The Impact of Arriving During Recessions on Immigrant Integration*

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November 14, 2026

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Temporary labor market conditions at arrival may have lasting consequences for immigrants' economic integration. I study this question using Canadian administrative data that link immigrant landing records to tax returns and employer information. To identify unexpected labor market conditions that are plausibly orthogonal to immigrant selection, I exploit provincial initial unemployment rate (IUR) shocks, defined as the difference between realized unemployment rates at arrival and their one-year-ahead predictions. A one percentage point (p.p.) increase in the shock reduces employment by 1.7 p.p. and annual earnings by 6.5% upon arrival, with effects remaining significant for five and six years, respectively. Employer linkages show that affected immigrants initially work at firms with lower wage premiums and partly recover through moving to higher-paying firms. Adverse entry conditions also increase interprovincial mobility. Observed switchers experience smaller losses than stayers, although Lee bounds accounting for treatment-induced mobility cannot distinguish adjustment from selection. Economic immigrants are significantly less affected than family-sponsored immigrants and refugees. Finally, parents' exposure to adverse entry conditions reduces their children's adult income, providing evidence of intergenerational scarring.

JEL Classification: E32, J15, J31, J61, J62

Keywords: Immigrant integration; Initial unemployment rate shocks; Scarring effects; Firm wage premiums; Job and geographic mobility; Intergenerational scarring

*I am extremely grateful to Philip Oreopoulos, Kory Kroft, and David Price for their guidance and support. I am also grateful to Carolina Arteaga, Michael Baker, Runjing Lu, Robert McMillan, Fern Ramoutar, Michael Stepner, Tianyi Wang, and Roman Zarate for their comments and suggestions; and to seminar participants at the Canadian Economic Association Conference 2025; the Chinese Economists Society 2026 North America Annual Conference; and the University of Toronto student workshops. I acknowledge the use of the Toronto Research Data Center for the completion of this research. All views expressed in this work are my own.

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1. Introduction

Immigrants' economic integration, particularly their employment and earnings trajectories in the host country labor market, is a central concern for both immigrants and host countries. In the context of population aging and declining fertility, immigrants are an increasingly important source of labor supply in OECD countries (Spielvogel and Meghnagi, 2018; OECD, 2025), and many host countries actively recruit skilled immigrants to sustain economic growth. Unsuccessful integration, however, can generate substantial economic and fiscal costs through lower tax revenues, greater reliance on government transfers, higher return migration, and persistent labor shortages.

Understanding the determinants of immigrant integration is therefore of first-order policy importance. One potentially important determinant is the labor market conditions immigrants face upon arrival. Entering during a recession reduces the probability of receiving job offers and increases the likelihood of accepting lower-quality jobs (Okun et al., 1973; McLaughlin and Bils, 2001). Although these temporary disadvantages may be mitigated through subsequent job mobility, a growing literature shows that unfavorable entry conditions can have persistent effects on workers' careers through mechanisms such as increasing job search frictions with age (Oreopoulos et al., 2012) and slower human capital accumulation early in the career (Kahn, 2010). Moreover, persistent earnings losses experienced by parents may spill over to the next generation by reducing investments in children's human capital, leading to worse labor market outcomes in adulthood (Oreopoulos et al., 2008; Coelli, 2011; Dahl and Lochner, 2012).

This paper examines how arriving during economic recessions affects immigrants' economic integration, focusing on Canada. Canada provides an ideal setting because it is one of the world's largest immigrant-receiving countries. In 2021, 23 percent of the Canadian population was foreign-born, nearly ten percentage points higher than the corresponding share in the United States. Immigration continues to expand as a policy response to population aging and low fertility.[1] In 2022 alone, Canada admitted 437,539 new permanent residents, accounting for almost all labor force growth, and immigration is projected to account for all population growth by 2032.[2]

To study this question, I primarily use the Canadian Longitudinal Immigration Database

[1] According to Statistics Canada, the growth rate of the population aged 65 and older was six times that of children aged 0–14 in 2021.

[2] Source: Immigration, Refugees and Citizenship Canada, *2023 Annual Report to Parliament on Immigration*.

(IMDB), which links immigrants' landing records to annual tax returns and follows their labor market outcomes throughout their careers. Importantly, the IMDB records immigrants' intended locations of destination and landing years. The IMDB also links immigrants to their foreign-born children who arrived in Canada before age 18, allowing me to examine intergenerational effects for the 1.5 generation. To investigate firm sorting and employer characteristics as mechanisms, I further link the IMDB to the Canadian Employer-Employee Dynamics Database (CEEDD).

My analysis focuses on male immigrants aged 18–54 at landing who arrived between 1982 and 2012 and had no prior Canadian work or study experience.[3] I exclude immigrants selected through Quebec's independent immigration system and provincial nominees to ensure that all immigrants in the sample are selected at the federal level. I also exclude immigrants admitted through business immigration categories, whose admission criteria and labor market attachment differ from those of other immigrants. These restrictions yield a final sample of approximately 1.1 million immigrants. The sample consists of economic immigrants (56.20%), who are selected primarily through a points-based system emphasizing human capital and labor market adaptability; family-sponsored immigrants (29.93%), who are admitted based on family ties to Canadian citizens or permanent residents; and refugees (13.88%), who are admitted on humanitarian and protection grounds.

The central empirical challenge is that immigrants' initial labor market conditions are not randomly assigned. Immigrants choose both when and where to migrate, making their arrival timing and destination potentially endogenous to local labor market conditions. A key feature of the immigration process, however, is the substantial delay between the initial migration decision and actual admission. During the study period, processing times typically exceeded one year. While applicants may choose to migrate based on expected labor market conditions when they submit their applications, they do not observe the realized conditions that prevail when they eventually arrive. Moreover, once permanent residence is approved, immigrants typically face only a limited period in which to complete the landing process, leaving little scope to adjust arrival timing in response to contemporaneous labor market fluctuations.

I exploit this institutional feature by defining initial unemployment rate (IUR) shocks as the difference between the realized unemployment rate at arrival and its one-year-ahead prediction. Because predicted unemployment captures the labor market conditions anticipated

[3]Restricting the sample to males yields a more homogeneous population with stronger and more stable labor force attachment and makes the analysis more comparable to the literature on the scarring effects of entering the labor market during recessions.

by immigrants when making their migration decisions, IUR shocks isolate the unexpected component of labor market conditions that is plausibly orthogonal to immigrant selection. To mitigate concerns that national immigration policy responds endogenously to aggregate business cycle conditions, identification relies on variation in IUR shocks across intended destination provinces within landing cohorts. Immigrants declare their intended province of destination at the beginning of the application process. Given the long processing times, these intended destinations are determined well before the realization of labor market conditions at landing and are therefore plausibly exogenous to contemporaneous IUR shocks. Predicted provincial unemployment rates are constructed by pooling forecasts from seven statistical prediction models, ranging in complexity from a simple one-year-lag benchmark to a neural network.

I assess the validity of this identification strategy using two sets of balance tests. First, provincial IUR shocks do not predict immigrant inflows. This finding is consistent with migration decisions being largely determined before the shocks are realized and provides no evidence that applicants systematically abandon their migration plans in response to unexpected labor market deterioration. Second, provincial IUR shocks are unrelated to immigrants' observable characteristics at landing. Together, these results support the identifying assumption that unexpected provincial labor market conditions are orthogonal to immigrant selection.

To implement this identification strategy empirically, I estimate the effect of IUR shocks at each year since arrival, controlling for predicted unemployment rates, landing-year fixed effects, destination-province fixed effects, and individual characteristics. Conceptually, this is a generalized difference-in-differences design, with destination provinces and landing years as the two dimensions of comparison and IUR shocks as a continuous treatment. Controlling for predicted unemployment accounts for immigrant selection based on anticipated labor market conditions, while IUR shocks capture the causal effects of unexpected labor market conditions at arrival. Because permanent residents are generally free to move across provinces after landing, the estimated effects should be interpreted as intention-to-treat effects rather than the effects of continued exposure to local labor market conditions.

The analysis yields four main findings. First, adverse initial labor market conditions have substantial and persistent effects on immigrant employment and earnings. A one percentage point increase in the initial unemployment rate (IUR) shock reduces immigrants' employment by 1.7 percentage points and annual earnings by 6.5 percent upon arrival. Although both employment and earnings gradually recover, the employment effects remain statistically

significant for five years and the earnings effects for six years after arrival. These findings are robust to alternative specifications, sample restrictions, and definitions of employment. They also imply larger employment losses than those reported by Lucchetti and Ruggieri (2025), who identify the impact of adverse landing conditions on immigrants in the U.S., while the estimated earnings effects are somewhat larger upon arrival but similarly converge to zero within ten years.

Second, employer sorting and subsequent job mobility help explain the evolution of earnings losses. To investigate these mechanisms, I use the linkage between the IMDB and CEEDD to observe immigrants' employers directly. Immigrants arriving under worse labor market conditions initially sort into firms with lower AKM wage premiums and higher shares of immigrant workers. Differences in firm wage premiums account for only 13% of the initial earnings penalty, leaving most of the penalty attributable to within-firm earnings differences. These differences may reflect lower wage rates, reduced time worked, occupational downgrading, or a combination of these mechanisms. Additional evidence from firm-level hiring suggests that higher-wage-premium firms reduce hiring more sharply during recessions, potentially contributing to this initial sorting pattern. Adverse entry conditions also sort immigrants into lower-paying industries. Affected immigrants are more likely to change employers in their first year after arrival and, among those who switch, exhibit a greater tendency to move toward higher-wage-premium firms in subsequent years. These patterns suggest that job mobility may contribute to the gradual recovery in firm wage premiums and earnings.

Third, geographic mobility provides another potential adjustment margin. Adverse entry conditions increase the probability that immigrants reside outside their intended destination province, and observed switchers experience smaller employment and earnings losses than stayers. Because initial IUR shocks are correlated with subsequent labor market conditions in the destination province, settling elsewhere or subsequently moving away may limit continued exposure to adverse local conditions. This comparison is not causal, however, because mobility responds endogenously to the initial shock and movers may be positively selected. I therefore adapt the Lee-bounds approach of Lee (2009) to account for treatment-induced mobility. The bounds continue to show negative short-run effects for always destination residents, but those for always non-destination residents are wide because treatment-induced switchers constitute a sizeable share of observed switchers. Consequently, the analysis cannot determine whether the more favorable outcomes of observed switchers reflect a protective effect of geographic mobility or differences in their composition.

Fourth, the effects of adverse entry conditions vary substantially across immigration classes. Economic immigrants experience significantly smaller employment and earnings losses than family-sponsored immigrants and refugees. Although immigrants with at least a bachelor's degree experience smaller employment losses, allowing the effects of IUR shocks to vary by educational attainment leaves the estimated differences across immigration classes largely unchanged. I also find little evidence of heterogeneity by English-language proficiency. These findings suggest that the greater resilience of economic immigrants may reflect selection on broader dimensions of labor market adaptability that are not fully captured by educational attainment or English-language proficiency. Firm-level sorting points to enclave networks as another potentially important distinction. Family-sponsored immigrants exposed to adverse entry conditions are more likely to work in firms with higher shares of co-national coworkers, particularly when they arrive in provinces with larger pre-existing enclaves. Greater enclave access does not, however, attenuate their employment losses and is associated with larger earnings losses and greater sorting into firms with lower wage premiums. These findings suggest that enclave networks shape where family-sponsored immigrants find employment during downturns but do not necessarily protect them from scarring.

Additional heterogeneity emerges along other dimensions. Older immigrants experience larger employment and earnings losses, whereas immigrants from economically advantaged origin countries are less affected by adverse entry conditions. Female immigrants experience larger initial employment losses and more persistent earnings losses than male immigrants, although most of the estimated gender differences are not statistically significant.

Beyond these four main findings, I examine the broader consequences of adverse entry conditions for immigrant integration. Immigrants arriving during recessions are more likely to receive government transfers and Employment Insurance benefits, delay childbearing, and become Canadian citizens. I find little evidence that adverse entry conditions affect health outcomes or language use. Finally, by linking immigrants to approximately 467,000 of their foreign-born children who arrived in Canada before age 18, I show that temporary labor market conditions at fathers' arrival have persistent intergenerational consequences. While fathers' IUR shocks have little effect on children's employment, they reduce children's income measured between ages 30 and 34 by approximately one percent, with the effects concentrated among children of family-sponsored immigrants.

This paper contributes to the literature on the scarring effects of adverse labor market conditions at labor market entry. Most existing studies focus on native workers and document persistent negative effects of entering the labor market during recessions, including male

college graduates (Kahn, 2010; Oreopoulos et al., 2012), new PhD economists (Oyer, 2006), and broader cohorts of young labor market entrants (Schwandt and Von Wachter, 2019; Wachter, 2020). To facilitate a direct comparison with the native literature, I replicate the analysis on a sample of immigrants that closely matches the sample in Oreopoulos et al. (2012) except for immigration status. I find that immigrants experience substantially larger employment and earnings losses than comparable native college graduates.

This paper also contributes to the growing literature on the effects of arriving during recessions on immigrant integration. Existing studies differ in the immigrant populations they examine, the data available, and the identification strategies they employ. Åslund and Rooth (2007) are among the first to study refugees using the Swedish refugee placement policy. Barsbai et al. (2025) examine family-sponsored immigrants in the United States using repeated cross-sectional Census data, while Vigezzi (2025) studies Government-Assisted Refugees in Canada using the quasi-random assignment of refugees across locations. Lucchetti and Ruggieri (2025) examine the overall immigrant population in the United States using initial unemployment rate (IUR) shocks similar to those in this paper. However, their data do not identify immigrants' admission categories or distinguish temporary from permanent residents at arrival, limiting their ability to restrict the sample to immigrants whose migration decisions are made well in advance of arrival. Collectively, these studies document persistent earnings losses but provide mixed evidence on employment effects.

My paper extends this literature in several ways. Using population-wide longitudinal administrative data from Canada, I study all three major immigration classes within a unified institutional setting and identification framework. The data allow me to restrict the sample to permanent residents without prior Canadian work or study experience, observe their intended destinations, follow their labor market outcomes over time, and explicitly account for tax non-filing and potential selective attrition. Beyond estimating the average scarring effects, I provide, to my knowledge, the first causal evidence on the effects of adverse entry conditions for economic immigrants and document substantial heterogeneity across immigration classes.

This paper further contributes to the literature emphasizing the importance of firm heterogeneity for labor market outcomes. Recent work shows that firm heterogeneity is central to understanding the earnings consequences of job displacement, domestic outsourcing, and other labor market shocks (Goldschmidt and Schmieder, 2017; Lachowska et al., 2020; Schmieder et al., 2023). In the context of immigrant integration, Dostie et al. (2023) and Lehrer and Rawling (2025) show that firm heterogeneity explains immigrants' earnings assimilation, while Kroft et al. (2026) demonstrate the importance of firm heterogeneity for

the returns to permanent residency. By linking immigrants to their employers, I provide the first evidence that adverse entry conditions initially sort immigrants into lower-paying firms and firms with higher shares of immigrant workers, highlighting firm sorting as an important mechanism underlying immigrant labor market scarring.

Finally, this paper contributes to the emerging literature on the intergenerational consequences of labor market scarring. De Vera et al. (2026) and Spencer and Matsuzawa (2025) study native families and reach mixed conclusions regarding the transmission of parents' labor market entry conditions to the next generation. Among immigrant studies, Vigezzi (2025) finds little evidence of intergenerational effects, which she argues may reflect extensive secondary migration. By linking immigrants to more than 467,000 of their foreign-born children who arrived before age 18, I provide the first evidence on the intergenerational consequences of adverse entry conditions for the overall immigrant population.

The remainder of the paper is organized as follows. Section 2 describes the institutional background. Section 3 introduces the data and sample selection. Section 4 constructs and validates the IUR shocks. Section 5 illustrates the identification strategy using a simple two-by-two difference-in-differences design and presents the empirical specification. Section 6 presents the four main findings and additional results. Section 7 concludes.

2. Institutional Background

In the early twentieth century, Canada enforced immigration restrictions based on race and nationality. Since the introduction of the points-based system in 1967, however, immigrant selection has been based on individual characteristics, with race and nationality no longer explicitly considered. Immigrants are admitted under three broad categories: economic immigrants, family-sponsored immigrants, and refugees. Today, Immigration, Refugees and Citizenship Canada (IRCC), formerly Citizenship and Immigration Canada (CIC), is the federal authority responsible for administering Canada's immigration system. Although immigrant selection is largely determined at the federal level, provincial governments participate in selection through Provincial Nominee Programs, and Quebec exercises broader selection authority under its own immigration system. In particular, Quebec selects economic immigrants and resettled refugees destined for the province, while the federal government retains selection authority over family-sponsored immigrants and protected persons. Since the 1980s, Canadian immigration policy has increasingly emphasized economic immigration. To further accelerate the processing of skilled workers, the Express Entry system, an electronic points-based selection mechanism, was officially launched in January 2015. Each year, IRCC

sets national immigration targets in consultation with provinces, territories, and stakeholders, specifying minimum and maximum admissions by detailed class, with the exception of Quebec, which maintains its own immigration plan. These targets, however, are not legally binding and may deviate from realized admissions in response to economic or administrative shocks.

The application process differs across the three major admission categories. Prior to the introduction of Express Entry, permanent-residence (PR) applications were generally processed on a first-come, first-served basis. The PR application process consists of two main stages: selection and admissibility screening. Economic immigrants are selected based on merit, evaluated using criteria such as age, education, language proficiency, work experience, and, where applicable, valid job offers. Family-sponsored immigrants and refugees, by contrast, are selected on the basis of family sponsorship and humanitarian considerations, respectively. Following an initial eligibility assessment, IRCC issues medical examination requests (IME) and police certificate requests for medical and criminal screening. Applicants are typically required to complete these requirements within one to two months of receiving the requests.

Upon final approval, IRCC issues a Confirmation of Permanent Residence (COPR) and, for applicants from visa-required countries, a PR visa. Immigrants must land in Canada within one year of completing the medical examination; failure to do so results in the expiration of the COPR and requires a new medical examination and reissuance of the COPR. Upon landing, immigrants formally acquire PR status and receive a PR card within several weeks. To maintain PR status, individuals must reside in Canada for at least two out of every five years, while a minimum of three years of physical presence within five years is required to apply for Canadian citizenship.

Before the introduction of Express Entry, processing times from application to final decision were often lengthy. Economic immigrants were admitted primarily through paper-based programs, including the Federal Skilled Worker Program, the Canadian Experience Class (from 2008), and Provincial Nominee Programs (launched in 1998), alongside smaller business and self-employment streams, with Quebec operating a separate selection system. CIC's 2011 Annual Report to Parliament on Immigration documents that the 2008 Ministerial Instructions were introduced to reduce the Federal Skilled Worker backlog and substantially shortened processing times from several years to several months, though still longer than the current six-month service standard under Express Entry.[4] During the same period, the service standard for Family Class spouses and dependent children abroad was within 12 months,

[4]A government evaluation reports that the processing time from application receipt to visa issuance for Federal Skilled Worker applications was still 10.9 months in June 2010 (see Figure 3-12 of the *Evaluation of Ministerial Instructions (Implementation)*).

while processing times for the Parent and Grandparent Program and for refugees frequently extended to multiple years (Alboim and Cohl, 2012). With the launch of Express Entry in 2015, the processing standard for most economic immigration applications was reduced to six months, although only candidates who receive an invitation to apply could submit a formal application.

At the initial application stage, immigrants self-report their intended geographical destination on the Generic Application Form for Canada (IMM 0008). This information is printed on the COPR and recorded in the landing file. For immigrants admitted through federal (non-provincial) programs, declared destination does not affect selection and is not subject to formal credibility assessment, implying limited incentives for strategic misreporting. These immigrants are also free to settle anywhere in Canada upon arrival. By contrast, immigrants admitted through Provincial Nominee Programs or Quebec programs are legally required to intend to reside in the nominating province. Failure to demonstrate such intent, or immediate relocation without bona fide settlement, may be treated as misrepresentation and carries a risk of PR status revocation.

Canadian immigration policy has also evolved substantially over time. Admission levels, processing priorities, and the composition of immigrant inflows have repeatedly been adjusted in response to demographic, economic, and political considerations. The share of immigrants admitted under the economic class increased from roughly 40 percent in the 1980s to 58.4 percent in 2022. The points system has undergone numerous revisions, including changes that increased the weight placed on English and French language proficiency in response to concerns about the recognition of foreign education and credentials by Canadian employers. To address occupation-specific labor shortages, Canada has also periodically adjusted the set of eligible occupations. The importance of job offers in economic selection has increased over time, particularly since the late 2000s, although the overall share of immigrants arriving with pre-arranged employment has remained relatively low.

Immigration policy may itself respond to macroeconomic conditions, potentially affecting both the number and composition of arriving immigrants. As documented by Green and Green (2004) and Grant and Sweetman (2004), Canadian immigration policy has historically balanced two objectives: selecting immigrants with strong long-run human capital and addressing short-term labor market needs. Before the 1990s, immigration levels were adjusted according to Canada's perceived capacity to absorb new immigrants, with admissions generally increasing during economic expansions and declining during downturns. Since the early 1990s, however, immigration policy has placed greater emphasis on maintaining relatively stable

admission targets based on longer-term objectives, making overall inflows less responsive to business cycle fluctuations.[5] Nevertheless, policymakers have continued to adjust immigrant selection criteria in response to perceived labor market needs. For example, in May 1982, the government introduced a requirement that most independent immigrant applicants have arranged employment in Canada, which was subsequently removed in January 1986 as economic conditions improved. In May 1991, the government introduced a list of designated occupations with high demand, awarding additional selection points to qualifying applicants (Green and Green, 1995).[6] Thus, relatively stable aggregate immigration targets do not necessarily rule out changes in immigrant selection over the business cycle.

3. Data

3.1. The Canadian Longitudinal Immigration Database

My primary data source is the Canadian Longitudinal Immigration Database (IMDB), an administrative dataset that includes all (permanent resident) immigrants who have landed in Canada since 1952 and non-permanent residents since 1980. The IMDB is linked to the T1 Family File (T1FF), which is constructed from annual individual income tax returns and provides detailed demographic and income information at both the individual and family levels. In my data, the T1FF is available for the period 1982–2022.

The IMDB contains rich information at landing, including gender, country of birth, immigration category, year and month of landing, birth year, pre-admission Canadian experience, education at admission, intended occupation, language proficiency, and intended geographic destination at a granular level (down to the census subdivision). In addition, the IMDB includes a family identifier that allows minor immigrants who landed before age 18 to be linked to their parents when the parents are also landed immigrants. These individuals are commonly referred to as the 1.5 generation, as they were born outside the host country in contrast to the second generation. A limitation of the IMDB is that native-born second generation individuals are not observed. The landing file also includes death indicators and information on citizenship acquisition from linked administrative sources.

All landed immigrants who have ever filed taxes between 1982 and 2022 appear in the

[5] Panel (a) of Figure A.2 illustrates this shift: planned immigration levels appear negatively related to national unemployment rates before 1991 but remain relatively stable thereafter.

[6] The first Ministerial Instructions, introduced in November 2008, also prioritized Federal Skilled Worker applications in designated high-demand occupations. However, the primary motivation was to reduce application backlogs and processing delays rather than to respond to the contemporaneous recession.

T1FF. The tax data provide annual information on location of residence, family composition (including spousal identifiers and number of children), and detailed income components.[7] These data allow me to infer internal migration across provinces over time. Importantly, the tax files report detailed information on employment income derived from T4 slips, along with the number of T4 slips received in each year and the principal industry of the main employer. Hourly wages and hours worked are not observed in the tax data. The intensive margin of labor supply is therefore not directly observable. Post-secondary enrollment in Canada can be inferred if individuals claim tuition tax credits.

The baseline panel for employment includes all immigrants from the IMDB landing records, including those who never file a tax return (who are coded as non-employed). All other outcomes derived from the T1FF, such as earnings and geographic mobility, are evaluated conditional on filing a tax return in that year.

3.2. Linkages to Census and the Canadian Employer-Employee Dynamics Database

The IMDB is linked to the 2016 Census of Population long-form files, which cover approximately 25% of Canadian households and are based on a randomly selected, nationally representative sample. The Census linkage complements the administrative data in three important ways. First, it provides information on post-secondary studies undertaken after landing and before 2016, which is particularly relevant for studying the education attainment of immigrant children. Second, whereas tax records do not contain occupational information, the Census provides detailed occupation codes that allow me to examine occupational outcomes. Third, the Census records the number of weeks worked in 2015, enabling an analysis of the intensive margin of labor supply.

I further augment the analysis using the Canadian Employer-Employee Dynamics Database (CEEDD), which links the universe of employer-employee relationships from 2001 to 2019. The CEEDD allows me to identify each worker's primary employer in a given year (defined as the firm paying the highest earnings when multiple employers are present) and to track job-to-job transitions. The database also provides rich firm-level characteristics from financial statements and hiring histories. These linkages enable me to decompose wage earnings into worker and firm components using the Abowd-Kramarz-Margolis (AKM) framework (Abowd

[7]Total income consists of market income and government transfers. Market income includes employment income, investment income, and other private income. Employment income or earnings include both wage earnings and self-employment earnings. Unless otherwise stated, however, I use the term "earnings" to refer specifically to wage earnings throughout the main text.

et al., 1999).

3.3. Labour Force Survey

Local economic conditions are proxied by unemployment rates. Monthly and annual labor-force statistics at the national and provincial levels are obtained from Statistics Canada's Labour Force Survey (LFS). Consistent with the focus of the analysis on male immigrants, I construct the primary unemployment rate measure using males aged 18 and older, excluding full-time members of the Canadian Armed Forces. Female unemployment rates are highly correlated with their male counterparts.

3.4. Main Estimation Sample and Descriptive Statistics

The detailed sample selection procedure is reported in Table 1. To study economic integration from the time of first arrival, I focus on immigrants with no prior Canadian experience. Specifically, I exclude individuals who held a work permit, study permit, or asylum status before obtaining permanent residency. Furthermore, I examine integration outcomes during the first ten years after arrival. Because the T1FF covers the period from 1982 to 2022, observing ten years of post-arrival outcomes for all individuals requires restricting the sample to landing cohorts from 1982 to 2012. This restriction ensures a balanced panel in which the same set of landing cohorts is observed at each year since arrival over the first decade spent in Canada. The sample period encompasses three major Canadian recessions: the early 1980s recession, the early 1990s recession, and the Great Recession of 2008 (see Figure 1).

I further restrict the sample to immigrants aged 18–54 at landing, ensuring that they remain of working age throughout the ten-year observation window. I exclude immigrants selected through Quebec's independent immigration system because Quebec's selection policies may respond to local economic conditions, potentially inducing a correlation between immigrant characteristics and labor market conditions at arrival.[8] I similarly exclude provincial nominees, whose selection may also respond to provincial labor market conditions. Finally, I exclude immigrants admitted through business immigration categories, such as entrepreneurs and investors, whose admission criteria and labor market attachment differ from those of other immigrants.

The main analysis focuses on male immigrants, yielding a final sample of approximately $1,134,000$ individuals. Focusing on men provides a more homogeneous sample with relatively

[8]Approximately 12% of male immigrants and 11% of female immigrants in the sample were selected through Quebec's immigration system.

strong and stable labor force attachment and facilitates comparisons with the existing literature, while still retaining substantial statistical power. To assess the generalizability of the findings, I replicate the main analyses for female immigrants and obtain qualitatively similar results. Using family identifiers in the IMDB, I also identify approximately 467,000 immigrant children (the 1.5 generation) linked to their fathers, allowing me to examine the intergenerational effects of adverse labor market conditions at arrival. Unless otherwise stated, all subsequent references to immigrants pertain to the male sample.

Table 2 reports the distribution of immigrants across detailed admission categories. Economic immigrants account for 56.20% of the sample, followed by family-sponsored immigrants (29.93%) and refugees (13.88%). Within the economic class, the vast majority are federal skilled workers, with a small share admitted as self-employed immigrants. Among federal skilled workers, only 0.16% (equivalent to 0.09% of the full sample) had arranged employment offers at landing, according to the administrative records. Among family-sponsored immigrants, spouses and partners constitute the largest group, followed by parents and grandparents and dependent children. Within the refugee class, government-assisted refugees (GARs) account for the largest share, followed by privately sponsored refugees (PSRs) and protected persons and their dependents.

Table 3 presents summary statistics on immigrants' landing characteristics and selected post-arrival outcomes. Column (1) reports statistics for the full sample, while columns (2)–(4) report results separately for economic immigrants, family-sponsored immigrants, and refugees. Panel A shows substantial variation in educational attainment across admission classes. Economic immigrants are concentrated at the bachelor's degree level, whereas family-sponsored immigrants and refugees are more likely to have a high school education or less. Panel B reports immigrants' regions of birth. Economic immigrants are most likely to originate from East Asia, while South Asia is the most common region of origin among family-sponsored immigrants. Refugees are most likely to originate from Europe, Africa, and the Middle East. Panel C shows that intended destinations are highly concentrated in Ontario (60.8%) and British Columbia (17.9%), with the remaining 21.3% intending to settle in other provinces. Refugees are more geographically dispersed across provinces, consistent with the government's role in determining initial resettlement destinations for GARs.

Panel D summarizes other characteristics at landing. The average landing age is approximately 33 years, with economic immigrants tending to be older and more likely to be married than other immigrants.[9] Economic immigrants are also more likely to speak

[9]Married individuals include those who are legally married or in common-law unions.

English at landing, followed by family-sponsored immigrants and refugees. Only 2.7% of immigrants speak French at landing, potentially reflecting the exclusion of Quebec-selected immigrants. Intended occupations are primarily relevant to the selection of economic-class principal applicants, whose occupational qualifications are assessed as part of the immigration process.[10] Consistent with this requirement, 82.5% of economic immigrants report an intended occupation, compared with substantially lower shares among other admission classes. I classify reported occupations into high- and low-skilled categories using the National Occupational Classification (NOC): managerial (0), professional (A), and skilled and technical (B) occupations are classified as high-skilled, while intermediate and clerical (C) and elementary and labouring (D) occupations are classified as low-skilled. Conditional on reporting an intended occupation, the majority of economic immigrants are classified as high-skilled, compared with approximately half of family-sponsored immigrants and refugees.[11]

Panel E reports post-landing tax-filing behavior and geographic mobility. Overall, 91.4% of immigrants file at least one tax return during the available observation period (1982–2021). Economic immigrants have the lowest probability of ever filing a tax return, while refugees have the highest. These differences may reflect variation in opportunities outside Canada, as nonfiling can result from emigration. I also examine the probability of permanently ceasing to file tax returns within the first ten years after landing, including never filing. Economic immigrants are again the most likely to stop appearing in the tax records. Using geographic information from tax returns, I identify immigrants who switch away from their intended destination province within the first ten years after landing. Refugees exhibit the highest rate of interprovincial mobility (27.0%), followed by economic immigrants (19.4%) and family-sponsored immigrants (11.0%).

Finally, Panel F summarizes employment and annual T4 earnings within the first 10 years in Canada. The average employment rate is 67.1%, although this measure also reflects nonfiling, as individuals without observed positive earnings are counted as not employed. Average annual T4 earnings are approximately $23,700 in 2016 Canadian dollars. Consistent with the observed skill levels, economic immigrants have higher average earnings than family-sponsored immigrants and refugees.

[10] According to the 2024 Longitudinal Immigration Database (IMDB) Technical Report, reporting an intended occupation is mandatory only for economic-class principal applicants. Applicants reporting a specific intended occupation must demonstrate the required educational qualifications and at least one year of relevant work experience.

[11] Reported intended occupations for non-economic immigrants should be interpreted with caution, as these applicants are generally not required to provide supporting evidence.

4. Unemployment Rate Shocks

In this section, I discuss the main threats to identification, describe the construction of initial unemployment rate (IUR) shocks, and present evidence supporting the exogeneity of these shocks.

4.1. Threats to identification and IUR shocks

4.1.1 Endogenous immigrant selection with business cycles

Unlike native-born college graduates, whose timing and location of labor market entry are largely determined before graduation, immigrants have considerable discretion over when and where they enter the host-country labor market. To illustrate the resulting selection problem, consider an extended Roy model (Borjas, 1987) in which migration decisions depend on both expected permanent earnings and nonpecuniary factors. Immigrant i from origin country j considers migrating to province p in landing year l, with the decision made at time $t < l$. The utility associated with this choice is

$$\begin{aligned} U_{ijtlp} &= E_t[y_{ijlp}] + g(\phi_p, Net_{jtp}, X_i) + \epsilon_{ijtlp} \\ &= E_t\left[f(\phi_p, Net_{jlp}, UR_{lp}, X_i)\right] + g(\phi_p, Net_{jtp}, X_i) + \epsilon_{ijtlp}, \end{aligned} \tag{1}$$

where y_{ijlp} denotes potential permanent earnings in the destination labor market, ϕ_p captures province-specific characteristics, Net_{jlp} measures immigrant networks from origin country j in province p at landing, UR_{lp} is the provincial unemployment rate at arrival, X_i denotes individual characteristics, and ϵ_{ijtlp} captures unobserved preferences. The function $g(\cdot)$ captures nonpecuniary considerations, including immigrant networks and destination amenities.

Let U_{ijt0} denote the utility of the outside option of remaining in the origin country, given by

$$U_{ijt0} = E_t[y_{ij0}] + \epsilon_{ijt0}, \tag{2}$$

where y_{ij0} denotes potential permanent earnings without migration and ϵ_{ijt0} captures unobserved preferences for remaining in the origin country. Immigrants choose the alternative that maximizes their expected utility:

$$a_i^* = \arg\max_{a \in \mathcal{A}_{ijt}} U_{ijta}, \qquad \mathcal{A}_{ijt} = \{0\} \cup \{(l,p) : l \in \mathcal{L}_t,\ p \in \mathcal{P}\}, \tag{3}$$

where $\mathcal{L}_t$ denotes the set of feasible landing years and $\mathcal{P}$ the set of destination provinces.

This framework illustrates why immigrant selection may be endogenous to business cycles. Since expected permanent earnings depend on local labor market conditions, immigrants may adjust both their arrival timing and destination province in response to anticipated economic conditions. Moreover, the importance of expected earnings relative to nonpecuniary factors may differ across individuals, generating heterogeneous migration responses and changes in immigrant composition. Consequently, realized unemployment rates at arrival may be correlated with both observed and unobserved immigrant characteristics that independently affect subsequent economic integration.

The extended Roy model suggests that unfavorable expected labor market conditions reduce the incentives to migrate, potentially affecting both immigrant inflows and destination choices. Consistent with this prediction, column (1) of Table 4 regresses landing-cohort-level immigrant inflows on the realized IUR and shows that a 1 percentage point (p.p.) increase in the national IUR is associated with 2,073 fewer new immigrants. A similar negative relationship emerges across destination provinces within landing cohorts after accounting for landing-year and destination-province fixed effects. Column (3) shows that a 1 p.p. increase in the provincial IUR is associated with 286 fewer immigrants choosing that province as their initial destination. These results suggest that immigrants systematically adjust both the timing and location of migration in response to local labor market conditions.

Adverse labor market conditions at arrival affect not only the number of new immigrants but also the composition of arriving cohorts. Figure 3 plots coefficient estimates relating national labor market conditions at entry to a range of immigrant landing characteristics. The gray diamonds and bars indicate that immigrants arriving during recessions are negatively selected on observable skills. Relative to immigrants arriving in better labor market conditions, they tend to be younger, less educated, less likely to be admitted through the economic class, less likely to report English proficiency, and less likely to report high-skilled intended occupations. Recession cohorts also differ along marital status and origin dimensions. They are more likely to be single rather than married or in a common-law relationship, and are disproportionately drawn from Europe, Oceania and other Asia, and South and Central America, while being less likely to originate from Southern and Eastern Asia.

