it then catches up with the remaining migrant population. Finally, most immigrants want to remain in Germany for many years.

IEB social security records To analyze the effect of terrorist events on labor market outcomes, we rely on the social security records, *Integrated Employment Biographies* dataset (IEB), for a random drawing of 40 percent of the full population of immigrants in the German labor market. The IEB dataset is provided by the Institute of

[5]The increase in intention to stay may be due to compositional changes and panel attrition. In Appendix 6.1, we show the share of immigrants in the GSOEP over time and discuss the different waves of migration to Germany in more detail.

Employment Research (IAB) of the German Federal Employment Agency.[6] The dataset includes detailed daily administrative longitudinal information on nationality,[7] occupation, educational background, industry, employment status, and earnings records of all individuals subject to social security in Germany.[8] Crucial for our empirical strategy, we have information on the exact date when immigrants register for unemployment, their occupation, their daily wages and the characteristics of the firms in which they are employed.

Global terror database We proxy safety conditions in the home country with the occurrence of terrorist events. Data on terrorist attacks worldwide come from the Global Terror Database (LaFree and Dugan, 2007). The Global Terror Database (GTD) data are collected daily using both human and machine intelligence methods.[9] The data collection procedure allows to identify unique attacks, record the details of each event (e.g., date, location, and number killed), and update the records of previously recorded events as new information becomes available (The Global Terrorism Database, 2019).

In Figure 7.1 in Appendix 7, we present descriptive statistics on the terror events from the GTD database. The left-hand-side panels of Figure 7.1 show the monthly number of 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 has experienced a spike in terror events in the last five years, in contrast to other countries that display a more even distribution of events over time. Additionally, while Poland and Kazakhstan have experienced only a few scattered events, Turkey has experienced frequent events since the 2000s.

In contrast to previous papers that have used the absolute number of casualties (see, Akay et al., 2020; Keita and Schewe, 2021; Sønderskov et al., 2021), we introduce a relative measure of terror that accounts for country-specific terrorism trends. This

[6] For the description of a 2-percent random sample from the IEB, the *Sample of Integrated labor Market Biographies* (SIAB), see Antoni et al. (2019), for applications of the IEB data see, for example, Card et al. (2013) and Dustmann et al. (2009).

[7] As individuals may naturalize, we use the nationality in their first record as proxy for their country of origin.

[8] Employers must report these data for each employee. Typically, reporting occurs once a year, although any contractual change is also reported, e.g. from part-time to full-time.

[9] 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.

measure is based on the idea that individuals coming from countries with a high number of terrorist attacks in the recent past have a different reference point in comparison to individuals from countries that have rarely experienced terrorist attacks.[10] For instance, one terrorist event in a country such as France in March 2016 is likely to create a greater shock to the perception of security and a greater reaction among French immigrants abroad than one terrorist event in Syria, which was experiencing a period of intense turmoil in 2016.

For each month-year-country of origin combination, a terror event is defined as follows:

- *Relevant* if there is at least one more terror attack in a given month than the past three-year monthly average.[11]
- *Isolated* if no other relevant events occurred in the pre- and post-period.[12]

We formally define a relevant and isolated terror event in Appendix 9. The *isolation* criterion is imposed such that the control group is not affected by any other relevant terror event. Together, the *relevant* and *isolation* criteria imply that we are selecting relatively calm periods in home countries that were followed by a relevant terror event. Hence, the control group is composed of individuals who were interviewed when perceived security was relatively high, while the treatment group includes individuals who were interviewed after a shock to those perceptions of security.

In all analyses, we use terror events that are both relevant and isolated. These are displayed in the right-hand side panels of Figure 7.1 in Appendix 7.[13] Additionally, we exploit available information on the number of deaths to measure immigrants' response to the intensity of terrorist events. For readability matters, we will refer to relevant and isolated terror events simply as relevant terror events.

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

[11] Alternatively, we use the past four-year and five-year averages. 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 standard reference period for all respondents.

[12] Effectively, we are restricting the analysis to relevant events where, in the preceding and following 3 months of the first month included in our analysis, there was no other relevant terror event. Alternatively, we use smaller bandwidths, such as 1 and 2 months. The main results do not change with the different bandwidths. The definition of an isolated event is similar to Graeber and Schikora (2021).

[13] Table 7.1 in Appendix 7 additionally shows the number of relevant and isolated terror events per country and the mean number of monthly terror attacks per relevant and isolated terror event.

3 Unsafe home countries and immigrants' return intentions

In this part of the analysis, we test whether a decrease in safety conditions (e.g., a terrorist event) in the home country positively affects immigrants' intention to stay in Germany. We hypothesize the following mechanism: a violent event in the home country works as a shock to immigrants' location preferences. This shock raises the attractiveness of the host country relative to the home country, which in turn increases the desire to either remain permanently in the host country or to delay the timing of return migration.

3.1 Empirical strategy

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

$$I_{i,o,y,m,f} = \alpha + \sum_{m=-3}^{3} \beta_m Time_{i,o,y,m} + \delta X_{i,y} + \mu_{o,y} + \phi_{m,y} + \lambda_f + \epsilon_{i,o,y,m,f} \quad (1)$$

where $I_{i,o,y,m,f}$ measures the intentions to stay in Germany of individual i from country of origin o, interviewed in year y, and month m and residing in federal state f. $Time_{i,o,y,m}$'s are dummies identifying periods around the event where m denotes months since a month with a relevant terror event (e.g., $m = -2$ for those i interviewed 2 months before the event).[15] The coefficients β_m identify dynamic treatment effects, m= -1 is the baseline omitted period. $\phi_{m,y}$ are interview month times year fixed effects, $\mu_{o,y}$ are country of origin times year fixed effects, and λ_f are federal state of residence fixed effects. $X_{i,y}$ is a set of individual controls that includes age, gender years since migration, years since migration squared, marital status, children, and educational achievement.

Including country-of-origin times year fixed effects ($\mu_{o,y}$) allows us to compare

[14] This design has been recently used to study the effect of terrorism on well-being (Akay et al., 2020; Clark et al., 2020; Graeber and Schikora, 2021) and political opinions (Peri et al., 2020), as well as the effect of football victories in international competitions on national identity sentiments (Depetris-Chauvin et al., 2020)

[15] Since the definition of a relevant terror event is based on a total number of terror attacks in a month, we exclude the month when a relevant event occurred in all our regressions and use only the days before as the control group and the days after as the treatment group. For example, if in March 2020 there is a relevant event, $m = -2$ refers to January 2020 and $m = 2$ to May 2020.

outcomes for immigrants from the same country of origin who were interviewed in the same year either right before or right after the relevant and isolated terror event. The year and month of the interview fixed effects ($\phi_{m,y}$) allow us to control for month-year events that affect individuals from all origin countries. Standard errors are clustered at the event level. The estimated coefficient averages the effects across countries of origin and terrorist events.

Our main specification includes only immigrants interviewed within a 90-day bandwidth from the relevant terror event. Using *isolated* terror events within a 90-day bandwidth ensures that the control group is not treated by any relevant terror event based on a 90-day criterion. In Section 3.3, we show the results using smaller bandwidths, such as 30 and 60 days. The pre- and post-treatment periods will vary according to the bandwidth used to classify a relevant event as isolated.

To summarize the average treatment effect over all periods, we also estimate:

$$I_{i,o,y,m,f} = \alpha + \beta PostTerror_{i,o,y,m} + \delta X_{i,y} + \mu_{o,y} + \phi_{y,m} + \lambda_f + \epsilon_{i,o,y,m,f} \qquad (2)$$

where the time dummies are substituted with the indicator $PostTerror_{i,o,y,m}$, 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 the respondent is interviewed within 90 days before that same event.

Our identification strategy relies on the quasi-random occurrence of terrorist events relative to the exact time at which immigrants were 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 such an event. This nonrandom selection of respondents may bias our results upward toward the intention to remain in Germany. To test our assumption, we first plot in Figure 10.2 in Appendix 10.1 the share of interviews around each country-specific terror event used in our main estimations.[16] Figure 10.2 shows that there is no evidence of a correlation between the implementation of

[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 point relative to the total number of interviews, e.g., ii) / (i).

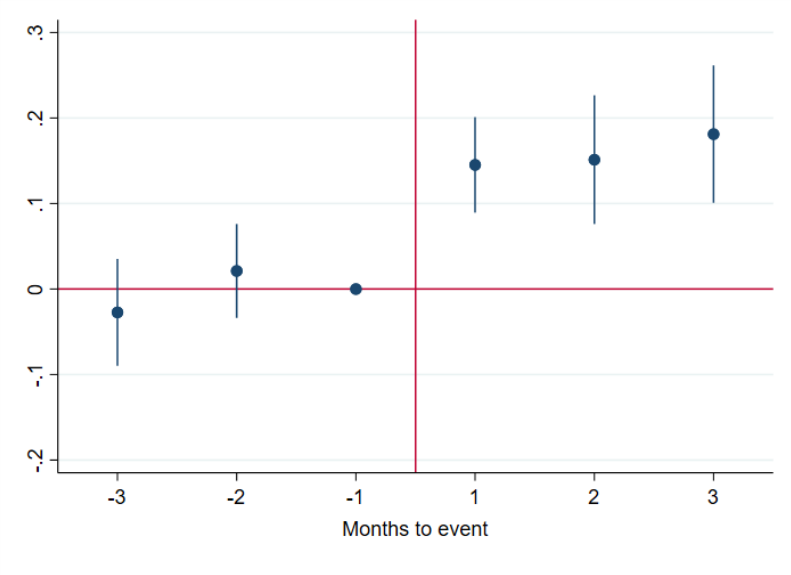
the survey and the occurrence of events.

As a second test, we show that respondents interviewed before or after a terror event are not different in their baseline characteristics. We regress the treatment status (i.e., being interviewed after a terrorist event in the home country) on each individual characteristic 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 displayed in Figure 10.1 in Appendix 10.1. There seems to be no difference between the treatment and control groups across all included characteristics.

3.2 Main results

In this section, we present our main results for the effect of violence in the home country on immigrants' intentions to remain in Germany. We first show graphical evidence of how intentions to remain in Germany evolve in the months around terror events when using a 90-day bandwidth and considering an event to be relevant if the number of terror attacks in a given month is higher than the past three-year average. Figure 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 the terrorist events are not significantly different from those for individuals interviewed in the month before the event, while the coefficients are significantly positive for immigrants interviewed after the event. Moreover, the plots show that the increase in intentions to remain in Germany lasts up to 3 months after the event.

Figure 2: Event study: terror events and intention to remain in Germany, 90-day bandwidth



Notes: Figure 2 displays the event study plot from the estimation of Equation 1, where the outcome is "Remain permanently in Germany".The regression considers a 90-day bandwidth. Country of origin-year fixed effects, month-year fixed effects and German federal state fixed effects are included. The bars indicate 95-percent confidence intervals.

In Table 1, we report the results based on Equation 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 monthly average. Column (1) uses only the baseline fixed effects; Column (2) adds gender, age, years since migration, and years since migration squared to the controls in Column (1); Column (3) adds marital status and the presence of children to the controls in Column (2); and Column (4) adds educational achievement to the controls in Column (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 15.4 per cent relative to the mean value of the outcome variable (0.798). Overall, the results suggest that terrorist events in the home country positively affect the intention to remain in the host country (Germany) permanently. In Section 4, we test whether changes in the intention to remain in Germany affect the integration of immigrants in the labor market.

Table 1: Terror events and intentions to remain in Germany, 90-day bandwidth

	Higher than average of last 3 years			
	(1)	(2)	(3)	(4)
Post-Terror	0.122***	0.122***	0.125***	0.123***
	(0.029)	(0.029)	(0.029)	(0.029)
Observations	6604	6604	6604	6604
Mean intention to stay	0.798	0.798	0.798	0.798
Origin country FE x Year FE	Yes	Yes	Yes	Yes
Year FE x Month FE,	Yes	Yes	Yes	Yes
State of Residency FE	Yes	Yes	Yes	Yes
Individual Controls	No	Some	Some	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 1 displays the coefficients from the estimation of Equation 2 where the outcome is "Remain permanently in Germany". All the results use a 90-day bandwidth. Column (1) uses only the baseline fixed effects; Column (2) adds gender, age, years since migration, and years since migration squared to the controls in Column (1); Column (3) adds marital status and the presence of children to the controls in Column (2); and Column (4) adds educational achievement to the controls in Column (3).

Given our definition of treatment, the coefficients reported in Table 1 summarize the effect of a shock to safety conditions at origin on immigrants return intentions. These effects capture the response at the extensive margin. In Table 2, we explore whether differences in the intensity of terrorist events matter for the intention to remain permanently in Germany. Therefore, we interact the *PostTerror* dummy in Equation 2 with a dummy variable that equals 0 if no or less than k individuals were killed and equals one if k or more individuals were killed, for k= 10, 30, 50. The results show that the intensive margin also matters. The greater the number of people killed is, the stronger the effect of terrorist events on return intentions is.

Table 2: Intensity of terror events and intentions to remain in Germany, 90-day bandwidth

	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)	(0.031)	(0.031)
Post-Terror × (k or > than killed)	0.096**	0.197***	0.223***
	(0.041)	(0.039)	(0.059)
Observations	6604	6604	6604
Mean intention to stay	0.798	0.798	0.798
Origin country FE x Year FE	Yes	Yes	Yes
Month FE x Year FE	Yes	Yes	Yes
State of residency FE	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 2 displays the coefficients from the estimation of Equation 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 the results use a 90-day bandwidth. Individual controls include age, sex, years since migration and its square, marital status, educational achievement, and children.

Additionally, we ask whether the response to terrorist events in the home country is the same for immigrants from countries with a durable conflict and those from

politically stable countries. Table 7.1 in Appendix 7 shows that there must be 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 periods with relevant events but only those that occurred in isolated periods, as explained in Section 3.1. We can see that while in Belgium or Norway, 2 terrorist attacks in one month are enough for this month to be considered relevant, in Colombia and Iraq, a terror event is relevant and isolated when there are 17 and 285 attacks, respectively. This is driven by the fact that Colombia and Iraq experience not only many terror attacks in a given month (relevance) but also many relevant terror events in consecutive months (isolation).

