Department of
Geography and
Environment

Papers in Economic Geography and Spatial Economics

Barriers to humanitarian migration, victimisation and integration outcomes: Evidence from Germany

Teresa Freitas-Monteiro and Lars Ludolph

Paper No. 27
Geography and Environment Discussion Paper Series

May 2021

All views expressed in this paper are those of the author(s) and do not necessarily represent the views of the editors or LSE. The results presented in the paper are not peer-reviewed.

Published by
Department of Geography and Environment
London School of Economics and Political Science
Houghton Street
London
WC2A 2AE

geog.comms@lse.ac.uk
www.lse.ac.uk/Geography-and-Environment

Barriers to humanitarian migration, victimisation and integration outcomes: Evidence from Germany*

Teresa Freitas-Monteiro[†] Lars Ludolph[‡]

May 5, 2021

Abstract

In this paper, we link the peril of asylum seekers' migratory journey to economically quantifiable outcomes in the destination country using refugee survey data from Germany collected in the aftermath of the 2015 refugee crisis. We start by showing that, accounting for selection effects, physical victimisation during the journey to Germany is strongly associated with significantly lower mental well-being and general health upon arrival in the destination. The physical victimisation experience severely distorts the human capital investment decision by leading affected refugees to favour joining the labour force and engaging in part-time and marginal employment over pursuing host-country education. We place our findings into both the psychiatric and experimental economic literature, which suggest that experiencing physical trauma in vulnerable situations results in a "loss of future directedness" or "impatience" among the victimised, leading them to discount future payoffs more heavily.

JEL codes: F22, J15, J21, O15
Keywords: Refugees; Victimisation; Labour market integration; Education

*The authors are grateful for helpful suggestions and constructive comments from Achim Ahrens, Herbert Brücker, Riccardo Crescenzi, Timo Hener, Nancy Holman, Yuliya Kosyakova, Angela Kunzler and Olmo Silva. We also thank seminar participants at the German Institute for Economic Research (DIW), the London School of Economics, the Maastricht School of Governance and the Migration, Health and Integration Virtual Symposium at Maastricht University. All errors and omissions remain our own.

†Institute for Employment Research, Regensburger Straße 104, 90478 Nürnberg

‡London School of Economics and Political Science, Houghton Street, London WC2A 2AE, United Kingdom

1 Introduction

One of the key features of humanitarian migration flows from developing into developed regions of the world is the significant risk these journeys entail for individuals who embark on them. According to the International Organisation for Migration's (IOM) Missing Migrant database, around 15,000 migrants perished in the Mediterranean Sea alone when trying to reach the territory of European Union (EU) member states between 2015 and 2019. Asylum seekers who survive the perilous journey often do not come out unscathed: They are subjected to violent acts on their journey carried out by escape agents and border enforcement agencies, with detrimental consequences for their physical and mental health [Albahari, 2018, Arsenijević et al., 2017, 2018]. Against the backdrop of a subdued economic and societal integration of newly-arriving humanitarian migrants in the EU [Brell et al., 2020], the potentially negative consequences of these victimisation events for future life trajectories of affected individuals -and thus the welfare of hosting countries- has increasingly found its way into the political debate. For example, the European Commission [2020] notes that "mental health is critical to migrants' integration" [p. 3] and "especially refugees, may be at higher risk of developing mental health problems due to [...] difficulties encountered during their migration journey" [p. 13].

In this paper, we explicitly analyse the potentially disruptive role of asylum seeker victimisation during their journey to safety for their economic activity in Germany, the main destination country of asylum seeker inflows into the EU. To analyse this link, we deploy novel refugee survey data collected in the aftermath of the large inflows of asylum seekers into the country in 2015/2016. We use this data to construct a physical and a financial victimisation indicator for each refugee based on detailed questions regarding negative events individuals experienced along the journey to Germany. We then link the survey to administrative employment biography data to study how both physical and financial victimisation affect refugees' economic activity trajectories upon arrival in the host country. To allow for a causal interpretation of our estimated coefficients, we control for a wide range of premigration characteristics and limit the variation in the data to narrowly defined fixed effects categories relating to the country of origin interacted with the exact month of departure, to within arrival cohorts and a measure of the geographical route taken to reach Germany.

Our results show that physically victimised asylum seekers in particular suffer from significantly lower life satisfaction as well as poorer physical and mental health upon arrival. While we detect no distortions at the external margin of economic activity, physically victimised refugees are less likely to invest into host-country education but take up employment faster than non-victimised and financially victimised migrants. This leads to the intuitively paradoxical finding of a higher employment rate among physically victimised refugees vis-à-vis other refugees in the early years upon arrival, an adjusted gap reaching 4.4 percentage points 31 months into refugees' stay in Germany. We show that this excess employment among the physically victimised is driven by marginal and part-time employment, work that is characterised by a lower level of income relative to other refugees. We further document larger financial hardship among the financially victimised but do not find distortionary effects on the human capital investment decision for this subgroup. Our findings thus suggest that, in line with previous research, physical victimisation has stronger effects on affected individuals' behaviour [Dolan et al., 2005, Mahuteau and Zhu, 2016, Johnston et al., 2018].

To conceptualise our findings, we draw on evidence from the sociology, psychiatry and economics literature. Evidence from sociology and psychiatry documents a "loss of future directedness" caused by potentially traumatic victimisation events [Beiser, 1987, Hauff and Vaglum, 1993a, Hunkler and Khourshed, 2020, Sagbakken et al., 2020]. As physically traumatized refugees adopt a more pessimistic outlook on life and discount their future more heavily, they tend to invest less into host-country specific education and are more likely to take up low-skill employment soon after arrival. Similarly, the experimental economics literature shows that time preferences can be affected by extreme events linked to violence, making victimised individuals more impatient in their decision-making [Voors et al., 2012, Callen et al., 2014, Jakiela and Ozier, 2019, Brown et al., 2019]. While these effects are not directly distinguishable from a general decline in mental well-being in our setting, one of the key strengths of our study is that the granular survey data allows us to rule out a number of competing theories that could explain our findings. These range from institutional mechanisms in-built into German asylum procedures to mechanisms related to financial hardship and other behavioural changes caused by victimisation events. Since the first survey interviews were conducted close to the arrival date of asylum seekers, our findings do not face the standard issue of reverse causality when linking victimisation and the resulting decline in mental well-being to individuals' labour market attachment [Brown et al., 2010, Kassenboehmer and Haisken-DeNew, 2009]. The relatively large number of observed individuals further allows us to split our sample along a range of dimensions. We show that all our findings are robust to restricting the sample to recognised refugees, hold for the subgroup of Syrians who receive protection with near certainty, and are driven by both males and females.

Our identification strategy relies on the random nature of victimisation among asylum seekers. We identify and group three main empirical challenges to a causal interpretation of the effect of victimization on labour market and health outcomes. First, a non-random selection at the country of origin at different expected victimisation rates may lead specific subsets of asylum seekers to choose Germany as their destination (selection at origin bias). If these groups show unobserved characteristics that also causally impact integration outcomes, estimated coefficients on the effect of victimisation will be biased. A second source of potential bias may arise from the fact that only those who succeed in making it to Germany are observed in our sample. This group may represent a sub-group of, for instance, particularly motivated or resilient individuals (survivor bias). Finally, victimisation itself may be correlated with other unobserved factors that determine the economic integration in Germany (omitted variable bias). To address selection at origin and survivor bias, we show that our baseline results are robust to a wide range of empirical specifications that limit variation to narrowly defined fixed effects categories. These fixed effects relate to the country of origin of asylum seekers, interacted with their precise time of departure and arrival. We show that, conditional on the geographical origin and the timing of migration, baseline differences between physically and non-physically-victimised groups indeed disappear, leaving us with a sample balanced along a large range of observable characteristics. By combining these balance tests with coefficient stability tests developed by Oster [2019], we are able to credibly rule out omitted variable bias as a driver of our results in all linear regressions. The rich survey data further allows us to add precision to our estimates by selecting controls for a wide range of pre-migration and post-migration characteristics based on both economic theory and machine learning techniques. Taken together, throughout our analysis, we thus compare victimised and non-victimised refugees with similar pre-migration and selected post-migration characteristics, who originate from the same country, migrated in the same year-month, took the same migration route and are part of the same arrival cohort.

Our study adds to the existing literature in a number of ways. We primarily add to the literature that links refugee victimisation to their economic behavioural response in the host country [Couttenier et al., 2019, Hunkler and Khourshed, 2020, Hauff and Vaglum, 1993a]. Unlike previous literature, our data allows us to explicitly focus on events asylum seekers endured during their journey as opposed to their country of origin, an important distinction for the design of asylum policies. Since victimisation of asylum seekers is interconnected with the choice of external border policies [Arsenijević et al., 2017, 2018], we further contribute to the growing literature on how asylum-seeker specific policies shape refugee labour market integration [Damm, 2009, Battisti et al., 2016, Hainmueller et al., 2016, Marbach et al., 2018, Zwysen, 2019]. One of the main takeaways of our study is that a rapid labour market integration as a general success metric for integration outcomes should be treated with care; higher victimisation rates may contribute to a relatively swift uptake of employment, nevertheless distorting optimal labour market matching. We further add to the recently developing stream of literature that links crime victimisation to labour market outcomes more generally [Bindler and Ketel, 2019]. We show that this link is context-specific and the perilous journey asylum seekers go through may not be easily compared to other settings. Finally, we also add to the much broader literature on violence and the human-capital investment decision by providing further evidence that experiencing traumatic events in conflict and high-crime settings lowers the willingness to invest in education [Blattman and Annan, 2010, Shemyakina, 2011, Leon, 2012, Akbulut-Yuksel, 2014, Koppensteiner and Menezes, forthcoming].

The remainder of this paper proceeds as follows. The next section discusses the conceptual framework that links the victimisation experience to economic activity outcomes in the destination country in more detail. Section three discusses our data sources and profiles victimised vis-à-vis non-victimised migrants. Section four introduces the estimation strategy and our approach to dealing with self-selection and survivor bias in detail. Section five shows the main results and section six discusses their robustness. In section seven, we test alternative hypotheses that could explain our findings. Section eight provides a concluding discussion.

2 Related literature and research hypotheses

Some evidence exists on the direct disruptive effect of victimisation events on affected individuals' future economic outcomes for the general (Western) population without an explicit focus on migrants or refugees. These studies unequivocally find negative consequences of victimisation regarding labour force participation, employment, earned income and increased welfare dependency [Bindler and Ketel, 2019, Ornstein, 2017, Velamuri and Stillman, 2008]. Other studies, primarily on developing countries, have documented distortions to the human-capital investment decision following potentially traumatic events in conflict-related or high-crime settings[Blattman and Annan, 2010, Shemyakina, 2011, Leon, 2012, Akbulut-Yuksel, 2014, Koppensteiner and Menezes, forthcoming]. A larger literature is concerned with the effect of victimisation on health and mental well-being outcomes, the structural mechanism linking victimisation events to disruptions in other areas of life and economic outcomes in particular. For example, Mahuteau and Zhu [2016] find that physical victimisation decreases subjective well-being. Dolan et al. [2005] estimate a measure of 'loss in quality-adjusted life years' to quantify the cost to victims and show that rape, followed by other serious physical assault decreases victims' quality-adjusted life years the most. Johnston et al. [2018] focus on the effect of crime victimisation on life satisfaction as an encompassing measure of mental well-being and find that physical assault and events that lead to a major worsening of affected individuals' financial situation have the strongest negative effect on life satisfaction for both men and women.

Despite these clear findings for the general population, it is crucial to note that the situation of asylum seekers who arrive in their host-country is unique and the extent to which general studies apply to migrants' victimisation experience requires a careful reflection. Unlike samples of victimised individuals drawn from the general population or other migrant groups, forcefully displaced migrants all start their economic activity trajectory at zero upon arrival. This has major implications for their choice set with respect to the host-country labour market and potential distortions to these choices caused by a victimisation event. Since the majority of refugees originate from countries where educational attainment is not regarded as equivalent to education in economically advanced countries, or lack proof of their formal degrees, refugees from less developed countries face the decision to i) either join the labour force immediately and accept a discount on their educational attainment they face in Western countries and take up low-skill, low-paid employment or ii) invest into host-country specific human capital to have access to better paid employment later on [Duleep and Regets, 1999, Cortes, 2004].

Evidence from both the sociology and psychiatric literature suggests that victimisation experiences have lasting effects on future-oriented planning. In a study closest to ours, Hauff and Vaglum [1993a] analyse a cohort of 145 Vietnamese refugees in Norway one year and three years after arrival. The authors find that those who experienced more potentially traumatic events were more likely to be in the labour force but tend to invest less into host-country specific education. In a separate study on the same cohort, the authors find a persistently worse mental health status among those who experienced traumatic events even seven years after arrival vis-à-vis the non-traumatised [Hauff and Vaglum, 1993b]. In combination, these findings suggest that experiencing trauma under extreme conditions may either trigger a behavioural change on the short-term versus long-term trade-off independent of the effect of victimisation on mental health or as a direct consequence of it. In a more recent study, Hunkler and Khourshed [2020] evaluate the integration outcomes of 252 Syrian refugees in Germany's federal state of Bavaria and broadly confirm the findings by Hauff and Vaglum [1993a]. The authors find that refugees who experienced potentially traumatic events in their country of origin or on their journey to Germany show better German language skills but their labour market outcomes do not differ from the non-victimised, a curious finding considering the clearly established link between language skills and economic integration success [Dustmann and Soest, 2001].

The tendency of refugees to adopt a more present-oriented mindset following extreme potentially traumatic episodes can be traced back in the psychiatric literature until Beiser [1987]. The author finds that refugees who went through extreme events while fleeing their country have a shortened sense of future, a potential explanation for favouring early employment over long-term educational investment. In a more recent study, Sagbakken et al. [2020] interview 78 asylum seekers in Norway and find that a loss of future directedness leads traumatized asylum seekers to "withdraw into passivity" [p. 1]. Research on self-harm and suicide among refugees show that the lack of future orientation also manifests itself into higher rates of suicide in these segments of the population, especially when having endured extreme traumatic events [Goosen et al., 2011, Hawton, 2009, Lerner et al., 2016].

These findings that relate traumatic events refugees endure to a lack of future-oriented planning find further support in recent experimental studies in the economics literature. While classic economic models assume that preferences are stable over time and unaffected by life-time experiences [Stigler and Becker, 1977], recent experimental studies suggest that individuals' risk aversion and time preferences can indeed be affected by extreme events linked to violence [Voors et al., 2012, Callen et al., 2014, Jakiela and Ozier, 2019, Brown et al., 2019], natural disasters [Eckel et al., 2009, Page et al., 2014, Callen, 2015, Cameron and Shah, 2015, Cassar et al., 2017, Hanaoka et al., 2018, Beine et al., 2020], health shocks [Decker and Schmitz, 2016], and financial and macroeconomic shocks [Guiso et al., 2018, Jetter et al., 2020, Malmendier and Nagel, 2011, Kettlewell, 2019]. The time preferences have in turn been found to affect decisions regarding borrowing [Meier and Sprenger, 2010], savings [Thaler and Benartzi, 2004] and financial literacy [Meier and Sprenger, 2013] among adults, and investment and human capital acquisition among the younger [Sutter et al., 2013, Cadena and Keys, 2015, Kemptner and Tolan, 2018]. Our analysis relates to this literature by indirectly measuring the time preferences of victimized versus non-victimized individuals.[1] This possible interpretation is based on the assumption that individuals indirectly reveal their time preferences by engaging in certain activities [DellaVigna and Paserman, 2005]. Individuals who attach higher value to long-term rewards are more likely to pursue activities that entail an immediate cost (such as investing in human capital) but which have delayed payoffs (access to higher quality employment in the future). On the other hand, impatient individuals are more likely to engage in activities which have immediate benefits (such as low-income employment) and delayed costs (lack of access to higher quality employment in the future).

In summary, we derive two hypotheses regarding the effect of victimisation events asylum seekers experience on their journey to Europe and their potential to cause disruptions with respect to economic integration. The first relates to refugees' decision to become economically active (e.g., participate in the labour force or acquire education) in the host-country at all. This is what we refer to as the **external margin of economic activity**: Previous studies on the victimisation-mental health-economic activity nexus predict lower economic

[1] Voors et al. [2012], for instance, find that individuals who were exposed to greater levels of violence display more altruistic behaviour, are more risk-seeking, and have higher time discount rates. Similarly, Cassar et al. [2017] conduct a series of experiments in rural Thailand and find that the 2004 tsunami led to long-lasting increases in risk aversion, pro-social behaviour, and impatience

activity among the victimised. The second hypothesis relates to the choice newly-arriving refugees face between (i) joining the labour force and looking for employment and (ii) investing into host-country education. This is what we refer to as the **internal margin of economic activity**: Victimisation during the journey may lead to disruptions regarding the host-country human capital investment decision of victimised and non-victimised refugees vis-a-vis the alternative of low-income employment through a loss of future directedness.

One of the limitations of our survey data is that while we are able to capture individual-level mental well-being in several ways, it is not possible to distinguish a potential loss of future directedness directly from a general decrease in mental well-being caused by victimisation events. We address this concern in two ways. First, the theoretical effect of victimisation on the external and the internal margin of economic activity go into opposite directions: a decrease in mental health is expected to lead to a lower labour market attachment while the loss of future directedness increases early labour force participation. It is therefore possible to settle the question empirically in parts. Second, our data allows us to rule out all alternative theories that could explain our findings.

There are indeed a number of competing mechanisms that could explain the relation between asylum seeker victimisation during the journey and integration outcomes. The first relates to the institutional design of the German asylum system. If victimised individuals received asylum faster since they have a more genuine case for protection and therefore get access to the labour market faster upon arrival, this would mechanically link victimisation to a faster labour market integration and could potentially bias the results. The German asylum system has a second key institutional feature that could encourage fast employment among specific segments of the asylum seeking population. Despite options being very limited in scope, obtaining employment before asylum can improve the chances of receiving a temporary protection status ("Duldung") in Germany [Brücker et al., 2019]. Finding employment upon arrival is therefore particularly incentivised for migrants with a low probability of receiving full protection status since employment disproportionally increases the probability of being allowed to stay in Germany for these individuals. If these individuals with a low chance of getting their asylum granted take higher risks on their journey to Germany (and get victimised relatively more often), they may therefore seek employment quickly to improve their chances of receiving asylum. We analyse these institutional mechanisms in detail in section 7.1. The second mechanism relates to asylum seekers' financial hardship that could be co-determined with victimisation experiences. Smugglers have been documented to be responsible for abuse of asylum seekers during their journey and often charge large amounts for their services Albahari [2018]. We analyse this mechanism in section 7.2. The third mechanism is related to the intended duration of stay in Germany. If victimised individuals intend to stay for a relatively shorter duration than non-victimised individuals because the difficult journey discouraged them, they could be more likely to seek low-skill employment instead of investing into host-country specific human capital [Cortes, 2004]. We analyse this mechanism in section 7.3 and show that our results are not driven by these hypotheses.

