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

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ABSTRACT

Asylum seekers who migrate from developing countries to Europe frequently experience victimization events during their journey. The consequences of these events for their economic integration into destination countries are not yet well explored. In this paper, we analyze how victimization during asylum seekers' journeys affects their labor market integration in Germany by using survey data collected in the aftermath of the 2015 refugee crisis. Our data allow us to account for the exact timing and geography of migration, such that samples of physically victimized and nonvictimized refugees are balanced along a wide range of characteristics. We find that, compared to nonvictimized refugees, refugees who were physically victimized during their journey to Germany favor joining the labor force and taking up low-income employment rather than investing in host country human capital. To explain these findings, we explore a range of potential mechanisms and find suggestive evidence that experiencing physical victimization in vulnerable situations is not only associated with a decline in mental health but also with a "loss of future orientation" among physically victimized refugees, leading them to discount future payoffs more heavily.

1. Introduction

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

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

Our empirical strategy is based on the plausibly quasi-random nature of victimization events during the journey of asylum seekers. We identify the following four sources of omitted variable bias when linking victimization to economic integration outcomes: selection bias

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at the origin, survivor bias that we observe in the sample of arrivals, the unobserved ability to navigate the journey to safety that could determine both the likelihood of victimization and integration outcomes at the destination and the misreporting of victimization events. To address the concerns about selection bias at the origin and survivor bias, we limit the variation in the data to narrowly defined fixed effects categories by using detailed information on the timing and geography of migration. We restrict the variation to narrowly defined migration route fixed effects and month-year of departure fixed effects interacted with both country of origin fixed effects and month-year of arrival fixed effects.

In our setting, a further concern is that an unobserved ability to navigate the journey could determine the likelihood of victimization and affect integration outcomes at the destination. We address this concern in several ways. First, if cohorts that migrate during times of higher victimization risk do so due to their better (unobserved) ability to navigate the journey, the use of time of departure times country of origin fixed effects accounts for different victimization risk levels. Second, we review qualitative evidence on the victimization events along the main migration routes used by the largest refugee groups in our sample. International organizations and local nongovernmental organizations document many violent acts targeted at asylum seekers along the main migration routes[1] that are carried out by state authorities, criminal gangs, and escape agents. The available evidence suggests that these acts of violence are largely unpredictable for migrants who navigate unknown geographical territory.[2] Third, we condition all our estimates on a large set of individual-level characteristics that include pre-migration information on education, employment, wealth, and knowledge of a foreign language. Thus, we are able to analyze the effect of victimization events on post-migration educational and labor market outcomes, while accounting for the pre-migration ability to succeed in education and the labor market. We show that once the geography and the timing of migration are accounted for, there is little difference in observable characteristics between the physically victimized and the nonvictimized. Fourth, to further mitigate concerns related to unobserved ability bias, we use a coefficient stability test developed by Oster (2019) to provide an estimate of the relative importance of unobserved factors compared to the observed factors in our regressions. These tests show that the importance of unobserved factors would have to be multiple times higher than the large set of observables for the true estimated effects in our main regressions to be zero. Fifth, to improve our proxy for pre-migration ability, we deviate from the theory-based selection of control variables and deploy a postdouble selection LASSO (PDS), a data-driven machine learning technique for selecting all potentially relevant information that sufficiently improves the model fit to justify a reduction in degrees of freedom. When using the PDS, we include additional individual characteristics and life events and allow for all interactions and nonlinearities between individual characteristics and migration and geographic fixed effects. Finally, to rule out the misreporting of victimization events, we conduct tests on the sample of refugees who agreed to answer journey-related questions, which shows that neither the willingness to answer nor social desirability is likely to bias the obtained results.

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

To investigate these findings, we test several potential mechanisms. These range from institutional factors within the German asylum system to mechanisms related to the effects of victimization on refugees' mental health, time and risk preferences, financial hardship, and intention to remain in Germany. Our analysis reveals two main insights. First, both financial and physical victimization are associated with a decline in mental health. Second, physical victimization leads to a "loss of future orientation" or "impatience". Drawing on the literature in economics, we expect that deteriorating mental health reduces the likelihood of human capital investment and labor market participation (Akbulut-Yuksel, 2014; Bindler & Ketel, 2022; Blattman & Annan, 2010; Koppensteiner & Menezes, 2021; Leon, 2012; Shemyakina, 2011). Studies in psychology and economics further suggest that as physically victimized refugees develop a more pessimistic outlook and discount their future more heavily, they may be less inclined to pursue host-country-specific education, opting instead for low-skilled employment shortly after arrival (Beiser, 1987; Cadena & Keys, 2015; Hauff & Vaglum, 1993a; Hunkler & Khourshed, 2020; Kemptner & Tolan, 2018; Stoddard et al., 2015; Sutter et al., 2013). Our main findings suggest that the effect of physical victimization on refugees' time preferences outweighs the negative impact of declining mental health.

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

The remainder of this paper proceeds as follows. Section 2 discusses in more detail the conceptual framework that links victimization experiences to economic activity in the destination country. Section 3 identifies our data sources and provides the context for the empirical design. Section 4 introduces our estimation strategy and approach used to address econometric challenges. Section 5 shows the main results, and Section 6 tests alternative hypotheses that could explain our findings. Section 7 provides a concluding discussion.

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

[2] This is true for the migration routes used by the wave of refugees from Syria, Iraq, Iran, Afghanistan, and Pakistan who migrated between 2014 and 2017, the groups we analyze in this paper. The situation for asylum seekers from the Horn of Africa is considerably different.

2. Outline and framework

The situation of refugees who arrive in their host country is not easily comparable to that of the general population. Forcibly displaced migrants start their economic activity trajectory at zero in their host country. Most refugees originate from less developed countries, and their educational attainment is not regarded as equivalent to education obtained in economically advanced countries (Ludolph, 2023), and in many cases, refugees lack proof of their formal degrees or previous work experience. Once refugee status is received in the host country, most refugees face the decision to either (i) join the labor force immediately, accept a discount on their human capital, and take up low-skilled employment, or (ii) invest in host country-specific human capital to have access to better-paid employment in the future (Cortes, 2004; Duleep & Regets, 1999).

In this study, we aim to examine the impact of victimization on short-term labor force participation and investment in host-country education. The direction of this effect is a priori ambiguous. On the one hand, victimization during the journey may detrimentally affect refugees' overall economic activity in the host country. For example, experiencing victimization may lead to a decline in mental well-being, subsequently reducing both labor force participation and the propensity to invest in host country education. On the other hand, victimization may not affect the overall economic activity but rather influence refugees' choice between entering the labor force or pursuing host country education upon arrival in the destination country. For instance, victimized individuals may prioritize immediate benefits such as low-income employment over investing in host country education, which offers access to higher-quality employment opportunities in the future. This shift in preference toward short-term gains may occur if, for instance, victimization events increase impatience or diminish long-term intentions to remain in the host country.

In the following discussion, we provide an overview of the mechanisms that link victimization events to the decision to join the labor force or obtain host country education among refugees based on existing literature.

Mental health. The decline in health and mental well-being following traumatic events is one likely mechanism that links victimization events and economic choices (Dolan et al., 2005; Johnston et al., 2018; Mahuteau & Zhu, 2016). Studies on the general population find negative consequences of victimization events, such as robbery or rape, on labor force participation, employment, earned income, and increased welfare dependency among those affected (Bindler & Ketel, 2022; Ornstein, 2017; Velamuri & Stillman, 2008). A large body of research, primarily conducted in developing countries, further documents distortions to human capital investment decisions following potentially traumatic events in conflict-related or high-crime settings (Akbulut-Yuksel, 2014; Blattman & Annan, 2010; Koppensteiner & Menezes, 2021; Leon, 2012; Shemyakina, 2011). Hence, due to their relatively poorer mental health, victimized refugees could be less attached to the labor market and invest less in education than nonvictimized refugees.