Similar patterns emerge in the province-level specifications shown in Figure 4.[12] The estimated coefficients on the realized IUR are generally smaller than those in Figure 3, likely because destination province and landing year fixed effects absorb part of the selection across provinces and landing cohorts. Nevertheless, immigrants arriving in provinces with

[12]Province-level regressions are weighted by province-by-landing-cohort immigrant inflows. Standard errors are clustered at the landing cohort level.

weaker labor market conditions continue to exhibit different observable characteristics. A 1 percentage point (p.p.) increase in the realized IUR is associated with a 0.6-year decrease in landing age and a 0.3 p.p. increase in the share of single immigrants. Educational attainment also differs: the share of immigrants with no formal education increases by 0.2 p.p., while the shares holding bachelor's and master's degrees decrease by 0.6 and 0.5 p.p., respectively. In terms of immigration class, the share of family-sponsored immigrants increases by 0.4 p.p. Source-country composition also varies, with immigrants arriving in provinces experiencing higher unemployment more likely to originate from Southern Asia and Oceania and Other Asia, and less likely to originate from Europe. A joint wild cluster bootstrap test strongly rejects the null hypothesis that the coefficients on the realized provincial IUR are jointly zero across the 24 landing characteristics ($F(24, 30) = 69$, $p < 0.001$).

4.1.2 IUR shocks and their construction

Although immigrants select their arrival timing and intended destination in response to anticipated labor market conditions, the realized IUR contains an unpredictable component. As illustrated by the extended Roy model, migration decisions are based on expected earnings, $E_t[y_{ijlp}]$, which depend on expected labor market conditions at arrival rather than their realized values. This distinction is particularly relevant in the Canadian immigration context, where the immigration process introduces a non-negligible time gap between the migration decision and actual arrival in Canada. Considerable time is required for IRCC to select candidates and process applications. As a result, when immigrants submit their applications and report their intended destination, they do not yet observe the local labor market conditions that will prevail at the time of landing. Although immigrants may delay arrival if conditions deteriorate after approval, their ability to do so is limited, as landing must occur within one year of completing the medical examination. Consequently, while immigrants may select into particular arrival cohorts and provinces based on expected labor market conditions, they cannot fully anticipate unexpected changes in unemployment rates between the migration decision and actual landing.

Because the IMDB does not record application dates, I cannot directly observe when immigrants formed these expectations. Nor is it possible to recover the specific information sets or forecast models used by individual applicants. I therefore impose a simplifying assumption that immigrants form expectations about local labor market conditions one year prior to landing. This assumption is consistent with two features. First, all cohorts in my sample landed before the introduction of Express Entry, when average processing times

typically exceeded one year. Second, forecasts made one year prior to landing are likely to be substantially more informative about realized conditions than forecasts formed further in advance, making this a conservative and empirically robust way to control for selection based on expected initial labor market conditions.

The IUR shock is defined as the difference between the realized and predicted IUR. Using unexpected shocks as part of the identification strategy is common in empirical economics. Many macroeconomic variables contain both anticipated and unanticipated components, and economic agents may adjust their behavior in response to anticipated changes before they are realized. Consequently, studies seeking to estimate the causal effects of macroeconomic fluctuations often isolate the unexpected component of policy or economic conditions. This approach has been widely applied to monetary policy (Kuttner, 2001; Romer and Romer, 2004), fiscal policy (Ramey and Shapiro, 1998; Romer and Romer, 2010), and oil price shocks (Kilian, 2008). More broadly, unexpected macroeconomic shocks also play a central role in shift-share (Bartik) designs, including studies of immigration and the China shock (Card, 2001; Autor et al., 2013; Borusyak et al., 2022, 2025).

By construction, IUR shocks are unexpected when immigrants submit their applications. Nevertheless, applicants may revise their migration decisions after observing deteriorating labor market conditions and choose to withdraw their applications. Such behavior could induce selection even if the initial application decision was based only on expected labor market conditions. Whether this is a concern depends on the timing of application withdrawal. If applicants withdraw early in the application process, the effect is primarily reflected in the relationship between predicted IUR and immigrant inflows. By contrast, withdrawals at later stages of the application process are more likely to affect the number of immigrants admitted in the same year and therefore have the potential to generate selection with respect to IUR shocks. Because immigrants who abandon their applications never obtain permanent resident status, they do not appear in the IMDB.

To assess the importance of late-stage application withdrawal, I obtained annual counts of Confirmation of Permanent Residence (COPR) documents issued and Permanent Resident (PR) admissions from Immigration, Refugees and Citizenship Canada (IRCC) for the period 2021–2024.[13] While columns (1) and (3) of Table A.4 show that both COPR issuance and PR admissions decline when the realized IUR is higher, consistent with Table 4, column (5) shows that the difference between the logarithm of PR admissions and the logarithm of

[13]Data on COPR documents issued were obtained through a private request. Data on PR admissions are publicly available at https://open.canada.ca/data/en/dataset/f7e5498e-0ad8-4417-85c9-9b8aff9b9eda/resource/5582034d-8f89-49d5-8597-483d628078a1.

COPR documents issued, which is an approximate measure of abandoned COPR documents, is essentially unrelated to the realized IUR. As shown later, the number of new immigrants is also unrelated to IUR shocks. Together, these findings suggest that applicants do not systematically abandon their applications in response to unexpected labor market shocks in a way that materially affects immigrant selection.

A second concern that threatens the exogeneity of IUR shocks is that immigration authorities may adjust the composition of admitted immigrants in response to business-cycle conditions. As discussed in Section 2, both the scale and composition of immigrant admissions have historically varied with labor market conditions, potentially inducing endogenous selection. Fortunately, the scope for province-specific policy responses is limited. With the exception of Quebec and the Provincial Nominee Program (PNP), Canadian provinces do not operate independent immigration systems.[14] Instead, immigrants are selected by the federal government regardless of their intended province of destination. Consequently, any province-specific changes in immigrant composition are unlikely to be driven by provincial immigration policies. I later exploit this institutional feature when introducing province-level IUR shocks.

A third concern is that IUR shocks may partly reflect the labor supply effects of immigrant inflows. If sufficiently large inflows increase local unemployment rates, IUR shocks should be positively associated with the number of new immigrants. As an additional robustness check, I re-estimate the main specifications controlling for the immigrant population to account for this potential labor supply channel. The results remain quantitatively unchanged.[15]

Overall, my goal is to construct provincial UR shocks. I proceed in two steps. First, I generate province-level predictions of unemployment rates using rich historical information up to one year prior to landing. The predictors include past unemployment rates in all provinces as well as several macroeconomic indicators. A simple benchmark uses the previous year's unemployment rate as the prediction. In addition, I estimate six forecasting models—Mixed Data Sampling Regression (MIDAS), ARIMAX (AM), Linear Regression (LM), Fixed-Effects Panel Regression (FE), Markov-Switching Models (MS), and Neural Networks (NN)—using a non-rolling, expanding-window estimation strategy. Each model captures different features of unemployment dynamics. The FE model accounts for province fixed effects, MS models allow for regime changes between expansions and recessions, and neural networks capture

[14]The baseline sample excludes Quebec-selected immigrants and Provincial Nominees to minimize potential selection induced by province-specific immigration policies. The results remain robust to including these groups.

[15]See Figure A.10.

nonlinearities. Additional model details are provided in Appendix A. Second, I combine the seven forecasts into a pooled prediction by taking the average, and define the UR shocks as the difference between the realized unemployment rate and this pooled prediction.

National predicted UR is constructed by aggregating provincial predicted URs using the share of employed immigrants residing in each province as weights. National UR shocks are then defined as the difference between the realized and predicted national UR. Figure 1 plots the realized national UR and the corresponding UR shocks. The realized national UR exhibits the well-known pattern of hysteresis, rising sharply during the recessions of the early 1980s, early 1990s, and the Great Recession of 2008–2009, followed by gradual declines during subsequent expansions. Consistent with recessions generating large unexpected deteriorations in labor market conditions, UR shocks are strongly positive at the onset of each downturn. Figure 2 illustrates the variation in provincial IUR shocks across provinces and landing years. Rather than being concentrated in a few provinces or periods, these shocks exhibit substantial variation across both dimensions. Provincial IUR shocks have a mean of 0.02 percentage points (p.p.) and a standard deviation of 1.14 p.p. By construction, UR shocks exhibit little serial correlation. Moreover, at both the national and provincial levels, the standard deviation of UR shocks is approximately half that of realized URs.

4.2. Evidence on the exogeneity of IUR shocks

If IUR shocks are exogenous to immigrant selection, they should be unrelated to both immigrant inflows and observable landing characteristics. To assess this hypothesis at the national level, I estimate the following regression using landing cohorts as the unit of observation:

$$y_l = \beta_1 \widetilde{UR}_l + \beta_2 \widehat{UR}_l + \epsilon_l, \tag{4}$$

where l indexes landing cohorts, y_l denotes either immigrant inflows or a cohort-level landing characteristic, and $\widetilde{UR}_l$ and $\widehat{UR}_l$ denote the national IUR shock and predicted IUR, respectively.

Column (2) of Table 4 reports the relationship between national IUR shocks, predicted IUR, and immigrant inflows. As expected, predicted IUR is negatively associated with the number of new immigrants, suggesting that immigrants respond to anticipated labor market conditions when making migration decisions. By contrast, IUR shocks are not significantly related to immigrant inflows. Moreover, the estimated coefficient is negative rather than positive,

providing no evidence that larger immigrant inflows mechanically increase unemployment rates.

The balance tests, however, reveal that national IUR shocks remain correlated with observable landing characteristics. The red circles and horizontal bars in Figure 3 show that immigrants arriving during unexpected labor market downturns are less educated, less likely to be admitted through the economic class, and less likely to report high-skilled intended occupations. They are also less likely to originate from Southern and Eastern Asia, more likely to be single, and more likely to report French proficiency. These findings suggest that national IUR shocks are not fully exogenous to immigrant selection. One possible explanation is that the federal government adjusts immigrant selection in response to national business cycle conditions. This pattern differs from the findings of Lucchetti and Ruggieri (2025), who report that national IUR shocks satisfy similar balance tests. The discrepancy may reflect differences in the sample period[16] or, more importantly, differences in the underlying data. My analysis focuses exclusively on newly landed permanent immigrants and excludes individuals who first entered Canada as temporary residents. By contrast, although Lucchetti and Ruggieri (2025) observe immigrants' year of arrival, they cannot distinguish permanent immigrants from individuals who initially entered as temporary residents, including international students, whose selection into migration may respond differently to labor market shocks.

I next conduct the same exercise using variation across destination provinces within landing cohorts. Specifically, I estimate the following specification:

$$y_{lp} = \beta_1 \widetilde{UR}_{lp} + \beta_2 \widehat{UR}_{lp} + \mu_l + \phi_p + \epsilon_{lp}, \tag{5}$$

where p indexes provinces, the fixed effect μ_l captures landing-cohort-specific characteristics common to all provinces, and ϕ_p differences across provinces of intended destination. Regressions are weighted by immigrant inflows at the intended-province-by-landing-year level, and standard errors are clustered by landing year.

Column (4) of Table 4 and the red circles and horizontal bars of Figure 4 show that provincial IUR shocks are unrelated to both immigrant inflows and observable landing characteristics. A one p.p. increase in the provincial IUR shock is associated with only 106 additional immigrants, an estimate that is both statistically and economically insignificant. This finding alleviates the concern that immigrants systematically withdraw their applications

[16] Lucchetti and Ruggieri (2025) study landing cohorts from 1990 to 2021. As a result, their sample is not balanced in years since arrival, with more recent cohorts observed for substantially shorter post-arrival periods than earlier cohorts.

in response to unexpected provincial labor market shocks. Consistent with this interpretation, columns (2), (4), and (6) of Table A.4 show that provincial IUR shocks are not significantly related to the number of COPR documents issued, the number of PR admissions, or the difference between the two. The results also mitigate the concern that provincial IUR shocks are driven by changes in immigrant inflows. Although the estimated relationship is positive, its magnitude is too small to meaningfully affect provincial unemployment rates. Moreover, the balance tests in Figure 4 provide further evidence that immigrant composition is not systematically affected by unexpected provincial labor market shocks. In contrast to the realized provincial IUR, a joint wild cluster bootstrp test fails to reject the null hypothesis that the coefficients on provincial IUR shocks are jointly zero across the 24 landing characteristics ($F(24, 30) = 10$, $p = 0.136$).

Taken together, these findings suggest that provincial IUR shocks are plausibly exogenous to immigrant selection. This result is consistent with the institutional setting. With limited province-specific immigration policy outside Quebec and the Provincial Nominee Program, immigrant selection is largely determined at the federal level. Moreover, immigrants are less likely to observe or respond to unexpected province-specific labor market shocks than to national shocks when making migration decisions.

5. Estimation Strategy

In this section, I proceed with a 2-by-2 DID example to motivate my empirical estimation strategies to examine the impact of entering the country during a recession on both intra- and inter-generational integration processes.

5.1. A 2-by-2 Difference-in-Differences illustration

To illustrate the identification strategy, I begin with a simple 2-by-2 difference-in-differences (DID) example. Consider British Columbia (BC) as the treated province and the 1998 landing cohort as the treated cohort. In 1998, BC experienced an unexpected labor market downturn following the Asian financial crisis, which reduced demand for BC exports and weakened employment in forestry, mining, and other resource industries. As the control group, I use Ontario (ON) and the 2004 landing cohort. Unlike a conventional DID design, this setting does not involve pre-treatment observations because immigrants are observed only after landing. Consequently, the control landing cohort need not precede the treated landing cohort. The corresponding provincial IUR shocks are shown below. Only BC experienced

a large positive IUR shock in 1998, so the treatment indicator equals one for immigrants intending to settle in BC in 1998 and zero otherwise.

IUR shocks

	1998	2004
BC	0.9%	−0.4%
ON	−0.5%	0.1%

Treatment indicator

	1998	2004
BC	1	0
ON	0	0

A key complication is that intended destination does not perfectly determine actual exposure to local labor market conditions. Permanent residents are free to settle anywhere in Canada after arrival, and many do not initially reside in their intended province or subsequently move across provinces. In this example, 20% of immigrants whose intended destination was BC in 1998 landed in another province, and within ten years after arrival, 25% had moved out of BC. Consequently, treatment assignment based on intended destination is subject to noncompliance. The DID estimator therefore identifies an intention-to-treat (ITT) effect rather than the effect of actual exposure to the local recession. Because adverse local labor market conditions may induce endogenous out-migration, these ITT estimates are likely to attenuate the effects of actual exposure toward zero.

Landed in a different province

	1998	2004
BC	20%	6%
ON	11%	5%

Switched to a different province within 10 years

	1998	2004
BC	25%	17%
ON	15%	14%

Under the DID framework, identification of the ITT effect requires the standard common trends assumption:

$$Y_{1,1}(0) - Y_{1,0}(0) = Y_{0,1}(0) - Y_{0,0}(0),$$

where $Y_{l,p}(D)$ denotes the potential outcome of immigrants who landed in year l and intended to settle in province p, depending on whether they were exposed to a recession at arrival ($D = 1$) or not ($D = 0$). Since I study dynamic immigrant integration over the first ten years after landing, the outcomes of interest are measured separately for each year since arrival, $e \in \{0, \ldots, 10\}$. For each year since arrival e, I estimate:

$$y_{lpt} = \beta D_{lp} + \mu_l + \phi_p + \epsilon_{lpt}, \quad \text{for } e = t - l \in \{0, \ldots, 10\}, \tag{6}$$

where t indexes calendar year.

Before implementing the DID estimator, I residualize log earnings with respect to predicted IUR to remove selection associated with anticipated labor market conditions.[17]

Panel (a) of Figure A.3 plots the residualized log earnings difference between BC and ON for the 1998 and 2004 landing cohorts over years since arrival. For the 2004 cohort, the earnings difference between BC and ON remains close to zero throughout the first ten years after arrival. By contrast, immigrants intending to settle in BC in the 1998 cohort initially earn approximately 13% less than those intending to settle in ON, but this gap narrows steadily and is nearly eliminated after ten years. Consistent with this pattern, panel (b) reports the DID estimates ($\hat{\beta}$ from specification 6) by years since arrival, showing large negative effects immediately after arrival that gradually fade toward zero.

5.2. Intragenerational effects

The 2-by-2 DID illustration can be extended to the full empirical setting by allowing for multiple destination provinces, multiple landing cohorts, and continuous treatment intensity. Identification exploits variation in initial unemployment rate (IUR) shocks across intended destination provinces and landing cohorts. To formalize the identification assumptions, let $Y_{(l,p),e}(d)$ denote the average potential outcome e years after arrival for immigrants in landing cohort l intending to settle in province p, under an IUR shock of magnitude d. Let $D_{lp} = \widetilde{IUR}_{lp}$ denote the realized shock. The target parameter is the average causal response to a marginal change in the IUR shock,

$$ACR_e(d) = \frac{\partial E[Y_e(d)]}{\partial d}, \tag{7}$$

which measures how a marginal change in initial labor market conditions affects immigrant integration e years after arrival. For earnings outcomes, this parameter can also be interpreted as the causal response of the earnings return to migration in the extended Roy model. Since counterfactual earnings without migration are unaffected by Canadian IUR shocks, changes in the return to migration arise entirely through changes in post-migration earnings.

Identification relies on three assumptions.

[17] The predicted IUR matrix is

	1998	2004
BC	8.3%	7.3%
ON	7.0%	6.3%

Residualization helps account for potential immigrant selection based on predicted IUR.

Assumption 1 (Generalized common trends). In the absence of IUR shocks, differences in potential outcomes across landing cohorts are common across intended destination provinces. Formally, for any two provinces p, p', any two landing cohorts l, l', and any year since arrival e,

$$Y_{(l,p),e}(0) - Y_{(l',p),e}(0) = Y_{(l,p'),e}(0) - Y_{(l',p'),e}(0). \tag{8}$$

This assumption implies that untreated potential outcomes can be decomposed into additive landing cohort and destination province effects, with both allowed to vary flexibly over years since arrival:

$$Y_{(l,p),e}(0) = \mu_{l,e} + \phi_{p,e}. \tag{9}$$

Assumption 2 (Strong parallel trends). For any two shock intensities d and d', the average treatment effect of a shock of magnitude d relative to zero does not depend on the realized shock intensity:

$$\begin{aligned} &E[Y_e(d) - Y_e(0) \mid D = d'] \\ &\quad = E[Y_e(d) - Y_e(0)]. \end{aligned} \tag{10}$$

As discussed by Callaway et al. (2024), generalized common trends alone does not identify causal responses to changes in continuous treatment intensity, because differences in treatment effects across shock levels may reflect selection on treatment effects. Strong parallel trends rules out such selection by requiring that average treatment effects at a given shock intensity do not systematically differ across groups experiencing different realized shocks.

Assumption 3 (Linearity). The average causal response is constant across shock intensities, although it may vary across years since arrival:

$$\frac{\partial E[Y_e(d)]}{\partial d} = \omega_e, \qquad \forall d. \tag{11}$$

This assumption allows the average effect of initial labor market conditions to evolve over time since arrival, while imposing a linear relationship between shock intensity and potential outcomes at each horizon.

Proposition 1 (Identification). Under Assumptions 1–3, and sufficient variation in IUR shocks after accounting for landing cohort and destination province effects, the average causal response $ACR_e(d)$ is identified and equals ω_e for all d.

To see this, generalized common trends implies that untreated potential outcomes can be represented by additive landing cohort and province effects. Strong parallel trends ensures that the average treatment effect at any given shock intensity is independent of the realized

shock intensity. Together with linearity, these assumptions imply

$$E[Y_{(l,p),e} \mid D_{lp} = d] = \mu_{l,e} + \phi_{p,e} + \omega_e d, \tag{12}$$

where ω_e identifies the average causal effect of a one-percentage-point increase in the IUR shock e years after arrival.

These assumptions are not directly testable. I therefore examine the sensitivity of the results to alternative specifications, including a quadratic specification that relaxes the linearity assumption.

To trace the evolution of immigrant integration over time, I interact IUR shocks with years since arrival, allowing their effects to vary dynamically. The baseline specification is

$$y_{ilpt} = \sum_{e \leq 10} \omega_e \mathbf{1}\{t-l=e\}\widetilde{IUR}_{lp} + \sum_{e \leq 10} \omega'_e \mathbf{1}\{t-l=e\}\widehat{IUR}_{lp} + \mu_l + \phi_p + \gamma_e + \alpha_t + \mathbf{X_i}\boldsymbol{\beta} + \epsilon_{ilpt}, \tag{13}$$

where individuals are indexed by i, landing year (cohort) by l, intended province of residence by p, and calendar year by t. The outcome y_{ilpt} includes employment, earnings, and other integration measures observed in year t. The variable $(\widetilde{IUR}_{lp})$ denotes the IUR shock in the intended province of destination in the landing year, while $(\widehat{IUR}_{lp})$ denotes the corresponding predicted IUR. The key coefficients of interest (ω_e) trace the effect of a one percentage point increase in the IUR shock (e) years after arrival, for $e = 0, \ldots, 10$. Including predicted IUR controls for immigrant selection based on anticipated labor market conditions.

I refer to specifications that use IUR shocks as the key regressor while controlling for predicted IUR as *shock-based specifications*. I contrast these with *realized-IUR specifications*, which regress outcomes on realized IUR without controlling for predicted IUR.[18]

The specification includes several sets of fixed effects. Landing cohort fixed effects, (μ_l), absorb national-level differences across arrival cohorts, including selection arising from immigrant supply and aggregate immigration policy. Destination province fixed effects, (ϕ_p), account for time-invariant differences across intended provinces of destination. Calendar year fixed effects, (α_t), capture aggregate shocks affecting all immigrants in a given year. Years since arrival fixed effects, (γ_e), flexibly control for average integration profiles over time since migration. The vector of individual controls, $(\mathbf{X_i})$, includes age, education at admission, country of birth, and marital status at admission. Standard errors are clustered

[18]Because realized IUR equals the sum of predicted IUR and the IUR shock, a specification that includes both realized and predicted IUR is algebraically equivalent to one that includes both the IUR shock and predicted IUR.

at the landing year level to allow for correlation in IUR shocks across provinces within the same arrival cohort (Abadie et al., 2023).

Although the baseline specification is parsimonious, it imposes the restriction that landing cohort effects, destination province effects, and the effects of observed characteristics remain constant over years since arrival. I also consider a more flexible specification motivated by the generalized common trends assumption, which implies that untreated potential outcomes can be represented by landing cohort effects and destination province effects that are allowed to vary flexibly over years since arrival.

The corresponding flexible specification is

$$y_{ilpt} = \sum_{e \leq 10} \omega_e \mathbf{1}\{t - l = e\} \widetilde{IUR}_{lp} + \sum_{e \leq 10} \omega'_e \mathbf{1}\{t - l = e\} \widehat{IUR}_{lp} + \mu_{l,e} + \phi_{p,e} + \mathbf{X_i}\boldsymbol{\beta}_e + \epsilon_{ilpt}, \quad (14)$$

where landing cohort effects, destination province effects, and individual controls are allowed to have year-since-arrival-specific effects on outcomes.[19]

Equation (14) is equivalent to estimating separate local-projection regressions for each year since arrival (Jordà and Taylor, 2025):

$$y_{ilpt} = \omega_e \widetilde{IUR}_{lp} + \omega'_e \widehat{IUR}_{lp} + \mu_{l,e} + \phi_{p,e} + \mathbf{X_i}\boldsymbol{\beta}_e + \epsilon_{ilpt}, \quad \text{for } e = t - l \in \{0, \ldots, 10\},$$

while stacking all horizons into a single regression. The stacked specification is computationally convenient and yields the same horizon-specific coefficient estimates as estimating separate local projections.

5.3. Intergenerational effects

The previous subsection examines how adverse labor market conditions at arrival affect immigrants' own economic integration. I next investigate whether these initial conditions have persistent consequences for the next generation. Specifically, I estimate the relationship between the labor market conditions faced by immigrant parents at arrival and the adult outcomes of their 1.5-generation children.

The estimating equation is

$$y_{ilpt} = \omega \widetilde{IUR}_{lp} + \omega' \widehat{IUR}_{lp} + \mu_l + \phi_p + \gamma_e + \alpha_t + \mathbf{X_i}\boldsymbol{\beta} + \epsilon_{ilpt}, \quad (15)$$

[19] Calendar year fixed effects are not included in this flexible specification because they are collinear with landing-cohort-by-years-since-arrival fixed effects.

where the dependent variable (y_{ilpt}) denotes an adult outcome of child (i), whose parent landed in year (l) and intended to settle in province (p). The variable ($\widetilde{IUR}_{lp}$) is the IUR shock faced by the parent at arrival, while ($\widehat{IUR}_{lp}$) denotes the corresponding predicted IUR. Landing cohort fixed effects, destination province fixed effects, year since arrival fixed effects, and calendar year fixed effects are included. Individual controls include the child's age and country of birth, as well as the parent's education and marital status at admission.

The outcomes of interest include employment and earnings measured when children are between ages 30 and 34, an age range commonly used as a proxy for permanent earnings (Adnan et al., 2023). For each child, I retain the earliest observed tax filing year between ages 30 and 34. Children who do not file a tax return during this age window are classified as not employed in Canada at age 30.

6. Empirical Results

This section reports the effects of arriving during recessions on various outcomes for immigrants and their children.

6.1. Intragenerational effects

6.1.1 Main results

Figure 5 reports the dynamic effects of initial unemployment rate (IUR) shocks on employment and annual earnings during the first ten years after arrival. Employment is defined as having positive employment earnings. In the panel data, individuals who do not file a tax return are coded as not employed.[20]

Panel (a) shows that adverse labor market conditions at arrival substantially reduce immigrants' employment during the first years after landing. Under the flexible shock-based specification, a one p.p. increase in the IUR shock lowers the probability of employment by approximately 1.7 p.p. in the landing year. The corresponding estimate from the restricted shock-based specification is about 1.2 p.p. Consequently, an immigrant arriving during a recession associated with a 4 p.p. IUR shock is approximately 6.8 p.p. less likely to be employed upon arrival under the flexible specification (4.8 p.p. under the restricted

[20]Figure A.4 examines the robustness of the employment results to alternative outcome definitions. Arriving during recessions slightly reduces the probability of filing a tax return. The figure also compares the baseline panel employment measure with a repeated cross-sectional employment measure that restricts the sample to tax filers. The estimated employment effects are nearly identical across the two employment definitions, indicating that selective non-filing (attrition) has little influence on the main results.

specification). The employment effects gradually diminish over time and become economically small by the tenth year after arrival.

The flexible and restricted shock-based specifications yield broadly similar dynamic patterns. The main difference is that the restricted specification estimates larger negative employment effects during years 8–10 after arrival, whereas the flexible specification indicates that the employment effects have essentially disappeared by the end of the first decade. Overall, however, the two specifications produce similar estimates throughout most of the post-arrival period.

The realized-IUR estimates are substantially more precise than the shock-based estimates since realized unemployment rates exhibit considerably more variation than unexpected IUR shocks. Nevertheless, the flexible realized-IUR estimates are systematically higher than the corresponding shock-based estimates, particularly between years 2 and 6 after arrival. Under the identifying assumptions discussed in Section 4.1.2, this pattern indicates that realized-IUR estimates are biased upwards because realized unemployment rates are confounded with immigrant selection. In other words, immigrants arriving during recessions appear positively selected on unobserved characteristics associated with subsequent employment, leading conventional realized-IUR estimates to understate the adverse employment effects of entering during a recession.

Panel (b) reports the corresponding effects on log annual earnings conditional on employment. Initial adverse labor market conditions generate sizeable and persistent earnings losses. Under the flexible shock-based specification, a one p.p. increase in the IUR shock reduces annual earnings by approximately 6.5 percent in the first year after arrival, while the corresponding estimate under the restricted shock-based specification is 4.4 percent.[21] Five years after arrival, the estimated earnings losses remain economically meaningful at 3.5 percent under the flexible specification and 3.1 percent under the restricted specification. Therefore, a 4 p.p. IUR shock lowers annual earnings by approximately 23 percent in the first year after arrival (16 percent under the restricted specification) and by 13 percent five years after arrival (12 percent under the restricted specification). These findings indicate substantial and persistent earnings scarring from entering the labor market during a recession.

The confidence intervals around the earnings estimates are sufficiently wide that the flexible and restricted shock-based specifications cannot be statistically distinguished. Likewise, the differences between the shock-based and realized-IUR estimates are generally small and statistically insignificant, suggesting that immigrant selection during recessions plays a small

[21]The year 0 estimate from the restricted specification is imprecisely estimated, likely because immigrants arrive at different times during the landing year, generating substantial variation in upon-arrival earnings.

role for earnings. The primary difference arises within the realized-IUR specifications, where the flexible model consistently produces larger earnings losses than the restricted model.

Given its weaker identifying assumptions and greater transparency, I use the flexible shock-based specification as the preferred specification throughout the remainder of the paper unless otherwise noted.

In the literature, the study by Lucchetti and Ruggieri (2025) is, to my knowledge, the only paper that examines the scarring effects of arriving during recessions for the overall immigrant population. Their estimated dynamic effects appear more volatile over years since arrival, which may reflect differences in sample construction. In particular, their analysis uses landing cohorts from 1990 to 2021, so the sample is not balanced across years since arrival. As a result, the estimated effects at different horizons are based on different sets of landing cohorts. In contrast, my analysis restricts attention to landing cohorts from 1982 to 2012, ensuring that the same cohorts are observed throughout the first ten years after arrival. The two studies also differ in the findings. Lucchetti and Ruggieri (2025) do not find statistically significant employment effects. By contrast, I find sizeable and persistent negative effects on employment during the first several years after arrival. For earnings, my estimated effects upon arrival are somewhat larger than theirs, although both studies find that the earnings losses gradually diminish and are close to zero within ten years after arrival. Note that Lucchetti and Ruggieri (2025) exploit only national variation in IUR shocks, whereas my identification comes from province-by-cohort variation in IUR shocks, thereby providing a more flexible empirical framework.

To place my estimates in the broader literature on the scarring effects of graduating during recessions, I replicate the analysis for a selected sample of immigrants who arrived between ages 22 and 24 with a bachelor's degree before 1995 and compare the results with those for Canadian native college graduates in Oreopoulos et al. (2012) and the state-IV estimates for U.S. native college graduates in Kahn (2010). Restricting the immigrant sample by age, education, and landing cohort makes the comparison as close as possible in terms of life-cycle stage, educational attainment, and observation window. Among the two studies, Oreopoulos et al. (2012) provide the more comparable benchmark because both my and their analyses use Canadian administrative panel data and exploit regional variation in local labor market conditions.

Figure A.5 shows that the estimates for native college graduates in Oreopoulos et al. (2012) are more precisely estimated than mine, likely because my selected immigrant sample is substantially smaller. Nevertheless, the patterns differ markedly. While employment effects for

native college graduates are small and statistically insignificant, immigrants experience much larger and persistent employment losses during the first three years after arrival. For earnings, both my estimates and those of Oreopoulos et al. (2012) exhibit gradual convergence toward zero, whereas the earnings losses in Kahn (2010) are considerably more persistent. Moreover, the initial earnings losses for immigrants are substantially larger than those for Canadian native graduates. Taken together, these comparisons suggest that immigrants are more adversely affected by entering the labor market during recessions than otherwise comparable native college graduates. One possible explanation is that immigrants face additional barriers in the labor market, including hiring discrimination (Bertrand and Mullainathan, 2004; Oreopoulos, 2011; Banerjee et al., 2018), which may become more binding when labor market conditions deteriorate.

6.1.2 Robustness checks

In the main specification, employment is defined as having positive annual employment earnings. Figure A.6 shows that the estimated employment effects are virtually unchanged when employment is instead defined using annual employment earnings exceeding $100, $1,000, or $5,000. Earnings are measured in logs in the baseline specification, which necessarily restricts the earnings analysis to individuals with positive earnings. Because adverse initial labor market conditions also affect employment, these estimates are conditional on employment and may be affected by selection into employment. Following Chen and Roth (2024), I estimate a Poisson specification that accommodates zero earnings and identifies percentage effects on earnings incorporating both the extensive and intensive margins. Figure A.7 compares estimates from the baseline log-linear specification with those obtained by Poisson pseudo-maximum likelihood (PPML). The two approaches yield similar estimates of the percentage effects on earnings.

The main specification exploits provincial variation in IUR shocks. As an alternative and more geographically granular measure of local labor markets, I use Census Metropolitan Areas and Census Agglomerations (CMAs/CAs), which more closely correspond to commuting-based labor markets. For simplicity, CMA/CA-level IUR shocks are constructed as the difference between realized and lagged unemployment rates. Figure A.9 shows that the baseline results are robust to using this lag-based shock measure. Figure A.8 further shows that the estimated employment and earnings effects remain persistently negative when identification relies on variation across CMAs/CAs, although the magnitudes are smaller than those obtained using provincial variation. There are at least two possible explanations for the smaller CMA/CA-

level estimates. First, unemployment shocks measured at the CMA/CA level may be noisier, generating greater measurement error and potentially attenuating the estimated effects toward zero. Second, immigrants may face lower costs of moving across CMAs/CAs than across provinces. Greater geographic mobility within provinces could therefore reduce continued exposure to adverse local labor market conditions and, in turn, lead to smaller estimated effects.

The baseline sample excludes Quebec-selected immigrants and provincial nominees to limit the influence of provincial governments on immigrant selection. Figure A.9 compares the baseline estimates (red) with estimates obtained from an expanded sample that includes both groups (blue). The estimated employment and earnings effects are nearly identical across the two samples. The figure also reports estimates for a balanced sample consisting only of immigrants who file taxes in every year during the first 10 years after arrival (gray). Restricting the sample in this way has little effect on the estimated impacts, suggesting that non-filing behavior including return migration does not drive the baseline results.

Figure A.9 further examines the sensitivity of the estimates to alternative fixed effect specifications. The final two series successively reduce the flexibility of the baseline model by interacting only landing cohort fixed effects, or only landing cohort and destination province fixed effects, with year since arrival fixed effects. The resulting estimates remain very similar to those from the fully flexible specification, indicating that the main findings are not driven by the choice of fixed effects.

Figure A.10 presents robustness checks for alternative model specifications. Excluding individual controls (blue) leads to less precise estimates but does not alter the overall pattern of the results. Motivated by the possibility that the effects of continuous unemployment shocks may be nonlinear, I also allow both the IUR shocks and the predicted IUR to enter the model quadratically (gray). The estimated effects remain virtually unchanged, suggesting that the baseline findings are not driven by the assumption of a linear functional form. I additionally control for the logarithm of the initial immigrant population in the destination province and the immigrant population from the same country of origin in the destination province. The estimated effects remain stable, alleviating the concern discussed in Section 4.1.2 that the constructed unemployment rate shocks may be confounded by changes in local labor supply. To address the concern that the balance tests in Figure 4 may have limited statistical power to detect relationships between provincial IUR shocks and immigrants' language or reported occupation, I add controls for language and reported occupation to the main specification. The estimated effects remain essentially unchanged. I also account for

the possibility that immigrants from particular countries of origin may face origin-specific migration incentives in certain years, or that immigrants from the same country who choose different destination provinces may differ systematically. I therefore include interactions between country of origin and both landing-year and destination-province fixed effects. The results remain stable, suggesting that the estimated effects are not driven by origin-specific migration patterns over time or differential sorting of immigrants from particular origin countries across provinces.