3 Data and definitions

This section contains a description of all data sources and introduces the main variables of interest and the outcomes. In the final subsection 3.4, we then turn to a first descriptive analysis of differences between victimised and non-victimised asylum seekers to both fill a gap in the literature and as a first motivation of our empirical strategy.

3.1 IAB-BAMF-SOEP refugee survey

The main data source for all our analyses is the IAB-BAMF-SOEP refugee survey. It is an extension to the established German Socio-Economic Panel (GSOEP) survey through an ad hoc module for the target population of asylum seekers and refugees. The sampling frame of the IAB-BAMF-SOEP survey is the German Central Register of Foreign Nationals. The survey has a panel structure with a total of 12,311 interviews carried out in three waves in 2016, 2017 and 2018 on 6,763 individuals.

The survey contains a wide range of baseline information on pre-migration characteristics and detailed information on individual and household characteristics, including on respondents' health. It also provides information on the time of displacement in the country of origin, within-country information on the province of origin and the time of arrival in Germany. Most importantly for our purposes, respondents are asked detailed questions on experiences they went through during the journey from their country of origin to Germany. 3,742 individuals, 55.2% of the total sample, agreed to provide information on these experiences. Our effective working sample for which we have all relevant information, including all necessary control variables and additional outcomes, consists of 2,314 individuals aged between 18 and 65.

Within these questions, our main interest lies on the survey question 'During the journey or escape, did you experience one or more of the following?' which allow surveyees to choose one or more answers from a list of negative experiences. Based on their responses, we create a **binary physical victimization indicator** taking the value 1 if an individual was subjected to sexual abuse, physical attacks, incarceration or shipwreck (or any combination of these). We further create a **binary financial victimization indicator** taking the value 1 if an individual was subjected to financial fraud, extortion, robbery or blackmail, or any combination of these. The summary statistics for the two victimisation indicators are shown in table 1.

Variable	Mean	Std. Dev.	Min.	Max.
Experienced robbery	0.133	0.34	0	1
Experienced extortion	0.155	0.362	0	1
Experienced fraud	0.287	0.452	0	1
Financial victimisation	0.39	0.488	0	1
Experienced sexual harassment	0.017	0.129	0	1
Experienced shipwreck	0.137	0.344	0	1
Experienced physical attack	0.134	0.341	0	1
Experienced incarceration	0.201	0.401	0	1
Physical victimisation	0.359	0.48	0	1
N		2314		

Table 1: Physical and financial victimization indicator

We note that by design of these indicators, individuals may experience both a financial and a physical trauma. Reassuringly, the correlation between these two (r=0.3) is sufficiently low not to be a cause for concern in our regression analyses. We further note that some migrants experienced more than one victimisation event, but nevertheless model our preferred indicators as binary for two main reasons. First, the majority of migrants experienced one victimisation event. Only 11% of all individuals in our sample experienced more than one physical victimisation event and 15% experienced more than one financial victimisation event. Second, there is no clear guidance in the victimisation literature on the correct functional form of the relation between our outcomes of interest and multiple victimisation events individuals experience during their flight to safety that lasted for 40 days on average. We explore this question further by relaxing the assumption of no additional behavioural effect when individuals experience additional victimisation events in section 6.1.

Important to our analysis, from the second wave of the survey onwards, individuals who were willing to answer questions regarding their escape journey were explicitly asked which route they took to reach their destination. We assign their answers to the five main migration routes: (1) The Eastern Mediterranean sea route, (2) the Central Mediterranean route, (3) the Western Mediterranean route, (4) the Eastern Mediterranean land route, (5) the Eastern Land border route and (6) travelling directly to Germany by plane. Since the survey questions on the route taken contain many missing values in the first wave of the survey, we impute the routes using an additional source of information. Individuals were invited to report on a virtual map all locations they passed through on their migratory journey from their country of origin to Germany. We use this data and apply the method developed by Guichard et al. [2021] to extract the geo-referenced points, infer the migration route and classify these to match the five first routes.[2] Applying these methods allows us to recover route

[2]The authors start by assigning the geo-coded points to all countries, and define a sequence of countries for each migration route. Secondly, they identify the last country before an individual entered the Schengen area and the first location in the Schengen zone. A path is assigned to (1) the Eastern Mediterranean sea route if the last non-Schengen country was Turkey and the first Schengen country was Greece; (2) the Central Mediterranean route if the last non-Schengen country was Egypt, Libya, Tunisia, or Turkey and the first Schengen country was Italy or Malta; (3) the Western Mediterranean route if the last non-Schengen country was Morocco or Algeria and the first Schengen country was Spain or France; (4) the Eastern Mediterranean

information for 77 % of the sample. We assign the remaining 23 % to a seventh category (7), no route information.

We then draw on the psychiatric and health economics literature reviewed in section 2 and use four main indicators to measure mental well-being and the general health of victimised and non-victimised refugees. Following Johnston et al. [2018], we use life satisfaction measured on a scale from 1 to 10 (with 10 being the highest), an encompassing measure of mental well-being, as our primary outcome of the mental health effect of victimisation. We complement this measure with a measure of self-assessed health (1-10), a mental component score (MCS) and a physical component score (PCS). The MCS is a standardised mental health index, based on six questions related to emotional and psychological problems and how these impact on daily activities [Nübling et al., 2006]. The factor loadings of the questions are used as weights and the score is then normed to range from 0 to 100. Similarly, the physical health index is constructed based on six questions related to physical health. We provide all details on the construction of the MCS and the PCS in section A of the appendix. The four measures are summarised in table 2, together with the backward-reported measures of pre-migration life-satisfaction and pre-migration self-reported health.

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Life satisfaction before migration (1-10)	7.09	2.839	0	10	2314
Health before migration (1-10)	8.339	2.419	0	10	2314
Current Life Satisfaction (1-10)	7.166	2.125	0	10	2314
Health after migration (1-10)	7.9	2.434	0	10	2314
MCS: Mental Component Scale	48.346	11.298	4.626	73.259	2277
PCS: Physical Component Scale	53.444	9.966	13.487	77.651	2277

Table 2: Mental well-being and health indicators

Finally, for the economic integration outcomes, our main interest lies on three measures: The external margin of economic activity, defined by those in the labour force and those in education or training. In the second step, we zoom into the generic economic activity indicator and break it up into those in the labour force and those in education and training to uncover potential distortions to the timing of entering the labour force caused by victimisation. We complement our main analyses with an analyses on employment rates, further split up into full-time, part-time and marginal employment, and net monthly income. These are shown in table 3 for the last observation available of each individual in the panel.

land route if the last non-Schengen country was Turkey and the first EU country was Bulgaria; (5) the Eastern Land border route if the last non-Schengen country was Romania, Ukraine, or Belarus, and the first Schengen country was Poland, Slovakia, or Hungary.

Variable	Mean	Std. Dev.	Min.	Max.	N
Economically active	0.759	0.428	0	1	2314
Labour Force Participation	0.741	0.438	0	1	2314
Education or training	0.078	0.268	0	1	2314
Education or training	0.078	0.268	0	1	2314
Employed	0.218	0.413	0	1	2314
Full-time employed	0.109	0.312	0	1	2314
Part-time or marginally employed	0.109	0.312	0	1	2314
Net income	892.736	571.574	0	3100	440

Table 3: Economic activity indicators

3.2 IAB integrated employment biographies

We use the IAB integrated employment biographies (IEB) to complement the employment labour market survey questions with more reliable individual administrative records. The IEB consist of all individuals in Germany who are characterised by at least one of the following employment status: employment subject to social security (in the data since 1975), marginal part-time employment (in the data since 1999), benefit receipt according to the German Social Code III or II, officially registered as job-seeking at the German Federal Employment Agency or (planned) participation in programs of active labour market policies (in the data since 2000). The social security notifications are filled by the employer for each employment relationship. Unique establishment identifiers allow collapsing the employee level (e.g. Establishment History Panel (BHP)) and to link workers, establishments and surveys (e.g. Linked-Employer-Employee-Data (LIAB)). The IEB is a comprehensive data set with no attrition and daily precision.

While the IEB data can only be linked to the survey questions for a 70% subset of our sample for a total of 1625 individuals and we therefore rely on employment outcomes in the survey data in our primary analyses, the more precise job market data with a longitudinal character adds three major components. First, it provides us with the exact date of the first job refugees took up in Germany, allowing us to address the question of the timing of employment uptake in greater detail. Second, the linkage allows us to follow refugees even when they leave the survey, mitigating attrition concerns. Finally, it also allows us to obtain information on refugees' pre-survey (un)employment history.

3.3 Further data sources

All survey information are further linked to the Uppsala Conflict Data Program and to the Syrian Shuhada Martyr Revolution database on the province-month level. These databases report aggregate number of fatalities by province and month between 2011 and 2019 for all countries of origin found among the refugee population in Germany. We use these data to calculate control variables for potentially traumatic experiences in the origin, which we proxy in two steps. First, we construct a province-specific conflict-related death count in the province of origin, defined as the three months rolling average of conflict related fatalities prior to departure. As argued by Aksoy and Poutvaara [2019], simple province-level death counts may not adequately capture conflict intensity, as all variation in the variable may come from few historically war-ridden countries with substantially different institutional settings. We follow their approach and calculate a second measure of conflict intensity the following way:

$$ConflictIntensity_{c,t-\mu} = \frac{\sum_{m=1}^{3} TotalDeaths_{c,t-\mu-m}}{3}.$$

For each country and month $t - \mu$, we then calculate the median conflict intensity, M, of all provinces and create three categories:

No conflict: All individuals departing at $t - \mu$ for whom $ConflictIntensity_{c,t-\mu} = 0$
Low conflict: All individuals departing at $t - \mu$ for whom $ConflictIntensity_{c,t-\mu} < M$
High conflict: All individuals departing at $t - \mu$ for whom $ConflictIntensity_{c,t-\mu} \geq M$

Thus, the conflict intensity measure is calculated based on within-country conflict variation over time[3]. The calculated variables are summarised in table 4.

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**
No conflict	0.096	0.295	0	1
Low conflict	0.362	0.481	0	1
High conflict	0.542	0.498	0	1
N		2314		

Table 4: Conflict intensity

3.4 Victimisation along the migration route to Germany

To the best of our knowledge, no systematically gathered statistics exist on the hardship asylum seekers endure during their journey to Europe. In 2014, the International Organisation of Migration's (IOM) "Missing Migrant" project started tracking fatalities along the main migration routes but the database does not feature information on survivors. Two findings from observational studies are relevant to our study. First, victimisation is frequent and leaves asylum seekers mentally scarred. Arsenijević et al. [2017] evaluate data on 992 asylum seekers on their way to Europe collected from Médecins sans Frontières' mobile mental health clinics in Serbia in 2015 and 2016. Almost a third of the asylum seekers transiting through Serbia was classified as physically traumatised by the medical staff. Second, perpetrators of violence are both state actors and people smugglers asylum seekers rely on to cross borders and the Mediterranean Sea [Albahari, 2018, Arsenijević et al., 2017]. Less is known about individual and circumstantial factors that lead to victimisation on the journey to Europe and to what extent these events are of random nature. Anecdotal evidence strongly suggests that crime victimisation is not limited to groups traditionally seen as vulnerable. For example, Arsenijević et al. [2018] show that young male migrants are often the victims of physical

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

assault and suffer severe mental health problems due to these events.

To shed more light on victimisation events endured by asylum seekers during their journey to Europe, we start out by presenting more detailed descriptive statistics comparing group characteristics of non-victimised, physically victimised and financially victimised migrants. We exploit the rich set of questions relating to both pre-migration characteristics of refugees and information related to their escape available in the IAB-BAMF-SOEP refugee survey. We utilise this information to explicitly profile refugees who were physically or financially victimised on their journey to Germany vis-à-vis the non-victimised refugee population along seven key dimensions: a) individual-level baseline characteristics, b) countries of origin, c) individual health status, d) psychological characteristics, e) reasons for migrating to Germany, f) financing of the escape, and g) route characteristics. These profiles are presented as descriptive comparisons between victimised and non-victimised groups in table 5.

Table 5: Summary statistics by victimisation experience - pre-migration and journey

	No victimisation			***Physical victimisation***			***Financial victimisation***			***Delta-1***	***Delta-2***
	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Obs	Phys. victimised	Financ. victimised
Baseline characteristics											
Age at migration	30.79	10.47	1698	28.39	9.47	1282	29.64	9.99	1300	2.51***	0.56
Female	0.39	0.49	1797	0.29	0.45	1368	0.29	0.46	1381	0.09***	0.08***
Low education	0.56	0.50	1797	0.59	0.49	1368	0.53	0.50	1381	-0.04**	0.05**
Medium education	0.27	0.44	1797	0.26	0.44	1368	0.27	0.44	1381	0.01	0.00
High education	0.17	0.38	1797	0.15	0.36	1368	0.21	0.40	1381	0.04**	-0.05***
Econ situation bm: Below average	0.25	0.43	1720	0.25	0.43	1289	0.21	0.41	1326	-0.02	0.04**
Econ situation bm: Average	0.49	0.50	1720	0.45	0.50	1289	0.44	0.50	1326	0.04*	0.04**
Econ situation bm: Above average	0.26	0.44	1720	0.30	0.46	1289	0.35	0.48	1326	-0.01	-0.08***
Number Of Children	1.97	2.10	1779	1.69	2.12	1355	1.71	1.99	1369	0.26***	0.23**
Married	0.65	0.48	1796	0.57	0.49	1368	0.61	0.49	1381	0.08***	0.02
Good German before migration	0.02	0.15	1797	0.01	0.10	1368	0.01	0.11	1381	0.01*	0.01
Region of origin											
Syria	0.56	0.50	1797	0.51	0.50	1368	0.60	0.49	1381	0.08***	-0.06***
Iraq or Iran	0.17	0.37	1797	0.14	0.35	1368	0.14	0.35	1381	0.02	0.02
Afghanisan or Pakistan	0.08	0.27	1797	0.17	0.38	1368	0.13	0.34	1381	-0.09***	-0.02
Sub-Saharan Africa	0.04	0.19	1797	0.11	0.32	1368	0.08	0.27	1381	-0.08***	-0.02**
Other	0.15	0.36	1797	0.07	0.25	1368	0.05	0.23	1381	0.06***	0.08***
Health status											
Health before migration (1-10)	8.20	2.63	1766	8.03	2.74	1337	8.14	2.59	1354	0.21*	0.04
Psychological measures											
Life satisfaction before migration	6.78	2.99	1769	6.55	3.21	1334	6.85	3.09	1355	0.35***	-0.12
Willingness to take risk	4.43	3.38	1695	4.69	3.43	1307	4.75	3.39	1324	-0.18	-0.29*
Resilience	24.96	3.54	1534	25.02	3.22	1210	25.03	3.30	1247	0.00	-0.02
Reasons for migration											
War or forced recruitment	0.81	0.39	1788	0.86	0.34	1363	0.89	0.31	1375	-0.03**	-0.08***
Persecution and discrimination	0.58	0.49	1788	0.66	0.47	1363	0.71	0.46	1375	-0.05**	-0.12***
Economic reasons	0.44	0.50	1788	0.49	0.50	1363	0.52	0.50	1375	-0.03*	-0.08***
Family and friends	0.22	0.41	1788	0.22	0.42	1363	0.23	0.42	1375	-0.00	-0.01
Other reason	0.12	0.32	1788	0.15	0.36	1363	0.14	0.35	1375	-0.03*	-0.01
No conflict region	0.18	0.38	1514	0.17	0.38	1153	0.14	0.34	1179	-0.02	0.04**
Low conflict region	0.33	0.47	1514	0.34	0.47	1153	0.35	0.48	1179	0.01	-0.02
High conflict region	0.49	0.50	1514	0.49	0.50	1153	0.51	0.50	1179	0.01	-0.02
Financing the escape											
Paid an escape agent	0.67	0.47	1148	0.86	0.35	1020	0.87	0.34	1015	-0.13***	-0.14***
Costs for Escape Agent on Escape in Euro	3174.26	5483.30	1144	4597.98	6180.03	1017	4976.13	6928.30	1014	-776.68**	-1400.38***
Escape funded by credit	0.07	0.25	1797	0.09	0.29	1368	0.10	0.30	1381	-0.02	-0.03**
Escape funded by savings	0.50	0.50	1797	0.49	0.50	1368	0.53	0.50	1381	0.02	-0.04**
Escape funded by selling assets	0.42	0.49	1797	0.50	0.50	1368	0.55	0.50	1381	-0.04*	-0.12***
Escape funded by relatives	0.30	0.46	1797	0.35	0.48	1368	0.34	0.47	1381	-0.05**	-0.03*
Escape funded by friend	0.09	0.29	1797	0.14	0.35	1368	0.12	0.33	1381	-0.04***	-0.02
Journey features											
Duration of journey	34.50	81.28	1755	61.25	102.67	1334	54.88	95.29	1354	-24.60***	-14.70***
Deviation from expected journey duration	-3.66	69.65	1754	9.00	83.19	1334	6.48	83.93	1354	-11.93***	-8.03**
Central Med: Sea route	0.06	0.24	1235	0.17	0.38	965	0.13	0.33	983	-0.11***	-0.03**
Western Med: Sea route	0.01	0.09	1235	0.01	0.07	965	0.01	0.11	983	0.01	-0.01
Eastern Med: Sea route	0.70	0.46	1235	0.71	0.45	965	0.73	0.45	983	0.01	-0.02
Eastern Med: Land route	0.12	0.32	1235	0.13	0.33	965	0.13	0.34	983	-0.01	-0.01
By plane	0.26	0.44	1797	0.19	0.39	1368	0.22	0.41	1381	0.07***	0.02
Arrived alone	0.21	0.41	1795	0.32	0.47	1368	0.28	0.45	1381	-0.11***	-0.04**

The column Delta-1 shows the difference between non-physically victimised and physically victimised individuals. The minuend includes non-victimised and financially victimised migrants. The column Delta-2 shows the difference between non-financially victimised and financially victimised individuals. The minuend includes non-victimised and physically victimised migrants.

It is first worth noting that the 36% of asylum seekers reporting physical victimisation as defined in the previous section is slightly above the share found by Arsenijević et al. [2017] whose data gathered in Serbia exclude the final stage of the journey to Germany. A further 39% of asylum seekers report financial victimisation on their journey and 46% of asylum seekers did not experience criminal victimisation, implying some overlap between physical and financial victimisation events.

