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Opportunities or Benefits: Local Conditions and Refugee Labor Market Integration

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Opportunities or Benefits: Local Conditions and Refugee Labor Market Integration

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Abstract

This paper studies how local labor market tightness and welfare benefit levels at the time of receiving protection shape refugees' labor market integration and location choices. We exploit a sequence of reforms that generated quasi-random variation in benefit levels, together with variation in local labor market tightness across the districts and months in which refugees receive protection. We find that tighter local labor markets raise employment, while more generous benefits slightly reduce it. These employment effects persist for about 2.5 years, but both shocks have lasting impacts on internal migration. Moreover, the negative employment effect of benefits is larger in tighter labor markets. Neither shock affects the probability of emigrating from Austria.

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1 Introduction

The economic and social integration of refugees remains a central policy challenge across advanced economies (Brell et al., 2020). Governments implement a range of policies not only to foster integration but also to shape migration flows, manage fiscal resources, and sustain public support for hosting refugees.¹ Some policies restrict refugees’ choices—such as dispersal rules that allocate them across regions—while others provide incentives, for instance by changing benefit generosity to foster labor market participation. Yet outcomes also depend on factors beyond policymakers’ control, such as the absorptive capacity of local labor markets. How refugees respond to the interplay of policy incentives and labor market conditions is therefore critical for designing effective integration strategies (Adda et al., 2022).

We study how local labor market tightness and welfare benefit levels—individually and in interaction—shape refugees’ labor supply and relocation decisions once restrictions on mobility and employment are lifted. Austria provides a particularly well-suited setting for this analysis. During the asylum process, refugees are subject to a dispersal policy and face strict limits on employment and relocation. These restrictions are lifted immediately upon recognition, at which point refugees gain full access to the labor market and freedom of movement across states. Three features make Austria distinctive. First, the sharp shift from restricted to unrestricted choice allows us to combine quasi-experimental variation in initial placement with observation of behavior under full mobility. Second, because most refugees must vacate their assigned accommodation within four months, they are forced to make consequential labor market and residential decisions in a compressed time frame, resembling the incentives embedded in work-first policies (Arendt, 2022; Arendt and Bolvig, 2023). Third, Austria’s federal welfare system generates large and time-varying cross-state differences in benefit generosity, enabling us to test whether refugees respond to financial incentives by relocating to more generous states.²

These features make Austria a good place to observe the underlying mechanisms, but they do not make the mechanisms Austrian. Refugees in any host country face the same two decisions once protection is granted: whether to take the work that is locally available, and whether to stay where they were placed. How they resolve that trade-off depends on the rate at which job offers arrive and on the income available while not working. What differs across countries is which of the two margins is open. Where recognized refugees remain tied to an assigned region for several years, as in Germany, the location margin is largely closed and refugees facing

¹We use the term refugee for an individual whose asylum application was approved (Hatton, 2020). We use the term asylum-seeker specifically for a person before being granted protection.

²For example, in 2015, a single refugee with subsidiary protection in Carinthia received only €200 in basic subsistence support, compared to more than three times that amount in Tyrol or Vienna.

weak local labor demand cannot substitute towards moving.³ Where benefits are minimal, the reservation-wage channel has little room to operate and the response to labor demand stands alone. Because Austria leaves both margins open, we can observe them jointly and separate them, which is also what allows our estimates to inform settings in which only one is available.

We find that many refugees relocate immediately once restrictions are lifted: roughly 30 percent of those initially placed outside Vienna move to the capital within four months, despite Vienna having a relatively weak labor market. Both local labor market tightness and welfare generosity at the assigned location shape this decision. Tighter labor markets—especially in low-barrier occupations—induce earlier and higher labor force participation among men, which in turn increases the probability of remaining in the assigned state. While these employment effects are temporary, location choices are persistently affected.

Benefit generosity also influences outcomes, though more modestly. A decrease in monthly benefits increases the propensity to relocate, raises male employment temporarily, and raises female employment more persistently. Importantly, these effects interact with labor market conditions: benefit reductions significantly increase employment only when local labor markets are tight, consistent with recent evidence from Denmark (Dustmann et al., 2024). Taken together, our results suggest that mobility responses are shaped by where refugees first resolve their immediate constraints rather than by longer-term labor market prospects. Consequently, the combination of dispersal policies with subsequent unrestricted mobility does not contribute to *greasing the wheels* of the labor market (Borjas, 2001).

Our contribution is threefold. First, identifying both effects jointly shows that the employment and location responses are two sides of one decision: where labor demand is weak, refugees do not stay and remain unemployed but relocate, predominantly to Vienna, which itself has weak labor demand. Benefit generosity moves the same margin but only weakly, which is evidence on the welfare magnet hypothesis. Second, we can distinguish responses to immediate opportunities from responses to expected long-run prospects. Demand in seasonal occupations, which are temporary arrangements rather than durable career tracks, raises employment and persistently raises the probability of staying almost as much as demand in non-seasonal occupations. Resolving the immediate constraint of finding work and housing therefore anchors refugees in their initial location, even when the job that resolved it does not last. Third, the negative cross-partial of tightness and benefit generosity is a testable implication of a standard

³Section 12a of the German Residence Act, introduced with the Integration Act of 2016, ties recognized refugees to their assigned state for three years unless they hold employment covering their living costs. See <https://www.bamf.de/SharedDocs/ProjekteReportagen/EN/Forschung/Integration/evaluation-wohnsitzregelung.html>, retrieved July 29, 2026. Brücker et al. (2020) find that refugees subject to such a requirement are less likely to be employed.

search model, in which higher tightness raises the offer arrival rate so that the reservation-wage effect of more generous benefits binds more tightly. Finding it for refugees, as documented for unemployment insurance among natives (Schmieder and von Wachter, 2016) and for refugees in Denmark (Dustmann et al., 2024), points to a common channel rather than to mechanisms specific to this population.

We now turn to a more detailed description of our setting, data, methodology, and results. We draw on the Austrian asylum system, which combines quasi-random assignment of asylum-seekers across regions with sharp changes in mobility and labor market access once protection is granted. This institutional structure creates exogenous variation in the local conditions refugees face at the time of labor market entry. Welfare benefits, set at the federal-state level, varied substantially across states, over time, and by protection status. Between 2015 and 2017, several states implemented large benefit cuts, some of which were later overturned by courts, further increasing variation in generosity.

Our analysis uses administrative data covering more than 40,000 refugees who arrived between 2011 and 2018, providing complete information on employment trajectories, social security contributions, and place of residence. Unlike related studies (Ferwerda et al., 2024; Dustmann et al., 2024; Azlor et al., 2020), we can exploit both spatial and temporal variation in local labor market tightness and welfare benefit levels, which allows us to separate their effects and better account for regional confounders.

The data also contain detailed records on job openings, which enable us to measure both the overall tightness of the local labor market and the demand for specific occupations. These features of the Austrian setting allow us to examine how refugees respond to both labor market opportunities and financial incentives at the time their choice set expands.

An interquartile-range increase in local labor market tightness—about 0.2 additional vacancies per unemployed person—raises the probability of employment twelve months after recognition by 4.2 pp. (percentage points) and reduces welfare use. The effect fades after roughly 30 months. Labor market tightness also increases the probability of remaining in the assigned state and decreases relocation to Vienna, the primary destination for movers. By contrast, benefit reductions have modest effects: a €100 decrease increases employment after one year by 0.5 pp. and monthly earnings by about €10, while decreasing the probability of remaining in the assigned state by one percentage point. These employment effects dissipate after 2.5 years, but location effects persist. Neither labor market tightness nor benefits affect the probability of leaving Austria altogether. Finally, consistent with Dustmann et al. (2024), benefit reductions raise employment and reduce welfare take-up only when local labor markets are tight.

We contribute to several strands of literature that are usually studied in isolation but are nonetheless closely interlinked.

Location choice of immigrants. The question of how immigrants choose a location has received extensive attention in the literature, but several issues remain contentious, such as the extent to which differences in welfare benefits and labor demand influence this decision. An empirical regularity observed in many contexts is that immigrants tend to cluster in large cities (Monras, 2023). Our context is no exception, with widespread relocation to Vienna as soon as this becomes possible. This phenomenon has been explained by higher place utility offered by cities, for example, through the existence of ethnic enclaves or amenities such as educational institutions (Damm and Rosholm, 2010). Another potential explanation is that migrants choose high-cost–high-income cities, as they consume a smaller share of their income locally and thus benefit disproportionately from higher wages (Albert and Monras, 2022). The labor market is unlikely to play an important role as a pull factor in our context. Vienna is one of the locations with the lowest labor demand, especially for low-skilled jobs; average wages for blue-collar workers are below the Austrian average (€30,509 vs. €35,564);⁴ and refugees in Vienna have some of the lowest employment rates in the country. As the labor market does not seem to draw refugees to Vienna, other explanations—such as networks, amenities, and benefit generosity—are likely to play a more important role.

Given the strong concentration of refugees in a single destination and the absence of significant benefit changes in Vienna, we do not seek to identify which factors attract them to particular locations (pull) but rather which characteristics of their assigned locations push them away. Such push models have been used to study immigrants’ location preferences, but for causal identification they require exogenous placement into initial locations (Damm, 2009), which is the case in our setting.

Borjas (2001) argues that migrants, who are less attached to a particular location and thus face lower moving costs, contribute to *greasing the wheels* of the economy by avoiding areas where their skills are not in demand. That migrants react to local conditions by leaving or staying is part of this process. Our results suggest that refugees do respond to the availability of jobs in their initial regions. While our causal design identifies only these push factors, the descriptive concentration of movers in Vienna—despite its relatively weak aggregate labor market—suggests that regional labor market aggregates are not the dominant pull factor for subsequent destination choices.

We also contribute to the question of whether the generosity of welfare benefits affects

⁴https://www.statistik.at/fileadmin/publications/Allgemeiner_Einkommensbericht_2020.pdf

immigrants’ location choices—known as the *welfare magnet hypothesis* (Borjas, 1999). Evidence on the hypothesis is mixed. Agersnap et al. (2020) study the same Danish reform analyzed in Dustmann et al. (2024) and show that the benefit reduction lowered migrant inflows with an implied elasticity of 1.3.⁵ At the subnational level, Ferwerda et al. (2024) study Switzerland and examine how the generosity of benefits in refugees’ initial placement locations affects subsequent relocation. This push approach is very similar to ours. They find that higher benefits reduce relocation, but only to a minor extent—at most two pp., even for very large differences in benefits. Our estimates of roughly one pp. for a €100 change in benefits are considerably larger. Exploiting some of the same reforms that we analyze, Huber and Dellinger (2022) find in an event-study design that reductions in benefit levels trigger significant outmigration.

Effect of initial labor market conditions. Our findings on the effect of labor market conditions at labor force entry complement an active literature on the (long-term) effects of initial conditions for refugees (Åslund and Rooth, 2007; Godøy, 2017; Fasani et al., 2022; Aksoy et al., 2023) and immigrants more generally (Chiswick et al., 1997; Chiswick and Miller, 2002; Barsbai et al., 2025).⁶ Our detailed administrative data on vacancies and unemployment allows for several refinements compared to previous work. First, we measure conditions in the district of residence at the time of receiving unrestricted labor market access, rather than at arrival, and thus better capture the relevant moment.⁷ Second, we use the initial vacancy-to-unemployment ratio (*IVUR*) instead of employment or unemployment ratios because it might be a better measure of local labor market conditions (Dustmann et al., 2024). In particular, the measure provides information not only on the general state of the labor market but also on labor demand for particular skills and sectors. Third, variation over time and space allows us to move beyond cross-sectional comparisons and to control for unobserved regional characteristics that could be correlated with *IVUR* and confound the results. Fourth, we can also look at the effects on a monthly level, finely capturing any dynamics playing out immediately after getting access to the labor market. Two concurrent papers (Degenhardt, 2025; Degenhardt and Nimczik, 2025) exploit the same Austrian dispersal setting to study single labor-market segments (hospitality and gig work, respectively); we compare their findings with ours in Section 5.7 alongside our

⁵Adema (2025) discusses several methodological concerns with this study and argues that the elasticity is about 0.28 for new arrivals.

⁶von Wachter (2020) provides a summary of the findings of graduating during different points of the business cycle on life outcomes: On average, entering the labor market in a recession reduces earnings, and these effects persist for up to 15 years, with large heterogeneities by socio-demographics (e.g., education, race, sex). For Austria, Brunner and Kuhn (2014) find that for labor market entrants during 1978-2000, a one pp. higher initial local unemployment rate decreases wages by about 1.3% over the life cycle and persists for 20 years after labor market entry.

⁷Åslund and Rooth (2007) discuss which is the relevant initial labor market area since refugees do not immediately enter the labor market.

own sectoral decompositions. Relative to the majority of findings in this literature, we find that the effects of initial labor market conditions on employment are not persistent beyond 2.5 years. This likely reflects the fact that labor supply and location decisions are made simultaneously. Conditions immediately after recognition matter for finding both employment and housing.⁸ Several factors might explain our short-lived effects. First, tight labor markets at the time of receiving protection may channel refugees into low-barrier jobs that do not serve as stepping stones: Degenhardt (2025) and Degenhardt and Nimczik (2025) show that early access to hospitality and gig work in Austria raises short-run employment but does not improve longer-term outcomes and may hinder investments in host-country-specific human capital (Battisti et al., 2022). Second, refugees in Austria are particularly mobile, and the importance of initial local conditions fades over time. We also study how initial labor market tightness affects mobility and find persistent effects on the probability of staying in the assigned location.

Effect of welfare generosity. Our work also contributes to a small literature on the link between welfare generosity and labor market integration of immigrants (Bratsberg et al., 2020). In the most closely related paper, Dustmann et al. (2024) study the effect of refugee benefit cuts in Denmark in an RD setting. The initial positive employment effects disappeared after 4–5 years, and the effects were larger in regions with more favorable labor demand conditions. Like Dustmann et al. (2024), we identify the interaction between these two factors. Our findings corroborate the conclusion of Dustmann et al. (2024) as we also see that changes in benefit levels have larger effects on employment rates and welfare use when labor markets are tight.

More broadly, we examine how local conditions and policies interact. Although several studies have recognized the need for this approach (e.g., Fasani et al., 2022; Müller et al., 2023), few have incorporated both elements. Recent work has examined the combined role of initial labor market conditions and attitudes toward immigrants (Müller et al., 2023; Aksoy et al., 2023; Schilling and Stillman, 2024), but aside from Dustmann et al. (2024), none have focused on the interaction with welfare policies.

The rest of the paper is organized as follows. Section 2 provides information on the Austrian asylum system, the dispersal policy, and the welfare benefits for refugees. Section 3 describes the data, sample, and key variables. Section 4 describes the empirical strategy. Section 5 presents the results, Section 6 discusses policy implications, and Section 7 concludes.

⁸The only other study we are aware of to analyze the effects on mobility is Barsbai et al. (2025), which finds that family migrants in the U.S. do not move in response to adverse economic conditions. However, unlike refugees, family migrants have strong personal links to the location of initial residence, making any moves more costly.

2 Institutional Background

Application for international protection

Refugees are allowed to apply for asylum as soon as they arrive in Austria. Upon application submission, authorities clarify whether Austria is responsible for handling the asylum application, as per the Dublin III Regulation. During this period, which usually takes days or a few weeks, asylum-seekers are hosted in initial reception facilities.

If Austria is found to be responsible for processing the application, applicants receive factual deportation protection. This means they are allowed to remain in Austria until a decision on their application is made. In such cases, the Federal Office for Immigration and Asylum (BFA) processes all first-instance asylum applications. The BFA aims to decide on each asylum application within six months, although this time frame was often exceeded during the peak periods of the 2015 refugee crisis.⁹

Dispersal policy and labor market access

During the asylum process, asylum-seekers are allocated to various Austrian states according to a state quota. The Federal Agency for Reception and Support Services (BBU) sets these quotas in proportion to each state's population. States below their quota are contacted first to receive new asylum-seekers. Since the states rely on municipalities to provide accommodation, they can reject requests if suitable accommodation is unavailable. While municipalities cannot discriminate against individuals, they can express preferences based on personal attributes like family status or gender.¹⁰ For instance, smaller municipalities with only a single facility to accommodate refugees might impose restrictions due to the availability of only single rooms or the absence of childcare facilities.

Once assigned to a state and municipality, asylum-seekers receive basic subsistence support (*Grundversorgung*), which covers essential living expenses. This support is tied to the state of initial assignment, effectively preventing asylum-seekers from moving to another state until they receive international protection. The basic subsistence support guarantees housing provisions. Refugees can opt for private housing within the state without forfeiting their basic subsistence support. However, the financial aid provided in such scenarios is minimal. This limited financial assistance makes relocation within a state very unlikely, and refugees mostly

⁹In our sample, asylum-seekers waited, on average, 13 months between arrival and a decision on their application.

¹⁰Although this is not formally stated by law, we have this information from interviews with multiple experts. They mentioned that municipalities are just asked to accept a particular group of people. However, they do not know exactly whom they are going to host.

remain in the assigned municipality.

Access to the Austrian labor market for asylum-seekers is highly restrictive. The “Bartenstein-Erlass”, a decree issued by the Federal Ministry of Labour in 2004, has limited labor market access for asylum-seekers to seasonal positions in the agricultural and tourism sectors, with very few other exceptions. This limitation is evident in the employment rates and monthly wage incomes, which are close to zero before individuals receive a protection status. Figure 3 (a) shows that employment rates at the moment of receiving a protection status are effectively zero.

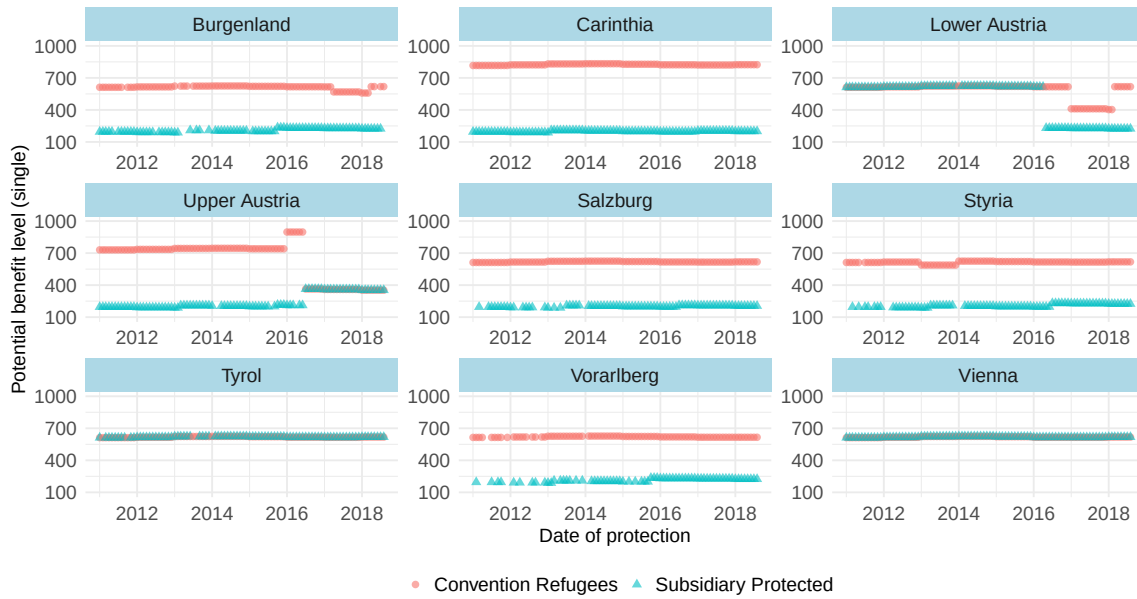
Once refugees receive protection as Convention refugees or subsidiary-protected individuals, they gain full access to the labor market. At the same time, both types of protection grant individuals the freedom to choose their place of residency within Austria. Figure 3 (b) shows that within four months of receiving protection, about 30% of refugees relocate to a different state.

