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PTSD and refugees' underemployment: Evidence from displaced Ukrainians*

Mette Foged†, Karen-Inge Karstoft‡, Edith Zink§

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Abstract

Employment gaps between refugees and natives are well documented, yet the role of trauma-related mental health in shaping these gaps remains underexplored, partly because most data sources lack measures of symptoms early after arrival. We assess probable PTSD shortly after displacement in an entire refugee arrival cohort and link these data to administrative tax records. We find that PTSD symptoms are associated with lower employment probabilities, explaining roughly one-quarter of the refugee-native employment gap one to two years after arrival. This difference is nearly twice as large as the difference attributable to English proficiency and comparable to the difference linked to pre-displacement employment. Among employed refugees, probable PTSD is associated with fewer hours worked per month, though not with lower hourly wages. Our findings underscore the potential of early psychological screening and support as complements to existing labor market integration policies.

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1 Introduction

A substantial literature documents lower employment rates of refugees compared to native populations and other immigrant groups (Dustmann et al., 2017; Bratsberg et al., 2017; Schultz-Nielsen, 2017; Sarvimäki, 2017; Brell et al., 2020; Fasani et al., 2022; Berbée and Stuhler, 2025). While labor market performance improves rapidly in the first couple of years after immigration, most studies find that a significant employment gap persists over time—and Denmark is no exception (Schultz-Nielsen, 2017). Some authors have suggested that a substantial part of this persistent gap may be attributable to poorer mental health among refugees stemming from exposure to traumatic events (e.g. Becker and Ferrara, 2019; Bratsberg et al., 2017; Brell et al., 2020; Fasani et al., 2022). But we lack empirical evidence linking refugees' exposure-related psychopathology to their subsequent economic integration trajectories.

A large epidemiological literature estimates the prevalence of trauma-related mental health problems in refugee populations, focusing most often on post-traumatic stress disorder (PTSD) as the relevant diagnosis. There is substantial variation in the estimated prevalence rates across studies; from 4.4% to 90% according to Johnson and Thompson (2008) or 9% to 36% according to Turrini et al. (2017). This variation comes from differences in methodology, different degrees of exposure to potentially traumatic events, and the specific samples used which are often small convenience samples (Fazel et al., 2005; Porter and Haslam, 2005; Bogic et al., 2015; Bryant et al., 2023). While the estimated prevalence of PTSD in refugee populations varies, it is clear that individuals who experienced combat, shelling, torture, and loss of close relatives due to war face an increased risk of developing psychiatric illnesses (Steel et al., 2009; Mesa-Vieira et al., 2022; Karstoft et al., 2024), which in turn likely diminishes their capacity for employment.

So far, only a few studies have examined the association between mental health and labor market outcomes of refugees (Lai et al., 2022; Disney and McPherson, 2020; Dietrich et al., 2023; Dang et al., 2023), most often focusing on employment as a mitigating factor for economic and psychological distress and not accounting for time since migration, which is a key factor in assessing the economic assimilation of migrants (Chiswick, 1978; Borjas, 1985; Lubotsky, 2007; Dustmann et al., 2017; Bratsberg et al., 2017; Schultz-Nielsen, 2017; Sarvimäki, 2017; Brell et al., 2020; Fasani et al., 2022). Establishing a causal link from war-related psychopathology to labor market outcomes is challenging due to the bidirectional relationship between employment and mental health, as well as the complex interplay of pre- and post-migration experiences and individual resilience in shaping psychiatric symptoms. Moreover, available data sources generally do not provide representative information on refugees' mental health assessed shortly after arrival.

We advance on current knowledge by assessing probable PTSD of an entire refugee arrival cohort early after arrival from war, relating it to subsequent labor market trajectories, and carefully examining the role of potential confounders in the estimated effect of probable PTSD on labor market outcomes. This allows

us to establish a strong, plausibly causal link between early signs of PTSD and labor market integration.[1] Specifically, we combine survey data on all adults who arrived in Denmark from Ukraine within the first seven months of the full-scale invasion (Wave I of the Danish Refugee Cohort (DARECO), response rate=45%, N=4,533) with monthly tax records. Hence, our data on PTSD is elicited shortly after arrival and representative of a whole arrival cohort which reduces potential biases from contagion of symptoms with post-migration factors and from non-random sample selection.[2] Additionally, we obtain a complete, register-based follow-up by linking the survey data to administrative register data. Our main outcome is an indicator variable for being in employment, the extensive margin of employment. We complement this with the employment as a fraction of fulltime, hours worked, hourly wages and total earnings. Through our survey, we obtain rich information on refugees not available in typical data sources and we show that predictors of labor market integration such as labor force participation before displacement and networks are uncorrelated with PTSD when conditioning on demographic and educational controls.[3] Additionally, we assess the sensitivity of our estimates to omitted variable bias by bounding the potential role of unobservables using Oster (2019) and Cinelli and Hazlett (2019). These checks show that unobserved confounders would have to explain extreme shares of the remaining variance in treatment and outcome—accounting for all register- and survey-controls—for our estimate to become insignificant. We consider the existence of such confounders unrealistic given the rich set of demographics, human capital variables, and other pre-migration factors we account for in our model.

The prevalence of probable PTSD assessed in early 2023 among Ukrainian refugees arriving in Denmark in 2022 is 29% (see also Karstoft et al., 2024). We follow refugees two years from arrival and find that probable PTSD is associated with a significant reduction in the subsequent employment probability. Probable PTSD reduces the employment rate by 7.4 percentage points (pp) and explains one quarter of the refugee-native employment gap—after an initial phase of rapid economic assimilation—likely more in refugee populations with higher PTSD prevalence. The robustness and stability of our results from one to two years after arrival underscore a central role of trauma in explaining refugees labor market outcomes in the longer run. The estimated PTSD gap is insensitive to inclusion of key predictors of labor market performance after controlling for demographic and educational information. We estimate that it is of similar magnitude as the positive gap from having formal employment before fleeing and almost twice as large as the employment gain from fluency in English. Among the employed, probable PTSD symptoms leads to fewer hours worked (–6.3%, 45% of refugee-native gap) but is unrelated to hourly wages.

Recent research shows that conflict intensity in origin countries influences prescription drug usage among

[1] Obtaining causal evidence on the effect of mental health on employment is generally challenging, also outside refugee populations; see the discussion in Section 3 of Prudhon (2025), who exploits delayed treatment for mental health conditions in an event-study design.

[2] Mental health measures based on administrative data—such as diagnoses or redeemed prescriptions—are prone to these biases, as newly arrived refugees typically access the healthcare system with substantial delay due to limited information and insufficient familiarity with how to navigate the destination country's healthcare services (Nørredam et al., 2005; World Health Organization, 2010).

[3] English proficiency is slightly unbalanced before conditioning on education but magnitudes are small and insignificant after controlling for education.

foreign-born populations (Sønderskov et al., 2021; Karadja et al., 2024), highlighting the role of indirect forms of exposure. This is particularly relevant in an era of constant connectivity through internet and social media (Yarkin, 2025). We, therefore, consider all individuals in our population to be at least indirectly exposed to the war in Ukraine. We observe variation in symptom severity, and show that employment gaps are larger for more severe symptoms. Direct and indirect war exposures vary in type and intensity.Numerous factors including the intensity and duration of exposure to war contribute to the risk of developing severe symptoms (Ben-Ezra et al., 2010; Karstoft et al., 2024). It is also clear that most individuals exposed to war do not actually develop PTSD (Hoppen and and, 2019; Steel et al., 2009), and many still work despite their symptoms. This paper provides the first evidence that trauma-related symptom severity at arrival is linked to subsequent employment probabilities in a cohort of war-refugees.

From the perspective of the destination country, the relevant margin with scope of action is to identify and help refugees who have severe PTSD symptoms following war exposure. Currently, such support is generally limited to small-scale efforts by NGOs and volunteers (Sijbrandij et al., 2017; Nosè et al., 2017; Marquez et al., 2018; UNHCR, 2024), while state policies address gaps in human capital, information, and incentives through language training, active labor market programs, and welfare generosity (Arendt et al., 2022; Foged et al., 2024; Fouka, 2024). Such policies are likely ineffective for refugees with severe functional impairments due to trauma. Embedding knowledge of PTSD and support for symptom management into existing integration programs (Rigsrevision, 2018; Marquez et al., 2018) and improving access to healthcare services (Nørredam et al., 2005; World Health Organization, 2010) seem promising directions for addressing the persistent underemployment among refugees early on.

2 Data

2.1 Data collection

Following Russia's full-scale invasion of Ukraine on February 24, 2022, all Ukrainian refugees were granted immediate and full access to Denmark and the Danish labor market. Upon arrival, they received a civil registration number. Anyone with that number can be traced across Denmark's administrative registers and be contacted through an electronic mailbox usually used for communication with authorities. Through this electronic mailbox, we invited all Ukrainian citizens who immigrated to Denmark between February 24 and September 30, 2022 and were 18 to 64 years old at arrival, if they were still in Denmark by February 17, 2023. The data collection was launched on the 23rd of February 2023 (one year after the full-scale invasion) and closed on April 16th 2023. We issued two electronic and one postal reminder. The overall response rates was 44.9%, with 38.0% providing complete responses.[4] The average time in Denmark when responding to our survey is 9 months (mode 10 months).

[4]This is a high response rate. Uptake may have been facilitated by familiarity with the electronic mailbox due to its use for welfare payments, and by communication about the study by NGOs, mainly Danish Red Cross and the Danish Refugee Council.

2.2 Population and weights

Our main analysis sample consists of 4,533 Ukrainian refugees who completed the survey and remained in Denmark for at least two years, as well as a 5% random sample (N= 136, 746) of the native population aged 18–64.[5]

Survey respondents are overall representative of the targeted population of Ukrainian refugees. We find a few minor differences and apply entropy weighting to balance up to third moment of the characteristics at arrival between respondents and non-respondents (Hainmueller, 2012). Respondents are on average 0.5 year younger, 6 percentage points (pp) more often female and 7 pp more likely to have children in Denmark. Differences with respect to arrival times and region of first settlement are mostly insignificant. We observe a couple of significant differences across arrival month but unrelated to time since full-scale invasion, and higher response rates in Mid-Jutland. With weights, all differences vanish. The weights are applied throughout, but do not affect any of our findings since initial imbalances are minor. Supplementary Material A describes all data sources and variables, and Supplementary Material B.1 provides details about the weighting.