External locus of control. Locus of control is a widely used concept in psychology, which describes the extent to which people believe that they have control over the outcome of events in their lives. Individuals with a strong external locus of control tend to believe external factors beyond their influence determine the events in their lives.

Previous research in psychology suggests that victimized children are more likely to report higher levels of external locus of control than non-victimized (Gong et al., 2021; Radliff et al., 2016). Hence, it could happen that victimized asylum seekers develop a belief that their lives are under the control of forces outside themselves, such as luck or the mercy of others. This, in turn, could lower both their propensity to invest in education and their job search efforts.

Time preferences. The choice refugees face between entering the labor force immediately and pursuing further education most closely resembles the choice set of adolescents and young adults who have just finished compulsory schooling. Within this group, exposure to violence is associated with lower educational investment (Stoddard et al., 2015) and a general loss of future orientation (Monahan et al., 2015; Ramos et al., 2013; Schmidt et al., 2018). Evidence from the sociology and psychology literature supports the view that victimization experiences have similar effects on future-oriented planning among refugees. Refugees victimized during their flight to safety have worse mental health outcomes (Hauff & Vaglum, 1993b) and are equally or more likely to be in the labor force than non-victimized refugees (Hauff & Vaglum, 1993a; Hunkler & Khourshed, 2020). Victimized refugees tend to invest less in host country-specific education than non-victimized refugees (Hauff & Vaglum, 1993a). A potential explanation for favoring early employment over long-term educational investment is that refugees who went through extreme events while fleeing their country have a shortened sense of their future (Beiser, 1987).

The economics literature supports this hypothesis. Recent experimental studies suggest that individuals' time preferences can be affected by extreme events linked to physical violence (Brown et al., 2019; Callen et al., 2014; Jakiela & Ozier, 2019; Voors et al., 2012) and health shocks (Decker & Schmitz, 2016).[3] Time preferences have, in turn, been found to affect human capital acquisition among younger individuals (Cadena & Keys, 2015; Kemptner & Tolan, 2018; Sutter et al., 2013).[4] Consequently, experiencing victimization during their journey can prompt refugees to prioritize entering the labor force and accepting low-income jobs instead of investing in the host country's human capital due to a 'loss of future orientation'.

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

Evidence mostly links risk preferences to individual labor market outcomes through a positive association between the willingness to take risks and self-employment (Schildberg-Hörisch, 2018). Other links are conceivable in the context of asylum-seeker victimization. For instance, a perilous journey could lead victimized refugees to become relatively more risk-averse in searching for their first job in the destination country, leading them to be less ambitious and settling for lower-paid positions.

Financial difficulties. People smugglers have been documented to be responsible for the abuse of refugees during their journey and often charge high fees for their services (Albahari, 2018). Data from the IAB-BAMF-SOEP refugee survey suggest that the average unconditional amount physically and financially victimized refugees paid to escape agents exceeded the amount that the non-victimized paid by EUR 1420

[3] Recent work has also found that time and risk preferences can be affected by natural disasters (Beine et al., 2020; Callen, 2015; Cameron & Shah, 2015; Cassar et al., 2017; Eckel et al., 2009; Hanaoka et al., 2018; Page et al., 2014) and financial and macroeconomic shocks (Guiso et al., 2018; Jetter et al., 2020; Kettlewell, 2019).

[4] Our analysis relates to this literature by indirectly measuring the time preferences of victimized versus non-victimized individuals. This interpretation assumes that individuals reveal their time preferences by engaging in certain activities (DellaVigna & Paserman, 2005). Individuals who attach more value to long-term rewards are more likely to pursue activities that entail an immediate cost (such as investing in human capital) but that have delayed payoffs (access to higher-quality employment in the future). On the other hand, impatient individuals are more likely to engage in activities with immediate benefits (such as low-income employment) and delayed costs (a lack of access to higher-quality employment in the future).

and EUR 1802, respectively.[5] Hence, one possibility is that victimized individuals want to enter the labor market faster to quickly recover the relatively high journey cost upon arrival. Another possibility is that victimization could reflect a lower ability to pay human smugglers or border patrols, which could lead to violent retaliatory acts by agents who demand payment.

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

Our empirical approach proceeds by first analyzing the effect of victimization on labor market outcomes and investment in host country education. We then test the different mechanisms discussed in this section as potential explanations for our findings. While many mechanisms can be at play simultaneously, we posit that the direction of the estimated effect of victimization on short-term labor market outcomes will capture whichever channels are stronger.

3. Data, definitions and background

3.1. Data and definitions

IAB-BAMF-SOEP refugee survey

The primary data source for our analyses is the Institute for Employment Research (IAB)-Federal Office for Migration and Refugees (BAMF)-German Socio-Economic Panel (SOEP) refugee survey.[6] The IAB-BAMF-SOEP refugee survey is an extension of the established German Socio-Economic Panel (GSOEP) and is designed for the population of asylum seekers and refugees in Germany. The sample was drawn from the German Central Register of Foreign Nationals (AZR), which makes the survey representative of asylum seekers arriving in Germany since 2013. The survey has a panel structure with interviews conducted in three waves in 2016, 2017, and 2018 with a total of 6763 individuals. For details on the survey's design, methodology, and response rate see Kroh et al. (2017).

The survey provides a wide range of pre- and post-migration information and detailed individual and household characteristics. Most importantly, the first-time respondents are interviewed (on average 18 months after migrating), they are asked detailed questions about the experiences that they went through during the journey from their country of origin to Germany. A total of 3742 individuals, 55.2% of the total sample, agreed to provide information on these experiences (we address this issue in Section 3.2). We have all relevant information, including all necessary control variables and additional outcomes, for our effective working sample that consists of 3004 individuals aged between 18 and 65 years.

Among the questions posed, our main interest lies in the survey question 'During your journey or escape, did you experience one or more of the following?' which allows respondents to choose one or more answers from a list of negative experiences. Based on their responses, we create a binary physical victimization indicator that takes the value of one if an individual was subjected to sexual abuse, physical attacks, incarceration, or a shipwreck (or any combination of these). We further create a binary financial victimization indicator that takes the value of one if an individual was subjected to financial fraud, extortion, robbery, or blackmail (or any combination of these). The reason we split the victimization indicator into one that captures the experience of more severe physical harm and one that captures financial harm is twofold. First, recent evidence on the link between crime victimization and labor market outcomes shows that the physical victimization experience has stronger adverse labor market consequences for the affected (Bindler & Ketel, 2022). Second, unlike physical victimization, financial victimization during asylum seekers' flight to safety may affect labor market outcomes through the need to recover financial losses once the destination is reached. We are able to capture this mechanism in our data. Table 1 shows the summary statistics for the two victimization indicators.

We note that some victimization events are likely correlated. For example, while extortion is primarily associated with financial loss, it can involve the use of force. Similarly, incarceration, primarily a form of physical punishment, may be part of an extortion scheme. Thus, individuals may experience both financial and physical victimization. Reassuringly, the correlation between these two ($r = 0.326$) is sufficiently low to not be a cause for concern in our regression analyses. We further note that some migrants experienced more than one victimization event, but we nevertheless modeled our preferred indicators as binary for two main reasons. First, the majority of migrants experienced one victimization event. Only 12.% of all individuals in our sample experienced more than one physical victimization event, and 15.4% experienced more than one financial victimization event. Second, there is no clear guidance in the literature on the correct functional form of the relation between our outcomes of interest and multiple victimization events that individuals experienced on their journeys, which lasted 42 days on average. We explore different constructions of the victimization indicators in Appendix N, where we consider both a discrete and a continuous measure of the number of victimization events.[7] Finally, we use the term "victimization" instead of "trauma" to remain neutral regarding the potential effects of these events on individuals. In the context of our research, this term seems most appropriate: even in events such as shipwrecks, where there is no direct aggressor, asylum seekers who experience these incidents are likely victims of smugglers who intentionally overload boats for financial gain.

