Journal of the Royal Statistical Society Series A: Statistics in Society, 2025, **00**, 1–16
https://doi.org/10.1093/jrsssa/qnae106

ROYAL STATISTICAL SOCIETY
DATA | EVIDENCE | DECISIONS
Journal of the Statistics Society
Series A
Statistics in Society

Original Article

Does ad hoc language training improve the economic integration of refugees? Evidence from Germany's response to the Syrian refugee crisis

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Abstract

Given the global displacement crisis, the integration of refugees has emerged as a critical policy issue for many host countries. A key challenge involves supporting refugees in learning the language of their host country. While several European nations have instituted publicly funded language training for asylum seekers and refugees soon after their arrival, evidence on the efficacy of these early language programmes in promoting economic integration remains limited. This study examines the impact of a pioneering, large-scale ad hoc programme introduced by German policymakers, which provided basic language training to over 230,000 refugees arriving in 2015–2016. Utilizing register data on the population of asylum seekers and exploiting a cut-off date in programme eligibility, we assess the programme's effectiveness using a regression discontinuity design. Our findings reveal no discernible effect on refugee employment over the subsequent 2 years. To explore whether language programmes are generally ineffective during refugee crises, we contrast these results with the impacts of a more comprehensive, preexisting, yet smaller-scale programme. Using a variety of difference-in-differences estimators, we find that this programme considerably increased refugee employment. These contrasting findings offer important insights for policymakers on designing effective language training programmes for refugees.

Keywords: immigrant integration, refugee language training programme, regression discontinuity design

1 Introduction

Over the last decades, policymakers in Europe and across the globe have grappled with major displacement crises that have resulted in a significant increase in the number of refugees fleeing conflict and persecution. Western destination countries face a significant challenge in designing

Received: January 31, 2022. **Revised:** September 18, 2024. **Accepted:** September 19, 2024
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policies and programmes that facilitate the integration of refugees into the host country's economy and society (Council of the European Union, 2014; Degler & Liebig, 2017; OECD, 2016; The Economist, 2015; UNHCR, 2013).

Given that refugees are often unfamiliar with the host country's language, one of the key policy challenges involves the provision of language training programmes (Dumont et al., 2016; Liebig, 2007; Scholten et al., 2017). Language acquisition is often the first step in the successful integration of refugees and serves as a vehicle for finding employment (Chiswick & Miller, 1995; Cortes, 2004; Dribe & Lundh, 2011; Dustmann & Fabbri, 2003; Schönwälder et al., 2005). Proficiency in the host country's language can facilitate labour market integration through at least three channels. First, language is a fundamental element of human capital (Chiswick & Miller, 1995; Dustmann & Fabbri, 2003; van Tubergen et al., 2004). Second, language complements and enhances the transferability of skills obtained abroad in the host country labour market (Berman et al., 2003; Chiswick & Miller, 2001). Third, employers and refugees in Europe often state that the lack of sufficient language skills is one of the main obstacles to employability, even for low-skilled jobs (Degler & Liebig, 2017; Fasani et al., 2018).

These findings suggest that early investments in language courses for refugees shortly after arrival may subsequently yield substantial returns in terms of improved economic integration (Bach et al., 2017). By improving economic integration, early language training might generate significant economic benefits, not only for refugees, who could more quickly find jobs that match their skill set but also for the host economy, in terms of higher tax contributions from employed refugees and lower welfare expenditures for unemployed refugees.

Between 2015 and 2017, nearly all OECD countries offered publicly financed language programmes for refugees. Considerable variety existed, however, regarding the content of the programmes and the number of instruction hours. As Figure 1 shows, the maximum course hours ranged from 70 in Croatia to 4,800 or more in Scandinavian countries (Denmark, Norway, and Sweden). Programmes also varied on several other dimensions, such as how soon after arrival refugees could enrol in the programmes, whether instruction followed a standardized curriculum, whether certificates were offered, and whether there were national standards for course providers (Konle-Seidl, 2018; OECD, 2016; Schönwälder et al., 2005; Stiftung, 2015).

But do early language training programmes for refugees actually improve their subsequent economic integration? And if so, what quantity and quality of instruction are required to achieve measurable improvements in economic integration? A sizable literature has documented strong links between language skills and improved labour market prospects for immigrants more generally (Berman et al., 2003; Bleakley & Chin, 2004; Chiswick, 1978; Chiswick & Miller, 2001; Cohen & Haberfeld, 2007; Delander et al., 2005; Dustmann & Fabbri, 2003; Hayfron, 2001; Lochmann et al., 2019; Lubotsky, 2007). However, there exists much less evidence on the impact of language training programmes for refugees in particular.

Perhaps the studies closest to ours are Foged et al. (2022) and Foged and Van der Werf (2023), which both consider language programmes for refugees that arrived in Denmark. Foged et al. (2022) evaluate a policy reform that increased the number of hours of language training and made training mandatory for refugees who arrived in Denmark after 1 January 1999. The policy change was accompanied by some other features introduced with the same cut-off, including a temporary reduction in welfare benefits and a new scheme for placing refugees in municipalities. Using a regression discontinuity design (RDD), the study finds that the policy as a whole increased the employment probabilities by around 4 percentage points and led to higher earnings. At the cut-off refugees accumulated around 200 hr of additional language training. Foged and Van der Werf (2023) leverages variation in the accessibility of local language training centres in Denmark to examine the impact of language classes for refugees on language fluency. The study finds that an additional 100 hr of language instruction improved refugees Danish language fluency by around 8–9%, looking at refugees who arrived from 2003 to 2013. The study also finds that language classes reduced the probability that refugees leave the localities in which they are initially placed.