To study this question in more detail, we use the Political Stability Index from the World Bank[17] to rank countries based on their level of 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 measures, countries of origin are categorized as follows: i) low political stability if the ranking is less than or equal to 25; ii) moderate political stability if the ranking is greater than 25 and less than or equal to 75; and iii) high political stability if the ranking is greater than 75.[19] The results are shown in Table 3 Columns (1) and (2) and exhibit no particular difference between countries with different political stability rankings. For instance, in Column (2), individuals interviewed after a terror event occurred in a country with a low level of political stability are 13.4 percentage points more likely to wish to remain in Germany permanently than individuals who were interviewed before the terror event. This figure compares to the 11.5 percentage points and 13.5 percentage points for individuals interviewed after a terror event occurring in a country with middle and high levels of political stability, respectively.

In Table 10.1 in Appendix 10.2.1 we use an alternative approach to measure political instability by taking the mean monthly number of terror attacks in the past three years used to classify terror events as relevant. The results are comparable.

[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 10.1 in the Appendix

Table 3: Overall political stability, terror events and intentions to remain in Germany, 90-day bandwidth

	Political stability index	
	Previous year	Mean previous 3 years
	(1)	(2)
Post-Terror × Pol. Stab. <=25	0.146***	0.134***
	(0.041)	(0.039)
Post-Terror × Pol. Stab. [25-75]	0.109***	0.115***
	(0.034)	(0.035)
Post-Terror × Pol. Stab. > 75	0.136***	0.135***
	(0.050)	(0.048)
Observations	6604	6604
Origin country FE x Year FE	Yes	Yes
Month FE x Year FE	Yes	Yes
State of residency FE	Yes	Yes
Individual controls	Yes	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 3 displays the coefficients from the estimation of Equation 2 interacted with a dummy variable proxing for political stability. All results use a 90-day bandwidth. Individual controls include age, sex, years since migration and its square, marital status, educational achievement, and children.

3.3 Placebo tests and robustness checks

In the previous section, we showed that terrorist acts in home countries positively impact immigrants' intention 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 reference point (e.g., past three-year monthly average) above which we consider a terror event to be relevant. In Table 10.4 in Appendix 10.4, we display the estimated coefficients when reducing the bandwidth from 90 days (i.e., the baseline bandwidth) to 60 days and then 30 days around the terror event and when considering if, in a given month, there is 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 are to the terror event, the greater the effect is on the intention to remain permanently in Germany.

Additional controls and different clustering First, to ensure that we are comparing individuals from the same home country who face similar circumstances, in Table 10.5 in the Appendix, we add country of origin times years since migration (Column (2)), country of origin times age groups (Column (3)) and country of origin times

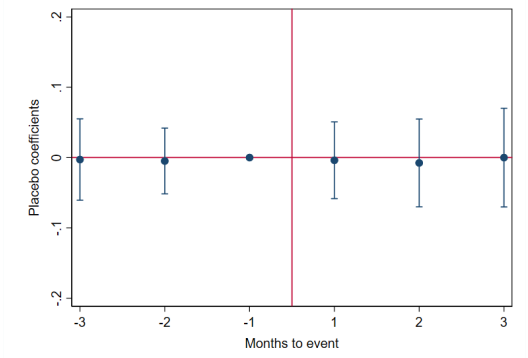
gender (Column (4)) to our baseline specification displayed in Column (1) of Table 1. The main results are robust to the inclusion of these control variables.

Second, in all our main results we cluster the standard errors at the terror event level (e.g., treatment). In Table 10.6 in the Appendix, we alternatively cluster the standard errors at the country of origin level and the year-country of origin level. This does not change the significance of our results.

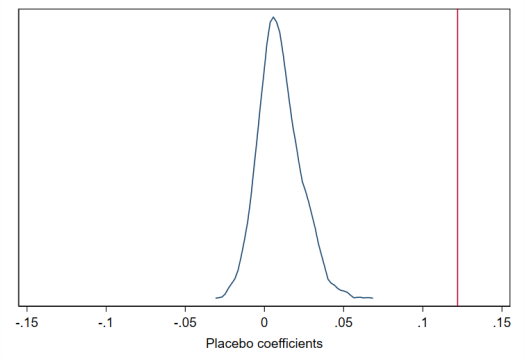
Placebo terror event date As a placebo test, we assign a random date to each relevant terror event for each country of origin. We replicate this procedure 300 times and estimate both Equation 1 and Equation 2 to obtain the coefficients of the placebo terror events for the dynamic and static specifications. Figure 3a displays the plot resulting from averaging event study coefficients across all 300 replications. We find no effect of the placebo terror events on the intention to remain in Germany permanently. Figure 3b shows that the distribution of placebo β coefficients from Equation 2 is concentrated around zero, far from the 0.12 estimated in Table 1 using the true dates of the relevant terrorist events.

Figure 3: Placebo tests using random terror dates

(a) Event study, 90-day bandwidth



(b) Coefficient distribution, 90-day bandwidth



Notes: Panel 3a displays the coefficients from the estimation of Equation 1 using placebo terror events. Panel 3b displays the distribution of the coefficients from the 300 estimations of Equation 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. The bars indicate 95-percent confidence intervals.

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 hostility towards foreigners,

crime in Germany, economic development, and the environment. A higher value reflects fewer worries.[20] As some of these variables rely on questions that are not asked in all survey waves, our sample differs with respect to the outcome. Table 4 shows the coefficients from estimating Equation 2 using these alternative outcomes. We see no significant effect of relevant terror events in the home country on these outcomes.

Table 4: Terror events and placebo outcomes, 90-day bandwidth

Worries about	Higher than average of last 3 years			
	Hostility Foreign.	Crime in Ger.	Econ. Develop.	Environment
	(1)	(2)	(3)	(4)
Post-Terror	0.034	0.054	0.017	-0.044
	(0.074)	(0.068)	(0.059)	(0.075)
Observations	6292	5097	5334	5085
Origin country FE x Year FE	Yes	Yes	Yes	Yes
Month FE x Year FE,	Yes	Yes	Yes	Yes
State of residency FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 4 displays the coefficients from the estimation of Equation 2 where the outcome is a dummy variable that equals one if the respondent is worried about a particular subject and zero otherwise. Column (1) reports on worries about the future of the EU, Column (2) about crime in Germany, Column (3) about economic development, and Column (4) about the environment. All the results consider a 90-day bandwidth. Individual controls include age, sex, years since migration and its square, marital status, educational achievement, and children.

Excluding a year or a country 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 while excluding countries of origin at a time. Figure 10.4 Panel a) in Appendix 10.4 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.

Different treatment event definition Our main treatment definition selects year-month combinations in which the number of terror attacks is above the average monthly number of attacks in the past three years (relevant), and no other relevant event occurred in the preceding three months (isolated).

While this definition ensures that we have a well-defined treated and control group, it also excludes countries that have been repeatedly exposed to an ever-growing

[20] For each of these worries, individuals could reply 1) big worries, 2) some worries, or 3) no worries

number of terrorist attacks. Hence, as a robustness check, we show a simpler specification that is not restricted to the relevance or isolation criterion.

In Table 10.7 in the Appendix, we estimate equation 2 using two alternative treatments: i) whether or not there was any terror attack in the 90 days before the interview, and ii) the ln number of terror attacks in the 90 days before the interview. The interpretation when using these treatments is slightly different since we are not necessarily capturing an unpredictable shock to the perception of security.[21] Nevertheless, we still find that there is a positive and significant effect of terror attacks on return intentions, albeit smaller in magnitude than when using relevant and isolated terror events.

Other types of violent events Our definition of safety at origin relies on the occurrence of terrorist attacks. To broaden this definition to other types of violence, we use armed conflict events in the country of origin. Data on armed conflicts come from the Uppsala Conflict Data Program (UCDP) Event Dataset.[22] In Appendix 10.4, we provide details on the UCDP Event Dataset and the procedure used to construct the relevant and isolated armed conflict events. The results in Table 10.8 in the Appendix show that the effect of armed conflict events on return intentions are broadly in line with those in Table 1 when using terror events.

One concern of using armed conflict as a violent shock is that we can only look at conflict-ridden countries, while with terror events, we are able to cover a broader set of countries across the globe.[23]

A second concern of using armed conflict as a shock is that, while the general population does not support terror events, in the case of state and non-state conflict, it is less clear. Individuals living at origin and abroad might support cases of government violence or opposing party violence (e.g., government-dominated ethnic group vs another ethnic group, ruling party vs. another political party).[24] Hence,

[21] For instance, consider Iraq, a country experiencing several terror attacks in 2000-18 and for which a terror event is considered relevant if there were more than 285 terror attacks in a given month. With the simpler treatments, we are also considering as treated individuals interviewed during a relatively stable period in Iraq but where there is still one terror attack.

[22] A common alternative is data from the Armed Conflict Location and Event Data (ACLED) Project. These data, however, cover armed conflicts since 1997 for Africa, while for other countries the data collection only started in 2016.

[23] Terror events take place in almost all countries across the Globe during the twenty-first century - including Europe, which has been less prone to armed conflict than other regions. Given the largest migrant groups in Germany (Table 6.1 in the Appendix), it is particularly relevant for our analysis to cover Europe.

[24] Examples of such conflicts include the Ethiopian government against the former ruling party Tigray People's Liberation Front, the ethnic wars in the Ex-Yugoslavia, the conflict over the government in Yemen, among others.

state and non-state conflict events might lead to an increased ethnic pride or group identification and ambiguous effects on return intentions.[25]

Terror events, on the other hand, are only supported by a very small minority of extremists who are unlikely to be captured in survey data. Hence, terror events are more likely to be perceived as generally negative by the whole population.

3.4 Heterogeneous effects

In this subsection, we investigate whether the effect of terrorist events on the intention to remain in Germany varies with the level of integration, visa group, employment status, years since migration, location of family members at the time of the event, and risk aversion. First, we test the hypothesis that the level of integration in Germany mediates the importance of terrorist acts in the home countries in determining the willingness to remain in Germany. If immigrants are highly integrated into German society, they are less likely to pay attention to events occurring in their home countries.

We proxy the level of integration by the self-reported level of oral German and run separate regressions for each level.[26] The coefficients are displayed in Figure 4a and show that for immigrants with a very good level of integration in Germany (i.e., highly integrated in Germany), the effect of terrorist events on the intention to stay is smaller when compared to immigrants with good or poor knowledge of German. This difference is statistically significant at 5% (Table 10.3 in the Appendix).

We also consider how terrorist events in the home country might affect individuals differently depending on the location of their close family members (e.g., parents, spouses, children, grandparents, and siblings). We expect that the return intentions of individuals with close family members abroad are more likely to be affected by events in their home country for two main reasons. First, relatives abroad may act as an information channel about these events. Second, the risk that relatives abroad are directly affected by terror events may increase the salience of these events for immigrants in Germany. If safety conditions in their home country abruptly worsen,

[25] To draw conclusions regarding the effect of state or non-state conflict on long-run return intentions, we would need some information about whether individuals support either side of the parties involved in the conflict - which we do not have in the GSOEP. See Adema et al. (2024) for an example related to the Russian-Ukrainian conflict.

[26] Previous research has shown that linguistic integration strongly correlates with social, political, and economic integration in the host country (Harder et al., 2018).

immigrants in Germany may be not only more likely to delay their return intentions, but may also wish to bring their left-behind family to Germany. Indeed, Figure 4b shows that individuals with close family living abroad are more likely to revise their intentions to remain in Germany than are those with close family living in Germany. This difference is statistically significant at 10% (Table 10.3 in the Appendix).

In Figure 4c, we allocate individuals into groups based on the number of years since arriving in Germany. We can see that the effect is more pronounced among recent arrivals (0-4 years) and immigrants living in Germany for 15 years or more.[27] Nevertheless, these differences are not statistically significant. Figure 4d compares individuals based on their entry visas to Germany. There are no significant differences between individuals who enter Germany as EU nationals,[28] asylum seekers/refugees or members of another group.

Critical to our analysis in Section 4, in Figure 4e, we look at the heterogeneous effects of terrorist events by employment status at the time of the interview. The results show that there is no significant difference between employed and unemployed individuals.

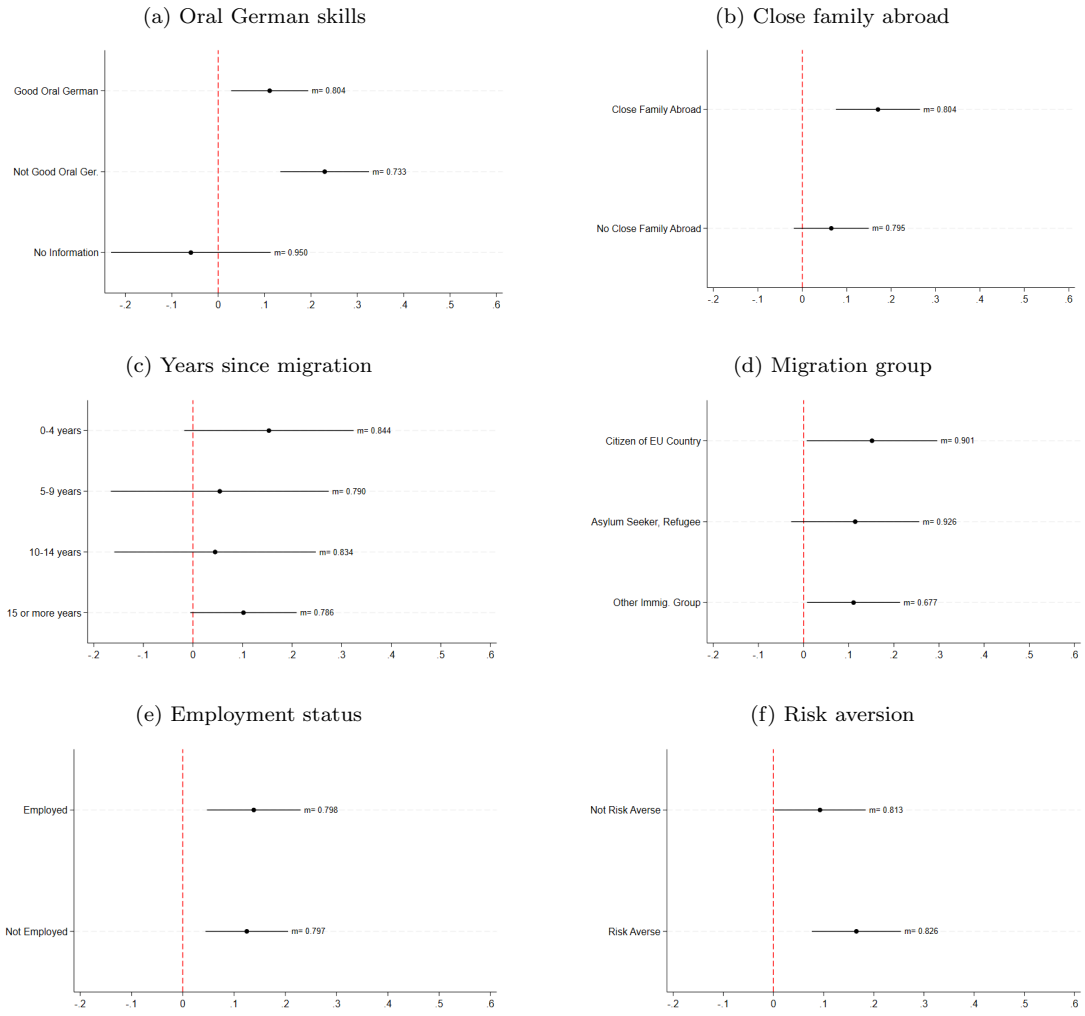
Finally, in Figure 4f, we look at the heterogeneous effects of terror events by risk aversion. Risk-averse individuals are more likely to place greater value on physical security. An increase in the incidence of terror events in the home country, for which the exact location and timing are unpredictable, creates a state of uncertainty and decreases the safety level. Hence, we expect risk-averse individuals to react more to changes in the sociopolitical conditions of their home countries. The results in Figure 4f show this is true even though the difference is not statistically significant at 10%.