Table 5 suggest that the group composition of non-victimised and victimised migrants differ along a range of dimensions. In summary, the victimised groups show some signs of being less likely to show features generically considered vulnerable. Victimised migrants are on average younger, less likely to be female and have fewer children. The victimised groups also show characteristics that can be classified as greater determination to reach Germany: On average, both physically and financially victimised asylum seekers spend larger amounts on their escape, take more perilous sea routes to Germany instead of arriving by plane, travel for longer and are more likely to travel alone. The differences we observe around the geography of victimisation are intuitive: For example, asylum seekers from Syria are likely to be at a higher risk of victimisation on their journey to Germany than an asylum seeker from Kosovo simply due to the fact that Syrians have to travel a longer distance and spend more time exposed to potential perpetrators of violence. Similarly, victimisation rates will intuitively differ by travel route and means of transport: For instance, flying into Germany from Syria is safer than crossing the Eastern Mediterranean Sea in a boat provided by an escape agent. Both the geography and route chosen may therefore reflect underlying financial constraints, which in turn correlate with other socio-economic characteristics. Finally, the timing of migration may further contribute to the observed differences as it likely reflect selection effects at different expected victimisation rates: For instance, relatively vulnerable migrants may choose not to migrate when the risk of getting victimised is high.

To systematically analyse these issues, we proceed as follows. We first show that, after balancing on the geographic origin, the migration route taken and the timing of migration, observed average group characteristics between victimised and non-victimised are very similar. We then use these first descriptive insights to structure our thinking regarding the empirical strategy when studying the effect of individual-level victimization during the journey on economic integration outcomes of refugees in Germany.

Table 6 shows a balance test conditional on the selection dynamics outlined above, under the null hypotheses that individual-level characteristics do not predict victimisation events. To test these multiple hypotheses, the physical victimisation indicator and the financial victimisation indicator are regressed on a set of backward reported pre-migration indicators respectively, conditional on their geographical origin, the time of migration (and their interaction term), and the migration route. Physical victimisation is further conditioned on having experienced financial victimisation, while financial victimisation is conditioned on the experience of physical victimisation. The regression outcomes are shown in column (1) and (2). Column (3) and (4) then also condition on the the time of arrival.

	(1)	(2)	(3)	(4)
	Physical Victim.	Financial Victim.	Physical Victim.	Financial Victim.
Female	0.00228	-0.0182	0.00346	-0.0172
	(0.0245)	(0.0268)	(0.0246)	(0.0268)
Age	0.00183	-0.00335	0.00158	-0.00385
	(0.00630)	(0.00662)	(0.00631)	(0.00663)
Age squared	-6.21e-05	1.31e-05	-5.93e-05	1.77e-05
	(8.21e-05)	(8.71e-05)	(8.22e-05)	(8.72e-05)
Willingness to take risk	0.00110	0.00325	0.00110	0.00338
	(0.00310)	(0.00327)	(0.00310)	(0.00327)
Resilience	-0.00163	-0.000257	-0.00165	-0.000376
	(0.00310)	(0.00329)	(0.00310)	(0.00330)
Life satisfaction BFM	-0.00158	0.00842*	-0.00167	0.00803*
	(0.00473)	(0.00484)	(0.00473)	(0.00486)
Health satisfaction BFM	-0.00340	-0.0187***	-0.00330	-0.0185***
	(0.00514)	(0.00527)	(0.00515)	(0.00527)
Employed BFM	0.0265	0.0583**	0.0271	0.0579**
	(0.0270)	(0.0285)	(0.0270)	(0.0285)
Education: Secondary	0.0238	0.0914***	0.0229	0.0882***
	(0.0280)	(0.0293)	(0.0280)	(0.0293)
Education: Vocational	-0.0379	-0.0199	-0.0392	-0.0218
	(0.0408)	(0.0427)	(0.0408)	(0.0427)
Education: Tertiary	-0.0471*	0.0636**	-0.0490*	0.0581**
	(0.0263)	(0.0291)	(0.0265)	(0.0291)
German skills BFM: Good	-0.165*	-0.193**	-0.159	-0.171*
	(0.0984)	(0.0879)	(0.0991)	(0.0887)
Economic Situation BFM < Avg	0.0231	-0.0103	0.0225	-0.00998
	(0.0289)	(0.0303)	(0.0289)	(0.0303)
Friends helped to move	-0.0404	-0.0829***	-0.0399	-0.0868***
	(0.0253)	(0.0268)	(0.0253)	(0.0267)
Arrived alone	0.00759	-0.00336	0.00817	-0.00503
	(0.0266)	(0.0279)	(0.0266)	(0.0279)
Observations	2,314	2,314	2,314	2,314
R-squared	0.229	0.158	0.230	0.163
Country of origin x Y-M left origin FE	Yes	Yes	Yes	Yes
Migration Route FE	Yes	Yes	Yes	Yes
Cohort FE	No	No	Yes	Yes
Physical Victimisation	Yes	No	Yes	No
Financial Victimisation	No	Yes	No	Yes

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

Note: BFM stands for backward reported "before migration" information. The term FE indicates fixed effects. The term cohort refers to the year-month of arrival in Germany. Willingness to take risk, Life satisfaction BFM and Health satisfaction BFM are measured on a scale from 1 (low) to 10 (very high). The baseline category for the education variable is below secondary education. The baseline category for the dummy variable "German skills BFM: Good" is no or very limited German skills before migration. The baseline for the dummy variable "Economic Situation BFM < Avg" is "Economic Situation BFM > Avg" and refers to the economic situation of individuals compared to the population in their country of origin. "Friends helped to move" and "Arrived alone" are dummy variables capturing if migrants received help from friends during their migratory journey and whether they arrived alone respectively.

Table 6: Conditional balance test

The results show a much more balanced sample for the physically victimised (columns 1 and 3). We note that, on average, tertiary educated individuals and those who spoke German before their forced displacement are slightly less likely to fall victim to physical attacks. This difference is only statistically significant at the 10 percent level (1), is no longer visible for the German language indicator when conditioned on cohort fixed effects (3) and could be the result of multiple hypotheses testing. Nevertheless, our estimations will routinely control for these variables. Experiencing financial victimisation, on the other hands is unsurprisingly correlated with indicators that identify the wealth status of individuals (columns (2) and (4)). Higher levels of education and employment before migration both predict financial victimisation events, likely due to the necessary condition of initial financial endowment to suffer from financial losses. Our data allows us to control for a wide range of pre-migration wealth indicators, mitigating the issue. Similarly, individuals who could rely on friends to help with their migratory journey - and are therefore less likely to be involved with escape agents - show a lower probability of victimisation on average. Finally, life and health satisfaction are further significant predictors of financial victimisation, albeit in opposing directions. Our suggestive interpretation is that life satisfaction is correlated with financial victimisation via its link to financial endowment, whereas individuals of relatively worse pre-migration health are easier targets for criminal actors. In sum, we conclude that while the sample of physically victimised migrants is balanced along a wide range of individual level pre-migration characteristics once conditioned on geography and the timing of migration, financial victimisation events occur less randomly. While our data allows us to control for a large set of potentially confounding variables to mitigate the problem, we deploy several additional tests, outlined in 4.4, to study the significance of unobserved factors that could bias our results.

4 Empirical strategy

As discussed in the previous section, the key assumption of the analyses in this study is that the victimisation of individuals during their journey to Europe, when conditioned on the right set of covariates, is a random event that affected individuals have no control over. Three empirical problems pose a risk when trying to interpret the unconditional effect of individual-level victimization on integration outcomes: First, a non-random selection at the country of origin at different levels of expected victimisation may lead specific groups of asylum seekers to choose Germany as their destination (selection at origin effect). If these groups show characteristics such as unobserved motivation that also causally impact on integration outcomes, estimated coefficients on the effect of victimisation pick up bias. Second, a potential bias may arise from the fact that only those who succeed in making it to Germany are observed in our sample. This group may represent a sub-group of, for instance, particularly motivated or resilient individuals (survivor bias). Finally, victimisation itself could be correlated with other unobserved factors that determine the integration in Germany (omitted variable bias). If, for instance, unobserved intelligence lowers the probability of victimisation and simultaneously improves economic decision-making in the destination, coefficients estimated on the victimisation indicator could pick this up. We describe our strategy to deal with these different empirical design challenges in detail in section 4.

We start by laying out our baseline specification in section 4.1 that estimates the effect of victimisation of asylum seekers along the route to Germany on their economic integration outcomes at the internal and external margin. The subsequent subsections 4.2, 4.3 and 4.4 explain the extensions.

4.1 Baseline specification

The rich set of background information available from the IAB-BAMF-SOEP survey data allows us to control for pre-migration and selected post-migration characteristics that are usually unobserved in migration studies. This allows us to reduce some concerns related to potential unobserved variable bias.

To identify the effect of potentially traumatic events occurring during the journey to the destination on economic integration outcomes, we start out by estimating the following empirical model:

$$\begin{aligned} Y_{i,f,c,t} =& \gamma_1 PhysicalVictim_i + \gamma_2 FinancialVictim_i + \zeta BaselineCharacteristics_{i,t} \\ &+ \eta PreMigCharacteristics_{i,\mu} + \rho ConflictIntensity_{i,\mu} + \theta_1 Route_{i,\mu} \\ &+ \theta_2 AsylumStatus_{i,t} + \theta_3 MSM_{i,t} + \theta_4 MSMsq_{i,t} + \alpha_c + \delta_f + \tau\hat{\lambda}_i + \epsilon_{i,f,c,t} \end{aligned} \tag{1}$$

where $Y_{i,c,t,f}$ captures the health or economic integration outcome of interest for individual i from country of origin c interviewed at time t who resides in the German federal state f. Both γ_1 and γ_2 are the coefficients estimated on the variables of interest, $PhysicalVictim_i$ and $FinancialVictim_i$.

$BaselineCharacteristics_{i,t}$ is a vector of individual level characteristics. It includes refugees' age, age squared, a dummy for female refugees, a measure of mental resilience and willingness to take risk and a set of dummies referring to individuals' educational attainment (ISCED-2011 categories).[4] We include mental resilience as a control since more resilient individuals might be better able to cope with distressful life events. Willingness to take risks is controlled for to account not only for self-selection into migration but also because of its confounding effect on time preferences. For the labour force participation, education and health related outcomes we include two categorical variables related to the residence of the spouse and to the location of the children.[5] For the health related outcomes, we further include a continuous variable reflecting the satisfaction with living arrangements, measured on a scale from 1 to 10.

$PreMigrationCharacteristics_{i,\mu}$ is a vector of individual level pre-migration characteristics measured at departure μ. These include information on the economic situation, knowledge of German, employment experience and backward reported measures of health and life satisfaction before migration. It further includes a dummy variable taking the value one for individuals who arrived alone in Germany, and zero otherwise. Finally, it includes a dummy

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

[5]The residence of the spouse variable contains the following categories: Single; the spouse lives in the same household, the spouse lives in a different household in Germany; the spouse resides abroad. The variable related to the location of children contains the following categories: No children; all children live in the same household; some children live in a different household.

variable that takes the value 1 for individuals who had help from relatives or acquaintances when moving to Germany.

The categorical variable $Route_{i,\mu}$ indicates the migratory route taken as defined in section 3.1. $ConflictIntensity_{i,\mu}$ measures the conflict intensity in the province of origin measured on within-country conflict variation as explained in section 3.3. We include the conflict intensity control in the baseline specification for two reasons: First, to account for previous findings from psychological research showing that the individual-specific response of victimisation depends on previous traumatic experiences [Yehuda, 2002, Breslau et al., 2008]. Second, to add further precision to our estimates by accounting for selection into migration dynamics at different levels of push factors at the origin [Aksoy and Poutvaara, 2019, Guichard, 2020]. $AsylumStatus_{i,t}$ is a time varying individual level characteristic, measured at the time of the survey t. It is a fixed effects term with four categories: "Asylum granted", "Temporary suspension of deportation", "Request to leave Germany" and "Decision pending". Only the first two give refugees unrestricted access to the labour market in Germany, an institutional feature we discuss in more detail in section 7.1.

MSM is the number of months a refugee spent in Germany and $MSMsq$ is its squared term. $\hat{\lambda}_{i,c,t}$ is an estimated Heckman correction term if the outcome requires individuals to be employed. α_c captures country of origin fixed effects and δ_f reflects the German federal state in which the refugee is residing at the time of the survey. We include the latter purely to gain precision as refugees are not allowed to move freely and are assigned to a federal state initially upon arrival [Aksoy et al., 2020]. Finally, $\epsilon_{i,f,c,t}$ is an error term. All standard errors are obtained using delete-cluster jackknife methods due to the estimated (unknown) $\hat{\lambda}_i$ if the Heckman correction term is included.[6] In the results section we refer to the set of controls included in 1 as "Baseline controls".

Since the survey has a longitudinal dimension but our variable of interest is non-time-varying, we estimate equation 1 in two ways. First, we estimate the model as a cross-section, (i) only using the first observation available of each individual to study the effect of physical and financial victimisation on outcomes related to the (mental) health and well-being. We use the first observation available to study these outcomes for two reasons. First, when refugees were interviewed for the first time, they had spent only 19 months in Germany on average. Thus, their mental well-being related outcomes can be expected to still be affected by negative experiences during the journey to Germany or in their home country. Second, the potential reverse causality problem of mental well-being and employment is minimized [Brown et al., 2010, Kassenboehmer and Haisken-DeNew, 2009]: Only 7% of our sample were employed in the month prior to the interview when first surveyed. We then (ii) use the last observation in the sample to study the effect of victimisation on economic integration outcomes. At this point, individuals had spent an average of 31 months in the country and 21.8% were employed. While the average difference between the first and last observation of each individual is therefore only 12 months -and thus concerns about potential sample attrition due to selective return migration are minimized- the additional variation we gain in our outcomes of interest is considerable.

[6] The industry standard here is to cluster-bootstrap standard errors. Due to the large amount of fixed effects in the regression, bootstrapping becomes computationally impossible. Note that the delete-cluster jackknife method produces slightly more conservative standard errors than bootstrapping [Efron, 1992]. The estimated Heckman correction term is only included if statistically significant in the second stage.

In a second step, we then also exploit the panel variation in the data and estimate a (individual i) random effects model under the assumption that $corr(\epsilon_{i,f,c,t}, X) = 0$.[7] The large number of time constant variables in the model - including the set of fixed effects related to the time of migration and the origin of individuals - makes this key assumption of a random effects model plausible in our setting [Wooldridge, 2010]. We note that, since all asylum seekers naturally start their stay in Germany as economically inactive and the likelihood of engaging in economic activity then increases over time, the panel estimates are not directly comparable to the cross-sectional estimates based on only the final observation of each individual.

4.2 Self-selection into migration to Germany at the origin

One of the key concerns in equation 1 as noted in section 3.4 is the bias the coefficients γ_1 and γ_2 may pick up due to potential selection effects at the country of origin. In our setting, selection at the origin relates to the concept of migration at different levels of expected victimisation risk. Since (past) cohort level victimisation rates can be understood as a indirect measure of the (expected) journey risk, these findings require careful consideration in our empirical strategy. We first note that limited evidence on the self-selection of forced migrants at the origin at different expected journey risk levels has started to emerge in recent academic literature. Aksoy and Poutvaara [2019] provide suggestive evidence that intended destinations change when country-specific risk levels are altered through stricter migration policies, with potential consequences for the cohort composition. The authors further show that a higher conflict intensity at the origin leads to self-selected migration of more highly educated asylum seekers, in particular among female migrants. It follows that at an increased expected journey risk, which can also be understood as an increase in migration cost, self-selection may become even more salient. Guichard [2020] further supports these findings. The author shows that liquidity constraints drive a positive self-selection of asylum seekers with respect to education for individuals from countries geographically distant to Germany, such as Afghanistan, Iraq and Syria.

Despite controlling for individuals' willingness to take risk, the route travelled and conflict intensity at origin around the time of migration in our baseline specification, the concerns around self-selection into migration at the origin can most efficiently be tackled by the use of a large set of fixed effects related to the country of origin and granular time of migration. The detailed information on migrants' journeys we obtain from our surveys allows us to add interactive country-of-origin by year-month-of-migration fixed effects, $\kappa_{c,m}$ to equation 1. We label this specification the Fixed Effects specification in our regression tables and, due to its efficiency in eliminating self-selection dynamics, refer to it as our preferred specification throughout this study.

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

4.3 Survivor bias

Not all forcefully displaced migrants who decide to embark on the journey to Germany make it to their preferred destination. If the selection of asylum seekers who eventually reach their targeted destination is a random subset of those individuals that initially decided to migrate there, selection during the journey would not be an empirical concern when studying the effect of victimisation during the journey on integration outcomes as long as self-selection at the origin is accounted for. If granular country of origin by time of departure fixed effects are integrated as suggested in section 4.2, these would absorb random shocks to migration cohorts: Even if specific cohorts then show relatively higher or lower victimisation rates, the restriction to within-departure-cohort variation would prevent estimates on variables of interest to pick up systematic bias.[8]

However, changes in the difficulty of the journey may potentially have non-random effects on the arrival cohort composition, even when narrowly conditioning on the selection at the origin at different points in time. We refer to this empirical issue as survivor bias. In theory, such change in the composition of asylum seeker arrival cohorts can influence not only the probability of victimisation but also their performance on the German labour market. While we are again able to mitigate this concern by controlling for a large set of observable characteristics discussed in section 3.4, unobservable compositional arrival cohort changes may introduce bias in regressions of integration outcomes on individual level victimisation experiences.

To the best of our knowledge, no previous research exists that could inform our empirical strategy with regards to survivor bias and the extent to which it is a concern in our setting. Empirically, we therefore address the issue of survivor bias by deploying a large set of dyadic departure - arrival fixed effects in robustness tests. In addition to our preferred specification suggested in section 4.2, we thus estimate a model that includes year-month of arrival fixed effect, interacted with the year-month of departure and region of origin $\varrho_{c,m,a}$, thereby limiting the variation to even narrower categories within the region of origin, the month of departure and the month of arrival. Due to the inevitable loss in degrees of freedom, we include broader categories of migrants' origin as shown in table 5 in these interactive fixed effects, in addition to the country of origin fixed effects.

4.4 Further methods to address omitted variable bias

The rich set of background information available from the IAB-BAMF-SOEP survey data allows us to control for pre-migration and selected post-migration characteristics, mitigating potential unobserved variable bias. Deploying a large set of fixed effects as explained in 4.2 and 4.3 allows us to further capture institutional changes and cohort characteristics that could otherwise bias the coefficients estimated on our variables of interest. A standard drawback of including a large set of fixed effects is a loss of statistical precision, which could lead to discarding estimated effects that show no significance at conventional statistical levels.

[8]The effect would be visible in changes in the number of arrivals at any given time but since our focus is on the comparisons at the individual level, this does not impact our estimation strategy.