Our study differs from previous work in that we examine the impact of language training programmes on refugees labour market integration in Germany in the context of a major displacement crisis characterized by mass arrivals. In particular, we leverage register population data to provide evidence on the impact of a large-scale language training programme that the German

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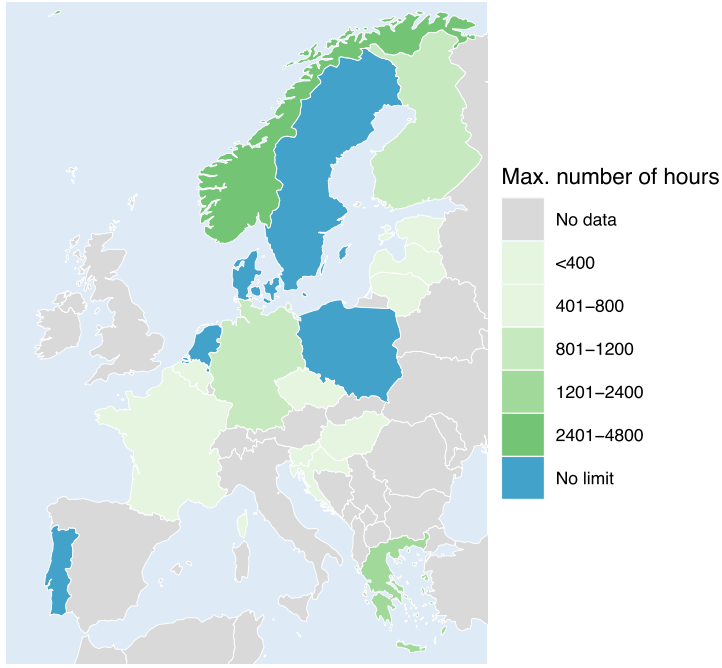

Figure 1. Maximum number of hours of public language courses in European countries around 2017. There is considerable heterogeneity in the maximum number of hours of language instruction refugees receive, ranging from 70 hr in Croatia to an unlimited number of hours in countries such as Denmark and Sweden. Data according to authors' communication with UNHCR and OECD (2016).

government rapidly developed and implemented in response to the large increase in refugees in 2015. We find no discernible effect of this programme on refugee employment over the following 22 months. To shed light on the question whether language programmes are generally ineffective during refugee crisis, we contrast this null finding with the impacts of a more comprehensive, pre-existing yet smaller-scale programme. Using a variety of difference-in-differences (DID) estimators, we find that this preexisting programme increased refugee employment by about between 4 and 5 percentage points 12 months after enrolment. Our contrasting findings illuminate the nuanced impacts of early language training programmes during a period when a large number of refugees arrived. Understanding these impacts is crucial for informing the design of language policies in Germany and beyond, and for preparing for future crises.

2 Language training and refugee integration

Research has shown that refugees experience significant employment and wage gaps compared to other immigrant groups and the native population, even many years after arrival (Cheng et al., 2021; Fasani et al., 2018). Studying early language training programmes for refugees during times of crisis is important, as refugees face many additional challenges compared to immigrants who migrate voluntarily through mechanisms like family reunification or for study and work (Becker & Ferrara, 2019; Beiser & Hou, 2000; Liebau & Schacht, 2016; Stiftung, 2015).

First, refugee crises are marked by large numbers of arrivals, leading to prolonged asylum processing times. This results in extended uncertainty about the prospects of staying in the host country, overcrowded accommodations, overburdened support programmes, higher unemployment rates among refugees, and often, political backlash against new arrivals (Fasani et al., 2018; Hainmueller et al., 2016; Hangartner et al., 2019; Pecoraro et al., 2022).

Second, many refugees have endured traumatic events in their home countries or during their journey to safety. The psychological impact of these experiences can impede their ability to focus

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on learning a new language, and the stress of resettlement and adjusting to a new culture can further hinder language acquisition (Bjertrup et al., 2018; Schlaudt et al., 2020).

Third, the language of the host country may be vastly different from the refugees' native languages, presenting a significant learning barrier (Asfar et al., 2019; Cheng et al., 2021).

Fourth, refugees often have limited opportunities to practice the language outside formal learning settings. They may reside in reception centres where their native language predominates, or they may face discrimination or social isolation, which can further impede their engagement with the new language (Van Tubergen, 2010).

These factors can significantly obstruct both language acquisition and labour market integration for refugees, underscoring the importance of studying the returns to language training for this group. Early language training might be especially critical given the existing evidence of a formative early window for integration. Studies have demonstrated that interventions shortly after arrival—such as early access to labour markets, voting rights, shorter asylum wait times, or better matching between refugee characteristics and host communities—can durably improve immigrants' subsequent integration trajectory (Bansak et al., 2018; Ferwerda et al., 2020; Hainmueller et al., 2016; Marbach et al., 2018).

3 Setting

In 2015/2016, Germany received over one million asylum seekers, a stark contrast to an average of 71,000 asylum applications per year during the period from 1995 to 2014 (BAMF, 2018). A significant portion of these refugees were escaping conflict and persecution in countries such as Syria, Afghanistan, and Iraq. It is important to note that for the purposes of this study, we use the terms 'refugees' and 'asylum seekers' interchangeably. Within our estimation sample, most individuals initially enter as asylum seekers but later receive some form of humanitarian protection, thereby transitioning into refugee status.

Prior to the 2015 crisis, Germany relied on a language programme offered by the Federal Office for Migration and Refugees (BAMF). This programme, called Integration Course (*Integrationskurs*) has been in operation since 2005 and is rather comprehensive in scope, covering up to 600 hr of instruction with a standardized curriculum. Between January 2015 and January 2017, approximately 500,000 individuals participated in the BAMF courses (Deutscher Bundestag, 2016). However, in the context of the mass arrivals of refugees during the same time period, this preexisting programme struggled to meet the demand during the refugee crisis. Since scaling up the preexisting programme proved challenging, policymakers established a novel, ad hoc language training programme called Introductory German Language Course (*Einstiegskurs zur Deutschförderung*), administered by the Federal Employment Agency (BA).

This programme was ambitious in scale and designed to promptly accommodate a large number of eligible participants from Syria, Iraq, Iran, and Eritrea. It took a less intensive approach, covering only 320 hr of instruction and lacking a standardized curriculum, and was rolled out rapidly. The first course began less than four weeks after announcement, in late October 2015. In total, about 230,000 refugees, approximately 38% of the eligible arrival population in 2015, enroled in this programme (Bundesrechnungshof, 2017), see Figure 2. The cost amounted to about 400 million Euros, or approximately 4.8 Euros per participant per training hour. Additional details on the two language training programmes are provided in the online supplementary materials.