2.3 Post-traumatic stress disorder

We assess PTSD and the more severe condition Complex PTSD (CPTSD) with the International Trauma Questionnaire (ITQ, Cloitre et al., 2018), following the International Classification of Diseases (ICD-11). The ITQ is a widely used assessment tool for PTSD and has been validated in many different populations (e.g. Seiler et al., 2023). While the ITQ mirrors the ICD-11 diagnostic criteria for PTSD and CPTSD, it is based on self-report and derived diagnoses should be seen as indicators of probable PTSD/CPTSD, *not* formal diagnoses. Despite this limitation, a clear advantage of self-report measures is that they can be used in large-scale studies early after arrival, covering all refugees. In contrast, clinical diagnoses or prescription data are limited to individuals who eventually seek care. By collecting self-reported data shortly after arrival, we capture initial signs of trauma-related distress across the entire population of interest. This is essential for understanding the broader mental health landscape soon after arrival from war and for limiting the confounding influence of post-migration experiences.

The ITQ assesses symptoms of PTSD in six items and CPTSD using six additional items. An individual is categorized with probable PTSD with the presence of symptoms in all three PTSD-domains (re-experiencing, avoidance and current sense of threat) and associated functional disturbance (assessed in three items). Individuals with PTSD who also endorse at least one symptom in each Disturbances in Self-Organisation (DSO) domain (affective dysregulation, negative self-concept, and disturbed relationships) and associated functional disturbance are categorized with probable CPTSD.

[5]The native population is defined as born in Denmark or children who has at least one parent who is both a Danish citizen and born in Denmark.

2.4 Outcome variables

We define employment as at least one hour of paid work in the month of observation. As secondary outcomes, we use employment as a fraction of fulltime (37 hours per week), hours worked, earnings, and hourly wages, all derived from monthly tax records available up until September, 2024. This allow us to track labor market outcomes for up to two years after arrival and more than one year post-survey.

2.5 Other variables

We obtain information on the age, gender, household composition and region of residence from Danish registers measured in month of arrival for refugees and in month of observation for natives. The highest completed education is aggregated from detailed information on education obtained in Denmark to match education information on refugees available from the survey. These are our baseline controls, summarized in Table 1.

To probe the sensitivity of the PTSD gap to potentially omitted confounders, we add refugee-specific controls listed in Figure 1. We define a series of variables based on the survey. These are indicators for partner and children above and below 18 left behind in Ukraine, network in Denmark, prior immigration spell(s), admission under the Special Law for People Displaced from Ukraine (versus other permits), oblast of residence in Ukraine before fleeing, main activity in Ukraine, English proficiency and questionnaire in Russian (versus Ukrainian).

3 Empirical approach

Our empirical strategy allows for flexible outcome trajectories by months since migration to fully capture the changing speed of economic assimilation in the first couple of years after arrival. The specifications we estimate are the following:

$$y_{it} = \alpha + \theta \,\text{refugee}_i + X_{it}\mu + \varepsilon_{it} \tag{1}$$

$$y_{it} = \alpha^r + \beta^r \,\text{PTSD}_i + X_{it}\mu^r (+Z^r_{it}\rho^r) + \varepsilon^r_{it} \qquad \text{if refugee}_i = 1 \tag{2}$$

where y_{it} is the labor market outcome of individual i in month t (defined in Section 2.4). The coefficients of interest are θ and β^r capturing the refugee-native gap and the PTSD gap. X_{it} contains gender, a third order polynomial in age, household composition, region of residence and education level (see Table 1).[6] Observation time fixed effects are important in studies pooling several years of heterogeneous arrival cohorts (Borjas, 1985) and changing business cycles (Bratsberg et al., 2005, 2006) but have negligible impact in our setting because all refugees arrive within seven months (March-September 2022), and we exclude time effects in equation (1) to obtain an average refugee-native gap across arrival months.[7] This

[6] Age and education level are typical Mincerian human capital controls included in most papers using e.g. U.S. census data. Education of newly arrived refugees is usually not available in administrative registers but we obtain this information from our survey.

[7] Appendix Section A show natives' labor market outcomes are essentially flat over the considered time period.

is our baseline model to compute the conditional refugee-native gaps by month since arrival. We use these to assess how much of it can be attributed to probable PTSD[8]:

$$\text{share explained}_t = \frac{\text{prevalence} \times \hat{\beta}^r}{\theta} \tag{3}$$

To reduce concerns of bias in the estimated PTSD-gap, $\hat{\beta}^r$, we add refugee-specific controls, Z^r_{it}, in equation (2). These variables are listed in Figure 1 and described in Section 2.5 as well as a third order polynomial of month of arrival which captures refugees' exposure to the war, and a third order polynomial of months in Denmark until surveyed to account for symptom development over time in safety.

Our focus is on differences in outcomes that arise due to psychological consequences of potentially traumatic experiences prior to arrival in the destination country. We believe this is informative about a more general and fundamental component of refugees' underemployment across destination countries compared to later symptoms in the destination country, subsequent diagnosis or use of prescription drugs since these are influenced by a diverse set of challenges in the destination country, navigation of the health care system, and self-selection into seeking treatment. We avoid those biases by relying on early symptoms assessed in a survey that we designed and sent to all Ukrainians upon arrival in Denmark after the onset of the war in 2022.

4 Results

4.1 Main results

Figure 2(a) shows the evolution of the unconditional monthly employment rates among Ukrainian refugees over the first 24 months after arrival, separated by probable PTSD status and relative to native-born. More than 10% of refugees have at least one hour of paid employment in month 0. Economic assimilation is steep in the following 4-6 months and a clear gap emerges between the groups with and without probable PTSD. Figure 2(b) shows the raw PTSD-related gap in monthly employment rates and then probes its sensitivity to potential confounders. We first include all the baseline characteristics, which are the typical Mincerian demographic and education level controls. Adding these controls reduces the estimated PTSD gap. However, once they are included our comprehensive set of refugee-specific variables has no detectable impact on the estimated PTSD gap, consistent with the balance across probable PTSD shown in Figure 1. In the Supplementary Material C.1, we show—for each of our outcomes—that the R^2s increase with the inclusion of each of the groups of controls indicating that they all contain relevant information about the labor market outcomes even though the coefficient on probable PTSD is not much affected by the inclusion of the refugee-specific information. This supports that the differences in labor market outcomes across probable PTSD are not driven by unobservables for refugees relevant to labor market outcomes.

[8] θ is very precisely estimated (Table 2) and we treat it as a constant in equation (3) to be able to obtain analytical standard errors.

Table 2 displays our main results pertaining to month 24 after arrival. Probable PTSD is associated with 7.4 pp (SE 0.015) lower employment probability at the extensive margin, and a 8.6 (SE 0.015) lower employment rate measured in fulltime equivalents. Conditional on some employment, we estimate a PTSD earnings gap of 7.1% (SE 0.026), driven entirely by fewer hours worked (-6.3%, SE 0.024). The estimated effect on hourly wages is economically and statistically indistinguishable from zero (-0.008, SE 0.011). The conditional employment gap relative to natives is 7.8 pp (SE 0.004) after 24 months in Denmark. Taking into account that the prevalence of probable PTSD is 29.1% in our population, this implies that we can explain 27.7% (SE 0.055) of the underemployment of refugees relative to natives two years after arrival from war by accounting for trauma-related psychopathology after arrival from war. Measuring employment in fulltime equivalents, we find that PTSD can account for 22.1% (SE 0.039) of the observed refugee-native employment gap. Employment assimilation is steep in the first year and then plateaus (Figure 2(a)). Interestingly, PTSD accounts for an increasing share of the remaining non-employment as employment increase and then explains roughly one quarter of the refugee-native employment gap from one to two years after arrival (Figure 2(c)), indicating that PTSD symptoms are important for understanding the more persistent parts of the underemployment of refugees.

The refugee-native gap in hourly wage is large (29,7%), but seems entirely driven by other factors than probable PTSD while about 45% of the refugee-native gap in hours worked two years after arrival appear to attributable to probable PTSD. Hence, symptoms of PTSD are linked to intensive and extensive margin of employment and not related to hourly pay.

4.2 Sensitivity

Figure 2(b) shows that the estimated PTSD employment gap is highly stable across specifications, including models that add an extensive set of refugee-specific controls capturing pre-migration labor supply, networks, language skills, household composition, and region of origin. Hence, while the refugee controls increase the explanatory power of the model (Supplementary Material C.1), they have negligible impact on the PTSD coefficient, indicating that refugee-specific predictors of labor market integration are largely orthogonal to PTSD status conditional on baseline demographics and educational information.

To further assess the sensitivity of our estimates to omitted-variable bias we follow two approaches and summarize the insights in Panel C of Table 2. First, we evaluate the potential importance of omitted variables comparing changes in coefficients and R^2 when new controls are added following Oster (2019). Oster (2019)'s maximal relative selection on unobservables for employment at month 24 tells that selection on unobserved factors would need to be nearly fourteen times stronger than selection on the full set of observed controls-—including detailed demographics, education, and refugee-specific characteristics—to reduce the estimated effect in Column 1 to zero. Given the breadth of included covariates, this seems highly implausible. Under the conservative assumption that unobservables are as important as all included controls, the estimated employment gap shows a modest decline from 7.4 to 7.1 pp (SE 0.015).

Second, we benchmark selection on unobservables against our most important observed covariate in terms of the partial R^2 with both the treatment and the outcome following Cinelli and Hazlett (2019). The adjusted coefficient using this approach corresponds to a hypothetical scenario in which an unobserved confounder, orthogonal to all included controls (baseline and refugee specific), is as strongly associated with both PTSD and employment as our most influential observed covariate (gender). Even under this extreme assumption, the estimated PTSD employment gap remains sizable and highly significant at 6.4 pp (SE 0.015). Moreover, such an omitted variable orthogonal to the included covariates would need to explain at least 7.3% of the residual variance in both PTSD and employment to fully eliminate the estimated gap. Given the richness of pre-migration, demographic, and socioeconomic controls, the existence of such an orthogonal confounder appears unrealistic. Supplementary Material B.2 provides more detailed discussion of the methodologies of Oster (2019) and Cinelli and Hazlett (2019) and Supplementary Material C presents additional analysis of potential omitted variable bias under varying assumptions.