In Figure 10.3 in Appendix 10.3, we show the heterogeneous effects by the chosen language of the newspaper read by the respondent (as an alternative measure to oral German), broad region of origin, gender and educational achievement.

[27] These could be individuals who are close to retirement and initially planned to return to their home countries but who have updated their return intentions following a terror event in their country of origin.

[28] We included German descendants from Eastern Europe in this group since there was a low share of individuals entering with this Visa.

Figure 4: Heterogeneity analysis, 90-day bandwidth



Notes: Each panel displays the coefficients from separate estimations of Equation 2 for each level of the variable in the graph title, where the outcome is "Remain in Germany permanently". 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 a 90-day bandwidth. For each level of the variable in the graph title, the mean intention to stay in the control group is displayed next to the coefficient estimate.

4 Unsafe home countries and immigrants' job search behavior

In the previous sections, we have shown that terrorist attacks in home countries increase the desired length of stay in Germany. In this section, we theoretically link these results to the labor market behavior of immigrants and empirically investigate whether terrorist events affect the job search behavior of immigrants in the host labor market.

Previous research has used structural models to show that changes in the intended length of stay among immigrants can affect investments in human capital, which in turn leads to differences in earnings and career profiles (Adda et al., 2022; Bratsberg et al., 2002; Cortes, 2004; Dustmann, 1993, 1999). Ideally, we want to test whether terrorist events affect immigrants' educational and labor market outcomes by changing relative location preferences and intentions to stay in the destination country. However, in a setting where terror events occur repeatedly, it is difficult to isolate the effect of each shock on immigrants' investments in human capital and career profiles. The primary reason is that completing an educational degree or changing one's career profile takes months, if not years, to materialize, and we can only effectively isolate short-term behavioral responses to violence-induced location preference shocks. The second reason is that we want to use an empirical strategy that resembles the one used for the analysis of return intentions described in Section 4.1, so that we can link the effect of terrorist events on return intentions to their effect on labor market behavior. Therefore, we limit the set of outcomes in our analysis. Specifically, we concentrate on immigrants who became unemployed in a month when a relevant terror event occurred in their home country and compare their short-term job search behavior with that of workers from the same origin country who became unemployed only a few months earlier in quiet times.

As a proxy for job search behavior, we use the time elapsed between the last day of employment and the first day of a new employment. This outcome is only available in the German Social Security Data (IEB) database. Therefore, for this analysis, we rely on 40 percent of all immigrants who were working in Germany between 2000 and 2018. Using the detailed information available in the IEB data, we additionally construct indicators that characterize the new job and firm after the period of unemployment. These indicators are the log of daily wage, full-time employment, change in occupation, change in industry, and the characteristics of the new firm.

The a priori effect of a negative event in the home country on immigrants entering unemployment is ambiguous. On the one hand, violence in the home country may shorten the unemployment duration of immigrants in Germany. This could happen if, by lowering the expected utility in the home country, terror events result in

lower reservation wages and greater job search effort in Germany.[29] Similarly, if the families of immigrants in the home country are directly affected by terrorist events, immigrants might also want to reenter employment faster to be able to send money to their relatives.

On the other hand, violence in the home country may increase the unemployment duration of immigrants in Germany. This could happen if terror events make immigrants more likely to pursue a long-term career in Germany. In this case, the present value of a job in Germany increases. Hence, while immigrants might increase their job search effort, they might also become more selective about the type of job they are willing to accept (e.g., more stability, higher future wage growth, better amenities or benefits), leading to an increase in the duration of unemployment. Additionally, the duration of unemployment may increase if violence in the home country decreases immigrants' mental well-being and if mental instability lowers their job search effort. Finally, terrorist events in the home country could also affect unemployment duration through the demand side. If employers discriminate against immigrants coming from "terrorist countries", then the latter will see their pool of job offers shrink and the length of their unemployment increase.[30]

In this study, we argue that one of the main mechanisms through which terror events could affect labor market outcomes is through their impact on immigrants' return intentions. In Section 4.3, we resort to GSOEP responses on well-being and remittances to rule out alternative channels through which terrorist events at the time of origin could affect job search behaviors in the host labor market.

4.1 Empirical strategy

In this section, we detail the empirical strategy that we adopt to investigate the impact of terrorism events on job search and employment outcomes, mirroring as closely as possible the empirical strategy used in Section 3 and summarized in

[29] If terror events affect the perception of security in the home country relative to the host country and lower return intentions, immigrants may reduce their reservation wage to match the wage they would earn back home. Similarly, immigrants' search efforts could increase because of an increased "fear" of having to leave Germany owing to unsustainable economic conditions or visa constraints. The economic conditions of unemployed individuals depend on savings and benefit eligibility, among other factors. The amount and duration of unemployment benefits depend on how long individuals have contributed and the salary they received before becoming unemployed. Furthermore, individuals with mini-jobs are not obliged to contribute to unemployment insurance, and self-employed individuals contribute voluntarily.

[30] Although natives in Germany are likely to be aware of terror events in countries with commercial or historical ties, it is unlikely that they are systematically aware of terror events in all home countries, ranging from Latin America to Asia. In Section 4.2, we provide supporting evidence that this is an unlikely channel

Equation 2.[31]

While we are conceptually using the same identification strategy, a major difference between the two settings is how the outcomes are measured. In Section 3, return intentions are measured on a specific day, while in this section, the time until the first job can span several days. Hence, this approach will require modifying the definition of the outcomes, treatment group, and control group. The treatment group comprises immigrants whose employment spell was terminated in a month when a relevant terror event occurred in their home country (time = 0).[32] We calculate the probability of entering employment within one month (time = 1) or three months (from time =1 to 3).[33] The control group consists of immigrants from the same country of origin who entered unemployment three months before the relevant terror event occurred in their home country (time = −4). Therefore, the relevant terror event (time = 0) does not affect the control group's probability of entering employment within one month (time = −3) and three months (from time = −3 to −1).[34]

We estimate the following model:

$$Y_{i,o,y,m,f} = \alpha + \beta \text{PostTerror}_{i,o,y,m} + \delta X_{i,y} + \rho Z_{i,y} + \mu_{o,y} + \phi_{m,y} + \lambda_f + \epsilon_{i,o,y,m,f} \quad (3)$$

where $Y_{i,o,y,m}$ is the labor market outcome of interest for individual i from country of origin o, entering unemployment in year y and month m, and residing in federal state f. $\text{PostTerror}_{i,o,y,m}$ takes the value of 1 if respondent i from the country of origin o entered unemployment in a month when a relevant terror event occurred in the home country, and 0 if a respondent entered unemployment three months before that same event. $\phi_{m,y}$ represents month times year fixed effects, $\mu_{o,y}$ represents country of origin times year fixed effects, and λ_f represents federal state of residence fixed effects. $X_{i,y}$ is a set of individual controls similar to those used in Section

[31] In Appendix 11 we compare in greater detail the two estimation strategies, highlighting similarities and differences

[32] We choose time = 0 over time = −1 since the job search activity of individuals entering unemployment in a month when a relevant terror event occurs is more "closely affected" by the terror events. On the other hand, the 30-day job search activity among those entering unemployment one month before the relevant terror event is not necessarily affected by the terror event (some will have been unemployed for 30 days when the terror event occurs). In Section 4.2, we show our results are robust to using alternative definitions of the treatment group.

[33] Any outcome measure between time = 0 and time = 3 is unaffected by any other terror event besides the one occurring at time = 0

[34] Note that the control group entered unemployment at time= −4 hence one month since unemployment corresponds to time = −3. Time here is the relative time of the terror event which takes place at time = 0.

3.1 (age, sex, a proxy for years since migration, years since migration squared, and educational achievement). $Z_{i,y}$ is an extended set of individual controls that ensures comparability between unemployed individuals in the treatment and control group. These are measured in the last job before unemployment and include the log of daily wage, the log of firm size, a dummy for full-time employment, a dummy for the share of high school graduates in the firm above the median, a dummy for the share of Germans in the firm above the median, and a dummy for the firm-specific wage premium above the median. The firm-specific wage premium is calculated using the Abowd-Kramarz-Margolis (AKM) model on the full IEB sample and is computed by the statistical division of the IAB (FDZ, see Bellmann et al. (2020)) using the Card et al. (2013) framework.

As with return intentions, including country of origin times year fixed effects ($\mu_{o,y}$) allows us to compare outcomes for immigrants from the same country of origin who entered unemployment in the same year but in different months. The year and month of entry to unemployment fixed effects ($\rho_{y,m}$) allow us to control for month-year events that affect individuals from all origin countries. Standard errors are clustered at the terror event level.

Our identifying assumption is that had the terror event not occurred, the difference in outcomes between unemployed individuals who entered unemployment with and without an event would have been zero. While we cannot directly test this assumption, we run a balance test between these two groups of unemployed individuals, comparing a large set of characteristics at the time of unemployment registration.[35] The results are reported in Figure 12.1 in Appendix 12.1. The only statistically significant difference is in the share of females. However, the size of this difference is extremely small; on average, individuals in the treatment group are 0.4 percentage points more likely to be females than those in the control group.

4.2 Main results

We now turn to our main results on the labor market outcomes of immigrants entering unemployment when a relevant terror event occurs in their home country.

[35]The beginning of an unemployment spell may be both voluntary and involuntary (e.g., following a layoff). The starting time of the unemployment spell is, therefore, non-random. However, for identification, we only need the ending time of the employment spell to be random relative to the occurrence of terror events in the home country.

The results are reported in Table 5 where Columns (1)-(2) and (3)-(4) show the estimated coefficients from Equation 3, using a dummy variable that equals one if an individual entered employment within one month and three months after unemployment, respectively. Columns (1) and (3) use a set of fixed effects and individual controls comparable to those in the return intentions section, and columns (2) and (4) add employment and firm-specific controls measured in the last job before unemployment.[36]

We find that immigrants whose employment spell was terminated in a month when a relevant terror event occurred in the home country are 0.9 and 1.7 percentage points more likely to be employed within one and three months, respectively, than individuals from the same home country whose employment spell ended three months before the relevant terror event.

Table 5: Effects of terror events on unemployed immigrants' outcomes

	Emp. within 1m		**Emp. within 3m**	
	(1)	(2)	(3)	(4)
Post-Terror	0.009**	0.009**	0.017***	0.017***
	(0.005)	(0.005)	(0.005)	(0.005)
Observations	47,305	47,305	47,305	47,305
Mean outcome	0.102	0.102	0.262	0.262
Origin country FE x Year FE	Yes	Yes	Yes	Yes
Month FE x Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Extended controls	No	Yes	No	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 5 reports the coefficients from the estimation of Equation 3 where the outcomes are finding a job within 1 month and 3 months. Post-Terror equals one for individuals entering unemployment in the same month-year when a terror event occurs in the home country, and it equals zero for individuals entering unemployment three months before that terror event. Individual controls: education, age, sex, years since migration, and its square. Extended controls (all measured in the last job before unemployment): ln of the wage, ln of the firm size, a dummy that equals one if full-time employment, and dummy variables for the share Germans in the firm above the median, the share of highly skilled workers in the firm above the median and the firm AKM above the median.

We assess the robustness of these findings following closely the tests performed in Section 3.3 for the analysis on return intentions. Appendix 12.3 displays all results. Overall, our estimated coefficients are robust to changes in the definition of relevant terror events, adding further interactions between country of origin and individual characteristics, excluding specific origin countries and years, and using placebo treatment dates.[37] In line with Section 3.3, we show in Appendix 12.3

[36] These include the ln of the wage, ln of the firm size, a dummy that equals one if full-time employment, and dummy variables for the share of Germans in the firm above the median, the share of highly skilled workers in the firm above the median and the firm AKM above the median.

[37] We do not use the alternative treatment definition (log number of terror attacks in the 90 days before

that, despite the smaller sample size, our main results remain consistent when using relevant armed conflict events.

We also perform additional robustness checks in which we change the definition of the treatment group. In this section, we defined the treatment group as immigrants whose employment spell was terminated in a month when a relevant terror event occurred in their home country. By doing so, we implicitly assumed that job separations in the short run are unaffected by terrorist events. According to German law, the default notice period for resignations is 30 days. Moreover, collective bargaining agreements often designate a longer notice period, and in practice, a three-month notice period is common. Nevertheless, in Table 12.4 in the Appendix, we show that our results are robust to using only individuals whose employment spell was terminated due to a mass layoff and individuals whose employment spell was terminated in the same month as the relevant terror event but before the first terror attack occurs.[38]

We now turn to the type of firms and jobs reported among immigrants who found a job within three months of becoming unemployed. Regarding job characteristics, we focus on four outcomes which reflect the quality of the jobs/matches individuals start. Recall that in all our regressions, we control for the log of daily wage, a dummy for full-time employment, a dummy for the share of high school graduates in the firm above the median, a dummy for the share of Germans in the firm above the median, and a dummy for the firm AKM above the median.