One way to more efficiently deal with the bias-variance trade-off while mitigating concerns around theory-driven model specifications is the use of machine learning methods for data-driven model selection. To test the sensitivity of our results to our modelling choice, we therefore follow Belloni et al. [2014b] who develop a 'post-double selection' (PDS) method to estimate separate least absolute shrinkage and selection operator (LASSO) regressions to find predictors of the selection equation(s) and the outcome equation when many potential controls are available. Their procedure thus allows us to consider all interactions and non-linearities that are not considered in theory-driven specifications. Hence, instead of simply including the control variables as in 1, the PDS approach considers all possible interactions and non-linearities between the control variables and the country of origin and time of departure fixed effects. Furthermore, we consider a set of additional control variables we did not include previously due to their large number of missing values. These variables include: The use of a smuggler, the log of the cost of the smuggler, the financing of the journey (e.g., sale of assets, borrowing, savings, among others), means of transportation used to reach Germany (e.g., boat, car, foot, train, plain), self-reported reason for migrating (e.g., persecution, discrimination, economic, among others), having stayed in another country for three or more months before coming to Germany, and the log of the duration of the journey in days.

We apply the PDS methodology to our two variables of interest, $PhysicalVictim$ and $FinancialVictim$ and proceed in a post-triple selection procedure: In a first step, we estimate the final outcome equation using LASSO, excluding both the $PhysicalVictim$ and the $FinancialVictim$ variable to obtain a first set of LASSO selected controls. In a second step, we estimate the probability of physical and financial victimisation separately on the same set of explanatory variables to obtain a second and third set of LASSO selected controls. Finally, in a third step, we estimate a linear model similar to our baseline equation 1 that includes the union of control variables selected by LASSO in the various steps. A more technical explanation of the procedure is provided in appendix B.

Despite controlling for a wide range of relevant pre-migration and selected post-migration variables as well as accounting for selection effects at various stages of the migratory journey, omitted variable bias cannot be ruled out with absolute certainty. We therefore follow Oster [2019] to test if controlling for observables, and the stability of the estimated coefficients on our variables of interest when conditioning on these, mitigates bias arising from unobservable factors. The technique suggested by Oster [2019] thus informs us about the salience of omitted variable bias in our setting: It provides an estimate of the relative importance of unobserved factors compared to those we do observe. We implement her methodology for our preferred specification and first define a value for R_{max}, the hypothetical R-squared value of a fully specified model which includes all relevant control variables. Oster [2019] recommends a value of $R_{max} = 1.3\hat{R}$, where $\hat{R}$ is the R-squared value obtained from the estimated model; we choose a slightly more conservative approach and set the R_{max} to $1.5\hat{R}$. In a second step, we then obtain the δ that informs us about the relative importance of omitted variables compared to those variables we condition our estimates on: For example, a value of $\delta = 1$ means that unobserved factors would have to be as important as those that are observed for γ_1 and γ_2 of equation 1 to equal zero instead of the obtained estimate. We report the estimated δ for our preferred specification in section 5.3.

5 Results

In this section, we first discuss the main results which link victimisation on asylum seekers' journey to Germany to the (mental) well-being outcomes and the economic integration at both the external and the internal margin in section 5.1. In section 6, we conduct a wide range of robustness tests. Finally, in section 7, we rule out various alternative explanations to the suggested "loss of future directedness" channel.

Two tradeoffs need to be balanced in our regression analyses. First, the large amount of fixed effects we introduce based on our analyses in sections 4.2 and 4.3 absorb a lot of the variation in our outcomes. They effectively deal with bias due to selection effects but lead to a loss in statistical precision. Second, some of the information on the arrival time we require to construct the dyadic time of departure - month of arrival fixed effect is not available for all individuals, decreasing the sample size. Our analyses throughout the paper suggest that these tradeoffs are best balanced by adding fixed effects on the country of origin, the time of departure and their interaction as suggested in section 4.2. We therefore refer to this specification as our preferred specification. All other regressions discussed in the previous section are nevertheless shown for completeness and robustness. We further show the estimated coefficients for all additional control variables of our preferred specification in appendix D.

5.1 Main results

We first turn to the results on the encompassing measure of mental well-being, life satisfaction, in table 7.

	Cross Section				Panel Data	
Life Satisfaction	Benchmark	Fixed effects	Dyadic FE	PDS	Benchmark	Fixed effects
	(1)	(2)	(3)	(4)	(5)	(6)
Physical victim.	-0.151	-0.166*	-0.228*	-0.214**	-0.109	-0.162**
	(0.0934)	(0.0977)	(0.129)	(0.100)	(0.0698)	(0.0739)
Financial victim.	-0.0984	-0.107	-0.193	-0.123	-0.0745	-0.102
	(0.0902)	(0.0966)	(0.125)	(0.0979)	(0.0680)	(0.0727)
Observations	2,261	2,261	1,711	2,112	4,864	4,864
R-squared	0.286	0.349	0.435			
C. of origin x Y-M left origin FE	No	Yes	Yes	Some	No	Yes
R. origin x Y-M left origin x Y-M arrival FE	No	No	Yes	No	No	No
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

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

The dependent variable captures self-reported life satisfaction on a scale from 1 to 10. Columns (1) to (4) use observations corresponding to first interview conducted, 19 months after arrival on average. Panel data results are derived from a random effects specification. The term FE indicates fixed effects. PDS refers to the post double-selection LASSO regressions. The term Y-M left origin refers to the year-month of forceful displacement from the home country. Y-M arrival refers to the year-month of arrival in Germany. C. of origin is the country of origin. Since the precise information on the year-month of arrival is not available for all individuals, the dyadic FE regressions are estimated only on the subset of individuals where this information is available. The PDS is estimated on the same sample as the Fixed Effects regression, but drops singleton observations.

Table 7: Life Satisfaction

Throughout specifications, the effect of physical victimisation on life satisfaction at the time of arrival in Germany is negative. All estimated coefficients are statistically significant at the 10% level or close to that threshold. In our preferred specification (column (2)), the magnitude of the coefficient is 0.166, which corresponds to a decrease of approximately 10% in the standard deviation of the measure. The estimated coefficients are stable across specifications. Financial victimisation also shows a negative effect on life satisfaction across specifications, but the estimated coefficients are smaller and less precisely estimated.

Turning to the self-reported health outcomes in table 8 confirms these results.

Health Satisfaction	Cross Section				Panel Data	
	Benchmark	Fixed effects	Dyadic FE	PDS	Benchmark	Fixed effects
	(1)	(2)	(3)	(4)	(5)	(6)
Physical victim.	-0.199*	-0.281**	-0.222	-0.271**	-0.175**	-0.267***
	(0.108)	(0.116)	(0.148)	(0.114)	(0.0846)	(0.0908)
Financial victim.	-0.212**	-0.183	-0.311**	-0.265**	-0.157*	-0.170*
	(0.106)	(0.114)	(0.147)	(0.113)	(0.0815)	(0.0875)
Observations	2,261	2,261	1,714	2,115	4,864	4,864
R-squared	0.267	0.328	0.414			
C. of origin x Y-M left origin FE	No	Yes	Yes	Some	No	Yes
R. origin x Y-M left origin x Y-M arrival FE	No	No	Yes	No	No	No
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

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

The dependent variable captures self-reported health on a scale from 1 to 10. Columns (1) to (4) use observations corresponding to first interview conducted, 19 months after arrival on average. Panel data results are derived from a random effects specification. The term FE indicates fixed effects. PDS refers to the post double-selection LASSO regressions. The term Y-M left origin refers to the year-month of forceful displacement from the home country. Y-M arrival refers to the year-month of arrival in Germany. C. of origin is the country of origin. Since the precise information on the year-month of arrival is not available for all individuals, the dyadic FE regressions are estimated only on the subset of individuals where this information is available. The PDS is estimated on the same sample as the Fixed Effects regression, but drops singleton observations.

Table 8: Health Satisfaction

In our preferred specification of column (2), physical victimisation decreases self-reported health by 0.281 points (p<.05), again corresponding to approximately 10% in the standard deviation of the measure. The estimated coefficient is stable across specifications. The negative effect of financial victimisation on self-assessed health is also visible across specifications, albeit being of smaller magnitude in out preferred specification (column 2). In summary, two main findings confirm the effect of victimisation on mental well-being and health established in previous studies on the general (non-refugee) population [Mahuteau and Zhu, 2016, Dolan et al., 2005, Johnston et al., 2018]. First, the event of victimisation has a negative effect on both measures. Second, physical victimisation leaves a stronger effect on the overall well-being of individuals and a marginally stronger effect on individuals' self-assessed health. In appendix A, we split the health measure into a physical and mental component and show that the overall result is driven by a combination of both.

Table 9 then turns to the effect of physical and financial victimisation on the external margin of economic activity, defined as those in the labour force or pursuing host-country specific education.

Labour force part. and Education	Cross Section				Panel Data	
	Benchmark	Fixed effects	Dyadic FE	PDS	Benchmark	Fixed effects
	(1)	(2)	(3)	(4)	(5)	(6)
Physical victim.	0.0378**	0.0382**	0.0362	0.0280	0.0314**	0.0373**
	(0.0177)	(0.0190)	(0.0253)	(0.0191)	(0.0135)	(0.0149)
Financial trauma	-0.0126	-0.0108	0.0128	-0.00895	0.00518	-0.00668
	(0.0175)	(0.0184)	(0.0244)	(0.0185)	(0.0135)	(0.0145)
Observations	2,314	2,314	1,754	2,159	4,864	4,864
R-squared	0.239	0.306	0.386			
C. of origin x Y-M left origin FE	No	Yes	Yes	Some	No	Yes
R. origin x Y-M left origin x Y-M arrival FE	No	No	Yes	No	No	No
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

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

The dependent variable captures all individuals in the labour force or those pursuing host-country education or training. Columns (1) to (4) use observations corresponding to the last interview conducted, 31 months after arrival on average. Panel data results are derived from a random effects specification. The term FE indicates fixed effects. PDS refers to the post double-selection LASSO regressions. The term Y-M left origin refers to the year-month of forceful displacement from the home country. Y-M arrival refers to the year-month of arrival in Germany. C. of origin is the country of origin. Since the precise information on the year-month of arrival is not available for all individuals, the dyadic FE regressions are estimated only on the subset of individuals where this information is available. The PDS is estimated on the same number of observations as the benchmark and the Fixed Effects regression, but drops singleton observations.

Table 9: Labour force participation and Education

We do not find a negative effect of victimisation during the journey on the external margin of economic activity in the cross-sectional regressions (1) to (4) estimated on individuals who, on average, had spent 31 months in Germany. In our preferred specification of column (2), physical victimisation even has a small positive effect on being economically active ($p<.05$). These results are further confirmed in the panel regressions (5) and (6). Since these panel regressions include observations of the same individuals at an earlier point in time (when individuals had spent 19 months in Germany on average) and the cross-sectional regressions do not include these, the stability of coefficients when comparing the cross-sectional and panel results is noteworthy; it appears that the gap in economic activity between physically and non-physically victimised already opens up at least 19 months after arrival. No such effect can be found for the financially victimised.

To shed more light on the drivers of this finding, table 10 shows the results of the regressions of labour force participation on our victimisation measures.

Labour force participation	Cross Section				Panel Data	
	Benchmark	Fixed effects	Dyadic FE	PDS	Benchmark	Fixed effects
	(1)	(2)	(3)	(4)	(5)	(6)
Physical victim.	0.0544***	0.0577***	0.0525**	0.0447**	0.0405***	0.0503***
	(0.0180)	(0.0193)	(0.0258)	(0.0191)	(0.0137)	(0.0150)
Financial victim.	-0.0109	-0.0119	0.0161	-0.00465	0.00885	-0.00822
	(0.0178)	(0.0188)	(0.0250)	(0.0186)	(0.0136)	(0.0147)
Observations	2,314	2,314	1,754	2,159	4,864	4,864
R-squared	0.245	0.310	0.392			
C. of origin x Y-M left origin FE	No	Yes	Yes	Some	No	Yes
R. origin x Y-M left origin x Y-M arrival FE	No	No	Yes	No	No	No
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

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

The dependent variable is binary and takes the value 1 for individuals in the labour force. Columns (1) to (4) use observations corresponding to the last interview conducted, 31 months after arrival on average. Panel data results are derived from a random effects specification. The term FE indicates fixed effects. PDS refers to the post double-selection LASSO regressions. The term Y-M left origin refers to the year-month of forceful displacement from the home country. Y-M arrival refers to the year-month of arrival in Germany. C. of origin is the country of origin. Since the precise information on the year-month of arrival is not available for all individuals, the dyadic FE regressions are estimated only on the subset of individuals where this information is available. The PDS is estimated on the same sample as the Fixed Effects regression, but drops singleton observations.

Table 10: Labour force participation

The coefficients on labour force participation show a strong, precisely estimated positive association of physical victimisation and joining the labour force across all specifications. In our preferred specification of column (2), this effect is estimated at 5.8 percentage points (p<.01). The effect remains visible and of only slightly smaller magnitude in the panel specifications (5) and (6), suggesting that physically victimised individuals indeed join the labour force sooner upon arrival. We do not find the same association between financial victimisation and labour force participation, where the estimated effect is close to zero across all specifications. Table 11 then shows the regression outcomes of the effect of victimisation on pursuing host-country education and training, the second part of what we coined the internal margin of economic activity.

Education and training	Cross Section				Panel Data	
	Benchmark	Fixed effects	Dyadic FE	PDS	Benchmark	Fixed effects
	(1)	(2)	(3)	(4)	(5)	(6)
Physical victim.	-0.0266**	-0.0297**	-0.0293*	-0.0244*	-0.00783	-0.0128
	(0.0116)	(0.0127)	(0.0168)	(0.0129)	(0.00804)	(0.00958)
Financial victim.	9.93e-05	-0.00098	0.00216	0.00464	-0.00674	-0.00328
	(0.0117)	(0.0124)	(0.0156)	(0.0126)	(0.00794)	(0.00911)
Observations	2,314	2,314	1,754	2,159	5,538	4,977
R-squared	0.115	0.209	0.301			
C. of origin x Y-M left origin FE	No	Yes	Yes	Some	No	Yes
R. origin x Y-M left origin x Y-M arrival FE	No	No	Yes	No	No	No
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

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

The dependent variable is binary and takes the value 1 for individuals pursuing host-country education or training. Columns (1) to (4) use observations corresponding to the last interview conducted, 31 months after arrival on average. Panel data results are derived from a random effects specification. The term FE indicates fixed effects. PDS refers to the post double-selection LASSO regressions. The term Y-M left origin refers to the year-month of forceful displacement from the home country. Y-M arrival refers to the year-month of arrival in Germany. C. of origin is the country of origin. Since the precise information on the year-month of arrival is not available for all individuals, the dyadic FE regressions are estimated only on the subset of individuals where this information is available. The PDS is estimated on the same sample as the Fixed Effects regression, but drops singleton observations.

Table 11: Education and training

By design, the results complement those of tables 9 and 10. Physical victimisation significantly decreases the propensity to pursue host-country specific education or training across all specifications. In our preferred specification of column (2), this negative effect reaches 2.97 percentage points, a very sizeable decrease considering that the total share of refugees in our sample pursuing education or training stands at 8.1 percentage points 31 months after arrival (see table 3). Three points are further noteworthy: First, the panel regressions of column (5) and (6) suggest that this effect only becomes visible after some time in the country. The estimated coefficients are smaller in magnitude and no longer distinguishable from zero when including observations on the same individuals closer to arrival. Second, the lower share of physically victimised refugees in education and training does not entirely close the gap to the higher labour force participation of the same group shown in table 10. In combination, these two observations suggest that the barriers to pursuing host-country education are higher and education opportunities open to refugees require more time to search for than joining the labour force. Finally, the coefficients estimated on the financial victimisation indicator again show no effects across all specifications.

In summary, our findings indicate that the physical victimisation event i) increases the propensity to join the labour force early on and ii) decreases the propensity to pursue host-country education and training. The results in tables 9, 10 and 11 thus suggest that the act of physical victimisation leads to a distortion in the timing of labour force entry, which appears to dominate the more general well-being related effects that would likely lead to lower economic activity rates. We interpret these findings as supportive of the "loss of future directedness" hypothesis.

In section 5.2, we further put this hypothesis to the test by explicitly i) considering whether the higher labour force participation rates indeed result in higher employment rates,

ii) analysing the type of employment victimised individuals engage in vis-à-vis the non-victimised and iii) shedding more light on the timing of first employment when comparing the different groups.

5.2 Employment outcomes

Our findings shown in section 5.1 strongly support the idea that the act of physical victimisation reduces future-oriented thinking among the affected. This section more explicitly considers the consequences of an early labour force entry: If the "loss of future directedness" was indeed a relatively stronger driving force within the group of the physically victimised, we would expect a lower reservation wage and thus a relatively higher take-up of readily available low-income employment among those who experienced physical victimisation events on their journey to Germany.

Table 12 shows regression results with different types of employment rates as the dependent variable for the full sample of refugees. We focus on the whole population as the underlying population - rather than only those in the labour force - to make these regressions directly comparable to those in table 10. We nevertheless report the same employment rates for only those in the labour force in appendix C. We further only report results for our preferred specification in both the cross-section and the panel in all subsequent analyses.

Employment	Any Employment		Full-Time		Part-Time or marginal		Log of Income	
	Cross S.	Panel D.	Cross S.	Panel D.	Cross S.	Panel D.	Cross S.	Panel D.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Physical victim.	0.0438**	0.0326**	0.0136	0.00931	0.0302**	0.0236**	-0.229	-0.128
	((0.0189)	(0.0134)	(0.0148)	(0.0104)	(0.0147)	(0.0106)	(0.146)	(0.108)
Financial victim.	00.0037	0.0010	0.0062	0.0039	-0.0025	-0.0028	0.0150	-0.00968
	(0.0185)	(0.0128)	(0.0145)	(0.00984)	(0.0142)	(0.0100)	(0.143)	(0.107)
Observations	2,314	4,864	2,314	4,864	2,314	4,864	408	750
R-squared	0.258		0.239		0.152		0.372	
C. of origin x Y-M left origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

The dependent variable is binary and takes the value 1 for employed individuals, with regressions showing employment outcomes for any employment ((1) and (2)), full-time employment ((3) and (4)) and part-time or marginal employment ((5) and (6)). All samples use observations corresponding to the last interview conducted, 31 months after arrival on average; Columns (7) and (8) are estimated on the sample of individuals employed after 31 months. Columns (1), (3), (5) and (7) show the results of a cross-sectional regression on the last interview conducted. Columns (2), (4), (6) and (8) are derived from a random effects panel specification. The term FE indicates fixed effects. The term Y-M left origin refers to the year-month of forceful displacement from the home country.