Comparing the ad hoc programme to the distribution of course hours in other European countries (see Figure 1), we observe that the ad hoc programme, with 320 hr, ranks towards the bottom of the distribution. In contrast, the preexisting course falls within the modal category of 600 hr. Despite being less intensive than the preexisting language course, the ad hoc programme shared the goal of integrating refugees into the labour market (BA Presseteam, 2015; BAMF, 2015). However, there is scant evidence that this shift in design and delivery of language programmes succeeded.

4 Data, measures, and statistical analysis

4.1 Data

We use monthly panel data compiled from the Integrated Employment Biographies (IEB) to study the impact of both programmes on labour market integration. We combine the IEB data with

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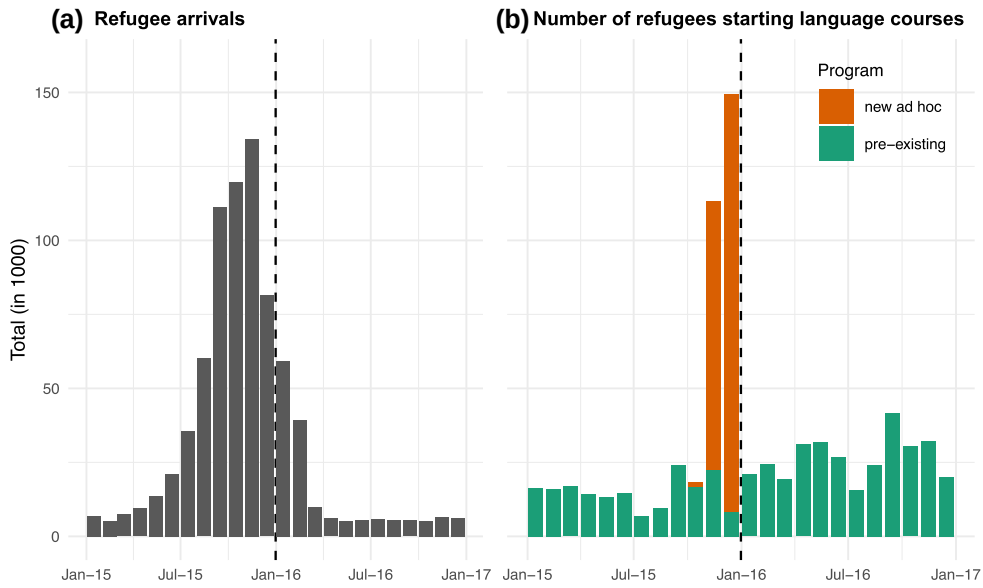

Figure 2. The left panel displays monthly registrations of refugee arrivals from Syria, Iraq, Iran, and Eritrea via the EASY-System (*Erstverteilung der Asylbegehrenden*-System) between January 2015 and January 2017. The right panel shows the number of refugees participating in the ad hoc language training programme (stacked top bars in orange). The vertical dashed line marks the cut-off date (31 December 2015) for ad hoc programme eligibility. For comparison, total enrolment in the preexisting language programme is also displayed (bottom green bars), with the last six months estimated based on the total number of courses starting. Sources: Federal Office for Migration and Refugees (BAMF) and Federal Employment Agency (BA).

another administrative individual data *Statushistorik Zuwanderung* (SHZ) that contains detailed information on socio-demographic characteristics and important migration-related information. These data are based on administrative records from the BA and allows us to cover almost all refugees who arrived in Germany.

We focus on refugees from Syria, Iran, Iraq, and Eritrea, with a observed and registered arrival date between June 2015 and June 2016, and ages 18–35 at the time of arrival. Note that at the time refugees from these four origin countries who applied for asylum in Germany were considered to have a high probability of getting protection status and therefore eligible to participate in the language training programmes and eligible to work.

To examine the effects of the ad hoc language training programme, we leverage the data on 210,369 refugees which we observe over approximately 2 years for a total of 5,487,256 person-months. Note that for about 96.8% of all refugees in the data, we have a record up until the last month in the panel, but about 3.2% drop before the end of the panel. In the robustness section below we examine the sensitivity of our results to this panel attrition.

To examine the effects of the comprehensive training programme, we use a random person sample of the population which includes 50,000 refugees (1,335,073 person-months). To enhance the comparability of estimates between the preexisting and the ad hoc programmes, we restrict this random sample to individuals who were eligible for the ad hoc programme (i.e. individuals who arrived before 31 December 2015). The reason why we use a random sample is to reduce computational burden for the panel models.

4.2 Measures

Our main outcome measure of labour market integration is the binary variable 'Has job' which measures employment status. It is coded as one for refugees when they are in full-time or marginal employment in a given month and zero otherwise. Note that marginal employment includes all part-time, low-wage jobs (including low-paying 'minijobs', apprenticeships, and paid internships).

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We also use two alternative outcomes. 'Has job (w/o minijob)' focuses only on full time employment and does not include any marginal employment. 'Has minijob' focuses only on marginal employment, respectively.

We also observe a limited set of covariates, including the age at arrival, a binary variable measuring education that takes the value of one if the individual completed more than primary school and zero otherwise, origin, and gender. More details on the data and measures is provided in the online supplementary material.

4.3 Descriptive statistics

Table 1 shows descriptive statistics for the estimation samples. The columns on the left show the full population we use to examine the effects of the ad hoc programme. The average refugee is 25 years old, the share of females is 25%, and the nationality shares are 73% Syrian, 16% Iraqi, 6% Iranian, and 5% Eritrean. Sixty percent have some schooling.

The mean of the variable measuring enrolment in the preexisting language training programme is only 2%. This low participation rate in the preexisting programme demonstrates the limited capacity of this programme to serve the large refugee population at the time and was the motivation for creating the large scale ad hoc programme.