Column (1) in Panel A of Table 6 uses the log of daily wage in the first job after unemployment as an outcome. As we do not observe contractual hours, we cannot compute hourly wages. However, we know whether individuals are engaged in full-time employment. Hence, in Column (2), we use a dummy variable that equals one if an individual obtained a full-time job (FTE) after unemployment. Columns (3) and (4) are dummy variables that equal one if an individual changed occupations or industry between the last job before unemployment and the first job after unemployment, respectively. The results in Panel A of Table 6 show that

unemployment) because it is unsuitable in this setting. If we considered the number of terror attacks in the 90 days before an individual leaves employment, we would be ignoring the occurrence of terror attacks during the job search period. Moreover, a prolonged exposure to violence may directly affect the decision to leave employment.

[38] Recall that we consider a terror event as relevant if there is at least one more terror attack in a given month than the past three-year average. Hence, a month with relevant and isolated terror events can have several attacks occurring on different days of the month.

immigrants whose employment spell was terminated in a month with a relevant terror event and those who find a job within three months have lower daily wages than immigrants who entered unemployment under stable conditions. Additionally, the results in Column (2) of Panel A in Table 6 show that these individuals are less likely to be in full-time employment in their first job after unemployment.

We find no statistically significant difference in the likelihood of changing occupations or industries (Columns (3) and (4)). However, it could be that treated individuals are more (less) likely to change to lower-quality occupations. In Table 12.1 in the Appendix, we analyze if immigrants whose employment spell was terminated in a month with a relevant terror event are more likely to experience an occupational downgrading. We find no evidence of an occupational downgrading among treated individuals.

Even if there are no immediate wage gains, immigrants could switch to companies that offer more stable jobs, better career prospects or better amenities. While we cannot measure all these outcomes directly in the IEB data, we use some proxies. In Column (1) of Panel B in Table 6, we use the log of firm size in the first job after unemployment as an outcome since some larger firms in Germany offer more stable jobs. Column (2) is a dummy variable that equals one if the share of Germans in the firm is above the median, and Column (3) is a dummy variable that equals one if the share of high school graduates in the firm is above the median. In Column (4), we use a dummy that equals one if the firm AKM is above the median.

The results in Panel B of Table 6 show that the immigrants whose employment spell was terminated in a month with a relevant terror event and those who find a job within three months are 3.4 percentage points less likely to change to a firm that has a wage premium above the median. They also seem to be less likely to change to larger firms, although the results are not significant at 10% level. We observe no change in the probability of entering a firm with a high share of German employees and interpret this as suggestive evidence that terror events at origin do not increase discrimination by native employers.

Finally, in Appendix 12.3, we rule out the possibility that changes in safety conditions in the home country affect not only immigrants' job search behavior but also the composition of immigrants who find a job within three months, explaining

the findings on realized wages, job, and firm characteristics. For example, this compositional change may occur if immigrants who would have left Germany within three months from unemployment are now more likely to remain. In Appendix Table 12.6, we provide evidence that immigrants who find a job within three months in the control and treatment group are identical in their baseline characteristics. Moreover, we directly test for changes in actual returns and show in Appendix Table 12.7 that there is no change in the number of immigrants leaving the register *within three months* from a relevant terror event.[39]

Taken together, these results suggest that immigrants who enter unemployment in a month with high levels of violence at home trade immediate job security for lower earnings, unstable jobs, and less-productive firms. Consistent with these findings, in Section 4.3, we show that for unemployed immigrants in the GSOEP, terrorist events in the home country have a negative effect on reservation wages and preferred number of working hours.

Table 6: Terror events and the job and firm characteristics among those employed within 3 months

	Panel A: Job characteristics (Emp. within 3 months)			
	Ln hourly wage	Full-time employ.	Change occup.	Occupational score
	(1)	(2)	(3)	(4)
Post-Terror	-0.095***	-0.051***	0.015	0.039
	(0.023)	(0.014)	(0.025)	(0.034)
Mean outcome	3.819	0.765	0.643	0.644
	Panel B: Firm characteristics (Emp. within 3 months)			
	Ln firm size	Share Germans above med	Share HS above med	AKM above med
	(1)	(2)	(3)	(4)
Post-Terror	-0.069	-0.007	-0.001	-0.034**
	(0.074)	(0.020)	(0.020)	(0.014)
Mean outcome	0.288	0.283	0.233	0.309
Observations	12,670	12,670	12,670	12,670
Origin c. FE x Year FE	Yes	Yes	Yes	Yes
Month FE x Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Extended controls	Yes	Yes	Yes	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 6 reports the coefficients from the estimation of Equation 1 where the outcomes are job (Panel A) and firm (Panel B) characteristics. Post-Terror equals one for individuals entering unemployment in the same month-year when a terror event occurs in the home country, and it equals zero for individuals entering unemployment three months before that terror event. Individual controls: education, age, sex, years since migration, and its square. Extended controls (all measured in the last job before unemployment): ln of the wage, ln of the firm size, a dummy that equals one if full-time employment, and dummy variables for the share Germans in the firm above the median, the share of highly skilled workers in the firm above the median and the firm AKM above the median.

[39]For each month-country of origin combination (around relevant terror events), we compute the log number of leavers and the share of leavers relative to the full population that appears in the register in the given month. We then run a regression using a regression model based on Equation 1.

4.3 Mediating channels

In Section 4, we argued that changes in return intentions could affect immigrants' job search activity, reservation wage, and preferred job types. Because of these changes in behavior, we expected to find differences in measurable labor market outcomes between immigrants entering unemployment when a terror event occurred in their home country and immigrants entering unemployment under stable conditions. In this section, we test for this behavioral mediating channel as well as alternative channels that could link the return intentions findings of Section 3.2 to the labor market findings of Section 4.2.

In Table 7, we analyze changes in the self-stated job search activity measures among GSOEP respondents who were unemployed at the time of the survey. For the outcome in Column (1), we rely on the question which asks, "Have you actively looked for work within the last four weeks?" and exclude the individuals who were interviewed in the first month after the terror event since for this group their job search period mostly refers to the pre-terror event period. In Columns (2) and (3), we create two dummy variables that equal one if individuals replied "absolutely sure" or "as soon as possible" to the questions "Do you intend to obtain employment in the future?" and "When, approximately, would you like to start working?". We rely on a question that asks respondents "What would your net income have to be for you to accept a position?" to calculate their reservation wage (Column (5)) and "How many hours per week would you have to work to earn this net income?" to deduce their preferred number of working hours (Column (4)). The sample size is considerably smaller and varies across outcomes since these questions are not consistently asked in each survey wave (particularly the reservation wage) and we are restricted to individuals who were unemployed at the time of the survey.

Table 7: Terror events, 90-day bandwidth, job search activity among the unemployed

Job search	Higher than average of last 3 years					
	Active in last 4 weeks	Plan to find job	Find job asap	Preferred nr hours	Ln monthly res. wage	Ln hourly res. wage
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Terror	0.179**	0.169***	0.067	-4.065	-0.289**	-0.179**
	(0.083)	(0.048)	(0.078)	(3.484)	(0.134)	(0.087)
Observations	1134	2797	1402	482	575	482
Mean dep. variable	0.373	0.338	0.595	31.858	6.953	2.195
Origin c. FE x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State of residency FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 7 displays the coefficients from the estimation of Equation 2 where the outcome is actively searched for a job in the past 4 weeks (0-1) in Column (1), plan to find a job (0-1) in Column (2), find a job as soon as possible (0-1) in Column (3), preferred number of working hours in Column (4), ln of the monthly reservation wage (euros) in Column (5) and the ln the monthly reservation wage divided by the number of preferred working hours in Column (6). All results consider a 90-day bandwidth. Individual controls include age, sex, years since migration and its square, marital status, educational achievement, and children.

The results in Table 7 show that terrorist events affect the job search activity, intention to find a job, and reservation wages of GSOEP respondents who were unemployed at the time of the survey. The negative effect of relevant terror events on the reservation wage might be driven by the fact that immigrants "anchor" their reservation wage in Germany to the wage below which they would prefer to return to their home country.

We also find suggestive evidence that unemployed immigrants who are interviewed after a terror event increase their intentions to find a job and their job search activity. Immigrants' search efforts could increase because of an increased "fear" of having to leave Germany owing to unsustainable economic conditions or visa constraints, for instance.

Alternative mediating channels, such as a deterioration in mental health or an increased need for financial resources, could also affect immigrants' labor market outcomes following terrorist events. For example, if immigrants' families residing in the home country are directly affected by terrorist events, immigrants in Germany may want to reenter employment faster to be able to send money to their relatives. Alternatively, terror events could affect the mental health of immigrants, such that they find it difficult to search for jobs. Since we have no information on health status or remittances in the administrative data, we rely on the GSOEP survey data. We proxy remittance behavior using information on whether immigrants send money

abroad and mental health using self-reported health satisfaction (1-10 scale).

The results are shown in Table 8. Terror events have no statistically significant effect on self-reported health satisfaction or on sending money abroad.[40]

Table 8: Terror events, 90-day bandwidth, other mediating channels

	Higher than average of last 3 years	
	Send money abroad	Self-rated health satisfaction
	(1)	(2)
Post-Terror	-0.039	-0.041
	(0.028)	(0.180)
Observations	6555	6489
Mean dep. variable	0.098	6.766
Origin country FE x Year FE	Yes	Yes
Month FE x Year FE	Yes	Yes
State of residency FE	Yes	Yes
Individual controls	Yes	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 8 displays the coefficients from the estimation of Equation 2 where the outcome is "Send money abroad" (0-1) in Column (1), "Satisfaction with health" (1-5) in Column (2). All results consider a 90-day bandwidth. Individual controls include age, sex, years since migration and its square, marital status, educational achievement and children.

Together with the findings in Table 6, the results in this section suggest that shorter unemployment spells following terror events are related to an increase in job search effort and a higher willingness to accept lower wages and contractual hours. These changes in job search materialize into employment spells with lower accepted wages and more part-time contracts, in less productive firms.[41]

5 Conclusions

Using plausibly exogenous shocks to safety conditions in the home country, this paper provides novel evidence on the determinants of return migration and immigrants' economic behavior in the host country. More specifically, this study exploits the quasi-random occurrence of terror events in the home country relative to the timing of survey interviews and job separations to estimate the effect of these violent events on the immigrants' decision to remain permanently in Germany and job search

[40] The negative effect on remittances could be driven by the fact that after a terror attack, immigrants perceive their home country as being more financially insecure or that they expect the terror attacks to affect the financial markets. Nevertheless, it is not statistically significant, and hence it is unlikely that the results in Section 4 are affected by the negative effect on remittances.

[41] We can not entirely rule out the possibility that employers exploit the situation in the migrant's home country to increase their bargaining power and offer lower wages. However, since treated immigrants have lower reservation wages, shorter unemployment durations, and find work in less productive firms, it is unlikely that lower offered wages is the main channel.

behavior.

We show that terror events in the home country affect immigrants' return plans by increasing the probability of staying abroad permanently. These effects are stronger for immigrants who are poorly integrated into the host society and have core family ties at the origin. The same violent events nearly simultaneously affect the job search behavior of unemployed immigrants. We find that immigrants whose employment spell ends when a terror event occurs in their home country stay unemployed for a shorter period of time compared to those who enter unemployment in stable times.

While these effects may benefit immigrants' employment outcomes in the short term, the long-term consequences of such behavioral responses may be negative as we find that the same immigrants are more likely to enter lower-paying jobs and less productive firms.

As immigrants' length of stay and employment outcomes abroad have economic consequences for the home and the host country (Barsbai et al., 2017; Colas and Sachs, 2023; Dustmann and Frattini, 2014; Foged and Peri, 2016; Mobarak et al., 2023), we believe that the results in this paper can contribute to a better definition of integration and migration policies in response to upsurging conflicts and violence worldwide.

Nevertheless, we reckon that our analysis relies on specific periods where there was an isolated, and therefore relatively unexpected, terror event. With our definition of relevant and isolated terror events, we are capturing an unexpected shock to the perception of security, and hence, we are likely considering only very salient terror events. Our empirical setup does not allow us to assess the effect of prolonged and consecutive terror attacks on return intentions.

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6 Immigrants in Germany

The current immigrant population in Germany essentially reflects three large immigration waves. The first wave started in the mid-1950s when, due to 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.

Table 6.1 shows 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 immigrants in the sample represents well the different migration waves.[42]

[42] We discuss the migration samples within the GSOEP in the Appendix 6.1

Table 6.1: Largest migrant groups in the GSOEP data in %

	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 6.1 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.

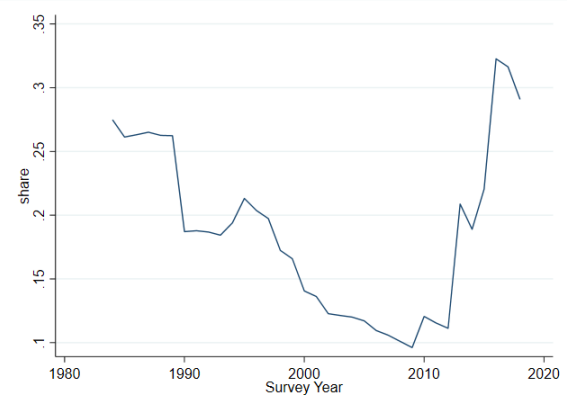
6.1 Immigrants in the GSOEP

Figure 6.1 shows the share of immigrants in the GSOEP sample. When the survey started in 1984, immigrants 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 immigrants fell until 1994 when a boost sample (D1 and D2) of immigrants who came to Germany after 1984 was added to consider the flow of ethnic Germans from the former Soviet countries. After the boost sample was added in 1994-95 the share of immigrants in the GSOEP fell steadily. To improve the representation of immigrants 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.[43] 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

[43]Sample M1 was added in 2013 with around 2,700 households and includes individuals who immigrated to Germany after 1995 or second-generation immigrants. Sample M2 was added in 2015 with around 1,100 households, including 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.

of the federal states.[44]

Figure 6.1: Share of immigrants in the GSOEP



Notes: Figure 6.1 displays the immigrants' share in the GSOEP respondents sample in each survey wave. The y-axis refers to the share. The time window is 1984-2019.

6.2 Summary characteristics in the GSOEP

Table 6.2 below shows the summary characteristics of the migrant population in the GSOEP data.

Table 6.2: Summary characteristics of the migrant population in the GSOEP data

	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

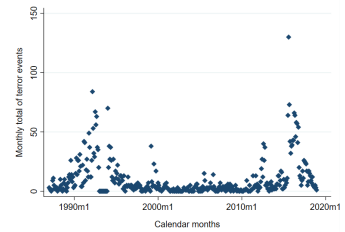
Notes: Table 6.2 reports the main characteristics of the full sample of immigrants in the GSOEP data (2000-2018). We report the mean, standard deviation, and median values for each variable. The last row reports the total number of immigrants.