For the economic integration outcomes, our main interest is on labor force participation, education and training, and employment. We complement our main analyses with a more detailed analysis of employment, which we split into full-time, part-time and marginal employment, and net monthly income.[8] The economic integration outcomes are measured in the last interview, which is 31 months after arrival on average.

We rely on different questions from the IAB-BAMF-SOEP refugee survey to proxy for our mechanisms of interest. Drawing on the psychology and health economics literature reviewed in Section 2, we use

[5] The average amount paid in Euros to a smuggler was EUR 3174 among non-victimized individuals, EUR 4598 among the physically victimized, and EUR 4976 among the financially victimized.

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

[7] In Table 13 in Appendix F, we provide statistics of the victimization rates across the different migration cohorts, main countries of origin, and main migration routes.

[8] Summary statistics for these measures are shown in Table 7 of Appendix A for the last observation available for each individual in the panel.

Table 1
Physical and financial victimization indicator.

Variable	Mean	Std. Dev.	Variable	Mean	Std. Dev.
Experienced robbery	0.133	0.340	Experienced sexual harassment	0.018	0.132
Experienced extortion	0.151	0.358	Experienced a shipwreck	0.137	0.344
Experienced fraud	0.282	0.450	Experienced a physical attack	0.148	0.356
			Experienced incarceration	0.198	0.398
Financial victimization	0.384	0.486	**Physical victimization**	0.363	0.481
Observations	3004		Observations	3004	

life satisfaction and self-assessed health measured on a scale from 1 to 10 (with 10 being the highest value) as our primary indicator to study the mental health effect of victimization (Johnston et al., 2018).[9]

To test the locus of control channel, we create a continuous variable that sums the answers to the following five questions: (1) "In comparison with others, I haven't achieved what I deserved to achieve"; (2) "What can be achieved in life is mainly a result of fate or luck"; (3) "I often find that other people dictate my life".; (4) "The options that I have in life are determined by social circumstances"; and (5) "I don't have much control over what happens in my life". In each question, individuals can rate themselves on a scale from 1 (totally disagree) to 7 (totally agree). Hence, a higher value reflects an increased external locus of control. Because these questions were asked for the first time in 2016, we have a considerably smaller sample size for the external locus of control variable.

While we cannot directly measure time preferences, we can provide evidence on the time preference channel by analyzing an additional outcome related to individuals' time *perception*. Contributions in the psychology literature have shown that time preferences affect how individuals perceive time itself. The link is intuitive; impatient individuals who discount payoffs in the future tend to experience a slower passage of time, are less comfortable waiting, and tend to overestimate their waiting time (Jokic et al., 2018; Wittmann & Paulus, 2008; Wittmann et al., 2015). Wittmann et al. (2015) further shows that the feeling of being under time pressure is directly linked to frequently thinking about adverse events that were experienced in the past. We measure time perception through the survey question, "How often in the last four weeks did you feel rushed or under time pressure?" We invert the original scale such that 1 corresponds to "Never" and 5 corresponds to "Always".[10] To measure risk aversion, we rely on a question that asks, "In general, are you ready to take risks, or do you try to avoid risks?" and allows respondents to rate themselves on a scale from 0 (risk averse) to 10 (fully prepared).

For the intention to stay channel, we use a question that asks, "Would you like to stay in Germany permanently?" and create a binary variable that takes the value of 0 if respondents answered "no" and takes the value of 1 if they answered "yes". To test the financial hardship hypothesis, we approximate the level of financial precariousness of refugees in Germany by the extent to which the survey respondents report being worried about their personal finances at the time of the first interview. We create a binary indicator that takes a value of 1 for individuals who state that they are "very concerned about their finances", and 0 otherwise. We measure mental health, life satisfaction, time pressure, risk preferences, external locus of control, intention to stay in Germany and worries about finances in the first interview since these measures may become endogenous to our outcomes of interest.[11]

IAB integrated employment biographies

We use the IAB integrated employment biographies (IEB) to complement the survey employment questions with more reliable individual administrative records. The IEB data consists of all individuals in Germany who are characterized by at least one of the following employment statuses: employment subject to social security, a marginal part-time job, benefit recipient, officially registered as job-seeking, or (planned) participation in programs of active labor market policies.[12] The IEB data form a comprehensive dataset with daily precision and very little attrition.

The IEB data can be linked to only 66% of our original sample,[13] and we, therefore, rely on the employment outcomes from the survey as our primary data source. Nevertheless, the more precise IEB job market data allow us to add some further suggestive evidence.[14]

Further data sources

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

3.2. Reliability of self-reported victimization

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

The first concern relates to a potential link between the employment status of respondents and the willingness to answer the journey-related questions. We note that asylum seekers are asked about the occurrence of victimization events only after agreeing to answer "a few questions about the experiences connected with your [their] escape".[15]

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

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

[11] Summary statistics for these measures are shown in Table 7 of Appendix A.

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

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

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

[15] The full question is "Next, we have a few questions about the experiences connected with your escape. Some of the questions will be about negative experiences. Would you like to answer questions about this subject or would you prefer not to answer these questions?".

Furthermore, the journey-related questions were part of the survey only in the first interview, when the average time since migration was seventeen months, and only 9.3% of the refugees in our sample were employed (in the last interview, 20.9% were employed). Thus, employment status is unlikely to significantly affect the willingness to answer the victimization question. To further mitigate this concern, we use our full sample of respondents who agreed and did not agree to answer the journey-related questions[16] (5543 individuals) and show in Panel A of Table 10 in Appendix E that employment status at the first interview has no significant effect on the willingness to answer the journey-related questions.[17]

The second potential source of bias relates to a potential systematic misreporting of victimization events. As we explain in Appendix E, the structure of the survey largely alleviates this concern. To further strengthen our argument, we show in Panel B of Table 10 in Appendix E that the willingness to respond to journey-related questions in the first survey wave is not a significant predictor of individuals' employment status in the last available survey wave.

A third and related potential problem could apply if only the least traumatized individuals agreed to reply to the journey-related questions. In Panel A of Table 10 in Appendix E, we show that the level of mental health in the first interview does not affect the willingness to reply to the journey-related questions.

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

3.3. Context: Victimization along the main refugee routes

For the purposes of our study, we are interested in the arbitrariness of physical and financial victimization events along the main migration routes with respect to observable individual-level characteristics once we account for geographical and time factors. Victimization along migration routes differs from victimization events observed in cities.[18] Although a fair share of crimes committed in a city deliberately target a specific individual or property, victimization events along migration routes are unlikely to be premeditated with respect to a specific person. The paths that asylum seekers take are not a day-to-day activity that they perform routinely. Indeed, most asylum seekers take a migration route only once and are navigating unknown territory. Border patrols along the migration routes are actively seeking to catch refugees attempting to cross borders — irrespective of the individual characteristics of refugees.

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

To narrow the scope of interest, we focus on the main cohort, countries of origin and migration routes taken by asylum seekers to reach Germany.[19] In our IAB-BAMF-SOEP working sample, the largest arrival cohort reached Germany in 2015 (65% of the sample), with fewer refugees arriving between 2012 and 2014 (20%) and from 2016 to 2017 (14%). These numbers accurately reflect the official statistics on the entry of asylum seekers into Germany found on Eurostat. The first and largest group of asylum seekers are Syrians (63% of the sample), followed by Iraqis and Iranians (16%) and Afghans and Pakistanis (10%).[20]

Asylum seekers from Syria, Iraq, and Iran typically cross land borders into Turkey and, from there, follow the Eastern Mediterranean route (EMR), which runs from Turkey to Bulgaria or Greece either through the mainland or by boat. Afghans and Pakistanis reach Turkey either through Iran or Lebanon (Crawley et al., 2016).[21]

Consistent with UNHCR data (UNHCR, 2017), approximately 69% of Syrians in our sample took the EMR (either by sea or land). This compares to 74% of Iraqis and Iranians and 64% of Afghans and Pakistanis (Table 12 in Appendix F). Once in Greece or Bulgaria, the most frequently used route by asylum seekers to reach Western and Northern Europe is the so-called Balkan route (IOM, 2015; UNHCR, 2017).[22]

The UNHCR, HRW, and Amnesty International have documented widespread chain pushback in Greece, Bulgaria, and along the Balkan route and a series of unlawful detentions within these countries (Amnesty International, 2016; Balla, 2016; Banich et al., 2016; Redden, 2015; UNHCR, 2017, 2018). These detentions and pushbacks have been characterized by the frequent and arbitrary use of violence and by the appropriation of asylum seekers' financial resources by local authorities (Amnesty International, 2016; Balla, 2016; Oxfam and Belgrade Centre for Human Rights and Macedonian Young Lawyers Association, 2017; Redden, 2015; UNHCR, 2017). Authorities have used violence against both male and female asylum seekers, which is

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

[17] The empirical specification is outlined in Section 3.4 and considers a series of pre- and postmigration characteristics and time and geographic controls.