Finally, we check that outmigration is not biasing our estimates. The majority of Ukrainians wish to stay even after the war is no longer a threat to their home town (Foged et al., 2025), and indeed only a small share of the Ukrainians in our sample staying at least one year have outmigrated so that we obtain similar estimates using all individuals and only those who remain in Denmark for the entire two-year period (Supplementary Material C.4).

4.3 Symptom severity and benchmarking magnitudes

Figure 3 shows that more severe symptoms are associated with larger employment gaps. We first plot our main result using a binary indicator for probable PTSD (including CPTSD), followed by a specification distinguishing PTSD from CPTSD. CPTSD—a more severe and likely more chronic condition than PTSD—is associated with a larger employment gap relative to "no diagnosis" (PTSD: -0.058, 95% CI [-0.095,-0.022]; CPTSD: -0.093, 95% CI [-0.132,-0.054]). Quartiles of the PTSD and the DSO scores provide a similar pattern: employment probabilities decline monotonically with symptom severity. The reference category is the bottom quartile of each score. Individuals in the top quartile are 9-10 pp less likely to be employed than those in the bottom quartile (PTSD score top quartile: -0.087, 95% CI [-0.125,-0.048]; DSO score top quartile: -0.105, 95% CI [-0.143,-0.068]). As an additional robustness check, we exclude from the ITQ the item asking whether symptoms affected work or the ability to work in the past month, addressing concerns about justification bias and reverse causality.[9] Removing this item from the diagnostic tool does not affect our conclusions.

To contextualize the magnitude of the estimated employment differences associated with probable PTSD at arrival, we conducted a parallel analysis using self-reported English proficiency upon arrival and main activity in Ukraine. Individuals who reported conversational or fluent English skills have a 4 pp (95%

[9]Note that our survey did not address labor market outcomes (the invitation letter, survey questions and response options can be found in the Supplementary Material A.1). Labor market outcomes are taken from the administrative registers.

CI [0.009,0.071]) employment advantage compared to those with no or only elementary English proficiency 24 months after arrival. Employment gaps among individuals with severe symptoms, as indicated by CPTSD or by the top quartiles of PTSD or DSO scores, are roughly double the size compared to proficiency in English. The positive effect of employment before fleeing (compared to non-employment excluding students) is of similar magnitude to the negative effect of CPTSD on destination country employment (0.091, 95% CI [0.052,0.131]).

5 Discussion and conclusion

In a nationally representative survey of an entire arrival cohort of war refugees, we find that individuals exhibiting early symptoms of probable PTSD have a 7.4 pp lower probability of employment two years after arrival. Following the economic assimilation literature, we model labor market outcomes as a function of time since arrival and document pronounced employment differences associated with probable PTSD. These differences are larger for individuals with greater symptom intensity, as captured by distinctions between PTSD and CPTSD and by quartiles of PTSD and DSO scores. Notably, the recently introduced ICD-11 diagnosis of CPTSD is associated with a larger employment gap than PTSD, aligning with theoretical expectations regarding greater functional impairment in CPTSD, though empirical evidence supporting this has so far been limited (Folke et al., 2019).

Our conclusions are robust to attrition and unlikely to be driven by unobserved confounders affecting labor market outcomes. More knowledge is needed to understand the extent to which our estimates generalize to other refugees populations, destination countries or time periods. In Denmark, the employment level of Ukrainian refugees is similar to other refugees arriving in the same year, the main difference being lower employment levels of non-Ukrainian refugee women (Zink et al., 2024). But a major obstacle remains the lack of representative data on the prevalence of PTSD upon arrival. Existing estimates generally come from small convenience samples and are not measured early after displacement / trauma exposure (Johnson and Thompson, 2008; Bogic et al., 2015; Porter and Haslam, 2005; Fazel et al., 2005; Turrini et al., 2017; Mellor et al., 2021; Bryant et al., 2023).

Our findings underscore the need for further research on the link between refugees' mental health and their labor market integration. They also point to the importance of developing new interventions and policy tools that go beyond traditional measures aimed at improving economic integration (Fouka, 2024; Foged et al., 2024; Arendt et al., 2022). In particular, mental health interventions that alleviate the psychiatric symptoms typical to war-refugees (Nosè et al., 2017; Turrini et al., 2021; Bruhn et al., 2022; World Health Organization, 2010; UNHCR, 2024) could be effective for refugees whose main barrier for employment are severe symptoms of PTSD.

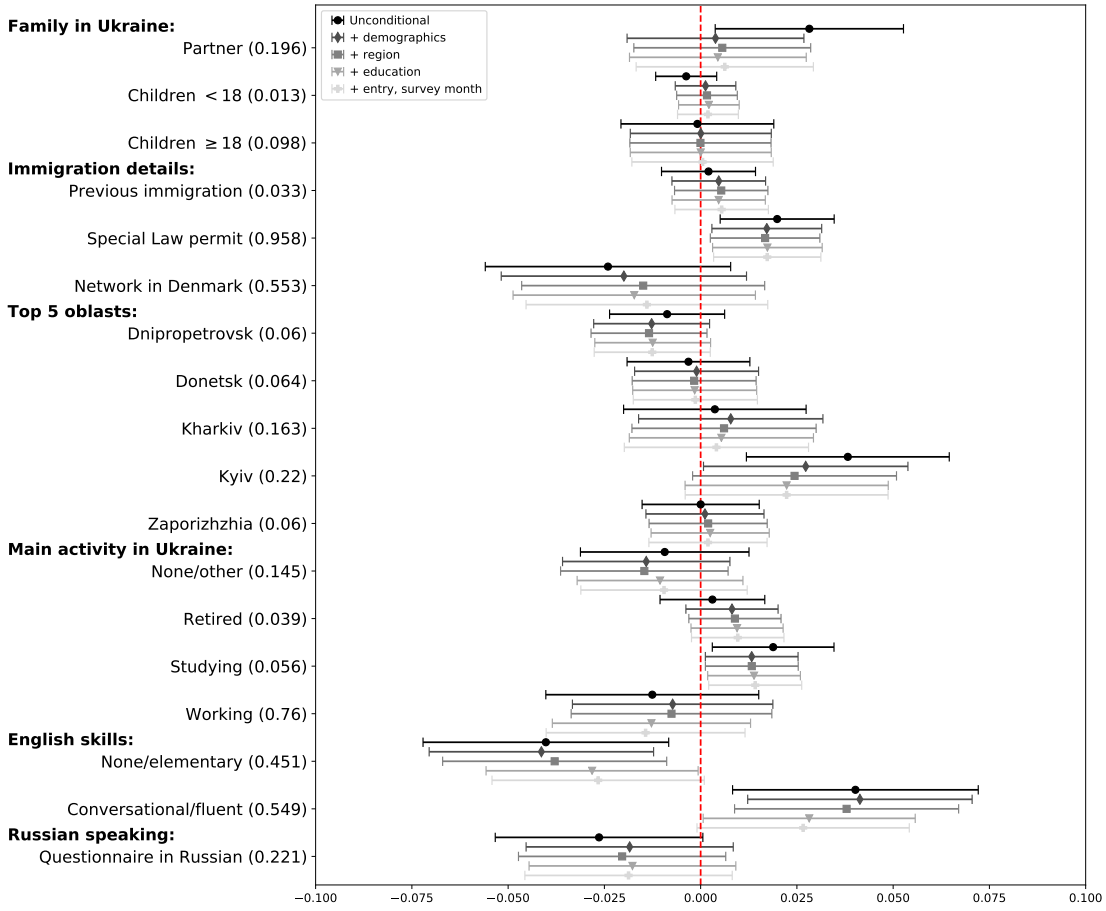
6 Figures and Tables

Table 1: Covariates for refugees and natives

	Natives			Refugees		
	Count	Mean	SD	Count	Mean	SD
Age		41.637	(13.238)		3.391	(0.976)
Female	67708	0.495	(0.500)	3698	0.758	(0.428)
Family composition						
No children	72499	0.53	(0.499)	2030	0.519	(0.500)
One child	25551	0.187	(0.390)	1434	0.273	(0.445)
Two children	28907	0.211	(0.408)	817	0.157	(0.364)
Three or more children	9789	0.072	(0.258)	252	0.052	(0.222)
Partner	94151	0.689	(0.463)	1356	0.305	(0.460)
Region						
Northern Juttland	14386	0.105	(0.307)	491	0.105	(0.307)
Mid-Jutland	23384	0.171	(0.377)	1242	0.246	(0.431)
Southern Denmark	27213	0.199	(0.399)	989	0.218	(0.413)
Capital region	40352	0.295	(0.456)	1259	0.293	(0.455)
Sealand	19718	0.144	(0.351)	552	0.138	(0.345)
Education level						
Secondary school or lower	38519	0.282	(0.450)	717	0.167	(0.373)
Vocational	64152	0.469	(0.499)	667	0.151	(0.358)
Bachelor/Junior Specialist	17512	0.128	(0.334)	1093	0.241	(0.428)
Master/Specialist or higher	16563	0.121	(0.326)	2056	0.441	(0.496)
Total	136746			4533		

Note: All variables are indicators, except age.

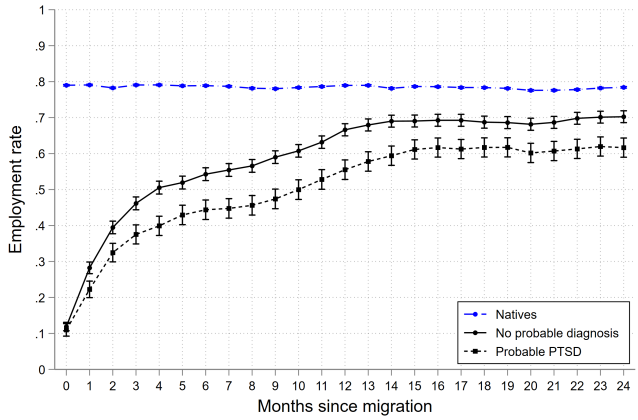
Figure 1: Conditional balance of covariates across probable PTSD



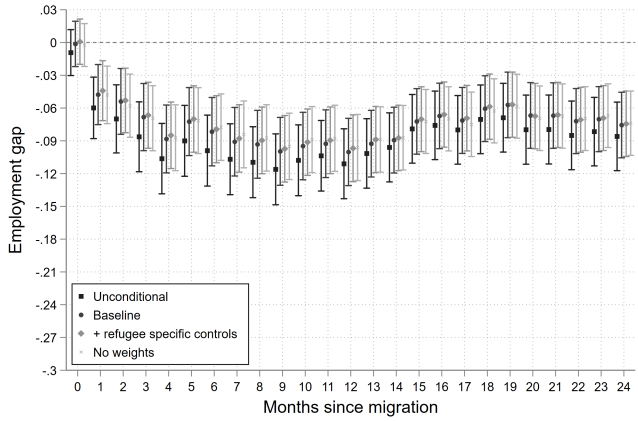
Note: Sample averages in parentheses, since all variables are indicators these correspond to the share of observation in each category.