[44]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 (GSOEP).

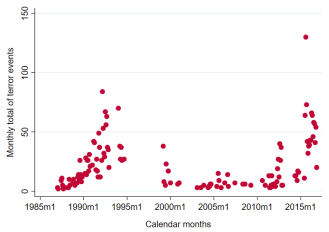
7 GTD Tables and Figures

Figure 7.1: All terror events and relevant terror events (higher than average of last 3 years)

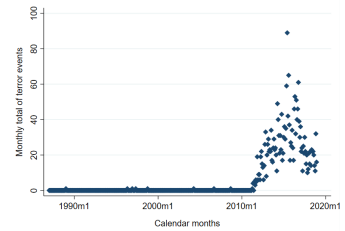
(a) Turkey: all terror events



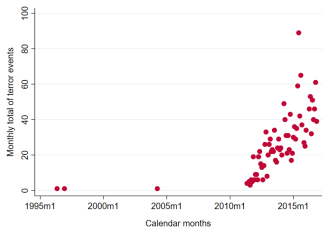
(b) Turkey: relevant terror events



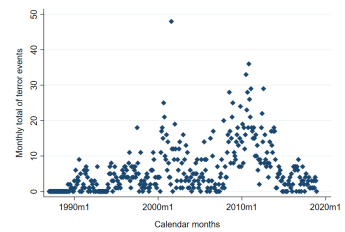
(c) Syria: all terror events



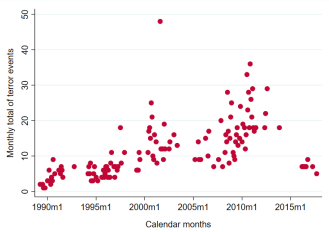
(d) Syria: relevant terror events



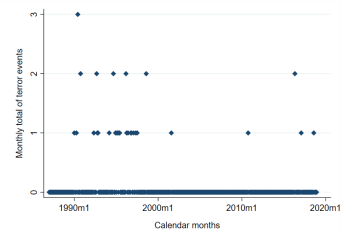
(e) Russia: all terror events



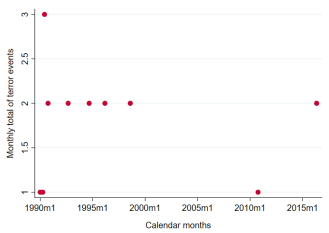
(f) Russia: relevant terror events



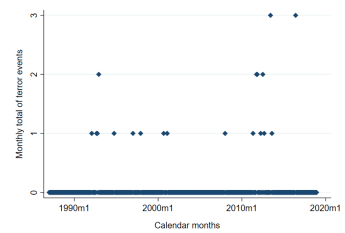
(g) Poland: all terror events



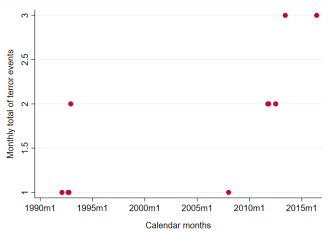
(h) Poland: relevant terror events



(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 month, there is at least one more terror attack than the past country-specific 3-year monthly average number.

Table 7.1: Effective sample: Isolated and relevant terror events

	Higher than average past 3 years, 90 days bandwidth	
	Number of rel. & isol. events	Mean number of monthly 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 7.1 reports the isolated and relevant events merged with the GSOEP. An event is defined as relevant if, in a given month, there is at least one 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 days.

8 UCDP Tables

Table 8.1: Effective sample: Isolated and relevant armed conflict events

	Higher than average past 3 years, 90 days bandwidth	
	Number of rel. & isol. events	Mean number of monthly armed conflicts
Armenia	1	3
Azerbaijan	2	4
Belgium	2	2
Brazil	1	2
Chile	1	10
Columbia	2	9
Eritrea	1	1
Ethiopia	1	9
France	1	1
Iran	3	5
Mexico	1	21
Pakistan	2	81
Peru	2	3
Philippines	2	19
Russia	2	13
Senegal	1	1
South Africa	1	2
Sri Lanka	1	1
Tajikistan	1	1
Thailand	2	17
Tunisia	2	4
USA	1	3
Ukraine	1	16
Spain	1	4
Turkey	2	12
Observations	25	

Notes: Table 8.1 reports the isolated and relevant armed conflict events merged with the GSOEP. An event is defined as relevant if, in a given month, there is at least one more armed conflict episode 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 armed conflict event have not experienced any relevant armed conflict event in the past 90 days.

9 Definition of a relevant and isolated terror event, treatment and control groups

9.1 Relevant and isolated terror event

A terror event is *relevant* if there is at least one more terror attack in a given month than the past three-year average. Formally, we define a *relevant* terror event as follows:

$$T^{\mathrm{R}}_{o,y,m} = \begin{cases} 1, & \text{if } T_{o,y,m} > \tau^{\mathrm{3yr}}_{o,y,m} \\ 0, & \text{otherwise} \end{cases} \tag{4}$$

where $T_{o,y,m}$ is the number of terrorist attacks in month m, year y, and country of origin o and $\tau^{\mathrm{3yr}}_{o,y,m} = \frac{1}{3}\sum_{j=-3}^{-1} T_{o,y+j,m-1}$ is the average of monthly terrorist attacks in the previous three years, starting from month $m-1$.[45] Alternatively, we use the past four-year and five-year averages.

A relevant terror event is *isolated* if no other relevant events occurred in the previous 3 months or in the following 3 months. Formally:

[45] $\tau^{\mathrm{3yr}}_{o,y,m}$ is rounded to have no decimal places, such that if $\tau^{\mathrm{3yr}}_{o,y,m} = 0$ for an event to be relevant $T_{o,y,m} \geq 1$

$$T^{\mathrm{R,I}}_{o,y,m} = \begin{cases} 1, & \text{if } \sum_{k=1}^{3} T^{\mathrm{R}}_{o,y,m+k} + \sum_{k=-3}^{-1} T^{\mathrm{R}}_{o,y,m+k} = 0 \\ 0, & \text{if } \sum_{k=1}^{3} T^{\mathrm{R}}_{o,y,m+k} + \sum_{k=-3}^{-1} T^{\mathrm{R}}_{o,y,m+k} > 0 \end{cases} \tag{5}$$

Alternatively, we use smaller bandwidths, such as 1 and 2 months.

9.2 Treatment and control groups

Define $D^{\mathrm{R,I}}_{o,y,m,1}$ as the first day of month m and year y in which $T^{\mathrm{R,I}}_{o,y,m} = 1$ and $D^{\mathrm{R,I}}_{o,y,m,\overline{d}}$ as the last day ($\overline{d} = 28, 29, 30, 31$) of month m and year y in which $T^{\mathrm{R,I}}_{o,y,m} = 1$ for country o. $V_{i,o,y,m,d}$ is the day in which individual i from country o is interviewed.

Respondent i from the country of origin o is assigned to the treatment group if he or she is interviewed within 90 days after a relevant and isolated terror event and to the control groups if he or she is interviewed within 90 days before that same event.

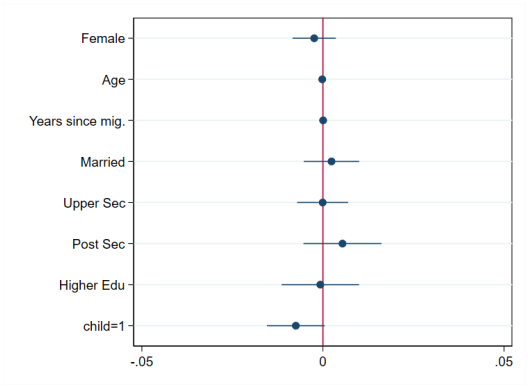
$$PostTerror_{i,o,y,m} = \begin{cases} 1, & \text{if } 0 < V_{i,o,y,m,d} - D^{\mathrm{R,I}}_{o,y,m,31} \leq 90 \\ 0, & \text{if } -90 \leq V_{i,o,y,m,d} - D^{\mathrm{R,I}}_{o,y,m,1} < 0 \end{cases} \tag{6}$$

10 GSOEP additional Tables and Figures

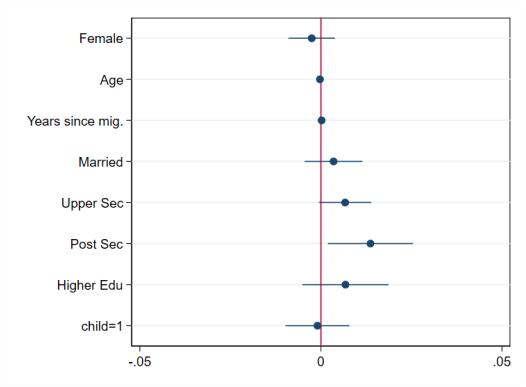
10.1 GSOEP joint balance tests and density test

Figure 10.1: GSOEP: Joint balance test, 90-day bandwidth

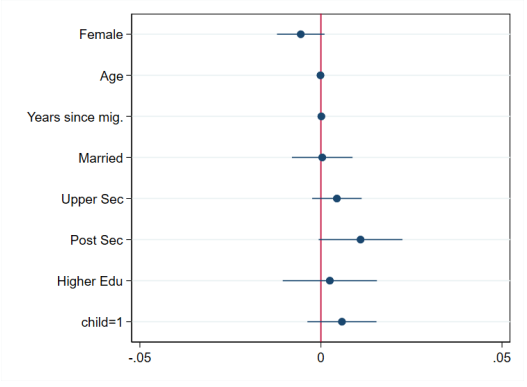
(a) Deviation to 3 year mean



(b) Deviation to 4 year mean

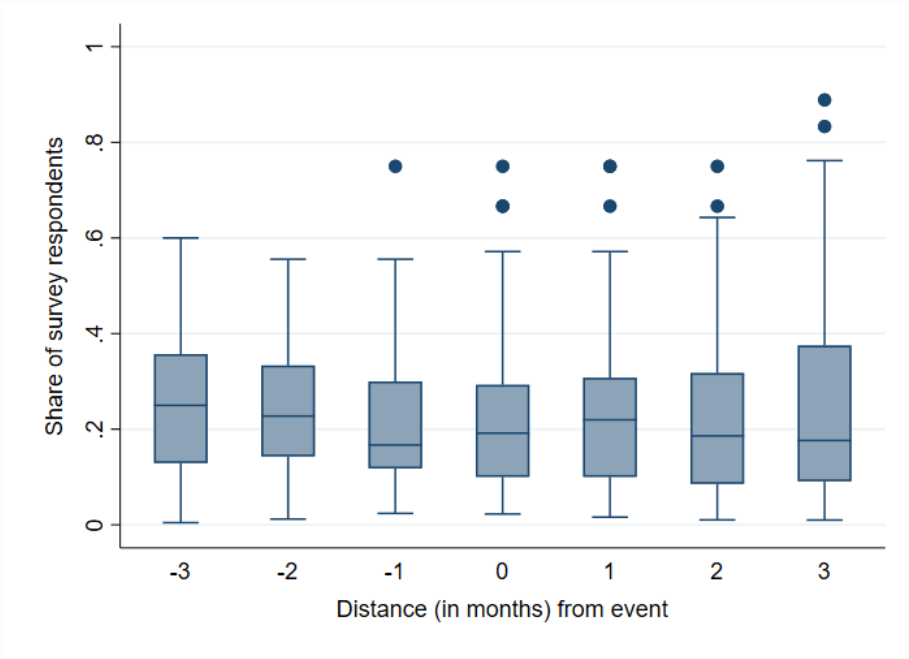


(c) Deviation to 5 year mean



Notes: Figure 10.1 displays the coefficients from regressing all the individual controls and fixed effects from Equation 2 on the treatment status. In Panel 10.1a, an event is considered to be relevant if the number of terror events in a given month is above the past 3-year mean of monthly terror events. Similarly, Panel 10.1b considers the past 4-year mean and Panel10.1c the past 5-year mean. Standard Errors in parenthesis clustered at the event level. Bars identify 95% confidence intervals.

Figure 10.2: GSOEP: Density of interviews around terror events, 90-day bandwidth



Notes: Figure 10.2 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, and 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.

10.2 GSOEP and political stability

Table 10.1: Distribution of the political stability index and mean month terror events

	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
MM terror prev. year	19.030	0.000	0.000	0.167	0.500	3.583	12.583	281.917
MM 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. MM refers to the mean number of terror attacks in one month

10.2.1 Alternative measure of political stability

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. We also use the mean monthly number of terror attacks in the past year to compare it with Table 3. 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.[46] The results are shown in Table 10.2. 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.

[46]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 10.1

Table 10.2: Overall political stability, terror events and intentions to remain in Germany, 90-day bandwidth

	Mean monthly terror	
	Previous year	Mean previous 3 years
	(1)	(2)
Post-Terror × >12 attacks month	0.190***	0.186***
	(0.036)	(0.037)
Post-Terror × [0-12] attacks month	0.115***	0.106***
	(0.032)	(0.031)
Post-Terror × 0 attacks month	0.110*	0.138**
	(0.063)	(0.059)
Observations	6604	6604
Origin country FE x Year FE	Yes	Yes
Month FE x Year FE	Yes	Yes
State of residency FE	Yes	Yes
Individual controls	Yes	Yes

Standard Errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 10.2 displays the coefficients from the estimation of Equation 2 interacted with a dummy variable proxing for political stability. All results use a 90-day bandwidth. FE refers to fixed effects. Individual controls include age, gender, years since migration and its square, marital status, educational achievement, and children.