Finally, Figure A.11 shows that the estimated employment and earnings effects are robust to alternative sample restrictions. Excluding immigrants with doctoral degrees, whose admission decisions may be more responsive to local labor market conditions, produces nearly identical estimates. Moreover, excluding immigrants who pursued post-secondary education in Canada after arrival, and therefore may have weaker labor force attachment immediately after landing, does not affect the results. The estimates are also unchanged when I exclude immigrants who arrived in January or December, for whom annual unemployment rates may provide a less accurate measure of labor market conditions at the time of arrival. Finally, I exclude immigrants who landed in 2008 or later, when processing times for economic immigrants shortened substantially and could potentially weaken the identification strategy. The estimated effects remain similar under this restriction.

6.1.3 Firm-level mechanisms

My baseline results show that arriving during recessions has persistent negative effects on immigrants' employment and earnings. Previous studies (Barsbai et al., 2025; Lucchetti and Ruggieri, 2025) have mainly examined the occupations in which immigrants work. An equally important question is whether adverse initial labor market conditions affect the firms to which immigrants are matched. By linking the IMDB to the CEEDD, I can identify immigrants' employers directly and investigate whether unlucky immigrants sort into different types of firms and how such sorting contributes to their long-run labor market outcomes.

Compared with the IMDB, the CEEDD covers a shorter period, from 2001 to 2019. Applying the baseline landing cohorts (1982–2012) would therefore produce an unbalanced panel because earlier cohorts are observed only in later years since arrival. I therefore restrict the analysis to immigrants who landed between 2000 and 2012 and examine outcomes during the first seven years after arrival. Figure A.12 also reports estimates from the corresponding unbalanced sample to assess the sensitivity of this restriction.

For each calendar year, I identify each immigrant's primary employer, defined as the firm

paying the highest annual wages. Immigrants without employment are not linked to any employer in that year. For employed immigrants, all firm-level outcomes are measured using the primary employer, and annual earnings are defined as wage earnings from that employer.

Figure A.12 shows that the CEEDD sample closely reproduces the baseline employment and earnings estimates. Under the flexible shock-based specification, immigrants exposed to larger IUR shocks experience initial employment and earnings penalties that gradually dissipate over time. The estimated effects are similar in magnitude and follow the same dynamics as those in Figure 5, although they are somewhat less persistent than the estimates based on the full IMDB sample. This similarity also indicates that earnings from an immigrant's primary employer closely track total annual wage earnings. Comparing the balanced and unbalanced CEEDD samples reveals virtually no difference in the employment estimates. However, the earnings effects in the balanced sample disappear after six years, whereas those in the unbalanced sample remain economically and statistically significant after seven years. This pattern suggests that differences in the composition of landing cohorts may account for part of the discrepancy in the persistence of the earnings effects.

The CEEDD contains the universe of worker-firm matches, allowing me to estimate worker and firm fixed effects within the AKM framework (Abowd et al., 1999). The estimation sample and procedure follow Dostie et al. (2023) and Kroft et al. (2026).[22] I interpret the estimated firm effects as firm-specific wage premiums and the worker effects as measures of workers' persistent earnings capacity. Following Card et al. (2024), I then aggregate the firm effects to the industry level, weighting each firm by its employment, to construct industry wage premiums.

Figure 6 plots the dynamic effects of arriving during recessions on firm characteristics and employer-switching behavior. Panel (a) shows that unlucky immigrants are more likely to work in firms with lower estimated wage premiums during the first three years after arrival.[23] One year after arrival, a one-percentage-point increase in the IUR shock reduces the AKM firm effect by 1%. The estimated decline in firm wage premiums accounts for approximately 13% of the initial primary-employer earnings penalty.

This initial sorting into lower-wage-premium firms may reflect either a greater reduction in overall hiring by higher-paying firms during recessions or a disproportionate reduction in their hiring of immigrants. To investigate these possibilities, Table A.5 examines how

[22]For the AKM estimation, I restrict the worker sample, which includes non-immigrants, permanent residents, and temporary residents, to individuals whose annual earnings exceed a full-time-equivalent threshold of $19,764 in 2016 dollars.

[23]Figure A.14 presents alternative measures of firm productivity, including log revenue and value added per worker, which exhibit similar patterns in response to IUR shocks.

firms' contemporaneous hiring responds to provincial labor market conditions, allowing the response to vary with estimated firm wage premiums.[24] Columns (1), (3), and (5) suggest that higher-wage-premium firms respond to adverse shocks by reducing overall hiring more sharply and decreasing the immigrant share of new hires. However, after controlling for industry-by-year fixed effects, the differential response in the number of new hires remains statistically significant, whereas the differential responses in the probability of hiring and the immigrant share of new hires are no longer significant. These findings suggest that reduced hiring by higher-wage-premium firms may contribute to immigrants' initial sorting into lower-premium employers. In contrast, there is no robust evidence that higher-paying firms disproportionately reduce their hiring of immigrants relative to other workers within the same industry and year.

With firm wage premiums accounting for 13% of the initial earnings penalty, the remaining 87% is attributable to within-firm earnings differences. Because annual earnings depend on both wage rates and time worked, this within-firm component may reflect lower hourly wages, fewer hours or weeks of work, or both. One potential explanation for lower wage rates is occupational downgrading. Barsbai et al. (2025) find that occupational downgrading explains most of the scarring effect on the hourly wages of family-sponsored immigrants. Similarly, Lucchetti and Ruggieri (2025) estimate that, in the absence of occupational downgrading, immigrants arriving under adverse conditions would catch up with their more fortunate counterparts within three years rather than twelve.

Unfortunately, the Canadian administrative data contain neither occupational information nor hours worked, preventing me from directly distinguishing among these mechanisms. To provide indirect evidence, I examine the characteristics of immigrants' coworkers. Panel (a) of Figure A.13 shows that immigrants arriving during recessions initially sort into firms whose coworkers have lower estimated AKM worker effects, although the estimates are not statistically significant.[25] I also examine the share of workers at each employer whose annual earnings fall below the full-time-equivalent threshold. I find that immigrants arriving during recessions are initially sorted into firms with a higher share of such low-earnings workers as shown in Panel (b) of Figure A.13.[26] These employers may offer more low-paid, part-time,

[24] New hires are defined as workers who are observed joining a firm for the first time. Given the available data, new hires can be identified beginning in 2002. Provincial labor market conditions are assigned based on firms' reported locations, which may correspond to their head offices rather than the actual locations of hiring, particularly for multi-establishment firms. The results should therefore be interpreted with caution.

[25] Average coworker AKM effects and the shares of immigrant and co-national immigrant coworkers within each firm are calculated on a leave-one-out basis, excluding the focal immigrant.

[26] Workers earning below the full-time-equivalent threshold are excluded from the sample used to estimate the AKM worker and firm effects.

or short-duration jobs that are also less well matched to immigrants' skills and experience. The results therefore suggest that occupational downgrading and reduced time worked may operate jointly, contributing to the large within-firm component of the earnings penalty.

An important question is whether the decline in firm wage premiums is partly attributable to sorting across industries. Figure 7 examines this possibility. Panel (b) shows that a one-percentage-point increase in the IUR shock reduces the wage premium of the employing industry by 0.4% in the first year after arrival.[27] Because industry wage premiums cannot be constructed for the full IMDB sample, panel (a) alternatively examines observable characteristics of immigrants' industries of employment using the IMDB sample. Immigrants exposed to adverse entry conditions are persistently sorted into lower-paying industries throughout their first ten years after arrival, whereas the estimated effects on the educational composition of these industries are generally smaller and not statistically significant. Although intended industries are not observed in the administrative data, I infer each immigrant's two most likely intended industries from the reported intended occupation.[28] Panel (a) of Figure A.22 provides suggestive evidence that immigrants exposed to adverse entry conditions are initially less likely to work in industries associated with their intended occupations, although the estimates are generally not statistically significant. These immigrants also appear more likely to switch industries during their early years in Canada. Over time, the estimates suggest an increasing likelihood of employment in their second-most-likely intended industry. Panel (b) further shows that these immigrants are less likely to work in construction and in professional, scientific, and technical services, but more likely to work in manufacturing and in administrative and support, waste management, and remediation services.

I find no statistically significant effects on firm employment size, as shown in panel (b) of Figure 6. Panel (c), however, shows that immigrants exposed to adverse entry conditions tend to work at firms with higher immigrant employment shares, with estimates statistically significant at the 10% level in years four and seven after arrival. This pattern is more pronounced when focusing on immigrant representation among the top 10% or 25% of earners within each firm. These findings suggest that affected immigrants are more likely to sort into firms where immigrants are relatively well represented not only in the overall workforce but also among higher-earning employees. This pattern does not appear to be driven by co-national referral networks: panel (d) shows no significant effects on the share of co-national immigrant coworkers, either overall or among the top 10% and 25% of earners within each

[27] The industry is at the 4-digit NOC level.

[28] The industry income and education indices, as well as the mapping from intended occupations to the two most likely industries, are constructed using the 2016 Census.

firm. This finding contrasts with studies showing that immigrants facing weak labor markets rely more heavily on ethnic networks in their job searches (Åslund and Skans, 2010; Barsbai et al., 2025). The comparison should be interpreted cautiously, however, because my estimates pool all immigration categories and economic immigrants constitute the majority of the sample. Moreover, because the sample excludes immigrants with prior Canadian work or study experience, most immigrants are likely to have relatively limited Canadian workplace and school-based networks at arrival, although they may still have access to family or co-ethnic networks.

The preceding results show that both the earnings penalty and the decline in firm wage premiums gradually dissipate. What drives this recovery? Panel (e) provides evidence on the role of job mobility.[29] Immigrants exposed to adverse entry conditions are more likely to change employers in their first year after arrival. Among those who change employers, panel (f) further shows that these immigrants are more likely to move to employers with higher firm effects in years two and four. Taken together, these findings suggest that job mobility facilitates movement toward higher-paying firms and may therefore contribute to earnings recovery. This pattern is broadly consistent with Oreopoulos et al. (2012), who highlight the role of job mobility in the earnings recovery of native college graduates entering weak labor markets.

Overall, the evidence suggests that immigrants who arrive during recessions initially sort into lower-paying firms with higher immigrant employment shares. These immigrants are more likely to switch employers shortly after arrival and, among those who switch, exhibit a greater tendency to move toward higher-wage-premium firms in subsequent years. These patterns suggest that job mobility may contribute to the gradual recovery in firm wage premiums and earnings.

6.1.4 Heterogeneity analysis

Figure A.15 presents the dynamic effects of IUR shocks on employment and earnings for female immigrants. Focusing on the preferred flexible shock-based specification, adverse initial labor market conditions have a larger initial effect on employment than in the male sample. A one p.p. increase in the IUR shock reduces female immigrants' employment rate by 2.6 p.p. one year since arrival, compared with 1.5 p.p. for male immigrants. However, the employment effects on females gradually diminish and are close to zero after six years since arrival as the effects on males. The earnings effects for female immigrants appear

[29] Job mobility is defined as a change in primary employer relative to the previous year.

more persistent than those for males. In particular, the estimated earnings effect remains statistically significant at (-2.4%) ten years after arrival. Nevertheless, the confidence intervals for the male and female estimates overlap substantially, and the estimated employment and earnings effects are generally not statistically distinguishable between the two groups.

Different immigration categories imply different selection criteria at admission and may therefore lead to different integration trajectories and different responses to adverse initial labor market conditions.

Figure 8 presents the dynamic employment and earnings effects by immigration class.[30] Panels (a) and (c) show that economic immigrants experience smaller employment and earnings losses than family-sponsored immigrants and refugees in the first ten years after arrival. To formally assess whether these differences are statistically significant, I estimate a specification that fully interacts the IUR shock, the predicted provincial IUR, and all fixed effects with an indicator for economic immigrants, and plot the coefficient on the interaction between the IUR shock and the economic-immigrant indicator in panels (b) and (d). Panel (b) shows that the interaction coefficient for employment is positive and statistically significant throughout the first ten years since arrival, indicating that economic immigrants experience significantly smaller employment losses than non-economic immigrants following adverse initial labor market conditions. Panel (d) shows a similar but less persistent pattern for earnings. The interaction coefficient is positive and statistically significant in year one (at the 5% level) and in years two, three, and five (at the 10% level), suggesting that economic immigrants experience smaller earnings losses in these years.

These heterogeneous effects are broadly consistent with previous evidence on specific immigrant groups. Relative to the family-sponsored immigrants studied by Barsbai et al. (2025), I find somewhat larger employment and earnings losses upon arrival. By contrast, the estimated effects for refugees are similar in magnitude to those reported by Vigezzi (2025).

A natural explanation for the heterogeneity by immigration class is that economic immigrants are positively selected on characteristics valued in the labor market, including education, language proficiency, and other productive skills. Although they do not receive

[30] Figure A.17 replicates the balance tests separately for economic immigrants, family-sponsored immigrants, and refugees to assess whether the identifying assumptions hold within each immigration class. Overall, landing characteristics exhibit no systematic relationship with provincial IUR shocks in any of the three classes. The only exceptions are among refugees: adverse IUR shocks are negatively associated with the share of refugees holding a doctoral degree and positively associated with the share having some postsecondary education, both significant at the 10% level. Meanwhile, education is included as a control in the main specifications, mitigating concerns about selection along this observable dimension. Moreover, given the large number of balance tests, an occasional statistically significant estimate may arise by chance even when the identifying assumptions hold. Columns (5)–(7) of Table 4 further show that immigrant inflows are unrelated to IUR shocks across immigration classes.

sponsorship from family members or the government, these characteristics may make them more resilient to adverse labor market conditions at arrival, resulting in smaller employment and earnings penalties than those experienced by family-sponsored immigrants and refugees.

To examine whether differences in human capital account for the greater resilience of economic immigrants, I estimate heterogeneous effects by educational attainment and English-language proficiency.[31] Panels (a) and (b) of Figure A.16 show that immigrants with at least a bachelor's degree experience significantly smaller employment losses during the first four years after arrival, but their earnings losses are not statistically distinguishable from those of less-educated immigrants. I find no statistically significant heterogeneity by English-language proficiency. These findings suggest that higher educational attainment may help cushion employment losses without necessarily mitigating earnings losses. Occupational downgrading offers one possible explanation for this distinction. Because higher-skilled immigrants have greater scope to be displaced into jobs below their qualifications during recessions, occupational downgrading may offset the earnings advantages associated with higher educational attainment (Chiswick et al., 2005).

More importantly, panels (c) and (d) of Figure A.16 show that allowing the effects of IUR shocks to vary by educational attainment leaves the estimated differences between economic and non-economic immigrants largely unchanged. These findings suggest that differences in educational attainment alone cannot account for the greater resilience of economic immigrants. One potential explanation is occupation-specific selection. Economic immigration programs select applicants partly on their expected labor-market adaptability, and some streams prioritize applicants with experience in occupations facing labor shortages. Because these targeted occupations span different skill levels, the benefits of such selection need not be captured by educational attainment or English-language proficiency. Immigrants with occupational experience that aligns more closely with Canadian labor demand may be better positioned to secure jobs that utilize their existing skills, even during recessions, potentially mitigating both employment losses and occupational downgrading. Family-sponsored immigrants and refugees, by contrast, are generally not admitted based on occupation-specific labor demand and may face greater difficulties translating their pre-migration skills and experience into suitable employment when labor market conditions are weak.

To further investigate why the effects differ across immigration classes, I examine heterogeneity in firm-level outcomes. Figure 9 presents the effects of IUR shocks by immigration

[31] Both education and English-language proficiency are measured at landing. Educational attainment is measured using an indicator for whether the immigrant has at least a bachelor's degree, while English-language proficiency is measured by whether the immigrant reported speaking English at landing.

class. Panel (a) reports the effects on firm wage premiums separately for economic immigrants, family-sponsored immigrants, and refugees, while panel (c) reports the corresponding effects on the share of co-national immigrants employed by the firm. Panels (b) and (d) report the coefficients and confidence intervals for the interaction between IUR shocks and an indicator for economic immigrants for these two outcomes.

Panel (b) suggests that economic immigrants experience smaller declines in firm wage premiums than non-economic immigrants, although the differences are imprecisely estimated except in year four after arrival. Panel (d) shows a clearer distinction: following adverse entry conditions, economic immigrants are significantly less likely than non-economic immigrants to be sorted into firms with higher shares of co-national coworkers. The class-specific estimates in panel (c) indicate that this difference is driven primarily by family-sponsored immigrants, who are persistently sorted into firms with higher co-national shares. These results indicate that the null average effect on co-national coworker concentration in panel (d) of Figure 6 masks substantial heterogeneity across immigration classes. One possible explanation is that family-sponsored immigrants have pre-existing family and community ties that facilitate access to co-ethnic job referrals, particularly when labor-market conditions are weak.

To examine whether access to a larger enclave moderates the effects of adverse entry conditions, I compare family-sponsored immigrants arriving in relatively large and small enclaves. I measure enclave size as the number of tax-filing immigrants from the same country of origin who resided in the destination province in the year before the immigrant's arrival. Using the lagged network stock ensures that the measure is not mechanically affected by the current landing cohort. I then interact the IUR shock with the logarithm of enclave size and allow the destination-province and landing-year fixed effects to vary with enclave size.[32] Figure 10 reports the resulting estimates for family-sponsored immigrants.[33]

Panel (d) shows that, among family-sponsored immigrants exposed to adverse entry conditions, those arriving in larger enclaves are significantly more likely to work in firms with higher shares of co-national coworkers. This pattern is consistent with larger enclaves facilitating greater reliance on co-ethnic networks during recessions. It also parallels Barsbai

[32]Enclave size varies systematically across destinations and origin groups. Ontario has relatively large enclaves because of its large immigrant population, while immigrants from South Asia also tend to have larger co-national networks. To reduce the possibility that the estimates capture heterogeneity associated with these characteristics, I additionally control for interactions of the IUR shock with indicators for Ontario as the destination province and South Asian origin. Enclave size may nevertheless proxy for other correlated characteristics, so this analysis should be interpreted as suggestive.

[33]I focus on family-sponsored immigrants because the preceding results indicate that their response to adverse entry conditions is most strongly associated with increased co-national coworker concentration. Corresponding heterogeneity by enclave size is limited among economic immigrants and refugees.

et al. (2025), who find that family-sponsored immigrants arriving during recessions are more likely to enter occupations with greater concentrations of co-nationals, particularly when migrant networks are larger. The consequences of network access, however, appear different in the present setting. Panel (a) provides no evidence that larger enclaves mitigate employment losses, while panel (b) indicates larger earnings losses in years two and three after arrival. Panel (c) further suggests that the network response directs family-sponsored immigrants toward firms offering lower wage premiums. Taken together, these estimates suggest that larger enclaves strengthen the co-ethnic sorting channel without necessarily improving labor-market outcomes. One possible interpretation is that enclave networks direct immigrants toward a narrower set of lower-paying employers whose jobs may be particularly vulnerable during downturns. This interpretation is consistent with the broader evidence in Dustmann et al. (2010) that immigrant employment is more cyclically sensitive, although the present analysis does not directly establish that firms with high co-national employment shares are more exposed to recessions.

Lastly, I find additional heterogeneity by landing age, expected labor market conditions, and country of origin. Panels (a) and (b) of Figure A.18 compare immigrants who landed at ages 45 or older with those who landed before age 45. Older immigrants experience significantly larger employment and earnings losses. This pattern is consistent with higher job search costs and lower job mobility at older ages, which may limit their ability to recover from adverse entry conditions, as suggested by Oreopoulos et al. (2012). Panels (c) and (d) compare immigrants who arrived during periods of relatively high and low predicted unemployment.[34] I find that a one-percentage-point increase in IUR shocks leads to larger earnings losses when predicted unemployment is low. One possible explanation is that a given positive shock represents a larger deterioration relative to expectations when the underlying labor market is initially strong. By contrast, Lucchetti and Ruggieri (2025) find that entering the U.S. labor market during a recession amplifies the negative effects on immigrants' earnings trajectories. Their recession measure is based on the realized national business cycle (or the official NBER Business Cycle Dating), whereas I define expected recessions using the predicted provincial unemployment rate. The two measures therefore capture different aspects of initial labor market conditions.

Finally, panels (e) and (f) compare immigrants from advantaged and disadvantaged origin countries. The classification follows Dostie et al. (2023) and defines advantaged countries as the U.S., Northern Europe, Australia, and New Zealand. Consistent with their finding

[34]Periods of expected recession are defined as those in which the predicted provincial IUR exceeds 7.5 p.p.

that immigrants from advantaged origin countries have better labor market outcomes upon arrival, I find that they also experience significantly smaller earnings losses following adverse initial labor market conditions. Interestingly, immigrants from advantaged countries become less likely to be employed 3–8 years after arrival. The delayed emergence of this pattern may partly reflect selective return migration, as immigrants from advantaged origin countries may have more attractive outside options in their home countries and thus be more likely to leave Canada in response to adverse entry conditions.

6.1.5 Interprovincial mobility as an adjustment mechanism

As discussed in Section 2, permanent residents are generally free to choose where to reside after landing. Although the intended destination province may be the optimal location when the immigration application is submitted, unexpected labor market conditions at arrival may alter the attractiveness of remaining there. Figure 11 shows that immigrants facing adverse initial labor market conditions are more likely to switch away from their intended destination province. A one p.p. increase in IUR shocks raises the probability of residing in another province ten years after arrival by 2.6 p.p. Given that 18% of immigrants in my sample switch to another province within the first ten years after arrival, this represents a sizeable effect. Because the outcome is a stock measure of migration, the dynamic pattern suggests that most interprovincial mobility effects occur within the first two years after arrival. In addition, immigrants exposed to adverse initial labor market conditions are more likely to switch to gateway cities (Toronto, Vancouver, and Montreal), although the estimates are only marginally statistically significant. This pattern is consistent with Vigezzi (2025), who finds a similar response among Government-Assisted Refugees. However, migration to gateway cities explains only part of the increase in interprovincial mobility.

To examine whether interprovincial mobility mitigates the consequences of adverse entry conditions, I compare the employment and earnings effects by whether immigrants switch to another province within the first ten years after arrival. Panels (a) and (c) of Figure 12 show that the estimates for stayers closely resemble the baseline results. By contrast, switchers experience earnings losses only in the landing year and exhibit positive employment and earnings effects at longer horizons. Panels (b) and (d) show that the differences between switchers and stayers are economically and statistically significant.

When local labor markets are geographically segmented, relocating to another province may reduce exposure to adverse conditions in the intended destination. This adjustment margin is particularly relevant if the initial positive IUR shock predicts persistently weaker

labor market conditions in that province. Figure A.19 reports estimates that additionally control for the contemporaneous unemployment rate in the intended destination province. Including this control attenuates both the employment and earnings effects, although the estimates remain significantly negative, particularly for earnings. This pattern suggests that the baseline estimates capture both the effect of labor market conditions at arrival and the consequences of continued exposure to correlated subsequent conditions. Immigrants who settle directly in a province other than their intended destination avoid exposure to labor-market conditions there from the outset, whereas those who initially settle in the intended province but subsequently move away can reduce their exposure to later local labor-market conditions.

The comparison between observed switchers and stayers may nevertheless be biased because mobility is endogenous to the IUR shock. Figure 11 demonstrates that adverse entry conditions affect the decision to relocate. Moreover, immigrants with stronger employment prospects or greater financial resources may be more able or willing to move, generating positive selection among observed switchers. To assess the implications of endogenous mobility, I adapt the trimming approach of Lee (2009) to bound the employment and earnings effects within principal strata defined by mobility status. The central monotonicity assumption requires that adverse entry conditions may induce additional immigrants to leave their intended destination province but cannot cause an immigrant who would otherwise leave to remain. Because this application uses a DID design without individual-level pre-treatment outcomes, additional assumptions are imposed to identify the bounds. Further details are provided in Appendix B. Because the canonical Lee-bounds procedure requires a binary treatment, I classify IUR shocks as either adverse or favorable. Figure 13 reports the resulting bounds for always destination residents and always non-destination residents—immigrants who would remain in or leave their intended destination province regardless of the shock—with both contrasts expressed as the effect of an adverse shock. For always destination residents, the employment and earnings bounds remain entirely below zero during the first few years after arrival. For always non-destination residents, however, the bounds are wide, include zero, and overlap with those for always destination residents. The lack of precision reflects the strong mobility response to adverse conditions: treatment-induced switchers constitute a sizeable share of observed switchers in adversely affected cells. The estimated trimming share is the proportion of observed switchers who are always non-destination residents; the complementary proportion is trimmed to account for treatment-induced switching. Retaining either the lower or upper tail of the always-resident share produces substantially different

outcome means and, consequently, wide bounds.

Taken together, the results show that adverse entry conditions induce substantial interprovincial mobility. The unadjusted comparisons indicate that observed switchers have more favorable subsequent employment and earnings trajectories than stayers, a pattern consistent with geographic mobility serving as an adjustment mechanism. This comparison alone does not establish that moving causes the improvement, however, because immigrants select into mobility and the composition of observed switchers changes with the IUR shock. The Lee-bounds analysis continues to identify negative short-run effects for always destination residents but yields uninformative bounds for always non-destination residents. After all, geographic mobility represents a potentially important adjustment margin, but identifying its causal effect would require exogenous variation in immigrants' incentives or ability to relocate. I leave this question for future research.

6.1.6 Additional outcomes

Figure A.20 examines the effects of initial labor market conditions on educational, health, political, and family outcomes. Panel (a) provides some evidence that immigrants respond to adverse entry conditions by investing more in human capital. In particular, immigrants exposed to larger IUR shocks are more likely to claim education deductions for part-time study one year after arrival and for full-time study around six years after arrival. Panel (b) shows little evidence that initial labor market conditions affect subsequent health outcomes. Panel (c) shows that unlucky immigrants are more likely to become Canadian citizens around five years after arrival[35], possibly because naturalization improves labor market opportunities or reduces barriers associated with immigrant status (Bratsberg et al. 2002; Gathmann and Keller 2018). Panel (d) examines federal political contributions. Although unlucky immigrants are less likely to make a positive contribution in year three, they are more likely to contribute in years six and nine. Panel (e) examines marriage, separation, divorce, and spousal immigrant status. Unlucky immigrants are more likely to separate or divorce between years two and four after arrival and, correspondingly, are less likely to be married around year three. Among those who are married, however, I find no evidence that adverse entry conditions influence the likelihood of having an immigrant spouse. Finally, Panel (f) suggests that adverse labor market conditions at arrival affect fertility: unlucky immigrants are less likely to have children and, conditional on having children, have fewer children.

[35]The estimate for the landing year is omitted, and the estimates for the first two years after arrival are close to zero, reflecting the minimum residence requirement of three years for Canadian citizenship.

Figure A.21 reports the effects of IUR shocks on additional IMDB outcomes. Panel (a) shows that the dynamic patterns for annual earnings and total income closely resemble those for annual earnings. Panel (b) indicates that unlucky immigrants are more likely to receive government transfers and Employment Insurance (EI) benefits, consistent with weaker labor market attachment following adverse entry conditions. Panel (c) provides little evidence that initial labor market conditions affect self-employment, but shows increases in the number of T4 slips and the probability of low-income status. Panel (d) further shows that unlucky immigrants receive more government transfers while reporting lower medical expenditures, capital gains, and self-employment earnings.

Finally, I use the linked 2016 Census to examine outcomes that are unavailable in the IMDB, including language use, educational attainment after landing, labor supply, occupation, and activity limitations, including those related to mental health conditions. The upper half of Figure A.23 reports results for immigrants in the main sample. I find little evidence that adverse entry conditions affect the language spoken at home or at work. There is some evidence that unlucky immigrants are more likely to obtain additional post-secondary education in Canada after landing, although the estimates are only marginally significant. Consistent with the IMDB results, unlucky immigrants have lower annual and weekly earnings in 2015, while neither extensive nor intensive margins of labor supply are significantly affected. I also find no significant effects on the probability of working in the same occupation as reported at landing, measures of occupational earnings and educational requirements, or activity limitations.

6.2. Intergenerational effects

Persistent labor market disadvantages experienced by immigrants may also affect the next generation. The literature shows that adverse labor market and income shocks experienced by parents can have lasting intergenerational consequences for their children's human capital accumulation and labor market outcomes (Oreopoulos et al., 2008; Coelli, 2011; Dahl and Lochner, 2012). Using the intergenerational specification in (15), Figure 14 examines whether adverse labor market conditions at fathers' arrival affect their children's labor market outcomes measured between ages 30 and 34.

Overall, I find little evidence that fathers' initial labor market conditions affect their children's employment. However, they do affect children's income. A one p.p. increase in fathers' IUR shock reduces their children's annual income by approximately 1%. These average effects mask substantial heterogeneity across immigration classes. Children of economic

immigrants are more likely to be employed than children of non-economic immigrants when their fathers experienced adverse entry conditions. By contrast, the negative income effects are concentrated among children of family-sponsored immigrants, and the differences relative to economic immigrants are statistically significant. One possible explanation is that adverse entry conditions have smaller effects on the labor market outcomes of economic immigrants themselves. If parents experience smaller earnings losses, the resulting reduction in family resources available for children's human capital investment may also be smaller. This interpretation is consistent with the heterogeneity documented in Figure 8, although the precise mechanisms cannot be identified from the data.

The refugee results differ from those for family-sponsored immigrants. I do not find statistically significant intergenerational effects of fathers' initial labor market conditions on refugee children. This finding is broadly consistent with Vigezzi (2025), who studies Government-Assisted Refugees and finds that adverse local labor market conditions at arrival have little lasting effect on children's educational attainment, employment, citizenship, or intermarriage outcomes. Vigezzi (2025) attributes this resilience partly to higher rates of secondary migration and greater post-arrival educational investment, mechanisms that may also mitigate intergenerational scarring in my setting.

Figure A.24 repeats the analysis using mothers' initial labor market conditions. I find little evidence of average intergenerational effects. However, children of family-sponsored and refugee mothers experience lower earnings and income at ages 30–34. These findings should be interpreted cautiously. They differ from Spencer and Matsuzawa (2025), who study native families rather than immigrants, making the underlying mechanisms potentially different. Moreover, mothers and fathers frequently immigrate together, implying that maternal and paternal entry conditions are highly correlated. As a result, the estimated effects of mothers' initial labor market conditions may partly reflect the joint labor market shocks experienced by both parents rather than maternal shocks alone.

Finally, I use the linked 2016 Census to examine additional long-run outcomes for 1.5-generation immigrant children, as shown in the lower half of Figure A.23. Most estimates are imprecise. I find little evidence that fathers' initial labor market conditions affect children's educational attainment, language use, occupations, or activity limitations. There is, however, some evidence that children of unlucky immigrants are more likely to appear in the 2016 Census, suggesting they are more likely to remain in Canada, and they earn lower weekly wage earnings in 2015. These results are broadly consistent with the administrative tax data, although the Census sample is substantially smaller and therefore provides less precise

estimates.

7. Conclusion

This paper uses rich Canadian administrative data to examine how labor market conditions at arrival shape immigrants' long-run economic integration. To identify the causal effect of initial labor market conditions, I exploit the time gap between migration decisions and actual admission and use province-by-cohort forecast errors in initial unemployment rates—defined as the difference between realized and predicted unemployment rates at arrival—as plausibly exogenous variation.

The results show that arriving during recessions has substantial and persistent effects on immigrants' employment and earnings. Linking immigrants to their employers reveals that an important mechanism is initial sorting into lower-paying firms with higher concentrations of immigrant workers. Although immigrants gradually recover through job mobility, the persistence of the earnings effects is consistent with costly search and imperfect matching in the labor market. Geographic mobility also appears to be an important adjustment mechanism. Immigrants exposed to adverse initial labor market conditions are more likely to switch to another province, and observed switchers experience smaller scarring effects than stayers. However, the wide Lee bounds for always non-destination residents imply that selection into mobility cannot be ruled out as an explanation for this difference.

The effects are not uniform across immigrant groups. Economic immigrants experience smaller employment and earnings losses than family-sponsored immigrants and refugees, and these differences cannot be explained by observed education or language proficiency. Family-sponsored immigrants are more likely to work in immigrant enclaves when they arrive during adverse labor market conditions, suggesting greater reliance on enclave networks in the job search process. However, I find no evidence that access to larger enclave networks attenuates the employment or earnings effects of adverse entry conditions. The consequences also extend beyond the first generation: fathers who arrive under unfavorable labor market conditions have children with lower income in adulthood, with these intergenerational effects concentrated among family-sponsored immigrants.

These findings suggest that the consequences of arriving during recessions are both persistent and far-reaching. In addition to reducing employment and earnings, adverse entry conditions increase the likelihood of low-income status and reliance on government transfers, implying broader fiscal and social costs. Policies that facilitate immigrants' adjustment to local labor market conditions may therefore generate benefits for both immigrants and host

countries.

Several policy implications emerge from the analysis. Existing programs provide stronger insurance against job loss than against unsuccessful job search at labor market entry. Expanding employment-related settlement services, particularly during periods of weak labor demand, could help immigrants obtain better initial job matches. The evidence on employer sorting and job mobility also suggests that employment support should extend beyond immigrants' initial job search. Programs that facilitate job-to-job transitions, provide information about better employment opportunities, and help immigrants find jobs that more closely match their skills could contribute to their subsequent earnings recovery. The results on geographic mobility similarly suggest a potential role for policies that reduce information and relocation frictions. For example, settlement services could provide immigrants with more localized information about labor market opportunities across regions, while relocation assistance could make it easier to respond to weak conditions in their initially intended destinations. In addition, extending the permissible window between the issuance of a COPR and landing in response to adverse labor market shocks could give prospective immigrants greater flexibility to adjust the timing of their arrival, potentially mitigating the long-term consequences of entering the Canadian labor market during a recession. Evaluating the effectiveness of these policy interventions is an important direction for future research.