The mean for the 'has job' outcome is only 7% indicating that most refugees are unemployed. The columns on the right show the random person sample of the population eligible for the ad hoc programme we use to examine the effects of the preexisting program. This sample mirrors the full population we use to examine the effects of the ad hoc programme with only very limited differences across covariates.

4.4 Identification and estimation strategy

4.4.1 RDD for ad hoc programme

To identify the effects of the ad hoc programme, we leverage a RDD based on the criterion that only refugees from Syria, Iran, Iraq, and Eritrea who received their registration on or before 31 December 2015, were eligible to participate. This allows us to isolate the effect of programme eligibility by comparing otherwise similar refugees who arrived just before this cut-off date to those who arrived just afterward in January 2016 and were ineligible. Note that refugees in our sample all had a strong incentive to register as soon as possible after their arrival in Germany in order to start the asylum application review and get access to rights and benefits. Therefore, it seems unlikely that refugees would try to manipulate their arrival date around the cut-off. Indeed, results from placebo tests indicate that the covariates are well balanced at the threshold, consistent with a quasi-random assignment of eligibility around the cut-off date (see online supplementary material, Table S2 and Figure S1).

To estimate the effect of the ad hoc programme, we use a local linear regression with a triangular kernel of the form:

$$Y_{it} = \alpha + \beta_1 Z_i + \delta D_i + \beta_2 (D_i * Z_i) + \epsilon_{it}$$

where Y_{it} measures the employment of refugee i in a specific month t after arrival, Z_i is the running variable that measures the distance between the day of arrival and the eligibility cut-off date (31 December 2015), D_i is a treatment indicator coded one if the arrival was before the cut-off date and zero otherwise, and ϵ_{it} is the error term. In this regression, δ identifies the intention-to-treat (ITT) effect of programme eligibility on employment.

Note that participation in the ad hoc courses is not observed in our register data, and therefore our estimation focuses on ITT effects. However, using a secondary dataset, we can also approximate the proportion of compliers close to the cut-off date to examine how the eligibility cut-off affected programme uptake (see online supplementary material, Figure S2).

We run separate regressions to measure the impact on Y_{it} for months $t = 6, 7, \ldots, 22$ after arrival. Since the programme duration was about 8 weeks, we would expect effects to emerge at the earliest after this initial lock-in period (although refugees were, in principle, allowed to work even while completing the programme). For the main specification, we use the common Mean Squared Error (MSE)-optimal bandwidth based on an automated selection algorithm

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Table 1. Descriptive statistics for monthly panel data

	Ad hoc programme				Preexisting programme			
Variable	Mean	*SD*	Min	Max	Mean	*SD*	Min	Max
Age								
In years	25.20	4.91	18	35	25.15	4.92	18	35
18–20	0.22	0.41	0	1	0.22	0.42	0	1
21–25	0.33	0.47	0	1	0.33	0.47	0	1
26–30	0.27	0.44	0	1	0.27	0.44	0	1
31–35	0.18	0.38	0	1	0.18	0.38	0	1
Gender								
Female	0.25	0.43	0	1	0.23	0.42	0	1
Nationality								
Syrian	0.73	0.44	0	1	0.74	0.44	0	1
Iraq	0.16	0.37	0	1	0.15	0.36	0	1
Iran	0.06	0.24	0	1	0.05	0.22	0	1
Eritrea	0.05	0.22	0	1	0.05	0.23	0	1
Schooling								
No	0.26	0.44	0	1	0.25	0.43	0	1
Yes	0.59	0.49	0	1	0.60	0.49	0	1
(Missing)	0.16	0.36	0	1	0.15	0.36	0	1
Employment								
Has job	0.07	0.26	0	1	0.08	0.27	0	1
Has job (w/o minijob)	0.04	0.20	0	1	0.04	0.20	0	1
Has minijob	0.04	0.19	0	1	0.04	0.19	0	1
Others								
Running Variable	78.36	67.07	−152	212	95.18	51.55	0	212
Residency	12.86	7.91	0	30	13.14	8.03	0	30
Enrolled preexisting prog.	0.02	0.15	0	1	0.03	0.16	0	1

Note. The data for the ad hoc programme covers 210,369 persons aged 18–35 arriving from Syria, Eritrea, Iraq, or Iran ($N = 5{,}487{,}256$). The data for the preexisting programme are a random person sample from these data covering 50,000 persons $N = 1{,}335{,}073$ that are eligible for the ad hoc programme.

(Calonico et al., 2014) and a heteroskedasticity-robust plug-in residuals variance estimator without weights.

4.4.2 DID for preexisting programme

To identify the effects of the preexisting language programme, we leverage the panel dimension of our data, as there is no discontinuity in programme eligibility. Hence, we employ a DID design, comparing refugees who enroled in the preexisting programme at different points in time.

We leverage two heterogeneous treatment effect robust estimators to identify the average treatment effect on the treated (ATT), including the DID imputation estimator with interactive fixed effects proposed by Liu et al. (2024) (also see Borusyak et al., 2024) and the doubly robust DID estimator proposed by Callaway and Sant'Anna (2021). Both estimators address recent concerns regarding the traditional two-way fixed effects linear regression estimator, which does not converge to a convex combination of treatment effects in the presence of dynamic treatment effect heterogeneity (Callaway & Sant'Anna, 2021; De Chaisemartin & d'Haultfoeuille, 2020; Goodman-Bacon, 2021).

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Let $D_{i,t}$ be a treatment indicator coded 1 if refugee i has ever enroled in the preexisting language programme at month t, and zero otherwise. Once a refugee enrols, he or she is considered treated for the rest of the study period, i.e. $D_{i,t} = 1 \Rightarrow D_{i,t+1} = 1$ for $t = 1, 2, \ldots, T$ (staggered adoption). Let $Y_{i,t}(1)$ and $Y_{i,t}(0)$ denote the potential outcomes of refugee i under the treatment and control conditions.