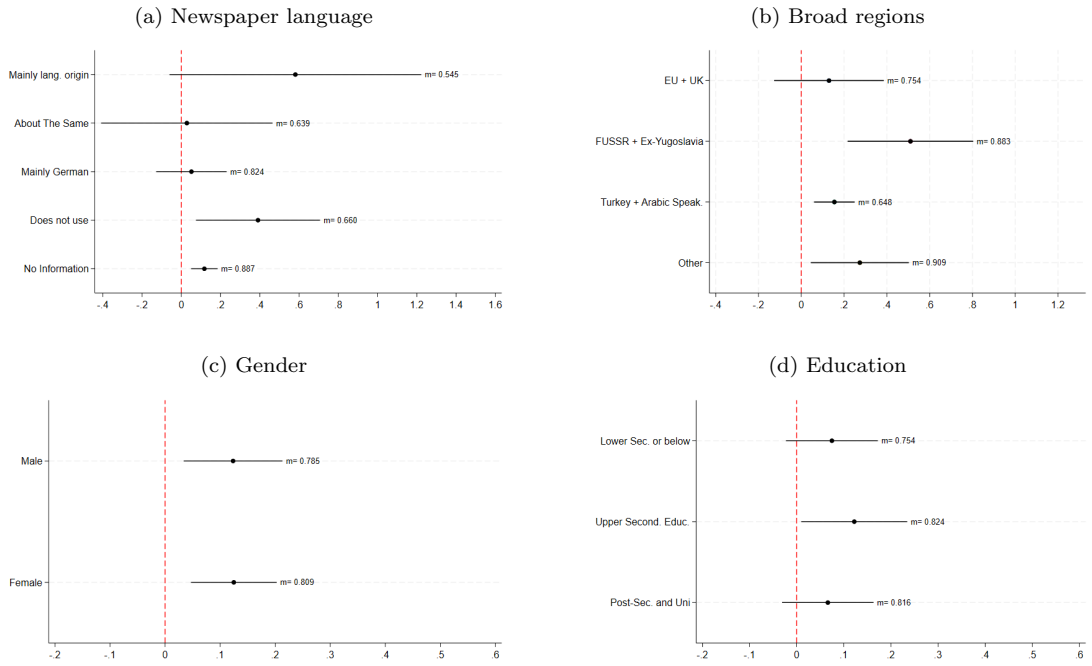
10.3 GSOEP extra heterogeneous effects

As an alternative proxy to the level of integration, we look at the language of the newspaper read by the respondent. Figure 10.3a shows that individuals who read newspapers in mainly the language of their country of origin are slightly more likely to update their return intentions following a terror event in their home country. although this difference is not significant at 10%.

In Figure 10.3b, we group individuals into broad regions of origin. The effect of terrorist events on return intentions is greater for individuals coming from the former USSR and ex-Yugoslavian areas, although the standard errors are also considerably larger.

In Figures 10.3c and 10.3d we look at heterogeneous effects by gender and education group. The update in return intentions following a terror event is relatively similar within these groups.

Figure 10.3: Heterogeneity analysis, 90-day bandwidth



Notes: Each panel displays the coefficients from separate estimations of Equation 2 for each level of the variable in the graph title, where the outcome is "Remain in Germany permanently". 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 a 90-day bandwidth. For each level of the variable in the graph title, the mean intention to stay in the control group is displayed next to the coefficient estimate.

Table 10.3: Testing differences in the heterogeneity analysis, 90-day bandwidth

	Close Family Abroad	Employed	Risk Averse	Female
F-stat	2.758	0.080	1.641	0.001
P-value	0.097	0.778	0.201	0.982

	Oral German	Migration Group	Education
F-stat 1	3.974	0.138	0.619
P-value 1	0.049	0.711	0.432
F-stat 2	2.135	0.280	0.041
P-value 2	0.147	0.597	0.839

	Broad Regions	Years Since Migration	Newspaper Language
F-stat 1	3.779	0.648	2.122
P-value 1	0.052	0.421	0.146
F-stat 2	0.051	0.832	2.332
P-value 2	0.821	0.362	0.127
F-stat 3	0.734	0.245	0.239
P-value 3	0.392	0.621	0.625
F-stat 4			1.983
P-value 4			0.160

Standard Errors in parenthesis clustered at terror event level, *p<.1; **p<.05; ***p<.01
Notes: Table 10.4 displays the coefficients from the estimation of Equation 2 where the outcome is "Remain in Germany permanently". The F-stats and p-values correspond to the heterogeneous effects presented in Figures 4 and 10.3. F-stat 1 refers to the difference between the baseline category (e.g., category 0) and category 1. Similarly, F-stat 2,3 and 4 refer to the difference between the baseline category (e.g., category 0) and category 2, 3, and 4 correspondingly.

10.4 GSOEP robustness checks

Changing bandwidth or reference point Columns (1)-(3) of Table 10.4 show the equivalent results to Column (1) in Table 1 when varying the reference point and/or the bandwidth and Columns (4)-(6) to Column (4) in Table 1.

Table 10.4: Robustness GSOEP: terror events and intentions to remain in Germany using different bandwidths and reference points

	Higher than average of last			Higher than average of last		
	5 years	4 years	3 years	5 years	4 years	3 years
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 30-day Bandwidth						
Post-Terror	0.328***	0.322***	0.329***	0.322***	0.321***	0.323***
	(0.052)	(0.055)	(0.054)	(0.047)	(0.052)	(0.049)
Observations	1915	2056	2671	1915	2056	2671
Panel B: 60-day Bandwidth						
Post-Terror	0.147**	0.202***	0.112***	0.144**	0.208***	0.120***
	(0.064)	(0.044)	(0.028)	(0.067)	(0.043)	(0.028)
Observations	3712	4078	4886	3712	4078	4886
Panel C: 90-day Bandwidth						
Post-Terro	0.074*	0.083**	0.122***	0.068*	0.080**	0.123***
	(0.041)	(0.039)	(0.029)	(0.041)	(0.037)	(0.029)
Observations	5328	5790	6604	5328	5790	6604
Origin country x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State of residency FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	No	No	Yes	Yes	Yes

Standard Errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 10.4 displays the coefficients from the estimation of Equation 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.

Additional controls and different clustering Table 10.5 shows the results when adding country of origin times years since migration (Column (2), country of origin times age (Column (3)) and country of origin times gender (Column (4)) to the baseline specification (Column (1)). Table 10.6 shows the baseline specification when using different standard error clusterings.

Table 10.5: Additional controls: Terror events and intentions to remain in Germany, 90-day bandwidth

	Higher than average of last 3 years			
	(1)	(2)	(3)	(4)
Post-Terror	0.123***	0.153***	0.125***	0.131***
	(0.029)	(0.039)	(0.036)	(0.029)
Observations	6604	6551	6533	6596
Mean intention to stay	0.798	0.798	0.798	0.798
Origin country FE x Year FE	Yes	Yes	Yes	Yes
Year FE x Month FE,	Yes	Yes	Yes	Yes
State of Residency FE	Yes	Yes	Yes	Yes
Origin country FE x YSM G. FE	No	Yes	No	No
Origin country FE x Age G. FE	No	No	Yes	No
Origin country FE x Gender FE	No	No	No	Yes
Individual Controls	Yes	Yes	Yes	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 10.5 displays the coefficients from the estimation of Equation 2 where the outcome is "Remain permanently in Germany". All the results use a 90-day bandwidth. Column (1) is the preferred specification shown in Column (4) of Table 1; Column (2) adds country of origin times years since migration (grouped) to the controls in Column (1); Column (3) adds country of origin times age groups to the controls in Column (1); and Column (4) adds country of origin times gender to the controls in Column (1). Individual controls include age, sex, years since migration and its square, marital status, educational achievement, and children.

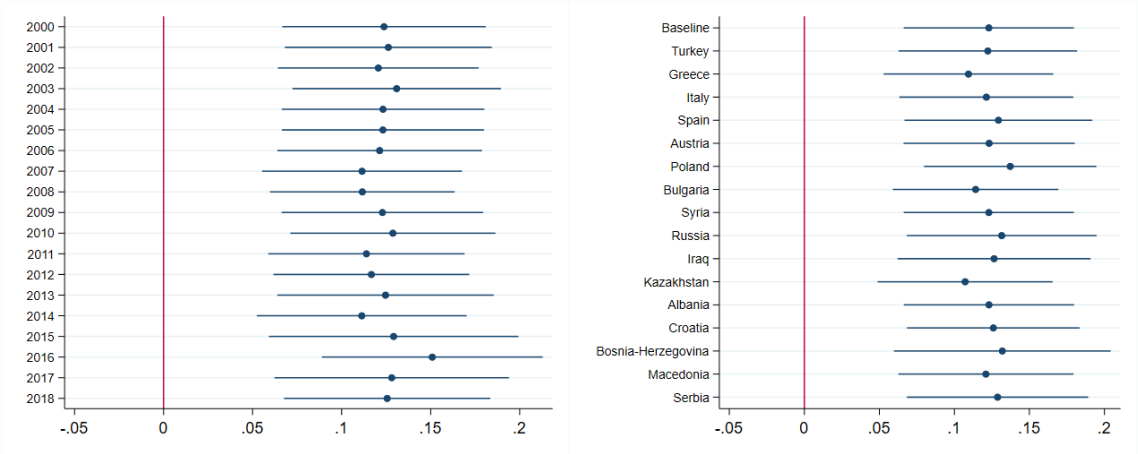
Table 10.6: Different SE clustering: Terror events and intentions to remain in Germany, 90-day bandwidth

	Standard errors clustered at:		
	Event level	Country of Origin	Country O. x Year
	(1)	(2)	(3)
Post-Terror	0.123***	0.123***	0.123***
	(0.029)	(0.023)	(0.031)
Observations	6604	6604	6604
Mean intention to stay	0.798	0.798	0.798
Origin country FE x Year FE	Yes	Yes	Yes
Year FE x Month FE,	Yes	Yes	Yes
State of Residency FE	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes

*p<.1; **p<.05; ***p<.01
Notes: Table 1 displays the coefficients from the estimation of Equation 2 where the outcome is "Remain permanently in Germany". All the results use a 90-day bandwidth. Individual controls include age, sex, years since migration and its square, marital status, educational achievement, and children. Column (1) is the preferred specification shown in Column (4) of Table 1 where the standard errors are clustered at the terror event level. In Column (2), the standard errors are clustered at the country of origin level, and in Column (3), at the year-country of origin level.

Excluding a year or a country Figure 10.4 excludes one survey year at a time from the baseline specification, and Figure 10.4 excludes one country of origin at a time from the baseline specification.

Figure 10.4: Robustness GSOEP: exclude one country and survey year at the time, 90-day bandwidth



a) Exclude one survey year, 90-day bandwidth

b) Exclude one country, 90-day bandwidth

Notes: Panel a) and b) display point estimates and 95% confidence intervals for regressions excluding one survey year and country at a time, respectively, where the outcome is "Remain permanently in Germany". 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. Standard Errors in parenthesis clustered at the event level. 90-day bandwidth

Different treatment event definition Table 10.7 shows of estimating equation 2 using two alternative treatments: i) whether or not there was any terror attack in the 90 days before the interview, and ii) the ln number of terror attacks in the 90 days before the interview. The interpretation of the results when using these treatments is slightly different since we are not necessarily capturing a shock to the perception of security. For instance, consider Iraq, a country experiencing several terror attacks in 2000-18 and for which a terror event is considered relevant if there were more than 285 terror attacks in a given month. With the simpler treatments, we are also considering as treated individuals interviewed during a relatively stable period in Iraq but where there is still one terror attack.

Table 10.7: Terror events and intentions to remain in Germany, 90-day bandwidth

	(1)	(2)
At least 1 terror attack, past 90d	0.014**	
	(0.006)	
ln number terror attacks, past 90d		0.011**
		(0.005)
Observations	70736	70736
Mean intention to stay	0.861	0.838
Origin country FE x Year FE	Yes	Yes
Year FE x Month FE,	Yes	Yes
State of Residency FE	Yes	Yes
Individual Controls	Yes	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 10.7 displays the coefficients from the estimation of Equation 2 where the outcome is "Remain permanently in Germany". All the results use a 90-day bandwidth. Individual controls include age, sex, years since migration and its square, marital status, educational achievement, and children.

Using another type of violent event To create a measure of armed conflict events, we use the armed conflict data from the Uppsala Conflict Data Program (UCDP) Event Dataset, which includes events on state conflict,[47] non-state conflict and one-sided violence across several countries of origin. UCDP defines an event as: "An incident where armed force was used by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death at a specific location and a specific date" (Sundberg and Melander, 2013).[48]

Using the UCDP data on armed conflict, we constructed a measure similar to the relevant and isolated terror events.[49] Table 8.1 shows the number of relevant and isolated armed conflict events per country and the mean number of monthly armed conflict episodes per relevant and isolated terror event. The number of relevant and isolated armed conflict events is, as expected, much smaller than the number of terror events, and hence our sample size drops by 77% to only 1505 observations.

The results using relevant and isolated armed conflict events as a shock to the perception of security are displayed in Table 10.8 and are broadly in line with the findings in Table 1 when using terror events.

[47] UCDP defines armed conflict as a "contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths in one calendar year.

[48] An alternative data-source for political violence would be the ACLED, however, the historical data coverage is focused on African countries. ACLED only expanded to the Middle East in 2016, to Asia and Latin America in around 2018, and to Europe, North America, and Oceania in 2020. Previous research has used both ACLED and UCDP data to capture the occurrence and intensity of conflict (Harari and Ferrara, 2018; McGuirk and Nunn, 2024).

[49] We count the number of armed conflict episodes and consider a violent event to be relevant if, in a given month-year-country of origin, the number of violent event episodes is above the past three-year average

Table 10.8: Armed conflict events and intentions to remain in Germany, 90-day bandwidth

	Higher than average of last 3 years			
	(1)	(2)	(3)	(4)
Post-Conflict	0.140**	0.115**	0.114*	0.106*
	(0.058)	(0.056)	(0.057)	(0.062)
Observations	1505	1505	1505	1505
Mean intention to stay	0.785	0.785	0.785	0.785
Origin country FE x Year FE	Yes	Yes	Yes	Yes
Year FE x Month FE,	Yes	Yes	Yes	Yes
State of Residency FE	Yes	Yes	Yes	Yes
Individual Controls	No	Some	Some	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 10.8 displays the coefficients from the estimation of Equation 2 where the outcome is "Remain permanently in Germany". All the results use a 90-day bandwidth. Column (1) uses only the baseline fixed effects; Column (2) adds gender, age, years since migration, and years since migration squared to the controls in Column (1); Column (3) adds marital status and the presence of children to the controls in Column (2); and Column (4) adds educational achievement to the controls in Column (3).

11 IEB empirical strategy

In Section 3.1, we defined the control group as those interviewed in the three months preceding a relevant terror event (between time $= -3$ and time $= -1$), conditional on not having experienced any other terror event in the past three months.[50] If we were to look at the time to employment among individuals entering unemployment one month before the focal terror event (time $= -1$), we would potentially be absorbing the effect of the terror event. If these individuals do not find a job within 1-30 days, the terror event (time $= 0$) will affect their job search activity. Similarly, in Section 3.1, we defined the treatment group as those individuals interviewed in the three months after a relevant terror event (between time $= 1$ and time $= 3$), with the requirement that in those three months, there is no other terror event besides the focal terror event. However, our design doesn't preclude the occurrence of another terror event three months after the focal terror event (time > 3). Hence, if individuals entering unemployment two months after a relevant terror event (time $= 2$) do not find a job within 1-30 days (until time $= 3$), another terror event may affect their job search activity.