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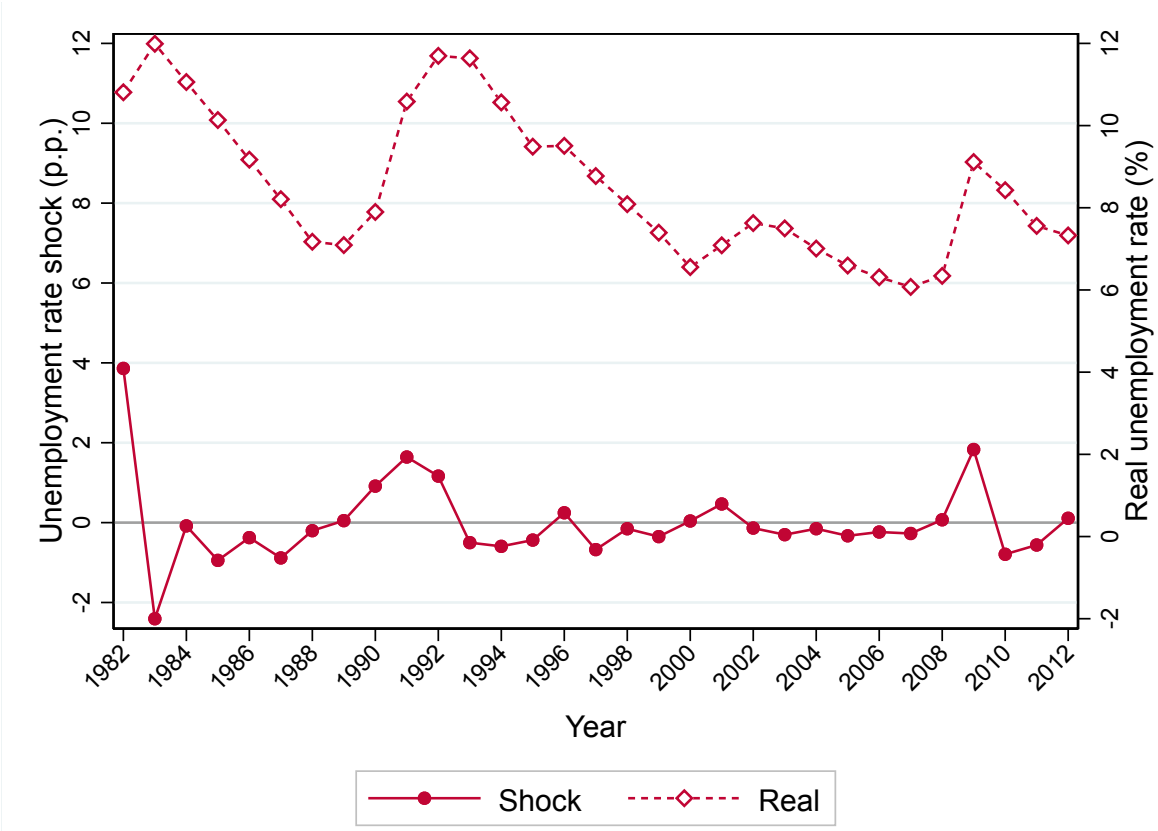
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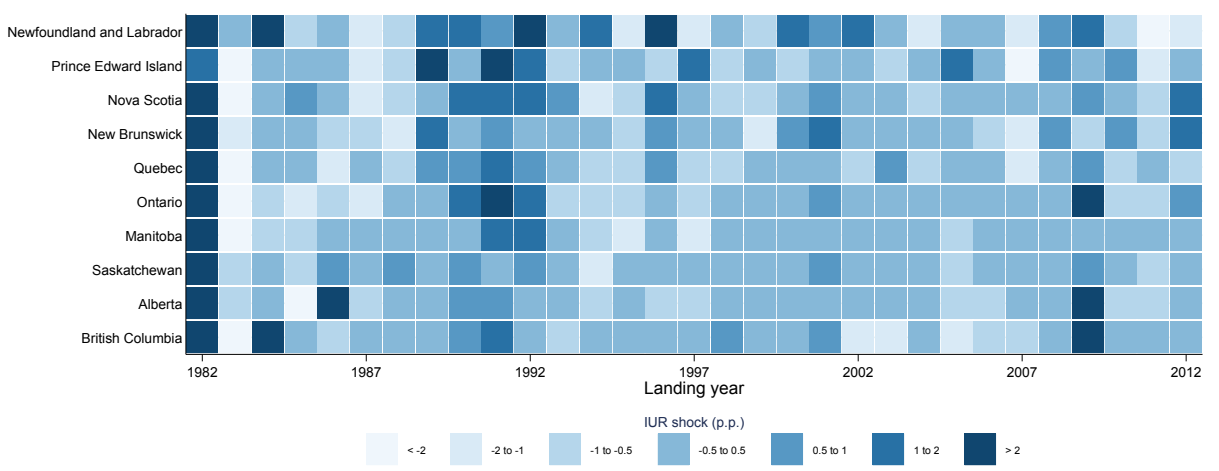
Figures and Tables

Figure 1: Trend of national unemployment rate (shocks)



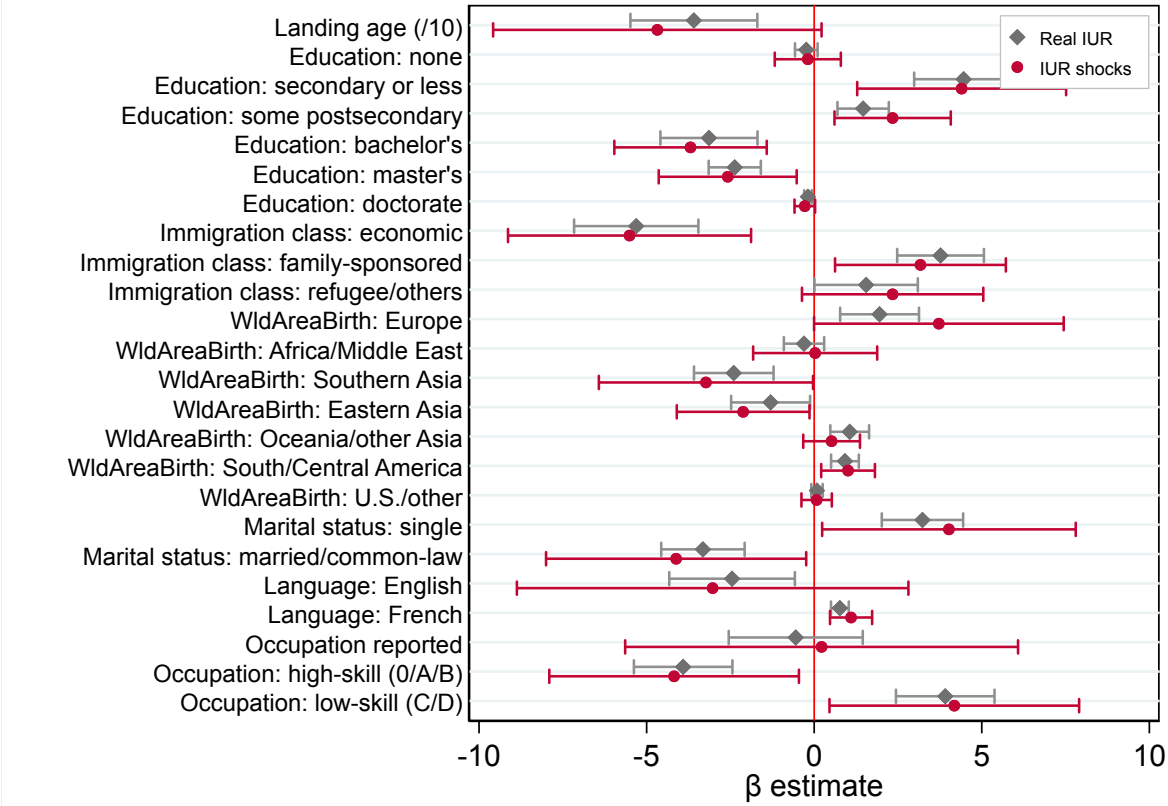
Notes: The figure plots the national unemployment rate and the unemployment rate shock measure from 1982 to 2012. The shock is defined as the difference between the realized unemployment rate and its one-year-ahead predicted value.

Figure 2: Distribution of provincial initial unemployment rate shocks



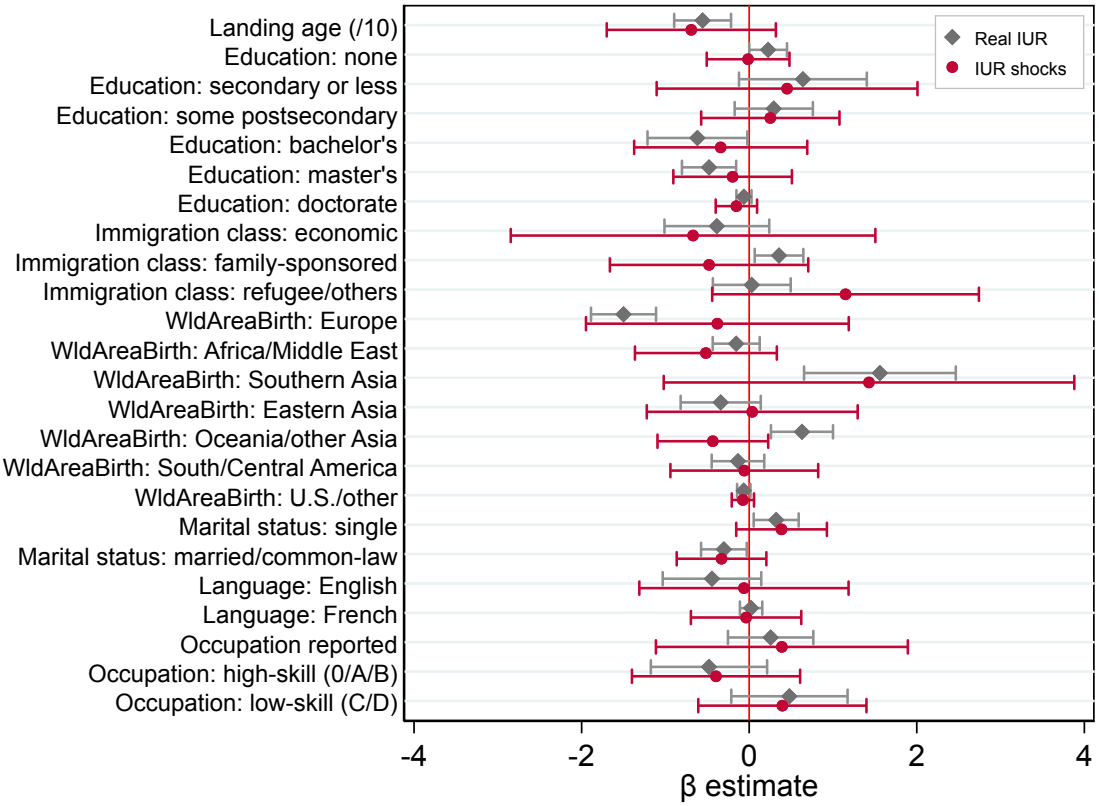
Notes: The figure displays the distribution of provincial initial unemployment rate shocks across Canadian provinces from 1982 to 2012.

Figure 3: Coefficient estimates relating national initial labor market conditions to landing characteristics



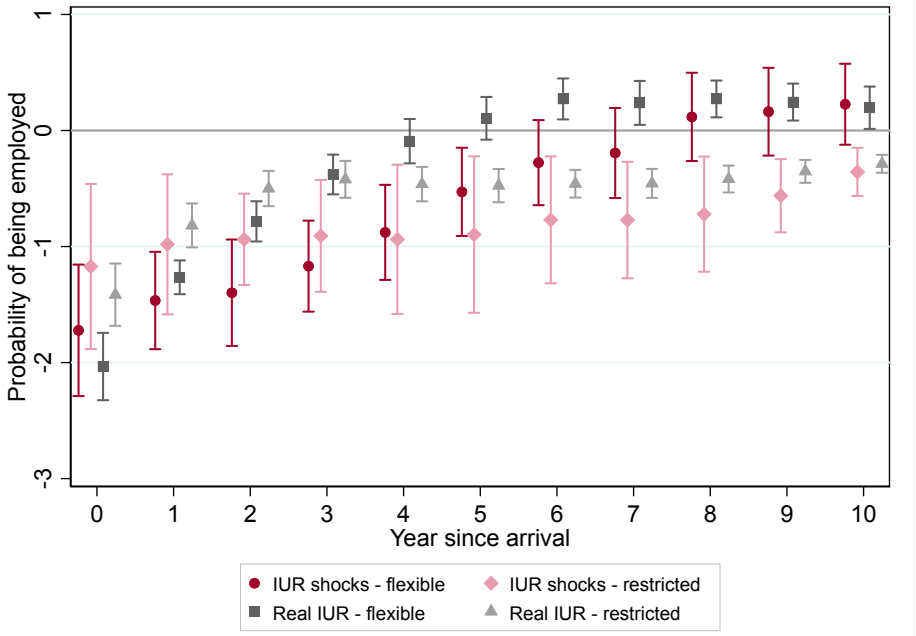
Notes: The figure reports coefficient estimates and 95% confidence intervals from regressions of landing-cohort-level immigrant landing characteristics on measures of national initial labor market conditions. Gray diamonds and horizontal bars correspond to regressions using the realized national IUR. Red circles and horizontal bars report the coefficient estimates on IUR shocks from regressions that include both IUR shocks and predicted IUR. The sample consists of male immigrant landing cohorts from 1982 to 2012.

Figure 4: Coefficient estimates relating provincial initial labor market conditions to landing characteristics

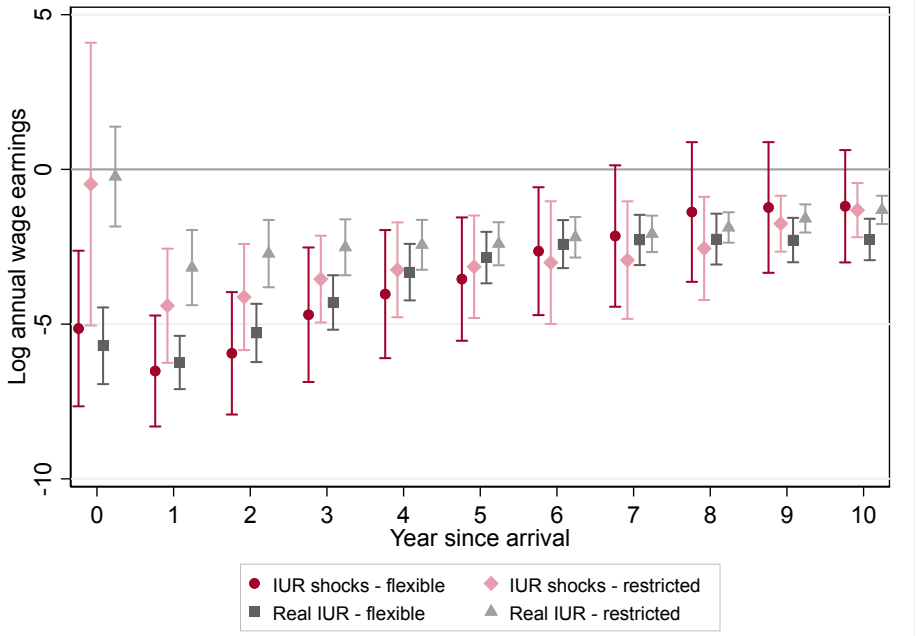


Notes: The figure reports coefficient estimates and 95% confidence intervals from regressions of province-cohort-level immigrant landing characteristics on measures of provincial initial labor market conditions. Gray diamonds and horizontal bars correspond to coefficient estimates from regressions using the realized provincial IUR, controlling for destination province and landing year fixed effects. Red circles and horizontal bars report coefficient estimates on IUR shocks from regressions that include both IUR shocks and predicted IUR, while controlling for the same fixed effects. The sample consists of male immigrant landing cohorts from 1982 to 2012.

Figure 5: Dynamic effects of the initial unemployment rate shocks on employment and earnings



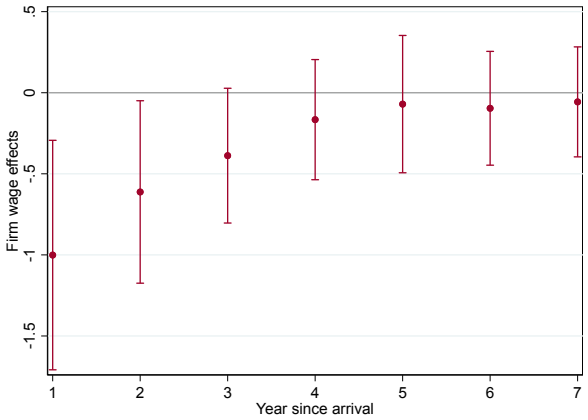
(a) Employment probability



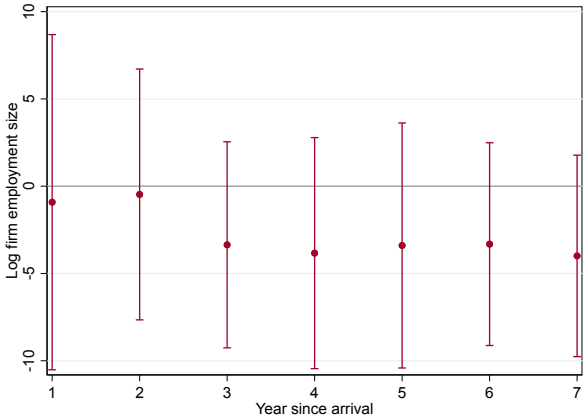
(b) Real annual earnings

Note: The figure plots the estimated dynamic effects of initial unemployment rate (IUR) shocks on employment (panel (a)) and annual earnings (panel (b)) over the first ten years since arrival. Dark red circles and vertical bars report coefficient estimates and 95% confidence intervals from the flexible shock-based specification in equation (14). Dark gray diamonds and bars report the corresponding flexible realized-IUR estimates. Light red and light gray series report estimates from the restricted shock-based specification in equation (13) and the corresponding restricted realized-IUR specification, respectively. The sample consists of male immigrants who landed between 1982 and 2012.

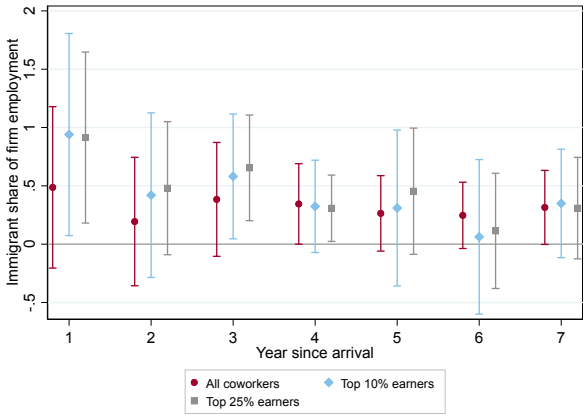
Figure 6: Dynamic effects of initial unemployment rate shocks on firm outcomes



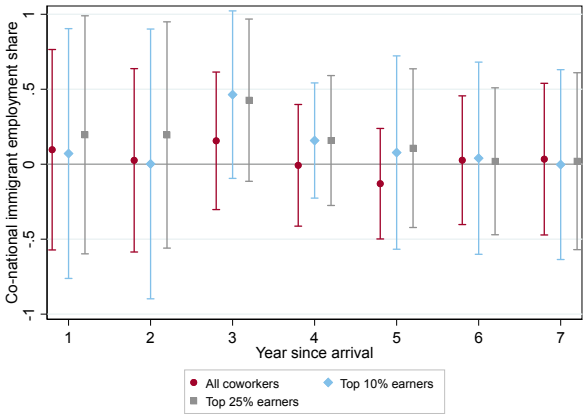
(a) Firm wage premium (AKM)



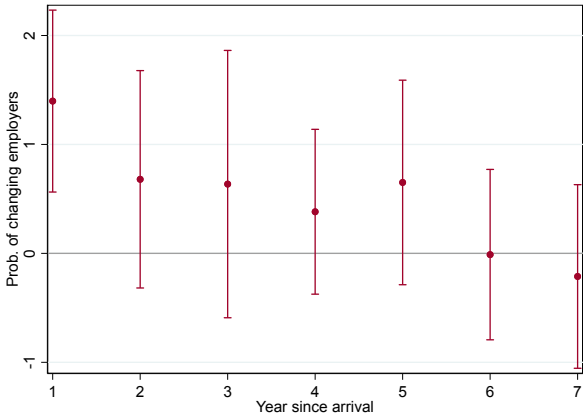
(b) Log firm employment size



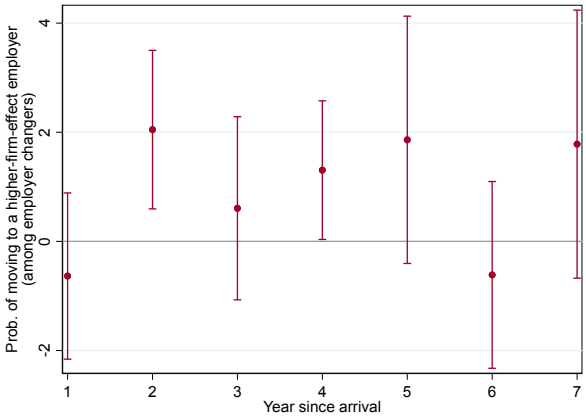
(c) Share of immigrants in the firm



(d) Share of co-national immigrants in the firm



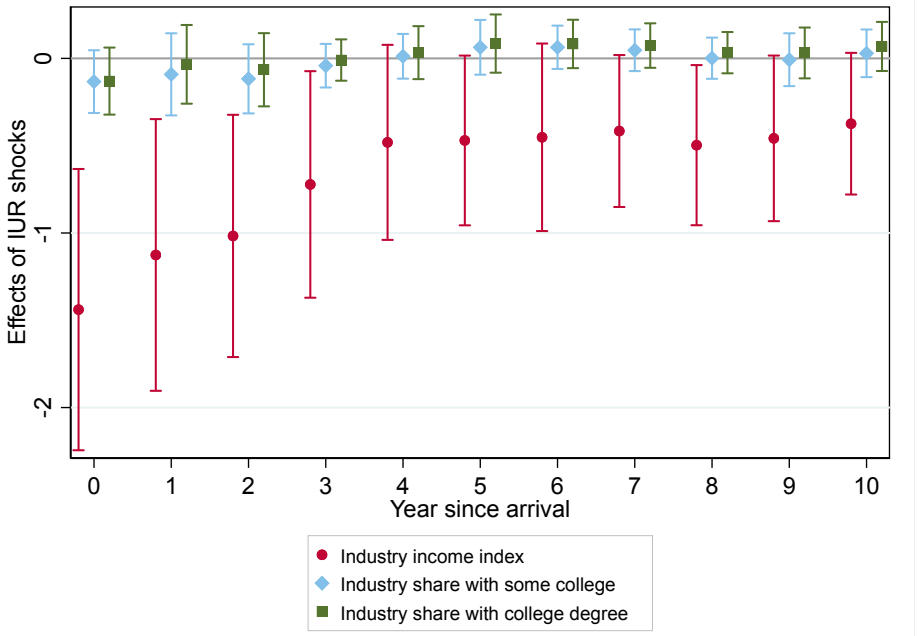
(e) Probability of changing employers



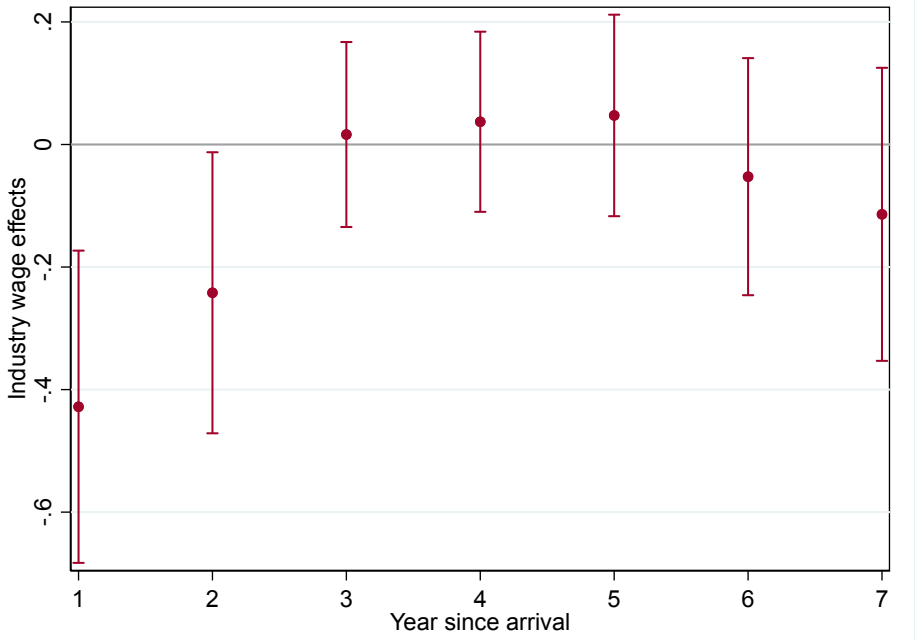
(f) Probability of moving to a higher-firm-effect employer among employer changers

Notes: The figure plots the estimated dynamic effects of initial unemployment rate (IUR) shocks on firm wage premiums (panel (a)), firm employment (panel (b)), the leave-one-out share of immigrants employed by the firm (panel (c)), the leave-one-out share of co-national immigrants employed by the firm (panel (d)), the probability of changing employers (panel (e)), and the probability of moving to a higher-firm-effect employer conditional on changing employers (panel (f)) over the first seven years after arrival. Firm-level outcomes are measured using each immigrant's primary employer, defined as the employer paying the highest annual earnings in a given year. Vertical bars denote 95% confidence intervals. Estimates are obtained from the flexible shock-based specification. The sample consists of male immigrants who landed between 2000 and 2012.

Figure 7: Dynamic effects of the initial unemployment rate shocks on industry sorting



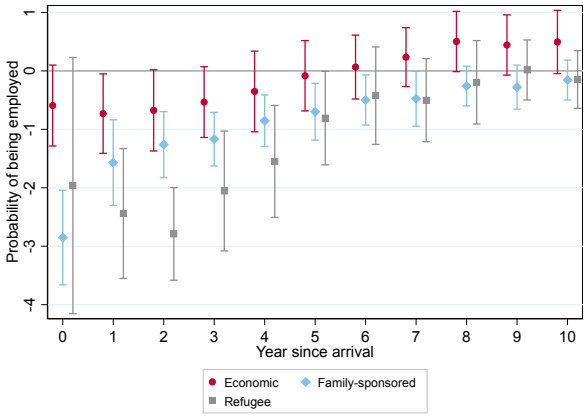
(a) Industry characteristics



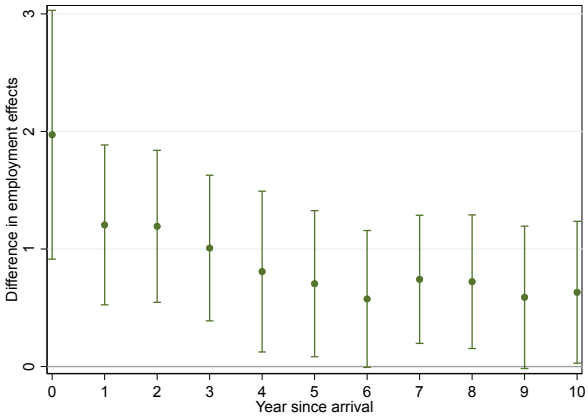
(b) Industry wage premium (AKM)

Note: The figure reports the dynamic effects of initial unemployment rate (IUR) shocks on characteristics of immigrants' industries of employment. In panel (a), the red, blue, and green series show the estimated effects on the average industry income index, the share of workers with some college education, and the share of workers with at least a college degree, respectively, measured at the two-digit NAICS level. Panel (b) reports the estimated effects on the industry wage premium, measured at the four-digit NAICS level. Vertical bars denote 95% confidence intervals. The sample in panel (a) consists of male immigrants who landed between 1982 and 2012, whereas the sample in panel (b) consists of male immigrants who landed between 2000 and 2012.

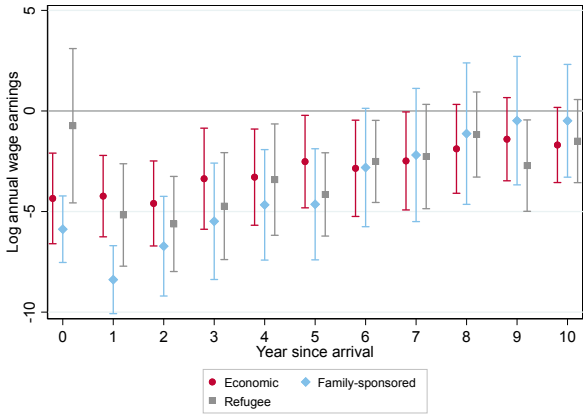
Figure 8: Heterogeneous effects of initial unemployment rate shocks by immigration class



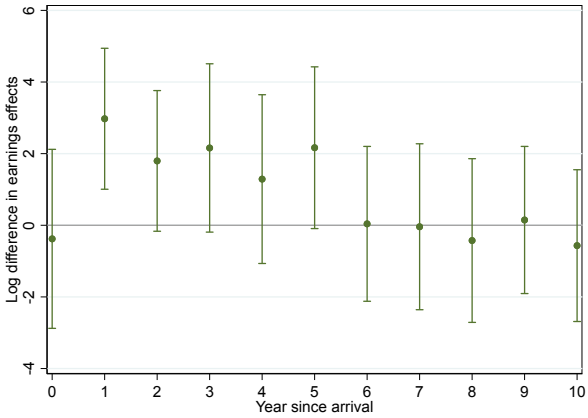
(a) Employment effects by immigration class



(b) IUR shock × economic immigrant: employment



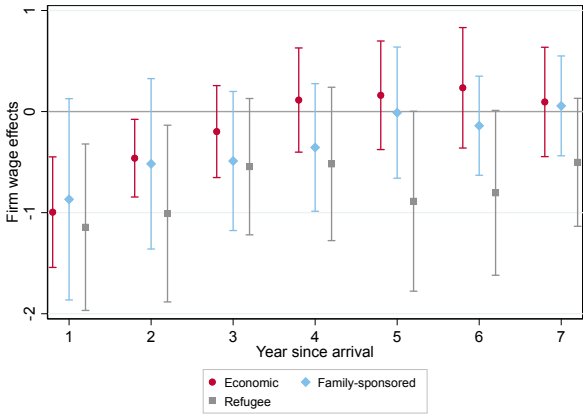
(c) Earnings effects by immigration class



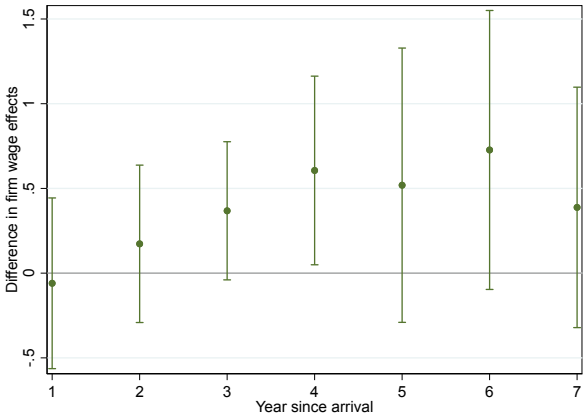
(d) IUR shock × economic immigrant: earnings

Notes: The figure plots heterogeneous effects of initial unemployment rate (IUR) shocks by immigration class. Panels (a) and (c) report the estimated employment and earnings effects separately for economic, family-sponsored, and refugee immigrants. Panels (b) and (d) report the interaction coefficient between the IUR shock and an indicator for economic immigrants, which tests whether the effects differ between economic and non-economic immigrants. Vertical bars denote 95% confidence intervals. Estimates are obtained from the flexible shock-based specification. The sample consists of male immigrants who landed between 1982 and 2012.

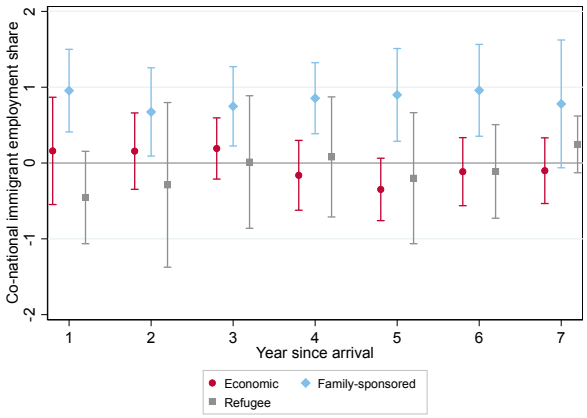
Figure 9: Heterogeneous effects of initial unemployment rate shocks on firm outcomes by immigration class



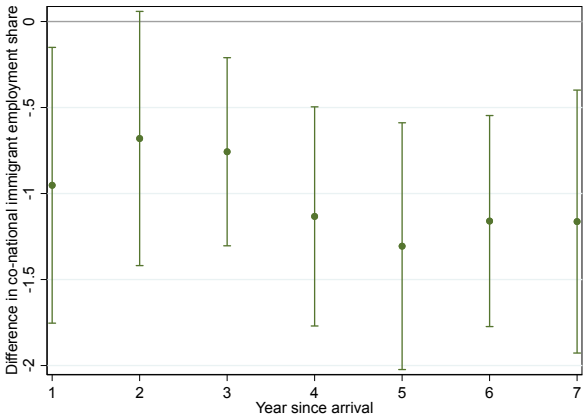
(a) Firm wage premium effects by immigration class



(b) IUR shock × economic immigrant: firm wage premiums



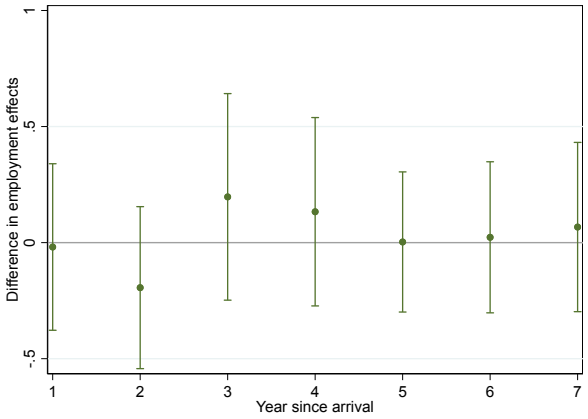
(c) Effects on share of co-national coworkers by immigration class



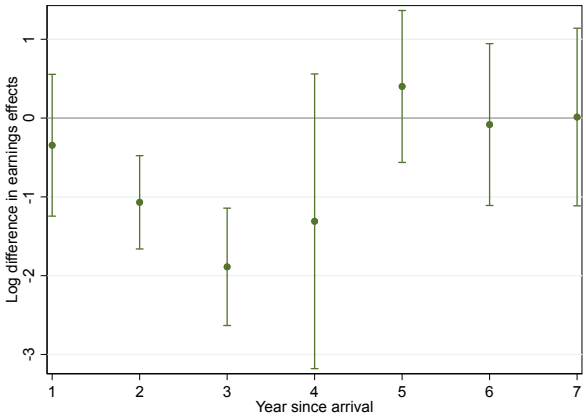
(d) IUR shock × economic immigrant: share of co-national coworkers

Notes: The figure plots heterogeneous effects of initial unemployment rate (IUR) shocks on firm outcomes by immigration class. Panels (a) and (c) report the estimated effects on firm wage premiums and share of co-national immigrant workers separately for economic, family-sponsored, and refugee immigrants. Panels (b) and (d) report the interaction coefficient between the IUR shock and an indicator for economic immigrants, which tests whether the effects differ between economic and non-economic immigrants. Vertical bars denote 95% confidence intervals. Estimates are obtained from the flexible shock-based specification. The sample consists of male immigrants who landed between 2000 and 2012.