[18] City crime has been studied in more detail in the economics literature (Bindler & Ketel, 2022; Mahuteau & Zhu, 2016; Ornstein, 2017) and Velamuri and Stillman (2008).

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

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

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

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

consistently characterized by physical abuse through the use of batons and by hitting and kicking (Amnesty International, 2015, 2016; HRW, 2016, 2018a, 2018b; Tondo, 2018).[23,24]

Despite the increasing amount of violence and pushback found in Greece and along the Balkan route, most asylum seekers still considered the route through Turkey and Greece to be less dangerous than traveling through Libya (Crawley et al., 2016). This has contributed to sustaining the flow of migrants through the Balkan route, which decreased only after the 2016 EU-Turkey deal.

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

3.4. Balance tests

The evidence presented in Section 3.3 supports the idea that border patrols and criminal actors along the Balkan route target asylum seekers who attempt to cross the border, irrespective of individual characteristics.

In Table 14 in the Appendix, we show a conditional balance test to evaluate the hypothesis that individual-level characteristics do not predict victimization events, conditional on migration timing and geography. To test this hypothesis, the physical and financial victimization indicators are regressed on a set of backward reported premigration indicators, conditional on their geographical origin, migration timing (and their interaction term), and the migration route. Additionally, physical victimization is conditioned on experiencing financial victimization, and vice versa.

The results show a balanced sample for the physically victimized (Column (1)). We note that, on average, individuals with a university degree and those speaking German before migration are marginally less likely to experience physical victimization. These variables are included in our proxy for premigration ability, which our estimations routinely control for. Experiencing financial victimization is correlated with the economic situation and employment levels before migration, suggesting that individuals with greater premigration wealth are more likely to experience financial victimization. This is expected because to be robbed or extorted during the journey, individuals had to be carrying financial resources with them. Finally, health satisfaction before migration and willingness to take risks are further significant predictors of financial victimization, albeit in opposing directions. Thus, we conclude that although the sample of physically victimized migrants is balanced along a wide range of individual-level premigration characteristics once conditioned on geography and migration timing, financial victimization events seem to occur less randomly. Even though our data allow us to control for a large set of potentially confounding variables to mitigate this problem, the coefficients on the financial victimization indicator should be interpreted as associations rather than causal estimates. We further deploy several additional tests, which are outlined in Section 4.3, to study the significance of unobserved factors that could bias our results.

4. Empirical strategy

The empirical strategy in this study relies on the assumption that conditional on individual premigration characteristics, as well as migration-related and geographical factors, physical victimization is a quasi-random event. We start by describing our main specification in Section 4.1 to estimate the effect of victimization on economic integration outcomes. Sections 4.2 and 4.3 explain the extensions.

4.1. Main specification

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

$$\begin{aligned} Y_{i,c,a,t,\mu,f} =& \gamma_1 PhysicalVictim_i + \gamma_2 FinancialVictim_i + \zeta Baseline_{i,t} \\ &+ \eta PreMig_{i,\mu} + \varphi PostMig_{i,t} \\ &+ Route_i + ConflictIntensity_{i,\mu} + \delta_f + \beta_a + \kappa_{c,\mu} + \epsilon_{i,c,t,\mu,f} \end{aligned} \quad (1)$$

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

The categorical variable $Route_i$ indicates the migratory route taken, namely, the Eastern Mediterranean Sea, Central Mediterranean, Western Mediterranean, Eastern Mediterranean land, Eastern Land border, and traveling directly to Germany by plane routes; a final option is if no route information is available.[25,26] $ConflictIntensity_{i,\mu}$ measures the conflict intensity in the province (within country) of origin around the time of migration as explained in Section 3.1 and Appendix B. Controlling for conflict intensity accounts for selection into migration at different levels of push factors at the origin (Aksoy & Poutvaara, 2021; Guichard, 2020) and for the possibility that the individual-specific response to victimization might depend on previous traumatic experiences (Breslau et al., 2008; Yehuda, 2002). δ_f is a fixed effects term that captures the German federal state where the refugee resides at the time of the survey, and β_a refers to the year of arrival fixed effects

Despite controlling for the route traveled and the conflict intensity at origin around the time of migration, the time-varying expected risk of victimization may still affect the decision to migrate.[27] For instance, cohorts that decide to migrate during times of higher victimization risk may do so due to their better (unobserved) ability to navigate the journey. Victimization events occurring within such cohorts may

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

[24] The stance taken against refugees in some Balkan countries, such as Hungary, seems to be driven by general xenophobia toward refugees and migrants (Assembly, 2016; Crawley et al., 2016; Deardorff Miller, 2018; Rankin, 2019). We provide some further details in Section G.

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

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

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

then capture ability, while in other cohorts, victimization events may capture the lack of such ability. To better capture self-selection into migration at the origin, we use country-of-origin by year-month-of-migration fixed effects ($\kappa_{c,\mu}$). This approach limits the variation in our variables of interest to groups of migrants who chose to migrate at the same time and originate from the same place.

$Baseline_{i,t}$ is a vector of individual-level characteristics which includes the age, age squared, and gender of respondents. One concern in specification (1) is that γ_1 and γ_2 may be biased by an intrinsic ability to navigate difficult situations that could also affect the ability to succeed in the destination country. While the qualitative and quantitative evidence presented in Sections 3.3 and 3.4 supports the random nature of victimization events in the specific context we are studying, we further mitigate this concern by conditioning on a proxy for intrinsic ability using a vector of time constant premigration information, $PreMig_{i,\mu}$. This vector includes premigration information on the relative wealth of a respondent's household, individual employment experience, educational attainment and knowledge of the German language. We further include backward reported measures of health and life satisfaction before migration. Collectively, these variables reflect the premigration capacity to achieve economic success, which is our primary outcome of interest in the destination country. Further discussion on the relationship between the premigration control variables and their ability to capture individuals' intrinsic ability is provided in Appendix I. $PostMig_{i,t}$ is a vector of individual postmigration information which includes the number of months that a refugee has spent in Germany and its squared term, the asylum status measured at the time of the survey t, and whether an individual arrived in Germany alone or accompanied.[28] For the labor market outcomes, we include a set of variables related to the residence of the spouse and the location of children.[29] $\epsilon_{i,c,t,\mu,f}$ constitutes the error term of this specification. We label this specification "Benchmark specification" in our regression tables and refer to it as our preferred specification throughout this paper.

Since the survey has a longitudinal dimension, but our variable of interest is not time-varying, we estimate Eq. (1) as a cross-section. When studying the effects of victimization on outcomes related to health, time perception, and other mechanisms, we use the first observation available for each individual. We do this because when the refugees were interviewed for the first time, they had spent only 19 months in Germany on average. Thus, negative experiences during the journey to Germany could still affect their mental well-being outcomes. Using the first observation also reduces the potential reverse causality problem of mental well-being and employment (Brown et al., 2010; Kassenboehmer & Haisken-DeNew, 2009); only 9.3% of our sample were employed in the month before the interview when first surveyed. We then use the last observation in the sample to study the effect of victimization on economic integration outcomes. At this point, individuals had spent an average of 31 months in the country, and 20.9% were employed. The average difference between each individual's first and last observation is only 12 months. Therefore, concerns about potential sample attrition due to selective return migration are minimized, while the additional variation that we gain in our outcomes of interest is considerable.