The core idea of the DID imputation estimator is to estimate a flexible model for the counterfactual outcome $Y_{i,t}(0)$ in the control observations $D_{i,t} = 0$ and then use the fitted model to impute the missing potential outcomes $\widehat{Y}_{i,t}(0)$ for each treated observation $D_{i,t} = 1$. One can then compute for the treated observations the individual treatment effects as $\hat{\tau}_{i,t} = Y_{i,t}(1) - \widehat{Y}_{i,t}(0)$, the difference between the observed $Y_{i,t}(1)$ and the imputed counterfactual outcomes $\widehat{Y}_{i,t}(0)$, and then average these to obtain the ATTs for each period $s > 0$ following the treatment onset (Liu et al., 2024):

$$ATT(s) = E[\tau_{i,t} \mid D_{i,t-s} = 0, D_{i,t-s+1} = D_{i,t-s+2} = \ldots = D_{i,t} = 1, \ \forall\, i \in T]$$

This method avoids the nonconvex weighting problem because treated observations are never used as controls and allows for heterogeneous treatment effects because missing counterfactual outcomes are imputed for each treated observation. Note that the preexisting language programme runs for about six months and enrolment is typically full-time. Therefore, we expect treatment effects to materialize, if at all, after around six months.

To model the counterfactual outcome, we leverage the DID imputation estimator with interactive fixed effects proposed by Liu et al. (2024). In this approach, we assume a factor-augmented model for untreated potential outcomes given by

$$Y_{i,t}(0) = \alpha + \gamma_i + \xi_t + \lambda_i' f_t + \epsilon_{it}$$

for all i, t. In this model, γ_i and ξ_t are refugee and calendar month fixed effects that control for time-invariant refugee characteristics (such as education, employment experience acquired in the origin country, or time-invariant cognitive skills and personality traits) and common shocks that vary at the month level. The model also includes an interactive fixed effects component given by $\lambda_i' f_t$, where $f_t = [f_{t,1}, \ldots, f_{t,r}]$ is a $(r \times 1)$ vector of unobserved common factors and $\lambda_i = [\lambda_{i,1}, \ldots, \lambda_{i,r}]$ is a $(r \times 1)$ vector of unknown factor loadings (Bai, 2009).

The idea of the interactive fixed effects component is to capture time-varying unobserved confounders by allowing a set of refugee-specific fixed effects to interact with time-varying factors. For example, an unobserved common shock is allowed to have a heterogeneous impact on each refugee or alternatively, the effects of unobserved time-invariant refugee confounders can change over time. We allow for two factors ($r = 2$), such that the interactive fixed component is modelled by $\lambda_{i,1} f_{t,1} + \lambda_{i,2} f_{t,2}$. Following Bai (2009), we estimate this model using an iterative algorithm that applies a factor analysis to the residuals from a linear model and then reestimates the linear model while incorporating the influence from a fixed number of the most important factors. To obtain uncertainty estimates, we use a nonparametric block bootstrap clustered at the refugee level (Liu et al., 2024).

The key assumption in this approach is that the model for the counterfactual outcome is correctly specified under a strict exogeneity assumption that states that the error is independent of treatment assignment and the unobserved temporal and cross-sectional heterogeneities across all refugees and time periods. This implies a conditional parallel trends assumption conditional on the refugee, month, and interactive fixed effects.

As an alternative approach, we also use the doubly robust DID estimator proposed by Callaway and Sant'Anna (2021) (henceforward, CS DR estimator). Let $G_{i,g}$ be a cohort indicator, coded as 1 if refugee i is first treated at time g, and zero otherwise. Cohorts are defined as groups of refugees who enrol in the same month. Let C be an indicator for a never-treated group, coded as 1 for refugees who never enrol and zero otherwise. The idea of this approach is to identify cohort-time specific ATTs given by:

$$ATT(g, t) = E[Y_t(g) - Y_t(0) \mid G_g = 1]$$

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for $t \geq g$. As shown in Callaway and Sant'Anna (2021), this estimand is analogous to a doubly robust augmented inverse probability weighting estimand given by:

$$ATT_{DR}(g,t) = E\left\{\left(\frac{G_g}{E[G_g]} - \frac{\frac{p_{g,t}(X)(1-D_t)}{1-p_{g,t}(X)}}{E\left[\frac{p_{g,t}(X)(1-D_t)}{1-p_{g,t}(X)}\right]}\right)\left(Y_t - Y_{g-1} - \mu^0_{g,t}(X)\right)\right\}$$

where $\mu^0_{g,t}(X) = E[Y_t - Y_{g-1} \mid X, D_t = 0, G_g = 0]$ is the counterfactual based on the comparison group of never-treated units, and $p_{g,t}(X) = P(G_g = 1 \mid X, G_g + (1 - D_t)(1 - G_g))$ is the propensity score for treatment assignment. We summarize the cohort-specific effects by computing the weighted average treatment effect for groups of refugees who have been exposed to the treatment for a given number of months. We remove the first six calendar months since there are too few observations.

The key assumption for the doubly robust approach is that either the outcome model or the treatment assignment model is correctly specified. For the outcome model, this assumes the conditional parallel trends assumption, stating that the counterfactual evolution of the outcome in the treatment cohorts follows that of the comparison group to estimate the cohort-time average treatment effects. For the treatment assignment model, this assumes that we correctly model the conditional probability of a refugee being in a treatment cohort given their covariates (Callaway & Sant'Anna, 2021; Sant'Anna & Zhao, 2020).

5 Results: effects of the ad hoc language programme

Figure 3 shows the RDD estimates of the effect of the ad hoc programme on refugee employment. Figure 3a shows the average employment rates as a function of the distance between the arrival date and the programme eligibility cut-off of 31 December 2015. If participation in the ad hoc language programme led to increased employment, we would expect a higher rate of employment among those refugees who arrived right before the cut-off date and were therefore eligible for the ad hoc courses compared to those refugees who arrived right after that date and were therefore not eligible. However, we find that there is no drop in the average employment rates among those who arrived right after the cut-off date compared to those who arrived right before. This indicates that the ad hoc programme had no discernible positive effect on enhancing the labour market participation of refugees for up to 22 months after arrival. Note that this null finding is not due to low programme take-up among the eligible refugees. Indeed, as we show in the online supplementary material, the estimated programme participation rate drops sharply by about 21 percentage points at the cut-off (see online supplementary material, Figure S2).