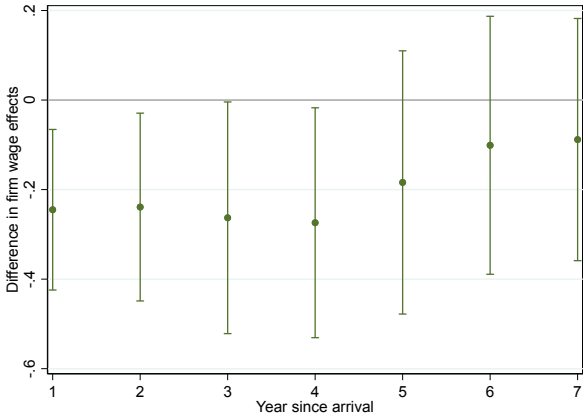
Figure 10: Heterogeneous effects of initial unemployment rate shocks on firm outcomes of family-sponsored immigrants by enclave size



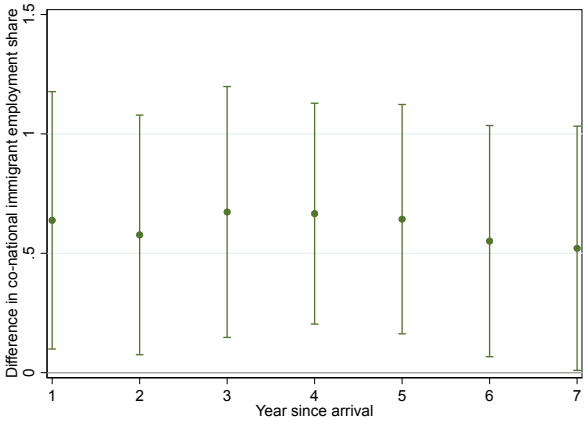
(a) IUR shock × enclave size: employment



(b) IUR shock × enclave size: log wage earnings



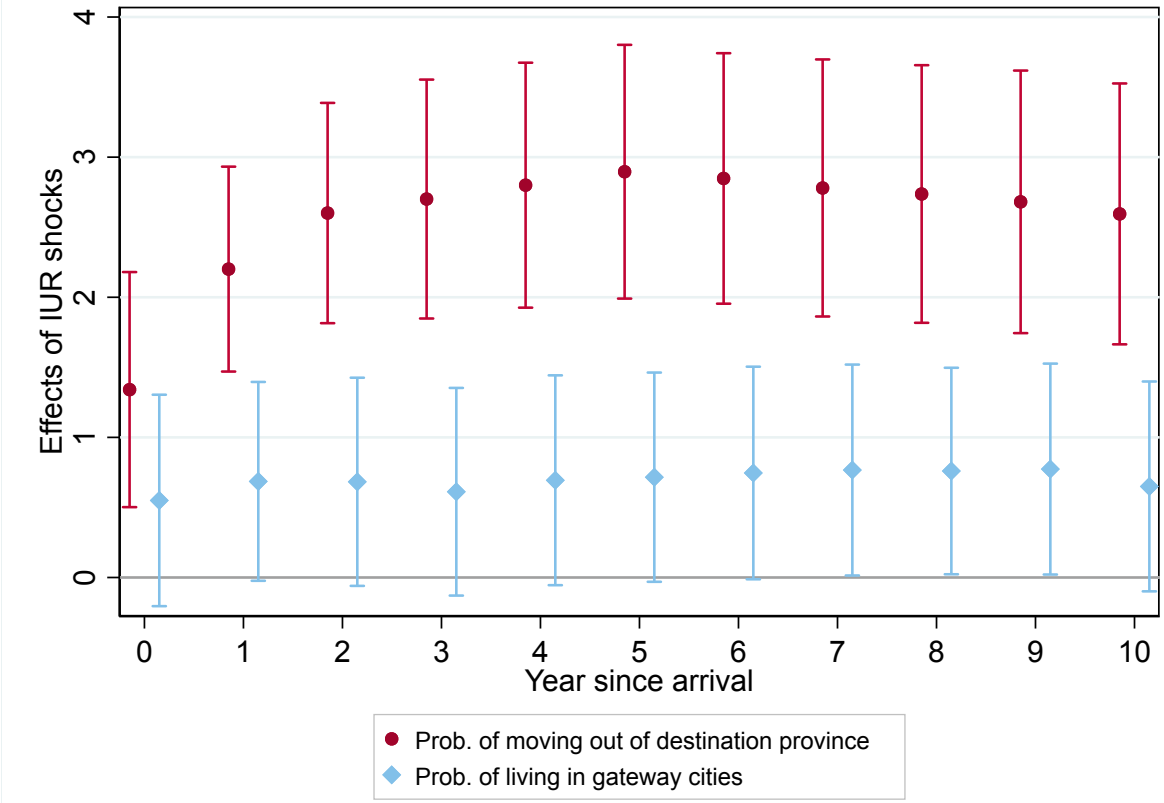
(c) IUR shock × enclave size: firm wage premiums



(d) IUR shock × enclave size: share of co-national coworkers

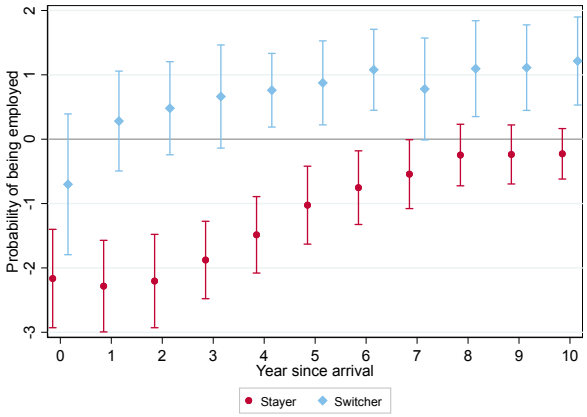
Notes: The figure plots heterogeneous effects of initial unemployment rate (IUR) shocks on firm outcomes for family-sponsored immigrants by enclave size. Panels (a), (b), (c), and (d) report the estimated coefficients on the interaction between the IUR shock and log enclave size for employment, log annual earnings, firm wage premiums, and the share of co-national immigrant workers in the firm, respectively. Vertical bars indicate 95% confidence intervals. Estimates are obtained from the flexible shock-based specification. The sample consists of male immigrants who landed between 2000 and 2012.

Figure 11: Dynamic effects of initial unemployment rate shocks on internal migration

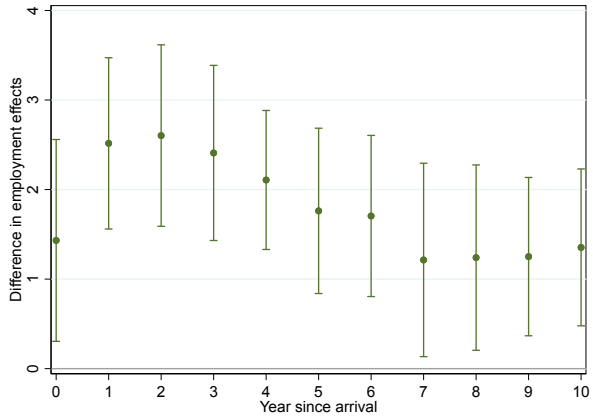


Notes: The figure plots the estimated dynamic effects of initial unemployment rate (IUR) shocks on the probability of moving out of the destination province (red) and the probability of living in a gateway city (blue) during the first ten years after arrival. Circles report coefficient estimates and vertical bars denote 95% confidence intervals from the preferred flexible shock-based specification. The sample consists of male immigrants who landed between 1982 and 2012.

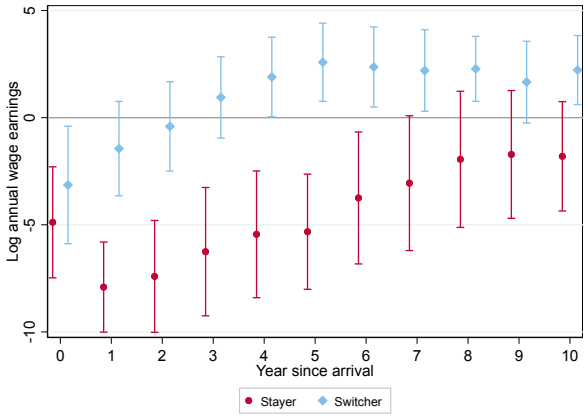
Figure 12: Heterogeneous effects of initial unemployment rate shocks by subsequent interprovincial mobility



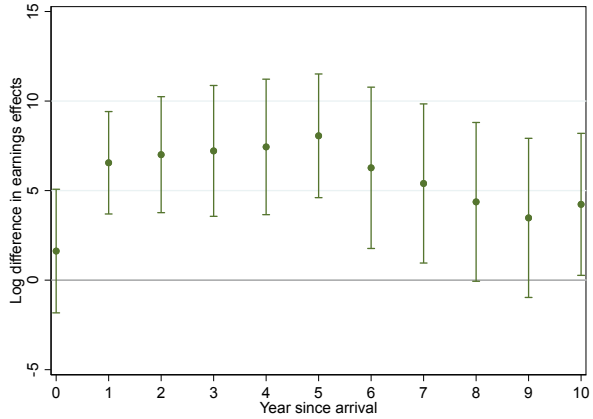
(a) Employment effects by mobility status



(b) IUR shock × mover: employment



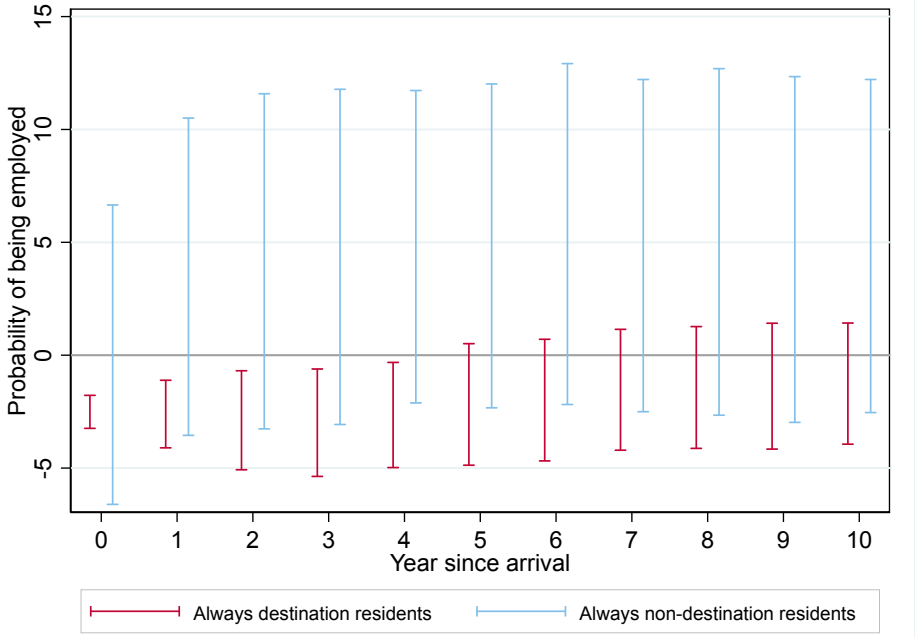
(c) earnings effects by mobility status



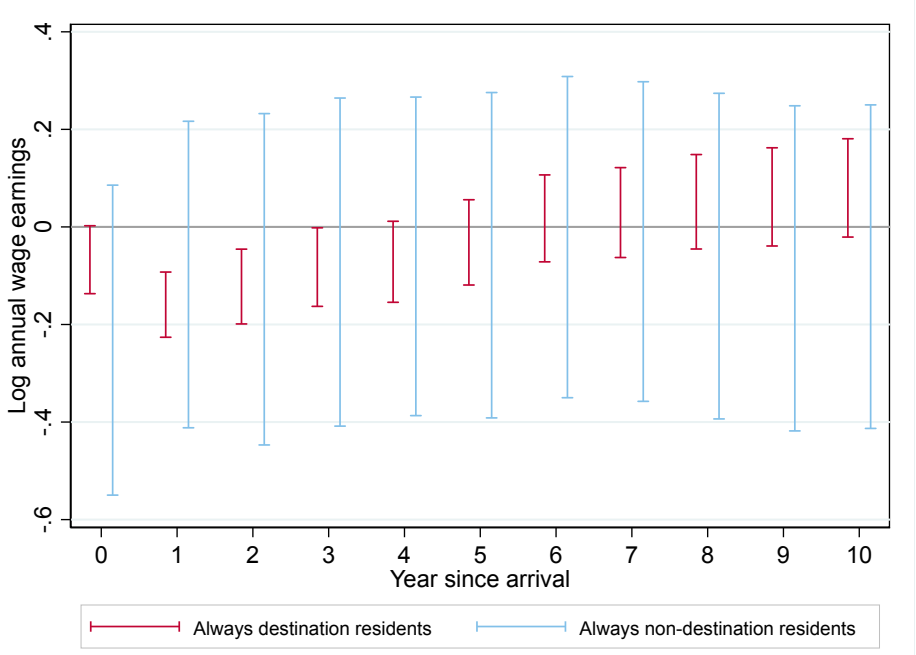
(d) IUR shock × mover: earnings

Notes: The figure plots heterogeneous effects of initial unemployment rate (IUR) shocks by post-arrival interprovincial mobility. Panels (a) and (c) report the estimated employment and earnings effects separately for immigrants who remain in their destination province throughout the first ten years after arrival and those who move to another province during that period. Panels (b) and (d) report the coefficient on the interaction between the IUR shock and an indicator for movers, which tests whether the effects differ between movers and stayers. Vertical bars denote 95% confidence intervals. The sample consists of male immigrants who landed between 1982 and 2012.

Figure 13: Lee bounds for the employment and earnings effects of initial unemployment rate shocks on always destination-residents and always non-destination residents



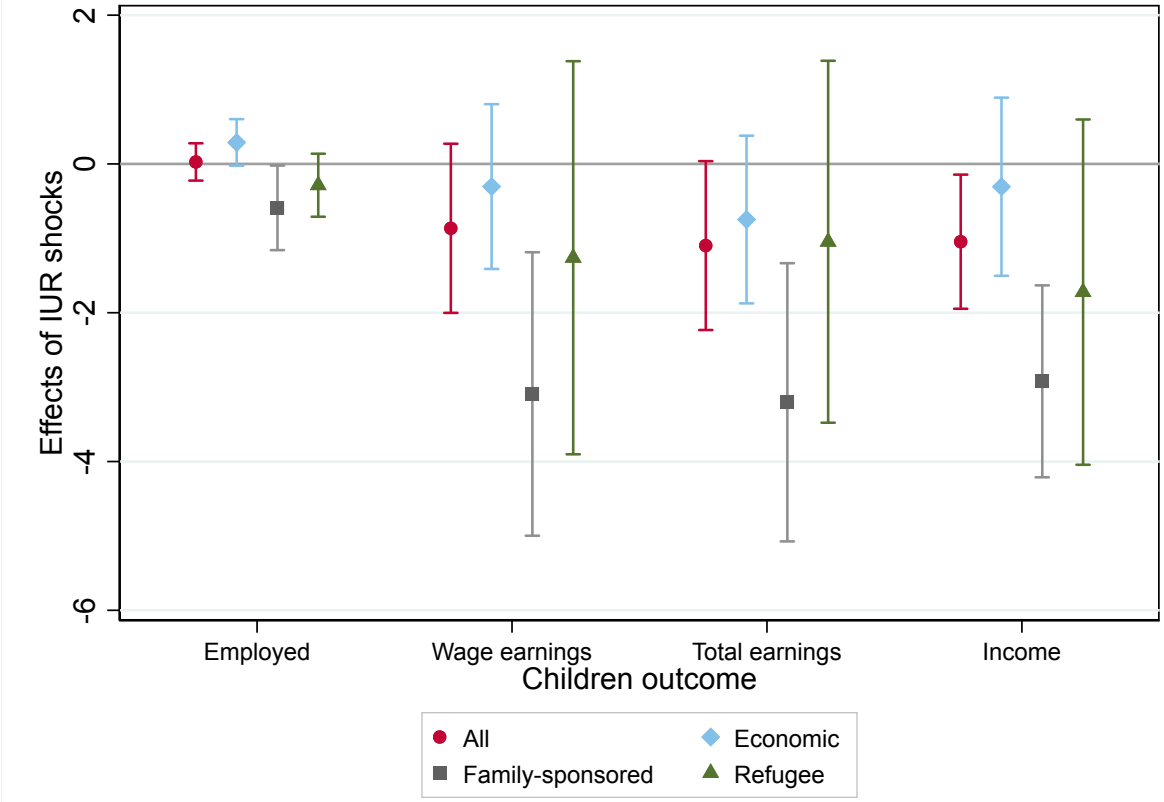
(a) Lee bounds on employment effects



(b) Lee bounds on earnings effects

Note: The figure reports Lee bounds on the dynamic effects of initial unemployment rate (IUR) shocks on employment and earnings. The red and blue series correspond to always destination residents and always non-destination residents, respectively. Vertical bars indicate the lower and upper Lee bounds.

Figure 14: Effects of fathers' initial unemployment rate shocks on adult children's labor market outcomes



Notes: The figure reports the estimated effects of fathers' initial unemployment rate (IUR) shocks on adult children's labor market outcomes measured between ages 30 and 34. Outcomes include employment, annual wage earnings, total annual earnings, and total annual income. Estimates are reported separately for all immigrants, economic immigrants, family-sponsored immigrants, and refugees. Vertical bars denote 95% confidence intervals. The sample consists of the 1.5-generation children of male immigrants who landed between 1982 and 2012.

Table 1: Detailed sample selection procedure

Sample restriction	Observations	Share retained (%)
All landed immigrants	10,163,000	100.00
Excluding immigrants with any temporary permits	7,095,000	69.81
Male immigrants only	3,407,000	48.02
Landed between 1982 to 2012	2,417,000	70.94
Aged 18–54 at landing	1,458,000	60.34
Excluding Quebec-selected immigrants, provincial nominees, entrepreneurs, and investors	1,134,000	77.76

Notes: The table reports the sample selection procedure. The share retained is calculated relative to the preceding selection step. Observation counts are rounded.

Table 2: Distribution of immigrants by admission category

Immigration category	Share (%)
Panel A: Economic Immigrants	**56.20**
Federal skilled workers	54.79
Of which: Arranged employment offers	*0.09*
Federal self-employed	1.41
Panel B: Family-Sponsored Immigrants	**29.93**
Sponsored spouses and partners	17.65
Sponsored parents and grandparents	9.51
Sponsored children	2.36
Other family-sponsored categories	0.41
Panel C: Refugees	**13.88**
Government-assisted refugees (GAR)	7.30
Privately sponsored refugees (PSR)	5.94
Protected persons and dependents	0.64

Notes: Shares are percentages of the full sample. Other family-sponsored categories include intercountry adopted children and the HC and NIE family sponsorship categories. Protected persons and dependents include protected persons and their dependents abroad. The indented row is a subset of federal skilled workers and is not counted separately in the total.

Table 3: Summary statistics: landing characteristics and selected post-arrival outcomes

Variable	All immigrants	By Immigration Class		
		Economic	Family-sponsored	Refugee
	(1)	(2)	(3)	(4)
Panel A: Education				
None	2.7%	1.7%	4.4%	3.2%
Secondary or less	31.7%	13.8%	54.8%	54.1%
Some postsecondary	16.5%	15.7%	14.5%	23.8%
Bachelor's degree	34.2%	45.4%	21.6%	16.4%
Master's degree	13.0%	20.3%	4.2%	2.0%
Doctorate	1.9%	3.1%	0.4%	0.4%
Panel B: World Area of Birth				
Europe	18.2%	18.1%	10.8%	34.4%
Africa and Middle East	16.4%	16.7%	9.3%	30.5%
Southern Asia	22.9%	23.7%	30.8%	2.5%
Eastern Asia	19.1%	27.0%	12.7%	1.1%
Oceania and other Asia	13.6%	9.0%	17.3%	24.0%
South and Central America	8.4%	4.6%	16.0%	7.3%
North America	1.4%	0.9%	3.2%	0.1%
Panel C: Intended Destination Province				
Newfoundland and Labrador	0.2%	0.1%	0.2%	0.7%
Prince Edward Island	0.1%	0.0%	0.1%	0.3%
Nova Scotia	0.8%	0.7%	0.6%	1.4%
New Brunswick	0.4%	0.2%	0.3%	1.0%
Quebec	6.7%	7.1%	5.8%	7.2%
Ontario	60.8%	63.8%	58.0%	54.6%
Manitoba	2.7%	1.4%	2.9%	8.0%
Saskatchewan	1.1%	0.5%	0.8%	4.0%
Alberta	9.2%	7.9%	10.0%	13.0%
British Columbia	17.9%	18.2%	21.4%	9.8%
Panel D: Other Characteristics at Landing				
Age at landing (years)	33.0	35.2	30.2	30.2
	(8.7)	(7.6)	(9.5)	(8.2)
Married	65.4%	72.7%	58.8%	50.0%
English speaking	62.4%	74.0%	56.4%	28.4%
French speaking	2.7%	3.1%	2.5%	1.9%
Reports intended occupation	61.3%	82.5%	25.6%	52.2%
Intended occupation: NOC 0/A/B	86.0%	94.8%	57.6%	59.9%
Intended occupation: NOC C/D	14.0%	5.2%	42.4%	40.1%
Panel E: Post-landing Tax Filing and Mobility				
Ever files a tax return	91.4%	88.2%	94.5%	97.5%
Stops filing within 10 years	19.0%	24.5%	13.3%	8.9%
Switches province within 10 years	17.9%	19.4%	11.0%	27.0%
Panel F: Labor Market Outcomes Within 10 Years				
Employed	67.1%	62.7%	74.0%	69.7%
Annual T4 earnings (2016 CAD)	23,700	25,800	21,900	19,100
	(34,600)	(40,000)	(27,100)	(23,600)

Notes: Entries are percentages unless otherwise indicated. Standard deviations are reported in parentheses below means where available. Annual T4 earnings are rounded to the nearest $100.

Table 4: The effects of initial labor market conditions on immigrant inflows

	National		Across Province		Across Province by Immigration Class		
					Economic	Family-sponsored	Refugee
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Real IUR	-2,073.0***		-286.0**				
	(447.7)		(122.2)				
IUR shock		-1,431.0		106.4	162.5	19.6	1.5
		(878.3)		(165.0)	(188.2)	(28.7)	(58.0)
IUR prediction		-2,124.0***		-450.7***	-109.3	-25.0	-70.9*
		(447.1)		(140.1)	(100.2)	(26.6)	(41.3)
Landing year FE	No	No	Yes	Yes	Yes	Yes	Yes
Destination province FE	No	No	Yes	Yes	Yes	Yes	Yes
Observations	30	30	300	300	290	300	310

Notes: This table reports the effects of initial labor market conditions on the number of new immigrants. Columns (1)–(2) use national landing year cohorts as the unit of observation. Columns (3)–(4) use destination-province-by-landing-year cohorts and include destination province and landing year fixed effects. Columns (5)–(7) further disaggregate destination-province-by-landing-year cohorts by three main immigration classes (economic, family-sponsored, and refugee). The top and bottom 1% of provincial IUR shocks are excluded in columns (3)–(7), with trimming performed separately for each sample, resulting in different observation counts across columns (5)–(7). Observation counts are rounded to the nearest 10. The sample consists of male immigrant landing cohorts from 1982 to 2012. Standard errors, clustered at landing year level, are in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix A Prediction of provincial unemployment rates

A.1. Data

To predict provincial annual unemployment rates from 1982 to 2012, I collect provincial monthly unemployment rates along with multiple standard macroeconomic indices (see Table A.1) from 1976 to 2012. All variables are seasonally adjusted.

Using monthly data to predict annual unemployment rates provides two key advantages. First, it increases the number of observations, particularly for the initial years of forecasting, which enhances model training. Second, monthly data contains richer information. For example, a declining trend in monthly unemployment rates throughout 1983 can provide a more reliable signal for forecasting the unemployment rate in 1984 than annual data alone.

Table A.1: List of macroeconomic predictors from the Federal Reserve Bank of St. Louis and the Canadian Labour Force Survey

Variable	Transformation	Unit
Industrial production index	Monthly change	%
Exchange rate: USD/CAD		%
CPI: Canada	Growth rate same period previous year	%
CPI: US	Growth rate same period previous year	%
10-year government bond yields: Canada		%
10-year government bond yields: US		%
M3: Canada	Monthly change	%
M3: US	Monthly change	%
M1: Canada	Monthly change	%
M1: US	Monthly change	%
NASDAQ composite index		%
Real GDP: Canada	Quarterly growth rate same period previous year	%
Real GDP: US	Quarterly growth rate same period previous year	%
3-month treasury bill minus federal fund rate		%
Unemployment rate: US		%
WTI oil price	Monthly change	%
Exports: Canada	Monthly change	%
Imports: Canada	Monthly change	%
Provincial labor force participation rate: Canada		%

A.2. Forecasting approach

The goal is to make the best prediction for the unemployment rate next year. The prediction accuracy improves with more training data. Therefore, the forecasting procedure employs a non-rolling expanding window approach. The model is first trained on monthly data from January 1976 through December 1981 and used to forecast unemployment rates in 1982. For each subsequent forecast year through 2012, the model is re-estimated using an expanding training window that always begins in January 1976 and ends in December of the preceding

year.

Before estimating the forecasting model, I apply principal component analysis (PCA) to the macroeconomic predictors and their first 12 monthly lags[36] and retain the first 15 principal components. This procedure summarizes the predictive variation in the high-dimensional macroeconomic data while reducing the risk of overfitting. The final predictor set comprises these 15 factors, the contemporaneous unemployment rate, and its first 24 monthly lags.

Besides a naive proximation using last year's unemployment rates, I evaluate six forecasting models: Mixed Data Sampling Regression, ARIMAX, Linear Regression, Fixed-Effects Panel Regression, Markov-Switching Models, and Neural Networks.

Mixed Data Sampling Regression (MIDAS): The MIDAS regression framework allows the dependent and explanatory variables in a time-series regression to be sampled at different frequencies (see Ghysels et al. 2007). I use this framework to predict annual unemployment rates using monthly predictors. The baseline MIDAS regression takes the form:

$$Y_{st} = \beta_0 + \beta_1 \sum_{k=12}^{36} B(k;\theta) L^{k/m} X_{st}^{(m)} + \epsilon_{st}^{(m)}, \tag{1}$$

where s and t denote province and year, respectively. The dependent variable Y_{st} is observed at the annual frequency, whereas the regressors $X_{st}^{(m)}$ are observed at the monthly frequency $m = 12$. $L^{k/m}$ is a lag operator such that $L^{k/m} X_{st}^{(m)} = X_{s,t-k/m}^{(m)}$. I use information from the most recent 24 months (lags 12 to 35) to forecast the unemployment rate for the following calendar year in each province.

To avoid estimating a large number of unrestricted lag coefficients, I impose parametric structure on the MIDAS lag weights $B(k;\theta)$. Specifically, I use a two-parameter exponential Almon lag polynomial to model the MIDAS weights. Because the training sample used for the 1982 forecast contains only four annual observations, I include only historical unemployment rates on the right-hand side to mitigate underfitting.

In contrast to the MIDAS regression, the remaining models produce monthly forecasts by generating predictions at horizons from one to twelve months ahead.

ARIMAX: The ARIMAX model extends the traditional ARIMA model by incorporating exogenous predictors. I apply the model using the following specification:

$$Y_{st} = c + \sum_{i=1}^{p} \phi_i Y_{s,t-i} + \sum_{j=1}^{q} \theta_j \epsilon_{s,t-j} + \beta \mathbf{X}_{s,t-12} + \epsilon_{st}. \tag{2}$$

[36]For each macroeconomic predictor, I include a separate lag for each of the preceding 12 months, substantially increasing the dimension of the predictor set.

where t indexes months. To ensure consistency with the multi-step forecasting horizon, $\mathbf{X}_{st-12}$ is constructed using predictors lagged by 12 months. This design ensures to only use macroeconomic conditions in year t to inform forecasts for all months in year t by making 12-steps ahead predictions. To reduce dimensionality and prevent overfitting, I employ Lasso regression to select relevant predictors. The ARIMAX parameters including $(p,\ q)$ are determined automatically using the `auto.arima()` function from the R package `forecast`.

Linear Regression (LM): To forecast month k in the prediction year, I estimate the following linear regression separately for each province:

$$Y_{st} = \beta_{sk}\mathbf{X}_{s,t-k} + \epsilon_{st}. \tag{3}$$

As in the ARIMAX model, Lasso is applied to select the most informative predictors.

Fixed-Effects Panel Regression (FE): Rather than estimating separate models for each province, I implement a panel regression with fixed effects for each k:

$$Y_{st} = \beta_{k}\mathbf{X}_{s,t-k} + \alpha_s + \epsilon_{st}. \tag{4}$$

This specification accounts for unobserved province-specific heterogeneity (α_s), while predictor coefficients β_k no longer differ across provinces. As before, Lasso is used to select the most informative predictors.

Markov-switching (MS): As noted in McGrane (2022), unemployment rates exhibit asymmetric dynamics, rising sharply during recessions but declining more gradually in expansions. To account for these nonlinearities, I apply a Markov-Switching Regression Model, following Goldfeld and Quandt (1973) and Hamilton (1989). Let c_t represent the economic state in month t, which can be either a recession or an expansion. Let $c_{st} \in \{1, 2\}$ denote the latent economic state of province s in period t, where $c_{st} = 1$ indicates an expansion and $c_{st} = 2$ indicates a recession. Conditional on $c_{st} = i$, the model is

$$r_{st} = r_{is} + \epsilon_{st}, \tag{5}$$

where r_{st} is the residual from the fixed-effects panel regression and r_{is} is the state-specific mean for province s. The latent state follows a first-order Markov process with province-specific transition matrix

$$P_s = \begin{bmatrix} p_{1s} & 1 - p_{1s} \\ 1 - p_{2s} & p_{2s} \end{bmatrix}$$

The (i, j)th element of P_s gives the probability of transitioning from state i in period $t-1$ to state j in period t. Thus, p_{1s} is the probability that province s remains in an expansion, while p_{2s} is the probability that it remains in a recession. The model parameters and regime probabilities are estimated separately for each province using the R package `MSwM`. The estimated regime probabilities are then used to classify each period as either an expansion or a recession.

Neural Networks (NN): There is a growing body of literature employing machine learning techniques to predict unemployment rates (see e.g. Smalter Hall and Cook 2017; Kreiner and Duca 2020; Gogas et al. 2022; Orozco-Castañeda et al. 2024). I implement a Feedforward Neural Network (FNN) using the R package `h2o`, leveraging its ability to capture nonlinear dynamics in time series data. In general, the network consists of an input layer, one or more hidden layers, and an output layers. Each hidden layer is composed of neurons that apply nonlinear activation functions to transform the input data. For each k-month-ahead forecast, I first optimize the network architecture—tuning the number of hidden layers, neurons per layer, and activation functions—using a validation split within the entire training sample.[37] For instance, a network with one hidden layer, containing 101 neurons and employing the Maxout activation function, yields the lowest out-of-sample Root Mean Squared Error (RMSE) in the one-month-ahead forecast horizon [38]. I then apply the tuned hyperparameters to generate year-by-year predictions.

I henceforth generate seven time series of monthly provincial unemployment rate predictions. Simply averaging these monthly predictions to obtain annual estimates would be misleading, as the annual unemployment rate might be not merely the mean of twelve seasonally adjusted monthly unemployment rates. Instead, I estimate empirical weights by regressing historical annual unemployment rates on monthly unemployment rates. The estimated coefficients from this regression serve as weights, allowing me to construct annual unemployment rate predictions as weighted sums of monthly forecasts. This approach ensures that the aggregation process reflects the historical relationship between monthly and annual unemployment rates rather than relying on an arbitrary assumption of equal weighting.

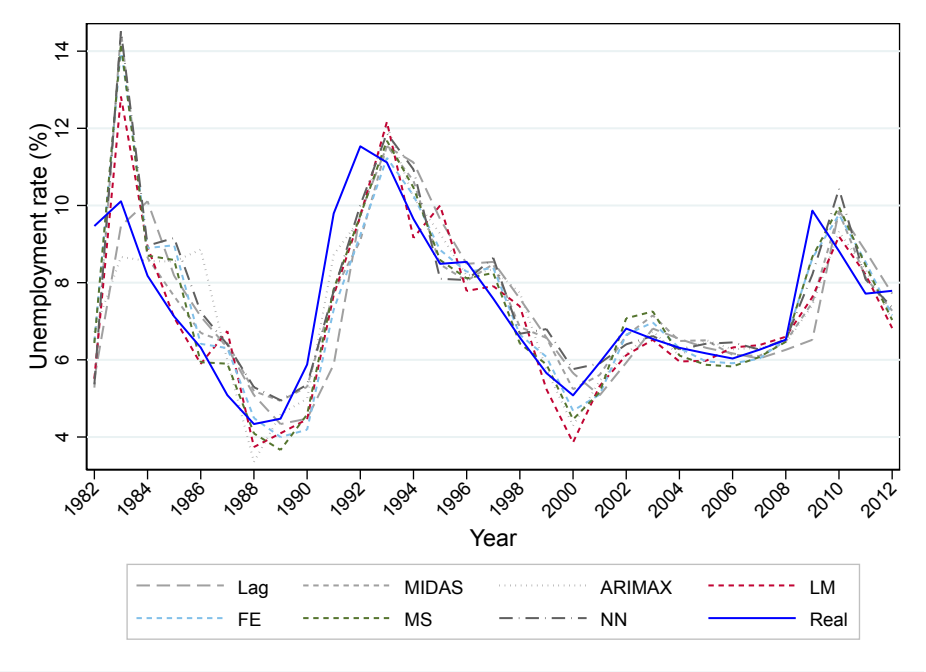
[37] For hyperparameter tuning, I use 80% of the training sample to estimate each candidate model and the remaining 20% to evaluate its out-of-sample RMSE.

[38] In a similar study that predicts the U.S. unemployment rates using covariates by Kreiner and Duca (2020), the best configuration for a four-quarter-ahead forecast uses three hidden layers and 97 neurons per layer.

A.3. Results

Figure A.1 presents predicted unemployment rates for Ontario.

Figure A.1: Monthly predicted unemployment rates in Ontario



The forecasting performance is evaluated using RMSE, with model performance compared against a naive benchmark, the last year's unemployment rates. Table A.2 presents the RMSE ratios across different models over the benchmark. ARIMAX has the worst forecasting performance among the seven models, perhaps because it makes all one to twelve months-ahead predictions in one ARIMAX process. MIDAS has the second worst performance, maybe because the prediction does not include any covariates. Surprisingly, NN does not improve the forecasting accuracy compared to the naive prediction. In contrast, simplier models such as LM, FE, and MS make better forecasts, where the MS that adds asymmetry to the FE model is the best. The average of the seven predictions (Pool) also performs well, achieving an RMSE 10% lower than that of the benchmark.

Table A.2: Forecasting performance

Model	RMSE Ratio
Lag	1.00
MIDAS	1.17
ARIMAX	1.45
LM	0.94
FE	0.90
MS	0.89
NN	1.00
Pool	0.90

Appendix B Lee bounds for always stayers and always switchers

B.1. A 2×2 DID framework

I first develop Lee-type bounds for always stayers and always switchers in a 2×2 difference-in-differences (DID) setting. An always stayer would reside in the intended destination province under either treatment state, whereas an always switcher would reside outside that province under either treatment state. I use the term *switcher*, rather than *mover*, because an immigrant observed outside the intended province need not ever have resided in that province.

B.1.1 Setup

Consider destination provinces p and p' and landing cohorts l and l'. Let $G = (L, P)$ index an immigrant's landing-cohort–destination-province cell. In the canonical 2×2 design, only cell $g_1 = (l, p)$ is treated: $D_{g_1} = 1$, while $D_g = 0$ for $g \in \{(l, p'), (l', p), (l', p')\}$. Empirically, treatment indicates a high adverse IUR shock.

For each year since arrival $e \in \{0, \ldots, 10\}$, let

$$S_e(d) \in \{ST, SW\}, \qquad d \in \{0, 1\},$$

denote the potential location status under treatment state d: ST means residing in the intended destination province and SW means residing elsewhere. Define the response type

$$T_e \equiv \Big(S_e(0), S_e(1)\Big).$$

The four possible response types are (ST, ST) (always stayers), (SW, SW) (always switchers), (ST, SW) (treatment-induced switchers), and (SW, ST) (treatment-induced stayers). Write

$$A_{SW}(e) = \{T_e = (SW, SW)\}, \qquad A_{ST}(e) = \{T_e = (ST, ST)\}.$$

Finally, let $Y_e(d, s)$ be the potential labor market outcome under treatment state d and location status s. This notation allows treatment to affect the outcome directly and through location.

B.1.2 Target parameters

The target parameter depends on whether the outcome is employment or earnings. Let $Y_e^{\text{emp}}(d,s)$ denote potential employment status and let $Y_e^{\text{earn}}(d,s)$ denote potential annual employment earnings, including zero earnings for non-employed immigrants. For $q \in \{SW, ST\}$, define

$$A_q(e) = \{T_e = (q,q)\}.$$

For employment, the conditional target parameter is the additive treatment effect

$$\tau_e^{q,\text{emp}}(x) \equiv E\left[Y_e^{\text{emp}}(1,q) - Y_e^{\text{emp}}(0,q) \mid G = g_1, X = x, A_q(e)\right].$$

Thus, $\tau_e^{SW,\text{emp}}(x)$ and $\tau_e^{ST,\text{emp}}(x)$ measure, respectively, the effects of an adverse IUR shock on the employment probabilities of always switchers and always stayers e years after arrival, conditional on pre-treatment covariates $X = x$. These effects are measured in percentage points.