Figure 2: Employment by months since arrival and probable PTSD

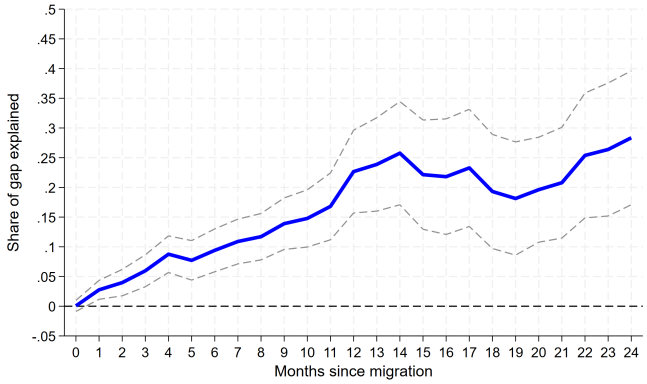
(a) Employment trajectories



(b) PTSD employment gaps



(c) Share of refugee-native gap explained



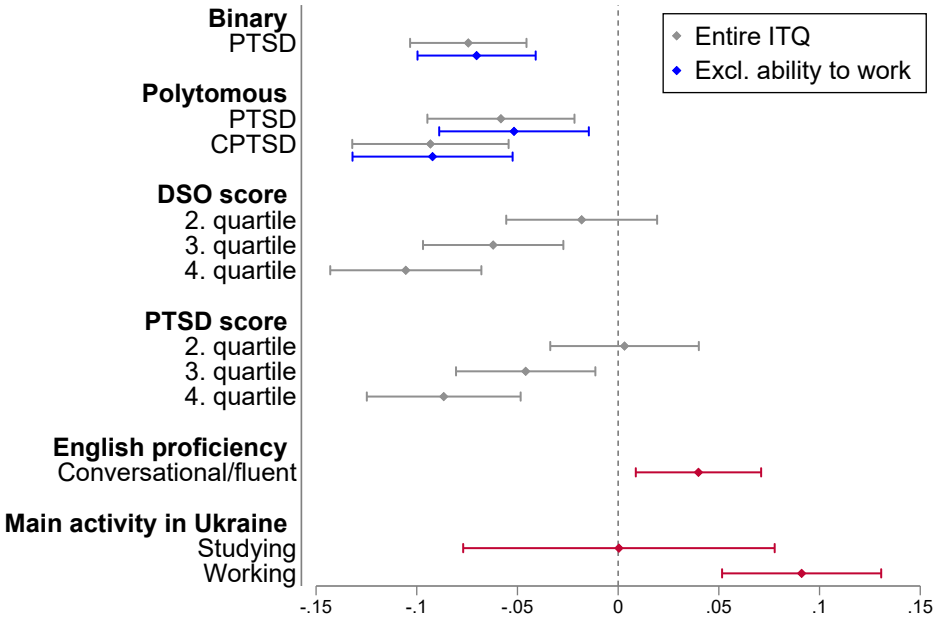
Note: Panel a plots the employment rates with 95-% confidence intervals by months since arrival and probable PTSD for refugees, and by months since May 2022 (modal arrival month of refugees) for native born. Panel b plots the estimated differences in employment rates with 95-% confidence intervals, sequentially adding controls. First, we plot the raw differences corresponding to the difference between the probable PTSD group and no probable diagnosis in Panel a. Second, we estimate our baseline. Third, we estimate our full model, Equation (2). Finally, we estimate the full model without weights. Panel c plots the share of the refugee-native gap explained by PTSD (solid blue line) and 95% confidence intervals (dashed gray lines) using equation 3

Table 2: Labor market integration and probable PTSD in month 24 after migration

	Unconditional		Conditional on employment		
	Employed	Share of fulltime	log(Earnings)	log(Hours)	log(Hourly wage)
Panel A: Refugee-native gap					
Refugee-native gap	0.078***	0.113***	0.334***	0.038***	0.297***
	(0.004)	(0.004)	(0.008)	(0.007)	(0.004)
Panel B: PTSD gap (refugees only)					
PTSD gap	-0.074***	-0.086***	-0.071**	-0.063**	-0.008
	(0.015)	(0.015)	(0.026)	(0.024)	(0.011)
	[-0.103,-0.046]	[-0.116,-0.056]	[-0.122,-0.021]	[-0.110,-0.016]	[-0.030,0.014]
Share of gap explained	0.277***	0.221***	0.056**	0.445**	0.007
	(0.055)	(0.039)	(0.020)	(0.168)	(0.010)
Panel C: Sensitivity					
Oster (2019)					
Adjusted coefficient	-0.071***	-0.077***	-0.059*	-0.055*	-0.004
	(0.015)	(0.016)	(0.027)	(0.023)	(0.011)
Max. relative selection	13.917	8.034	5.355	6.717	2.021
Cinelli and Hazlett (2019)					
Adjusted coefficient	-0.064***	-0.066***	-0.038	-0.040	0.002
	(0.015)	(0.015)	(0.026)	(0.024)	(0.011)
Robustness value	7.267%	8.073%	4.878%	4.688%	1.321%
PTSD prevalence	0.291	0.291	0.265	0.265	0.265
R^2	0.120	0.150	0.119	0.072	0.110
R^2 adj.	0.108	0.139	0.102	0.055	0.093
N natives	136746	136746	113376	113376	113376
N refugees	4533	4533	3102	3102	3102

Note: $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Estimates based on the full model, standard errors in parentheses, and 95% confidence intervals in squared brackets. Since the outcomes conditional on employment in columns (2)-(4) are in logs, the coefficient for monthly earnings is equal to the sum of the coefficients for monthly hours and hourly wage. The adjusted coefficient following Oster (2019) assumes proportional selection on observables and unobservables ($\delta = 1$) and a maximal R^2 1.3 times larger than the R^2 from the full model ($R^2_{max} = 1.3 \times R^2$). The adjusted coefficient following Cinelli and Hazlett (2019) assumes the existence of an orthogonal confounder as important as our most important covariate, gender. See Supplementary Material B.2 for more details on Oster (2019) and Cinelli and Hazlett (2019), and C.2 for additional sensitivity checks under alternative assumptions.

Figure 3: Heterogeneous effects



Note: Estimates and 95% confidence intervals

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Supplementary Material for

PTSD and refugees' underemployment

Evidence from displaced Ukrainians

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A Materials

Our analysis is based on a combination of register data and survey data. The Survey Division at Statistics Denmark, DST Survey, was responsible for running our survey and providing us with a pseudonymized version of the data that we could link to relevant information from Danish administrative registers through a unique individual identifier. In the following subsections, we provide details on the survey data we collected (section A.1), the register data (section A.2), and how we selected the analysis sample (section A.3).

A.1 Danish Refugee Cohort (DARECO), Wave I

The Danish Refugee Cohort (DARECO) is a survey panel on Ukrainian refugees in Denmark. For the present analysis, we use the first round of data collection that is the earliest after arrival (Wave I).
DST Survey drew the following population on February 17, 2023 for DARECO Wave I:

- Individuals who immigrated to Denmark on or after February 24, 2022 (date of Russian full-scale invasion in Ukraine),
- have not emigration by February 17, 2023, are Ukrainian citizens on February 17, 2023,
- and at least 18 years old as of February 1, 2023.

In total 18,949 individuals satisfied all criteria and 18,389 could be contacted.

The survey was sent to the full population ($n = 18,389$) and 44.89% participated and 38.03% responded to all questions.

A.1.1 Invitation Letter

The invitation read:

Will you participate in our study "Ukrainians in Denmark"?
Dear «NAME» *Statistics Denmark performs a study on behalf of the Department of Psychology, University of Copenhagen, to shed light on the situation and well-being of Ukrainians arriving in Denmark since February 24th 2022. In the questionnaire, we will ask you about your background and experiences in Ukraine and Denmark, reactions and feelings in relation to events and your health. You can hear more about the study here: LINK TO VIDEO ABOUT THE STUDY*

Your response should only take 20-30 minutes. Some questions might be difficult to answer or feel very personal. Your responses are highly valued and we hope you will take the time to respond. Among those who fill out the questionnaire, we will draw 8 persons who will receive a money prize on 5000, 2500 or 1000 Danish Kroner.

We treat your responses confidentially. The results are used only for statistical purposes and in analyses so that you as an individual cannot be recognized. Participation is voluntary, but the study will improve with more participants and you cannot be replaced by someone else. If you do not wish to participate, you can click here: (refusal link). If you have questions for Statistics Denmark you can contact us via email or phone (weekdays from 09-16).

If you are worried about your mental health, or if some of the questions in this survey causes any distress that you would like to talk to a professional about, you can call the Danish Refugee Council hotline on 33 73 53 14 in hours 9-10 or 17-18 Monday to Friday.

A.1.2 Questionnaire (survey variable list)

Table A1 list the variables used from DARECO Wave I, the question texts and answer options.