Figure 3b shows the estimation results from the local linear regression, which identifies the employment effects of the ad hoc programme at the cut-off date, measured from six to up to 22 months after arrival. The estimated optimal bandwidth is on average 28 days around the cut-off date (the underlying estimates are also presented in online supplementary material, Table S3). Consistent with the graphs in Figure 3a, we find that the programme had no discernible positive effect on average employment rates. The point estimate for the effect 7 months after arrival is 0.03 with a narrow 95% confidence interval ranging from −0.34 to 0.40. For 12 months after arrival, the effect estimate is −0.00 with a 95% confidence interval of −0.75 to 0.75. For longer follow-up periods, the effects, if anything, turn more negative but also less precise. For month 17, the estimate is −0.92 with a 95% confidence interval from −2.17 to 0.33, and for month 22, the estimate is −2.01 with a confidence interval from −4.14 to 0.12. Note that for two of the months, 19 and 20, the effect estimates are negative and significant at the 95% confidence level, but the confidence intervals for those two months overlap with those of the null effect estimates for the other months, and this negative effect is also not robust across alternative specifications (see below). Given the totality of the evidence we interpret this as a null finding and clear evidence against a positive effect of the programme, but we refrain from interpreting this as a negative effect.

To gauge whether these null effects are due to a lack of statistical precision of our RDD, we conduct a series of inferiority tests (one-sided t-tests of the null hypothesis that an effect at least as

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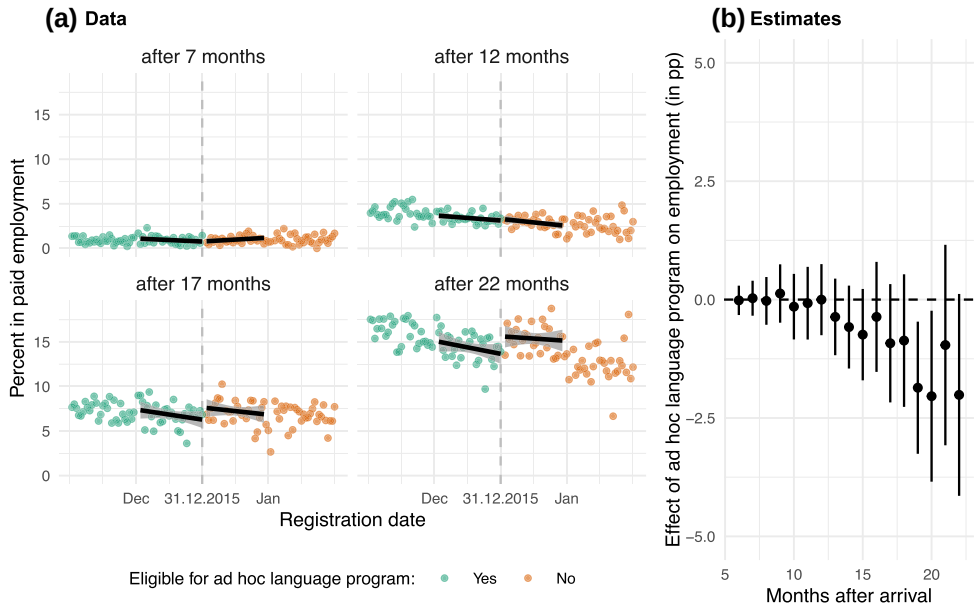

Figure 3. Estimates from regression discontinuity design show that eligibility for participation in the ad hoc language programme had no discernible effect on employment outcomes six to 22 months after arrival. Left panel (a) shows the average employment rates for various months after arrival for refugees conditional on their day of arrival. The dashed vertical line is the eligibility cut-off date (31 December 2015). Green dots are daily averages for refugees who arrived before the cut-off and were thus eligible for the ad hoc programme, orange dots are daily averages for refugees who arrived after the cut-off and were thus not eligible for the ad hoc programme. Black solid lines are the fitted local linear regressions using a symmetric 28 day bandwidth around the cut-off date. Right panel (b) shows point estimates and 95% confidence intervals from the local linear regressions with (Mean Squared Error)-optimal bandwidth that estimate the intention-to-treat effect of programme eligibility at the cut-off date.

large as a prespecified threshold can be rejected). For all months except 21 after arrival, we can reject effect sizes of 1.5 percentage points or larger with p-values <0.05. Together, these tests suggest that we can rule out all but very small benefits of the programme.

In the online supplementary material, we present results from various robustness checks that corroborate the main findings. First, we replicate the models to estimate the effects by employment type: all jobs, marginal employment, and jobs excluding marginal employment. We find no discernible effects across all employment types (online supplementary material, Figure S4). Second, we replicate the models but also adjust for covariates (age, nationality, schooling, and sex), and the estimates are similar, as expected given that they are balanced across the eligibility threshold (online supplementary material, Figure S5). Third, we check if the estimates are affected by attrition and replicate the main models while imputing a zero for all years post-arrival without an observed employment outcome (e.g. due to emigration out of Germany or death). The idea behind this imputation strategy is that any form of formal employment should be observed in our register data. We again find that the null effects are robust to this adjustment (online supplementary material, Figure S6). Fourth, we find that the null findings are not the result of a specific bandwidth or model specification (online supplementary material, Figure S7). Fifth, we find that the effects are robust when we use a (one-sided) doughnut RDD that excludes observations to the left of the threshold (online supplementary material, Figure S8). Sixth, we check whether the null findings might be driven by the fact that those who are ineligible for the ad hoc programme may subsequently enrol in the preexisting comprehensive training programme. To do so, we replicate the models while excluding all individuals that at some point during our study period (also) enrol in the preexisting programme. Again, we find no discernible effects for the ad hoc programme (online supplementary material, Figure S9). We also find no significant effect of the eligibility for the ad hoc programme on the probability of enroling in the preexisting programme (online supplementary material, Figure S10). Seventh, we estimate the results separately for men and

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women, and the results are similar, with slightly more variability for men (online supplementary material, Figure S11). Eighth, we estimate the ad hoc programme broken down by refugees' state of residence at arrival. We find that there is some heterogeneity, but the distribution of effects is centred on zero and the null effects are robust across the large majority of states (online supplementary material, Figure S12). Lastly, we replicate the models broken down by refugees' education levels; we find some limited heterogeneity but the distribution of estimates centred on zero (online supplementary material, Figure S13).