Table 12: Employment

The results in table 12 indeed suggest that the higher labour force participation among physically victimised is driven by an increased uptake in part-time and marginal employment vis-à-vis the non-victimised. Column (1) reports the excess employment rate of physically victimised refugees for the last observation available of each individual, at an average duration of stay in Germany of 31 months. It is 4.4 percentage points higher than the employment rate among the non-victimised at that point. The panel regression of column (2) - which again includes earlier observations of the same individuals when employment rates were closer to zero and estimates the average employment gap over time - confirms this result. In line with the "loss of future directedness" hypothesis, early employment uptake is characterised by a poor quality of jobs available to refugees. More than two thirds of the excess employment rates among the physically victimised is explained by employment in part-time and marginal jobs (columns (5) and (6)). Less than one third of the effect is explained by full-time employment, a magnitude that is no longer statistically distinguishable from zero at conventional levels (columns (3) and (4)). Column (7), estimated only on the sample of employed refugees, provides suggestive evidence that 31 months after arrival, these differences already result in a 22% wage gap between the non-physically-victimised and the physically victimised. We note that this difference is likely to increase in the future when the non-physically-victimised complete their training and education, an idea further supported by the slightly smaller coefficient estimated in the panel regression of column (8) where earlier observations are included in the sample.

We have so far not conclusively addressed the question of the timing of employment uptake of victimised individuals compared to the non-victimised: While comparing the cross-sectional (column 1) results, which only contain the last observation of each individual, to the panel results (column 2) of table 12 strongly suggests that joining the labour force early allows for faster access to part-time and marginal employment, the larger sample size in the panel regressions could also simply add statistical precision to the estimates. To shed more light on the timing of first employment in Germany, we turn to the linked employment biography data, which contains information on the date of first employment. Figure 1 shows the unconditional Kaplan-Meier curve of time to first employment, where failure is defined as obtaining employment and the x-axis shows the number of month since arrival in Germany. The analysis is based on a subsample of 1,625 survey respondents who gave their consent to be linked to administrative employment records. Of these individuals, 751 obtained employment at some point over the observation period; we note that this share is larger than the 21.8% in our cross-sectional regressions. The difference is explained by the IEB data extending beyond the last available survey wave. The cross-sectional regressions we presented so far thus correspond to the 31-months-point on the x-axis.

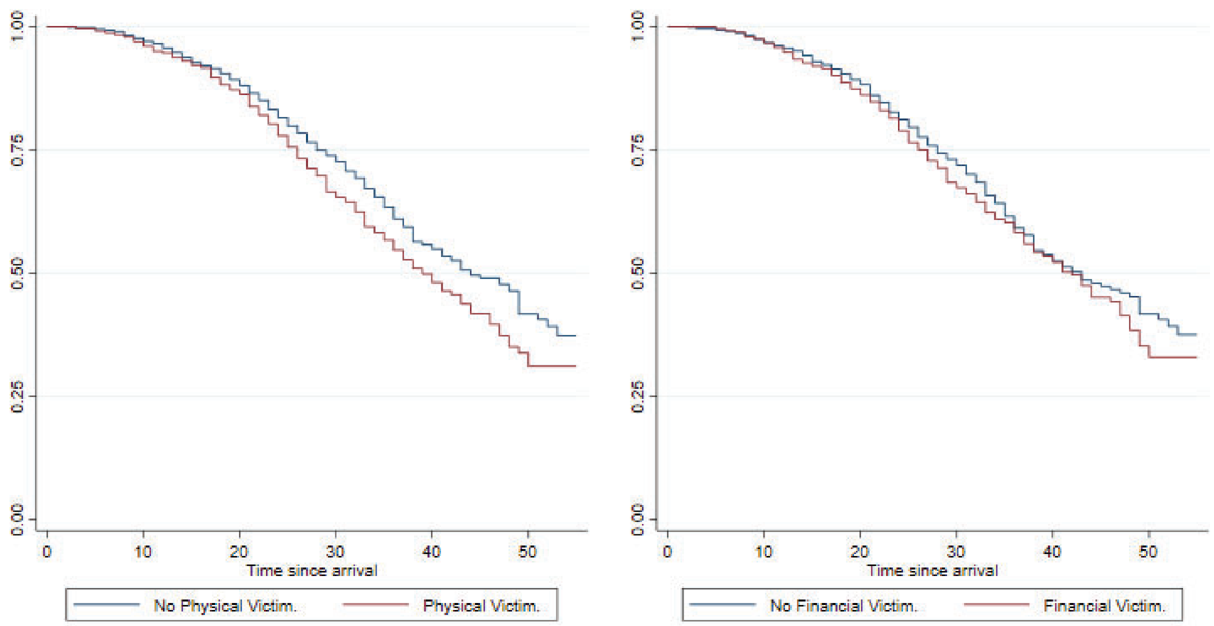

Figure 1: Kaplan-Meier estimates of time to first employment

The left hand-side graph in figure 1 shows that, compared to the non-victimised refugees, physically victimised refugees obtain employment faster. The gap starts to open around 18 months after arrival, a dynamic we explicitly analyse further in section 7.1. The right hand-side graph shows the same comparison for the financially victimised, where we again do not detect any effect. Table 13 further reports the estimated output of the simple cox proportional hazard model.

	Time to employment
Physical Victimization	0.209***
	(0.0782)
Financial Victimization	0.00746
	(0.0772)
Observations	1,625

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

Table 13: Cox proportional hazard model

The parameter estimates show the increase in the expected log of the relative hazard for the physical victimisation and financial victimisation groups vis-à-vis the non-victimised. Exponentiating the parameter estimates shows that the expected hazard, equal to finding employment, is 1.23 times higher for the physically victimised than the non-victimised on average. Although not causal, these results lend further support to the interpretation that physical victimisation events lead to a more present-oriented mindset that attaches more value to immediate payoffs.

We demonstrated in table 12 that early employment uptake is generally characterised by part-time and marginal employment (columns 5 and 6). The IEB contains further information on the level of task requirement for a subset of 569 of the 751 employed individuals in our sample, shown in table 14.

	No F.V.		F.V.		No P.V.		P.V.		Total	
	%	Obs	%	Obs	%	Obs	%	Obs	%	Obs
1 Unskilled/semi-skilled task	34.4	116	33.6	78	30.9	111	39.5	83	34.1	194
2 Skilled task	46.9	158	45.3	105	47.4	170	44.3	93	46.2	263
3 Complex task	4.7	16	3.9	9	5.6	20	2.4	5	4.4	25
4 Highly complex task	13.9	47	17.2	40	16.2	58	13.8	29	15.3	87

F.V. refers to financial victimization and P.V. to physical victimization

Table 14: Level of job requirement

The tabulation shows that physically victimized individuals seem to take up jobs with unskilled or semi-skilled task requirements at a higher frequency. Row 1 of table 14 shows that the excess share of physically victimised vis-à-vis the non-physically-victimised stands at 8.6 percentage points; on the other hand, the employed share of physically victimised in the skilled task, complex and highly complex task categories is relatively smaller. We note two limitations of this analyses. First, the sample size in most categories is small and should be interpreted with care. Second, since employment is measured at an early stage after arrival, it is likely that the returns to host-country education are not yet fully captured and will pay off at a later stage. Nevertheless, the analysis of skill-requirements for the jobs performed lends further support to the idea that faster employment uptake among the physically victimised is characterised by low-skill employment.

5.3 Testing for the significance of unobserved confounding variables

The key identifying assumption we make in this study is that once we condition on the geography and the timing of migration, and thus self-selection into migration at different expected victimisation levels, the coefficients we obtain on the victimisation variables themselves are unbiased. To strengthen that case, our main regressions further control for a wide range of individual level (pre-migration) socio-economic characteristics to mitigate the risk of omitted variable bias. A further test to assess the likelihood of unobserved confounders as the driving force behind our results was developed by Oster [2019]. In this section, we follow her methodology as laid out in 4.4 and estimate how large the effect of unobserved variables would have to be to obtain zero-coefficients on the victimisation indicators in our preferred specification.

Table 15 shows the estimated δ values corresponding to the results of our preferred specification in tables 7 (column (2)), 9 (column (2)), 10 (column (2)), 11 (column (2)) and 12 (column (1), (3) and (5)).

Table 15: Oster Test

	Life Sat.	Sat. Health	LFP& Educ	LFP	Educ	All employ.	FT employ.	Part-Time or marginal
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
δ (Physical victim.)	1.919	-12.32	1.990	2.697	36.51	1.572	0.588	7.009
δ (Financial victim.)	0.761	2.589	-0.338	-0.315	1.111	0.0389	0.254	-0.338
Observations	2,266	2,269	2,314	2,314	2,314	2,314	2,314	2,314
R-squared	0.352	0.331	0.306	0.310	0.209	0.297	0.239	0.178
C. of origin x Y-M left origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

We first turn to the test results obtained on physical victimisation events. All obtained δ values clearly indicate that the explanatory power of omitted variables would have to be very large compared to those variables included in the model for the estimated coefficients on physical victimisation to be zero. For example, in table 11, column (2), we estimate that refugees who were physically victimised on their journey to Germany were 3 percentage points less likely to be in education or training 31 months after arrival compared to the non-physically victimised. For the obtained coefficients to be zero instead, unobserved variables would have to be 36.5 times larger than those control variables included in the model. The only value below the $\delta = 1$ threshold recommended by Oster [2019] is the obtained coefficient obtained on full-time employment in table 12, column (3). However, the coefficient is not statistically significant from zero at any conventional level in our estimation and the result is therefore unsurprising. In sum, all test results suggest that the estimated effect of physical victimisation on integration outcomes are highly robust to omitted variable bias.

Unsurprisingly, almost all obtained δ values on the estimated financial victimisation coefficients lie below the $\delta = 1$ threshold: None of them are significantly different from zero in our main regressions at any conventional level, suggesting no effect of financial victimisation events during the flight to Germany on individuals' well-being and economic integration outcomes.

6 Robustness

In this section, we show the results using alternative ways of aggregating the victimization experiences in subsection 6.1 and for specific types of victimisation events in subsection 6.2. We then present a range of split-sample regressions to show that the main results are not driven by any particular subsample in the data. Subsection 6.3 shows the main results split by major countries of origin, subsection 6.4 splits the sample by gender,, and subsection 6.5 zooms in on different arrival cohorts.

6.1 Effects by sum of physical victimisation and sum of financial victimisation events

We start by showing that our results are robust to different specifications of the physical and financial victimization. In particular, one of our modelling choices in the analyses so far has been to code the victimisation events as binary indicators. For our integration outcomes, this choice implicitly assumes that once individuals had to endure a physical or financial victimisation event, additional victimisation events do not alter their well-being and behaviour further. In this subsection, we relax this assumption and explicitly consider the precise number of victimization events individuals endured. Table 16 summarises the number of physical (financial) victimisation events by the share of individuals who endured them. The acronym P.V.E. denotes physical victimisation events(s) and F.V.E. financial victimisation events(s).

Variable	Mean	Std. Dev.
Physical victimisation		
None	0.641	0.48
1 P.V.E	0.249	0.433
2 P.V.E	0.091	0.288
3 P.V.E	0.018	0.134
4 P.V.E	0.001	0.029
Financial victimisation		
None	0.61	0.488
1 F.V.E	0.24	0.427
2 F.V.E	0.114	0.318
3 F.V.E	0.035	0.185
N	2314	

Table 16: Summary statistics - number of physical and financial victimisation events

In table 17 we first turn to the regression results of our preferred specification using a linear and a squared measure of the number of physical victimization events, that ranges from zero to a maximum of four, and of the number of financial victimization events, that ranges from zero to a maximum of three.

	LFP & Educ. (1)	LFP (2)	Educ. (3)	All employ. (4)	FT employ. (5)	Part-Time or marginal (6)
Number of physical victim.	0.0221	0.0511*	-0.0427**	0.0598**	0.00665	0.0532**
	(0.0280)	(0.0287)	(0.0192)	(0.0293)	(0.0232)	(0.0230)
Number of physical victim. squared	0.00276	-0.00708	0.0133	-0.0195	0.00263	-0.0221**
	(0.0112)	(0.0117)	(0.00830)	(0.0128)	(0.0105)	(0.0101)
Number financial of victim	-0.000405	-0.00836	0.00648	0.0265	0.0256	0.000962
	(0.0282)	(0.0288)	(0.0197)	(0.0291)	(0.0230)	(0.0230)
Number financial of victim. squared	-0.00543	-0.000620	-0.00560	-0.0122	-0.0113	-0.000872
	(0.0116)	(0.0118)	(0.00823)	(0.0121)	(0.00967)	(0.00971)
Observations	2,314	2,314	2,314	2,314	2,314	2,314
R-squared	0.306	0.310	0.210	0.258	0.240	0.153
C. of origin x Y-M left origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

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

The dependent variable is binary and takes the value 1 for individuals in employment/labour force participation/education, and zero otherwise. FT means full-time. Results are only shown for our preferred specification, corresponding to column (2) in the main result tables.

Table 17: Number of victimization experiences (continuous)

The results confirm our main results but their interpretation changes. Column (2) now shows that any *additional* physical victimisation event increases the probability of joining the labour force by 5.1 percentage points, an effect again driven by the take up of marginal and part-time employment (column 6) and at the cost of not pursuing host-country education (column 3). We also note that the estimated coefficients on the squared number of victimisation events are close to zero and not statistically significant at any conventional level. Adding polynomials that allow for a more flexible curvilinear relation between victimisation events and integration outcomes are therefore likely to be unnecessary. Similar to our main results, we find no effect of financial victimisation events on economic integration outcomes in Germany.

In table 18 we then turn to the results where the different number of victimisation events enter as categorical variables, against the base category of zero victimisation events.

	LFP & Educ.	LFP	Educ.	All employ.	FT employ.	Part-Time or marginal
	(1)	(2)	(3)	(4)	(5)	(6)
1 P.V.E.	0.0443**	0.0623***	-0.0230*	0.0558***	0.0111	0.0446***
	(0.0209)	(0.0212)	(0.0138)	(0.0210)	(0.0163)	(0.0165)
2 or more P.V.E.	0.0335	0.0510*	-0.0360*	0.00823	0.0179	-0.00963
	(0.0283)	(0.0285)	(0.0200)	(0.0308)	(0.0252)	(0.0227)
1 F.V.E.	0.000862	-0.00544	0.0112	0.00622	0.00313	0.00309
	(0.0204)	(0.0210)	(0.0146)	(0.0212)	(0.0166)	(0.0165)
2 or more F.V.E.	-0.0316	-0.0226	-0.0223	0.00658	0.0110	-0.00439
	(0.0259)	(0.0262)	(0.0162)	(0.0270)	(0.0220)	(0.0206)
Observations	2,314	2,314	2,314	2,314	2,314	2,314
R-squared	0.307	0.310	0.210	0.259	0.239	0.155
C. of origin x Y-M left origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

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

The dependent variable is binary and takes the value 1 for individuals in employment/labour force participation/education, zero otherwise. FT means full-time. Results are only shown for our preferred specification, corresponding to column (2) in the main result tables.

Table 18: Number of victimization experiences (discrete)

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

6.2 Effects by victimisation event

Our analyses so far did not differentiate between different types of victimisation events individuals endured on their flight to Germany beyond a distinction between physical and financial harm. We therefore implicitly assumed that all physical and financial victimisation events have a similar effect on economic integration outcomes. In this subsection, we relax this specification and break down the binary physical and financial victimisation indicators into their respective events. Table 19 shows the regression results for the same economic integration measures as in all previous analyses, based on our preferred specification.

	LFP & Educ.	LFP	Educ.	All employ.	FT employ.	Part-Time or marginal
	(1)	(2)	(3)	(4)	(5)	(6)
Exp. robbery	-0.0251	-0.0112	-0.0299*	0.0180	-0.00383	0.0219
	(0.0274)	(0.0275)	(0.0177)	(0.0291)	(0.0226)	(0.0237)
Exp. extortion	-0.0231	-0.0130	0.00882	-0.0359	-0.0385*	-0.0137
	(0.0249)	(0.0253)	(0.0175)	(0.0254)	(0.0202)	(0.0192)
Exp. fraud	-0.00174	-0.0104	0.0000	0.0182	0.0263	-0.00809
	(0.0202)	(0.0206)	(0.0143)	(0.0249)	(0.0202)	(0.0192)
Exp. sexual harass.	0.0180	0.0467	0.00612	0.164**	0.0110	0.153**
	(0.0765)	(0.0777)	(0.0507)	(0.0701)	(0.0471)	(0.0633)
Exp. shipwreck	0.00829	0.0231	-0.0123	-0.0131	-0.00280	-0.0103
	(0.0254)	(0.0256)	(0.0157)	(0.0255)	(0.0191)	(0.0196)
Exp. physical attack	0.0611**	0.0670***	-0.0346**	0.0488	0.0399*	0.0089
	(0.0248)	(0.0250)	(0.0177)	(0.0302)	(0.0240)	(0.0237)
Exp. incarceration	0.0205	0.0252	-0.00646	0.0117	0.00396	0.0077
	(0.0222)	(0.0226)	(0.0154)	(0.0231)	(0.0187)	(0.0177)
Observations	2,314	2,314	2,314	2,314	2,314	2,314
R-squared	0.307	0.310	0.210	0.261	0.242	0.155
C. of origin x Y-M left origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

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

The dependent variable is binary and takes the value 1 for individuals in employment/labour force participation/education, zero otherwise. FT means full-time. Results are only shown for our preferred specification, corresponding to column (2) in the main result tables.

Table 19: Breakdown by type of victimisation event

We start by noting that the lack of an association between financial victimisation and our economic integration outcome measures is generally not driven by opposing signs of financial victimisation events that otherwise affect the outcome. Two individually estimated coefficients among the financial victimisation events show an association with outcome measures that is significant at the 10% statistical level. Having experienced robbery during the journey shows a negative association with pursuing host-country education (column 3), while individuals who experienced extortion are moderately less likely to be full-time employed (column 6). Absent other clear patterns, a suggestive explanation for these findings is that, unlikely fraud, robbery and extortion may be more likely to occur concomitant with physical victimisation events. However, we also note that statistical significance at the 10% level of two out of 18 coefficients estimated is well within the expected range of randomly drawn coefficients and should therefore be interpreted with care. On the other hand, the results obtained on the physical victimisation events show a much clearer pattern. Column (2) shows that events of sexual harassment, shipwreck, physical attacks and incarceration are all positively linked to labour force participation, while column (3), with the exception of sexual harassment, shows a negative association of these events with pursuing host-country education. We further note that while the estimated coefficients on the different physical victimisation events in table 19 are generally of the same sign, having experienced physical attacks has the strongest effect on economic integration outcomes, both in magnitude and statistical precision. The finding is well in line with the victimisation literature that finds physical abuse to be the strongest predictor of mental well-being [Dolan et al., 2005, Mahuteau and Zhu, 2016, Johnston et al., 2018]. A further suggestive explanation is that physical abuse directly carried out by agents along the migration route leaves larger mental scars on victims than second-order victimisation events such as shipwreck and short-term incarceration that are only indirectly caused by perpetrators.