Welfare system

The type and generosity of benefits refugees receive depend on multiple variables. Convention refugees are entitled to minimum income support (*Bedarfsorientierte Mindestsicherung*, BMS) from the moment protection is granted, while basic subsistence support (*Grundversorgung*) continues to cover them for a transitional period of up to four months. By contrast, those with subsidiary protection in many states remain in the less generous basic subsistence support (*Grundversorgung*). In this decentralized system, federal states have significant leeway over benefit levels for refugees. Several states implemented reforms that drastically altered benefits. We hand-coded data on the welfare system in place in each state for our observation period. Appendix B.2 provides an overview of the welfare system for refugees and the reforms happening during the observation period. Figure 1 shows the amount of monthly benefits for a single refugee in the various states. A corresponding graph for families of four is shown in Figure A.1 in the Appendix. These benefits cover the cost of living and exclude housing support, which is organized differently across states. In several states, reforms drastically changed benefit levels. While benefit levels in Tyrol and Vienna did not vary by type of protection or over time during our period of study, Burgenland, Lower Austria, and Upper Austria experienced various changes from 2016 onwards.

We use these data to identify the level of welfare benefits a refugee could receive in the state and at the time of protection. We term this variable *IPBL* - Initial Potential Benefit Level.

Figure 1: Welfare benefit levels for refugees (singles) by federal state

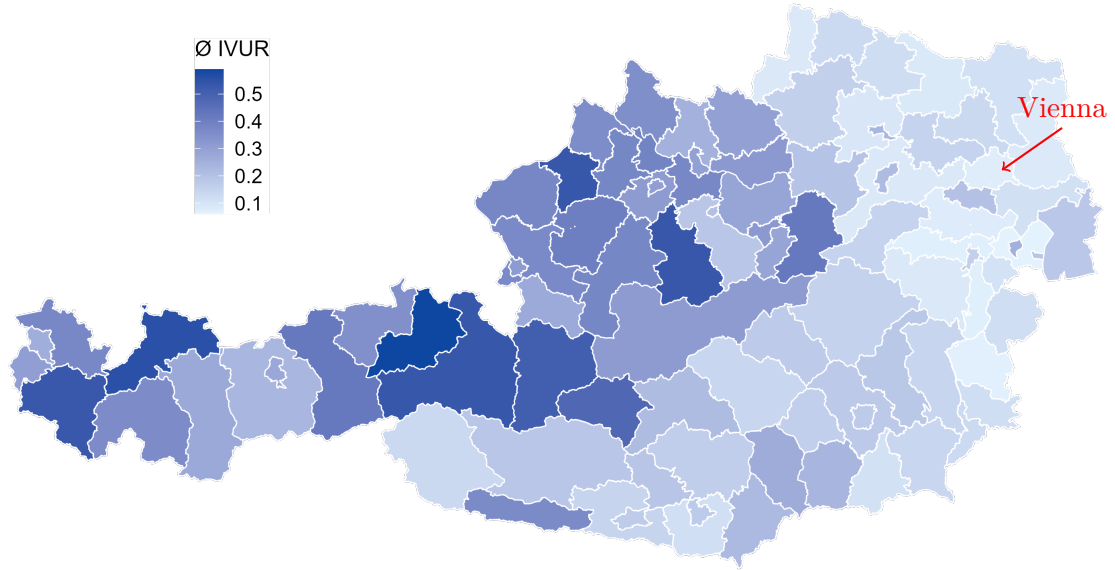


Notes: The figure shows the amount of monthly benefits in euros (excluding housing benefits) for singles by federal state and type of protection.

Local economic conditions

In addition to cross-state differences in benefit generosity, we exploit variation in the initial vacancy-to-unemployment ratio ($IVUR$) across districts over time. Figure 2 shows the average $IVUR$ during our period of analysis (2011–2018), providing a snapshot of the regional variation used in our analysis. The graph shows that Vienna and districts in the eastern and southern states of the country display low average labor demand during this period. These patterns are consistent with other labor market statistics at the state-level. For instance, Vienna exhibited the highest unemployment rate (8.4%) in 2022 in Austria (OECD, 2022).

Figure 2: Mean Vacancy-to-Unemployment Ratio by District



Notes: The map shows the mean vacancy-to-unemployment ratio (*IVUR*) by district, averaged across the period 2011–2018. Vienna is displayed as a single district.

3 Data and Descriptive Statistics

3.1 Data sources and sample

We use administrative data from the Austrian Social Security Database (ASSD), which covers all social insurance episodes and provides information on demographics, employment sectors, labor income, and spells of employment and unemployment. We access these records through the Austrian Labor Market Database (*Arbeitsmarktdatenbank*, AMDB), which is operated by the Austrian Public Employment Service (PES) together with the Federal Ministry of Labour. We augment the ASSD with variables provided by the Federal Ministry of Labour, including protection type, asylum date, participation in educational programs, language proficiency, marital status, residency ZIP codes, basic income support, education level, previous and desired professions. These supplementary data cover all individuals granted protection status and registered with the PES at any point in time. Although registration at the PES is voluntary, access to the social benefits linked to registration encourages it. In Appendix B.1, we test whether the number of refugees in our data, as a share of all individuals from those countries of origin in a district, correlates with our main explanatory variables. We find a slight positive association between initial labor market tightness and the probability of appearing in our sample. However, this association is quantitatively small and concentrated primarily among women, suggesting that any resulting overall selection bias is minor.

Asylum-seekers are granted special health insurance upon arrival, designated as “O4” or

“OE” in the ASSD, signaling coverage under basic subsistence support. This unique classification enables the precise identification of asylum-seekers, distinguishing them from recipients of other insurance types. It also allows us to see whether subsidiary-protected individuals still receive this form of welfare benefits after being granted protection.

Our analysis encompasses all refugees granted protection under the Geneva Convention or subsidiary protection between January 2011 and August 2018.¹¹ We focus on individuals aged 18 to 60 at the time of immigration, excluding those who were granted protection status on the day of arrival, indicative of family reunification. We further exclude refugees for whom calculating treatment variables was impractical,¹² and individuals receiving protection more than three years after arrival.¹³

Our final sample consists of 41,212 refugees, among whom 39,694 maintained active insurance coverage for a minimum of five years post-protection. In Section A.2 of the Appendix, we examine the impact of our treatment variables on the probability of continuous insurance coverage over this five-year span. Finding no significant influence, we consequently limit our further analysis exclusively to those individuals who can be observed until the end of the five-year period.

Additionally, we enhance our dataset with detailed municipality characteristics from Statistics Austria and utilize the ASSD to ascertain the number of vacancies and unemployed individuals at the district level.

3.2 Treatment variables

Initial vacancy-to-unemployment ratio (*IVUR*). We measure labor market tightness over the month of protection and the two following months through the initial vacancy-to-unemployment ratio (*IVUR*), utilizing aggregated monthly data on registered unemployment and vacancies at the district level from the ASSD. The *IVUR* is defined as follows:

$$IVUR_{d^0(i), m^0(i)} = \frac{\sum_{k=0}^2 \text{open positions}_{d^0(i), m^0(i)+k}}{\sum_{k=0}^2 \text{unemployed individuals}_{d^0(i), m^0(i)+k}} \quad (1)$$

This ratio is calculated by dividing the sum of open positions by the sum of unemployed individuals within a regional entity over this window. We use a three-month window because most refugees must vacate their assigned accommodation within four months of receiving pro-

¹¹The selection of 2011 as the starting point is due to improved data quality from this year onwards. The cutoff in August 2018 allows for a five-year observation period following the granting of protection status.

¹²In a few districts, the *IVUR* could not be computed for certain observations during the early years of our observation period due to official zeroes in the unemployment count.

¹³We conduct robustness exercises with different sample restrictions in Section 5.8.

tection, so this is the period in which the initial labor market can shape both job search and the decision whether to stay. $m^0(i)$ refers to the month in which individual i receives protection, and $d^0(i)$ to the district of residence at that time. We choose political districts as they roughly align with labor markets.

Initial Potential Benefit Level (*IPBL*). Using the information on benefit levels introduced in Section 2, we define the Initial Potential Benefit Level (*IPBL*) as follows:

$$IPBL_{p(i),f(i),s^0(i),m^0(i)} = \frac{PB_{p(i),f(i),s^0(i),m^0(i)}}{1000} \quad (2)$$

Here PB denotes the potential benefit level in euros per month, that is, the sum of cost-of-living support for which a refugee with no other income or wealth is eligible. It depends on the type of protection $p(i)$, family status $f(i)$,¹⁴ the state of residence at the time of protection $s^0(i)$, and the month of protection $m^0(i)$.¹⁵ For Convention refugees, PB is the minimum income support to which they are entitled from the month of protection; for subsidiary-protected refugees it is basic subsistence support, except in Tyrol and Vienna where they are topped up to the minimum income standard. PB covers cost-of-living support only. Housing support is not comparable across benefit systems, states, and accommodation types and additionally depends on local housing policy, so restricting the measure to cost-of-living support makes it comparable across all cells (Appendix B.2). We present *IPBL* in units of 1,000 euro to facilitate a clearer graphical presentation of our findings. Consequently, *IPBL* varies across federal states, protection statuses, family statuses, and over time, reflecting a sequence of policy reforms. A caveat is that our measure of benefit eligibility is imperfect, not covering other forms of benefits and only approximating the benefits for families. However, variation induced by the reforms should be captured very well. A second caveat is that *IPBL* measures *potential* rather than *received* benefits. Refugees in private accommodation are directly affected by the cash component captured by *IPBL*, while refugees in organized accommodation receive most support in-kind and are only indirectly exposed—through the changed value of moving to private accommodation. As we do not observe accommodation type at the individual level, the *IPBL* coefficients in Section 5 average over both groups. We return to the implications for interpretation in Section 6.

Importantly, both *IVUR* and *IPBL* are assigned to each individual upon the granting of

¹⁴We determine family benefit eligibility if a person’s child is insured in Austria or if there is a partnership status with another asylum-seeker and at least one dependent child co-insured in Austria at the time of protection receipt. This is different from our family arrival variable used to define the arrival groups in our identification strategy, where we consider asylum-seekers who arrive in Austria on the same date.

¹⁵For notational parsimony, the subscript omits the date of arrival in Austria. The 2016 Upper Austrian reform applies only to refugees who arrived after November 15, 2015, and our construction of PB uses the exact arrival date recorded in our data to assign benefit levels accordingly (see Appendix B.2.3).

protection and remain unchanged thereafter. These indicators reflect the labor market tightness and the welfare benefits system that an asylum-seeker is subject to at the time of initial authorization to enter the labor market and to move within Austria. Later changes in these variables due to relocation, amendments to benefit rules, or economic conditions are not considered as they are no longer assigned exogenously.

3.3 Outcomes

We examine three categories of outcomes at the individual level: labor market integration, mobility, and social support. We express each metric on a monthly basis, with time standardized relative to the month of protection receipt. Labor market performance encompasses a) an indicator for being employed¹⁶, b) being in marginal employment¹⁷, c) monthly real labor earnings, and d) logged real monthly wage income¹⁸. For mobility, we track whether a refugee remains in the same state as when protection was granted and whether they reside in Vienna, specifically for those initially located in a different federal state. The mobility measure is based on the contact address available to the Social Security Administration. Social support metrics include a) receipt of any Public Employment Service (PES) support within a month, such as language courses, job training, or counseling, and b) receipt of any welfare benefits, and separated by basic needs support and basic subsistence support sources. Additionally, an active insurance spell serves as a proxy for a refugee’s presence in Austria.¹⁹

3.4 Descriptives

Table 1 presents the summary statistics for our analysis sample, which consists of 39,694 individuals. Among these, 31,597 are Convention refugees, and 8,097 have subsidiary protection status. Two-thirds of refugees (64.7%) are male, with an average age of 30.49 years at the time of immigration. More than half of the sample, 54.6%, originates from Syria, followed by significant populations from Afghanistan (18.5%), Iraq (7.8%), and Iran (7.3%). A substantial portion, 59.6%, exhibits limited German language proficiency (A1/A2 level), and 69.2% possess only primary education or lack it entirely. Noteworthy disparities exist between Convention

¹⁶Employment is defined as any employment spell subject to social insurance contributions.

¹⁷Marginal employments are employment spells with earnings below the marginal earnings threshold (€406 in 2015).

¹⁸The earnings used to calculate real labor earnings and logged real monthly wage income are reported as pre-insurance, pre-tax gross income for each employer-employee relationship. For employment spells before 2019, earnings were reported annually, while after 2019, they have been reported on a monthly basis. We calculated average daily earnings for each employment contract and summed them up in cases of multiple simultaneous spells.

¹⁹Our focus on individuals who received protection makes it rather unlikely that individuals switch to an informal status and remain in Austria, which would be an alternative explanation for an end to insurance in Austria.

refugees and subsidiary-protected individuals regarding their countries of origin, the duration of asylum processes (12.1 months for Convention refugees versus 15.9 months for those with subsidiary protection), and their eligibility for family benefits²⁰ (35.9% versus 24%). *IVUR* is similar across groups, but on average, the *IPBL* for subsidiary-protected individuals is €308 lower due to a lower likelihood of being eligible for family benefits and generally less generous benefits for this group (see Section 2).

The final panel of the table details outcomes measured in the 12th month post-protection. Around 95% have an active insurance spell. The employment rate is 11.3% with mean monthly earnings of €203. Subsidiary-protected refugees do slightly better than Convention refugees on both metrics. 71% of Convention and 61% of subsidiary-protected refugees still live in the same state as initially assigned to. Given the low labor market integration, dependence on welfare benefits is high; 75% receive some form of benefits, and approximately 38% receive support from the PES.

Figure 3 shows how the employment rate (left) and the share of refugees who remain in the initial state (right) evolve from the moment of receiving protection. The employment rate starts from near zero at the time of protection. For men, it rises steadily and reaches about 60% after five years. For women, the employment rate is less than 20% after five years. The other notable aspect is high mobility rates immediately after receiving protection. Within six months, a quarter of refugees have moved to another state. Mobility is higher for men than for women. Figure A.3 in the Appendix shows the evolution of other outcome variables by gender over time.

Mobility patterns after receiving protection vary substantially between federal states (see Figure 4). In Vienna, 96% of refugees remain, while only 26% do so in Burgenland, the other end of the distribution. The overwhelming majority of individuals who move between federal states move to Vienna. Twelve months after protection, 90 or more percent of all refugees are either in their initial states or in Vienna. The other states play only a marginal role as destinations for internal mobility.

Vienna is not only an attractive destination among refugees but also among other migrants. While 22% of the Austrian population live in Vienna, this share is about 40% among foreigners. On the other end, Burgenland, with 3.3% of the population, only hosts 2% of foreigners in Austria (Statistik Austria, 2024).

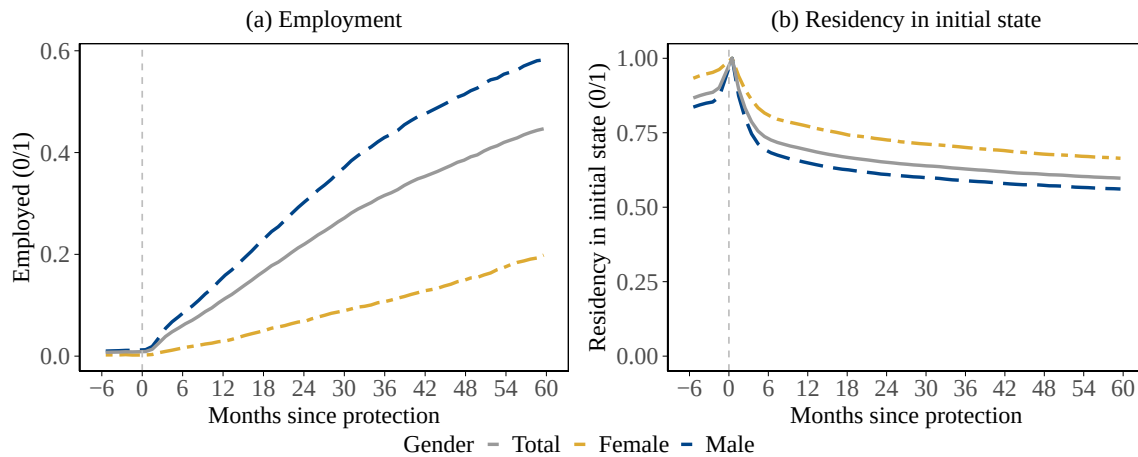
²⁰We define eligibility for family benefits when receiving protection for refugees as applicable to those who are co-insured or provide co-insurance—through a partnership or a parent-child relationship—to another individual who has been registered as a refugee in Austria on or before the date of protection.