The results so far have shown that the ad hoc, large-scale language training programme rapidly established by the German government in response to the surge in refugees did not have any discernible impacts on the labour market integration of refugees on average. This raises two important questions: Are these null effects a consequence of specific features of this ad hoc programme? Or do these findings indicate more general and systemic limitations of language programmes' potential to foster economic integration during a large scale displacement crisis? To begin addressing these questions, we contrast the estimates from the ad hoc programme with the effects of the much smaller scale preexisting language programme during the same time period and in the same context.

6 Results: effects of preexisting language programme

Figure 4 displays the effect estimates for two estimators: the DID imputation estimator with interactive fixed effect (top panel) and the doubly robust CS DR DID estimator (bottom panel). The effects are shown for three outcomes: all jobs (left), all jobs but minijobs (middle), and minijobs (right). The underlying estimates are also presented in online supplementary material, Tables S4 and S5.

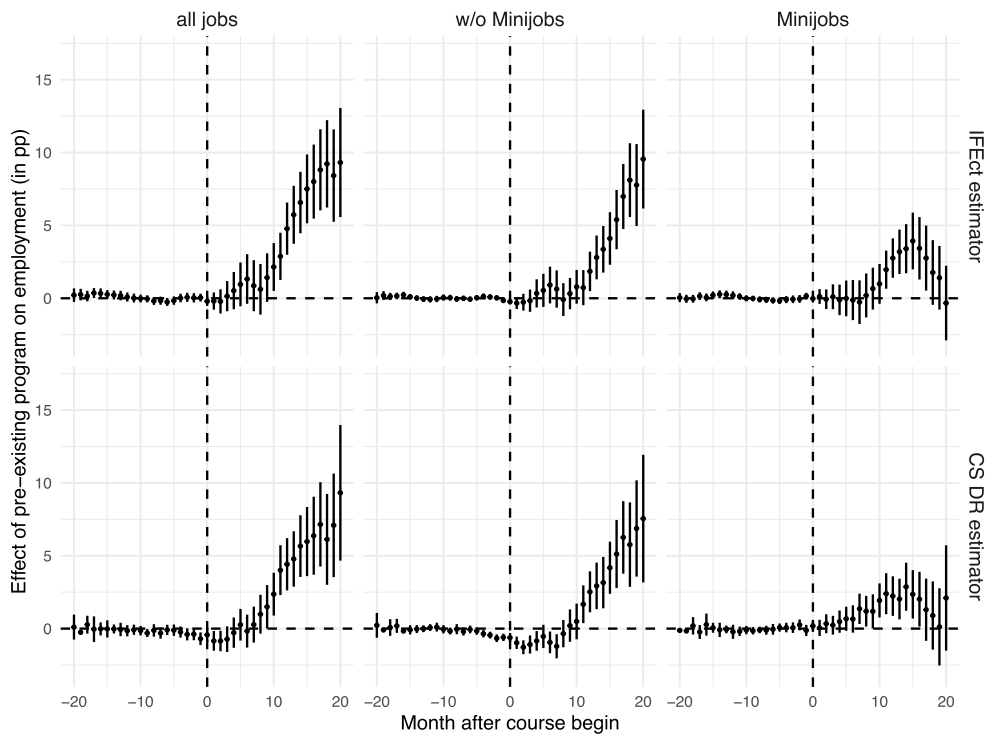

Figure 4. Top panel: Estimates of the effects of the preexisting language training programme from difference-in-difference (DID) imputation estimator with interactive fixed effects for three outcomes: all jobs (left), all jobs but minijobs (middle), minijobs (right). Bottom panel: Estimates from doubly robust CS DR DID estimator. Overall the estimates suggest that the preexisting language programme increased employment following the six months course duration.

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The results indicate that enroling in the preexisting language course significantly increased employment following the six-month course duration. This result holds across both estimators. For instance, at month 12 following enrolment in the course, the estimated effect from the imputation estimator on the 'all jobs' outcome is an increase of 4.8 percentage points with a standard error of 0.9. The corresponding estimate from the CS DR estimator is an increase of 4.4 percentage points with a standard error of 0.9. The results in the middle and left panels suggest that the increase in employment in later periods is not primarily driven by minijobs. While the point estimates are similar up until month 14 (imputation estimator) and 15 (CS DR estimator) for the minijob outcome and the all jobs but minijobs outcome, the effects for the all jobs but minijob outcome continues to grow while it declines for the minijob outcome.

Consistent with the parallel trends assumption, there are no large differences in employment rates in the months prior to enrolment for all three outcomes. In online supplementary material, Table S6, we present F-tests for three selected pretreatment periods (3 to 1 month before enrolment, 9 to 1 month, and 29 to 1 month) and all three employment outcomes. We observe one statistically significant difference in pretreatment estimates across the three employment outcomes but the substantive magnitude of this difference is small, as shown in Figure 4.

In the online supplementary material, we present several robustness checks that corroborate the main results. First, we reestimate the CS DR estimator using the not-yet and the never-treated units as controls, and the estimates are very similar (online supplementary material, Figure S14). Second, we examine whether the results are affected by panel attrition by reestimating the models while imputing any missing outcomes as zero, and the results are again similar (online supplementary material, Figure S15). Third, we reestimate the models separately for male and female refugees. The effects are roughly similar across both males and females, with the estimates for women being somewhat elevated in magnitude but with larger confidence intervals due to the reduced estimation sample size (online supplementary material, Figure S16).