For earnings, the conditional target parameter is instead the log ratio of potential mean earnings:

$$\theta_e^{q,\text{earn}}(x) \equiv \log\left[\frac{E\left[Y_e^{\text{earn}}(1,q) \mid G = g_1, X = x, A_q(e)\right]}{E\left[Y_e^{\text{earn}}(0,q) \mid G = g_1, X = x, A_q(e)\right]}\right],$$

provided that the denominator is positive. Therefore, $\theta_e^{SW,\text{earn}}(x)$ and $\theta_e^{ST,\text{earn}}(x)$ measure proportional treatment effects on the mean annual employment earnings of always switchers and always stayers. The exact percentage effect is

$$100\left[\exp\left(\theta_e^{q,\text{earn}}(x)\right) - 1\right].$$

The employment and earnings parameters are defined differently because the two outcomes are estimated under different common trends restrictions. Employment is a binary outcome observed for every immigrant, and I impose additive principal common trends. Annual employment earnings are nonnegative and equal to zero for non-employed immigrants. I therefore impose multiplicative principal common trends that include zero earnings. Including zero earnings avoids conditioning on positive earnings, which would create an additional post-treatment selection problem because the IUR shock itself affects employment. Accordingly, the earnings estimand captures changes along both the extensive employment and intensive earnings margins and should not be interpreted as an earnings effect conditional on employment.

Let $F_e^q(x)$ denote the covariate distribution among members of principal stratum $(A_q(e))$ in the treated cell. Following Semenova (2025), the aggregate employment effect is the average of the conditional level effects:

$$\tau_e^{q,\text{emp}} = \int \tau_e^{q,\text{emp}}(x) dF_e^q(x).$$

For earnings, the conditional potential mean levels must instead be aggregated before taking their ratio. Define

$$M_{d,e}^{q,\text{earn}} = \int E\left[Y_e^{\text{earn}}(d,q) \mid G = g_1, X = x, A_q(e)\right] dF_e^q(x), \qquad d \in 0, 1.$$

The aggregate proportional earnings effect[39] is then

$$\theta_e^{q,\text{earn}} = \log\left(\frac{M_{1,e}^{q,\text{earn}}}{M_{0,e}^{q,\text{earn}}}\right).$$

B.1.3 Identifying assumptions for always switchers

The analysis of always switchers relies on monotonicity, a restriction on changes in principal-stratum shares, and a restriction on untreated potential outcomes. All assumptions are imposed separately for each e and, unless noted otherwise, conditional on $X = x$.

Assumption 1 (monotonicity).

$$S_e(1) = ST \implies S_e(0) = ST,$$

or, equivalently, $S_e(0) = SW \implies S_e(1) = SW$. Thus, an adverse IUR shock may induce switching but cannot induce an immigrant who would otherwise switch to stay. This rules out the (SW, ST) type. Consequently, observed switchers in an untreated cell are always switchers, whereas observed switchers in the treated cell are a mixture of always switchers and treatment-induced switchers.

[39] In general, this aggregate parameter is not equal to a weighted average of the conditional log mean ratios $\theta_e^{q,\text{earn}}(x)$.

Assumption 2 (multiplicative common sorting trends, M-CST). Let $\pi^A_{e,g}(x) = P[A_{SW}(e) \mid G = g, X = x]$. Then

$$\frac{\pi^A_{e,(l,p)}(x)}{\pi^A_{e,(l,p')}(x)} = \frac{\pi^A_{e,(l',p)}(x)}{\pi^A_{e,(l',p')}(x)}.$$

Thus, the proportional difference in the always-switcher share between destination provinces p and p' is stable across landing cohorts l and l'. Equivalently, the cohort change in that share is proportionally the same in the two provinces.

Under monotonicity, $\pi^A_{e,g}(x) = P[S_e = SW \mid G = g, X = x]$ in every untreated cell. Hence M-CST identifies the always-switcher share in the treated cell as

$$\pi^A_{e,(l,p)}(x) = P(S_e = SW \mid l, p', x)\frac{P(S_e = SW \mid l', p, x)}{P(S_e = SW \mid l', p', x)},$$

provided the denominator is positive.

Assumption 3A (additive principal common trends). For outcomes analyzed in levels, such as employment,

$$\begin{aligned} &E[Y_e(0, SW) \mid l, p, x, A_{SW}(e)] - E[Y_e(0, SW) \mid l, p', x, A_{SW}(e)] \\ &\quad = E[Y_e(0, SW) \mid l', p, x, A_{SW}(e)] - E[Y_e(0, SW) \mid l', p', x, A_{SW}(e)]. \end{aligned}$$

This assumption states that, among always switchers, the untreated cohort difference is common across destination provinces (or, equivalently, the untreated province difference is common across cohorts).

Assumption 3M (multiplicative principal common trends). For nonnegative outcomes analyzed multiplicatively, such as earnings including zeros,

$$\frac{E[Y_e(0, SW) \mid l, p, x, A_{SW}(e)]}{E[Y_e(0, SW) \mid l, p', x, A_{SW}(e)]} = \frac{E[Y_e(0, SW) \mid l', p, x, A_{SW}(e)]}{E[Y_e(0, SW) \mid l', p', x, A_{SW}(e)]},$$

whenever the denominators are positive. This is a common-trends restriction on mean earnings in levels, not on mean log earnings. Including zero earnings avoids conditioning the earnings analysis on post-treatment employment.

B.1.4 Point identification of response type proportions

Let $\pi^{SW}_{e,g}(x) = P(S_e = SW \mid G = g, X = x)$ denote the observed switching rate. Based on Assumption 1 and 2, the share of always switchers among observed switchers in the treated cell is

$$\begin{aligned}\gamma^{SW}_e(x) &\equiv P[A_{SW}(e) \mid G = (l,p), X = x, S_e = SW] \\ &= \frac{\pi^{A}_{e,(l,p)}(x)}{\pi^{SW}_{e,(l,p)}(x)} \\ &= \frac{\pi^{SW}_{e,(l,p')}(x)}{\pi^{SW}_{e,(l,p)}(x)} \frac{\pi^{SW}_{e,(l',p)}(x)}{\pi^{SW}_{e,(l',p')}(x)}.\end{aligned}$$

B.1.5 Bounding the always-switchers treatment effects

The form of the bounds depends on whether the outcome is employment or annual employment earnings. Let $o \in \{\text{emp}, \text{earn}\}$ index the outcome and define

$$m^{SW,o}_{e,g}(x) \equiv E[Y^o_e \mid G = g, X = x, S_e = SW]$$

for an untreated cell g. Under monotonicity, observed switchers in an untreated cell are always switchers, so this observed mean equals the corresponding mean for the $A_{SW}(e)$ principal stratum.

For employment, Assumption 3A identifies the untreated potential mean for always switchers in the treated cell as

$$\begin{aligned}m^{0,SW,\text{emp}}_{e,(l,p)}(x) &\equiv E[Y^{\text{emp}}_e(0, SW) \mid G = (l,p), X = x, A_{SW}(e)] \\ &= m^{SW,\text{emp}}_{e,(l,p')}(x) + m^{SW,\text{emp}}_{e,(l',p)}(x) - m^{SW,\text{emp}}_{e,(l',p')}(x).\end{aligned}$$

For earnings, Assumption 3M instead identifies the untreated potential mean multiplicatively:

$$\begin{aligned}m^{0,SW,\text{earn}}_{e,(l,p)}(x) &\equiv E[Y^{\text{earn}}_e(0, SW) \mid G = (l,p), X = x, A_{SW}(e)] \\ &= m^{SW,\text{earn}}_{e,(l,p')}(x) \frac{m^{SW,\text{earn}}_{e,(l',p)}(x)}{m^{SW,\text{earn}}_{e,(l',p')}(x)},\end{aligned}$$

provided the denominator is positive. Thus, Assumption 3A recovers the counterfactual employment rate through an additive DID comparison, whereas Assumption 3M recovers counterfactual mean earnings through a ratio-in-ratios comparison.

The treated potential mean for always switchers is not point identified because observed switchers in the treated cell combine always switchers, (SW, SW), and treatment-induced switchers, (ST, SW). Following Lee (2009), I bound the treated mean by placing always switchers at the two extremes of the observed outcome distribution among treated switchers. For outcome o, let $F_{e,x}^{1,SW,o}$ denote this distribution and define the lower- and upper-tail means of probability mass γ as

$$M_L(F,\gamma) = \frac{1}{\gamma}\int_0^{\gamma} F^{-1}(u)\,du, \qquad M_U(F,\gamma) = \frac{1}{\gamma}\int_{1-\gamma}^{1} F^{-1}(u)\,du.$$

This integral definition accommodates discrete outcomes and mass points by allowing fractional trimming at the cutoff. The *sharp* bounds on the treated potential mean are

$$\begin{aligned}
\underline{m}_e^{1,SW,o}(x) &= M_L\left(F_{e,x}^{1,SW,o}, \gamma_e^{SW}(x)\right), \\
\overline{m}_e^{1,SW,o}(x) &= M_U\left(F_{e,x}^{1,SW,o}, \gamma_e^{SW}(x)\right).
\end{aligned}$$

Because the employment effect is defined in levels, its conditional Lee bounds are

$$\begin{aligned}
\underline{\tau}_e^{SW,\text{emp}}(x) &= \underline{m}_e^{1,SW,\text{emp}}(x) - m_{e,(l,p)}^{0,SW,\text{emp}}(x), \\
\overline{\tau}_e^{SW,\text{emp}}(x) &= \overline{m}_e^{1,SW,\text{emp}}(x) - m_{e,(l,p)}^{0,SW,\text{emp}}(x).
\end{aligned}$$

By contrast, the earnings effect is defined as a log ratio of potential mean earnings. Its conditional bounds are therefore

$$\begin{aligned}
\underline{\theta}_e^{SW,\text{earn}}(x) &= \log\left(\frac{\underline{m}_e^{1,SW,\text{earn}}(x)}{m_{e,(l,p)}^{0,SW,\text{earn}}(x)}\right), \\
\overline{\theta}_e^{SW,\text{earn}}(x) &= \log\left(\frac{\overline{m}_e^{1,SW,\text{earn}}(x)}{m_{e,(l,p)}^{0,SW,\text{earn}}(x)}\right).
\end{aligned}$$

To aggregate across covariates, the relevant weights are the covariate distribution among always switchers in the treated cell. In particular,

$$\omega_e^{SW}(x) = \frac{P(X = x \mid G = (l,p))\pi_{e,(l,p)}^{A}(x)}{\sum_{x'} P(X = x' \mid G = (l,p))\pi_{e,(l,p)}^{A}(x')}.$$

For discrete X, the aggregate employment bounds are obtained by averaging the conditional

level-effect bounds:

$$\left[\sum_x \omega_e^{SW}(x)\underline{\tau}_e^{SW,\text{emp}}(x),\ \sum_x \omega_e^{SW}(x)\overline{\tau}_e^{SW,\text{emp}}(x)\right].$$

For the aggregate earnings effect, the bounded treated and point-identified counterfactual means must first be aggregated in levels:

$$\begin{aligned}
\underline{M}_{1e}^{SW,\text{earn}} &= \sum_x \omega_e^{SW}(x)\underline{m}_e^{1,SW,\text{earn}}(x),\\
\overline{M}_{1e}^{SW,\text{earn}} &= \sum_x \omega_e^{SW}(x)\overline{m}_e^{1,SW,\text{earn}}(x),\\
M_{0e}^{SW,\text{earn}} &= \sum_x \omega_e^{SW}(x)m_{e,(l,p)}^{0,SW,\text{earn}}(x).
\end{aligned}$$

The aggregate bounds on the log earnings ratio are then

$$\left[\log\left(\frac{\underline{M}_{1e}^{SW,\text{earn}}}{M_{0e}^{SW,\text{earn}}}\right),\ \log\left(\frac{\overline{M}_{1e}^{SW,\text{earn}}}{M_{0e}^{SW,\text{earn}}}\right)\right].$$

The framework is related to Rathnayake et al. (2026), who derive bounds for always-observed units in DID designs. My setting differs in that no pre-treatment labor market outcome is observed because treatment is assigned at landing, so untreated landing-cohort–province cells provide the counterfactual comparison. The analysis is also related to Kroft et al. (2026), who bound effects for workers who would remain at low- or high-AKM-premium firms under either treatment state. Three differences are worth noting. First, in their application the two response types occupy different baseline firm types, whereas here always stayers and always switchers may report the same intended destination at application, so the strata are not distinguished by any baseline characteristic. Second, they trim the distribution of earnings *changes* around a pre-treatment base period; because treatment is assigned at landing in my setting, no pre-treatment outcome exists and the trimming is applied to outcome levels, which are considerably more dispersed and therefore yield wider intervals at a comparable trimming share. Third, their framework accommodates multilayered selection over $\{\text{unemployment}, L, H\}$ but is implemented under an assumed full-employment restriction, with a sample requiring strictly positive earnings and a linked AKM firm effect throughout the window. In my setting non-employment is common and is itself affected by the landing shock, so assuming it away is not available. I instead define the earnings outcome to include zeros, which keeps the outcome defined for the whole population and places the extensive

margin inside the estimand rather than outside the sample.

B.1.6 Bounding the always-stayers treatment effects

For the always-stayer analysis I reverse the labeling of the binary treatment: an immigrant is now assigned $\widetilde{D} = 1$ if the landing-year IUR shock in the intended province is negative and $\widetilde{D} = 0$ if it is positive. In the 2×2 design, cell (l, p) is the negative-shock cell and the other three cells are positive-shock cells. The reason for the relabeling is mechanical: under Assumption 1 an adverse shock can only push immigrants out of the intended province, so among *stayers* it is the negative-shock cells that are contaminated. Making those cells the focal ones restores the structure of the always-switcher problem, in which the object requiring trimming sits in a single cell.

This relabeling introduces no new behavioral restriction. Writing $\widetilde{S}_e(\widetilde{d}) = S_e(1 - \widetilde{d})$, the response type in the reversed labeling is

$$\widetilde{T}_e = \Big(\widetilde{S}_e(0), \widetilde{S}_e(1)\Big) = \Big(S_e(1), S_e(0)\Big),$$

so the two components of the type simply exchange places. The excluded type (SW, ST) of Assumption 1 therefore maps to (ST, SW) under the reversed labeling, and the monotonicity condition

$$\widetilde{S}_e(0) = ST \implies \widetilde{S}_e(1) = ST$$

is *the same restriction as Assumption 1*, not an additional one: both state that a favorable shock cannot push an immigrant out of the intended province, equivalently that an adverse shock cannot pull one in. A single monotonicity assumption thus supports both principal strata. Under this condition, stayers in the three positive-shock cells are always stayers, (ST, ST), whereas stayers in the negative-shock cell are a mixture of always stayers and shock-induced stayers, (SW, ST).

Under the reversed labeling, the bounds identify the effect of a negative IUR shock. Reporting them alongside the always-switcher estimates requires only a change of sign, and this is an algebraic identity rather than an assumption. Because the binary treatment is defined by the sign of the shock, both analyses contrast the same two cell types—negative-shock and positive-shock cells—and differ only in which is labeled treated. For the employment parameter, defined in levels,

$$\widetilde{\tau}_e^{ST,\text{emp}} = E[Y_e^{\text{emp}}(\text{neg})] - E[Y_e^{\text{emp}}(\text{pos})] = -\tau_e^{ST,\text{emp}},$$

and for the earnings parameter, defined as a log ratio of means, $\log(M_{\text{neg}}/M_{\text{pos}}) = -\log(M_{\text{pos}}/M_{\text{neg}})$. Negation is order-reversing, so if $[L_e^{ST,-}, U_e^{ST,-}]$ bounds the effect of the negative-shock contrast, then $[-U_e^{ST,-}, -L_e^{ST,-}]$ bounds the effect of the adverse-shock contrast.

Two caveats follow from the relabeling. First, the always-stayer parameter is an effect of treatment on the untreated in the adverse-shock metric, since the focal cell is one that did not experience an adverse shock, whereas the always-switcher parameter is an effect on the treated. Second, the two parameters are defined for different principal strata and different focal cells, so they are not components of a decomposition of a common aggregate effect and should not be summed.

B.2. Empirical implementation

The conceptual framework above considers a single 2×2 design. In the empirical application, however, treatment varies across many landing cohorts and destination provinces. I therefore generalize the additive and multiplicative common-trends restrictions using landing-cohort and destination-province fixed effects and combine the resulting comparisons through regression estimands.

The IUR shock is continuous in the main analysis. For the bounding exercise, I define a binary adverse-shock treatment using the sign of the shock. The resulting estimands compare outcomes above and below the zero-shock threshold. They should not be interpreted as marginal effects of a one-percentage-point increase in the IUR shock.

I describe the implementation for always switchers. The always-stayer analysis follows the same steps after reversing the treatment indicator, as discussed above, and the resulting bounds are reoriented to report the adverse-shock contrast.

B.2.1 Two routes to a multi-cell estimand

Moving from a single 2×2 design to many landing-cohort–destination-province cells raises a question that does not arise in the canonical Lee (2009) setting, where a single treated group is compared with a single control group. With many cells, the analyst must choose how to combine the cell-specific comparisons into one number, and the choice interacts with the trimming in a way that matters for whether sharp bounds can be computed at all.

There are two natural routes. The first is to let a regression do the aggregation: specify a cell-level model with landing-cohort and destination-province fixed effects, and report the coefficient on treatment. This is what applied work in this design ordinarily does, and it is attractive because the fixed effects impose the multi-cell analogue of common trends

automatically. The second route is to construct the aggregate directly: point identify the counterfactual level in each treated cell, bound the treated level by trimming, and combine the two using explicit population weights.

The two routes differ in a way that becomes decisive once trimming is introduced. Trimming does not deliver a single value for the treated cell means; it delivers an interval for each treated cell, and the bounds on the aggregate are the extrema of the aggregation map over the resulting rectangle. Whether those extrema can be located depends on how the map behaves.

When the estimand is *linear* in the cell means with weights that do not depend on them, the extrema are attained at a vertex of the rectangle, chosen coordinate by coordinate according to the sign of each cell's weight. This is the case for the weighted least squares coefficient used for employment, because the post-trimming weights depend on the trimming *fraction* but not on which tail is retained. Proposition **??** formalizes this and gives the resulting sign rule. Notably, the rule accommodates design weights of either sign, so the well-known possibility of negative weights in two-way fixed effects designs does not threaten validity here; it affects only the interpretation of the estimand, as discussed in Remark **??**.

When the estimand is *nonlinear*, the argument fails. Assumption 3M suggests a log-link specification for earnings, but a PPML treatment coefficient is a nonlinear function of all cell outcomes, and its implicit weights change when the trimmed means change. A tail assignment computed under one configuration need not remain optimal at the configuration it induces, so the vertex obtained by applying a sign rule is not guaranteed to be the global extremum. Rather than searching over configurations, I take the second route for earnings: I use the log-link model only to recover the point-identified counterfactual levels, and I construct the aggregate ratio explicitly with positive population weights. The resulting map is strictly increasing in each treated cell mean, so its extrema lie at the two extreme corners of the rectangle and Proposition **??** requires no condition beyond Assumptions 1–3M. A related direct-aggregation strategy is used by Kroft et al. (2026). They estimate treatment effects separately for each treated group using not-yet-treated groups as comparisons and then aggregate the resulting group-specific effects.

The remainder of this section implements these two routes. Section B.2.3 estimates the trimming shares, Section B.2.4 characterizes the identified set that both routes take as their input, and Sections B.2.5 and B.2.6 derive the bounds for employment and earnings respectively.

B.2.2 Trimming by covariates

Following Lee (2009) and Semenova (2025), I condition on pre-treatment covariates before trimming. This conditioning can tighten the bounds by restricting the possible allocation of principal strata to immigrants with similar predicted outcomes. The covariates include country of origin, education, age at landing, marital status, intended occupation, and the predicted IUR rate. A fully saturated partition would require interacting more than 100 origin countries with several discrete characteristics and a continuous predicted IUR rate, thereby creating many sparsely populated cells.

To reduce dimensionality, I estimate an outcome-prediction model among switchers in untreated cells and apply it to every immigrant in the corresponding analysis sample. I then divide the sample into ten groups defined by deciles of the predicted outcome. The prediction model and bin assignment are constructed separately for each outcome and year since arrival. Response-type shares and outcome bounds are subsequently estimated within landing-cohort–destination-province–prediction-bin cells, and the empirical analogues of Assumptions 1–3 are imposed conditional on the prediction bin. This approach uses the covariates to rank immigrants by their expected outcomes while avoiding the instability of a fully saturated covariate partition.

B.2.3 Estimating response-type proportions

Let $c = (l, p)$ index a landing-cohort–destination-province cell, let b index the prediction bin, and let D_c indicate an adverse IUR shock. For each year since arrival e and prediction bin b, I estimate the observed switching probability using the PPML conditional-mean specification

$$E\left[\mathbb{1}\{S_{ie} = SW\} \mid c, b\right] = \exp\left(\delta_{eb}^{SW} D_c + \alpha_{leb}^{SW} + \lambda_{peb}^{SW}\right).$$

The landing-cohort and destination-province fixed effects impose a multiplicatively separable counterfactual switching rate. This specification is the multi-cell analogue of M-CST, but it is also an additional functional-form restriction: PPML restricts the treatment effect on the switching rate to be homogeneous on the log scale across treated cells within (e, b).

Let N_{ebc} denote the total number of immigrants in cell (e, b, c) and let r_{ebc} denote their observed switching rate. The number of observed switchers is therefore

$$n_{ebc}^{SW} = N_{ebc} r_{ebc}.$$

The fitted switching rate is

$$\widehat{\mu}_{ebc} = \exp\left(\widehat{\delta}_{eb}^{SW} D_c + \widehat{\alpha}_{leb}^{SW} + \widehat{\lambda}_{peb}^{SW}\right).$$

For a treated cell, setting $D_c = 0$ gives the estimated counterfactual switching rate

$$\widehat{\pi}_{ebc}^{0,SW} = \exp\left(\widehat{\alpha}_{leb}^{SW} + \widehat{\lambda}_{peb}^{SW}\right).$$

At the population values implied by the model,

$$\frac{\pi_{ebc}^{0,SW}}{\mu_{ebc}} = \exp\left(-\delta_{eb}^{SW}\right) \qquad \text{for every treated cell } c.$$

It therefore implies the common trimming share

$$\widehat{\gamma}_{eb}^{SW} = \exp\left(-\widehat{\delta}_{eb}^{SW}\right).$$

The PPML first-order condition for δ_{eb}^{SW} also yields

$$\exp\left(\widehat{\delta}_{eb}^{SW}\right) = \frac{\sum_{c:D_c=1} N_{ebc} r_{ebc}}{\sum_{c:D_c=1} N_{ebc} \widehat{\pi}_{ebc}^{0,SW}},$$

and hence

$$\widehat{\gamma}_{eb}^{SW} = \frac{\sum_{c:D_c=1} N_{ebc} \widehat{\pi}_{ebc}^{0,SW}}{\sum_{c:D_c=1} N_{ebc} r_{ebc}}.$$

Equivalently, over treated cells with $r_{ebc} > 0$,

$$\widehat{\gamma}_{eb}^{SW} = \sum_{c:D_c=1,\, r_{ebc}>0} \left(\frac{N_{ebc} r_{ebc}}{\sum_{j:D_j=1} N_{ebj} r_{ebj}}\right) \frac{\widehat{\pi}_{ebc}^{0,SW}}{r_{ebc}}.$$

Thus, the model-implied common trimming share can also be written as an average of the cell-specific ratios of counterfactual to observed switching rates, weighted by the number of observed switchers. Monotonicity requires $\delta_{eb}^{SW} \geq 0$ and therefore $0 < \gamma_{eb}^{SW} \leq 1$ in the population. An estimate outside this range would be inconsistent with the maintained monotonicity restriction and could not be interpreted as a valid trimming share.

B.2.4 Constructing the Lee bounds

For each treated cell c in prediction bin b, let $\underline{m}_{ebc}^{1}$ and $\overline{m}_{ebc}^{1}$ denote the lower- and upper-tail trimmed means obtained by retaining a fraction $\widehat{\gamma}_{eb}^{SW}$ of the observed treated-switcher outcome

distribution. The lower bound retains individuals with the lowest outcomes, whereas the upper bound retains those with the highest outcomes, allowing fractional trimming at mass points.

Under the maintained assumptions, the mean treated outcome among always switchers satisfies

$$m^1_{ebc} \in \left[\underline{m}^1_{ebc}, \overline{m}^1_{ebc}\right].$$

These are the sharp Lee bounds on the cell-specific mean (Lee, 2009). Untreated cells are not trimmed because monotonicity implies that observed switchers in these cells are always switchers.

To obtain bounds on the treatment effects in the multi-cell setting, I combine these cell-specific trimmed means using the two estimation approaches described in Section B.2.1. The following subsections explain how the lower and upper endpoints are constructed for employment and earnings.

B.2.5 Employment bounds: the regression route

Employment is analyzed in levels under Assumption 3A. I use weighted least squares (WLS) to combine the cell-specific trimmed employment rates while controlling for landing-cohort and destination-province fixed effects.

Given $\widehat{\gamma}^{SW}_{eb}$, the estimated number of always switchers represented by each cell is

$$\widehat{n}^A_{ebc} = n^{SW}_{ebc}\left[(1 - D_c) + D_c\widehat{\gamma}^{SW}_{eb}\right].$$

All observed switchers are retained in untreated cells, whereas only the estimated always-switcher fraction is retained in treated cells.

Let $\overline{Y}^{SW}_{ebc}$ denote the observed employment rate among switchers. In untreated cells, the mean employment rate among always switchers equals $\overline{Y}^{SW}_{ebc}$. In treated cells, it is bounded by the lower- and upper-tail trimmed employment rates, denoted by $\underline{m}^{1,\text{emp}}_{ebc}$ and $\overline{m}^{1,\text{emp}}_{ebc}$.

For any admissible set of always-switcher cell means, $\overline{Y}^A_{ebc}$, I consider the cell-level regression

$$\overline{Y}^A_{ebc} = \beta^{\text{emp}}_{eb} D_c + \alpha^{\text{emp}}_{leb} + \lambda^{\text{emp}}_{peb} + \varepsilon_{ebc},$$

estimated by WLS with $\widehat{n}^A_{ebc}$ as the cell weight.

Let $\widetilde{D}_{ebc}$ denote the residual from projecting D_c on the landing-cohort and destination-province fixed effects using these same weights. By the Frisch–Waugh–Lovell theorem, the

treatment coefficient can be expressed as

$$\widehat{\beta}^{\text{emp}}_{eb} = \frac{\sum_c \widehat{n}^A_{ebc} \widetilde{D}_{ebc} \overline{Y}^A_{ebc}}{\sum_c \widehat{n}^A_{ebc} \widetilde{D}^2_{ebc}}.$$

Importantly, the residualized treatment indicator $\widetilde{D}_{ebc}$ can be negative even in treated cells. Consequently, assigning the lower-tail trimmed mean to every treated cell does not necessarily minimize the treatment coefficient. Instead, the assignment depends on the sign of $\widetilde{D}_{ebc}$.

For the lower bound, I assign

$$\overline{Y}^{A,L}_{ebc} = \begin{cases} \overline{Y}^{SW}_{ebc}, & D_c = 0, \\ \underline{m}^{1,\text{emp}}_{ebc}, & D_c = 1,\ \widetilde{D}_{ebc} \geq 0, \\ \overline{m}^{1,\text{emp}}_{ebc}, & D_c = 1,\ \widetilde{D}_{ebc} < 0. \end{cases}$$

For the upper bound, I reverse the assignments:

$$\overline{Y}^{A,U}_{ebc} = \begin{cases} \overline{Y}^{SW}_{ebc}, & D_c = 0, \\ \overline{m}^{1,\text{emp}}_{ebc}, & D_c = 1,\ \widetilde{D}_{ebc} \geq 0, \\ \underline{m}^{1,\text{emp}}_{ebc}, & D_c = 1,\ \widetilde{D}_{ebc} < 0. \end{cases}$$

Substituting these values into the WLS coefficient formula gives the estimated lower and upper endpoints:

$$\underline{\widehat{\beta}}^{\text{emp}}_{eb} = \frac{\sum_c \widehat{n}^A_{ebc} \widetilde{D}_{ebc} \overline{Y}^{A,L}_{ebc}}{\sum_c \widehat{n}^A_{ebc} \widetilde{D}^2_{ebc}}, \qquad \overline{\widehat{\beta}}^{\text{emp}}_{eb} = \frac{\sum_c \widehat{n}^A_{ebc} \widetilde{D}_{ebc} \overline{Y}^{A,U}_{ebc}}{\sum_c \widehat{n}^A_{ebc} \widetilde{D}^2_{ebc}}.$$

These assignments minimize and maximize the WLS coefficient over the admissible cell-specific Lee intervals. The post-trimming weights, and therefore $\widetilde{D}_{ebc}$, remain unchanged between the two exercises because they depend only on the trimming fraction, not on which tail is retained. Under the maintained additive outcome restriction, the resulting endpoints are used to estimate the bounds on the employment effect among always switchers.

B.2.6 Earnings bounds: the direct route

For earnings, I use the direct aggregation approach described in Section B.2.1. Rather than bounding a PPML treatment coefficient, I estimate counterfactual untreated earnings and

combine them with the bounded treated earnings to construct the proportional treatment effect.

Let

$$m_{ebc}^{d,\text{earn}} = E\left[Y_e^{\text{earn}}(d, SW) \mid A_{SW}(e), c, b\right], \qquad d \in \{0, 1\}.$$

Here, $m_{ebc}^{1,\text{earn}}$ is the treated mean earnings among always switchers, whereas $m_{ebc}^{0,\text{earn}}$ is their counterfactual untreated mean.

Observed switchers in untreated cells are always switchers under monotonicity, so their observed mean earnings identify $m_{ebc}^{0,\text{earn}}$ in these cells. Let $\overline{Y}_{ebc}^{\text{earn}}$ denote mean earnings among observed switchers. Under Assumption 3M, I estimate the multiplicatively separable conditional-mean model

$$E\left[\overline{Y}_{ebc}^{\text{earn}} \mid D_c = 0\right] = \exp\left(\alpha_{leb}^{0,\text{earn}} + \lambda_{peb}^{0,\text{earn}}\right),$$

using PPML on untreated cells with n_{ebc}^{SW} as frequency weights.

The predicted counterfactual earnings for a treated cell are

$$\widehat{m}_{ebc}^{0,\text{earn}} = \exp\left(\widehat{\alpha}_{leb}^{0,\text{earn}} + \widehat{\lambda}_{peb}^{0,\text{earn}}\right), \qquad D_c = 1.$$

For each treated cell, trimming the observed earnings distribution by $\widehat{\gamma}_{eb}^{SW}$ yields

$$m_{ebc}^{1,\text{earn}} \in \left[\underline{\widehat{m}}_{ebc}^{1,\text{earn}}, \overline{\widehat{m}}_{ebc}^{1,\text{earn}}\right].$$

The lower and upper endpoints retain the bottom and top $\widehat{\gamma}_{eb}^{SW}$ fractions of the observed earnings distribution, respectively.

I aggregate these trimmed means across treated cells using the estimated number of always switchers,

$$\widehat{n}_{ebc}^{A} = n_{ebc}^{SW}\widehat{\gamma}_{eb}^{SW}.$$

The bin-specific proportional earnings effect is defined as

$$\theta_{eb}^{\text{earn}} = \log\left(\frac{\sum_{c:D_c=1} n_{ebc}^{A} m_{ebc}^{1,\text{earn}}}{\sum_{c:D_c=1} n_{ebc}^{A} m_{ebc}^{0,\text{earn}}}\right),$$

where the common normalizing factor for the number of always switchers cancels from the ratio.

Because the counterfactual earnings are estimated from untreated cells, they remain fixed across the lower- and upper-bound calculations. Moreover, the aggregation weights are

positive, so the aggregate treated earnings are increasing in every treated cell mean. The lower and upper endpoints can therefore be estimated directly by aggregating the corresponding trimmed means:

$$\underline{\widehat{\theta}}_{eb}^{\text{earn}} = \log\left(\frac{\sum_{c:D_c=1} \widehat{n}_{ebc}^{A} \underline{\widehat{m}}_{ebc}^{1,\text{earn}}}{\sum_{c:D_c=1} \widehat{n}_{ebc}^{A} \widehat{m}_{ebc}^{0,\text{earn}}}\right),$$

$$\overline{\widehat{\theta}}_{eb}^{\text{earn}} = \log\left(\frac{\sum_{c:D_c=1} \widehat{n}_{ebc}^{A} \overline{\widehat{m}}_{ebc}^{1,\text{earn}}}{\sum_{c:D_c=1} \widehat{n}_{ebc}^{A} \widehat{m}_{ebc}^{0,\text{earn}}}\right).$$

Unlike the employment regression approach, the direct aggregation approach uses positive population weights, so the lower and upper endpoints are obtained by consistently selecting the lower- and upper-tail trimmed earnings means across all treated cells.

B.2.7 Aggregation across prediction bins

Finally, I aggregate the bin-specific bounds according to the scale of each outcome. For employment, which is analyzed in levels, I average the lower and upper endpoints separately using the estimated number of always switchers in each bin,

$$\widehat{N}_{eb}^{A} = \sum_{c:D_c=1} \widehat{n}_{ebc}^{A},$$

as weights:

$$\underline{\widehat{\beta}}_{e}^{\text{emp}} = \frac{\sum_b \widehat{N}_{eb}^{A} \underline{\widehat{\beta}}_{eb}^{\text{emp}}}{\sum_b \widehat{N}_{eb}^{A}}, \qquad \overline{\widehat{\beta}}_{e}^{\text{emp}} = \frac{\sum_b \widehat{N}_{eb}^{A} \overline{\widehat{\beta}}_{eb}^{\text{emp}}}{\sum_b \widehat{N}_{eb}^{A}}.$$

For earnings, averaging bin-specific log ratios would generally produce a different parameter. I therefore aggregate the bounded treated earnings and the predicted counterfactual earnings across bins before calculating their log ratio:

$$\underline{\widehat{\theta}}_{e}^{\text{earn}} = \log\left(\frac{\sum_b \sum_{c:D_c=1} \widehat{n}_{ebc}^{A} \underline{\widehat{m}}_{ebc}^{1,\text{earn}}}{\sum_b \sum_{c:D_c=1} \widehat{n}_{ebc}^{A} \widehat{m}_{ebc}^{0,\text{earn}}}\right),$$

$$\overline{\widehat{\theta}}_{e}^{\text{earn}} = \log\left(\frac{\sum_b \sum_{c:D_c=1} \widehat{n}_{ebc}^{A} \overline{\widehat{m}}_{ebc}^{1,\text{earn}}}{\sum_b \sum_{c:D_c=1} \widehat{n}_{ebc}^{A} \widehat{m}_{ebc}^{0,\text{earn}}}\right).$$

The resulting endpoints provide estimates of the bounds on the employment and proportional earnings effects among always switchers, accounting for uncertainty about which observed treated switchers would also have switched in the absence of treatment.