To overcome these issues, we define the outcomes, treatment group, and control group in the following way. The treatment group comprises immigrants entering unemployment in a month when a relevant terror event occurs in their home country (time $= 0$). We calculate the probability of entering employment within one month

[50] In Section 3.1, we considered different bandwidths, 30, 60, or 90 days. For most of our analysis, we used 90 days, corresponding to three months.

(time = 1) and three months (from time = 1 to 3).[51] The control group consists of immigrants from the same country of origin who entered unemployment three months before the relevant terror event occurred in their home country (time = −4).[52] Therefore, the relevant terror event (time = 0) does not affect the control group's probability of entering employment within one month (time = −3) and three months (from time = −3 to −1).[53]

12 IEB additional Tables and Figures

12.1 IEB joint balance tests

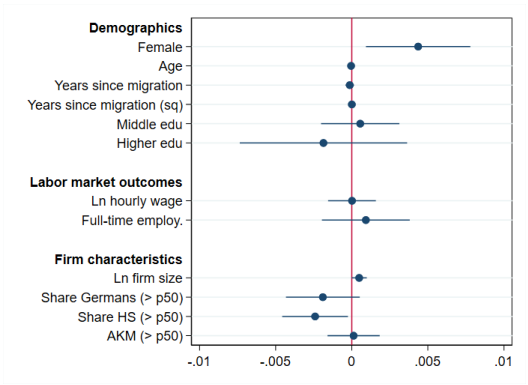
[51] Any outcome measure between time = 0 and time = 3 is unaffected by any other terror event besides the focal one.

[52] The way we defined a relevant terror event in Section 2 ensures that individuals interviewed three months before a relevant terror event have not experienced any other relevant terror event in the past three months. Hence, between time = −6 and time = −4, there was no other relevant terror event.

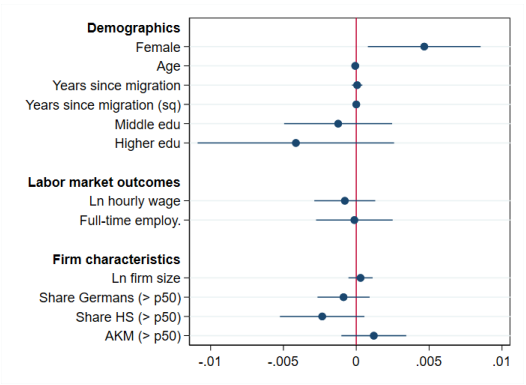
[53] Note that the control group entered unemployment at time= −4 hence one month since unemployment corresponds to time = −3. Time here is the relative time of the terror event that takes place at = 0.

Figure 12.1: IEB: Joint balance test

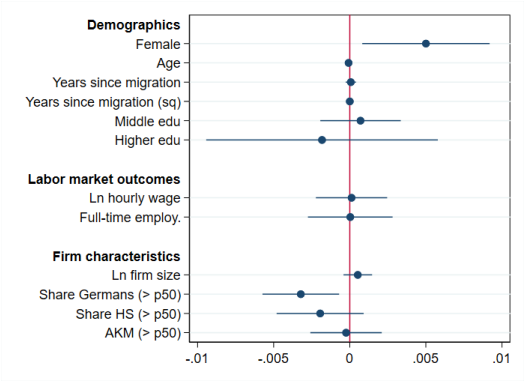
(a) Deviation to 3 year mean



(b) Deviation to 4 year mean



(c) Deviation to 5 year mean



Notes: Panel 12.1a displays the coefficients from regressing the treatment status on all the individual controls and fixed effects as in Equation 3. In Panel 12.1a, an event is considered to be relevant if the number of terror events in a given month is above the past 3-year mean of monthly terror events. Similarly, Panel 12.1b considers the past 4-year mean and Panel12.1c the past 5-year mean. Standard Errors in parenthesis clustered at the event level. Bars identify 95% confidence intervals.

12.2 Occupational downgrading

In Table 12.1 below, we show additional results regarding the characteristics of the job among individuals who find employment within three months. Using a sample of 2% of all native Germans in the social security data between 2007 and 2017, we created occupational characteristics that capture the share of natives by educational level, the mean daily wage, and the mean hourly wage from full-time employment (daily wage divided by 8 restricting to full-time employees). We follow on Barsbai et al. (2024) and measure occupational quality as the log mean wage income of a native worker in the same occupation, state, and year of observation (log occupational income score).[54] Additionally, we compute occupational quality scores based on the

[54]We report results for the occupational income score using either the mean daily wage and the full-time mean hourly wage (dividing the daily wage by a factor of 8 as in Brücker et al. (2021)

share of individuals with different levels of education within an occupation, the main task content of each occupation, and the level of regulation within the occupation (Vicari, 2014). For these measures, we include the occupational characteristics of the last job before unemployment as controls and use the occupational characteristics of the first job after unemployment as the outcome.

Columns (1)-(5) of Table 12.1 show different measures of occupational downgrading, constructed following the approach in Barsbai et al. (2024). In Columns (6)-(10), the outcomes are the share of individuals in each occupation for whom the main task is one of the five tasks mentioned in the column headers. Finally, Column (11) reports the results of a regression where the outcome is the degree of occupational regulation, measured using the procedure developed in Vicari (2014) and capturing the degree to which the 3-digit occupation is regulated (i.e., requires a nationally or regionally approved certification).

Table 12.1: Terror events and the job and firm characteristics among those employed within 3 months

	Occupational scores										
	Mean education			**Ln mean wage**		**Mean task**					Degree of occ. regulation
	Low	Middle	High	Hourly	Daily	Analytic non-routine	Interactive non-routine	Cognitive routine	Manual routine	Manual non-routine	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Post-Terror	0.001	0.018	-0.018	-0.004	-0.007	-0.004	0.001	-0.007	0.000	0.010	-0.018
	(0.003)	(0.014)	(0.016)	(0.019)	(0.021)	(0.025)	(0.006)	(0.020)	(0.015)	(0.012)	(0.013)
Observations	12,670	12,670	12,670	12,670	12,670	12,665	12,665	12,665	12,665	12,665	12,666
Mean outcome	0.153	0.756	0.091	3.970	2.104	0.132	0.087	0.134	0.141	0.506	0.048
Origin c. FE x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extended + extra controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01

Notes: Table 12.1 reports the coefficients from the estimation of Equation 3 where the outcomes are the occupational scores in the first job after unemployment. Post-Terror equals one for individuals entering unemployment in the same month-year when a terror event occurs in the home country, and it equals zero for individuals entering unemployment three months before that terror event. Individual controls: education, age, sex, years since migration, and its square. Extended controls (all measured in the last job before unemployment): ln of the wage, ln of the firm size, a dummy that equals one if full-time employment, and dummy variables for the share Germans in the firm above the median, the share of highly skilled workers in the firm above the median and the firm AKM above the median. The extra control is the occupational score of the respective column in the last job before unemployment.

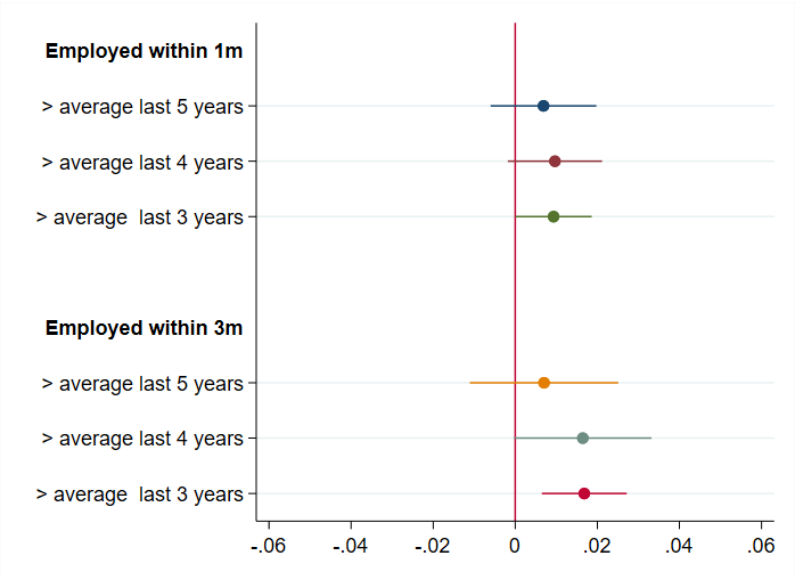
12.3 IEB robustness checks

In this section, we test the stability of our results using placebo tests and robustness checks very similar to those in Section 3.3.

Changing 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 12.2 displays the estimated coefficients 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.

In the labor market empirical design used in this section, the outcome is itself related to the bandwidths and for this reason in the main results we show directly the effect of terror on finding a job within one and three months.

Figure 12.2: Robustness IEB: higher than the average of last 3, 4 or 5 years



Notes: Figure 12.2 reports the estimated coefficients for regressions of the outcome on the terror indicator using Equation 3. Post-Terror equals one for individuals entering unemployment in the same month-year when a terror event occurs in the home country, and it equals zero for individuals entering unemployment three months before that terror event. All regressions include country of origin times year fixed effects, month times year fixed effects, state of residency fixed effects, individual controls, and extended controls. Standard Errors in parenthesis clustered at the event level. Bars identify 95% confidence intervals.

Additional controls and different clustering To ensure that we are comparing individuals from the same home country who face similar circumstances, in Table

12.2, we add country of origin times years since migration (Columns (1) and (4)), country of origin times age groups (Columns (2) and (3)) and country of origin times gender (Column (3) and (6)) to our baseline specification displayed in Columns (2) and (4) of Table 5. The main results are robust to the inclusion of these control variables.

In all our main results, we cluster the standard errors at the terror event level (e.g., treatment). In Table 12.3, we alternatively cluster the standard errors at the country of origin level and the year-country of origin level.

Table 12.2: Additional controls: effects of terror events on unemployed immigrants' outcomes

	Emp. within 1m			**Emp. within 3m**		
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Terror	0.009*	0.014**	0.013**	0.028***	0.017***	0.020***
	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)	(0.005)
Observations	47,220	47,248	47,286	47,220	47,248	47,286
Mean outcome	0.102	0.102	0.102	0.262	0.262	0.262
Origin c. FE x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Extended controls	Yes	Yes	Yes	Yes	Yes	Yes
Origin c. FE x YSM G. FE	Yes	No	No	Yes	No	No
Origin c. FE x Age G. FE	No	Yes	No	No	Yes	No
Origin c. FE x Gender FE	No	No	Yes	No	No	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 12.2 reports the coefficients from the estimation of Equation 3 where the outcomes are finding a job within 1 month and 3 months. Post-Terror equals one for individuals entering unemployment in the same month-year when a terror event occurs in the home country, and it equals zero for individuals entering unemployment three months before that terror event. Individual controls: education, age, sex, years since migration, and its square. Extended controls (all measured in the last job before unemployment): ln of the wage, ln of the firm size, a dummy that equals one if full-time employment, and dummy variables for the share Germans in the firm above the median, the share of highly skilled workers in the firm above the median and the firm AKM above the median.

Table 12.3: Different SE clustering: effects of terror events on unemployed immigrants' outcomes

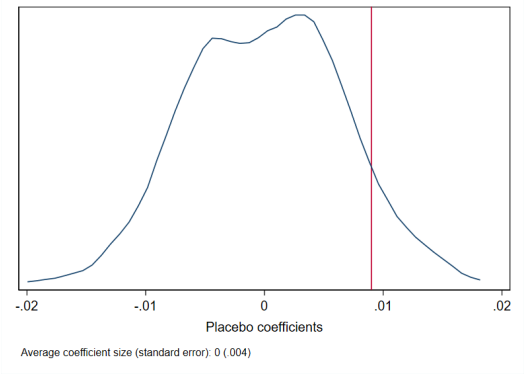
	Emp. within 1m			Emp. within 3m		
Standard errors clustered at:	Event level	Country of Origin	Country O. x Year	Event level	Country of Origin	Country O. x Year
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Terror	0.009**	0.009	0.009	0.017***	0.017**	0.017*
	(0.005)	(0.006)	(0.006)	(0.005)	(0.008)	(0.009)
Observations	47,305	47,305	47,305	47,305	47,305	47,305
R-squared	0.018	0.018	0.018	0.046	0.046	0.046
Mean outcome	0.102	0.102	0.102	0.262	0.262	0.262
Origin c. FE x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Extended controls	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parenthesis clustered at the level specified in each column, *p<.1; **p<.05; ***p<.01
Notes: Table 12.3 reports the coefficients from the estimation of Equation 3 where the outcomes are finding a job within 1 month and 3 months. Post-Terror equals one for individuals entering unemployment in the same month-year when a terror event occurs in the home country, and it equals zero for individuals entering unemployment three months before that terror event. Individual controls: education, age, sex, years since migration, and its square. Extended controls (all measured in the last job before unemployment): ln of the wage, ln of the firm size, a dummy that equals one if full-time employment, and dummy variables for the share Germans in the firm above the median, the share of highly skilled workers in the firm above the median and the firm AKM above the median.

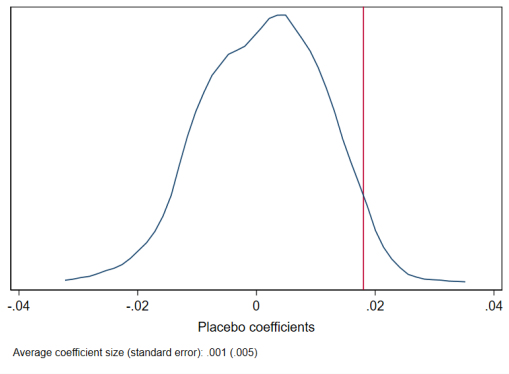
Placebo terror event date One concern is that there are other factors driving the effects on labor market outcomes, and we would observe the same pattern in the absence of the terrorist event. To address this issue, we randomly assign the binary treatment status 100 times across all observations. If there are x-treated and y-controls across all observations, the total number of treated and controls does not change, but x and y are reshuffled across observations. We then estimate the effect of placebo treatment status on unemployment duration. Figure 12.3 shows the distribution of the 100 estimated coefficients for the four outcomes of interest used in the main analysis in Table 5. The red vertical lines indicate the point under the true treatment assignment (the same coefficients reported in Table 5).