7 Discussion

The results indicate that while the large-scale ad hoc programme had no discernible effects on employment, the smaller-scale preexisting programme led to considerable increases in employment. This latter finding refutes the hypothesis that language courses are generally ineffective in facilitating economic integration during times of large-scale displacement crises. However, the contrasting results raise a new question: Which programme features account for their differential effectiveness? Without randomizing the different features of the programmes, it is difficult to disentangle their individual contributions to the overall effect. Nonetheless, the two programmes can be distinguished along several dimensions that past research has shown to be important for effectiveness, including the duration of the course (Rolstad et al., 2005), the provision of skill certificates (Brücker et al., 2021; Desiderio, 2016; Pecoraro & Wanner, 2019; Tani, 2017), and the classroom composition (Sprietsma & Pfeil, 2015).

While the maximum class size was fairly similar between the two programmes, the preexisting programme was much longer, with 600 hr compared to only 320 hr in the ad hoc programme. In addition, the preexisting programme had a consistent curriculum, set standards for course providers, and provided successful course participants with a certificate they can show to potential employers. The ad hoc programme lacked these features (see online supplementary material, Table S1 for details).

Given these differences, it appears likely that the ad hoc programme lacked the quality and quantity of instruction necessary for participants to acquire sufficient German proficiency that would translate into more success in the labour market. An alternative interpretation is that the programme did lead to improved language skills, but the lack of certification meant that participants could not credibly signal those skills to potential employers. We hope that future research will be able to shed light on the relative impact of these and other programme features.

It is also important to recognize that selection of refugees into the two programmes may have contributed to the heterogeneous effects we find. When comparing the characteristics of the participants in the preexisting programme with the rest of the sample, we observe that the participants in the preexisting programme had a slightly higher share of Syrians and refugees with some education (online supplementary material, Table S5). To the extent that the courses had a greater

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effect on refugees who are positively selected, this could partly explain why the preexisting programme had more positive effects.

8 Conclusion

Our findings carry important implications for the design of language policies and programmes during periods of mass refugee displacement. Over the past decades, millions of displaced individuals have arrived in Europe and been granted refugee status and subsidiary protection. Mastering the host country's language is typically the first, and potentially most crucial, step toward successful integration into the host country's economy and society. While virtually all European host countries offer language programmes, it remains unclear how best to expand their capacity during crises and periods of high demand.

Our results suggest that the ambitious initiative by policymakers in Germany to establish a large-scale, ad hoc language programme meant that many refugees received basic language training shortly after arrival. However, ultimately, this programme proved ineffective in improving their employment prospects. To investigate whether these null effects stem from particular features of the ad hoc programme or arise from the general challenge of delivering effective language programmes during refugee crises, we contrast our analysis with a preexisting, more comprehensive language course. We find that this programme nearly doubled refugees' chances of employment. This indicates that more comprehensive programmes can successfully facilitate labour market integration even during periods of large refugee arrivals and associated pressures on the asylum system and host communities.

Our findings regarding the divergent effects of the two programmes highlight a significant trade-off in designing an effective response to refugee crises. While the preexisting programme yielded high returns, it served far fewer refugees, whereas the ad hoc programme reached more refugees but failed to generate discernible employment benefits. Ultimately, the null finding from the ad hoc programme suggests that it prioritized quantity over quality in an effort to swiftly assist the maximum number of individuals. It appears that a more promising approach would involve investing in scaling up comprehensive programmes that provide at least some employment benefits to participants, even if it means not everyone can be served immediately. Determining the optimal level of comprehensiveness for such investments remains an open and crucial question for future research.

Last but not least, it is important to recognize two limitations of the study. First, since there was no variation in the participation in the different components of the two programmes, we could only evaluate the effects of the programmes as they were rolled out in reality. While this evidence is of first-order policy importance, it does not quantify the relative returns to the different components of each programme (e.g. course duration, certification, quality of instruction). Future research on this question would be important to help design optimal programmes.

Second, given our data limitations, we could only examine effects on employment outcomes. It is well understood that immigrant integration is a multidimensional concept that goes beyond economic success. To better inform policy debates, future research should examine the impacts on other important dimensions of refugee integration, such as linguistic, psychological, political, or social integration (Harder et al., 2018).

Acknowledgments

The authors thank Joelle Pianzola and Jonathan Homola for helpful comments on earlier versions of this draft. We also thank the participant at the IAB-Colloquium in Nüremberg.

Conflicts of interest: None declared.

Funding

D.H. acknowledges funding from the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation programme (Grant No. 804307), the Swiss Network for International Studies, and the Leverhulme Trust. J.H. and D.H. acknowledge funding from the U.S. NSF (grant no. 1627339). The funders had no role in the design, data collection,

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analysis, decision to publish, or preparation of the manuscript. Any opinions, findings, and conclusions or recommendations expressed in this material are those of the authors and do not necessarily reflect the views of the funders.

Data availability

Our analysis builds on two datasets: the Integrated Employment Biographies (IEB V14.00.00-190927) and Status History Migration (SHZ V03.03.00-201904). The data are provided by the Institute for Employment Research (IAB), Regensburger Str. 104, 90478 Nuremberg, Germany, homepage: www.iab.de, email: iab@iab.de. The data source is social security data with administrative origin, which are processed and kept by IAB according to Social Code Book III. Due to sensitive information, the access to this data is subject to the confidentiality regulations according to §35 of the Germany Social Code Book I and requires an application granted by the Germany Federal Ministry of Labor and Social Affairs. Therefore, for replication purposes these data can only be accessed on-site at the IAB upon application and approval. For further information regarding the data and replication purposes please contact the authors. Replication code will be made available at the Harvard Dataverse: https://doi.org/10.7910/DVN/ICCFHJ.

Author contributions

M.M., E.V., N.H., J.H., and D.H. designed and performed research; M.M. and E.V. analysed data; J.H., M.M., and D.H. wrote the paper with input from E.V. and N.H.

Supplementary material

Supplementary material is available online at *Journal of the Royal Statistical Society: Series A*.

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