B.3. Results

B.3.1 Trimming rates

Table A.3 reports the estimated trimming shares, $\widehat{\gamma}_e^{ST}$ and $\widehat{\gamma}_e^{SW}$, by prediction bins. Approximately 80% of immigrants reside in their intended destination province during their first 10 years after arrival. Consistent with this high staying rate, $\widehat{\gamma}_e^{ST}$ remains close to one. By contrast, treatment-induced switchers constitute a considerably larger share of observed treated switchers.[40] Trimming requirements also vary considerably across prediction bins. Bins with higher predicted earnings contain a larger proportion of treatment-induced switchers and therefore require more extensive trimming.[41]

Table A.3: Estimated trimming shares for stayers and switchers

	Years since arrival, e		
Prediction bin	1	5	10
Panel A: always stayers			
Bin 1	1.0157***	0.9840***	0.9847***
Bin 5	0.9441***	0.9183***	0.8994***
Bin 10	0.9547***	0.9364***	0.9343***
Panel B: always switchers			
Bin 1	0.9620*	0.8702***	0.8624***
Bin 5	0.7649***	0.7535***	0.7528***
Bin 10	0.8179***	0.8001***	0.8445***

Notes: The table reports estimated trimming shares at 1, 5, and 10 years since arrival for prediction bins 1, 5, 10. The trimming share is the estimated proportion of always stayers or always switchers among observed treated stayers or switchers, respectively.

B.3.2 Bounds

The full-sample estimates show that arriving during a recession initially reduces both employment and earnings, with the effects gradually fading toward zero. Estimates based on realized location status suggest that these adverse effects are concentrated among stayers, while switchers appear largely unaffected. Because realized location is itself affected by initial

[40] Note that treatment is defined as a negative IUR shock for stayers but as a positive IUR shock for switchers.

[41] The trimming share is assumed to be no greater than one. A small number of estimated trimming shares exceed one, primarily in the earliest years since arrival and among immigrants in the lowest predicted-outcome bins. These estimates are very close to one and are capped at one in practice.

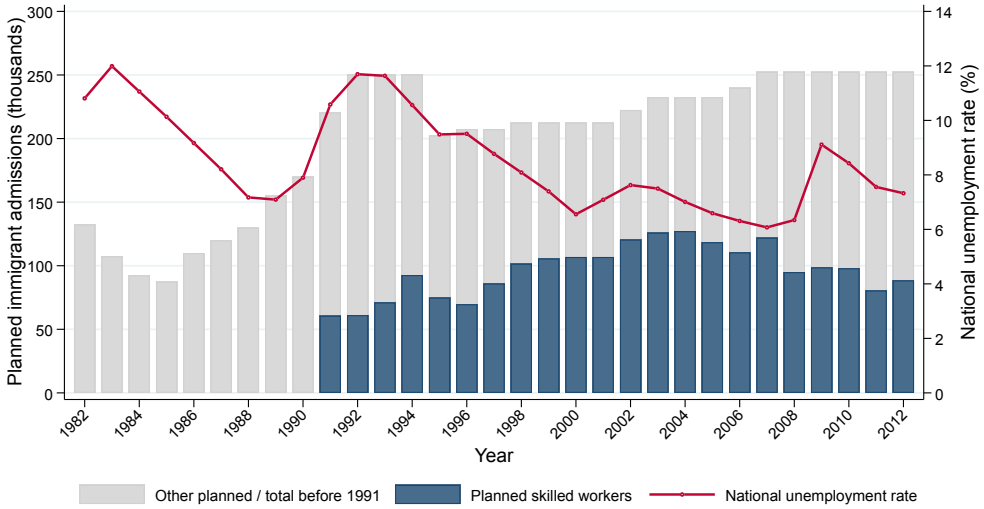
labor-market conditions, however, this comparison does not necessarily represent genuine treatment-effect heterogeneity between always stayers and always switchers.

The estimated trimming share for stayers, $\widehat{\gamma}_e^{ST}$, is close to one, so constructing the always-stayer bounds requires removing relatively few observations from the treated cells. Consequently, the bounds remain informative: even after allowing treatment-induced stayers to occupy either tail of the treated outcome distribution, the upper bounds on the employment and earnings effects for always stayers remain below zero in the first few years after arrival. By contrast, the lower values of $\widehat{\gamma}_e^{SW}$ require substantially more trimming among treated switchers. The resulting bounds for always switchers are considerably wider, include zero, and overlap with the always-stayer bounds.

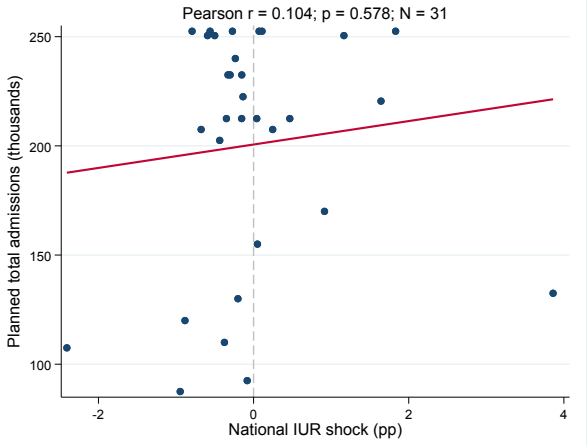
These results show that the treatment-induced change in cross-provincial mobility substantially limits what can be learned about the outcomes of always switchers. In particular, the apparent difference between realized stayers and switchers need not reflect genuine treatment-effect heterogeneity across the two principal strata. For example, if treatment-induced switchers are disproportionately drawn from the upper tail of the treated-switcher outcome distribution, their relatively favorable outcomes could mask negative effects among always switchers. Because the always-switcher bounds allow for this possibility and overlap with the always-stayer bounds, I cannot reject the hypothesis that the treatment effects are the same for the two principal strata.

Appendix C Appendix Figures

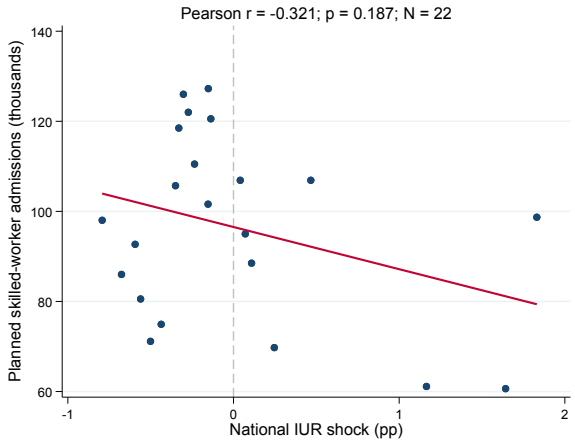
Figure A.2: Planned immigration levels and national labor market conditions



(a) Planned immigration levels and national unemployment rate



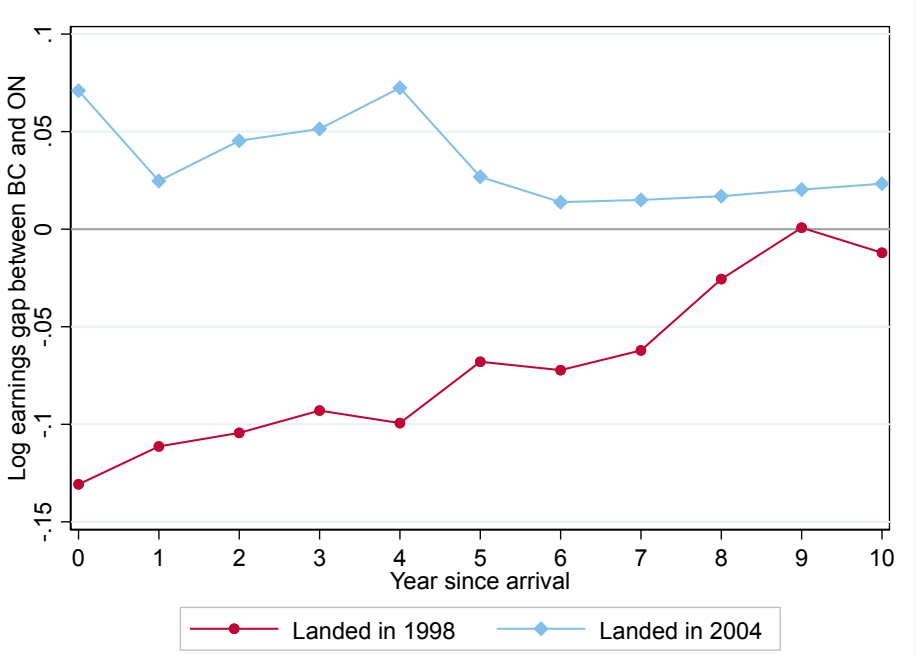
(b) National IUR shocks and planned total immigration



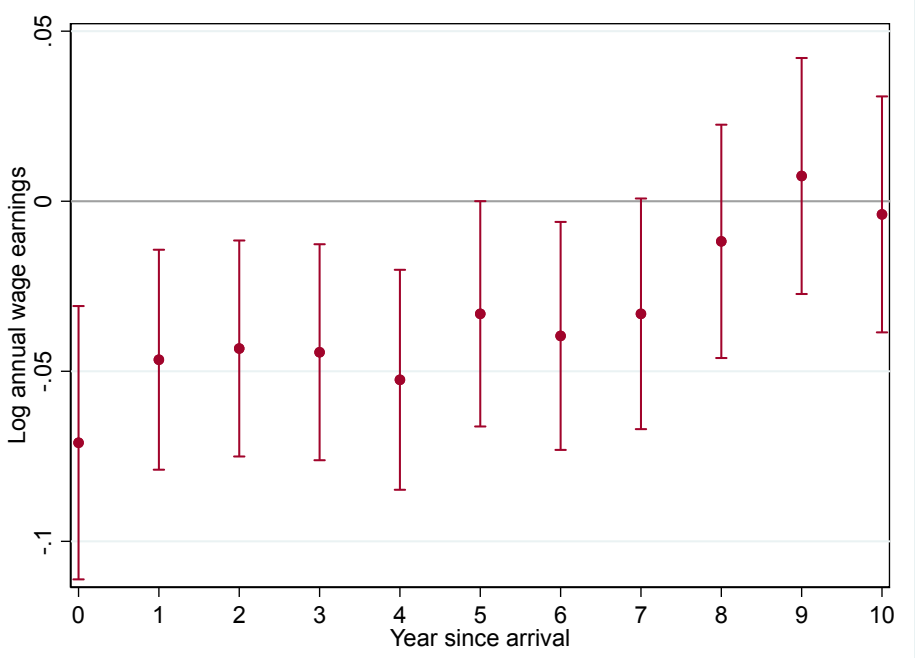
(c) National IUR shocks and planned skilled-worker immigration

Note: This figure presents planned immigration levels and national labor market conditions in Canada. Panel (a) plots the midpoint of planned total immigration levels (gray bars), planned skilled-worker admissions (blue portions of the bars), and the realized national unemployment rate (red line) from 1982 to 2012. The skilled-worker breakdown is available from 1991 onward. For 1991–1995, skilled workers include independent immigrants and assisted relatives, along with their spouses and dependents. For 1996–2012, skilled workers include both federal and Quebec skilled workers. The decline in planned skilled-worker admissions in 2008 partly reflects the introduction of the Canadian Experience Class. Panels (b) and (c) plot national initial unemployment rate (IUR) shocks against planned total admissions (1982–2012) and planned skilled-worker admissions (1991–2012), respectively. Solid lines represent linear fits, with Pearson correlation coefficients and corresponding p-values reported. Immigration-level plans are compiled from historical publications of Immigration, Refugees and Citizenship Canada (formerly Citizenship and Immigration Canada) and Statistics Canada.

Figure A.3: A 2-by-2 Difference-in-Differences illustration



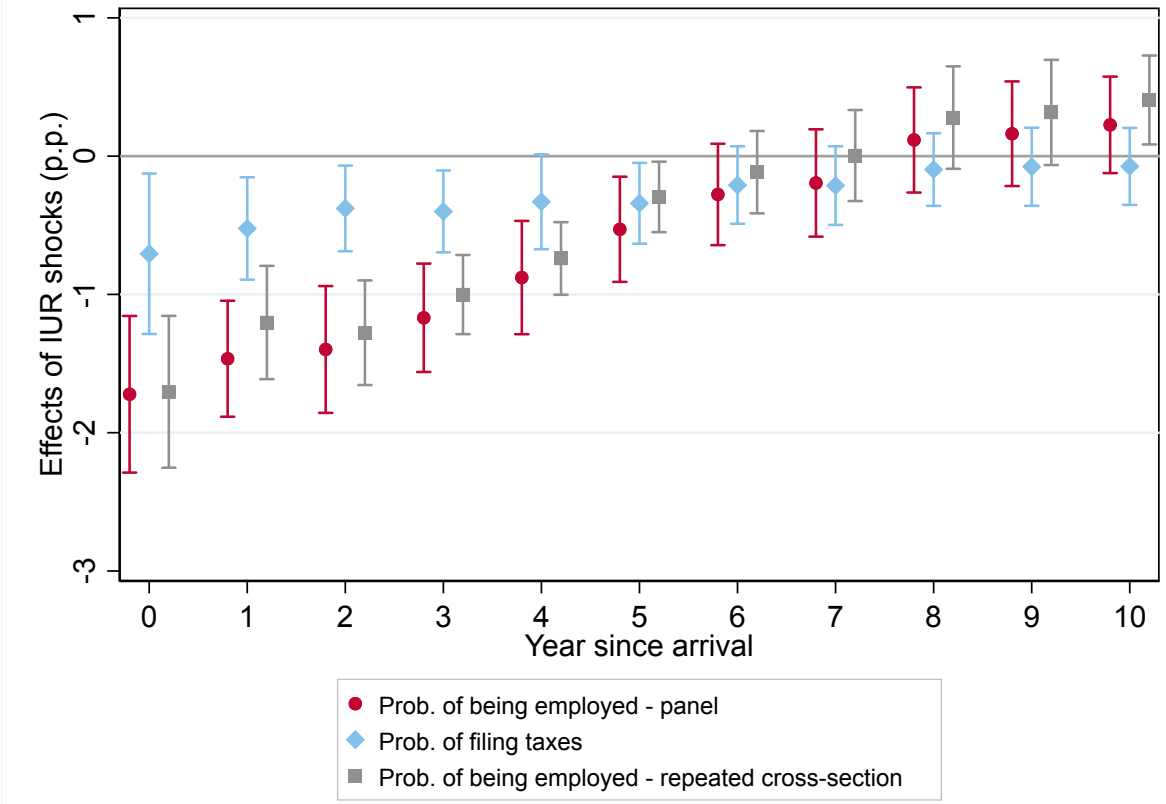
(a) Log earnings difference between British Columbia and Ontario by landing cohort



(b) Difference-in-differences estimates by years since arrival

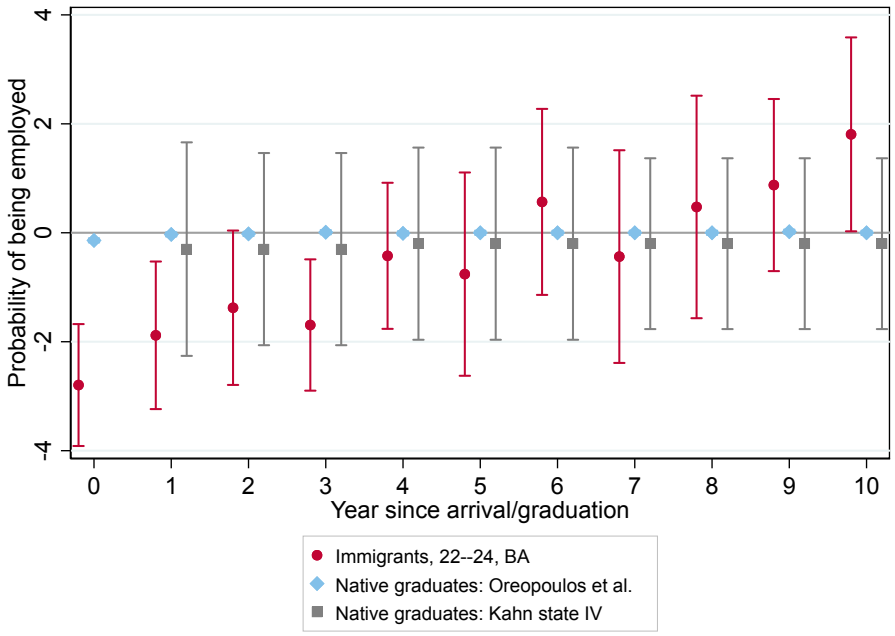
Note: The figure illustrates the difference-in-differences design using male immigrants who landed in 1998 and 2004 and initially intended to reside in British Columbia (BC) or Ontario (ON). Panel (a) plots the log earnings difference between BC and ON for each landing cohort over years since arrival. Panel (b) reports the corresponding difference-in-differences estimates at each year since arrival, which account for differences in cohort cell sizes, with vertical bars denoting 95% confidence intervals.

Figure A.4: Robustness to alternative measures of labor market attachment

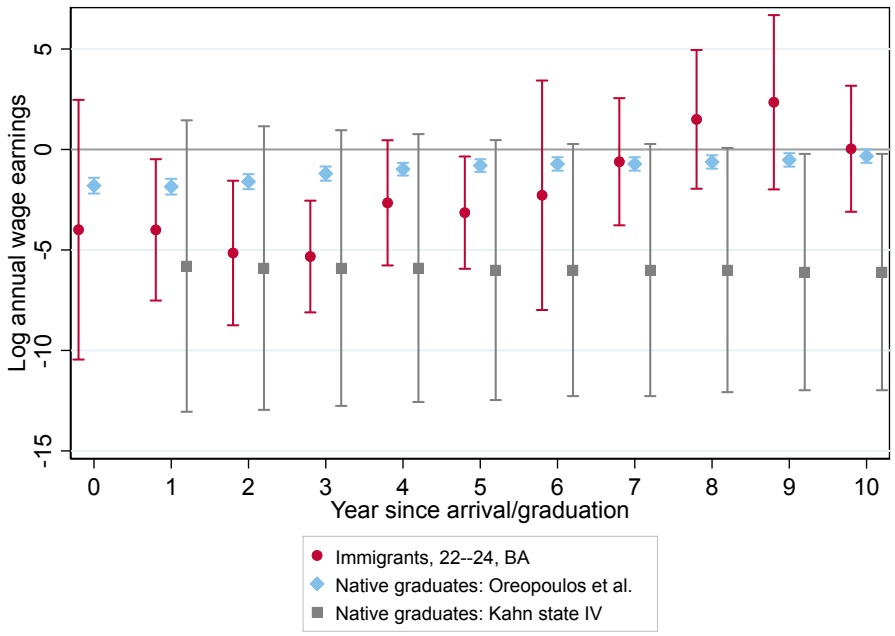


Notes: The figure reports the estimated dynamic effects of initial unemployment rate (IUR) shocks using three alternative outcome definitions. Red circles and vertical bars report estimates from the baseline panel specification, in which individuals who do not file a tax return are coded as not employed (i.e., having zero employment earnings). Blue diamonds and vertical bars report estimates using tax filing as the outcome. Gray squares and vertical bars report estimates from repeated cross-sectional regressions that restrict the sample to tax filers and use positive employment earnings as the employment outcome. Vertical bars denote 95% confidence intervals. The sample consists of male immigrants who landed between 1982 and 2012.

Figure A.5: Dynamic effects of initial unemployment rate shocks on employment and earnings: immigrants versus native graduates



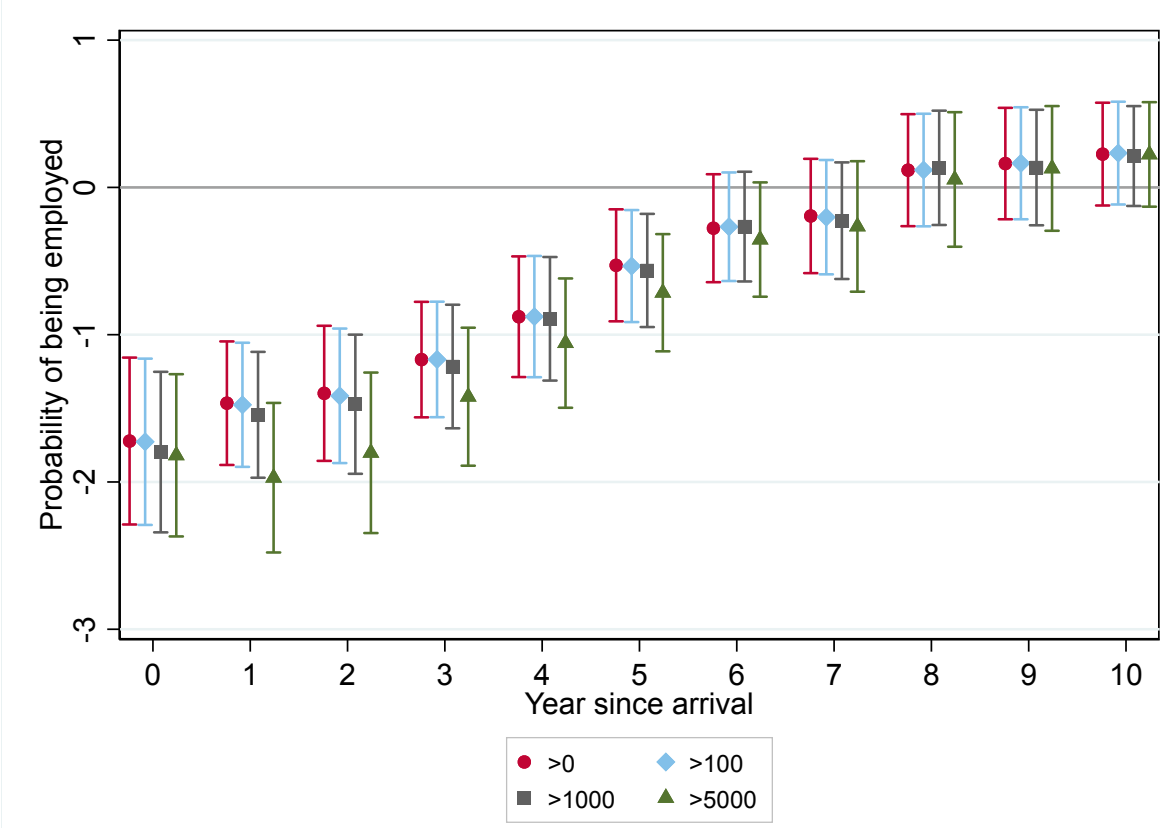
(a) Employment probability



(b) Real annual earnings

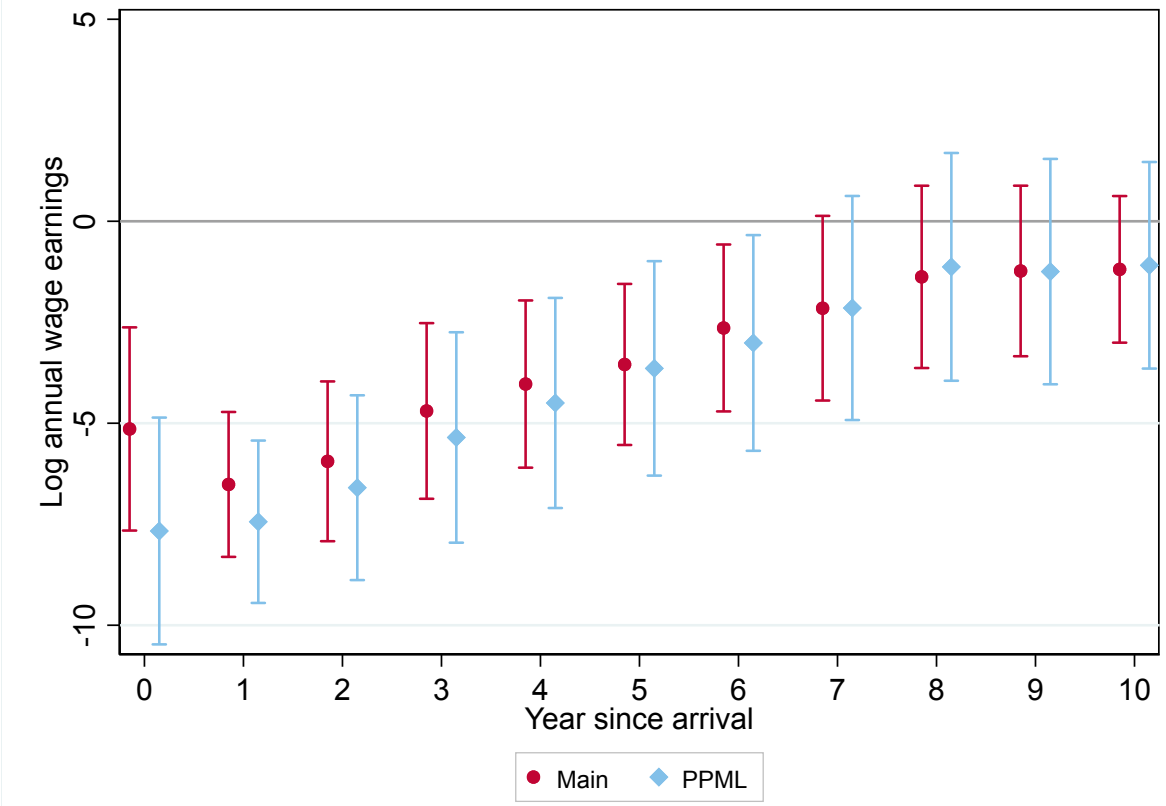
Note: The figure compares the estimated dynamic effects of initial unemployment rate (IUR) shocks on employment (panel (a)) and annual earnings (panel (b)) for immigrants and native college graduates. The red series reports estimates for immigrants who arrived between ages 22 and 24 with a bachelor's degree before 1995 from the flexible shock-based specification in this paper. The blue series reports estimates for Canadian native college graduates from Oreopoulos et al. (2012), and the gray series reports state-IV estimates for U.S. native college graduates from Kahn (2010). Coefficient estimates for the published studies are obtained from the reported figures, while standard errors are taken from the corresponding regression tables whenever available and otherwise approximated from the available values. Vertical bars denote 95% confidence intervals.

Figure A.6: Robustness of employment effects to alternative employment earnings thresholds



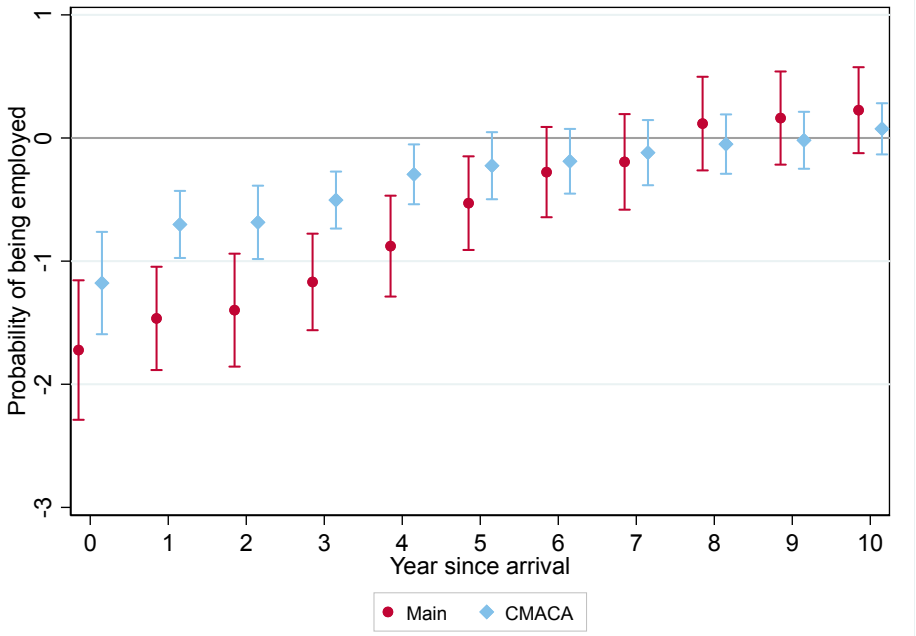
Notes: This figure examines the robustness of the estimated employment effects to alternative definitions of employment based on annual employment earnings. Employment is defined as having annual employment earnings exceeding 0 (baseline), $100, $1,000, or $5,000. Each marker reports the estimated effect of IUR shocks on the employment rate in each year since arrival. Vertical bars denote 95% confidence intervals. The sample consists of male immigrants who landed between 1982 and 2012.

Figure A.7: Robustness of earnings effects to PPML estimation

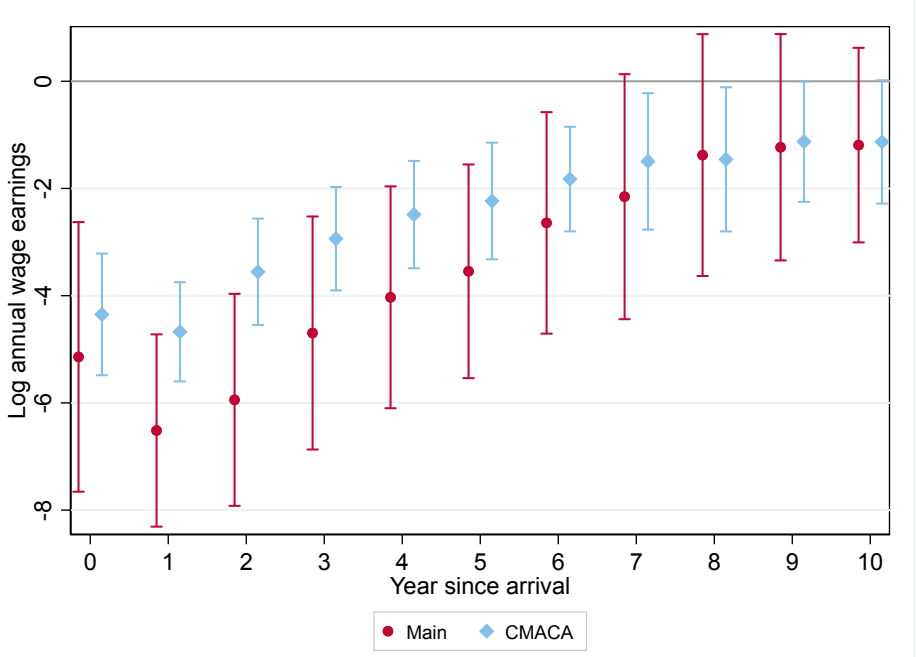


Notes: This figure examines the robustness of the estimated earnings effects to an alternative functional-form specification. The red circles report estimates from the baseline specification using log annual wage earnings, while the blue diamonds report estimates from a Poisson pseudo-maximum likelihood (PPML) specification using annual wage earnings in levels. Each marker reports the estimated effect of a one-percentage-point increase in the initial unemployment rate (IUR) shock in each year since arrival. Vertical bars denote 95% confidence intervals. Estimates are obtained from the flexible shock-based specification. The sample consists of male immigrants who landed between 1982 and 2012.

Figure A.8: Robustness of employment and earnings effects to alternative geographic variation in IUR shocks



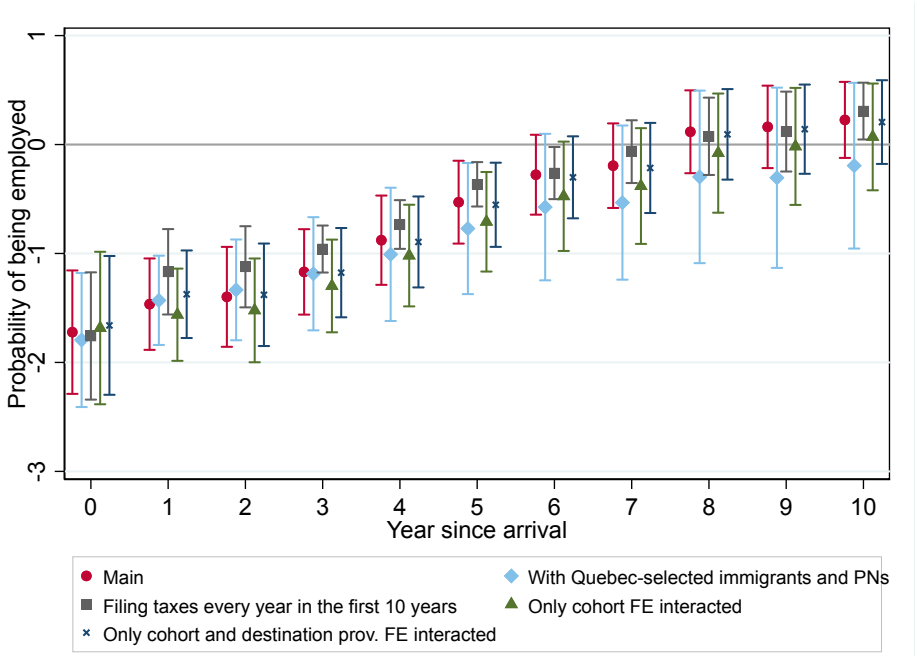
(a) Employment probability



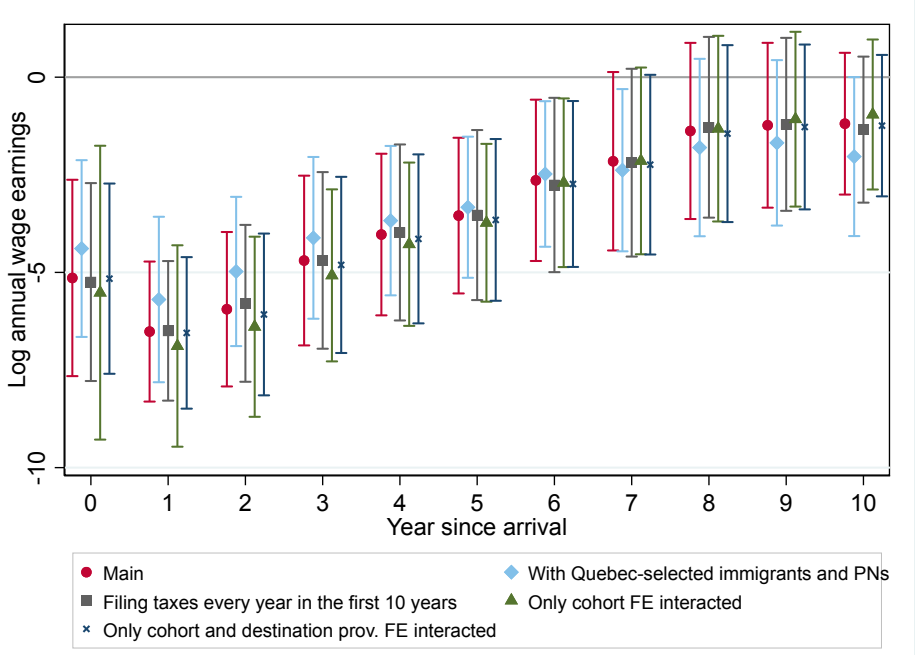
(b) Real annual earnings

Note: The figure examines the robustness of the estimated effects of initial unemployment rate (IUR) shocks to alternative geographic variation for identification. Panel (a) reports effects on employment, and Panel (b) reports effects on log annual wage earnings. The red circles report estimates from the baseline specification, which exploits variation in IUR shocks across provinces within landing cohorts. The blue diamonds report estimates from an alternative specification that exploits variation across Census Metropolitan Areas and Census Agglomerations (CMAs/CAs) within landing cohorts. Each marker reports the estimated effect of a one-percentage-point increase in the IUR shock in each year since arrival. At the CMA/CA level, IUR shocks are defined as the difference between realized and lagged unemployment rates. Vertical bars denote 95% confidence intervals. Both sets of estimates are obtained from the flexible shock-based specification. The sample consists of male immigrants who landed between 1982 and 2012.