Table 1: Summary statistics

	Convention refugee	Subsidiary-protected	Total
Time-invariant characteristics			
Gender (female=0)	0.63 (0.48)	0.70 (0.46)	0.65 (0.48)
Age at immigration	30.75 (9.17)	29.49 (9.46)	30.49 (9.25)
Origin country			
Afghanistan	4440 (14.1%)	2903 (35.9%)	7343 (18.5%)
Iran	2779 (8.8%)	124 (1.5%)	2903 (7.3%)
Iraq	1806 (5.7%)	1287 (15.9%)	3093 (7.8%)
Other	1623 (5.1%)	639 (7.9%)	2262 (5.7%)
Russia	433 (1.4%)	158 (2.0%)	591 (1.5%)
Somalia	631 (2.0%)	1190 (14.7%)	1821 (4.6%)
Syria	19885 (62.9%)	1796 (22.2%)	21681 (54.6%)
Duration asylum process	12.08 (9.02)	15.90 (9.50)	12.86 (9.25)
German skill level			
A	18786 (59.5%)	4872 (60.2%)	23658 (59.6%)
B/C	6019 (19.0%)	1258 (15.5%)	7277 (18.3%)
unknown	6792 (21.5%)	1967 (24.3%)	8759 (22.1%)
Education level			
primary	21344 (67.6%)	6122 (75.6%)	27466 (69.2%)
secondary	4469 (14.1%)	640 (7.9%)	5109 (12.9%)
tertiary	1913 (6.1%)	219 (2.7%)	2132 (5.4%)
unknown	3871 (12.3%)	1116 (13.8%)	4987 (12.6%)
Family benefit eligibility	0.36 (0.48)	0.24 (0.43)	0.34 (0.47)
Initial local conditions			
IVUR	0.19 (0.15)	0.20 (0.16)	0.19 (0.15)
IPBL (in €1,000)	0.83 (0.29)	0.52 (0.29)	0.76 (0.32)
Outcomes in month twelve			
No Austrian insurance	0.06 (0.23)	0.03 (0.16)	0.05 (0.22)
In employment	0.10 (0.30)	0.17 (0.37)	0.11 (0.32)
Monthly real earnings	180.27 (551.53)	292.51 (663.54)	203.16 (577.92)
In marginal employment	0.03 (0.18)	0.03 (0.18)	0.03 (0.18)
Residency in initial state	0.71 (0.45)	0.61 (0.49)	0.69 (0.46)
Residency in Vienna	0.51 (0.50)	0.58 (0.49)	0.52 (0.50)
PES services	0.38 (0.48)	0.39 (0.49)	0.38 (0.49)
Receiving welfare benefits	0.76 (0.43)	0.74 (0.44)	0.75 (0.43)
Observations	N=31597	N=8097	N=39694

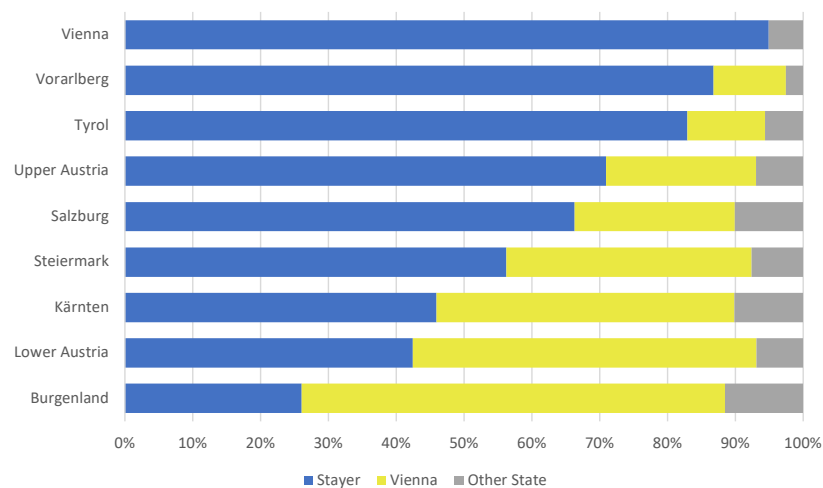
Notes: The table reports means and standard deviations (in brackets) for continuous and binary variables. The number of observations and the percentage within groups are reported for categorical variables. Time-varying (outcome) variables are reported for the 12th month after protection was received. The sample is restricted to refugees who received protection between January 2011 and August 2018, who were between 18 and 60 years old at the time of immigration and whose asylum process lasted longer than a day but less than three years, and where full data are available for 60 months after the date of protection. Family benefit eligibility is an indicator equal to one if children who are co-insured with this person or their partner are registered in Austria at the time of receiving protection. Duration of asylum process is denoted in months, earnings are denoted in real 2015 euros, and IPBL is denominated in €1,000 (real 2015 values) to match the convention used throughout the regression analysis and all figures.

Figure 3: Share of refugees in employment and residency in initial state by time since receiving protection



Notes: The figure shows the share of employed refugees and of refugees who remained in their initial state by months since protection and gender. The x-axis shows the months since receiving protection. The y-axis displays the shares of refugees in (a) employment or (b) residing in the state of protection (initial state).

Figure 4: Residence of refugees twelve months after receiving protection by initial state



Notes: The figure shows the state of residence of refugees twelve months after receiving protection. Stayers remain in the initial state (blue). Yellow bars indicate the shares that move from another state to Vienna. Gray bars indicate the shares that move to a state other than Vienna.

4 Empirical Approach

In an ideal experiment to understand the impact of two treatments and their interaction, one would randomize the receipt of those two treatments. However, such randomization is conceptually not feasible in our context. While it is conceivable to assign benefit generosity, labor demand, driven by the economy’s evolution, cannot be manipulated in the same manner. Therefore, we must conceptualize treatment assignments differently. Rather than assigning treatments to individuals, we allocate individuals to treatments based on their location, timing, protection status and family status. Each combination of place, time, and status cells comes with values for the treatment variables, $IVUR$ and $IPBL$.

Approaching identification from this angle requires separately thinking about two levels of identifying assumptions. First, are individuals exogenously assigned to cells? Second, are there other characteristics of these cells that are correlated with the value of the treatment variables also affecting the outcomes and thus confounding the analysis?

Subsequently, we describe our empirical strategy that seeks to address both levels of the identification problem. We start with a description of the econometric specification and subsequently explain how this specification is linked to our identification problem.

4.1 Econometric specification

The essence of our empirical strategy is to exploit exogenous variation in the initial local labor market conditions and welfare benefit generosity that refugees face at the time and place of protection. For each individual i , we denote the calendar month of protection as $m^0(i)$, the district of residence at protection as $d^0(i)$, and the state at protection as $s^0(i)$. Both the initial vacancy-to-unemployment ratio ($IVUR_{d^0(i),m^0(i)}$) and the initial potential benefit level ($IPBL_{p(i),f(i),s^0(i),m^0(i)}$) are measured for the district or state and month of protection. Here, $p(i)$ denotes protection type and $f(i)$ family status.

Outcomes Y_{it} are measured at time t for the location where the individual resides at t , which may differ from the initial location. All fixed effects for the month-year of protection, arrival group, and district of residence at protection are allowed to vary by the outcome period. This specification ensures that all group and contextual adjustments are flexibly modeled for each time horizon. Crucially, $IVUR$ and $IPBL$ are fixed at the protection-date values for the entire 60-month outcome window, so $\beta_{1,t}$ and $\beta_{2,t}$ at long horizons identify the effect of *initial* conditions on later outcomes, not the effect of the contemporaneous labor market or benefit regime that a relocated refugee may no longer face.

Formally, the primary specification estimated for each outcome period t is:

$$\begin{aligned}
Y_{it} = & \alpha_t + \beta_{1,t} IVUR_{d^0(i),m^0(i)} + \beta_{2,t} IPBL_{p(i),f(i),s^0(i),m^0(i)} \\
& + \gamma_{m^0(i),t} + \delta_{g(i),t} + \eta_{d^0(i),t} + X_i' \Gamma_t + \epsilon_{it}
\end{aligned} \tag{3}$$

where Y_{it} denotes the outcome for individual i observed at time t , indexed in months since the granting of protection, and measured for the district in which i resides at t . $IVUR_{d^0(i),m^0(i)}$ and $IPBL_{p(i),f(i),s^0(i),m^0(i)}$ capture initial conditions at the time and place of protection. $\gamma_{m^0(i),t}$, $\delta_{g(i),t}$, and $\eta_{d^0(i),t}$ denote fixed effects for the month-year of protection, arrival group, and district at time of protection, all specific to outcome period t . X_i is a vector of time-invariant individual controls, including age at immigration and its square, type of protection status, country of origin, and eligibility for family benefits, and ϵ_{it} denotes the error term.

4.2 Identification strategy

Identification of the *IVUR* and *IPBL* effects in Equation 3 rests on two conditional independence assumptions.²¹ Conditional on arrival-group, month-year-of-protection, and district-of-protection fixed effects ($\delta_{g(i),t}$, $\gamma_{m^0(i),t}$, $\eta_{d^0(i),t}$) together with our individual controls X_i , we assume that (i) $IVUR_{d^0(i),m^0(i)}$ is mean-independent of the outcome residual, and (ii) the same holds for $IPBL_{p(i),f(i),s^0(i),m^0(i)}$. Sections 4.2.1 and 4.2.2 justify these assumptions in turn.

4.2.1 Assignment to location-time-protection status-cells

First, we consider the allocation of refugees to the districts where they reside when receiving protection. This is the district where they get access to the Austrian labor market and that determines under which state's welfare regime they initially fall. As explained in Section 2, municipalities might indicate preferences regarding the characteristics of asylum-seekers they can host. Thus, we expect systematic differences in the characteristics of refugees between municipalities. We address this issue by implementing arrival-group fixed effects $\delta_{g(i),t}$. Within groups of people of the same gender who arrived in Austria in the same quarter, were initially

²¹Schmandt et al. (2025) formalize two concerns that arise whenever random placement is used to estimate the effect of a group-level variable. First, random placement assigns group membership rather than any particular local variable, so estimates of a specific local variable carry a multiple-treatment component. We therefore estimate the two local variables we study jointly rather than one at a time, and $\eta_{d^0(i),t}$ absorbs all time-invariant district characteristics; Section 5.7 shows that adding state-level FPÖ voting intentions leaves the *IPBL* effects and the *IVUR* effect on employment unchanged but halves the *IVUR* effect on remaining in the initial state, which we attribute to bad control. Second, onward mobility related to initial conditions biases estimates of the effect of sustained exposure to those conditions. Our estimand is different: it is the effect of the conditions prevailing at the time of protection, inclusive of any mobility they induce. That refugees who leave are no longer exposed to those conditions is part of the effect we measure, and it is the leading explanation for why the employment effects fade while the location effects persist.

registered in the same state, and arrived alone or as a family, the assignment to municipalities depends on the next available housing unit at that point in time. Thus, within arrival groups, it is a haphazard process to determine which asylum-seeker goes to which municipality. Further, asylum-seekers are effectively banned from relocating to another state as they would lose benefits and are mostly unable to relocate within a state due to financial constraints.²² Given these constraints, conditional on the arrival group, denoted as $\delta_{g(i),t}$ in our specifications, asylum-seekers find themselves quasi-randomly assigned to the district where they live when their protection is granted.

The processing duration of an asylum claim depends primarily on bureaucratic capacity and cannot be systematically manipulated by the applicant. Although administrative factors such as case complexity may influence exact case sequencing, residual variation in the specific month of recognition is plausibly exogenous to individual unobservables conditional on our comprehensive arrival-group, origin, and district fixed effects.

IPBL also depends on protection type and family status. Both factors might directly influence our outcomes of interest. Thus, we condition on eligibility for family benefits and protection type to only compare individuals within the respective group.

In sum, asylum-seekers cannot select a district based on perceived labor market prospects or welfare benefits, nor determine the timing of receiving protection. Municipalities can only express preferences over broad characteristics, which is why we seek to make comparisons within arrival groups.

While we cannot directly ascertain if asylum-seekers could self-select into specific districts or influence the duration of their asylum process, we can investigate if *IVUR* and *IPBL* correlate with individual characteristics. We achieve this by regressing i) the district-level *IVUR* and ii) the state-level *IPBL* on district, month-year of protection, country of origin, and arrival group fixed effects. Additionally, we control for the type of protection and an indicator for family benefit eligibility, which are established confounders for the *IPBL* since the *IPBL* varies with protection type and family size.

Columns (1) and (3) of Table 2 present the results without arrival group fixed effects, while columns (2) and (4) include them. All individual coefficients are close to zero and negligible in magnitude. For example, conditional on arrival group fixed effects, individuals with secondary education face an *IVUR* that is only 0.002 smaller than those with primary education, amounting to roughly 1.3 percent of a standard deviation of *IVUR*. Similarly, the coefficient

²²The basic subsistence support usually provides housing in organized accommodations. In the case of independent relocation to private housing within the state, asylum-seekers only receive a small monthly compensation, which is usually insufficient to pay for rent.

Table 2: Correlation of initial vacancy-to-unemployment ratio and initial potential benefit level with migrant characteristics

	(1) IVUR	(2) IVUR	(3) IPBL	(4) IPBL
Asylum process duration	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.001)
Age at immigration	0.000 (0.000)	0.000 (0.000)	0.000** (0.000)	0.000 (0.000)
Education (reference category: Primary)				
Secondary education	−0.004** (0.002)	−0.002 (0.001)	−0.005 (0.003)	−0.004 (0.003)
Tertiary education	−0.003* (0.002)	−0.002 (0.001)	−0.007** (0.003)	−0.008*** (0.003)
Unknown education	−0.001 (0.001)	0.000 (0.001)	0.004 (0.004)	0.003 (0.004)
German skills (reference category: A)				
B/C german skills	0.000 (0.001)	−0.001 (0.001)	−0.005** (0.002)	−0.005** (0.002)
Unknown german skills	−0.001 (0.001)	0.000 (0.001)	−0.001 (0.002)	0.000 (0.002)
Known Confounders IPBL				
Convention refugee	−0.002 (0.001)	−0.001 (0.001)	0.265*** (0.041)	0.262*** (0.042)
Family benefit eligibility	0.002** (0.001)	−0.001 (0.001)	0.607*** (0.011)	0.611*** (0.011)
District FE	X	X	X	X
Month and year FE	X	X	X	X
Country of origin FE	X	X	X	X
Arrival group FE		X		X
N	39 776	39 776	39 776	39 776
Wald-Test (p-value) IVUR	0.306	0.367	-	-
Wald-Test (p-value) IPBL	-	-	0.003	0.005

Notes: Coefficients from OLS regressions of the *IVUR* and *IPBL* at the month of protection on migrant characteristics as well as a full set of district-at-protection, year-month, and country-of-origin fixed effects. Columns 2 and 4 also include arrival-group fixed effects. The Wald test statistics test the full models against a restricted model with i) only fixed effects for the *IVUR* and ii) fixed effects and dummy variables indicating the type of protection and assumed family status at protection date. Coefficients and standard errors displayed as 0.000 are smaller than 0.0005 in absolute value, in the *IPBL* columns, measured in €1,000, this corresponds to less than 50 cents. The number of observations differs marginally from the estimation sample used elsewhere in the paper (39,694), as this table is based on a slightly updated data extraction. Standard errors are clustered at the state-year-of-protection level. */**/** denote statistical significance at the 10/5/1 percent level.

for *IPBL* suggests that individuals with tertiary education face an *IPBL* that is about eight euros lower than those with primary education, representing approximately 2.5 percent of a standard deviation of *IPBL*.

The overarching Wald tests do not reject the null hypothesis of zero correlation for *IVUR* (p -values of 0.306 in column 1 and 0.367 in column 2), but they do reject the null for *IPBL* with p -values of 0.003 in column 3 and 0.005 in column 4.²³ Given our large sample size, the Wald tests have substantial power to detect even minor imbalances. However, because the economic magnitude of these imbalances is extremely small, they are unlikely to introduce meaningful bias. To ensure our estimates are unaffected by these minor differences, we directly control for the complete set of individual characteristics (X_i) in our primary regression specifications. Overall, these results support the argument that the placement of asylum-seekers to districts is quasi-randomly assigned.

4.2.2 Exogeneity of *IVUR* and *IPBL*

Second, we consider a possible correlation between *IVUR* and *IPBL* and other local characteristics affecting refugees' labor market success. For example, regions with high *IVUR* and *IPBL* could also have a more welcoming civil society that helps refugees integrate better. To address this concern, we include district fixed effects $\eta_{d^0(i),t}$ that control for all time-constant regional characteristics with constant effects. Controlling for month-year of protection, fixed effects $\gamma_{m^0(i),t}$ hold constant all factors that influence all regions in the same way, such as national policy changes. Because $\eta_{d^0(i),t}$ absorbs each district's long-run average tightness, the *IVUR* variation our specification exploits is by construction a within-district deviation: our coefficient $\beta_{1,t}$ identifies responses to transient tightness shocks at the time of protection, not to differences in average labor-market quality across destinations.

Given these fixed effects, we deem it unlikely that *IVUR* is confounded by other local factors influencing the outcome. The argument requires more care for *IPBL*. Conditional on our fixed effects, most of the identifying variation in *IPBL* comes from four state-level reforms enacted between April 2016 and April 2017 in three of the nine federal states, described in Appendix B.2. These reforms might result from a broader change in attitudes towards refugees, which might also affect the outcomes.

Two features of the setting bear on this concern. First, three of the four reforms were annulled by courts: the Constitutional Court struck down the Lower Austrian and Burgenland

²³For the *IVUR*, we contrast the model with all presented personal characteristics against the model with only fixed effects. For the *IPBL*, we compare the full model to one that includes only the fixed effects and the known confounders.

reforms in March 2018, and the European Court of Justice struck down the Upper Austrian reform in November 2018. Courts annul state legislation on constitutional and EU-law grounds, and the timing of their rulings is governed by litigation rather than by local public opinion. Second, the two Constitutional Court rulings fall within our sample window, so refugees granted protection in Lower Austria and Burgenland in the final months of our sample are assigned the restored, higher *IPBL*, independent of local attitudes.

While we cannot fully rule out attitude-driven confounding, we argue that we can sign the direction of the resulting bias for each outcome. States with higher *IPBL* are likely to have more pro-refugee attitudes, which themselves may affect refugee outcomes. For the probability of remaining in the initial state, both channels plausibly operate in the same direction: pro-refugee attitudes and more generous benefits might each encourage staying. If so, omitted attitudes would bias our coefficient upward, meaning our estimates may slightly overstate the true causal effect of benefits on location retention. For employment, these channels likely operate in opposite directions: pro-refugee attitudes may raise refugee employment through reduced employer discrimination, partially offsetting the negative effect of more generous benefits on employment. If this is the case, such confounding would attenuate our estimate toward zero, suggesting the true negative effect of benefits on employment could be even larger than we report.

4.2.3 Inference

Our two treatments vary at different levels of aggregation: *IVUR* at the district $\times$ month level, *IPBL* at the state $\times$ protection-type $\times$ family-status $\times$ month level. No single clustering level is therefore correct for both coefficients. Clustering at the district level would ignore that *IPBL* is common to all refugees receiving protection in a state-month. Clustering at the state level would leave only nine clusters, too few for the asymptotic approximation on which cluster-robust standard errors rely. We report standard errors clustered at the state $\times$ year-of-protection level, which sits between the two levels of treatment variation. District fixed effects absorb time-invariant district and state unobservables, so the residual error contains primarily localized, time-varying shocks, and we assume these are concentrated within a state’s annual protection cohort. Examples are cohort-specific bottlenecks in processing or localized peer effects among refugees granted protection at the same time. Because the identifying variation in *IPBL* comes primarily from a small number of state-level reforms, the effective number of independent shocks behind $\beta_{2,t}$ is closer to that count than to the number of clusters and we therefore also report wild-cluster bootstrap intervals. Section 5.8 compares six clustering choices

alongside the bootstrap.

5 Results

An increase of one unit in *IVUR* signifies the addition of as many new job openings in a district as there were unemployed individuals, while holding the unemployment count constant. A one-unit increase in *IPBL* represents an increase in potential benefit levels by €1,000. To obtain easy-to-interpret effect sizes, we use a shift approximately equal to the standard deviation (s.d.) and the interquartile range of the *IVUR* and *IPBL* as a reference. For the *IVUR*, this shift denotes an increment of 0.2 ($s.d. = 0.153, IQR = 0.211$), and for the *IPBL*, an increase of 0.3 (€300, $s.d. = 316.9, IQR = 456$).²⁴ Subsequently, we term this a *standard shift* or a *standard increase*. Multiplying the shift with $\beta_{1,t}$ for the *IVUR* and $\beta_{2,t}$ for the *IPBL* gives us the change in the outcome for each respective increase.

5.1 Effects on labor market outcomes

Figure 5 presents the primary results for selected labor market outcomes. Panels (a) through (d) illustrate how an increase of one unit in *IVUR* or *IPBL* impacts (a) the likelihood of being in employment, (b) the unconditional monthly real earnings (in 2015 euro), (c) the log of real monthly wage income (conditional upon employment), and (d) the likelihood of being in marginal employment.