Additionally, in Appendix P.2, we exploit the panel variation in the data and estimate a (individual i) random effects model under the assumption that $corr(\epsilon_{i,c,t,\mu,a,f}, X) = 0$.[30] A large number of time-constant variables in the model, including the set of fixed effects related to the time of migration and the origin of individuals, makes this key assumption of a random effects model plausible in our setting (Wooldridge, 2010).[31]

4.2. Survivor bias

Not all forcibly displaced migrants who decide to embark on the journey to Germany make it to their preferred destination. Changes across time in the journey's difficulty may have nonrandom effects on arrival cohort composition, even when narrowly conditioning on the selection at the origin at different points in time.[32] We refer to this empirical issue as survivor bias. In theory, such a change in the composition of asylum seeker arrival cohorts can influence not only the probability of victimization but also their performance in the German labor market.

To the best of our knowledge, no previous research exists that could inform our empirical strategy regarding survivor bias and the extent to which it is a concern in our setting. Empirically, we partially address the issue of survivor bias by deploying a large set of dyadic fixed effects (departure-arrival-origin). Thus, in addition to our preferred specification shown in Eq. (1), we estimate a model that includes the year-month of arrival fixed effects interacted with the year-month of departure and country of origin, $\varrho_{c,\mu,a}$. When using this triple interaction, our sample size is reduced since we are left with some singleton cells where we have only one asylum seeker from a specific country of origin departing in a given month and arriving in a given month, such that the individual observation is dropped.

4.3. Further methods to address omitted variable bias

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

To further test the sensitivity of our results to the modeling choice and improve our proxy for ability, we extend the benchmark specification (1) by allowing for all possible interactions and nonlinearities between individual characteristics and the country of origin, time of departure, conflict intensity, route and cohort of arrival fixed effects. In this more demanding specification, we also include further potentially relevant information drawn from the survey data that captures the following: willingness to take risks; the use of an escape agent; the cost of the escape agent; the means of financing the escape journey; the means of transportation used to reach Germany; the self-reported reason for migrating; having stayed in another country for three or

[28] Since asylum status directly affects an individual's right to work in Germany, it is necessary to include it as a control variable - despite being determined after migration. Refugee status grants full access to the labor market, while asylum seekers awaiting a decision can work only under certain conditions (Asylum Information Database, 2024). During the main observation period of our study, asylum seekers were eligible to apply for a work permit after three months, provided they met specific requirements related to their likelihood of receiving protection (Degler et al., 2017).

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

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

[31] We note that since all asylum seekers naturally start their stay in Germany as economically inactive and the likelihood of engaging in economic activity then increases over time, the panel estimates that capture the average effects over time are not directly comparable to the cross-sectional estimates based on only the final observation of each individual.

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

more months before coming to Germany; and the duration of the journey in days.[33]

A drawback of including such large sets of fixed effects and interaction terms in the model is a loss of statistical precision. To balance this trade-off, we use a least absolute shrinkage and selection operator (LASSO) to select variables that sufficiently improve the model fit to justify the reduction in degrees of freedom.[34] We follow Belloni et al. (2014) and use a postdouble selection LASSO (PDS) to find predictors of the selection equation and outcome equation, where the selection equation refers to the predictors of physical and financial victimization. A more technical explanation of these selection methods is provided in Appendix J.

Despite conditioning all estimates on relevant premigration and selected postmigration variables and accounting for selection effects at various stages of the migratory journey, omitted variable bias may nevertheless be a concern. To obtain an idea of the severity of such potential bias, we follow Oster (2019) and analyze the sensitivity of our coefficients of interest to their conditioning on observables. If the coefficients are stable after the inclusion of observed controls, then we can consider this suggestive evidence that omitted variable bias is limited.[35]

5. Results

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

5.1. Labor market outcomes

Table 2 Panel A reports the effects of physical and financial victimization on overall economic activity, which is defined as refugees in the labor force or those who pursue host country-specific education. Column (1) refers to a basic specification that does not include any control variables. Columns (2)-(4) refer to the specifications described in Section 4.[36] We do not find a negative effect of victimization during the journey on economic activity in the cross-sectional regressions (1) to (4). In our preferred specification under Column (2), physical victimization has a positive effect on being economically active. No such effect can be found for the financially victimized.

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

Table 2 Panel C shows the effects of victimization on pursuing host country education and training. By design, the results complement those of Panels A and B. Physical victimization significantly decreases the propensity to pursue host country-specific education or training across all specifications. In our preferred specification, this negative effect reaches 3.1 percentage points, which is a sizeable decrease considering that the total share of refugees in our sample who pursue education or training stands at 8.4 percentage points 31 months after arrival (see Table 7). The coefficients estimated on the financial victimization indicator show no effects across all specifications.

Accordingly, our findings indicate that a physical victimization event (i) increases the propensity to join the labor force early on, and (ii) decreases the propensity to pursue host country education and training. We explore the mechanisms that could be behind this finding in Section 6.

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

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

To understand the underlying mechanisms, we now turn to the expected consequences of early labor force entry and look at the type of jobs and wages refugees obtain. Table 3 shows the results with different types of employment rates as the dependent variable for the full sample of refugees. We report only the results for our benchmark specification.

The results in Table 3 suggest that among physically victimized individuals there is an increased uptake of part-time and marginal employment vis-à-vis the nonvictimized. Column (1) reports the employment rate of physically victimized refugees for the last observation available of each individual, at an average duration of stay in Germany of 31 months. The rate is 3.4 percentage points higher than the employment rate among the nonvictimized at this point. This early employment uptake is characterized by the poor-quality jobs available to refugees. Around two-thirds of the higher employment rate among the physically victimized is explained by employment in part-time and marginal jobs (Column (3)). Less than one-third of the effect is explained by full-time employment, which is a magnitude that is no longer statistically distinguishable from zero at conventional levels (Column (2)). Column (4) is estimated only for the sample of employed refugees. Although imprecisely estimated, the results provide suggestive evidence that 31 months after arrival, these differences

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

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

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

[36] In Column (3), the sample size is smaller because the model specification is very demanding (triple interaction), and we are left with several single cells.

Table 2
Economic activity.

	Basic (1)	Benchmark (2)	Dyadic FE (3)	PDS (4)
Panel A: LFP or Education				
Physical victim.	0.0509***	0.0424**	0.0447**	0.0254
	(0.0168)	(0.0167)	(0.0211)	(0.0161)
Financial victim.	0.0187	−0.0111	0.0057	−0.0124
	(0.0168)	(0.0161)	(0.0207)	(0.0157)
Panel B: Labor force participation				
Physical victim.	0.0631***	0.0608***	0.0676***	0.0367**
	(0.0172)	(0.0170)	(0.0215)	(0.0164)
Financial victim.	0.0188	−0.0152	0.0071	−0.0168
	(0.0172)	(0.0165)	(0.0213)	(0.0161)
Panel C: Education and training				
Physical victim.	−0.0197*	−0.0306***	−0.0275*	−0.0191*
	(0.0108)	(0.0117)	(0.0148)	(0.0111)
Financial victim.	0.0156	0.0090	0.0075	0.0111
	(0.0111)	(0.0114)	(0.0141)	(0.0109)
Observations	3004	3004	2321	3004
Individual controls	No	Yes	Yes	Some
Year of arrival FE	No	Yes	No	Some
C.origin*Departure FE	No	No	No	Some
C.origin*Departure FE*Arrival FE	No	No	Yes	Some

Huber-White standard errors; *p < .1; **p < .05; ***p < .01
Notes: The dependent variable is binary and takes a value of 1 for individuals in the labor force or education (Panel A), individuals in the labor force (Panel B), or those pursuing host country education or training (Panel C). LFP stands for labor force participation. Columns (1) to (4) use observations that correspond to the last interview conducted, which is 31 months after arrival on average. The term FE indicates fixed effects. PDS refers to the post-double-selection LASSO. Departure FE signifies the year-month of displacement from the home country, and Arrival FE identifies the year-month of arrival in Germany. C.origin refers to country of origin fixed effects. In the dyadic FE regressions, the sample size is smaller because of singleton observations in the triple interaction.