6.3 Heterogeneous effects by major countries of origin

One of the main concerns using sensitive survey data on victimisation is the reliability of responses [Krumpal, 2013]. In our setting, a relevant problem is the so-called "desirability bias": Respondents could be inclined to over-report victimisation if they think vulnerability is expected of them by the host community. This general problem of sensitive survey questions is exacerbated by the uncertain legal status many of the surveyed refugees faced at the time of their first interview in 2016 when about 36 percent of the respondents were still awaiting their asylum decision. Two institutional features alleviate this concern. First, interviewers make it clear to all respondents that the survey is conducted independent of the asylum procedure itself and information provided in the survey cannot be used against surveyees. Second, asylum is granted based on individuals' safety in their home country, rather than during their journey. Nevertheless, some respondents may still give answers they deem favourable with regards to their chances of receiving protection. If such misreporting was systematically correlated with future individual labour market outcomes, this could bias the estimated coefficients. We address the issue by splitting up our our analysis by country of origin, exploiting the fact that Syrians who were displaced from Syria between 2014 and 2016 are particularly unlikely to give socially desired survey responses. Due to the war in Syria that spread across the entire country, the rate of Syrians who were granted protection in Germany was extremely high and stood at 97 percent over our observation period. In fact, the German government acknowledged the general need for protection of displaced Syrians and introduced so-called simplified asylum procedures for Syrians already in November 2014. These allowed Syrian asylum seekers to get their asylum status granted by simply filling in a ten-page questionnaire and by proving that they were actually from Syria [Grote, 2018]. [9]

Tables 20 show the results for the countries of origin where more than 100 respondents are available, grouping Iraq and Iran as well as Afghanistan and Pakistan.

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

	LFP			Education		
	Syrian	Iraq & Iran	Afghanistan & Pakistan	Syrian	Iraq & Iran	Afghanistan & Pakistan
	(1)	(2)	(3)	(4)	(5)	(6)
Physical victim.	0.0551**	0.0578	0.0623	-0.0190	-0.0124	-0.130***
	(0.0228)	(0.0503)	(0.0745)	(0.0156)	(0.0267)	(0.0488)
Financial victim.	-0.0170	0.0708	0.0432	-0.00940	0.0382	-0.00181
	(0.0219)	(0.0526)	(0.0685)	(0.0153)	(0.0236)	(0.0378)
Observations	1,543	375	203	1,543	375	203
R-squared	0.272	0.378	0.588	0.182	0.423	0.500
C. of origin x Y-M left origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
	FT employed			Part-Time or marginal		
	Syrian	Iraq & Iran	Afghanistan & Pakistan	Syrian	Iraq & Iran	Afghanistan & Pakistan
	(7)	(8)	(9)	(10)	(11)	(12)
Physical victim.	0.00303	0.0586	-0.0761	0.0319*	0.0481	-0.00439
	(0.0180)	(0.0386)	(0.0662)	(0.0178)	(0.0344)	(0.0614)
Financial victim.	0.0115	0.00810	0.0388	-0.0127	0.0357	0.0741
	(0.0172)	(0.0374)	(0.0522)	(0.0173)	(0.0327)	(0.0525)
Observations	1,543	375	203	1,543	375	203
R-squared	0.162	0.286	0.504	0.089	0.251	0.509
C. of origin x Y-M left origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

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

The dependent variable is binary and takes the value 1 for individuals in the labour force/education, zero otherwise. Results are only shown for our preferred specification, corresponding to column (2) in the main result tables.

Table 20: Outcomes by main country of origin

We conclude that the main results of physical victimisation are not driven by individuals of any country of origin in particular. Columns (1), (2) and (3) show that physically victimised individuals are more likely to join the labour force rather than pursuing host-country specific education (columns 4, 5 and 6), regardless of their origin. We note that the magnitude of effects is largest for migrants originating from Afghanistan and Pakistan (columns 3 and 6). A likely explanation can be found in subsection 6.2: In our sample, Afghans and Pakistanis were significantly more likely to experience physical abuse (28%) than Syrians (10%) and individuals originating from Iraq or Iran (14%). The association of physical victimisation with part-time employment (column 11, 12 and 13) on the other hand is driven primarily by Syrians, both in magnitude and statistical precision. We note that Syrians constitute by far the largest group and the more precisely estimated results are therefore not unexpected. Financial victimisation shows no association with economic integration measures across the different estimations, adding further robustness to our main results.

6.4 Heterogeneous effects by gender

The asylum seekers entering into Germany between 2013 and 2017 mainly originated from countries where women have culturally different economic roles than men [Fuchs et al., 2020]. If individuals regress to a present-oriented mindset in response to victimisation experiences, negative events occurring during the flight to Germany could have effects on the economic integration of refugees that differ between men and women. For example, if joining the labour force represents a bigger step for women than men, potentially traumatic events and their negative effect on mental well-being may discourage women relatively more from becoming economically active. Table 21 therefore shows the main results of our preferred specification by gender.

	LFP		Education		FT employ.		Part-Time or marginal	
	Male	Female	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Physical victim.	0.0422**	0.0641	-0.0195	-0.0351*	0.0268	-0.00236	0.0225	0.0439**
	(0.0210)	(0.0447)	(0.0171)	(0.0183)	(0.0223)	(0.00941)	(0.0204)	(0.0222)
Financial victim.	-0.00164	0.0184	-0.000105	-0.00479	0.00727	0.0136	0.00103	-0.00224
	(0.0206)	(0.0426)	(0.0168)	(0.0194)	(0.0215)	(0.0110)	(0.0192)	(0.0235)
Observations	1,472	734	1,472	734	1,472	734	1,472	734
R-squared	0.261	0.319	0.227	0.300	0.244	0.249	0.171	0.185
C. of origin x Y-M left origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

The dependent variable is binary and takes the value 1 for individuals in the labour force/education/employment, zero otherwise. Results are only shown for our preferred specification, corresponding to column (2) in the main result tables.

Table 21: Outcomes by Gender

The results indicate that both men and women are affected very similarly by victimisation events. While noting that the smaller samples lead to a loss in statistical precision, the estimated coefficients on physical victimisation in columns (2) and (4) indicate that physical victimisation events affect women's decision to join the labour force instead of pursuing host-country education at an even larger magnitude than men's (column 1 and 3). For both men and women, the higher probability to join the labour force following physical victimisation only increases the uptake of part-time employment (columns 7 and 8). Financial victimisation shows no association with economic integration outcomes when splitting the sample between men and women. We therefore conclude that our main results are not driven by any gender in particular.

6.5 Heterogeneous effects by arrival cohort

One of the main challenges in the setting at hand is to separate selection effects at different expected victimisation (and thus, risk) levels from individual level effects of victimisation. In our empirical strategy laid out in section 4, we discuss our approach in detail and argue that narrowly defined fixed effects relating to both the time and geography of migration (and their interaction) are necessary to mitigate the issue. A related issue is that asylum seeker arrival cohorts may differ significantly in their composition over time for entirely exogenous reasons: Displacement happens in different geographical regions at different points in time, leading to heterogeneous arrival cohorts that reach their destination when labour market conditions are potentially more or less favourable to integration. If, for example, arrival cohorts differ along their educational attainment and also face a different victimisation risk, this could provide an explanation for between-cohort differences in labour market outcomes. While our empirical specification discussed in section 4.3 covers this issue by including dyadic fixed effects that limit variation to within-cohorts, it is worth exploring if our results are driven by any arrival cohort specifically. To do so, we split the sample by the different arrival cohorts. The regression results for three different cohorts arriving in Germany in 2013-2014, 2015 and 2016-2017 are shown in table 22.

	LFP			Education		
	2013-2014	2015	2016-2017	2013-2014	2015	2016-2017
	(1)	(2)	(3)	(4)	(5)	(6)
Physical trauma	0.0338	0.0487**	0.157**	-0.0295	-0.0318**	-0.0196
	(0.0567)	(0.0229)	(0.0751)	(0.0419)	(0.0152)	(0.0458)
Financial trauma	-0.0515	0.00365	0.0375	0.000815	0.0110	-0.0440
	(0.0507)	(0.0226)	(0.0722)	(0.0453)	(0.0145)	(0.0468)
Observations	408	1,527	292	408	1,527	292
R-squared	0.526	0.299	0.463	0.425	0.190	0.321
C. of origin x Y-M left origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
	FT employ.			Part-Time or marginal		
	2013-2014	2015	2016-2017	2013-2014	2015	2016-2017
	(7)	(8)	(9)	(10)	(11)	(12)
Physical victim.	-0.0135	0.0003	0.0202	0.0510	0.0274	0.0339
	(0.0536)	(0.0176)	(0.0378)	(0.0666)	(0.0172)	(0.0547)
Financial victim.	-0.0557	0.0276	0.0345	-0.0871	-0.0118	0.107*
	(0.0497)	(0.0179)	(0.0294)	(0.0576)	(0.0164)	(0.0552)
Observations	408	1,527	292	408	1,527	292
R-squared	0.434	0.237	0.265	0.490	0.136	0.366
C. of origin x Y-M left origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

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

The dependent variable is binary and takes the value 1 for individuals in the labour force/education, zero otherwise. Results are only shown for our preferred specification, corresponding to column (2) in the main result tables.

Table 22: Outcomes by Cohort

No clear pattern emerges when comparing the effect of physical victimisation events on economic outcomes across arrival cohort. All estimated coefficients are of the expected sign. We note that the 2015 cohort is far larger than the 2013-2014 cohort and the 2016-2017, which accurately reflects the magnitude of asylum seeker inflows into Germany but is also likely to explain the higher statistical precision on estimates related to the cohort (columns 2, 5 and 11). We therefore conclude that differences in arrival cohorts are unlikely to provide an explanation for the effect of physical victimisation on refugees' decision to invest into host-country education or take up employment shortly after arrival.

7 Testing alternative mechanisms

One of the limitations of the "loss of future directedness" channel (or higher time discounting rates) we propose as an explanation to our findings is that it is not easily distinguishable from more general mental well-being effects. In other words, since mental health likely affects both the external and the internal margin of economic activity, our results imply that the effect on the internal margin dominates. Since we cannot test the "loss of future directedness" directly, we go through alternative mechanisms that could plausibly explain our findings as laid out in section 2.

We first zoom into the asylum procedure more closely in section 7.1 to analyse if our results could be mechanically driven by design features of the German asylum system. In 7.2, we then test if disproportional financial hardship among the physically victimised could explain their faster uptake of low-income employment. Finally, in section 7.3, we test if the negative experience during the journey could have an off-putting effect on victimised individuals' intention to stay in Germany shortly after arrival, which could in turn make the investment into host-country specific human capital less attractive. In this section we show that none of this mechanisms is likely to be driving our findings.

7.1 Institutional design: Asylum procedures

If victimised individuals received asylum faster since they have a more genuine case for protection and therefore got access to the labour market faster upon arrival, this would mechanically link victimisation to a faster labour market integration and could potentially explain our main results. If the differences in the length of asylum procedures are very large, this could even lead to a scarring effect, permanently deterring those stuck in the procedure from labour market participation beyond the duration of the procedure itself [Hainmueller et al., 2016]. A similar reasoning holds for the general case for asylum: If victimised individuals have a more legitimate claim for protection, their refugee status could allow them to integrate into the labour market in larger numbers by design.[10]

To test this, we compare the outcome and the length of asylum procedures between victimised and non-victimised individuals explicitly in table 23. There is no visible difference between financially victimised individuals and those that were not victimised with regard to the share that ultimately received protection status. Among the physically victimised, the share was even slightly lower (69.4 percent) than among the non-victimised (75.4 percent). We also note that the average unconditional duration of the asylum procedure is slightly longer among both physically and financially victimised individuals compared to those who did not experience victimisation during their journey.

Variable	Mean	Std. Dev.	Min.	Max.	N
Non-victimised					
Asylum granted	0.754	0.431	0	1	1258
Length of asylum procedure in months	7.552	6.359	0	47	841
Physically victimised					
Asylum granted	0.694	0.461	0	1	994
Length of asylum procedure in months	7.782	6.372	0	44	595
Financially victimised					
Asylum granted	0.734	0.442	0	1	1026
Length of asylum procedure in months	7.473	6.151	0	44	673

Note: The variable measuring the length of the asylum procedure is not available for all asylum seekers. Our tests that the variable is not systematically missing are available upon request.

Table 23: Summary statistics asylum pocedure

The German asylum system has a second key institutional feature that could encourage

[10] Although this point is partially addressed by the inclusion of a categorical variables capturing each individual's asylum status in our main specification.

fast employment among specific segments of the asylum seeking population. Despite options being very limited in scope, obtaining employment before asylum can improve the chances of receiving a temporary protection status ("Duldung") in Germany [Brücker et al., 2019]. Finding employment upon arrival is therefore particularly incentivised for migrants with a low probability of receiving full protection status since employment disproportionally increases the probability of being allowed to stay in Germany for these individuals. If some individuals' migration decision is motivated by economic reasons in addition to humanitarian reasons and these individuals take higher risks during their journey, these asylum seekers could then also be more motivated to increase their chances of being granted a permission to stay by taking up employment before the end of their asylum procedure.

We test this possibility on the IEB employment biography data by mapping employment rates between victimised and non-victimised refugees for a) the time of arrival and the point in time when asylum was granted and b) after asylum was granted. The exercise of a pre-trend and post-trend comparison allows us to test at what point employment rates start to diverge.

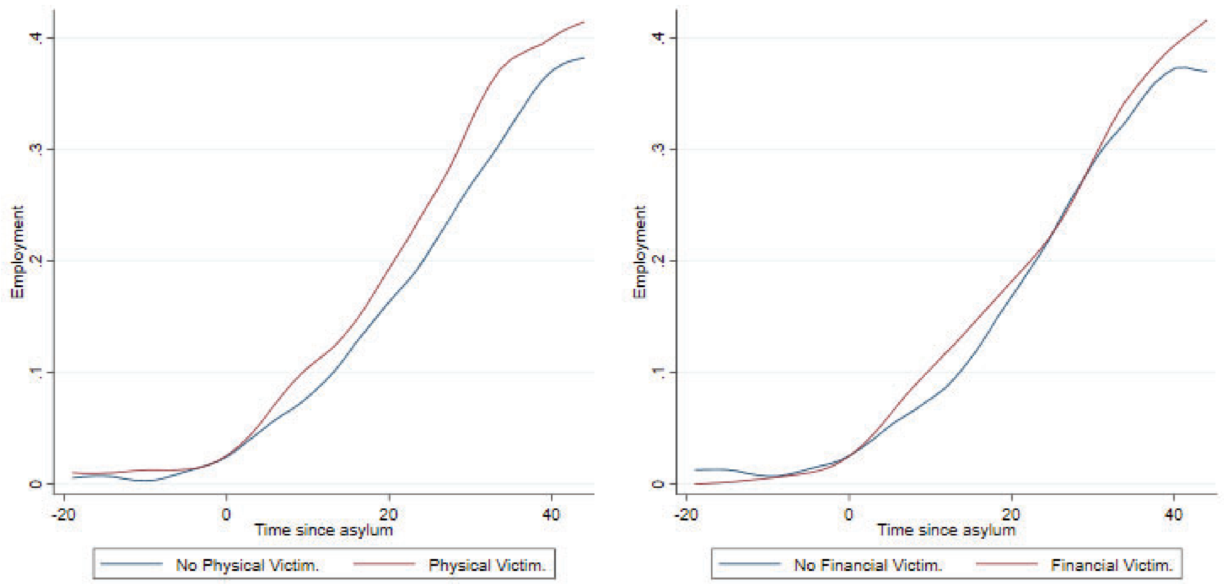

Figure 2: Pre- and post-protection trends in employment

Figure 2 shows the result of this exercise, with the x-axis starting at the time of arrival and $t = 0$ indicating the month in which asylum was granted. We do not find any evidence that employment rates diverge already prior to the end of the asylum procedure.

7.2 Behavioural changes due to financial difficulties

Smugglers have been documented to be responsible for abuse of asylum seekers during their journey and often charge large amounts for their services Albahari [2018]. We document this difference for a subset of our sample in table 5: On average, physically and financially victimised asylum seeker paid 1420 Euro and 1802 Euro more to escape agents than non-victimised migrants. If this means that victimised migrants relied more heavily on the services of escape agents and fell victim to criminal acts for that reason, the victimisation variables we construct would to some extent capture this choice. It would then be possible that the faster labour market integration of victimised individuals is caused by their attempt to recover the relatively high cost of the journey as soon as possible once they reach the destination country. To test this hypothesis, we approximate the level of financial precariousness of refugees in Germany by the extent to which they state to be very worried about their personal finances when arriving in Germany. The regression outcome is shown in table 24.

Very worried about finances	Cross Section		Panel Data	
	Benchmark	Fixed effects	Benchmark	Fixed effects
	(1)	(2)	(3)	(4)
Physical victim.	0.0149	0.00991	0.0295	0.0246
	(0.0217)	(0.0233)	(0.0186)	(0.0210)
Financial victim.	0.0310	0.0413*	0.0332*	0.0450**
	(0.0210)	(0.0221)	(0.0180)	(0.0199)
Observations	2,180	2,180	3,380	3,380
R-squared	0.102	0.194		
C. of origin x Y-M left origin FE	No	Yes	No	Yes
Baseline Controls	Yes	Yes	Yes	Yes

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

The dependent variable is binary and takes the value 1 for individuals who state they are "very concerned about their finances" at the time of the interview. The cross-sectional regressions are run on the first available observation of each individual, 19 months after arrival on average.

Table 24: Very worried about finances

We do not detect an effect of physical victimisation on financial hardship at any conventional statistical level. Unsurprisingly, financially victimised refugees are indeed more likely to voice concern about their financial situation, an effect most pronounced shortly after arrival (column (1) and (2)). In our preferred specification of column (2), the magnitude of the effect is an excess 4.5 percentage points over other refugee groups.

7.3 Intention to remain in Germany

If victimised individuals intend to stay for a relatively shorter duration than non-victimised individuals because the difficult journey discouraged them, they could be more likely to seek low-skill employment quickly instead of investing into host-country specific human capital. The hypothesis follows directly from the classic human capital investment model for migrants which posits that the longer migrants intend to stay, the more they invest into host-country specific education and the less likely they are to take up low-skill employment [Cortes, 2004]. We test this mechanism by analysing differences in refugees' intention to stay in Germany upon arrival in table 25.