A standard shift in the *IVUR* increases the employment probability in the 12th month after receiving protection by 4.2 pp. The effect occurs almost immediately after receiving protection and starts to weaken after 18 months, turning zero in month 30. This finding is qualitatively in line with other results in the literature that study the effect of economic conditions at labor market entry on employment outcomes.²⁵ Relative to the mean employment rate of 11.3% in month twelve, a standard increase for *IVUR* increases the employment probability by 37%.

Before turning to the *IPBL* result, recall that *IPBL* measures the potential cash benefit: the coefficient pools refugees in private accommodation (directly exposed) and in organized accommodation (only indirectly exposed via the option of moving to private accommodation), and should therefore be read as a diluted average effect (see Section 6). A standard increase in the *IPBL* decreases the employment probability by 1.51 pp. in month twelve (13.3% of the mean). This effect also disappears after 30 months.

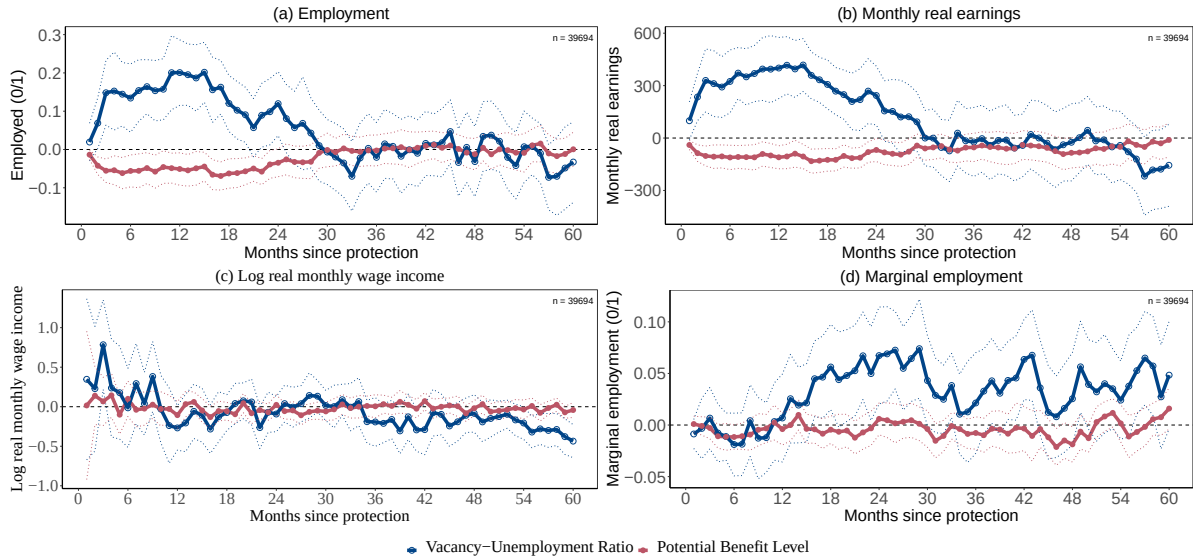
²⁴We use round numbers in the ballpark of the standard deviation and the IQR for ease of interpretation.

²⁵For example, Barsbai et al. (2025) find that family migrants who face a higher unemployment rate at immigration are initially less likely to be employed, with the effect disappearing after four years.

Panel (b) shows the effects on monthly earnings. The patterns of the effects closely follow the employment effect. A standard shift in the *IVUR* increases earnings by €81 (39% relative to the mean in month twelve), and a standard shift in *IPBL*, decreases earnings by €33 (-15.8% relative to the mean in month twelve), in monthly real earnings. Another way to interpret the effect of *IPBL* is that every €100 increase in potential benefits results in a decrease of €11 in monthly real earnings.

The similarity of the patterns in Panels (a) and (b) strongly suggests that changes in employment drive the effects on earnings. To explore the intensive margin, we use log real monthly wage income (conditional on having any income) as an outcome in Panel (c). We observe no statistically significant differences in conditional wages across treatments. While this flat wage profile is consistent with the extensive margin driving our unconditional earnings results, this estimate should be interpreted with caution. Because the treatments alter the probability of employment, compositional changes among the pool of working refugees could potentially mask underlying individual wage effects. Panel (d) suggests a tiny positive effect of a higher *IVUR* on the likelihood of marginal employment starting after twelve months, with the *IPBL* showing no discernible effect.

Figure 5: Effect of the *IVUR* and *IPBL* on selected labor market outcomes



Notes: The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either i) the vacancy-to-unemployment ratio or ii) the potential benefit level, measured in €1,000 (real 2015 values), on selected outcomes. The panels show the effects on (a) having any type of part- or full-time employment (binary); panel (b) the unconditional monthly real earnings (in 2015 euro); panel (c) the log real monthly wage income (conditional on being employed); and panel (d) the probability of having a mini-job (below €406 in 2015). All regressions include a full set of arrival-group, year-month, and district-of-protection fixed effects. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

5.2 Effects on internal mobility

Better labor market conditions and higher benefits should reduce the likelihood of relocating, which is very common among refugees after receiving protection (see Figure 3). We show results for mobility in Figure 6.

In Panel (a), we observe that an increase in the *IVUR* has a statistically significant positive impact on the likelihood of refugees staying in the state. Specifically, a standard shift in the *IVUR* leads to a 3.9 pp. increase in this probability in month twelve. Similarly, a standard shift in the *IPBL* results in a 2.8 pp. increase in the same likelihood in month twelve. The point estimates become somewhat weaker over time but remain positive over the full 60 months, suggesting that enhanced welfare benefits and a strong labor market both positively influence the likelihood of refugees staying in their original state.

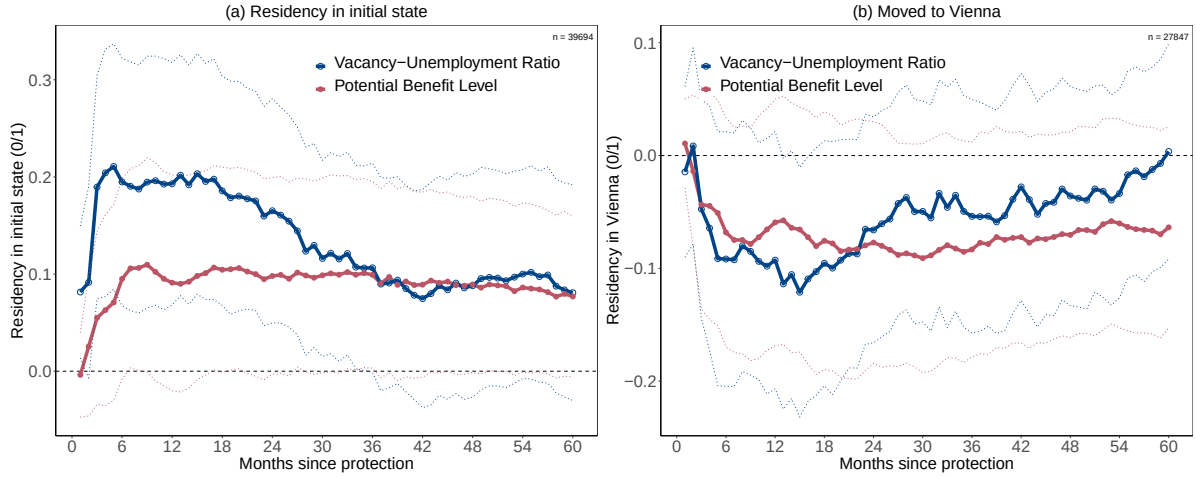
These findings are further supported by the observations in panel (b). Here, we note a consistent negative effect of the *IPBL* on the likelihood of relocating to Vienna for those not initially assigned to Vienna. The *IVUR* also has a negative but not persistent and only marginally significant effect.

The similarity of approaches allows for a direct comparison with the results of Ferwerda et al. (2024), who study the effect of current benefit levels on the probability of immigrants moving within Switzerland. They find that increasing benefits from less than 750 CHF to 751-1500 CHF increases the likelihood of staying by about 1.5 pp. while an increase to 1500-3000 CHF increases it by about 2 pp. Our effects are significantly larger, as a 2 pp. change in the probability is triggered by a roughly €200 change in benefit levels. A likely explanation is that the effects in the Austrian context are larger because the treatment is applied when refugees receive protection, when they are not yet strongly tied to a specific location and are potentially more mobile.²⁶

In Figure 7, we show how the effects on employment and location choice interplay. A higher *IVUR* positively affects the joint probability of being employed and in the initial state (panel (a)) and negatively affects the joint probability of being not employed and in another state (panel (d)). That is, favorable initial labor markets specifically increase the joint probability that refugees secure local employment and remain in their assigned state, while reducing the

²⁶Alternatively, we can approximate an elasticity of the mover rate with respect to benefit levels. However, providing a single elasticity is difficult, as the result depends on the point of evaluation, i.e. which benefit level is considered (which varies substantially across states) and which mover rate is taken as the baseline. As an example, here is a back-of-the-envelope calculation for Upper Austria. The mean mover rate is 0.29. The reform reduced benefits from roughly €1,000 to €700 (incl. housing benefits), and we estimate that a €100 reduction increases the mover rate by roughly one pp. Evaluating the elasticity at the midpoint benefit level of €850 and an implied mover rate of 0.29, the percentage change in benefits is $-100/850 \approx -11.8\%$ while the corresponding change in the mover rate is $0.01/0.29 \approx 3.4\%$. This yields an elasticity of $3.4/11.8 \approx 0.29$ in absolute terms.

Figure 6: Effect of the *IVUR* and *IPBL* on mobility outcomes



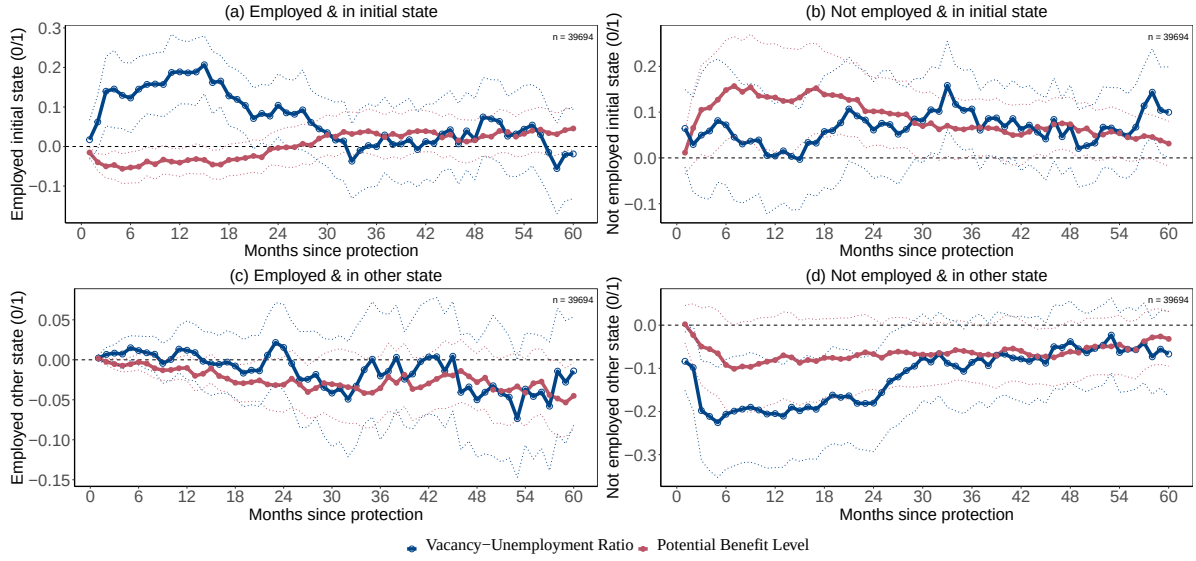
Notes: The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either i) the vacancy-to-unemployment ratio or ii) the potential benefit level, measured in €1,000 (real 2015 values), on selected outcomes. Panel (a) shows the effects on the probability of living in the state of protection; panel (b) shows the effect on moving to Vienna (conditional on residing outside Vienna at the time of protection). All regressions include a full set of arrival-group, year-month, and district-of-protection fixed effects. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

likelihood that they leave the state without employment. This compositional shift strongly supports the mechanism that finding early local work anchors refugees in their initial placement.

In contrast, the *IPBL* has a stronger effect on the joint probability of being not employed and in the initial state (panel (b)). However, this is primarily due to a higher likelihood of remaining in the state, rather than to changing employment prospects. This becomes apparent from the negative effect on not being employed and in another state (panel (d)).

This suggests that strong labor markets increase the probability that individuals remain in their initial state, owing to job availability. In contrast, weak labor markets might incentivize relocation to another state without prospects of work in the destination state. Additionally, these findings indicate that benefit levels are relevant for the location choice but less so for employment trajectories.

Figure 7: Effect of the *IVUR* and *IPBL* on employment and location choice



Notes: The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either i) the vacancy-to-unemployment ratio or ii) the potential benefit level, measured in €1,000 (real 2015 values), on selected outcomes. Panel (a) shows the effects on the joint probability of being employed and living in the initial state; panel (b) shows the effect on the joint probability of being not employed and living in the initial state. Panel (c) shows the effects on the joint probability of being employed and living in another state; panel (d) shows the effect on the joint probability of not being employed and living in another state. All regressions include a full set of arrival-group, year-month, and district-of-protection fixed effects. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

5.3 Effects on receiving social support

The third group of outcomes consists of measures of social support receipt. Figure 8 shows the effects on the likelihood of (a) receiving services from the PES and (b) receiving welfare benefits.

We do not find any effect of the *IVUR* or *IPBL* on receiving PES services (Panel (a)). If anything, a higher *IPBL* has a small negative effect in the first nine months.

Panel (b) shows a significant negative impact of the *IVUR* on the probability of receiving welfare benefits in the first 30 months. Interestingly, the magnitude of this effect is consistent with the positive effect of the *IVUR* on employment (Figure 5, Panel (a)). Furthermore, the impact on receiving welfare benefits diminishes and becomes statistically insignificant after 30 months, mirroring the trajectory of the employment effect. This suggests that refugees who secure employment due to favorable local labor market conditions are less likely to rely on welfare benefits.

Similarly, we observe that a higher *IPBL* has a small but significant effect on the likelihood of receiving welfare benefits, particularly in the years 2 and 3 after receiving protection.

The effect is largest about 24 months after receiving protection, when a standard shift in *IPBL* increases the probability of receiving welfare benefits by about 2.5 pp. Again, the pattern mirrors the effect of *IPBL* on employment.

Figure 8: Effect of the *IVUR* and *IPBL* on selected social support outcomes



Notes: The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either i) the vacancy-to-unemployment ratio or ii) the potential benefit level, measured in €1,000 (real 2015 values), on selected outcomes. The panels show the effects on (a) the probability of making use of PES services and (b) the probability of receiving welfare benefits. All regressions include a full set of fixed effects for arrival group, year-month, and district of protection. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

5.4 Interplay of labor market tightness and benefit levels

Our main specification, as presented in Equation 3, estimates how *IVUR* and *IPBL* independently affect refugees' economic and social integration after receiving protection. To further explore the relationships between these variables, we introduce an interaction term between *IVUR* and *IPBL* in Equation 4:

$$Y_{it} = \alpha_t + \beta_{1,t} \widetilde{IVUR}_i + \beta_{2,t} \widetilde{IPBL}_i + \beta_{3,t} \widetilde{IVUR}_i \times \widetilde{IPBL}_i + \gamma_{m^0(i),t} + \delta_{g(i),t} + \eta_{d^0(i),t} + X_i' \Gamma_t + \epsilon_{it}, \quad (4)$$

where $\widetilde{IVUR}_i = IVUR_{d^0(i),m^0(i)} - \overline{IVUR}$ and $\widetilde{IPBL}_i = IPBL_{p(i),f(i),s^0(i),m^0(i)} - \overline{IPBL}$.

A negative cross-partial of *IVUR* and *IPBL* on employment is consistent with a standard equilibrium search framework (Pissarides, 2000), in which higher labor market tightness raises the offer arrival rate, so the disincentive effect of a higher reservation wage from more generous benefits binds more tightly when offers are plentiful. Schmieder and von Wachter (2016) document the same cross-partial empirically for unemployment insurance across varying labor

market conditions.

Because the treatments enter demeaned, $\beta_{1,t}$ and $\beta_{2,t}$ measure the effect of each treatment at the mean of the other, and Equation 4 differs from Equation 3 only in that the treatments are demeaned and the interaction term is added. Identification of $\beta_{3,t}$ requires an assumption beyond the two stated in Section 4.2, since mean-independence of *IVUR* and of *IPBL* separately does not imply mean-independence of their product. Our framework supplies it: individuals are assigned to place-time-status cells, and both treatments and their interaction are functions of the assigned cell. If that assignment is quasi-random conditional on our fixed effects, as argued in Section 4.2.1, the interaction inherits the same property. This specification allows us to calculate and interpret the effects of changes in the *IPBL* for different levels of labor market tightness.

Table 3 reports the results for selected outcomes twelve months after receiving protection. The coefficients for the main effects remain comparable to our previous results in direction, magnitude, and statistical significance. The interaction term has a significant, opposing effect on the probability of being in employment and receiving welfare benefits (columns (1) and (4), respectively). For the probability of remaining in the initial state in column (2), we find no statistically significant effect of the interaction, but we find a positive and significant effect on the probability of receiving assistance by the PES in column (3).

The marginal effects are calculated for the *IPBL*. Therefore, we fix the *IVUR* at zero on the first row, the mean in the second row and at 0.2 above the mean in the third row.

Column (1) shows the effect of changes in benefit levels on the probability of being employed at different levels of labor market tightness. As predicted by the search framework above, changes in benefit levels have practically no effect on the employment probability when the labor market is slack. However, benefit levels affect employment probability strongly when the labor market is tight. We see the inverse effects for the receipt of welfare benefits (column (4)). These results are in line with both the search prediction and the empirical findings of Dustmann et al. (2024), who find that cuts in welfare benefits in Denmark only affect the employment probability in strong labor markets.

Column (3) suggests that when the labor market is tight, increasing benefits increases the likelihood of receiving assistance from the PES. This is potentially due to the combination of the incentives for public agencies to fill the labor demand, but also to reduce the fiscal cost due to high benefit levels.

Table 3: Effects of *IVUR* and *IPBL* with interactions of treatment variables and marginal effects of *IPBL* for different levels of *IVUR*, twelve months after protection

	(1)	(2)	(3)	(4)
	In employment	In initial state	PES support	Welfare benefits
Coefficients				
IVUR	0.188*** (0.044)	0.189** (0.062)	-0.021 (0.056)	-0.218*** (0.048)
IPBL	-0.050** (0.018)	0.091** (0.030)	-0.005 (0.025)	0.048* (0.024)
IVUR:IPBL	-0.294*** (0.043)	-0.091 (0.112)	0.230*** (0.055)	0.280*** (0.071)
Marginal Effects				
IPBL (IVUR=0)	0.006	0.108	-0.049	-0.006
IPBL (IVUR=0.192)	-0.050	0.091	-0.005	0.048
IPBL (IVUR=0.392)	-0.109	0.073	0.041	0.104

Notes: The upper part of the table reports OLS estimates for *IVUR*, *IPBL* and their interaction. The lower part reports marginal effects for *IPBL*, fixing *IVUR* at zero, the mean (0.192) or 0.2 above the mean. The column title shows the outcome variable. All regressions include a full set of arrival-group, year-month, and district-of-protection fixed effects. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. */**/** denote statistical significance at the 10/5/1 percent level.