Table 3
Employment.

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

Huber-White standard errors; *p < .1; **p < .05; ***p < .01
Notes: The dependent variable is binary and takes a value of 1 for employed individuals, with the regressions showing employment outcomes for any form of employment (Column (1)), full-time employment (Column (2)) and part-time or marginal employment (Column (3)). When employment is divided, the other types of employment are set to zero. All samples use observations that correspond to the last interview conducted, which is 31 months after arrival on average. The dependent variable in Column (4) is the log of income among employed individuals. The term FE indicates fixed effects. The term Departure FE refers to the year-month of displacement from the home country and C.origin denotes country of origin fixed effects.

result in a 14% wage gap between the nonphysically victimized and the physically victimized. We note that this difference is likely to increase in the future when the nonphysically victimized complete their training and education.

The IAB-BAMF-SOEP refugee sample contains further information on the training requirements for a subset of 569 of the 751 employed individuals in our sample, as shown in Table 15 in Appendix M. The tabulation shows that physically victimized individuals take up jobs that do not require any training at a higher frequency when compared to nonphysically victimized. In contrast, the share of physically victimized employed in jobs requiring professional training or university is considerably smaller. The analysis of job skill requirements further supports the idea that the faster employment uptake among the physically victimized is characterized by low-skilled employment.[37]

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. Fig. 1 shows the unconditional Kaplan–Meier curve of time to first employment, where failure is defined as obtaining employment, and the x-axis indicates the number of months since arrival in Germany. The analysis is based on a subsample of 1625 survey respondents who gave their consent to be linked to administrative employment records. Of these individuals, 751 obtained employment at some point over the observed time period; we note that this share is larger than the 21.8% in our cross-sectional regressions. The difference is explained by the IEB data, which extends beyond the last available survey wave. The cross-sectional regressions that we present thus far correspond to the 31-month point on the x-axis.

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

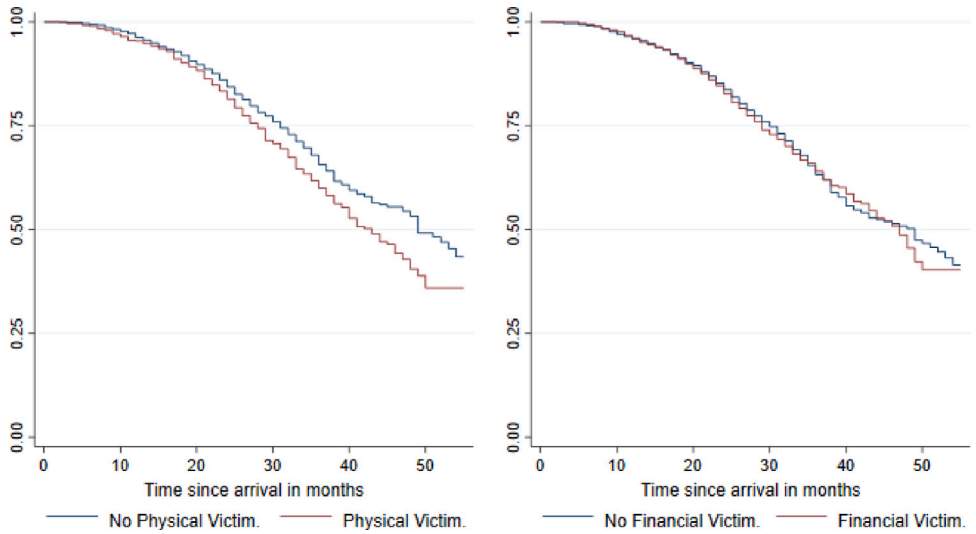

Fig. 1. Kaplan–Meier estimates of time to first employment. Fig. 1 is based on a subsample of 1625 survey respondents who gave their consent to be linked to the IEB data. Of these individuals, 751 obtained employment at some point over the observed time period; we note that this share is larger than the 21.8% in our cross-sectional regressions. The difference is explained by the IEB data, which extends beyond the last available survey wave.

The graph on the left in Fig. 1 shows that compared to the non-victimized refugees, physically victimized refugees obtain employment faster. The gap starts to expand approximately 18 months after arrival; we explicitly analyze this dynamic further in Section 6. The graph on the right shows the same comparison for the financially victimized, where we do not detect any effect. Table 17 in Appendix M further reports the estimated output of the simple Cox proportional hazard model. The parameter estimates show an increase in the expected log of the relative hazard for the physical and financial victimization groups vis-à-vis the nonvictimized. Exponentiating the parameter estimates shows that the expected hazard, which is equal to finding employment, is 1.23 times higher for the physically victimized than for the nonvictimized on average.

5.2. Testing for the significance of unobserved confounding variables

In our main regressions, we control for a wide range of individual-level (premigration) socioeconomic characteristics, which should mitigate the risk of omitted variable bias. Nevertheless, to assess the relative importance of unobserved factors, we apply the coefficient stability test developed by Oster (2019) as discussed in Section 4.3.

Table 4 shows the estimated δ values that correspond to the results of our preferred specification in Table 2 (Column (2)), and Table 3 (Columns (1)-(3)). All δ values indicate that the explanatory power of omitted variables would have to be very large compared to the variables included in the model for the estimated coefficients on physical victimization to be zero. For example, in Table 2, Panel C, Column (2), we estimate that refugees who were physically victimized on their journey to Germany were 3.1 percentage points less likely to be in education or training 31 months after arrival compared to the non-physically victimized. For the obtained coefficients to be zero instead, the unobserved variables would have to be 5.4 times larger than the control variables included in the model. The only value below the $\delta = 1$ threshold recommended by Oster (2019) is the obtained coefficient found on full-time employment in Table 3 Column (2). However, the coefficient is not statistically significant from zero at any conventional level in our estimation. Accordingly, all test results suggest that the estimated effect of physical victimization on integration outcomes is highly robust to omitted variable bias.

6. Mechanisms

Our findings strongly support the hypothesis of a distortion in the decision to enter marginal employment rather than investing in host-country-specific education or training. In this section, we explore which of the mechanisms discussed in Section 2 could be driving these findings. Self-reported measures related to mental health, time preferences, risk preferences, external locus of control, financial difficulties, and the intention to stay in Germany are based on the first interview of the panel conducted 19 months after arrival on average. Measuring these variables shortly after arrival mitigates the risk of endogeneity to our outcomes of interest. All the results use our benchmark specification as in Column (2) of Table 2. The sample size varies slightly since not all questions used to study these mechanisms are asked in all survey waves.

Health outcomes. Health outcomes are captured by the encompassing measures of mental well-being and life satisfaction in Columns (1)-(2) of Table 5. Column (1) shows that the effect of physical victimization on self-reported health at the time of arrival in Germany is negative. Using our preferred specification, the magnitude of the coefficient is 0.22 points ($p < .05$), which corresponds to a decrease of approximately 11% in the standard deviation of the measure. Financial victimization shows an even larger negative effect on self-reported health (0.31 points; $p < .01$).[38] The life satisfaction outcomes in Column (2) show a similar pattern.

These findings thus confirm the negative effect of victimization on the mental well-being and health of refugees established in previous studies on the general (non-refugee) population (Dolan et al., 2005; Johnston et al., 2018; Mahuteau & Zhu, 2016).

As discussed in Section 2, existing literature strongly points to a negative effect of a decline in mental health on labor force participation, education, and short-term employment. If the mental health channel were the stronger mechanism driving our findings, we would have found a negative effect of physical victimization on labor force participation, education, and short-term employment. However, we find that physical victimization leads to a distortion around the decision to

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

Table 4
Oster stability test.