Intention to stay in GER	Cross Section		Panel Data	
	Benchmark	Fixed effects	Benchmark	Fixed effects
	(1)	(2)	(3)	(4)
Physical victim.	-0.0204	-0.0159	-0.0245**	-0.0178
	(0.0125)	(0.0136)	(0.0106)	(0.0126)
Financial victim.	0.00567	0.00424	0.00662	0.00650
	(0.0116)	(0.0128)	(0.00991)	(0.0117)
Observations	2,180	2,180	3,380	3,380
R-squared	0.055	0.126		
C. of origin x Y-M left origin FE	No	Yes	No	Yes
Baseline Controls	Yes	Yes	Yes	Yes

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

The dependent variable is binary and takes the value 1 for individuals who state they are "intend to stay in Germany permanently" at the time of the interview. The cross-sectional regressions are run on the first available observation of each individual, 19 months after arrival on average.

Table 25: Intention to stay in GER

We first note that the cross-sectional regressions run on the first available observation after arrival to Germany shown in columns (1) and (2) are most informative: At later stages of the time spent in Germany, the intention to stay in Germany may become endogenous to our outcomes of interest. If individuals don't manage to integrate economically, they may have a lower propensity to remain in the host country. Our results lend only suggestive support to the hypothesis that the physical victimisation event during the journey to Germany decreases the likelihood of wanting to stay in Germany. Our preferred specification of column (2) shows a small and statistically insignificant negative effect of physical victimisation on the likelihood of wanting to stay in the country. All other panel specifications confirm these results; even if the more imprecisely estimated coefficients can partly be explained by the relatively smaller sample size due to missing values in the response, the magnitude of the effect is too small to explain the observed differences between physically victimised migrants and others.

8 Conclusion

One of the key features of humanitarian migration flows from developing into developed regions of the world has been the extreme conditions under which these movements take place. While it is clear that it is exactly the perilous journey itself that limits and deters migration flows, the unintended consequences of such restrictive policies are not yet well understood. In this paper, we analyse how victimisation on asylum seekers' flight to safety, a direct consequence of restrictive migration policies, interacts with labour market outcomes in the destination country. We first show that physical and to a lesser extent financial victimisation events that occurred during the escape journey negatively affect the mental well-being and health among the refugee population in Germany at the time of arrival. In a second step, we then show that physical victimisation in particular distorts trajectories of economic activity in the destination countries at the internal margin: Physically victimised individuals join the labour force faster through the uptake of part-time or marginal employment instead of investing into host-country specific human capital. We do not find a similar effect for financially victimised refugees, suggesting that, in line with the previous victimisation literature, the act of physical victimisation has relatively stronger effects on life trajectories. Financially victimised experience financial hardship in relatively larger numbers when arriving at their destination, but we do not detect a distortion on economic activity in the host-country.

We conceptualise our findings as a "loss of future directedness", a concept closely related to that of "impatience" (e.g. higher time discounting rates) in the economics literature: Events of physical victimisation lead to less forward-looking decision-making. In the framework of the migrant-specific human capital investment model, this can be seen as a distortion to the trade-off refugees face upon arrival to either invest into education to get access to higher quality employment at a later stage or take up lower-quality employment shortly after arrival. These distortions are considerable in magnitude: Three years after arrival, refugees who were physically victimised during their journey are more than 5 percentage points more likely to have joined the labour force to work in low-income professions and are more than 3 percentage points less likely to pursue host-country specific education or training.

The results strongly suggest that entry restrictions to asylum seekers, while undoubtedly limiting the numbers of new-arrivals, have short and long-run welfare implications for destination countries beyond those brought about by the absolute number of refugees they host. The victimisation events reported by refugees in the surveys match those systematically measured around the EU's external border, suggesting that at least some of the physical violence inflicted on asylum seekers is directly carried out by border agents [Arsenijević et al., 2017]. Our findings imply that these deterrent measures have consequences for the mental well-being of asylum seekers that extend into their economic integration in the host-country. In this context, we note that one limitation of our study is that the sample only consists of newly arrived refugees. Distortions to the optimal timing of labour market entry are likely to result in larger welfare losses to the host-country in the medium to long-term. The magnitude of these distortions is in turn linked to other variables, such as return migration patterns among refugees. Revisiting and quantifying the economic cost of victimisation events at a later stage is therefore a promising avenue for future research.

In addition to uncovering potentially costly repercussions of restrictive migration policies for optimal labour market trajectories in the destination, all of our findings also cast doubt on the notion of a swift labour market integration as a success metric for refugees more generally. While clearly useful to judge the efficacy of supportive integration policies, we show that the speed of labour market integration on the aggregate also reflects unintended consequences of policies that serve entirely different purposes. Headline figures on labour force participation and employment among refugees should therefore be cross-referenced with the type of employment individuals engage in, and the potential loss in human capital stemming from distorted educational investment decisions.

References

M. Akbulut-Yuksel. Children of war the long-run effects of large-scale physical destruction and warfare on children. *Journal of Human resources*, 49(3):634–662, 2014.

C. G. Aksoy and P. Poutvaara. Refugees' self-selection into europe: who migrates where? *Available at SSRN 3373580*, 2019.

C. G. Aksoy, P. Poutvaara, and F. Schikora. First time around: Local conditions and multi-dimensional integration of refugees. *IZA DP No. 13914*, 2020.

M. Albahari. From right to permission: Asylum, mediterranean migrations, and europe's war on smuggling. *Journal on Migration and Human Security*, 6(2):121–130, 2018.

S. Andersen, G. W. Harrison, M. I. Lau, and E. E. Rutstroem. Eliciting risk and time preferences. *Econometrica, vol.76(3)*, 2008.

J. Arsenijević, E. Schillberg, A. Ponthieu, L. Malvisi, W. A. E. Ahmed, S. Argenziano, F. Zamatto, S. Burroughs, N. Severy, C. Hebting, et al. A crisis of protection and safe passage: violence experienced by migrants/refugees travelling along the western balkan corridor to northern europe. *Conflict and health*, 11(1):1–9, 2017.

J. Arsenijević, D. Burtscher, A. Ponthieu, N. Severy, A. Contenta, S. Moissaing, S. Argenziano, F. Zamatto, R. Zachariah, E. Ali, et al. "i feel like i am less than other people": Health-related vulnerabilities of male migrants travelling alone on their journey to europe. *Social Science & Medicine*, 209:86–94, 2018.

M. Battisti, G. Peri, and A. Romiti. Dynamic effects of co-ethnic networks on immigrants' economic success. Technical report, National Bureau of Economic Research, 2016.

M. Beine, G. Charness, A. Dupuy, and M. Joxhe. Shaking things up: On the stability of risk and time preferences. *IZA Discussion Papers 13084*, 2020.

M. Beiser. Changing time perspective and mental health among southeast asian refugees. *Culture, Medicine and Psychiatry*, 11(4):437–464, 1987.

A. Belloni, V. Chernozhukov, and C. Hansen. Inference on treatment effects after selection among high-dimensional controls. *Review of Economic Studies, 81, 608-650*, 2014a.

A. Belloni, V. Chernozhukov, and C. Hansen. Inference on treatment effects after selection among high-dimensional controls. *The Review of Economic Studies*, 81(2):608–650, 2014b.

A. Bindler and N. Ketel. Scaring or scarring? labour market effects of criminal victimisation. *ECONtribute Discussion Papers Series 030*, 2019.

C. Blattman and J. Annan. The consequences of child soldiering. *The review of economics and statistics*, 92(4):882–898, 2010.

C. Brell, C. Dustmann, and I. Preston. The labor market integration of refugee migrants in high-income countries. *Journal of Economic Perspectives*, 34(1):94–121, 2020.

N. Breslau, E. L. Peterson, and L. R. Schultz. A second look at prior trauma and the posttraumatic stress disorder effects of subsequent trauma: a prospective epidemiological study. *Archives of General Psychiatry*, 65(4):431–437, 2008.

R. Brown, M. Montalva, D. Thomas, and A. Velásquez. Impact of violent crime on risk aversion: Evidence from the mexican drug war. *The Review of Economics and Statistics, MIT Press, vol. 101(5)*, 2019.

S. Brown, J. Roberts, and K. Taylor. Reservation wages, labour market participation and health. *Journal of the Royal Statistical Society: Series A (Statistics in Society)*, 173(3): 501–529, 2010.

H. Brücker, P. Jaschke, S. Keita, and R. Konle-Seidl. Zum gesetzentwurf der bundesregierung über duldung bei ausbildung und beschäftigung sowie zu den anträgen der fraktionen der fdp, bündnis 90/die grünen und die linke. Technical report, IAB-Stellungnahme, 2019.

B. C. Cadena and B. J. Keys. Human capital and the lifetime costs of impatience. *American Economic Journal: Economic Policy, 7(3)*, 2015.

M. Callen. Catastrophes and time preference: Evidence from the indian ocean earthquake. *Journal of Economic Behavior & Organization, Elsevier, vol. 118*, 2015.

M. Callen, M. Isaqzadeh, J. D. Long, and C. Sprenger. Violence and risk preference: Experimental evidence from afghanistan. *American Economic Review*, 104(1):123–48, 2014.

L. Cameron and M. Shah. Risk-taking behavior in the wake of natural disasters. *Journal of Human Resources, vol. 50(2)*, 2015.

A. Cassar, A. Healy, and C. von Kessler. Trust, risk, andtime preferences after a natural disaster: Experimental evidence from thailand. *World Development, vol. 94, 90-105*, 2017.

D. Chetverikov, Z. Liao, and V. Chernozhukov. On cross-validated lasso. *ArXiv Working Paper No. arXiv:1605.02214*, 2019.

K. E. Cortes. Are refugees different from economic immigrants? some empirical evidence on the heterogeneity of immigrant groups in the united states. *Review of Economics and Statistics*, 86(2):465–480, 2004.

M. Couttenier, V. Petrencu, D. Rohner, and M. Thoenig. The violent legacy of conflict: Evidence on asylum seekers, crime, and public policy in switzerland. *American Economic Review*, 109(12):4378–4425, 2019.

A. P. Damm. Ethnic enclaves and immigrant labor market outcomes: Quasi-experimental evidence. *Journal of Labor Economics*, 27(2):281–314, 2009.

S. Decker and H. Schmitz. Health shocks and risk aversion. *Journal of Health Economics, Elsevier, vol. 50(C)*, 2016.

S. DellaVigna and M. D. Paserman. Job search and impatience. *Journal of Labor Economics, vol.23(3)*, 2005.

P. Dolan, G. Loomes, T. Peasgood, and A. Tsuchiya. Estimating the intangible victim costs of violent crime. *British Journal of Criminology*, 45(6):958–976, 2005.

H. O. Duleep and M. C. Regets. Immigrants and human-capital investment. *American Economic Review*, 89(2):186–191, 1999.

C. Dustmann and A. v. Soest. Language fluency and earnings: Estimation with misclassified language indicators. *Review of Economics and Statistics*, 83(4):663–674, 2001.

C. Eckel, M. El-Gamal, and R. K. Wilson. Risk loving after the storm: A bayesian-network study of hurricane katrina evacuees. *Journal of Economic Behavior & Organization, vol. 69*, 2009.

B. Efron. Bootstrap methods: another look at the jackknife. In *Breakthroughs in statistics*, pages 569–593. Springer, 1992.

European Commission. Action plan on integration and inclusion 2021-2027, 2020.

L. M. Fuchs, Y. Fan, and C. von Scheve. Value differences between refugees and german citizens: Insights from a representative survey. *International Migration*, 2020.

German Federal Office for Migration and Refugees. Press release nr. 002/2020: Widerrufsprüfungen für 2019 fristgerecht erledigt – widerrufsquote bei 3,3 prozent. 2020.

S. Goosen, A. E. Kunst, K. Stronks, I. E. van Oostrum, D. G. Uitenbroek, and A. J. Kerkhof. Suicide death and hospital-treated suicidal behaviour in asylum seekers in the netherlands: a national registry-based study. *BMC public health*, 11(1):1–8, 2011.

J. Grote. The changing influx of asylum seekers in 2014-2016: Responses in germany; focussed study by the german national contact point for the european migration network (emn). *Working Paper BAMF*, 2018.

L. Guichard. Self-selection of asylum seekers: Evidence from germany. *Demography*, pages 1–28, 2020.

L. Guichard, I. Issifou, and S. Keita. Price adjustments on the market for human smuggling: Evidence from a large demand shock. *Working paper*, 2021.

L. Guiso, P. Sapienza, , and L. a. Zingales. Time varying risk aversion. *Journal of Financial Economics, vol. 128(3)*, 2018.

J. Hainmueller, D. Hangartner, and D. Lawrence. When lives are put on hold: Lengthy asylum processes decrease employment among refugees. *Science advances*, 2(8):e1600432, 2016.

C. Hanaoka, H. Shigeoka, and Y. Watanabe. Do risk preferences change? evidence from the greateast japan earthquake. *American Economic Journal: Applied Economics 10, 298-330*, 2018.

E. Hauff and P. Vaglum. Integration of vietnamese refugees into the norwegian labor market: the impact of war trauma. *International migration review*, 27(2):388–405, 1993a.

E. Hauff and P. Vaglum. Vietnamese boat refugees: The influence of war and flight traumatization on mental health on arrival in the country of resettlement: A community cohort study of vietnamese refugees in norway. *Acta Psychiatrica Scandinavica*, 88(3):162–168, 1993b.

K. Hawton. van hk. suicide. *Lancet*, 373(9672):1372–81, 2009.

C. Hunkler and M. Khourshed. The role of trauma for integration. the case of syrian refugees. *SozW Soziale Welt*, 71(1-2):90–122, 2020.

J. Jacobsen, J. Klikar, and J. Schupp. Scales manual iab-bamf-soep survey of refugees in germany. Technical report, SOEP Survey Papers, 2017.

P. Jakiela and O. Ozier. The impact of violence on individual risk preferences: Evidence from a natural experiment. *The Review of Economics and Statistics, vol. 101(3)*, 2019.

M. Jetter, L. M. Magnusson, and S. Roth. Becoming sensitive: Males' risk and time preferences after the 2008 financial crisis. *European Economic Review, vol. 128(C)*, 2020.

D. W. Johnston, M. A. Shields, and A. Suziedelyte. Victimisation, well-being and compensation: Using panel data to estimate the costs of violent crime. *The Economic Journal*, 128(611):1545–1569, 2018.

S. C. Kassenboehmer and J. P. Haisken-DeNew. You're fired! the causal negative effect of entry unemployment on life satisfaction. *The Economic Journal*, 119(536):448–462, 2009.

D. Kemptner and S. Tolan. The role of time preferences in educational decision making. *Economics of Education Review, vol. 67(C)*, 2018.

N. Kettlewell. Risk preference dynamics around life events. *Journal of Economic Behavior & Organization, vol. 162*, 2019.

M. F. Koppensteiner and L. Menezes. Violence and human capital investments. *Journal of Labor Economics*, forthcoming.

I. Krumpal. Determinants of social desirability bias in sensitive surveys: a literature review. *Quality & Quantity*, 47(4):2025–2047, 2013.

G. Leon. Civil conflict and human capital accumulation the long-term effects of political violence in perú. *Journal of Human Resources*, 47(4):991–1022, 2012.

E. Lerner, G. A. Bonanno, E. Keatley, A. Joscelyne, and A. S. Keller. Predictors of suicidal ideation in treatment-seeking survivors of torture. *Psychological trauma: theory, research, practice, and policy*, 8(1):17, 2016.

S. Mahuteau and R. Zhu. Crime victimisation and subjective well-being: panel evidence from australia. *Health economics*, 25(11):1448–1463, 2016.

U. Malmendier and S. Nagel. Depression babies: Do macroeconomic experiences affect risk taking? *The Quarterly Journal of Economics, vol. 126(1)*, 2011.

M. Marbach, J. Hainmueller, and D. Hangartner. The long-term impact of employment bans on the economic integration of refugees. *Science Advances*, 4(9):eaap9519, 2018.

S. Meier and C. Sprenger. Present-biased preferences and credit card borrowing. *American Economic Journal: Applied Economics vol. 2(1)*, 2010.

S. Meier and C. D. Sprenger. Discounting financial literacy: Time preferences and participation in financial education programs. *Journal of Economic Behavior & Organization, vol. 95*, 2013.

M. Nübling, U. Stößel, H.-M. Hasselhorn, M. Michaelis, and F. Hofmann. Measuring psychological stress and strain at work-evaluation of the copsoq questionnaire in germany. *GMS Psycho-Social Medicine*, 3, 2006.

P. Ornstein. The price of violence: Consequences of violent crime in sweden. Technical report, Working Paper, 2017.

E. Oster. Unobservable selection and coefficient stability: Theory and evidence. *Journal of Business & Economic Statistics*, 37(2):187–204, 2019.

L. Page, D. A. Savage, and B. Torgler. Variation in risk seeking behaviour following large losses: A natural experiment. *European Economic Review, Elsevier, vol. 71(C)*, 2014.

M. Sagbakken, I. M. Bregård, and S. Varvin. The past, the present and the future: a qualitative study exploring how refugees' experience of time influences their mental health and well-being. *Frontiers in Sociology*, 5:46, 2020.

O. Shemyakina. The effect of armed conflict on accumulation of schooling: Results from tajikistan. *Journal of Development Economics*, 95(2):186–200, 2011.

G. J. Stigler and G. S. Becker. De gustibus non est disputandum. *The american economic review*, 67(2):76–90, 1977.

M. Sutter, M. G. Kocher, D. Rutzler, and S. T. Trautmann. Impatience and uncertainty: Experimental decisions predict adolescents' field behavior. *American Economic Review 103 (1): 510-31*, 2013.

R. H. Thaler and S. Benartzi. Save more tomorrow$^{\text{TM}}$: Using behavioral economics to increase employee saving. *Journal of Political Economy vol. 112(S1)*, 2004.

R. Tibshirani. Regression shrinkage and selection via the lasso. *Journal of the Royal Statistical Society, Series B 58, 267-288*, 1996.

M. Velamuri and S. Stillman. The impact of crime victimisation on individual well-being: Evidence from australia. *Labour, Employment and Work in New Zealand*, 2008.

M. J. Voors, E. E. Nillesen, P. Verwimp, E. H. Bulte, R. Lensink, and D. P. Van Soest. Violent conflict and behavior: a field experiment in burundi. *American Economic Review*, 102(2):941–64, 2012.

J. M. Wooldridge. *Econometric analysis of cross section and panel data*. MIT press, 2010.

R. Yehuda. Post-traumatic stress disorder. *New England journal of medicine*, 346(2):108–114, 2002.

W. Zwysen. Different patterns of labor market integration by migration motivation in europe: the role of host country human capital. *International Migration Review*, 53(1):59–89, 2019.