5.5 Effect heterogeneity by gender

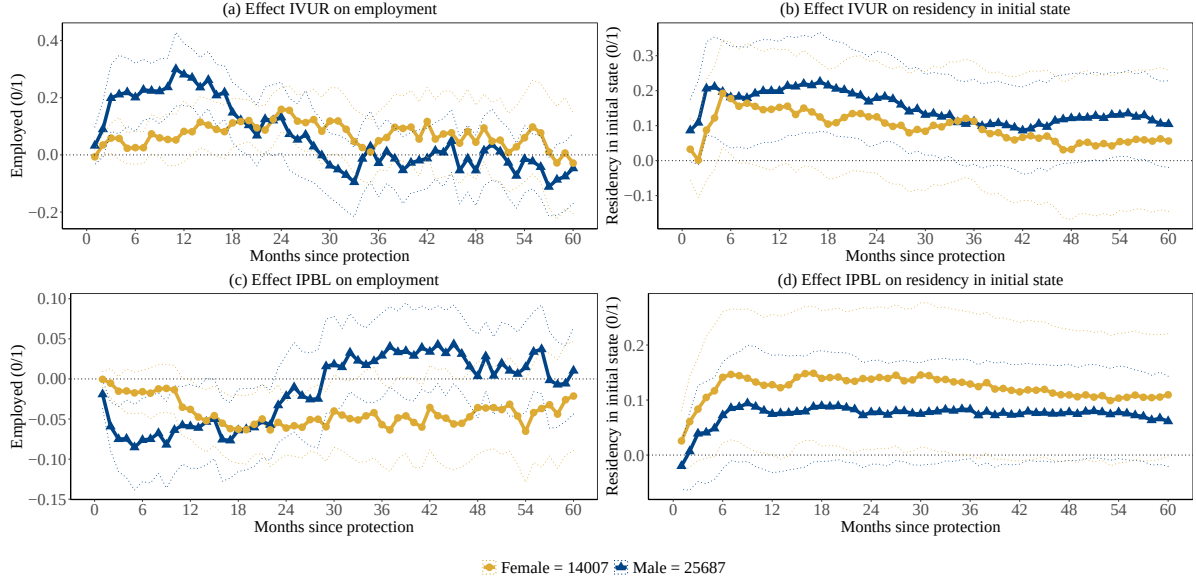
One notable observation is the significant disparity in labor market integration between genders, with women’s employment rates at only 20% even five years post-protection, in stark contrast to men’s, which is 60% (see Figure 3).

In this section, we investigate whether men and women also differ in their reactions to *IVUR* and *IPBL*. For this, we conduct the analysis separately for men and women with the results shown in Figure 9.

Panel (a) shows the effects of *IVUR* on employment. *IVUR* strongly affects the employment probability of men in the first 18 months after protection, but has hardly any effect on women. Still, for women, we do see some positive effects from around month twelve onwards. While this effect is small in absolute terms, it is not in relative terms, given the extremely low employment rate of female refugees. Panel (b) shows the effect of *IVUR* on mobility. Point estimates are slightly larger for men, but overall, the pattern is similar. Panel (c) shows the effect of *IPBL* on employment. While men initially reduce their employment more in response to higher benefits, the effect persists only for 30 months. For women, the negative effect of *IPBL* on employment is small but relatively persistent. Finally, Panel (d) shows that the effect of *IPBL* on the likelihood of staying in the state is somewhat stronger for women and persistent for both groups.

These findings align with the behavior predicted by traditional household models. When men primarily engage in market activities and actively seek employment, their sensitivity to

Figure 9: Effect heterogeneity by gender



Notes: The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either (i) the vacancy-to-unemployment ratio or (ii) the potential benefit level, measured in €1,000 (real 2015 values), on selected outcomes. Panels (a) and (b) show the effects of *IVUR* on employment and on the probability of staying in the initial state, respectively. Panels (c) and (d) show the effects of *IPBL* on employment and on the probability of staying in the initial state, respectively. Estimates are shown separately for men and women. All regressions include a full set of arrival-group, year-month, and district-of-protection fixed effects. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

labor market tightness is expected to be higher. For women, labor market tightness is less influential. Regarding mobility, while our data do not allow us to link individuals within households to formally test joint moves, these patterns suggest that labor market tightness indirectly affects the non-working partner's decisions, especially if the household relocates due to the male partner's unfavorable labor market conditions.

5.6 Relevant initial labor market

As noted by Åslund and Rooth (2007), it is not clear at what point in time initial local labor market conditions should be measured for refugees. While most of the literature focuses on labor market conditions at the time of immigration, institutional factors limit refugees' labor market access until they have been granted protection (about 13 months after arrival in our sample).

Hence, to compare our results to the standard specification in the literature, we add *IVUR* and *IPBL* measured at the time of immigration to our regressions. The arrival measures are constructed exactly like their counterparts at approval, but read in the month of arrival: *IVUR*

Table 4: Effects of *IVUR* and *IPBL* at approval on employment and residency controlling for *IVUR* and *IPBL* at arrival

	Employed	Residency in initial state
IVUR at approval	0.195*** (0.050)	0.183*** (0.061)
IVUR at arrival	−0.010 (0.059)	0.009 (0.061)
IPBL at approval	−0.017 (0.028)	0.183** (0.077)
IPBL at arrival	−0.050* (0.026)	−0.138* (0.073)

Notes: The table reports coefficients from OLS regressions estimating the effects at month 12 after receiving protection of i) the vacancy-to-unemployment ratio (*IVUR*) and ii) the potential benefit level (*IPBL*) measured in €1,000 (real 2015 values), both at the time of approval and arrival, on two outcomes: employment status (left) and residency in the initial state (right). All regressions include a full set of arrival-group, year-month, and district-of-protection fixed effects, as well as additional controls for age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. */**/** denote statistical significance at the 10/5/1 percent level.

at arrival is the vacancy-to-unemployment ratio in the district of protection, and *IPBL* at arrival applies the benefit schedule for the state of protection and the protection type and family status subsequently assigned. Both therefore differ from the approval measures only in timing, so the arrival coefficients are identified by within-district fluctuations in labor market tightness and by welfare reforms occurring between arrival and the granting of protection. Table 4 shows the results for two outcomes, employment and residency in the initial state, for month 12 after receiving protection.

Controlling for *IVUR* and *IPBL* at arrival leaves the effects of *IVUR* at approval on employment and residency virtually unchanged. For *IPBL* at approval, the point estimate on employment is reduced to about one third and loses statistical significance, while the effect on residency approximately doubles.

IVUR at arrival is small and statistically insignificant for both outcomes. *IPBL* at arrival is negative for both and marginally significant at the 10% level. The two *IPBL* measures coincide for every refugee who experienced no benefit reform between arrival and protection, so they are highly correlated and the division of the joint association between them is imprecisely estimated. The robust finding is that conditioning on conditions at arrival leaves the *IVUR* effects at approval intact and does not overturn the effect of *IPBL* at approval on residency.

5.7 Mechanisms

In this section we explore whether particular types of occupations drive the *IVUR* results. We generate alternative measures of the *IVUR*: first, by desired occupations, which are usually registered by caseworkers at the PES for each job seeker, and second, by seasonal and non-seasonal jobs. Additionally, we explore the concern that the benefit cuts could be politically driven by controlling for the vote share intentions for the FPÖ (Freedom Party of Austria), a far-right populist party.

Labor demand by most desired occupations. A broad measure of labor demand reflects the overall situation of the local labor market. However, many jobs that firms are seeking to fill might not be accessible to refugees with short tenures in the host country due to a lack of necessary language skills or recognized qualifications. To investigate whether the occupations in demand matter, we use the detailed nature of the vacancy data to create alternative versions of the *IVUR*.

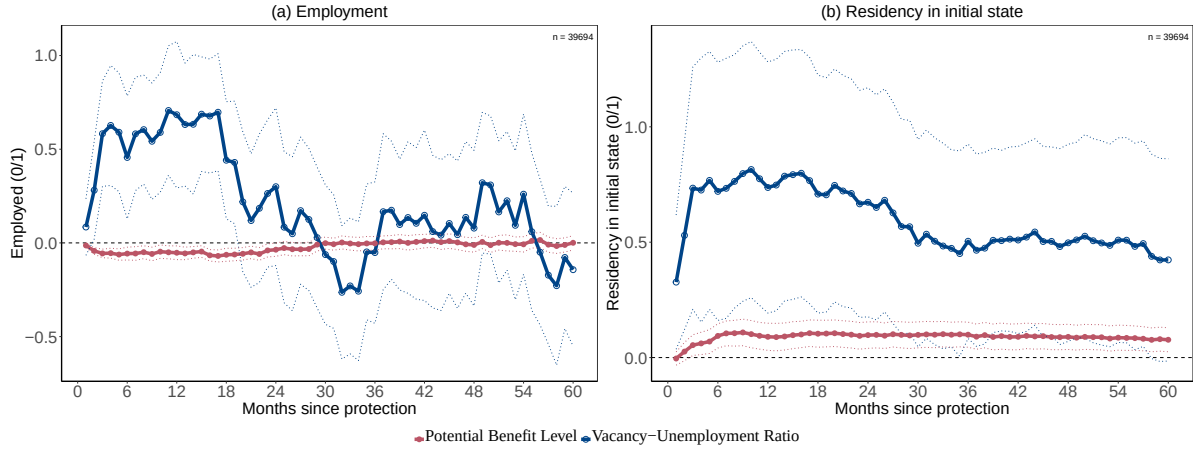
First, we create a measure of demand for occupations with particularly low entry barriers. The PES records *desired occupations* for every person registering with the PES. Desired occupations are not necessarily the jobs a job seeker desires, but those that a caseworker considers realistic based on the job seeker's prior experience and skills. We see a strong clustering for refugees, with four occupations accounting for more than 50% of the desired occupations: construction, cleaning, cooking and kitchen assistance, and general manual labor. Thus, we consider labor demand in these four occupations.

Figure 10 shows the effect of the *IVURs* in desired occupations on employment and mobility. The impact of labor demand in desired occupations on employment is approximately three times larger than that of the overall measure (left figure). However, this effect is short-lived, diminishing sharply after 18 months. In contrast, mobility is also strongly affected in the short term, and the effects, while somewhat declining, persist over the 60-month observation period (right figure).²⁷

Labor demand by seasonal occupations. We create a measure for demand in jobs labeled

²⁷This analysis has two caveats. First, these jobs account for only a small fraction of all vacancies, and the regional variation in this *IVUR* measure is significantly smaller. Thus, while the effect of labor demand in this occupation is large, the low variation implies that the overall effect of demand in these occupations on explaining mobility and employment differences between regions is small. Second, labor demand in these occupations might be correlated with labor demand in other occupations. In this case, the estimates would be upward-biased. To address this concern, we run a model that includes labor demand in all occupations other than the desired ones. The estimated effect of demand in the other occupations is slightly smaller but similar to the main result. Desired occupations have a stronger but more short-lived effect on employment than other occupations. However, they account for only a small share of all occupations.

Figure 10: Effect of *IVUR* in desired occupations on employment and mobility



Notes: The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either i) the occupation-specific vacancy-to-unemployment ratio or ii) the potential benefit level, measured in €1,000 (real 2015 values), on employment (left) and mobility (right). All regressions include a full set of arrival-group, year-month, and district-of-protection fixed effects. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

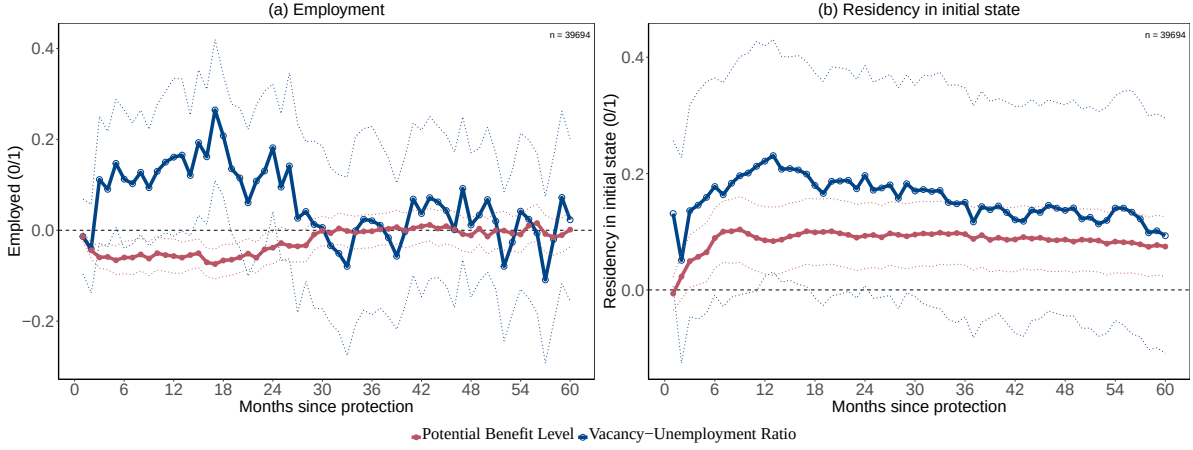
as seasonal occupations, often found in the tourism industry. These jobs are of particular interest as they reflect truly short-term opportunities rather than long-term economic perspectives.

For both types of occupations, we create measures for the *IVUR* by dividing the number of open positions in the respective categories by the number of job seekers. Note that we do not restrict job seekers to those looking in these specific occupations, as they often search in a broader range of fields.

Figure 11 shows the effects of labor demand in seasonal occupations, Figure 12 for the non-seasonal occupations. Due to district fixed effects, this demand does not reflect differences in regional economic structure, such as tourist areas. Instead, it reflects pure short-term demand for labor due to likely idiosyncratic reasons. For example, certain weather conditions might increase or decrease labor demand in the tourist industry or agriculture at specific times (Baumgartner et al., 2023). Because these seasonal occupations frequently represent temporary arrangements rather than durable career tracks, they offer a way to isolate the impact of immediate, short-term opportunities from broad structural labor market advantages. Consistent with the importance of immediate opportunities, we find that seasonal labor demand increases the likelihood of employment and persistently increases the likelihood of staying, with the effects being only slightly smaller than for non-seasonal occupations.

Together, these results indicate that resolving the immediate constraint of finding initial work and housing generates strong path dependence. The immediate opportunities provided by these jobs facilitate the difficult transition from refugee shelters to independent housing, an-

Figure 11: Effect of *IVUR* in seasonal occupations on employment and mobility



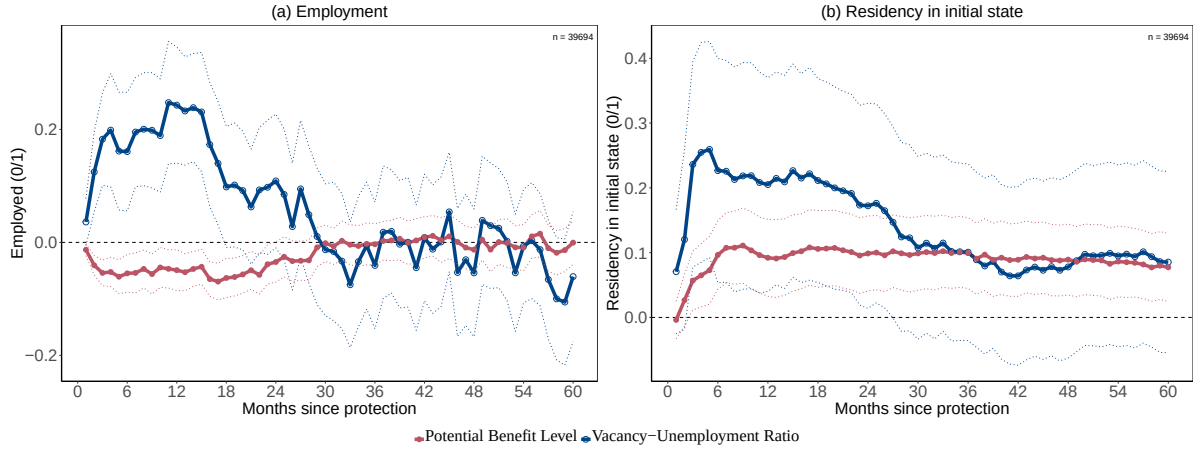
Notes: The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either i) the occupation-specific vacancy-to-unemployment ratio or ii) the potential benefit level, measured in €1,000 (real 2015 values), on employment (left) and mobility (right). All regressions include a full set of arrival-group, year-month, and district-of-protection fixed effects. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

choring refugees in their initial location even if the job itself is temporary. However, these initial job opportunities do not set refugees on a different career track with long-term implications.

Comparison with concurrent single-sector studies. Two concurrent papers exploit the same Austrian dispersal setting but focus on single labor-market segments. Degenhardt (2025), exploiting within-region, within-year seasonal variation in a hospitality-sector vacancy-to-unemployment ratio, finds an initial employment gain of up to 3 pp. that diminishes after the first year, alongside positive cumulative earnings over three years and increased labor market segregation—complementary to our seasonal-occupations analysis above. Degenhardt and Nimczik (2025) use the geographic expansion of online food delivery firms as a low-barrier work treatment, finding that gig availability accelerates initial job-finding but traps workers in low-paid, unstable jobs, with negative effects on human capital and job search. Our general *IVUR* measure nests these sectoral exercises in a single framework, finds a similar qualitative pattern with a longer fade horizon (around 30 months), and additionally jointly identifies the role of welfare benefit generosity.

Benefit cuts and political decisions as confounder. In our analysis, we exploit variation in benefit levels across states and refugee types (i.e., singles vs. families). One potential concern is that these benefit cuts may result from political decisions. For instance, more immigration-skeptical parties might attempt to reduce perceived *pull factors* by reducing benefit levels. If

Figure 12: Effect of *IVUR* in non-seasonal occupations on employment and mobility



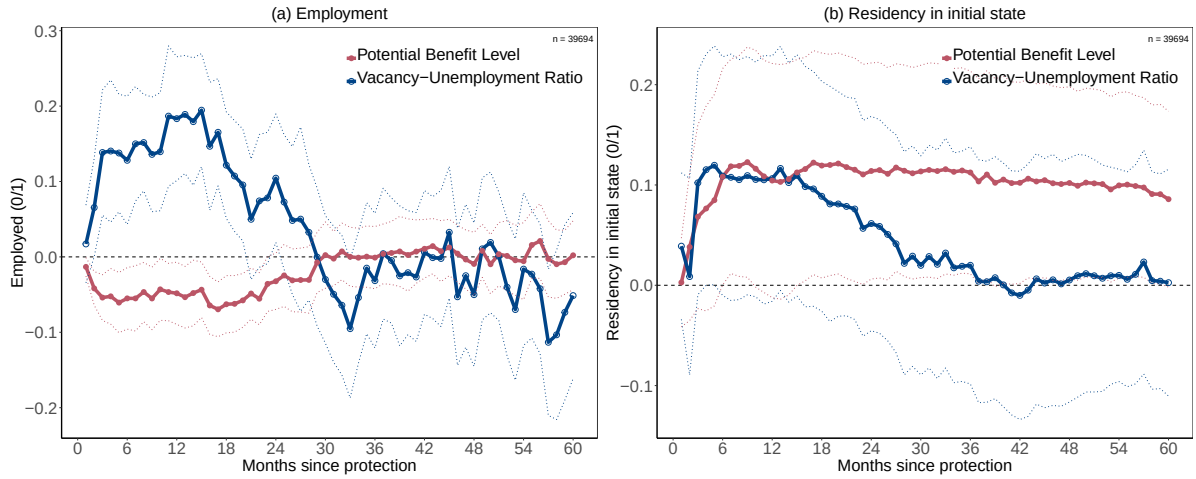
Notes: The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either i) the occupation-specific vacancy-to-unemployment ratio or ii) the potential benefit level, measured in €1,000 (real 2015 values), on employment (left) and mobility (right). All regressions include a full set of arrival-group, year-month, and district-of-protection fixed effects. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

this is the case, then political decisions or ideology would be confounding our results and the effects of *IPBL* would be upward-biased.