Table A1: Survey variables

Variable	Question text	Answer options
Intro text: The following questions deal with your previous life in your home country and your current situation in Denmark.		
Oblast	Where did you live in Ukraine before February the 24th 2022?	(1) Autonomous Republic of Crimea; (2) Vinnytsia Oblast; (3) Volyn Oblast; (4) Dnipropetrovsk Oblast; (5) Donetsk Oblast; (6) Zhytomyr Oblast; (7) Zakarpattia Oblast; (8) Zaporizhzhia Oblast; (9) Ivano-Frankivsk Oblast; (10) Kirovohrad Oblast; (11) Kyiv Oblast; (12) Luhansk Oblast; (13) Lviv Oblast; (14) Mykolaiv Oblast; (15) Odesa Oblast; (16) Poltava Oblast; (17) Rivne Oblast; (18) Sumy Oblast; (19) Ternopil Oblast; (20) Kharkiv Oblast; (21) Kherson Oblast; (22) Khmelnytskyi Oblast; (23) Cherkasy Oblast; (24) Chernihiv Oblast; (25) Chernivtsi Oblast; (26) Other
War in hometown	Has your home town in Ukraine been affected by actions of war and/or under Russian invasion any time since February the 24th 2022?	(1) No; (2) No, but there has been actions of war close to my home town; (3) Yes, but my home was not damaged; (4) Yes, and my home has been damaged.
... present	Were you present in your home town when this happened?	(1) No actions of war; (2) No; (3) Yes
War before 2022	Have you lived in a place affected by war before the Russian invasion in February 2022?	(1) No; (2) Yes
Loss of family/relatives	Have you lost any family members/close friends as a result of the war?	(1) No; (2) Yes
Exposure to combat	Combat exposure to war-zone (in the military or as a civilian)	(1) No; (2) Yes
... direct	Happened to me personally	(1) No; (2) Yes
Network in Denmark before arrival	Did you know any Ukrainians/have any acquaintances living in Denmark before your arrival?	(1) No; (2) Yes
Family in Denmark at arrival	Do you have near family that are also in Denmark because of the war?	
	– Yes, my spouse/partner	(1) Yes; (2) No
	– Yes, my minor children (0-17 year olds)	(1) Yes; (2) No
	– Yes, my grown-up children (18+ year old)	(1) Yes; (2) No
Family still in Ukraine at arrival	Do you have near family that still live in Ukraine?	
	– Yes, my spouse/partner	(1) Yes; (2) No
	– Yes, my minor children (0-17 year olds)	(1) Yes; (2) No
	– Yes, my grown-up children (18+ year old)	(1) Yes; (2) No
Education level	What is your highest level of education?	(1) No education; (2) Primary education (have a middle school diploma); (3) Secondary education (have a secondary school diploma); (4) Vocational education (have a vocational school diploma); (5) Short-term higher education (have a junior specialist diploma); (6) Medium-term higher education (have a bachelor's degree); (7) Long-term higher education (have a master's degree, specialist degree or higher)
English skills	How are your English language skills?	(1) I do not know English at all; (2) I know the elementary phrases – I can say hey, ask for directions, ask for prices in stores; (3) I can have non-complicated conversations and make myself understandable; (4) I am capable of having a conversation (also in the phone), but the content of the conversation is still simple; (5) I can participate in most conversations and give a presentation fluently; (6) I speak and write English independently and confidently

Main activity	What was your occupational status in Ukraine?	(1) Anything else; (2) I had a job; (3) I was not in a job/studying/taking care of my children from home; (4) I was retires/early-retirement; (5) I was studying; (6) I was taking care of my children from home
Intro text ITQ: The following questions deal with reactions, emotions or feelings that you may be experiencing after having experienced a potentially traumatic event. Keeping your worst traumatic event in mind, please read each item carefully, then circle one of the numbers to the right to indicate how much you have been bothered by that problem in the past month.		
Re-experiencing A	Having upsetting dreams that replay part of the experience or are clearly related to the experience?	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
Re-experiencing B	Having powerful images or memories that sometimes come into your mind in which you feel the experience is happening again in the here and now?	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
Avoidance A	Avoiding internal reminders of the experience (for example, thoughts, feelings, or physical sensations)?	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
Avoidance B	Avoiding external reminders of the stressful experience (for example, people, places, conversations, activities, objects, or situations)?	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
Sense of threat A	Being "superalert" or watchful or on guard?	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
Sense of threat B	Feeling jumpy or easily startled	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
In the past month, have the previous symptoms:		
PTSD functional impairment A	Affected your relationships or social life?	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
PTSD functional impairment B	Affected your work or ability to work?	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
PTSD functional impairment C	Affected any other important part of your life such as parenting, or school or college work, or other important activities?	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
Below are problems or symptoms that people who have had stressful or traumatic events sometimes experience. The questions refer to ways you typically feel, ways you typically think about yourself and ways you typically relate to others. Answer the following thinking about how true each statement is of you.		
Affective dysregulation A	When I am upset, it takes me a long time to calm down	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
Affective dysregulation B	I feel numb or emotionally shut down	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
Negative self-concept A	I feel like a failure	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
Negative self-concept B	I feel worthless	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
Disturbances in relationships A	I feel distant or cut off from people	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
Disturbances in relationships B	I find it hard to stay emotionally close to people.	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
In the past month, have the above problems in emotions, in beliefs about yourself and in relationships:		
DSO functional impairment A	Created concern or distress about your relationships or social life?	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
DSO functional impairment B	Affected your work or your ability to work?	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
DSO functional impairment C	Affected any other important parts of your life such as parenting, or school or college work, or other important activities?	(1) Not at all; (2) A little bit; (3) Moderately; (4) Quite a bit; (5) Extremely
Background data from survey		
Survey date	Date of replying to survey	
Survey in Russian	Respondents' choice of questionnaire language	Ukrainian/Russian

A.1.3 PTSD and related measures

We assess probable PTSD-symptoms and *Disturbances in Self-Organization* (DSO), and classify the fulfillment of criteria for PTSD and complex PTSD (CPTSD), using the International Trauma Questionnaire (ITQ). The ITQ consists of 18 items (listed in Table A1). Six items cover PTSD-symptoms in the domains of re-experiencing, avoidance, and sense of current threat, with two symptoms in each domain. The participant is asked to report how much he or she has been bothered by that problem in the past month (sample item from the re-experiencing domain: *"Having upsetting dreams that replay part of the experience or are clearly related to the experience?"*). Each item is answered on a five-point Likert scale, with the categories: *"Not at all"*, *"A little bit"*, *"Moderately"*, *"Quite a bit"*, and *"Extremely*. In addition to the six symptoms, three items assess functional impairment related to the symptoms (sample item: *"In the past month, have the above problems affected your relationships or social life?"*). A symptom is considered endorsed with a score of *moderately* or higher. If a participant endorses at least one symptom in each domain, while at the same time fulfilling at least one of the functional impairment criteria, that participant is categorized as a probable PTSD-case.

Similarly, six items cover *"Disturbances in Self-organisation"* (DSO), two for each of the domains affect dysregulation, negative self-concept, and disturbed relationships (sample item for affect dysregulation: *"I feel numb or emotionally shut down."*). The DSO-symptoms are followed by three items covering functional impairment related to the symptoms. As for PTSD, when at least one symptom in each domain is endorsed *and* at least one of the items for functional impairment is endorsed, the DSO-criteria are fulfilled. When both PTSD and DSO-criteria are fulfilled, the participant is categorized as a probable CPTSD-case.

Table A2 provides an overview of the variables defined based on the ITQ. Beyond the definitions of PTSD and CPTSD following WHO's diagnostic classification system (ICD-11), we defined similar variables excluding DSO functional impairment B (*"In the past month, have the previous symptoms... affected your work or ability to work?"*) due to concerns that it may create reverse causality whereby people who do not work are more likely to state that they are unable to. However, the classification of PSTD and CPTSD is not very sensitive to excluding this question and our findings are similar (see Figure 2 in the paper). Finally, we define the PTSD score and the DSO score as a simple sum of the symptom variables used to define PTSD and CPTSD, respectively.

Table A2: Variables based on the ITQ from the survey

Variable	Definition	Values
PTSD	(Re-experiencing A ≥ 3 or Re-experiencing B ≥ 3) and (Avoidance A ≥ 3 or Avoidance B ≥ 3) and (Sense of threat A ≥ 3 or Sense of threat B ≥ 3) and (PTSD functional impairment A ≥ 3 or PTSD functional impairment B ≥ 3 or PTSD functional impairment C ≥ 3)	(1) No diagnosis; (2) PTSD
CPTSD	PTSD and (Affective dysregulation A ≥ 3 or Affective dysregulation B ≥ 3) and (Negative self-concept A ≥ 3 or Negative self-concept B ≥ 3) and (Disturbances in relationships A ≥ 3 or Disturbances in relationships B ≥ 3) and (DSO functional impairment A ≥ 3 or DSO functional impairment B ≥ 3 or DSO functional impairment C ≥ 3)	(1) No diagnosis; (2) CPTSD
Adjusted PTSD	(Re-experiencing A ≥ 3 or Re-experiencing B ≥ 3) and (Avoidance A ≥ 3 or Avoidance B ≥ 3) and (Sense of threat A ≥ 3 or Sense of threat B ≥ 3) and (PTSD functional impairment A ≥ 3 or PTSD functional impairment C ≥ 3)	(1) No diagnosis; (2) PTSD
Adjusted CPTSD	Adjusted PTSD and (Affective dysregulation A ≥ 3 or Affective dysregulation B ≥ 3) and (Negative self-concept A ≥ 3 or Negative self-concept B ≥ 3) and (Disturbances in relationships A ≥ 3 or Disturbances in relationships B ≥ 3) and (DSO functional impairment A ≥ 3 or DSO functional impairment C ≥ 3)	(1) No diagnosis; (2) CPTSD
PTSD score	Sum of Re-experiencing A, Re-experiencing B, Avoidance A, Avoidance B, Sense of threat A and Sense of threat B (all 0-indexed)	Continuous (range 0-24)
DSO score	Sum of Affective dysregulation A, Affective dysregulation B, Negative self-concept A, Negative self-concept B, Disturbances in relationships A and Disturbances in relationships B (all 0-indexed)	Continuous (range 0-24)

A.2 Register Data

A.2.1 Monthly labor market outcomes

Monthly data on labor market outcomes comes from BFL (employment for salaried employees register).[10] The register contains all workers in Denmark. The source of BFL is income information reported to the tax authorities by employers. At the time of our analysis we could access this information up to and including third quarter of 2024. Hence, we can follow everyone in our analysis data at least 24 months. We defined the outcome variables listed in Table A3. For individuals with several employers, we simply sum hours and earnings across employers.

A.2.2 Background variables

We used a combination of information from the population register (BEF), the migration register (VNDS) and the admission register (OPHG) to define the variables listed in Table A4.