Appendices

A Mental and physical health scores

The mental and physical health scores are constructed strictly following Jacobsen et al. [2017], who describe all necessary calculations in detail (p.23-24). A higher score reflects better health. The mental health scale (MCS) is based on the following questions:

- Did you feel in low spirits and melancholy?
- Did you feel calm and balanced?
- Did you feel full of energy?
- Due to psychological or emotional problems, did you achieve less in your work or everyday activities than you actually intended?
- Due to psychological problems or emotional problems, did you perform your work or everyday activities less carefully than usual?
- Due to health or psychological problems, have you been restricted in terms of your social contact to for example friends, acquaintances or relatives?

The response scale for all questions related to the mental scale is 1 (Very often), 2 (Often), 3 (Sometimes), 4 (Almost never), 5 (Never).

The physical health scale (PCS) is based on the following questions:

- How would you describe your current state of health (scale: 1 (Poor) to 5 (Very Well))?
- If you have to climb stairs, i.e. walk up several floors: Does your state of health restrict you (scale: 1 (A lot), 2 (A little), 3 (Not at all))?
- What about other strenuous activities in everyday life, e.g. when you have to lift something heavy or need to be mobile: Does your state of health restrict you a lot, a little or not at all (scale: 1 (A lot), 2 (A little), 3 (Not at all))?
- How often in the last four weeks did you suffer from severe physical pain?
- How often in the last four weeks, due to health problems of a physical nature, did you achieve less in your work or everyday activities than you actually intended?
- How often in the last four weeks, due to health problems of a physical nature, have you been restricted in the type of tasks you can perform in your work or everyday activities?

The response scale for the last three questions is 1 (Very often), 2 (Often), 3 (Sometimes), 4 (Almost never), 5 (Never).

The regression results of the effect of victimisation on the outcomes are shown in tables 26 and 27.

Mental Scale	Cross Section				Panel Data	
	Benchmark	Fixed effects	Dyadic FE	PDS	Benchmark	Fixed effects
	(1)	(2)	(3)	(4)	(5)	(6)
Physical victim.	-1.033*	-1.202**	-1.059	-1.641***	-0.975**	-1.082**
	(0.527)	(0.557)	(0.742)	(0.546)	(0.440)	(0.471)
Financial trauma	-1.495***	-1.643***	-2.454***	-0.599	-1.130***	-1.342***
	(0.497)	(0.537)	(0.741)	(0.521)	(0.422)	(0.459)
Observations	2,235	2,225	1,677	2,127	4,814	4,814
R-squared	0.153	0.230	0.307			
C. of origin x Y-M left origin FE	No	Yes	Yes	Some	No	Yes
R. origin x Y-M left origin x Y-M arrival FE	No	No	Yes	No	No	No
Year-month arrival FE	No	No	Yes	No	No	No
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

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

The dependent variable is a mental health score on a scale from 0 to 100 at the time of the interview. The cross-sectional regressions are run on the first available observation of each individual. Results are only shown for our preferred specification, corresponding to column (2) in the main result tables. The term FE indicates fixed effects. The term Y-M left origin refers to the year-month of forceful displacement from the home country. Y-M arrival refers to the year-month of arrival in Germany. C. of origin is the country of origin, R. of origin is the wider region of origin as shown in table 5.

Table 26: Mental Scale

Physical Scale	Cross Section				Panel Data	
	Benchmark	Fixed effects	Dyadic FE	PDS	Benchmark	Fixed effects
	(1)	(2)	(3)	(4)	(5)	(6)
Physical victim.	-0.850**	-1.194***	-1.056*	-1.203***	-0.909**	-1.264***
	(0.423)	(0.449)	(0.604)	(0.450)	(0.364)	(0.393)
Financial trauma	-0.730*	-0.640	-0.435	-0.531	-0.225	-0.328
	(0.411)	(0.441)	(0.594)	(0.434)	(0.354)	(0.384)
Observations	2,235	2,225	1,677	2,127	4,814	4,814
R-squared	0.258	0.323	0.383			
C. of origin x Y-M left origin FE	No	Yes	Yes	Some	No	Yes
R. origin x Y-M left origin x Y-M arrival FE	No	No	Yes	No	No	No
Year-month arrival FE	No	No	Yes	No	No	No
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

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

The dependent variable is a physical health score on a scale from 0 to 100 at the time of the interview. The cross-sectional regressions are run on the first available observation of each individual. Results are only shown for our preferred specification, corresponding to column (2) in the main result tables. The term FE indicates fixed effects. The term Y-M left origin refers to the year-month of forceful displacement from the home country. Y-M arrival refers to the v of arrival in Germany. C. of origin is the country of origin, R. of origin is the wider region of origin as shown in table 5.

Table 27: Physical Scale

B Least absolute shrinkage and selection operators

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

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

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

The second term of equation (3) represents the cost of including many regressors. λ is the penalization term.[11] The effect of the penalization is that LASSO sets the $\hat{\beta}_j$s of the variables that contribute little to the model fit to zero.

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

In our setting with two variables of interest, $PhysicalVictim$ (PT) and $FinancialVictim$ (FT), we amend this method to a post-triple selection. The first step in this procedure is to estimate the outcome equation (labour market outcomes) using LASSO, without including PT nor FT:

$$Y_{i,f,c,t} = x_{i,f,c,t}'\beta_j + \epsilon_{i,f,c,t}, \tag{3}$$

where we denote the set of LASSO-selected controls by A. The vector $x_{i,f,c,t}$ these controls are selected from include a large set of time constant and time varying individual characteristics, country of origin fixed effects, year-month fixed effects, year-quarter fixed effects, and the interaction between all these variables.

The second step is to estimate the probability of physical victimisation:

$$PT_{i,c,t} = x_{i,c,t}'\delta_j + \epsilon_{i,f,c,t}, \tag{4}$$

where we denote the set of LASSO-selected controls by B.

The third step is to estimate the probability of financial victimisation:

$$FT_{i,c,t} = x_{i,c,t}'\eta_j + \epsilon_{i,f,c,t}, \tag{5}$$

where we denote the set of LASSO-selected controls by C.

The final step is to use OLS to estimate

$$Y_{i,f,c,t} = \gamma_1 PT_i + \gamma_2 FT_i + w_{i,f,c,t}'\beta_j + \epsilon_{i,f,c,t}, \tag{6}$$

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

where $w_{i,f,c,t}$ is the union of the selected controls from steps 1,2 and 3 (e.g., $w_{i,f,c,t} = A \cup B \cup C$).

Belloni et al. [2014a] argue that LASSO can be used to select controls because moderate model selection mistakes of the LASSO do not affect the asymptotic properties of the estimator of the low-dimensional parameters of interest. Hence, modelling the nuisance component of our structural model can be seen as a prediction problem [Andersen et al., 2008].

C Employment outcomes - in labour force

	Any employment		Full-time		Part-time or marginal	
	Cross S.	Panel D.	Cross S.	Panel D.	Cross S.	Panel D.
	(1)	(2)	(3)	(4)	(5)	(6)
Physical victim.	0.0317	0.0225	0.00658	0.00814	0.0252	0.0141
	(0.0245)	(0.0189)	(0.0194)	(0.0149)	(0.0206)	(0.0156)
Financial victim.	0.0140	0.0119	0.0142	0.00721	-0.000255	0.00493
	(0.0242)	(0.0179)	(0.0193)	(0.0140)	(0.0199)	(0.0149)
Observations	1,679	3,344	1,679	3,344	1,679	3,344
R-squared	0.297		0.249		0.189	
C. of origin x Y-M left origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Country of origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

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

The dependent variable is binary and captures all employed individuals inside the labour force, with regressions showing employment outcomes for any employment ((1) and (2)), full-time employment ((3) and (4)) and part-time or marginal employment ((5) and (6)). All other types of employment are set to zero in regressions (2)-(6). Columns (1), (3) and (5) show the results of a cross-sectional regression on the last interview conducted. Columns (2), (4) and (6) are estimated on the panel using random effects. The term FE indicates fixed effects. The term Y-M left origin refers to the year-month of forceful displacement from the home country.

Table 28: Employment

D Full Results

Table 29: Fixed Effects Results, main outcomes

	Life Sat.	Health	LFP& Educ	LFP	Educ	All emp.	FT emp.	PT,M emp.	Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Physical victim.	-0.166*	-0.281**	0.0382**	0.0577***	-0.0297**	0.0436**	0.0136	0.0302**	-0.229
	(0.0977)	(0.116)	(0.0190)	(0.0193)	(0.0127)	(0.0189)	(0.0148)	(0.0147)	(0.146)
Financial victim.	-0.107	-0.183	-0.0108	-0.0119	-0.000904	0.00148	0.00616	-0.00246	0.0150
	(0.0966)	(0.114)	(0.0184)	(0.0188)	(0.0125)	(0.0184)	(0.0145)	(0.0142)	(0.143)
Central Med. Route	-0.494**	0.0445	-0.0324	-0.0301	0.0135	0.00554	-0.00815	-0.0170	-0.0849
	(0.243)	(0.263)	(0.0454)	(0.0454)	(0.0375)	(0.0530)	(0.0447)	(0.0386)	(0.275)
Western Med. Route	0.126	0.222	0.195**	0.201**	-0.0649*	-0.0277	0.0716	-0.0977**	0.139
	(0.514)	(0.691)	(0.0804)	(0.0830)	(0.0349)	(0.121)	(0.119)	(0.0444)	(0.818)
Eastern Med. Route (land)	0.325*	0.479***	0.0207	0.0263	-0.0346*	-0.0228	0.00200	-0.0188	-0.151
	(0.174)	(0.182)	(0.0298)	(0.0306)	(0.0205)	(0.0331)	(0.0271)	(0.0261)	(0.227)
Eastern Land Borders	-0.154	0.863	0.161	0.154	-0.0236	-0.0889	0.0109	-0.0157	-1.146
	(0.453)	(0.743)	(0.102)	(0.102)	(0.0534)	(0.0916)	(0.0652)	(0.108)	(1.000)
By plane directly to Germany	-0.0838	-0.165	0.0157	0.0178	0.0709*	-0.0213	-0.0451	0.0105	0.316
	(0.205)	(0.282)	(0.0467)	(0.0483)	(0.0377)	(0.0494)	(0.0296)	(0.0417)	(0.358)
No route information	0.00925	-0.0491	-0.0382	-0.0481*	0.0182	-0.0332	-0.00860	-0.0271	0.115
	(0.119)	(0.135)	(0.0279)	(0.0285)	(0.0180)	(0.0253)	(0.0203)	(0.0192)	(0.208)
Female	0.0897	-0.702***	-0.219***	-0.215***	-0.0189	-0.151***	-0.0957***	-0.0440**	-0.618*
	(0.111)	(0.127)	(0.0239)	(0.0247)	(0.0158)	(0.0204)	(0.0146)	(0.0174)	(0.331)
Age	-0.0250	-0.0272	0.0181***	0.0254***	-0.0155***	0.0191***	0.0157***	0.00676*	0.0482
	(0.0314)	(0.0360)	(0.00666)	(0.00671)	(0.00387)	(0.00522)	(0.00378)	(0.00396)	(0.0629)
Age squared	0.000172	-0.000326	-0.000250***	-0.000337***	0.000166***	-0.000317***	-0.000236***	-0.000109**	-0.000442
	(0.000415)	(0.000486)	(8.50e-05)	(8.58e-05)	(4.73e-05)	(6.55e-05)	(4.72e-05)	(4.91e-05)	(0.000977)
Low conflict intensity	0.500	-0.873***	0.103	0.104	0.0126	0.121**	0.0990***	-0.0254	-0.106
	(0.409)	(0.312)	(0.0770)	(0.0763)	(0.0256)	(0.0544)	(0.0353)	(0.0536)	(0.565)
High conflict intensity	0.509	-0.612**	0.0929	0.0801	0.0242	0.0976*	0.0956***	-0.0392	-0.0392
	(0.404)	(0.306)	(0.0766)	(0.0759)	(0.0250)	(0.0534)	(0.0345)	(0.0530)	(0.530)
Willingness to take risk	-0.00579	0.0160	4.56e-05	-0.00146	0.00421**	0.00376	-0.00195	0.00336	-0.0310*
	(0.0147)	(0.0166)	(0.00265)	(0.00268)	(0.00180)	(0.00266)	(0.00212)	(0.00217)	(0.0173)
Resilience	0.0655***	0.0367**	0.00680**	0.00589**	0.00305*	-0.00151	0.000406	-0.00285	0.0110
	(0.0151)	(0.0162)	(0.00279)	(0.00282)	(0.00164)	(0.00263)	(0.00213)	(0.00213)	(0.0190)
Life satisf. BFM	0.0361	-0.0362	-0.00547	-0.00309	0.000269	-0.00106	0.00175	-0.00375	0.0242
	(0.0234)	(0.0251)	(0.00370)	(0.00380)	(0.00244)	(0.00383)	(0.00295)	(0.00320)	(0.0251)
Good German BFM			-0.0278	0.00258	-0.0395	0.0137	0.0324	0.0979	-0.459
			(0.0704)	(0.0681)	(0.0424)	(0.0718)	(0.0524)	(0.0817)	(0.568)
No info. on German BFM			0.137	0.182	-0.0855	-0.0590	0.00178	-0.0638	-0.141
			(0.168)	(0.173)	(0.0557)	(0.0966)	(0.0799)	(0.0415)	(1.473)
Help from FriendsAcquaint. to move			-0.0662***	-0.0604**	-0.0172	-0.00841	-0.0182	0.00271	-0.131
			(0.0237)	(0.0240)	(0.0139)	(0.0205)	(0.0153)	(0.0167)	(0.181)
Arrived alone	0.210*	-0.259*	-0.0157	-0.000725	0.00151	0.0179	0.00632	0.00281	-0.0312
	(0.124)	(0.137)	(0.0205)	(0.0211)	(0.0177)	(0.0244)	(0.0195)	(0.0189)	(0.159)
Employed before mig.	-0.0753	-0.0787	0.113***	0.138***	-0.0178	0.0443**	0.0320**	0.0144	-0.121
	(0.121)	(0.128)	(0.0253)	(0.0262)	(0.0176)	(0.0212)	(0.0156)	(0.0171)	(0.221)

... continue

Table 30: Fixed Effects Results, main outcomes

... continue

Upper Second. Educ.	-0.0744	0.177	0.0413*	0.0458*	-0.00799	0.0205	0.0124	0.000850	-0.0521
	(0.117)	(0.133)	(0.0233)	(0.0239)	(0.0154)	(0.0228)	(0.0182)	(0.0177)	(0.162)
Post-Sec. and Uni .	-0.362***	0.0489	0.0206	0.0252	0.0204	0.0381*	0.0172	0.0250	-0.0190
	(0.111)	(0.123)	(0.0213)	(0.0217)	(0.0154)	(0.0217)	(0.0174)	(0.0173)	(0.164)
Time in GER (in months)	-0.0110	-0.00469	0.0186***	0.0174***	0.00773***	0.00429	0.00268	0.000536	0.0280
	(0.0233)	(0.0313)	(0.00361)	(0.00370)	(0.00241)	(0.00334)	(0.00283)	(0.00285)	(0.0345)
Time in GER (in months) sq.	0.000277	-0.000334	-0.000178***	-0.000159***	-6.79e-05*	6.91e-05	2.94e-05	2.57e-05	-0.0004
	(0.000480)	(0.000685)	(5.25e-05)	(5.39e-05)	(3.71e-05)	(5.17e-05)	(4.58e-05)	(4.53e-05)	(0.0004)
Health BFM.	-0.0210	0.316***	0.00474	0.00277	0.00155	0.00867**	0.00280	0.00797***	-0.0398
	(0.0242)	(0.0314)	(0.00430)	(0.00437)	(0.00241)	(0.00372)	(0.00279)	(0.00290)	(0.0467)
Econ. situation BFM Bellow Avg.	-0.118	0.0309	0.00152	-0.00255	-0.0276*	-0.0382*	-0.000322	-0.0260	0.113
	(0.128)	(0.150)	(0.0238)	(0.0242)	(0.0151)	(0.0231)	(0.0175)	(0.0186)	(0.198)
Temporary Susp. of Deportation	-0.821***	-0.908***	-0.0468	-0.0395	-0.0290	-0.0403	-0.00224	-0.0284	0.106
	(0.268)	(0.282)	(0.0413)	(0.0417)	(0.0216)	(0.0370)	(0.0293)	(0.0279)	(0.422)
Request to Leave Germany	-0.269	-0.139	-0.113**	-0.107**	0.0366	-0.0781	-0.0848**	0.0132	0.221
	(0.380)	(0.350)	(0.0535)	(0.0538)	(0.0312)	(0.0477)	(0.0364)	(0.0392)	(0.710)
Decision Still Open	-0.362***	-0.268*	-0.0625	-0.0585	0.00735	-0.0528*	-0.0253	-0.0175	-0.209
	(0.138)	(0.154)	(0.0468)	(0.0474)	(0.0229)	(0.0306)	(0.0213)	(0.0233)	(0.729)
Status Unknown	-0.109	-0.420	-0.0196	-0.0358	-0.00847	-0.0385	-0.00264	-0.0350	0.00842
	(0.255)	(0.325)	(0.0353)	(0.0369)	(0.0241)	(0.0335)	(0.0266)	(0.0254)	(0.260)
Spouse in same HH	0.415***	-0.0338	-0.0297	-0.00963	-0.0386**	0.0294	0.00925	0.0253	
	(0.158)	(0.182)	(0.0273)	(0.0280)	(0.0173)	(0.0273)	(0.0203)	(0.0220)	
Spouse in diff. HH in GER	0.172	0.353	0.0415	0.0306	0.0175	0.0273	0.00581	0.0305	
	(0.218)	(0.256)	(0.0401)	(0.0413)	(0.0380)	(0.0438)	(0.0360)	(0.0355)	
Spouse Abroad	-0.366*	-0.0749	0.0861**	0.0929***	-0.0269	0.0858**	0.0398	0.0498	
	(0.197)	(0.222)	(0.0341)	(0.0352)	(0.0235)	(0.0389)	(0.0291)	(0.0336)	
All children live same HH	0.107	0.0357	-0.0589**	-0.0597**	-0.0422**	-0.121***	-0.0620***	-0.0421*	
	(0.151)	(0.180)	(0.0284)	(0.0288)	(0.0175)	(0.0284)	(0.0224)	(0.0229)	
Some children not in same HH	-0.167	-0.315	-0.0618*	-0.0520	-0.0430**	-0.0621*	-0.0290	-0.0184	
	(0.206)	(0.234)	(0.0338)	(0.0342)	(0.0187)	(0.0375)	(0.0288)	(0.0295)	
Satisf.with Living Situation - General	0.333***	0.149***							
	(0.0189)	(0.0205)							
Observations	2,261	2,264	2,314	2,314	2,314	2,314	2,314	2,314	408
R-squared	0.349	0.328	0.306	0.310	0.209	0.297	0.239	0.152	0.469
C. of origin x Y-M left origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-month left origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Huber–White standard errors

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