To address this concern, we control for political sentiment using state-level voting intentions for the FPÖ—a far-right populist party—measured at the time of asylum approval. We gathered information on the voting intentions for the FPÖ from various polls as a proxy for the political sentiment in the states at the time when refugees received labor market access.

Figure 13 shows the results. Including the FPÖ vote share intentions as a control does not significantly affect the effects of *IPBL* on employment and mobility. However, the estimated effect of *IVUR* on remaining in the initial state is halved compared to our main specification. This suggests that political sentiment might be a bad control for *IVUR*. If labor demand affects political preferences, then the FPÖ vote shares may themselves directly be affected by our variable of interest *IVUR*.

Figure 13: Robustness: Controlling for FPÖ Vote Share at Asylum Approval



Notes: The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either i) the vacancy-to-unemployment ratio or ii) the potential benefit level, measured in €1,000 (real 2015 values), on selected outcomes. The panels show the effects on (a) having any part- or full-time employment (binary) and (b) on the probability of living in the state of protection. All regressions include a full set of fixed effects for arrival group, year-month, and district of protection. Additional control variables include the FPÖ vote share at the state level at the time of protection, age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

5.8 Robustness

We perform several robustness checks to assess the sensitivity of our results to various sample restrictions and different specifications.

For our main specifications, we limited our sample to people who have been observed for a full 60 months. Since our data also cover refugees who received protection after August 2018, we can run our estimations with an extended sample, though the sample composition then changes with the outcome horizon. Figure A.4 presents the results of our main findings without the restriction of being observed for 60 months. By including all refugees who received protection up to the start of 2021, our sample size increases to 45,665 individuals. The coefficients remain mostly unchanged, and the 95% confidence intervals become slightly narrower for earlier months.

Second, we only consider refugees for whom we can determine in our data that they were in an initial reception facility at the beginning of their stay in Austria. For this group of 13,020 individuals, the assumption of exogenous placement is particularly credible. Figure A.5 shows the main results for this sample. Again, the overall patterns remain similar, with the effects of *IPBL* on employment being somewhat smaller in magnitude.

In a related spirit, we exclude individuals who received protection within one month after first registering in Austria. This unusually quick process might still hint at individuals who

came via family reunification.²⁸ This tighter restriction excludes 2,036 observations, leaving a sample of 37,658 observations. However, results remain virtually unchanged (see Figure A.6).

Third, we exclude individuals who received protection within four months of any of the welfare reforms. For these individuals, the *IPBL* treatment is hard to interpret, as their benefit level changes immediately before/after receiving protection. As this concerns only 674 individuals, results hardly change from the main specification (see Figure A.7).

Fourth, we exclude individuals who moved between states in the month before receiving protection. This concerns 6,428 individuals. We suspect that these individuals already knew about the imminent granting of a protection status and moved preemptively. Figure A.8 shows that excluding these observations leaves the results practically unchanged.

Fifth, we examine the robustness of inference to alternative clustering levels. Figure A.10 shows 95% confidence intervals at six choices: commuting zone, district, district $\times$ year, state, state $\times$ year-of-protection (our headline), and a wild-cluster bootstrap to address Austria's small number of states (nine). As set out in Section 4.2.3, no single level is correct for both treatments. The two conservative choices, clustering on the nine states and the wild-cluster bootstrap, are the relevant stress test for *IPBL*, whose identifying variation comes from a small number of state-level reforms.

The four estimates respond differently to this test. The effect of *IVUR* on employment at short horizons is unaffected: the intervals exclude zero under every clustering choice, including the bootstrap. The other three are sensitive. For the effect of *IPBL* on employment, the state-clustered and bootstrap intervals include zero, while the finer clusterings leave it distinguishable from zero. The same pattern holds for the effect of *IPBL* on residency in the initial state, where the bootstrap interval is wide enough to contain zero. The effect of *IVUR* on residency is bounded away from zero under the finer clusterings but marginal under state clustering and the bootstrap.

We take two things from this. First, our headline result, the effect of initial labor market tightness on early employment, is robust to the most conservative inference we can apply. Second, the precision of the *IPBL* estimates turns on how much independent variation one attributes to the benefit reforms. Our headline clustering treats each state-year of protection as an independent draw, while the reforms are enacted at the state level and remain in force across several years, so the number of independent policy shocks behind $\beta_{2,t}$ is smaller than the number of clusters. Clustering on states themselves is not a remedy, since with nine clusters cluster-robust standard errors are unreliable (Section 4.2.3); the wild-cluster bootstrap, rather

²⁸In the main specification, we exclude individuals who received protection on the same day as they registered in Austria.

than the state-clustered intervals, is therefore our conservative benchmark.

These intervals also bound the effects from above, which is informative independently of the test against zero. The upper ends of the intervals in Figure A.10 limit how large the response to benefit generosity can be, and that limit is tighter under our headline clustering than under the bootstrap. The evidence thus constrains the magnitude of the *IPBL* effects even where it cannot separate a moderate effect from none.

To ensure a particular nationality or state does not drive our results, we re-estimate the effects by systematically excluding either one nationality at a time (Figure A.11) or one state at a time (Figure A.12) in our main specification. In the leave-one-nationality-out analysis, the estimated effects remain largely unchanged, indicating robustness across different nationalities. For the leave-one-state-out analysis, we modify our main specification by including an additional interaction of eligibility-for-family-benefits*state*type-of-protection. This adjustment is necessary since—otherwise—we would be using variation stemming from differences between refugee status (Convention vs. subsidiary protection) and eligibility for family benefits across states. Again, we see little change in our main results. If anything, the effects on mobility are larger when excluding Upper Austria (upper blue line) and are halved when excluding Vienna (lower blue line), likely due to the high concentration of refugees in this city.

Finally, we use separate month and year-of-protection fixed effects instead of year-month fixed effects. This modification does not alter the results meaningfully (see Figure A.9).

Overall, our point estimates are robust to these changes in the sample and specification, while their precision depends on the clustering choice discussed above.

6 Discussion and Policy Simulation

Several additional considerations are important for interpreting our findings, particularly regarding the timing of our analysis, the temporal horizon of welfare benefits, and the impact of housing policies.

One insight from this paper is that regional differences in welfare generosity and labor market tightness at the time of protection—when refugees are particularly mobile—explain only a minor share of the observed variation in the propensity to move to Vienna (see Figure 4). It is beyond the scope of this paper to assess the relative importance of other factors, but we would like to mention some candidates. One potentially important aspect is variation in housing policy. Federal states adopt very different approaches to supporting refugees with housing after they receive protection, and some evidence suggests that these differences are correlated with outmigration to Vienna (Dellinger, 2021).

There is also a clear east–west gradient: migration to Vienna is more common among refugees initially placed in eastern states, which are geographically closer to Vienna. While labor market conditions in the east are generally less favorable than in the west, our results suggest that these differences are not large enough to fully explain the observed migration patterns. Refugees in eastern regions likely face fewer barriers—both in terms of distance and cost, including through network effects—when considering a move to Vienna.

While a portion of our outcome window extends into the period affected by the COVID-19 pandemic, by controlling for the exact timing of protection status, we account for macro-level temporal shocks that are common across regions, including aggregate pandemic effects. It remains possible, however, that the pandemic may affect the external validity of our findings; outcomes might differ if the analysis were conducted in a different period or under different macroeconomic conditions. Importantly, it should be emphasized that the pandemic does not influence our estimates for the first 12 months after protection—the period where most of the observed effects occur—since our treatment window concludes in 2018. Thus, the core short-term results are entirely unaffected by pandemic-related developments.

The interpretation of our estimated effect sizes requires consideration of the variation in exposure to the policy change. Refugees living in private accommodation are directly affected by the reduction in benefit levels, as they experience immediate changes in disposable income. By contrast, individuals in organized accommodation receive almost all support in-kind. They are therefore not directly impacted by the reform, although the reduced benefit levels may still affect their decision-making if they consider transitioning to private accommodation. As we do not observe the type of accommodation at the individual level, our analysis necessarily mixes these types of exposure. Based on official statistics from the Grundversorgungsdatenbank,²⁹ approximately 35% of subsidiary-protected individuals outside Vienna reside in private accommodation. If we treat refugees in organized accommodation as fully unresponsive to benefit cuts—a strong assumption given the indirect channel just noted—our estimates for subsidiary-protected refugees scale by roughly a factor of three. This rescaling is therefore best read as a suggestive upper bound on the ATT for those directly exposed, since the in-kind group is unlikely to be entirely unaffected. For example, our standardized effect of a €300 reduction in benefit levels on the probability of staying in the assigned state for subsidiary-protected refugees is about -2.8 pp. at month 12; multiplying by three yields an ATT of -8.4 pp. The corresponding effect on employment is about +1.5 pp. at month 12, implying an ATT of +4.5 pp.

²⁹See <https://www.migration-infografik.at/gvs-statistiken-2018>, retrieved July 30, 2025.

Table 5: Counterfactual results of various IPBL levels on employment and mobility

Protection Status	Employed (%)				Moved to Vienna (%)			
	12 months		60 months		12 months		60 months	
	Base	CF	Base	CF	Base	CF	Base	CF
<i>a. IPBL in all states to Viennese levels</i>								
Overall	13.9	13.4	45.7	45.7	26.6	25.9	33.7	32.9
Convention refugees	11.7	11.6	43.6	43.6	22.8	22.5	30.7	30.5
Subsidiary-protected	20.4	19.1	52.1	52.1	37.5	35.4	42.0	39.7
<i>b. IPBL for subsidiary-protected set to the levels for Convention refugees in the state</i>								
Subsidiary-protected	20.4	19.3	52.1	52.1	37.5	35.8	42.0	40.1

Notes: The table shows counterfactual predictions for employment and residency in Vienna under alternative IPBL scenarios. In Scenario a, we set the IPBL levels for all protection types in each state to match the corresponding Vienna levels of each year. Column “Base” shows the baseline predictions for employment and staying in Vienna from our main specification. Column “CF” shows the counterfactual shares under the policy change. In Scenario b, we adjust the IPBL levels for subsidiary-protected refugees to equal those of Convention refugees within the same state and year. The sample includes only refugees who received protection in 2017.

Policy Simulation

We also use our estimates to conduct back-of-the-envelope simulations of two hypothetical policies by predicting the outcomes for specified IPBL levels.

First, we assess the effect of setting benefit levels in all federal states equal to those in Vienna. Panel (a) of Table 5 presents these results. This policy would slightly reduce employment rates after twelve months but has no effect after 60 months. It would lower the share of subsidiary-protected individuals who initially lived outside Vienna and later moved to Vienna by about two pp., while having almost no effect on Convention refugees, whose benefit levels were already more uniform across states.

Second, we assess the impact of aligning benefit levels for subsidiary-protected individuals with those of Convention refugees within each state. This policy — affecting only the subsidiary-protected — would reduce the employment rate by 1.1 pp. twelve months after protection and decrease the likelihood of moving to Vienna by slightly less than two pp. (see Panel (b)).

7 Conclusion

This study examines the interplay between local labor market conditions and welfare generosity on the labor market integration and internal mobility of refugees. Utilizing detailed register data, we analyze how refugees respond to the conditions they encounter when their choice set expands upon receiving protection. Many initially dispersed refugees relocate as soon as they

are allowed to do so.

We show that labor demand, particularly in occupations with low entry barriers, immediately and significantly increases employment among men. However, this effect fades after 2.5 years. At the same time, tighter labor markets raise the likelihood of staying in the assigned state throughout the five years we observe. Higher benefit levels decrease the likelihood of relocation but temporarily lower men’s employment levels and more persistently affect women’s employment, although the effect sizes are relatively small and less precisely estimated.

Examining relocation and employment jointly shows how the two margins combine. Tighter local labor markets raise the probability of being both employed and in the initial state, while higher benefit levels raise the probability of remaining in the initial state without employment rather than moving and being unemployed elsewhere. Overall, our findings suggest that mobility responses to initial labor-market conditions and welfare benefit levels are more reflective of short-term needs than long-term optimization regarding labor market prospects, consistent with findings on work-first policies (Arendt, 2022; Arendt and Bolvig, 2023).

Lastly, when examining the interaction between labor market tightness and benefit levels, we find that changes in welfare benefits have a stronger effect on employment rates when local labor markets are tight and are negligible otherwise, consistent with similar findings for Denmark (Dustmann et al., 2024).

From a policy perspective, our findings show that supporting refugees in finding employment at the moment of, or even before, receiving protection may prevent their migration to other regions. Additionally, they highlight the importance of considering the labor market conditions in the regions to which refugees are allocated when designing policies. Changes in benefits or welfare reforms aimed at incentivizing employment among refugees will only affect labor market integration in strong labor markets but might fail in weaker markets with potentially unintended consequences.

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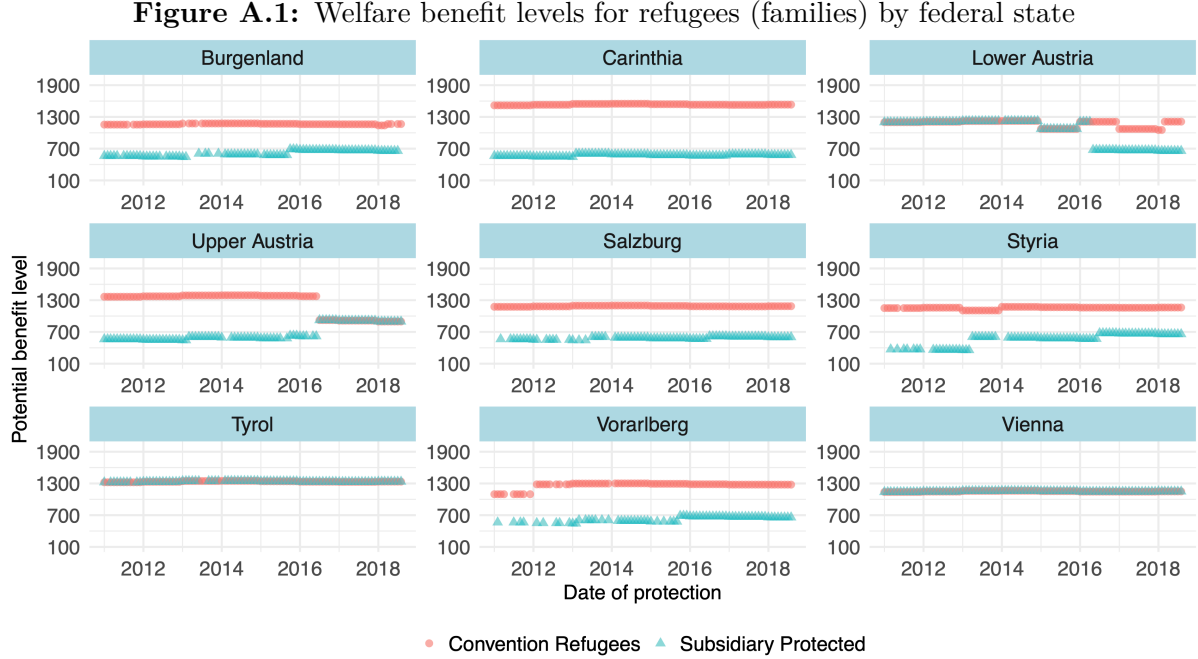
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Appendix

A Additional Tables and Figures

A.1 Welfare benefit levels for refugees (families) by federal state



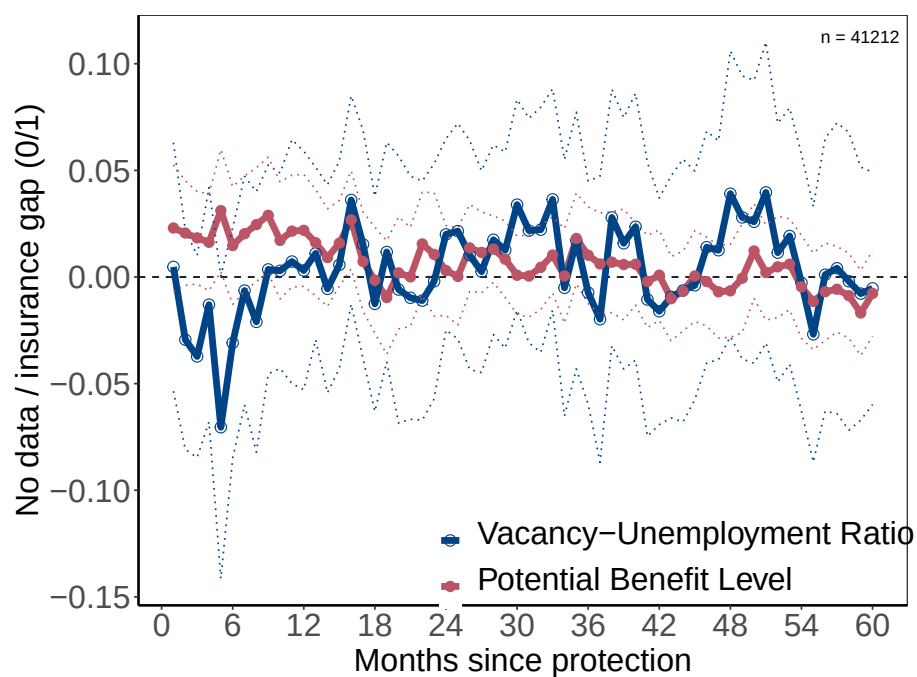
Notes: The figure shows the amount of monthly benefits in euros (excluding housing benefits) for families consisting of two adults and two children by federal state and type of protection.

A.2 Effect on likelihood of remaining in Austria

To test for potential sample selection, we evaluate whether *IVUR* and *IPBL* affect the likelihood of leaving Austria, indicated by a gap in social insurance coverage. Figure A.2 illustrates the impact of a one-unit rise in *IVUR* and a €1,000 increase in *IPBL* over time. It displays the month-specific coefficients $\beta_{1,t}$ and $\beta_{2,t}$ from our main model (Equation 3) for the initial 60 months post-protection.