Age is calculated as the difference between date of birth and date of immigration for immigrants and captures age at immigration, while for natives we measure age in March 2022. We define indicator variables for female, prior immigration(s) to Denmark, month of immigration, and residency based on a Special Law for People Displaced from Ukraine. Finally, children can be connected to their parents via a mother

[10] https://www.dst.dk/da/TilSalg/data-til-forskning/generelt-om-data/dokumentation-af-data/hoejkvalitetsvariable/beskaeftigelse-for-loenmodtagere---bfl, last accessed May 22, 2025.

and a father ID. We compute the number of children and the age of children at immigration. Similarly married or cohabiting partners also have an ID which we use to define whether someone immigrated with a partner to Denmark.

Table A3: Outcomes from monthly tax records

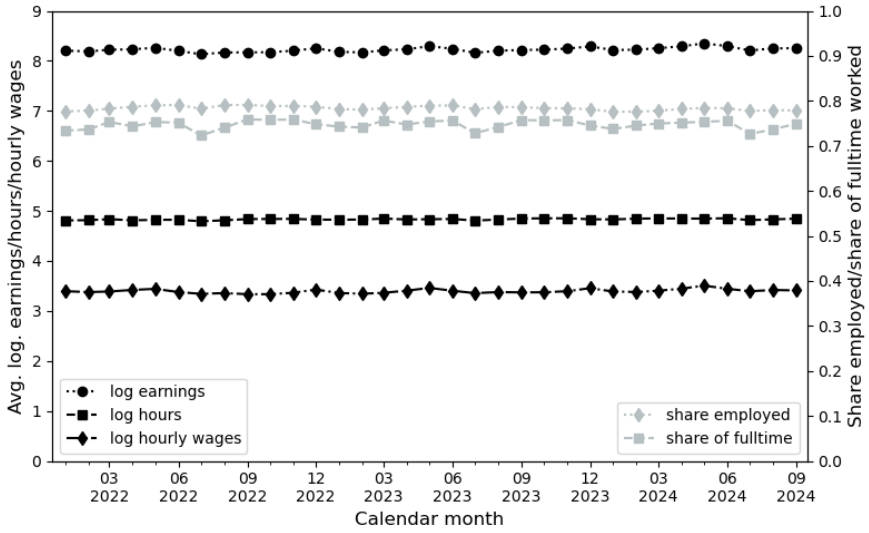
Variable	Description
Employed	Indicator for ≥1 hour of paid work in the month.
Share of fulltime worked	Total hours worked per month divided by fulltime amount (37 hours/week)
Earnings	Total monthly earnings.
Hours	Total number of hours worked in the month.
Hourly wage	Total earnings divided by total hours in the month.

Note: All outcomes are monthly.

Table A4: Remaining register variables

Variable	Description
Age	Calculated using date of birth.
Female	Indicator based on biological sex.
Been in DK before	Indicator equal to one for individuals with one or more immigration spells in Denmark prior to February 24, 2022.
Month of immigration	Based on date of immigration on or after February 24, 2022.
Special Law Permit	Indicator equal to one if the individual is in Denmark under the Special Law for People Displaced from Ukraine and zero for all other admission categories. Based on the admission category obtained closest in time to the date of immigration.
Number of children in Denmark, ≤ 18	Indicator variables for children in Denmark: 1, 2, ≥3 (antboernf).
Partner in DK	Partner present in Denmark in month of immigration.
Region of first address	From first data entry in population register.

Figure A1: Natives' labor market outomes over time



A.3 Analysis sample

A.3.1 Main analysis sample

Our sample selection criteria for refugees mirrors those described in Section A.1 for the survey population with a few additional restrictions.[11] The additional restrictions were imposed to be able to follow everyone of working age for at least 24 months before our outcome panel ended in September, 2024 (see Section A.2.1). Hence, we only keep refugees who were 18 to 64 years old at the time of immigration, entered Denmark before September 30, 2022 and stayed in Denmark at least 24 months. This leaves us with 4,533 respondents in our main analysis sample.[12]

We define the native sample as a 5% random sample of the native-born residents in Denmark aged 18-64 in March 2022 and we follow them until September, 2024. We have 136,746 natives.

A.3.2 Sample used to construct weights and analyze attrition

To be able to study the role of attrition between month 12 and 24 since immigration, we define a bigger sample of refugees who stay in Denmark 12 months or longer, and we calculate the weights with respect to non-response in this sample. We have 12,608 refugees and 4,782 respondents (complete responses) who stayed at least 12 months (see Table B1). Since our survey was launched one year after the full-scale invasion of Ukraine, the first to arrive would have been in the country close to one year and our survey is not representative across arrival month for the very short stays. The very short stays (less than one year) are also less relevant when the purpose is to study assimilation in the destination country.

[11] We replicate the selection criteria for the survey population using the register recording all in- and out-migrations (VNDS) and the quarterly population registers (BEF). The contacted populated is a perfect subset of our population. However, a few additional individuals that were not contacted appear in our population. We believe this is because we accessed the registers at a later time so that updates and corrections that have been recorded in the meantime would lead to differences. In particular, more individuals are registered as immigrated in the relevant period compared to when Statistics Denmark drew the population in real time.

[12] Respondents were able to complete the questionnaire but still have missing values if they for example reply *"don't know"* or *"don't want to answer"*. We group such responses together those who did not complete the questionnaire.

B Methods

B.1 Non-response weighting

Table B1 compares respondents to the underlying population of displaced Ukrainians in Denmark, based on the register variables listed in Table A4. Columns 1-4 of Table B1 show that respondents are generally representative of the population but there are some small differences that are statistically significant. For instance, respondents are on average half a year younger, more often female (and single mother) and more likely to have arrived on a Special Law permit and with accompanying children than the underlying population.

Columns 5-7 of Table B1 show the same summary statistics after re-weighting respondents using entropy balance weights targeting up to third moments of the underlying population (see Hainmueller, 2012). Hence, the weights in column 5 are constructed to minimize the differences in means (first moment), standard deviation (second moment) and skewness (third moment) between respondents and the population (respondents and non-respondents). Figure B1 shows the distribution of the three weighting schemes. Our analysis in the paper is based on weights matching up to third moment.

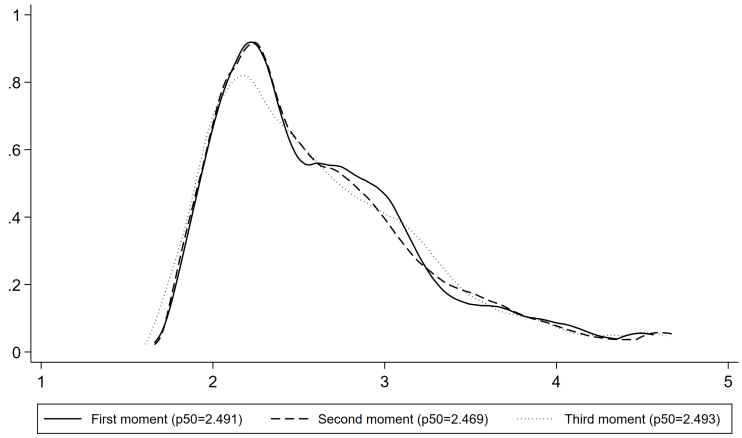
The modal weight for all weighting schemes is close to two and the median is between 2.5 and 2.6. All weighting schemes perform equally well in terms of matching the means among the respondents to the means in the population.
While the weighting procedure balances observed covariates well, respondents could differ in terms of unobservable factors. This is usually impossible to check as often one has very limited data on the background population. For instance, we only observe the ITQ for respondents in the survey. However, we observe labor market outcomes for everyone in the register data. Such outcomes should not be used in the weights but it is informative to see how similar respondents are to the underlying population on these outcomes too. Table B2 shows exactly this following a similar structure as Table B1 with an additional column showing the labor market outcomes for a 5% random sample of native born that we also use in the analysis.

Table B1: Representativeness of respondents and weighting

	Population	Respondents			Respondents, weighted		
	Mean	Mean	Difference	p-value	Mean	Difference	p-value
Age at entry	38.616	38.096	0.519	0.008	38.615	0.001	0.997
Female	0.764	0.821	-0.057	0.000	0.764	-0.000	0.982
Partner in DK	0.300	0.293	0.007	0.399	0.300	0.000	0.994
Number of children indicators							
No children	0.516	0.445	0.071	0.000	0.516	0.000	0.981
One child	0.272	0.316	-0.044	0.000	0.273	-0.000	0.991
Two children	0.160	0.184	-0.024	0.000	0.160	-0.000	0.994
Three or more children	0.051	0.055	-0.004	0.336	0.051	-0.000	0.984
Previous immigrations	0.037	0.033	0.005	0.135	0.037	0.000	0.996
Special Law permit	0.943	0.958	-0.014	0.000	0.943	-0.000	0.992
Entry month							
2022m3	0.055	0.054	0.001	0.701	0.056	-0.000	0.974
2022m4	0.248	0.265	-0.016	0.026	0.248	-0.000	1.000
2022m5	0.332	0.345	-0.013	0.109	0.332	0.000	0.999
2022m6	0.175	0.164	0.012	0.068	0.175	0.000	0.992
2022m7	0.055	0.042	0.012	0.001	0.055	0.000	0.994
2022m8	0.062	0.056	0.006	0.165	0.062	0.000	0.995
2022m9	0.073	0.074	-0.002	0.698	0.073	0.000	0.999
Region of first settlement							
Northern Juttland	0.108	0.111	-0.003	0.559	0.108	0.000	0.998
Mid-Jutland	0.247	0.276	-0.028	0.000	0.247	-0.000	0.994
Southern Denmark	0.219	0.218	0.000	0.978	0.218	0.000	0.995
Capital region	0.289	0.274	0.015	0.051	0.289	0.000	0.990
Sealand	0.137	0.121	0.016	0.005	0.137	-0.000	0.987
Observations	12608.000	4782.000	.	.	4782.000	.	.

Note: Table entries are sample means differences in means in parentheses. All variables but age are indicator variables. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Figure B1: Densities of weights targeting first, second, and third moments in the population



Note: Weights are constructed using entropy balancing (Hainmueller, 2012).

Table B2 shows that respondents are slightly (insignificant 2 percentage points) more employed than the underlying population around the time of the survey, while differences are negligible both economically and statistically on the intensive margin of employment. The weighting scheme reduces these distances in employment outcomes such that the outcomes of the weighted respondents get closer to population after weighting.