	LFP &. Educ. (1)	LFP (2)	Educ. (3)	All employ. (4)	FT employ. (5)	PT or marg. employ. (6)
δ (Physical trauma)	2.407	3.611	−5.403	1.283	0.487	3.875
δ (Financial trauma)	−0.393	−0.461	5.995	−0.175	−0.0891	−0.292
R-squared	0.317	0.322	0.205	0.297	0.238	0.175
Observations	3004	3004	3004	3004	3004	3004

*p < .1; **p < .05; ***p < .01
Notes: The table shows the estimated δ values based on a test for the salience of unobserved confounders following Oster (2019), which show the relative importance of omitted variables compared to those variables we condition our estimates on. The R_{max}, the hypothetical R-squared value of a fully specified model, is set to $1.5\hat{R}$ where $\hat{R}$ is the R-squared value obtained from the respective estimated model. These $\hat{R}$ are obtained from our preferred specification in Tables 2 (Panel A, B and C, Column (2)), and 3 (Columns (1)–(3)). LFP means labor force participation. Educ. means education. FT means full-time, PT part-time and marg. marginal.

Table 5
Mechanisms.

	Health satisf. (1)	Life satisf. (2)	External locus of control (3)	Feeling under time pressure (4)	Willing. to take risks (5)	Very worried about finances (6)	Intention to stay in GER (7)
Physical victim.	−0.2216**	−0.2039**	−0.3471	0.1427**	−0.2195	0.0204	−0.0113
	(0.1060)	(0.1000)	(0.3473)	(0.0564)	(0.1506)	(0.0210)	(0.0117)
Financial victim.	−0.3136***	−0.2272**	0.0969	0.0733	0.1694	0.0536***	−0.0032
	(0.1051)	(0.0971)	(0.3279)	(0.0555)	(0.1446)	(0.0202)	(0.0111)
Observations	2901	2901	1460	2981	2901	2790	2790
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of arrival FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
C.origin*Departure FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Huber-White standard errors; *p < .1; **p < .05; ***p < .01
Notes: The dependent variable in Columns (1) and (2) captures self-reported life satisfaction or health on a scale from 1 to 10, with 10 being the highest score. The dependent variable in Column (3) captures feeling time pressed in the past four weeks on a scale from 1 (never) to 5 (always); in Column (4), the willingness to take risks measured on a scale from 0 (risk averse) to 10 (risk taker); and in Column (5), the external locus of control which sums the values of five questions each taking values between 1 and 7. The dependent variable in Column (6) is binary and takes a value of 1 for individuals who report being "very concerned about their finances" at the time of the interview. The dependent variable in Column (7) is binary and takes a value of 1 for individuals who report that they "intend to stay in Germany permanently" at the time of the interview, and Column (7) is a continuous variable reflecting external locus of control. Columns (1) to (7) use observations that correspond to the date of the first interview conducted, which is 19 months after arrival on average. The term FE indicates fixed effects. Departure FE refers to the year-month of displacement from the home country and C.origin indicates country of origin fixed effects.

enter into marginal employment rather than investing in host-country-specific education or training. Hence, while mental health is affected by victimization, its effect on refugees' economic activity seems to be dominated by another channel.

External locus of control. As shown in Column (5) of Table 5, the impact of physical and financial victimization events on this outcome is not statistically different from zero.[39] Therefore, it is unlikely that this particular factor is the driving force behind our findings.

Altered time preferences. One possible mechanism driving the positive effect on labor force participation and employment and a negative effect on education and job quality is the "loss of future orientation", or impatience. As discussed in Section 3.1, the best proxy for impatience in our data is the self-assessed feeling of being under time pressure, which is a measure that the psychology literature has linked to forward-looking planning. Table 5 shows the regression results based on our main specifications.[40]

The results obtained from our preferred specification shown in Column (3) indicate that a physical victimization event increases the feeling of being under time pressure by 0.14 points on a 1 to 5 scale, which is an increase that corresponds to a 12% standard deviation of the measure ($p < .05$). The coefficients estimated on the financial victimization event are significantly smaller and are not statistically significant at conventional levels.

We interpret this finding as suggesting that experiencing physical victimization may result in a "lack of future orientation" or increased impatience, which in turn affects labor market behavior. This interpretation is grounded on the premise that individuals indirectly reveal their time preferences through their choice of activities (DellaVigna & Paserman, 2005).[41] We note that these findings lower potential remaining concerns about unobserved ability bias. It is challenging to think of an unobserved characteristic not accounted for in our analysis that would directly influence individual time perception, the likelihood of experiencing physical victimization, and decisions related to the labor market.

Altered risk preferences. The results obtained from our preferred specification in Column (4) of Table 5 indicate the hypothesized negative association between physical victimization events and the willingness to take risks, but the coefficient is imprecisely estimated. Hence, we do not find compelling evidence favoring altered risk preferences among the victimized vis-à-vis the non-victimized.

[39] The smaller sample size for the external locus of control stems from the fact that this question has only been included in surveys since 2016.

[40] We lose 311 observations of the main working sample because of item non-response. Our main results shown in Section 5.1 remain unchanged for the smaller sample.

[41] As discussed in Section 2, the interpretation of a negative association between higher time discounting and educational attainment also finds strong backing in the development economics and psychology literature (Adelabu, 2008; Cadena & Keys, 2015; Fersterer & Winter-Ebmer, 2003; Kemptner & Tolan, 2018; Stoddard et al., 2015; Sutter et al., 2013).

Financial difficulties. An alternative hypothesis to explain our main results is that the faster labor market integration of victimized individuals is caused by an attempt to quickly recover the relatively high journey cost upon arrival. Another possibility is that victimization reflects a lower ability to pay human smugglers or border patrols, which could lead to violent retaliatory acts by agents who demand payment.

We partly address this concern in our main specifications by including relative wealth in the country of origin before departure. Additionally, in our PDS specification, we include both the incomplete smuggler costs variable as a control and a dummy variable that equals 1 if the individual financed the flight to safety through credit or borrowing.[42] Our data allow us to test the "financial hardship" hypothesis in a second way; we approximate the level of financial precariousness of refugees in Germany by the extent to which the survey respondents report being very worried about their finances at the time of the first interview. The results are shown in Column (6) of Table 5. We do not detect an effect of physical victimization on financial hardship at any conventional statistical level. Financially victimized refugees are 5.4 percentage points more likely to voice concerns about their financial situation, providing reassurance to the accuracy of both measures.

Intention to remain in Germany. In Column (6) of Table 5, we analyze the differences in refugees' stated intention to stay in Germany permanently upon arrival. Using our preferred specification, Column (7) shows a small, statistically insignificant negative effect of physical victimization on the likelihood of wanting to stay in Germany.

In summary, while health outcomes show a negative association with victimization events of any kind, our results suggest that only more severe physical victimization events shift our proxy for time preferences. Our main results shown in Table 2 further suggest that this shortened sense of future orientation dominates the negative health effect on the labor market. In the following, we further corroborate this interpretation by testing alternative explanations to our main findings related to the German institutional setting and returns to education.

Asylum procedures. If asylum procedures take less (more) time for victimized individuals, then they will secure earlier (later) access to the labor market upon arrival. This would mechanically link victimization to faster (slower) labor market integration and potentially bias the results. In our setting, it is conceivable that victimized individuals have a more legitimate claim for protection, and their refugee status could therefore allow them to integrate into the labor market in larger numbers by design.[43] We note here that asylum is granted based on reasons related to human rights violations and persecution that individuals face in the country of origin (rather than on their flight to safety). However, asylum decisions are made based on the judgment of asylum officers, and the lower mental health of refugees victimized during the journey could make their asylum claims more convincing, which could lead to faster procedures.