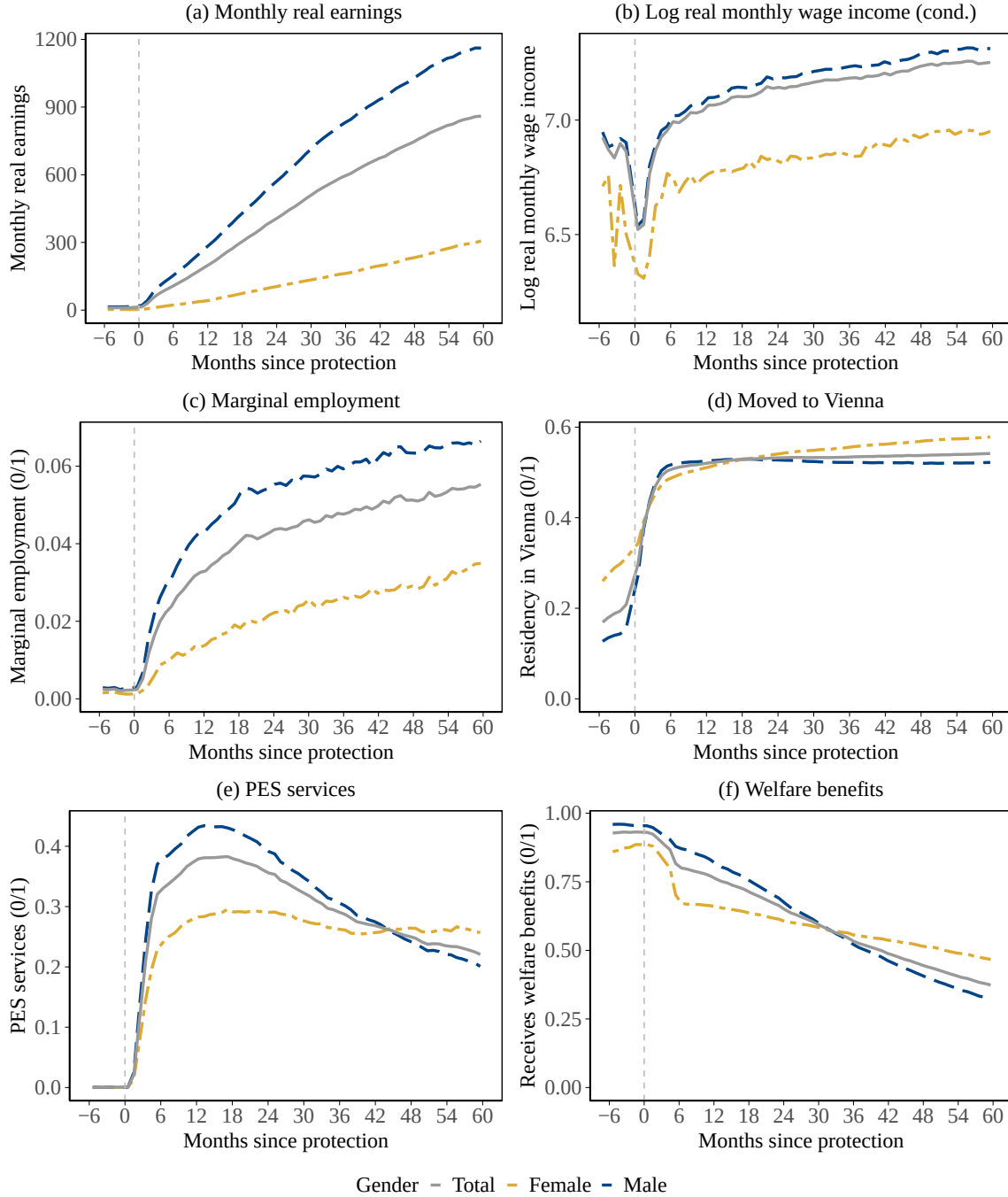
IVUR and *IPBL* have no effect on the likelihood of such a gap. In essence, higher labor demand or increased welfare benefits do not influence the decision to stay in or leave Austria. While interesting in itself, this provides reassuring evidence that our core treatments do not systematically affect aggregate sample attrition. We therefore proceed with our main analysis using the balanced cohort of 39,694 individuals observed for the full five years, noting that while differential compositional sorting remains theoretically possible, the net likelihood of remaining in Austria is orthogonal to our variables of interest.

Figure A.2: Effect of *IVUR* and *IPBL* on the probability of a gap in social insurance coverage in Austria



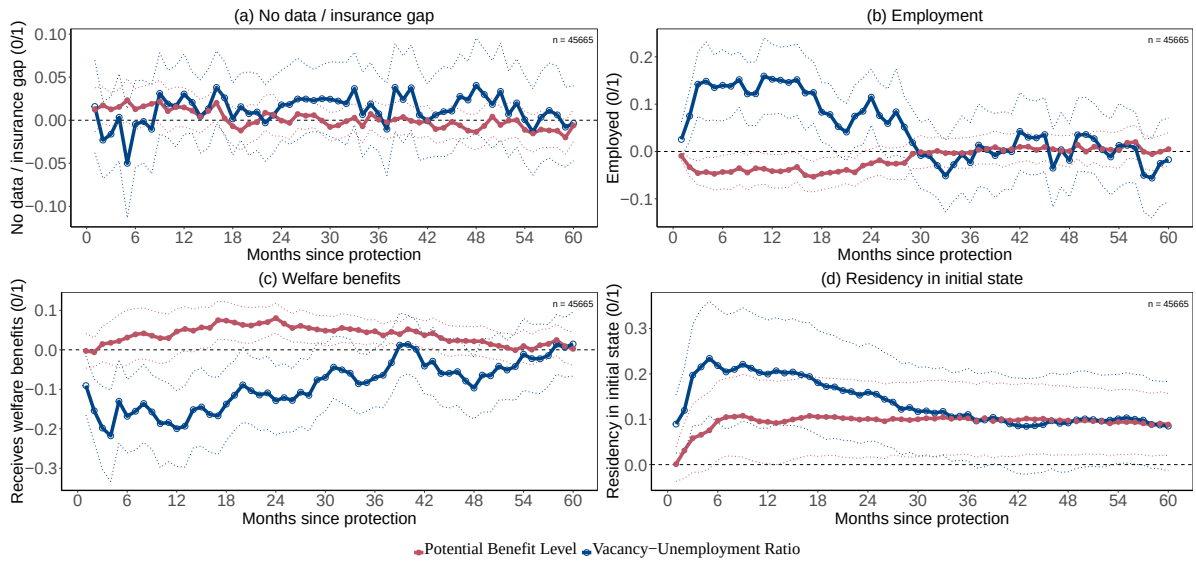
Notes: The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either i) the vacancy-to-unemployment ratio or ii) the potential benefit level, measured in €1,000 (real 2015 values) on having a gap in social insurance coverage. All regressions include a full set of arrival-group, year-month, and district-of-protection fixed effects. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin (see Equation 3). Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

Figure A.3: Shares and means of selected outcome variables for refugees by months since protection was received



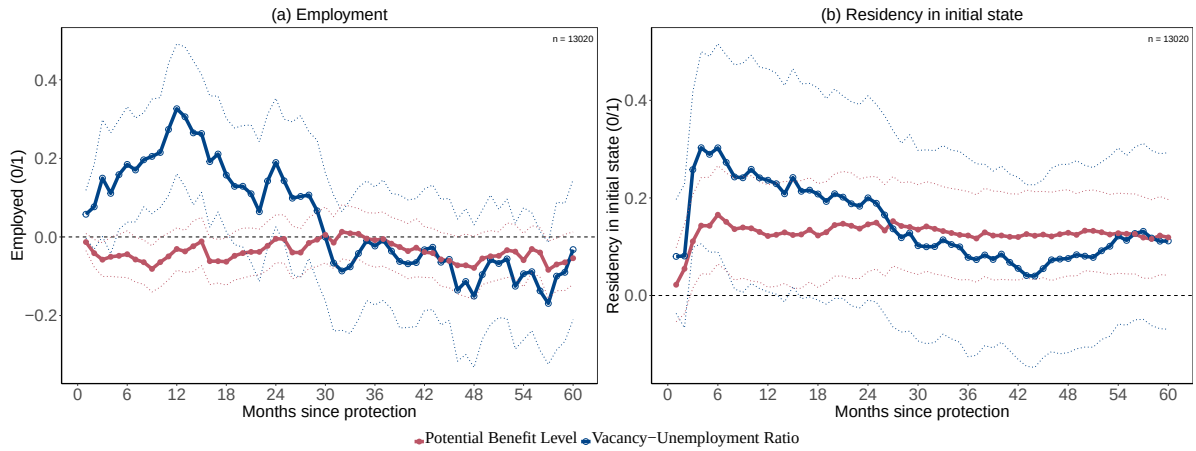
Notes: The x-axis shows the months since protection was received. The y-axis displays either the means of (a) monthly real earnings (in 2015€) and (b) log real monthly wage income (conditional on receiving any income) or the shares of refugees in (c) marginal employment, (d) residing in Vienna, (e) receiving support by the PES and (f) receiving welfare benefits, by gender.

Figure A.4: Effect of the *IVUR* and *IPBL* on selected outcomes: Protection granted until 2021



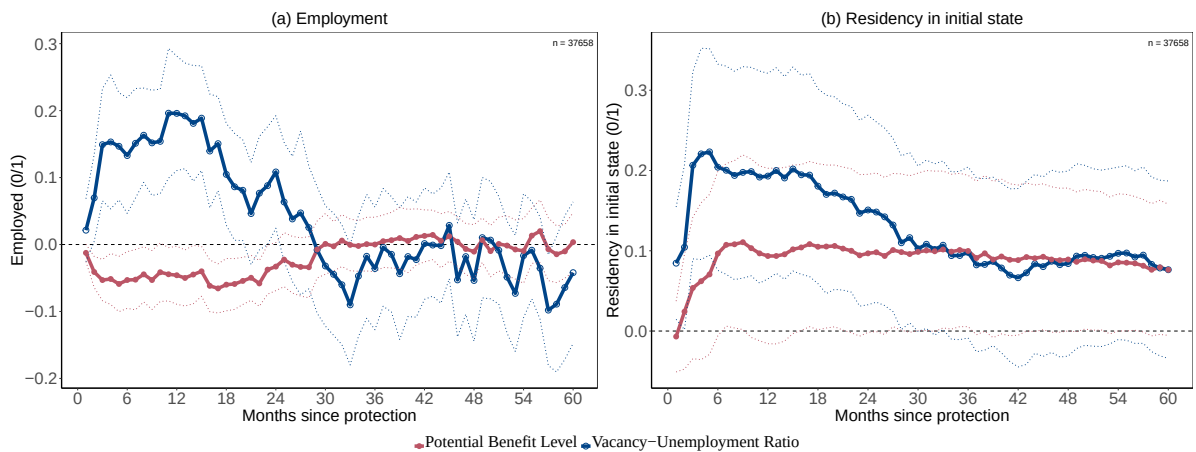
Notes: This figure shows selected main outcomes without placing restrictions on being observed for 60 months. The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either i) the vacancy-to-unemployment ratio or ii) the potential benefit level, measured in €1,000 (real 2015 values), on selected outcomes. The panels show the effects on (a) having a gap in social insurance coverage; panel (b) having any type of part- or full-time employment; panel (c) the probability of receiving welfare benefits; and panel (d) the probability of living in the initial state. All regressions include a full set of arrival-group, year-month, and district-of-protection fixed effects. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

Figure A.5: Effect of the *IVUR* and *IPBL* on employment and location choice for those who initially lived in a reception facility



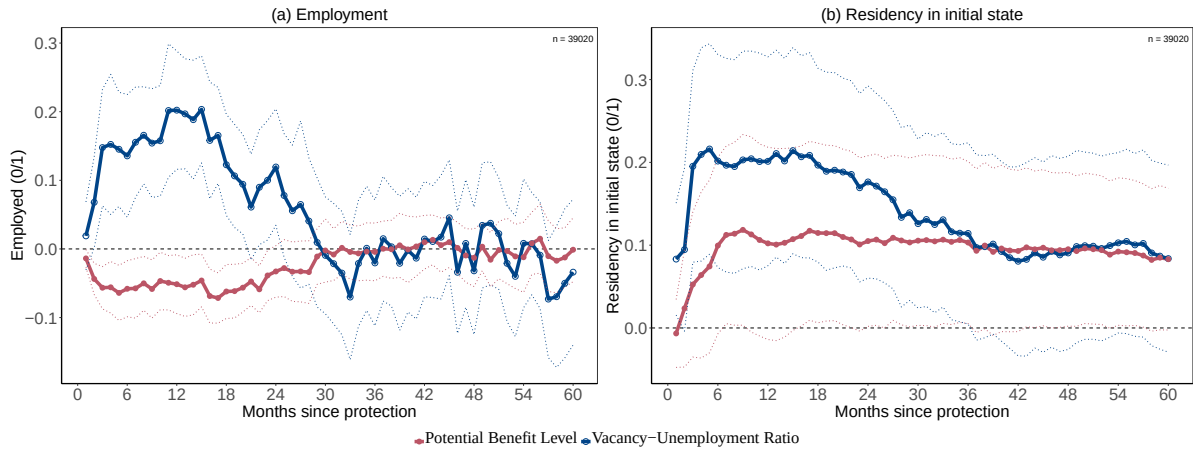
Notes: The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either i) the vacancy-to-unemployment ratio or ii) the potential benefit level, measured in €1,000 (real 2015 values), on employment (left) and mobility (right). All regressions include a full set of arrival-group, year-month, and district-of-protection fixed effects. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

Figure A.6: Effect of the *IVUR* and *IPBL* on employment and location choice for those with asylum processes longer than one month



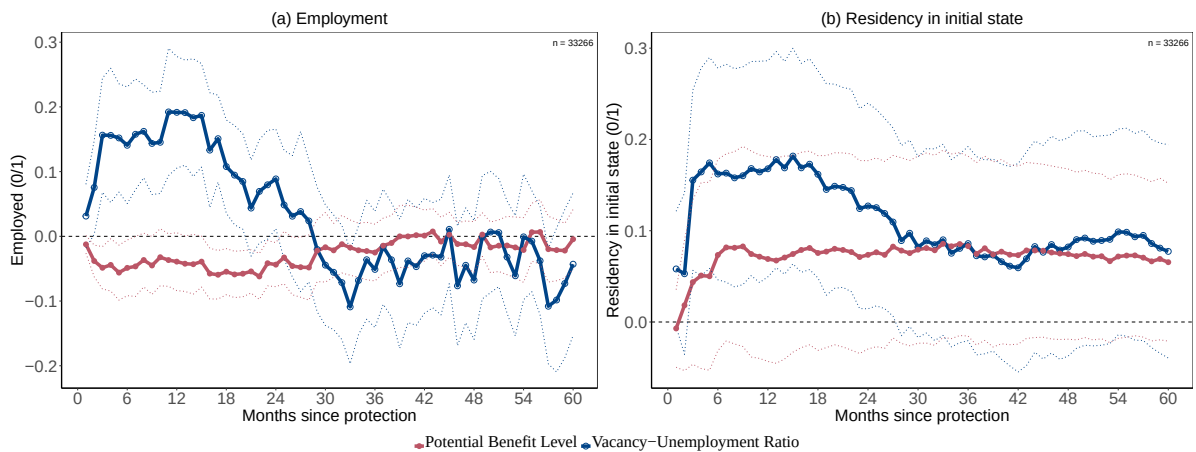
Notes: The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either i) the vacancy-to-unemployment ratio or ii) the potential benefit level, measured in €1,000 (real 2015 values), on employment (left) and mobility (right). All regressions include a full set of arrival-group, year-month, and district-of-protection fixed effects. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

Figure A.7: Effect of the *IVUR* and *IPBL* on employment and location choice excluding protection within four months of reform



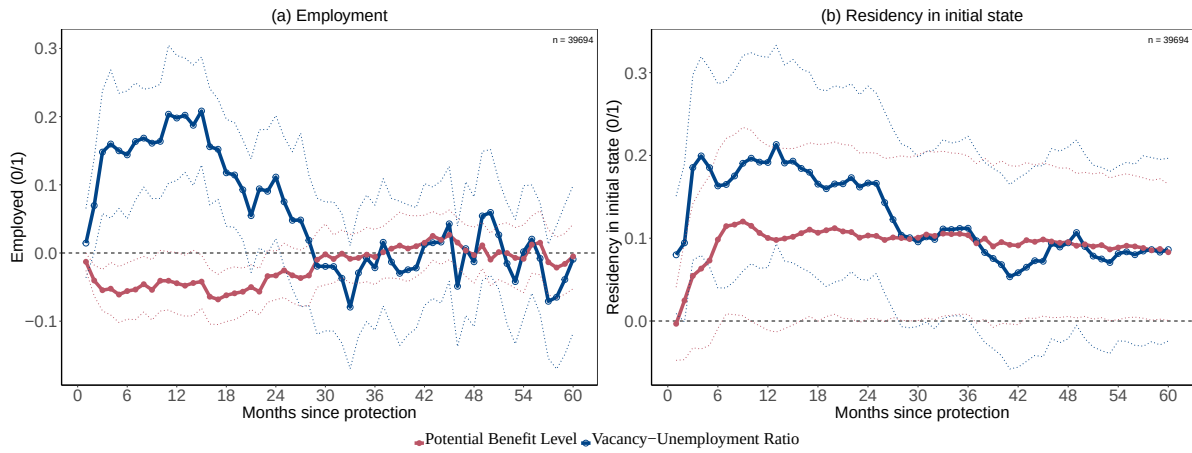
Notes: The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either i) the vacancy-to-unemployment ratio or ii) the potential benefit level, measured in €1,000 (real 2015 values), on employment (left) and mobility (right). All regressions include a full set of arrival-group, year-month, and district-of-protection fixed effects. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

Figure A.8: Effect of the *IVUR* and *IPBL* on employment and location choice for those who lived in their state at least one month prior to receiving protection



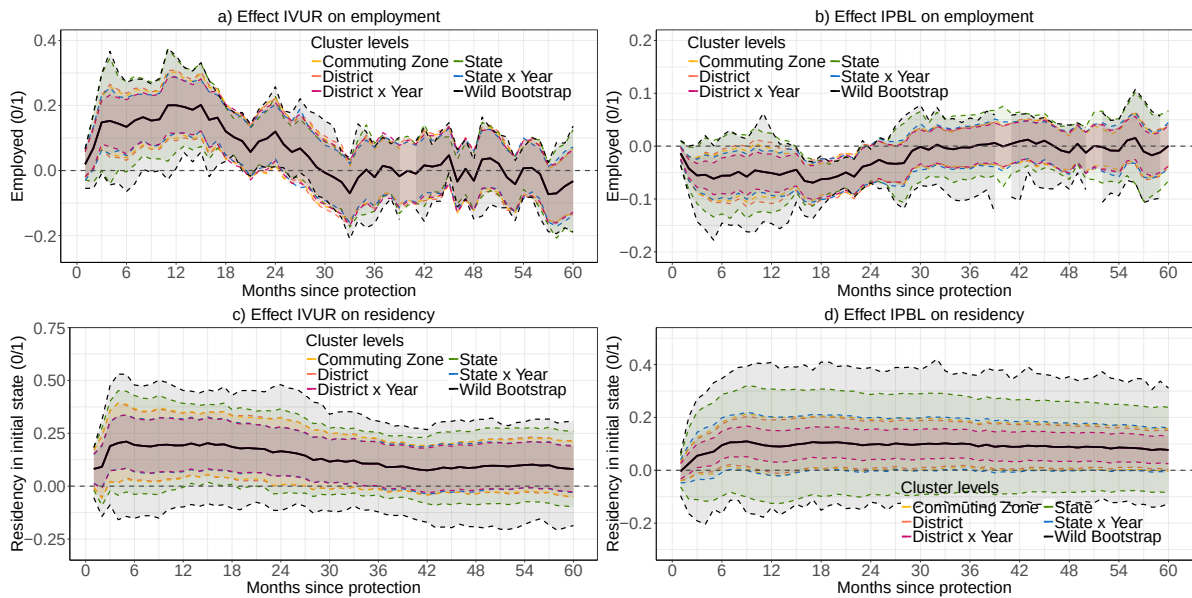
Notes: The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either i) the vacancy-to-unemployment ratio or ii) the potential benefit level, measured in €1,000 (real 2015 values), on employment (left) and mobility (right). All regressions include a full set of arrival-group, year-month, and district-of-protection fixed effects. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