All results in the main part are computed applying the weights matching up to third moment (right-most columns in Tables B1 and B2).

Table B2: Labor market outcomes 10 months after migration

	Natives	**Refugees**	**Respondents**			
			Unweighted	1. moment	2. moment	3. moment
Panel A: Full sample						
Employed	0.784 (0.001)	0.554 (0.005)	0.576 (0.007)	0.579 (0.005)	0.576 (0.005)	0.576 (0.005)
Share of fulltime	0.756 (0.001)	0.466 (0.005)	0.478 (0.007)	0.490 (0.005)	0.486 (0.005)	0.486 (0.005)
Observations	136746	11832	4533	4533	4533	4533
Panel B: Working population						
Log(monthly earnings)	8.209 (0.003)	7.687 (0.009)	7.672 (0.015)	7.705 (0.009)	7.701 (0.009)	7.700 (0.009)
Log(monthly hours)	4.847 (0.002)	4.666 (0.009)	4.649 (0.014)	4.675 (0.008)	4.673 (0.008)	4.672 (0.008)
Log(hourly wages)	3.362 (0.001)	3.021 (0.003)	3.023 (0.005)	3.030 (0.003)	3.029 (0.003)	3.028 (0.003)
Observations	107171	6554	2609	2609	2609	2609

Note: Table entries are sample means and standard errors on the means in parentheses. Weights are constructed using entropy balancing (Hainmueller, 2012). Outcomes are measured in month 10 after arrival, which is the modal month of survey response in our sample due to the large arrivals in the spring 2022.

B.2 Bounding the influence of unobserved factors

In the case of unobserved variables which are associated in the same direction with both PTSD and employment, our estimate will be an lower bound of the true causal effect in absolute terms. In the case of unobserved variables that are positively associated with PTSD but negatively with employment (or vice versa), our estimate will be an upper bound of the true causal effect in absolute terms. One such factor could be pre-existing vulnerabilities which increase the likelihood of developing PTSD but also decreases

employability. It is these kinds of factors that we are most worried about because they would bias our estimates away from zero and result in us detecting a significant negative effect of PTSD when in fact it is driven by omitted variables.

Oster (2019)

We assess the sensitivity of our main estimate—the PTSD gap in employment outcomes—to unobserved confounders using two different methods. First, we follow the bounding argument by Oster (2019) that allows researchers to assess the sensitivity of treatment effects by defining two parameters: the maximum R^2 if all observables and unobservables in the model were included (R^2_{max}), and a measure of proportional selection of unobserved relative to observed variables (δ).
The R^2_{max} is the maximum explanatory power that one would have in the full model where both observed and unobserved variables are included along with the treatment. For the results in the main part of the paper, we use $R^2_{max} = 1.3 \times R^2$. The factor 1.3 comes from Oster's original paper, where she uses a sample of articles with treatment effects from randomized treatments published in top journals (Oster, 2019). In this sample, 1.3 is the factor which would allow at least 90% of randomized results to survive. Note that $R^2_{max} = 1$ would be needlessly restrictive since an $R^2 = 1$ is not usually obtained in employment regressions.

The interpretation of δ, as the degree of proportional selection capturing how strongly the unobserved factors drive treatment assignment relative to the observed factors, is not straightforward. As Cinelli and Hazlett (2019) point out, δ captures not only the relative influence of observed and unobserved over the treatment, but also their association with the outcome. For the results in the main paper, we chose $\delta = 1$. In Appendix Section C.2, we compute bias corrected coefficients for varying values of δ and R^2_{max} as well as the value of δ for different values of R^2_{max} that would be needed to drive our main results to zero, referred to as the maximal relative selection.

Cinelli and Hazlett (2019)

We report the robustness value and the adjusted effect estimate following Cinelli and Hazlett (2019) in the main part of the paper. The adjusted effect estimate is benchmarked against the female indicator in our regression and shows how our main estimate would change if we had included unobserved confounders that are orthogonal to all included controls, and as important as the female indicator in terms of their associations with both the treatment and the outcome. It is very unlikely that such a orthogonal confounder would exist given the large set of included controls, but nevertheless reassuring that our main result is quite robust to this hypothetical exercise. Appendix Section C.2 reports further sensitivity analysis using contour plots from Cinelli and Hazlett (2019).

C Sensititivity analysis

C.1 Conditional balance and coefficient stability

Tables C1 to C6 show estimation results sequentially adding the covariates (Columns 1-3) and dropping the weights (Column 4) for all four labor market outcomes: extensive margin of employment, employment measured in fulltime equivalents, log earnings, log hours and log hourly wages. Hence, Table C1 shows the estimation results corresponding to the coefficients plotted in Figure 2.b in the main text. For all outcomes, we see that once we added our demographic and educational controls, adding further details from the survey does not affect the coefficient on probable PTSD much, while R^2 increases showing that the survey variables provide important information about the employability of the refugees.

Table C1: Employed

	Unconditional	Baseline	+refugee controls	Full model, no weights
Panel A: 12 months after migration				
PTSD gap	-0.109	-0.098	-0.094	-0.094
	(0.016)	(0.015)	(0.015)	(0.015)
	[-0.14,-0.078]	[-0.128,-0.068]	[-0.124,-0.065]	[-0.124,-0.065]
R^2	0.01	0.105	0.14	0.123
R^2 adj.	0.01	0.102	0.129	0.112
N refugees	4782	4782	4782	4782
Panel B: 24 months after migration				
PTSD gap	-0.086	-0.076	-0.074	-0.074
	(0.016)	(0.015)	(0.015)	(0.015)
	[-0.117,-0.055]	[-0.106,-0.046]	[-0.104,-0.044]	[-0.103,-0.044]
R^2	0.007	0.093	0.12	0.105
R^2 adj.	0.007	0.09	0.108	0.094
N refugees	4533	4533	4533	4533

Note: Standard errors in parentheses, and 95% confidence intervals in squared brackets. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table C2: Impact of each of the baseline controls

	(1) No controls	(2) Age	(3) Female	(4) Family	(5) Region	(6) Education	(7) All
PTSD gap	-0.086***	-0.090***	-0.075***	-0.085***	-0.084***	-0.088***	-0.076***
	(0.016)	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)	(0.015)
	[-0.117,-0.055]	[-0.120,-0.060]	[-0.106,-0.043]	[-0.116,-0.054]	[-0.115,-0.052]	[-0.119,-0.056]	[-0.106,-0.046]
R^2	0.007	0.079	0.013	0.017	0.010	0.009	0.093
R^2 adj.	0.007	0.078	0.012	0.016	0.009	0.008	0.090
N	4533	4533	4533	4533	4533	4533	4533

Table C3: Share of fulltime worked

	Unconditional	Baseline	+refugee controls	Full model, no weights
Panel A: 12 months after migration				
PTSD gap	-0.116	-0.093	-0.088	-0.088
	(0.016)	(0.015)	(0.015)	(0.015)
	[-0.147,-0.085]	[-0.123,-0.064]	[-0.118,-0.059]	[-0.117,-0.06]
R^2	0.011	0.127	0.162	0.145
R^2 adj.	0.011	0.124	0.152	0.135
N refugees	4782	4782	4782	4782
Panel B: 24 months after migration				
PTSD gap	-0.112	-0.09	-0.086	-0.086
	(0.016)	(0.015)	(0.015)	(0.015)
	[-0.144,-0.08]	[-0.12,-0.059]	[-0.116,-0.056]	[-0.116,-0.056]
R^2	0.011	0.119	0.15	0.132
R^2 adj.	0.01	0.115	0.139	0.121
N refugees	4533	4533	4533	4533

Note: Standard errors in parentheses, and 95% confidence intervals in squared brackets. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table C4: log(Earnings)

	Unconditional	Baseline	+refugee controls	Full model, no weights
Panel A: 12 months after migration				
PTSD gap	-0.082	-0.049	-0.044	-0.042
	(0.034)	(0.035)	(0.033)	(0.031)
	[-0.149,-0.015]	[-0.117,0.018]	[-0.109,0.021]	[-0.104,0.019]
R^2	0.002	0.079	0.112	0.101
R^2 adj.	0.002	0.074	0.095	0.083
N refugees	2972	2972	2972	2972
Panel B: 24 months after migration				
PTSD gap	-0.107	-0.078	-0.071	-0.073
	(0.028)	(0.027)	(0.027)	(0.027)
	[-0.162,-0.052]	[-0.131,-0.025]	[-0.125,-0.018]	[-0.125,-0.02]
R^2	0.005	0.087	0.119	0.103
R^2 adj.	0.005	0.083	0.102	0.086
N refugees	3102	3102	3102	3102

Note: Standard errors in parentheses, and 95% confidence intervals in squared brackets. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table C5: log(Hours)

	Unconditional	Baseline	+refugee controls	Full model, no weights
Panel A: 12 months after migration				
PTSD gap	-0.078	-0.055	-0.053	-0.048
	(0.032)	(0.033)	(0.031)	(0.03)
	[-0.14,-0.015]	[-0.119,0.01]	[-0.115,0.008]	[-0.106,0.009]
R^2	0.002	0.05	0.082	0.073
R^2 adj.	0.002	0.045	0.063	0.055
N refugees	2972	2972	2972	2972
Panel B: 24 months after migration				
PTSD gap	-0.087	-0.067	-0.063	-0.063
	(0.025)	(0.025)	(0.025)	(0.025)
	[-0.136,-0.037]	[-0.116,-0.018]	[-0.113,-0.013]	[-0.112,-0.013]
R^2	0.004	0.049	0.072	0.063
R^2 adj.	0.004	0.044	0.055	0.045
N refugees	3102	3102	3102	3102

Note: Standard errors in parentheses, and 95% confidence intervals in squared brackets. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table C6: log(Hourly wage)

	Unconditional	Baseline	+refugee controls	Full model, no weights
Panel A: 12 months after migration				
PTSD gap	-0.004	0.005	0.009	0.006
	(0.012)	(0.012)	(0.012)	(0.012)
	[-0.028,0.02]	[-0.018,0.029]	[-0.015,0.033]	[-0.017,0.029]
R^2	0	0.052	0.095	0.082
R^2 adj.	-0	0.047	0.077	0.064
N refugees	2972	2972	2972	2972
Panel B: 24 months after migration				
PTSD gap	-0.02	-0.01	-0.008	-0.01
	(0.012)	(0.011)	(0.011)	(0.011)
	[-0.043,0.002]	[-0.033,0.012]	[-0.03,0.014]	[-0.031,0.011]
R^2	0.001	0.067	0.11	0.099
R^2 adj.	0.001	0.062	0.093	0.082
N refugees	3102	3102	3102	3102

Note: Standard errors in parentheses, and 95% confidence intervals in squared brackets. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

C.2 Bounding the influence of observables

This section provides additional details on the sensitivity analyses reported in Section 4.2. We first describe the implementation of Oster (2019) bounds, followed by the partial-R^2 framework of Cinelli and Hazlett (2019).