Figure 12.3: Placebo IEB: terror events and labor market outcomes

(a) Employment within 1 month



(b) Employment within 3 months

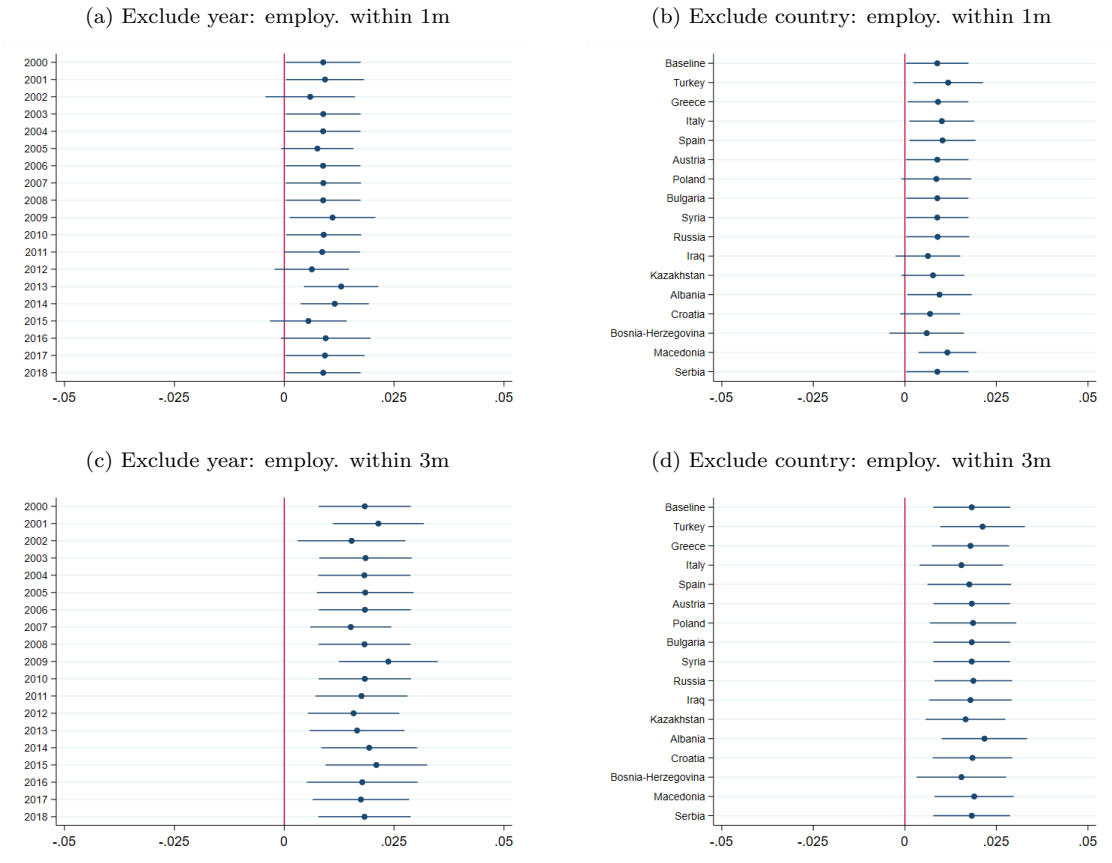


Notes: Figure 12.3 reports the distribution of the placebo coefficients from 300 regressions. The vertical red line indicates the baseline coefficient.

In Table 5, we found a positive and significant effect of entering unemployment in a month with a terror event for all immigrants on the probability of finding employment within one and three months. Figure 12.3 shows that assigning placebo treatment status to all immigrants who did not in reality experience a terrorist event has, on average, zero effects on the probability of entering employment. These findings provide an important piece of evidence in favor of our baseline results.

Excluding a year or a country Here, we test whether our results are driven by specific countries or years. We run the baseline regression, excluding one year at a time and excluding one of the main countries of origin at a time. The graphs on the left-hand-side of Figure 12.4 show the estimated coefficients for each regression in which a survey year is excluded for each of the two outcomes in Table 5. The graphs on the right-hand side of Figure 3 show the estimated coefficients for each regression in which a country of origin is excluded. The y-axis displays the excluded year or country of origin. Overall, our results are stable throughout these robustness tests.

Figure 12.4: Robustness IEB: exclude one country and year at the time



Notes: Figure 12.4 displays the point estimates and 95% confidence intervals for regressions excluding one year and country at a time, respectively. The y-axis refers to the excluded 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 (equation 3). The baseline estimates refer to those in Table 5. Standard Errors in parenthesis clustered at the event level.

Different treatment group definition In Section 4, we defined the treatment group as immigrants whose employment spell was terminated in a month when a relevant terror event occurred in their home country. By doing so, we implicitly assume that job separations in the short run are unaffected by terrorist events. According to German law, the default notice period for resignations is 30 days. However, collective bargaining agreements often designate a longer notice period, and in practice, a three-month notice period is common. Therefore, we believe it is plausible to assume that individuals do not decide to leave their jobs and actually leave in less than 30 days. Similarly, individuals who have announced that they would leave their jobs do not revert their announcement within 30 days.

Nevertheless, as a robustness check, in Table 12.4, we show that our results are

robust to using only individuals whose employment spell was terminated due to a mass layoff (Panel A, Columns (1) and (2)) a(Panel A, Columns (1) and (2)) and individuals whose employment spell was terminated in the same month as the terror event but within the 15 days before and the 15 days after the first terror attack (Panel A, Columns (3) and (4)). We also show a different treatment group definition, where we consider individuals whose employment spell was terminated in the same month when a relevant terror event occurred in the home country but before the first terror attack (Panel B, Columns (1) and (2)) plus individuals whose employment spell was terminated in the last 15 days of the month prior to the month with a relevant terror event (Panel B, Columns (3) and (4)). The results are robust to these alternative definitions.

To define whether job displacement comes from a mass layoff, we followed the literature on mass layoffs, which has extensively used the German social security data. In particular, we combine both the mass-layoff definition based on workers' flow used in Schmieder et al. (2023) and the mass-layoff definition based on firms' bankruptcies used in Fackler et al. (2021) to identify displaced workers. We then run the analysis only on the sub-sample of the identified displaced workers.

Table 12.4: Robustness IEB: different treatment group definition

Panel A:	**Unemp. due to mass layoff in month with terror event**		**Unemp. in month with terror event −15/+15d before/after 1st attack**	
	Emp. within 1m	Emp. within 3m	Emp. within 1m	Emp. within 3m
	(1)	(2)	(3)	(4)
Post-Terror	0.044**	0.058**	0.030**	0.038**
	(0.018)	(0.023)	(0.012)	(0.015)
Observations	4,099	4,099	28,107	28,107
Mean outcome	0.109	0.248	0.113	0.285
Panel B:	**Unemp. in month with terror event before 1st attack**		**Unemp. in month with terror event before 1st attack + 15d prior month**	
	Emp. within 1m	Emp. within 3m	Emp. within 1m	Emp. within 3m
	(1)	(2)	(3)	(4)
Post-Terror	0.011	0.062*	0.024	0.062***
	(0.020)	(0.032)	(0.020)	(0.020)
Observations	23,689	23,689	25,091	25,091
Mean outcome	0.103	0.262	0.103	0.263
Origin c. FE x Year FE	Yes	Yes	Yes	Yes
Month FE x Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Extended controls	Yes	Yes	Yes	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 12.4 reports the coefficients from the estimation of Equation 3 where the outcomes are finding a job within 1 month and 3 months. Panel A considers individuals whose employment ended in the same month as a relevant terror event in the home country. Columns (1) and (2) consider only individuals whose employment ended due to a mass layoff, and Columns (3) and (4) consider only individuals whose employment spell was terminated in the same month as the terror event but within the 15 days before and the 15 days after the first terror attack. Panel B considers individuals whose employment ended in the month before the relevant terror event in the home country. Columns (1) and (2) consider only individuals whose employment ended within the last 5 days of the month, and Columns (3) and (4) consider only individuals whose employment ended within the last 15 days of the month. Individual controls: education, age, sex, years since migration, and its square. Extended controls (all measured in the last job before unemployment): ln of the wage, ln of the firm size, a dummy that equals one if in full-time employment, and dummy variables for the share Germans in the firm above the median, the share of highly skilled workers in the firm above the median and the firm AKM above the median.

Using another type of violent event In line with Section 3.3, we use armed conflict events in the home country as an alternative shock to the perception of safety in the country of origin. The results in Table 12.5 are consistent with those of Table 5.

Table 12.5: Effects of armed conflict events on unemployed immigrants' outcomes

	Emp. within 1m		**Emp. within 3m**	
	(1)	(2)	(3)	(4)
Post-Conflict	0.011	0.011	0.035***	0.035***
	(0.007)	(0.007)	(0.010)	(0.010)
Observations	13,154	12,790	13,154	12,790
Mean outcome	0.091	0.091	0.217	0.217
Origin country FE x Year FE	Yes	Yes	Yes	Yes
Month FE x Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Extended Controls	No	Yes	No	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 12.5 reports the coefficients from the estimation of Equation 3 where the outcomes are finding a job within 1 month and 3 months. Post-Conflict equals one for individuals entering unemployment in the same month-year when an armed conflict event occurs in the home country, and it equals zero for individuals entering unemployment three months before that armed conflict event. Individual controls: education, age, sex, years since migration, and its square. Extended controls (all measured in the last job before unemployment): ln of the wage, ln of the firm size, a dummy that equals one if in full-time employment, and dummy variables for the share Germans in the firm above the median, the share of highly skilled workers in the firm above the median and the firm AKM above the median.

Characteristics of individuals who find employment within 3 months The findings in Table 6 refer to the sub-population of individuals who find a job within 3 months. This might be a selected sample of individuals in the sense that individuals who find a job relatively early might have different characteristics from those who find a job later. As long as the characteristics of these individuals are not different between the treated and control groups, our results cannot be attributed to a compositional change among those finding a job within three months following a terror event in the home country. As an additional check, we run several regressions where we use the individual characteristics as outcomes:

$$X_{i,o,y,m} = \alpha + \rho \text{PostTerror}_{i,o,y,m} + \delta \text{Emp(3 months)}_{i,o,y,m} + \beta \text{PostTerror}_{i,o,y,m} * \text{Emp(3 months)}_{i,o,y,m} + \mu_{o,y} + \phi_{m,y} + \lambda_f + \epsilon_{i,o,y,m} \quad (7)$$

The results on the interaction term are shown in Table 12.6 below. For all the included characteristics, we find no statistically significant difference between immigrants who find a job within three months in the treatment versus control group. This provides evidence that the results in Table 6 are internally valid and not attributable to compositional changes. Nonetheless, we acknowledge that we cannot extend our results on the job and firm characteristics to those who find a job later

than 3 months.

Table 12.6: Individual characteristics, finding a job within 3 months, Post-terror

Characteristic	**Estimated Coeff.**	**Standard Error**
	(1)	(2)
Lowe edu	-0.001	0.012
Middle edu	-0.010	0.011
High edu	0.011	0.007
Age	0.296	0.249
Female	0.003	0.010
Years since migration	0.191	0.183
Ln daily wage	-0.001	0.015
Full-time employ.	0.001	0.009
Ln firm size	0.012	0.044
Share Germans (> p50)	-0.005	0.014
Share HS (> p50)	-0.003	0.009
AKM (> p50)	0.011	0.013
Observations	47305	
Origin country FE x Year FE	Yes	
Year FE x Month FE,	Yes	
State FE	Yes	

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table 12.6 reports the coefficients on the interaction PostTerror $*$ Emp(3 months) from the estimation of Equation 7 The outcomes are the individual characteristics listed on the left-hand-side column. Post-Terror equals one for individuals entering unemployment in the same month-year when a terror event occurs in the home country, and it equals zero for individuals entering unemployment three months before that terror event. Emp(3 months) equals one for individuals finding a job within three months, and it equals zero otherwise.

Actual returns To test whether our results on labor market outcomes are driven by an immediate reduction in returns to the home country and, more generally, whether in the three months after a terror event, the change in return intentions materializes into a change in actual returns, we run the following analysis.

First, we computed the natural log number and the share of leavers for each country of origin, month, year, and German federal state. Leavers' are defined as the last spells of individuals who disappear from the social security records.[55] We then merged this data with the terror events data and used a design identical to the GSOEP analysis on return intentions. That is, we compare actual returns in the periods before and after terror attacks, including country of origin times year fixed effects, month times year fixed effects, and German federal state fixed effects. We estimate the equation:

[55]We restrict the analysis to the time window 2000-2016 to reduce the problems induced by the right-censoring of the data in 2017. In a similar fashion, Bahar et al. (2022) use exits from the IAB data of Yugoslavian nationals to measure return migration to Yugoslavia.

$$L_{o,y,m,f} = \alpha + \sum_{m=-3}^{3} \beta_m Time_{o,y,m} + \delta X_y + \mu_{o,y} + \phi_{m,y} + \lambda_f + \epsilon_{o,y,m,f} \qquad (8)$$

where $L_{o,y,m,f}$ measures actual returns (log or share) of individuals from country of origin o, in the register in year y and month m and residing in federal state f. $Time_{o,y,m}$'s are dummies identifying periods around the event where m denotes months since a month with a relevant terror event (e.g., $m = -2$ for those individuals in the register 2 months before the event month).

The results of this exercise are reported in Table 12.7. Using both measures, the effects on actual returns are virtually zero, which suggests that a change in return intention takes time to materialize. Moreover, it suggests that our employment results are not affected by the self-selection of stayers.

Table 12.7: Actual Returns

	Ln number of leavers	Share of leavers
	(1)	(2)
Post-Terror	0.017	0.001
	(0.027)	(0.001)
Observations	57249	57249
Mean Outcome	0.957	0.011
Origin country FE x Year FE	Yes	Yes
Year FE x Month FE,	Yes	Yes
State FE	Yes	Yes
Mean indiv. characteristics	Yes	Yes

Standard errors in parenthesis clustered at the event level, *p<.1; **p<.05; ***p<.01
Notes: Table reports the coefficients on Post-Terror using equation 8 where the outcome is the ln number of leavers (Column (1)) and the share of leavers (Column (2)) for each country of origin, month, year, and German federal state.