To test this, we compare the outcome and length of asylum procedures between victimized and nonvictimized individuals using our preferred specification (Eq. (1)), excluding the asylum status controls. The results in Table 6 show that there is no statistically significant difference in the likelihood of being allowed to stay in Germany or in the length of the asylum procedure between victimized individuals and those who were not victimized.[44]

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

We test this possibility with IEB employment biography data by mapping the employment rates between victimized and nonvictimized refugees for (a) the time of arrival and the point in time when asylum was granted and (b) after asylum was granted. The exercise of a pre-trend and post-trend comparison allows us to see at what point employment rates start to diverge. Figure 5 in the appendix shows the results of this exercise, with the x-axis starting at the time of arrival and $t = 0$ indicating the month in which asylum was granted. We do not find any evidence that employment rates diverge before the end of the asylum procedure.

Returns to education. If refugees who experienced victimization are older than those who did not, then the negative effect of victimization on education could be driven by the fact that older people have lower returns to education. Our analyses show that this is unlikely to be the case. We start by noticing that 90% of refugees in our sample are less than 45 years old, and 67% are less than 35 years old (Table 11 in Appendix F). Second, the conditional balance test results listed in Appendix H show that the conditional age difference between the victimized and nonvictimized is precisely zero, and we control for age and age squared in all regressions. Nevertheless, Fig. 2 shows the results of our main economic outcomes of interest obtained when progressively restricting the analysis to younger cohorts. The results are relatively stable across age groups, suggesting that age differences between victimized and non-victimized refugees do not explain the negative effect of physical victimization events on the decision to invest in education in the destination country.

7. Conclusion

One of the key features of refugee flows from the developing to developed regions of the world has been the extreme conditions under which these movements occur. We show in this paper that the physical victimization events that individuals endured during their journeys affect their economic integration outcomes in the destination country. Three years after arrival, refugees who were physically victimized during their journey are 6.1 percentage points more likely to have joined the labor force by taking up low-income employment and are 3.1 percentage points less likely to pursue host country-specific education or training compared to the nonvictimized refugee population. We do not find a similar effect for financially victimized refugees, which suggests that in line with the previous victimization literature, physical victimization has stronger effects on life trajectories.

We conceptualize our findings as a "loss of future orientation", a concept closely related to "impatience" in the economics literature, where events of physical victimization lead to less forward-looking decision-making. In the migrant-specific human capital investment model framework, this can be interpreted as a distortion of the trade-off that refugees face upon arrival to either invest in education to gain

[42] Appendix P, Table 21 Column (6) shows the results for the main outcomes by using our preferred specification and by additionally controlling for variables "used a smuggler" and "financed escape by credit or borrowing". These variables do not change our coefficient of interest and are not statistically different from zero themselves. We note that although most undocumented migrants before the 2015 refugee crisis accumulated debt with smugglers to be able to finance their journeys to Europe, this is not the case for the 2014–2016 wave of asylum seekers, which constitute the majority of our sample.

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

[44] The length of the asylum procedure is missing for individuals with unknown status and for whom the outcome of the asylum procedure is not yet known. We have no information on either the start or ending date of the procedure for another 405 individuals.

Table 6
Asylum procedure.

	Allowed to stay in Germany				Length of asylum
	All	Excl. no decision yet	Excl. unknown	Excl (2)–(3)	procedure in months
	(1)	(2)	(3)	(4)	(5)
Physical victim.	−0.0172	−0.0058	−0.0155	−0.0016	0.3131
	(0.0145)	(0.0136)	(0.0114)	(0.0074)	(0.3162)
Financial victim.	−0.0103	−0.0112	−0.0039	−0.0022	−0.4897
	(0.0142)	(0.0131)	(0.0113)	(0.0070)	(0.3011)
Observations	3004	2722	2730	2459	2046
Individual controls	Yes	Yes	Yes	Yes	Yes
Year of arrival FE	Yes	Yes	Yes	Yes	Yes
C.origin*Departure FE	Yes	Yes	Yes	Yes	Yes

Huber-White standard errors; *p < .1; **p < .05; ***p < .01
Notes: The dependent variable in Columns (1)–(4) is binary and takes a value 1 for individuals who were granted protection status or were allowed to stay in Germany, and it equals 0 for individuals who were requested to leave Germany, have unknown status and who are still waiting to know the outcome of the asylum procedures. Column (2) excludes all individuals who do not yet know the outcome of the asylum procedure, Column (3) excludes those with unknown outcomes, and Column (4) excludes both. The dependent variable in Column (5) is the length of the asylum procedure in months. The term FE indicates fixed effects. The term departure refers to the year-month of forceful displacement from the home country. C. of origin is the country of origin.

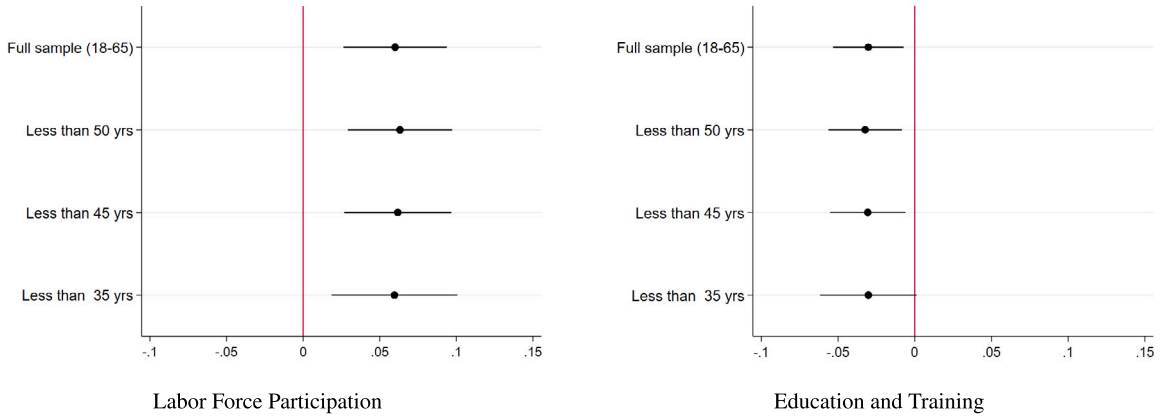

Fig. 2. Effect of physical victimization on LFP and education with age restrictions. Notes: The dependent variable is binary and takes a value of 1 for individuals in the labor force (left-hand side) or those pursuing host country education (right-hand side). LFP stands for labor force participation. Each line corresponds to a different regression, with the restriction as in the y-axis label. All regressions correspond to the benchmark specification and use observations that correspond to the last interview conducted, which is 31 months after arrival on average. The coefficients in each line correspond to the coefficient on physical victimization.

access to higher-quality employment at a later stage or to take up lower-quality employment shortly after arrival. Our findings, therefore, cast doubt on the notion of swift labor market integration as a general success metric for refugees. Although beneficial to determine the efficacy of supportive integration policies, we show that the aggregate speed of labor market integration also reflects unintended consequences of policies that serve entirely different purposes.

A more general lesson of this study relates to the contextual factors of victimization events. Past studies have documented that traumatic events experienced by adolescents and young adults may lower their human capital investment and lead to less future-oriented planning (Monahan et al., 2015; Ramos et al., 2013; Schmidt et al., 2018; Stoddard et al., 2015). Our findings imply that the negative consequences of physical victimization events may similarly affect individuals who are starting their economic trajectories from zero, regardless of their age.

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

CRediT authorship contribution statement

Teresa Freitas-Monteiro: Writing – original draft, Visualization, Validation, Supervision, Resources, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Lars Ludolph:** Writing – original draft, Visualization, Validation, Investigation, Data curation, Conceptualization.

Declaration of competing interest

The authors declare the following financial interests/personal relationships which may be considered as potential competing interests: Teresa Freitas-Monteiro reports financial support was provided by European Union's H2020 research and innovation programme under the Marie Sklodowska-Curie grant agreement No. 765355. This paper was written while the corresponding author was a PhD student at Humboldt University of Berlin and the Institute for Employment Research in Nuremberg If there are other authors, they declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.worlddev.2024.106833.

Data availability

The authors do not have permission to share data.

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