C.2.1 Oster (2019)

For the sensitivity analysis in the main analysis, we use $R^2_{max} = 1.3 \times R^2$ and $\delta = 1$. In Figure C1, we vary both R^2_{max} and δ to explore the sensitivity of the bounds to these parameters. A few important insights emerge from this exercise.

First, the bias adjusted employment gaps for $R^2_{max} = 1.3 \times R^2$ are not statistically different whether we choose $\delta = 0.5$ or $\delta = 1$. Second, our coefficient would vanish only for very large values of R^2_{max} and δ. For $R^2_{max} = 4 \times R^2$, the bias adjusted coefficients are close to insignificant with a $\delta = 0.5$ and insignificant with a $\delta = 1$. This implies a level of R^2 which is not usually achieved in employment regressions. Moreover, given the rich information we have on demographics, education, main activity in Ukraine, network in Denmark etc., we think a 4 times increase in the R^2 if unobservables were to be included is highly unrealistic. We, therefore, conclude that our results are very robust and unlikely to be driven by unobserved factors. One would need unrealistically high value of R^2_{max} and δ to drive the estimated PTSD gap in outcomes to zero. Third, we pattern is similar whether we analyze the extensive margin of employment or employment measured in fulltime equivalents. This is reassuring since it shows this exercise does not show too little sensitivity simply because R^2 is smaller and less sensitive for binary compared to continuous outcomes.

C.2.2 Cinelli and Hazlett (2019)

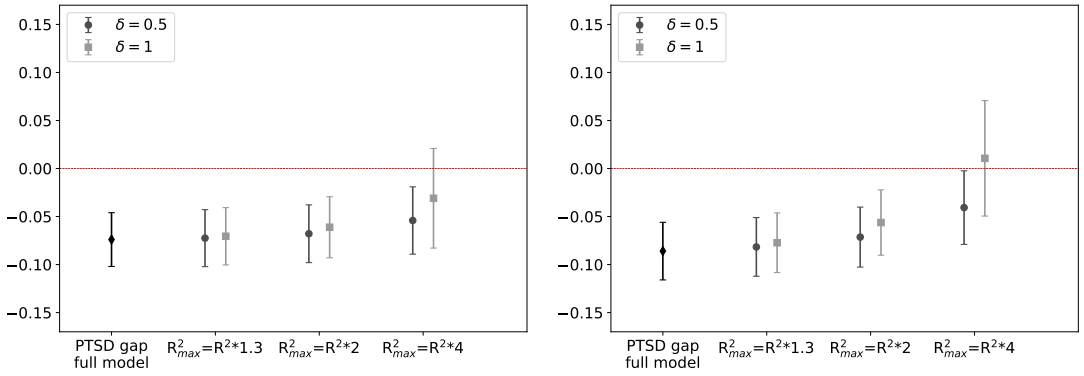
Cinelli and Hazlett (2019) characterizes omitted variable bias in terms of two intuitive quantities: the share of residual variance in the treatment (probable PTSD) and the outcome (employment) that would need to be explained by an unobserved confounder, conditional on all included controls. Figure C2 plots hypothetical estimates of the PTSD-employment gap for different combinations of the partial-R^2 of hypothetical confounders with the outcome (y-axis) and with the treatment (x-axis). These curves are bench-marked against the importance of the female indicator, which is among the strongest observed predictors of both employment and probable PTSD and has the largest impact on the estimated PTSD coefficient when included in the regression. The results show that even a confounder three times as important as gender-—in terms of explaining residual variation in both PTSD and employment—-would not be sufficient to reduce the estimated PTSD employment gap to zero. These findings reinforce the conclusion that the main results are robust to plausible forms of omitted variable bias.

C.3 Summary

Taken together, the Oster (2019) and Cinelli and Hazlett (2019) sensitivity analyses indicate that unobserved confounding would need to be implausibly strong-—relative to a rich set of observed covariates—to alter our conclusions. While these approaches do not establish causal identification, they demonstrate that the estimated association between early PTSD symptoms and subsequent employment outcomes is highly robust and unlikely to be driven by omitted variables.

Figure C1: Main and bias adjusted coefficients following Oster (2019)

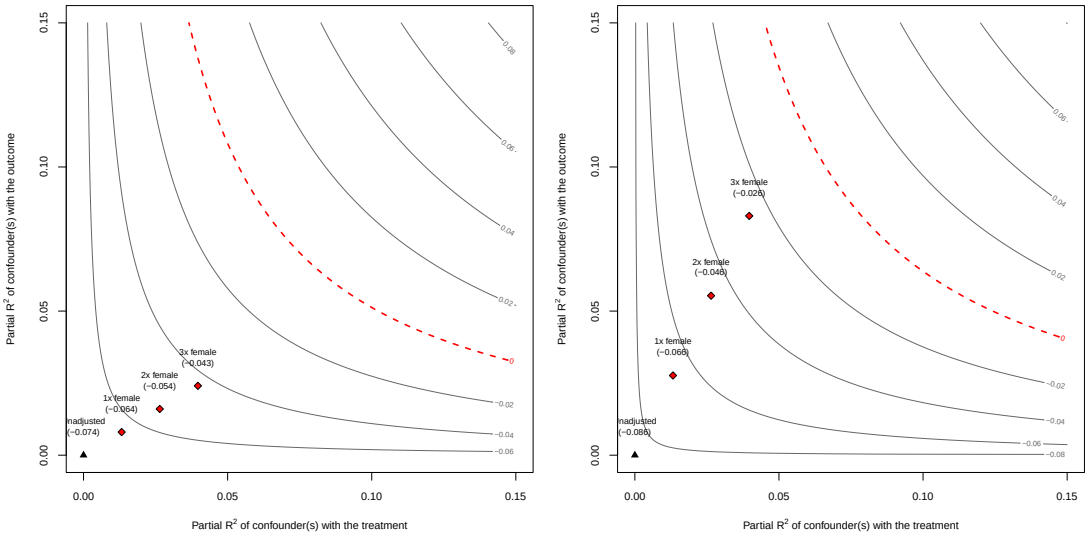
(a) Employed **(b)** Share of fulltime worked



Note: The graph shows our main estimate of the PTSD gap in employment from Table 2 in the main text together with bias adjusted coefficients following Oster (2019) for different values of the relative selection on observables versus unobservables and for the maximum R^2. Panel a reports results for the extensive margin of employment, while Panel b reports results for employment measured in full-time equivalents.

Figure C2: Contour plots following Cinelli and Hazlett (2019)

(a) Employed **(b)** Share of fulltime worked



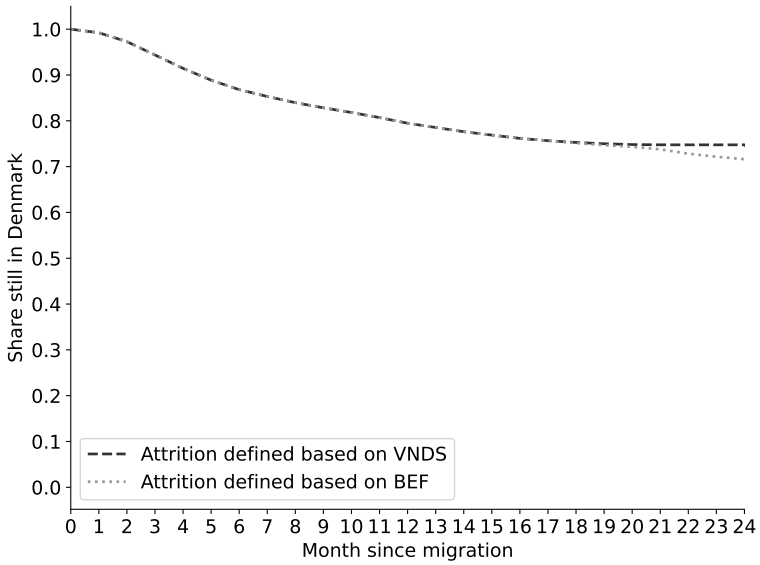
Note: The graph shows our main estimate of the PTSD gap in employment from Table 2 in the main text (black triangle) together with sensitivity contours following Cinelli and Hazlett (2019) showing combinations of partial R^2 of confounder(s) with the outcome and with the treatment (PTSD indicator) that would give rise to the same bias adjusted coefficient, as well as the size of the bias adjusted coefficient if an orthogonal confounder is 1, 2 or 3 times more important than the most important included covariate (red diamonds). Panel a reports results for the extensive margin of employment, while Panel b reports results for employment measured in full-time equivalents

C.4 Time Horizon and Attrition

We have labor market outcomes until September 2024 and we include arrivals until September 2022 such that it is possible to follow everyone for 24 months after arrival. We currently only have the exact date of emigration from the migration register (VNDS) until December 31, 2023, meaning we do not observe migrations after 2023. In this section we rely on quarterly draws from the population register (BEF) to identify emigrations in 2024. This approach reveals that 134 people emigrated in 2024. If they all emigrated on the first day of each quarter, this would reduce the number of individuals that we can observe for 18 months from 4,533 (main sample) to 4,382 observations.

Figure C3 plots the share of refugees who are still in Denmark by months since immigration using the two alternative ways to define presence in Denmark in 2014. We slightly underestimate attrition in month 18-24 since migration using only VNDS. The quarterly draws from the population register BEF give us all individuals residing in Denmark at the end of a quarter so we overestimate attrition for month 18-24 since migration. The true attrition is between the two lines, i.e. between 9-11% in month 24. However, this alternative definition of attrition has no detectable impact on our estimates.

Figure C3: Share still present in Denmark by month since arrival



Note: Two different methods to define presence in Denmark.

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