Figure A.9: Effect of the *IVUR* and *IPBL* on employment and location choice with year and month fixed effects



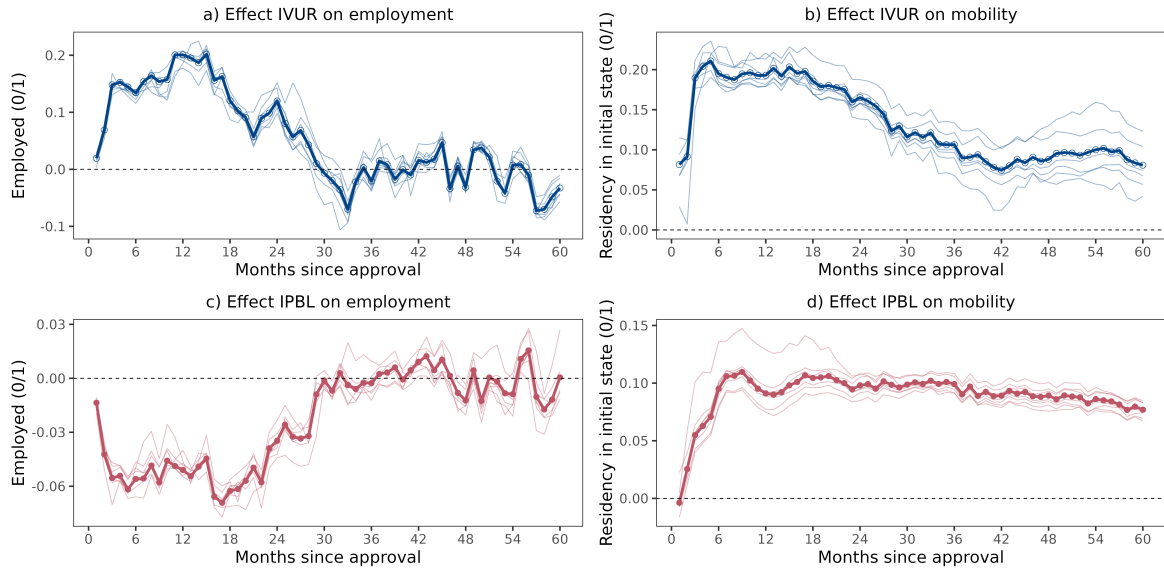
Notes: The x-axis shows the months since protection was received. The y-axis displays the effect of a one-unit increase in either i) the vacancy-to-unemployment ratio or ii) the potential benefit level, measured in €1,000 (real 2015 values), on employment (left) and mobility (right). All regressions include a full set of arrival-group, separate year-of-protection and calendar-month, and district-of-protection fixed effects. Additional control variables include age and age squared at immigration, type of protection, family status at protection date, and country of origin. Standard errors are clustered at the state-year-of-protection level. Dashed lines indicate 95% confidence intervals.

Figure A.10: Robustness: Different Standard Errors and Wild Cluster Bootstrap



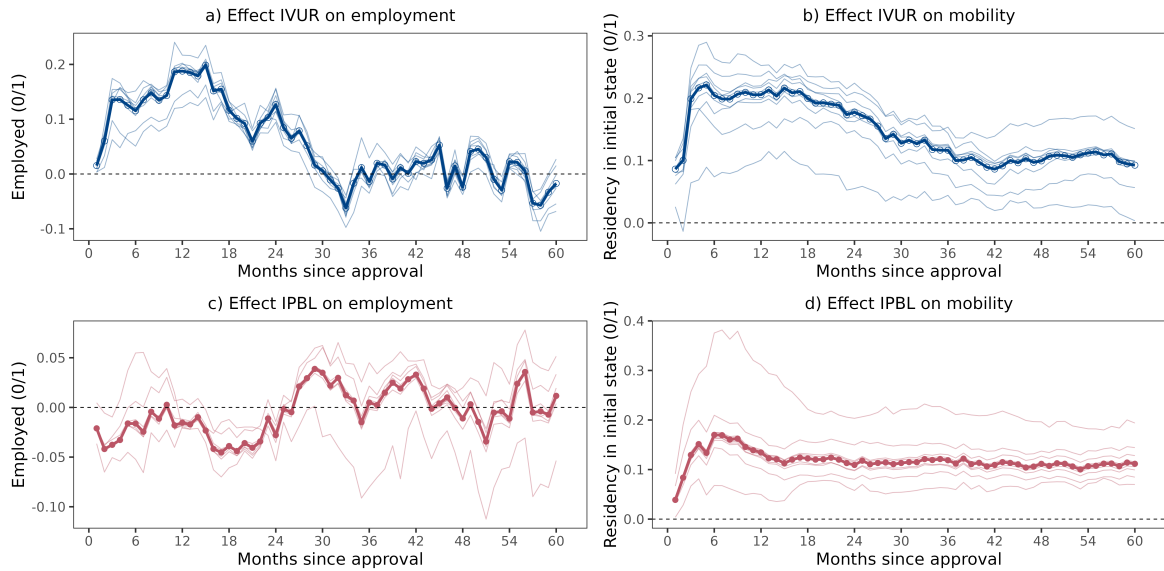
Notes: Comparison of the estimated effects of the vacancy-to-unemployment ratio (left) and the potential benefit level (right) on employment (top row) and residency in the state of protection (bottom row). Each plot shows the point estimates and confidence intervals using standard errors clustered at alternative aggregation levels (district, state, commuting zone, state $\times$ year, district $\times$ year), as well as wild cluster bootstrap intervals. Each dotted pair of confidence intervals corresponds to a different clustering level for the calculation of standard errors. Black lines indicate confidence intervals from wild cluster bootstrap procedures using Rademacher weights for the IVUR and Webb weights for the IPBL.

Figure A.11: Robustness: Leave-One-Nationality-Out Analysis



Notes: The thick lines correspond to the main results from Figures 5(a) and 6(a). Each thin line corresponds to re-estimating the main model after excluding one nationality group from the sample.

Figure A.12: Robustness: Leave-One-State-Out Analysis



Notes: The thick lines correspond to the main results from Figures 5(a) and 6(a). Each thin line corresponds to re-estimating the main model after excluding one state from the sample.

B Data Appendix

B.1 Sample selection: registrations at the PES

In our dataset, we only have information on the status granted and the date of asylum for those individuals who registered at the Public Employment Services (PES). Hence, there could be a concern that our shocks (*IVUR* and *IPBL*) correlate with the probability of registering at the PES and might bias our results.

Using our registry data (ASSD), we collapse the number of individuals by sex and citizenship living in district d at the moment of getting asylum by year of protection for 2011–2020. We keep only individuals from the top 8 asylum-applicant countries: Afghanistan, Iraq, Iran, Kosovo, Nigeria, Russia, Somalia, and Syria.

We merge these collapsed headcounts of refugees with the total population of foreigners from the top 8 asylum-applicant countries at the district level as of January 1 of the following year. These are data provided by STATcube by country of birth and gender and are stocks as of January 1 of each year. For example, we merge the headcounts by protection status of those individuals who received protection in 2015 with the total population of foreigners from these countries in a district as of January 1, 2016. We then divide our ASSD headcounts on refugees by status by the total population of individuals from these eight asylum-applicant countries at the district level to obtain a share of refugees with a status among the foreign population.

Additionally, we merge the yearly averages of *IVUR* at the district level and the ones from *IPBL* at the state level by year of protection. Finally, we run a regression of the share of refugees with status among the foreign population on our treatment variables, year, district, nationality, and type of protection fixed effects.

Table B.1 presents these results. The first three columns show the results for shares by nationalities as outcomes, while the latter three columns use the aggregated shares for all top 8 asylum-applicant countries as outcomes. The coefficients for the *IVUR* are positive but statistically not significant in columns (1)–(3). Since our dependent variable ranges from zero to one, a one unit increase in the *IVUR* would increase the share of (registered) refugees in a district by 2.4 pp. To put this in standard increase terms, as in our main analysis, a 0.2 units increase in the *IVUR* would increase the share of registered refugees by 0.4 pp., which is relatively small compared to the effect sizes we find in our main specification. In columns (4) and (5), where we use the overall shares as an outcome, the effect of the *IVUR* becomes statistically significant, at the 5% level for all refugees and at the 1% level for women. Among men, in column (6), the coefficient is smaller and remains insignificant, so this association is driven primarily by

Table B.1: Correlation of initial vacancy-to-unemployment ratio and initial potential benefit level with the share of refugees in a district

	(1)	(2)	(3)	(4)	(5)	(6)
	Shares by nationalities			Aggregated shares		
	All	Females	Males	All	Females	Males
IVUR	0.0237 (0.0313)	0.0329 (0.0217)	0.0423 (0.0358)	0.0303** (0.0143)	0.0365*** (0.0091)	0.0257 (0.0185)
IPBL - singles	0.0358 (0.0583)	-0.0372 (0.0324)	0.0458 (0.0559)	0.0229 (0.0364)	-0.0014 (0.0360)	0.0363 (0.0383)
IPBL - family	-0.0042 (0.0412)	0.0512** (0.0252)	-0.0144 (0.0388)	-0.0099 (0.0265)	0.0050 (0.0256)	-0.0175 (0.0280)
Year FE	X	X	X	X	X	X
District FE	X	X	X	X	X	X
Refugee status FE	X	X	X	X	X	X
Nationality FE	X	X	X	-	-	-
Observations	14,480	13,542	14,278	1,874	1,866	1,874
R2	0.11	0.10	0.10	0.53	0.51	0.49

Notes: Coefficients from OLS regressions of the share of refugees in the total foreign population of refugee-origin countries at the district level on the *IVUR* and *IPBL*. Columns (1)-(3) regress the shares by nationalities on the covariates and nationality fixed-effects. Columns (4)-(6) regress the aggregated shares for all top 8 refugee-origin countries on the covariates, without nationality fixed-effects. Since the regressions are at the district level, we include both benefits averages, by singles and families. Each share is calculated by country of origin and gender. The regressions include controls for year, district, nationality, and type of protection fixed-effects. Standard errors are clustered at the district-year-of-protection level. */**/** denote statistical significance at the 10/5/1 percent level.

women. This positive relation hints at a positive effect of *IVUR* on registration with the PES. If the individuals who register when the labor market is tight but not otherwise are individuals who are relatively more inclined to take up employment, our estimates for the effect of *IVUR* on employment might be upward biased. However, the magnitude of such a bias is minor.

For the *IPBL*, the only significant coefficient is the one for the family benefits on the share of registered females in column (2) but not when aggregating the shares across nationalities. Given the low share of singles and the low employment rates for women, they do indeed mostly qualify for family benefits. Thus, higher benefit levels might indeed have increased the incentive to register at the PES in order to receive benefits. These positive benefits effects on the registration of female refugees might bias our estimates of *IPBL* on benefit receipt upward and on employment downwards. However, since the effect is again small in magnitude, we consider the size of such potential biases again to be minor.

Overall, these associations are small. While the aggregate *IVUR* coefficient is statistically significant, the subgroup estimates reveal that this significance is concentrated entirely among women, with no significant effects observed for men. They could, however, indicate an upward bias of our effects.

B.2 Welfare benefits

This section provides a comprehensive overview of the welfare benefits system for vulnerable foreign nationals in Austria and explains the creation of the measure for the potential benefit level. The welfare system is administered by the federal states and differs significantly between states, time, protection status (Convention refugees, persons entitled to subsidiary protection, and displaced persons), and family status. During the analysis period, two sources of potential welfare benefits were relevant for refugees: *Grundversorgung* (basic subsistence support), and *Bedarfsorientierte Mindestsicherung* (BMS).

Possible welfare benefits are divided into housing support (recurring expenses for rent, heating, electricity, and other charges) and cost-of-living support (food, clothing, healthcare). Our measure of potential benefits, PB , uses the cost-of-living component only. We exclude housing support because it is not comparable across the systems, states, and accommodation types we observe. Under basic subsistence support, housing is a flat maximum rate paid only to refugees in individual accommodation, while those in organized accommodation receive housing in kind. Under minimum income support, the housing share is a fixed percentage of the state's minimum standard, generally 25 percent but 18 percent in Upper Austria, and several states cap it at district-specific maximum rents that reflect local housing policy. Restricting PB to cost-of-living support therefore yields a measure that is defined in the same way across states, systems, and years.¹

B.2.1 *Grundversorgung* (Basic Subsistence Support)

Basic subsistence support provides aid to cover basic needs for vulnerable individuals during the ongoing asylum process and for some groups also after the asylum process. The *Grundversorgungsvereinbarung* under Article 15a of the Federal Constitutional Law (B-VG) between the federal government and the states was introduced on May 1, 2004. The law specifies the eligible persons and the types of support services and also defines uniform admission conditions and maximum cost rates for the entire country. This agreement ensures that basic subsistence support is implemented similarly across Austria, while still leaving the specific implementation to the federal states.

Eligible Persons. According to the *Grundversorgungsvereinbarung*, the following vulnerable and protected foreigners are eligible for basic subsistence support:

¹The Austrian welfare system includes a range of other support measures, such as child allowance, care allowance and others. Those also partly differ between states. We do not consider these measures as part of our benefit level, as they did not change with the benefit reforms we consider. Variation in these measures that is constant within districts or common across the country is absorbed by our fixed effects.

- Asylum seekers until the final conclusion of the process
- Persons entitled to subsidiary protection
- Convention refugees during the first four months after asylum recognition
- Persons with a legally negative outcome of the asylum process and persons without a residence permit if they are not deportable for legal or factual reasons
- Persons with a specific residence permit for reasons worth considering (displaced persons)

For the context of this paper, it is worth highlighting that basic subsistence support is the respective welfare system for subsidiary-protected but not Convention refugees. Those switch to a different welfare system after being granted the protection status. This change happens within four months of receiving protection.

Income limits. Basic subsistence support may be terminated or restricted if the income exceeds €110.

Housing Support The basic subsistence support system has two forms of accommodation support: individual and organized living.

- **Individual living:** Rent allowance for individuals or families, food allowance for adults, minors, or unaccompanied minors.
- **Organized living:** Meals are either provided by the shelter or individuals receive a food allowance, max. €40 pocket money/month, max €10 recreational money/month.

Additional services may be provided depending on needs and situation (school supplies max. €200/year, transport costs, funeral costs, information and counseling, clothing assistance max. €150/year, pocket money). The underlying data record basic subsistence support for food and rent in individual accommodation per person and month; *PB* uses the cost-of-living component only.

B.2.2 *Bedarfsorientierte Mindestsicherung BMS* (minimum income support)

In 2010, Austria introduced the “Bedarfsorientierte Mindestsicherung” aimed to prevent poverty and social exclusion. Individuals have a general legal entitlement to minimum income support if they cannot cover their basic needs through their own means or priority social benefits (pensions, unemployment benefits, etc.). However, existing income and assets must be used up before

accessing minimum income support. In addition, people must show a willingness to take up employment.

Eligible Persons Convention refugees are entitled to minimum income support from the time they are granted protection status.² In Tyrol and Vienna persons entitled to subsidiary protection receive additional benefits from minimum income support. Thus, benefit levels for subsidiary-protected and Convention refugees do not differ.

Components of minimum income support The minimum income support consists of two parts: a basic amount to cover cost of living and a share to cover housing costs. Monthly rates are set in the laws of the federal states. The monthly cash benefit for single persons is based on the compensatory allowance reference rate for single persons in pension insurance. This value also serves as a reference value for the minimum standards of other population groups (adults, minors). Minimum income support is calculated at the household level for multiple-person households.

Housing Support The level of housing support is regulated differently in the various state execution laws. Generally, 25% of the minimum standard is earmarked for housing costs, and 18% in Upper Austria. If the actual housing costs exceed this amount, the standard rate can be increased to 30%. These cash benefits may be limited upwards by set rent rates. For example, in Tyrol, housing support is based on the number of people in the household and the district to counteract higher local housing costs. In areas where differentiated housing costs apply, the benefits are calculated according to the set guidelines for the respective number of persons in the household and the specific district. In other regions where no differentiated housing benefits apply, the percentage minimum standard for housing support benefits for singles and families is calculated based on the initial amount.

Labor market and additional income limits Individuals are only entitled to minimum income support if they cannot cover their basic needs through their own means, benefits with higher priority (e.g., unemployment benefits), their own labor, or the use of income and assets. The exempt asset amount is about €5,270. Minimum income support has a higher earnings limit and smoother phase-in rules than basic income support. The specific amounts depend on prior employment and the duration of benefit receipt.

²They need to move out of basic income support within four months after receiving protection.

B.2.3 Reforms of the benefit system

Major welfare benefit reforms occurred from 2016 onward in Lower Austria, Upper Austria, and Burgenland, as described by Huber and Dellinger (2022). Based on Table A5 of their working paper, we summarize here the most relevant aspects of the reforms:

1. **Lower Austria:** In April 2016, the state revoked minimum income support (BMS) eligibility for **subsidiary-protected refugees**. For example, for a single adult, benefits were reduced by 56% (from €838 to €365).
2. **Upper Austria:** In July 2016, the state lowered the minimum income standard **for both subsidiary-protected and Convention refugees** who came to Austria after November 15th, 2015. The new minimum income standard was €522. In November 2018, the European Court of Justice revoked the reform.
3. **Lower Austria:** In January 2017, the state introduced a new minimum income standard for **Convention refugees** residing in Austria for less than 5 of the past 6 years. Single adults were eligible for €572.50, while families and people living in a shared flat got a cap of €1,500 regardless of household size. The Constitutional Court revoked the reform in March 2018.
4. **Burgenland:** In April 2017, the state introduced a new minimum income standard for **Convention refugees** residing in Austria for less than 5 of the past 6 years. Single adults were eligible for €585, while families and people living in a shared flat got a cap of €1,500 regardless of household size. The Constitutional Court revoked the reform in March 2018.

Because eligibility under the Upper Austrian reform depends on the date of arrival, we use the exact arrival date recorded in our data to assign the new standard only to refugees who arrived after November 15, 2015. The residence-duration conditions in the Lower Austrian and Burgenland reforms are satisfied by construction in our sample, since we exclude individuals who received protection more than three years after arrival.

C Use of Artificial Intelligence

In preparing this manuscript we used large language models, specifically Claude Code (Opus 5) and refine.ink, to check the text for internal consistency and to improve the writing. The consistency checks covered agreement between the text, tables, and figures, including reported signs, magnitudes, and variable definitions, as well as the consistency of notation and cross-references across sections. The language editing covered rephrasing and tightening of existing

prose. We also used large language models to produce first drafts of individual passages, which the authors then revised and approved. All output was reviewed and revised by the authors, who take full responsibility for the content of the paper. Large language models were not used to construct the data or to produce any of the reported estimates.