Figure A.9: Robustness of employment and earnings effects to alternative samples and model specifications



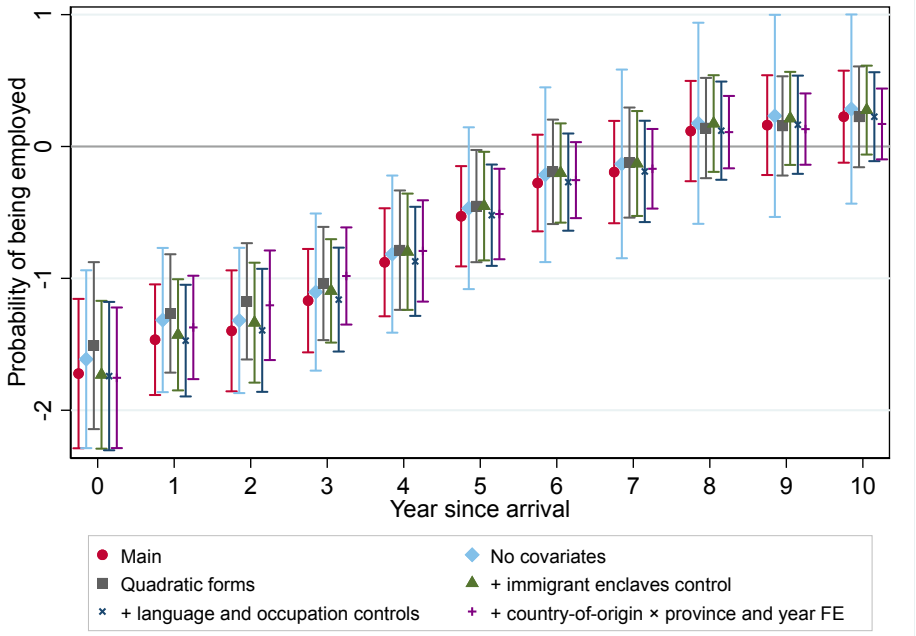
(a) Employment probability



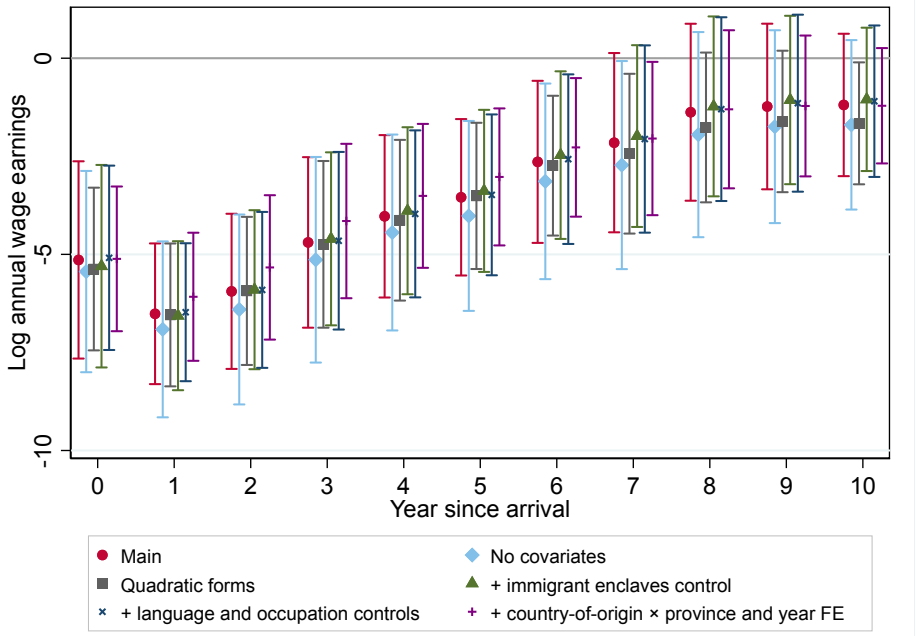
(b) Real annual earnings

Note: The figure reports robustness checks for the employment and earnings estimates. The five series correspond to: (1) the baseline flexible specification; (2) including Quebec-selected immigrants and Provincial Nominees; (3) restricting the sample to immigrants who file taxes in every year during the first 10 years after arrival; (4) interacting only landing cohort fixed effects with year since arrival fixed effects; and (5) interacting only landing cohort and destination province fixed effects with year since arrival fixed effects. Vertical bars denote 95% confidence intervals.

Figure A.10: Robustness of employment and earnings effects to alternative model specifications



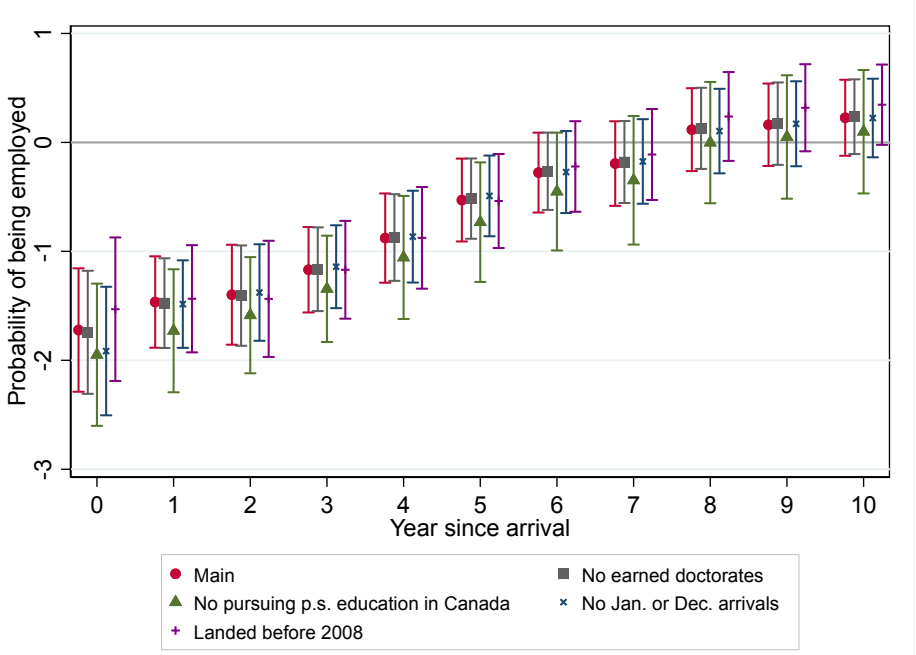
(a) Employment probability



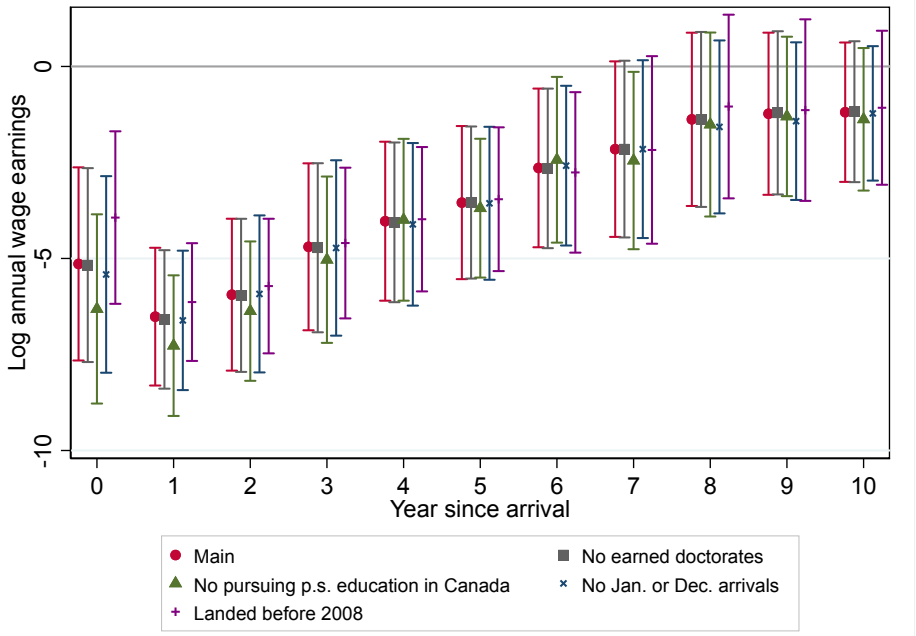
(b) Real annual earnings

Note: The figure examines the robustness of the estimated employment and earnings effects to alternative model specifications. The four series correspond to: (1) the baseline flexible specification; (2) excluding individual controls; (3) including quadratic terms for both IUR shocks and prediction; and (4) additionally controlling for the size of local immigrant enclaves. Vertical bars denote 95% confidence intervals. The sample consists of male immigrants who landed between 1982 and 2012.

Figure A.11: Robustness of employment and earnings effects to alternative samples



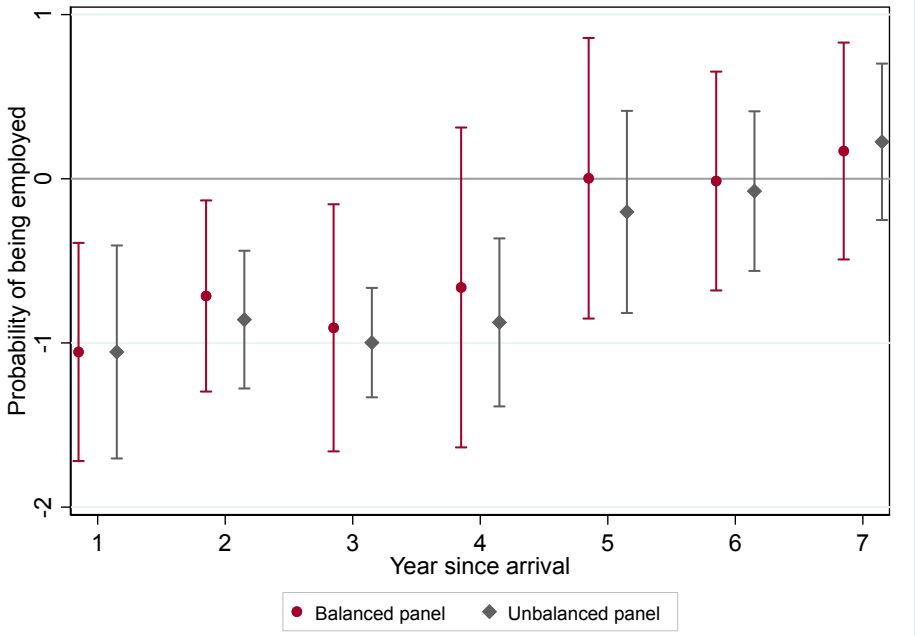
(a) Employment probability



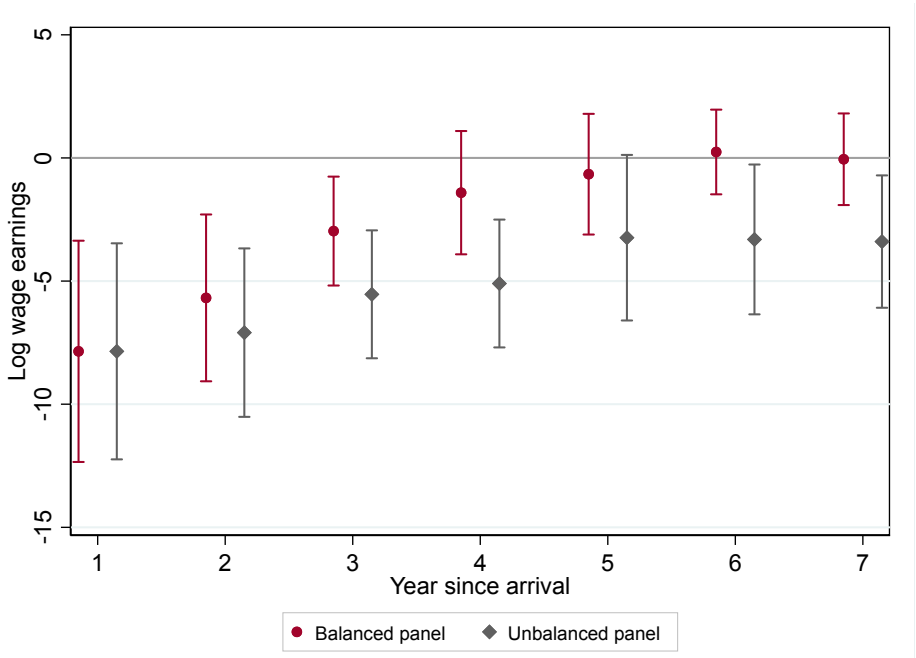
(b) Real annual earnings

Note: The figure examines the robustness of the estimated employment and earnings effects to alternative sample restrictions. The five series correspond to: (1) the baseline sample; (2) excluding immigrants with doctoral degrees; (3) excluding immigrants who pursued post-secondary education in Canada after arrival; (4) excluding immigrants who arrived in January or December; (5) immigrants who landed before 2008. Vertical bars denote 95% confidence intervals.

Figure A.12: Dynamic effects of initial unemployment rate shocks on employment and earnings: evidence from the CEEDD sample



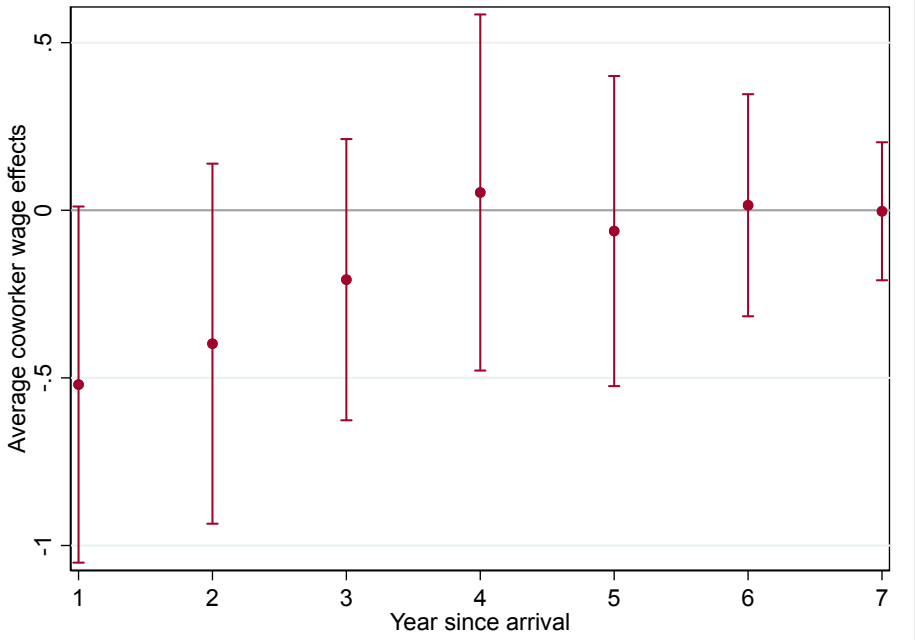
(a) Employment probability



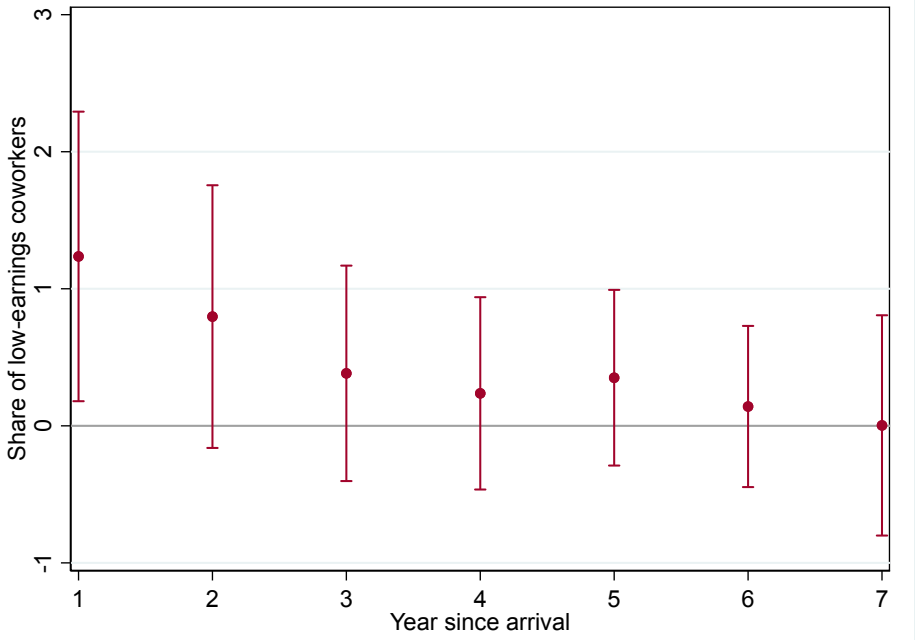
(b) Real annual earnings

Note: The figure plots the estimated dynamic effects of IUR shocks on employment (Panel (a)) and annual earnings (Panel (b)) over the first seven years after arrival. Each panel reports estimates from two flexible specifications: one using the balanced CEEDD sample and the other using the unbalanced CEEDD sample. Vertical bars indicate 95% confidence intervals.

Figure A.13: Dynamic effects of initial unemployment rate shocks on coworker composition



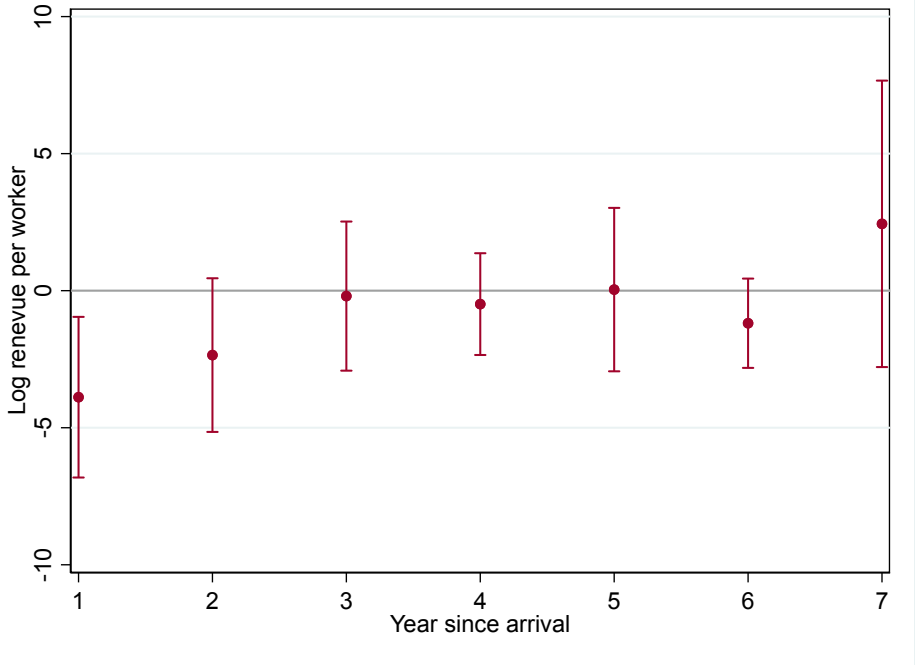
(a) Average coworker AKM worker effect



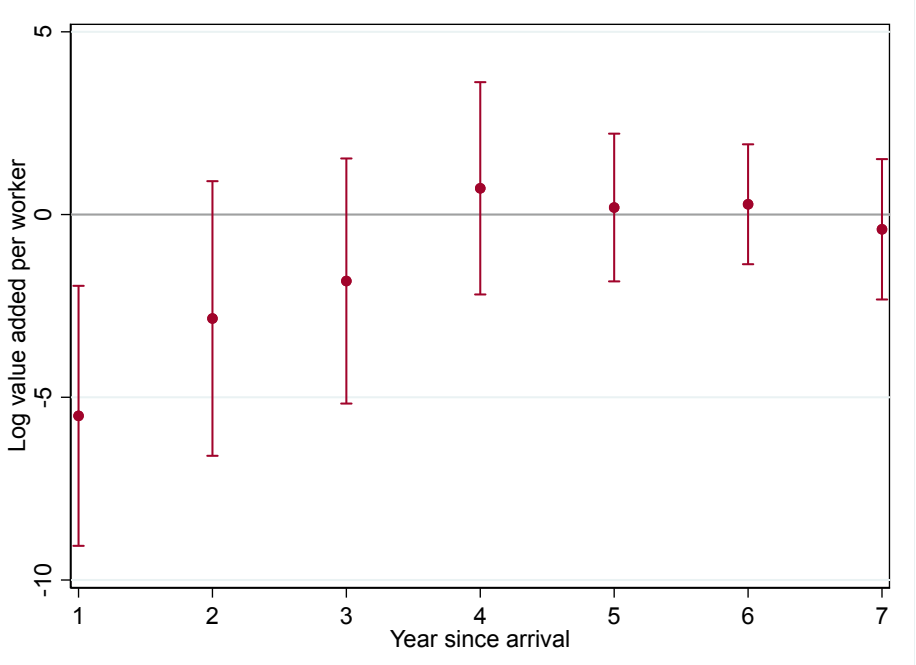
(b) Share of low-earnings coworkers

Notes: The figure plots the estimated dynamic effects of initial unemployment rate (IUR) shocks on coworker composition within immigrants' primary employers over the first seven years after arrival. Panel (a) reports effects on the leave-one-out average AKM worker effect among coworkers. Panel (b) reports effects on the share of coworkers with annual earnings from the firm below $19,764 in 2016 dollars. Vertical bars denote 95% confidence intervals. Estimates are obtained from the flexible shock-based specification. The sample consists of male immigrants who landed between 2000 and 2012.

Figure A.14: Dynamic effects of initial unemployment rate shocks on total revenue and value added per worker



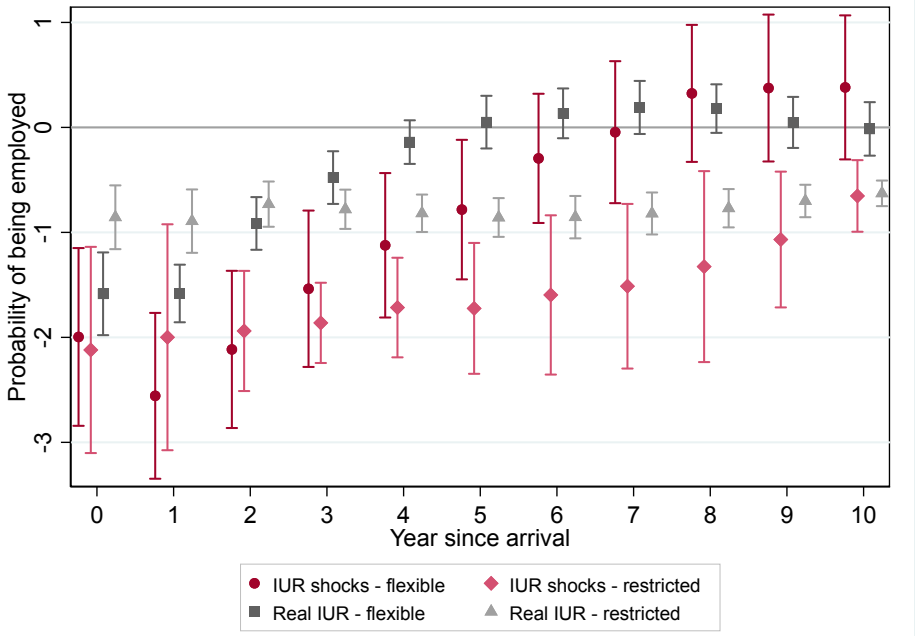
(a) Log revenue per worker



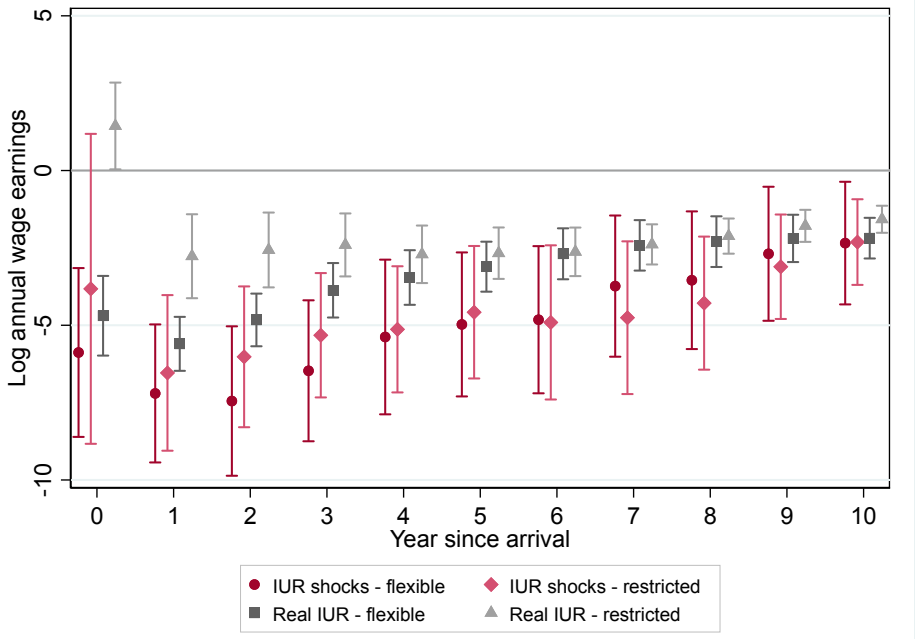
(b) Log value added per worker

Notes: The figure plots the estimated dynamic effects of initial unemployment rate (IUR) shocks on log revenue per worker (panel (a)) and log value added per worker (panel (b)) over the first seven years after arrival. Vertical bars denote 95% confidence intervals. Estimates are obtained from the flexible shock-based specification.

Figure A.15: Dynamic effects of the initial unemployment rate shocks on employment and earnings: female immigrants



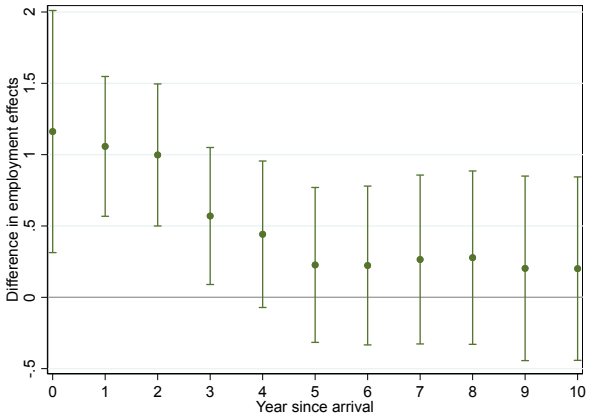
(a) Employment probability



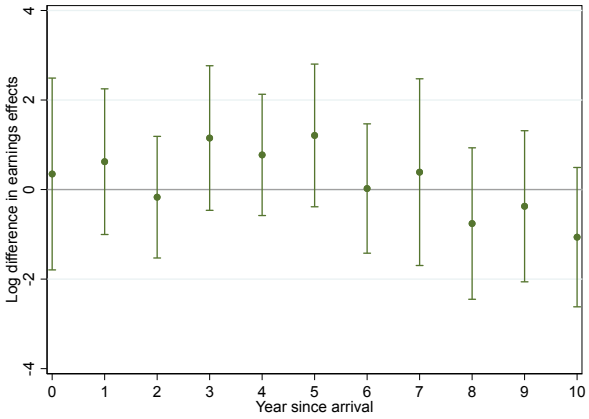
(b) Real annual earnings

Note: The figure plots the estimated dynamic effects of initial unemployment rate (IUR) shocks on employment (panel (a)) and real annual earnings (panel (b)) over the first ten years after arrival for female immigrants. The four series correspond to: (1) the flexible shock-based specification; (2) the restricted shock-based specification; (3) the flexible realized-IUR specification; and (4) the restricted realized-IUR specification. Vertical bars denote 95% confidence intervals. The sample consists of female immigrants who landed between 1982 and 2012.

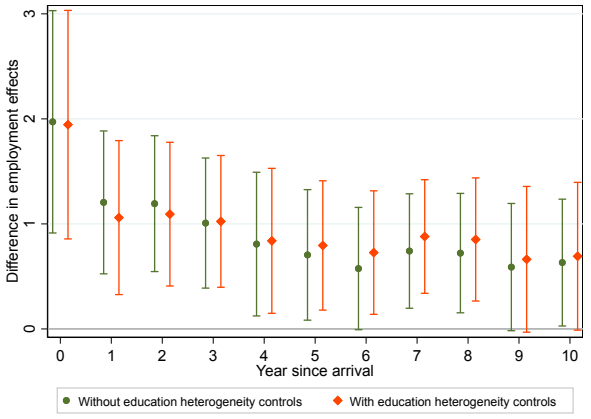
Figure A.16: Heterogeneous effects of initial unemployment rate shocks by education and immigration class



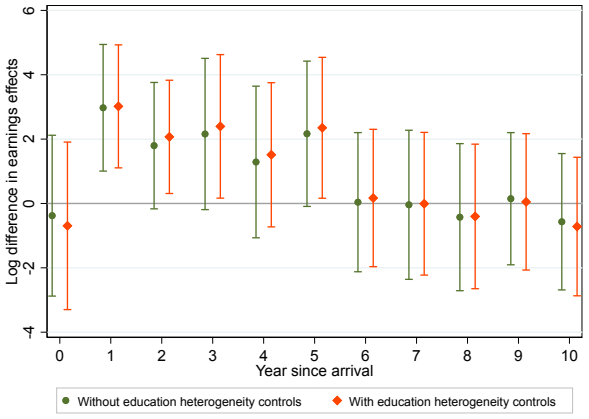
(a) Education heterogeneity: employment



(b) Education heterogeneity: earnings



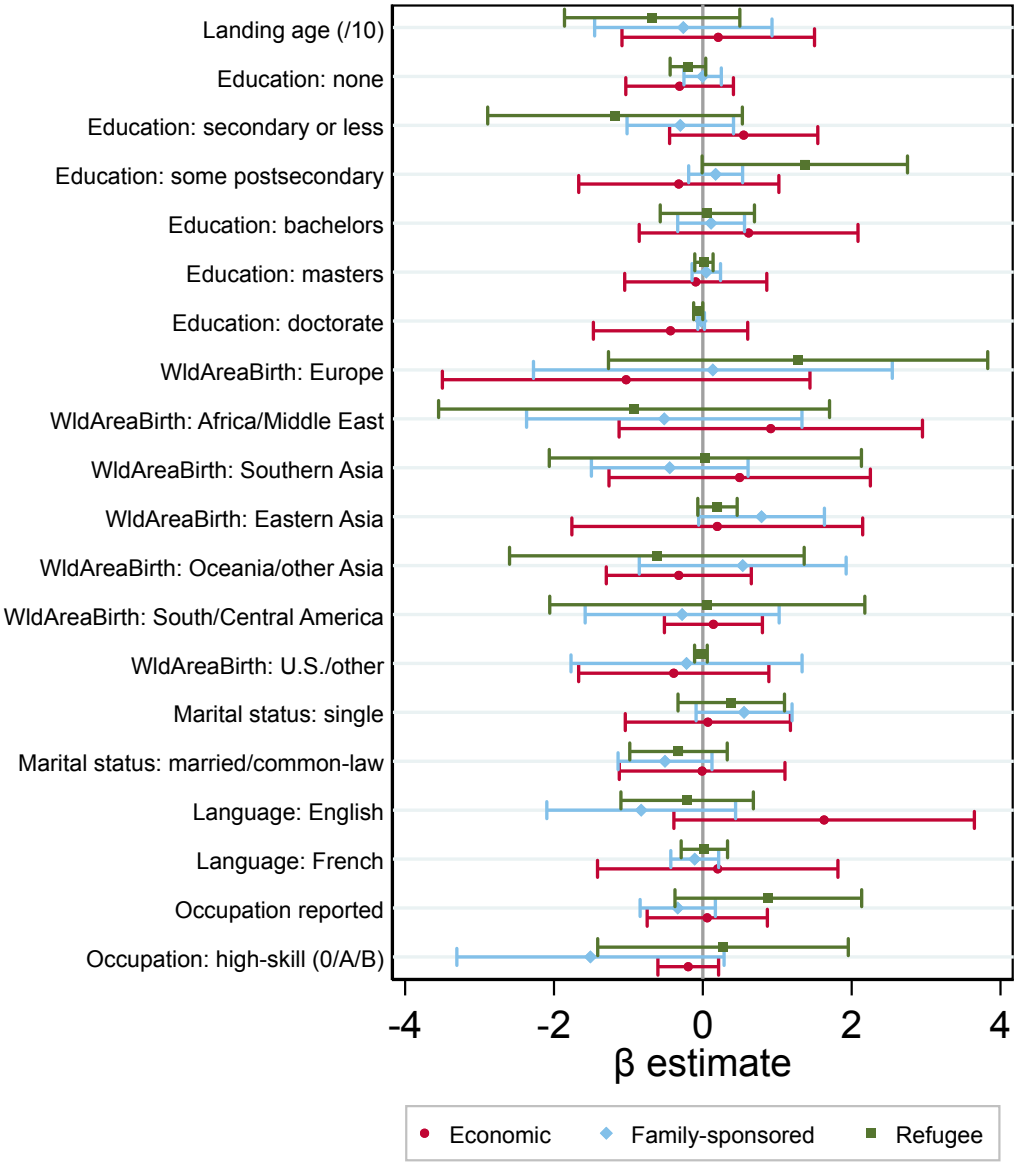
(c) Immigration-class heterogeneity: employment



(d) Immigration-class heterogeneity: earnings

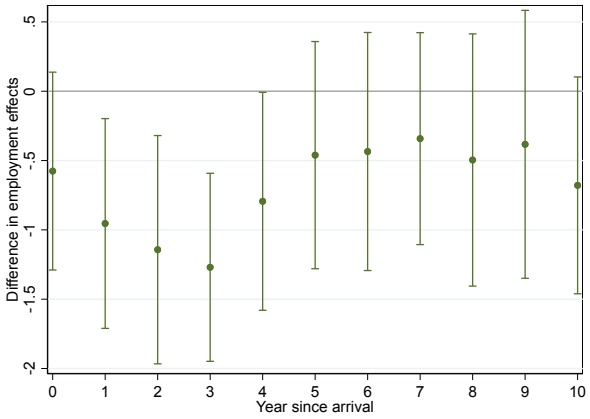
Notes: This figure examines heterogeneity in the effects of initial unemployment rate (IUR) shocks by educational attainment and immigration class. Panels (a) and (b) report the coefficients on the interaction between the IUR shock and an indicator for having at least a bachelor's degree, for employment and earnings, respectively. Panels (c) and (d) report the differences in the effects of IUR shocks between economic and non-economic immigrants. Green circles show estimates from specifications without controls for education heterogeneity, while orange diamonds show estimates from specifications that additionally allow the effects of IUR shocks to vary by educational attainment. All panels report point estimates and 95% confidence intervals.

Figure A.17: Coefficient estimates relating provincial initial labor market conditions to landing characteristics, by immigration class

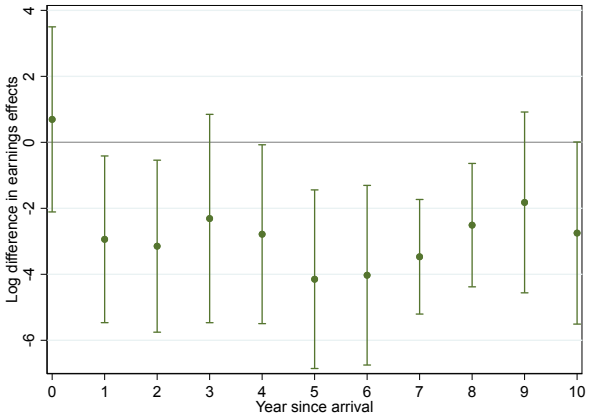


Notes: The figure reports coefficient estimates on the initial unemployment rate (IUR) shock and 95% confidence intervals from regressions of province-by-landing-cohort immigrant landing characteristics on both the IUR shock and the predicted provincial IUR, controlling for destination-province and landing-year fixed effects, estimated separately for economic, family-sponsored, and refugee immigrants. Red, blue, and green circles correspond to economic, family-sponsored, and refugee immigrants, respectively. The sample consists of male immigrant landing cohorts from 1982 to 2012.

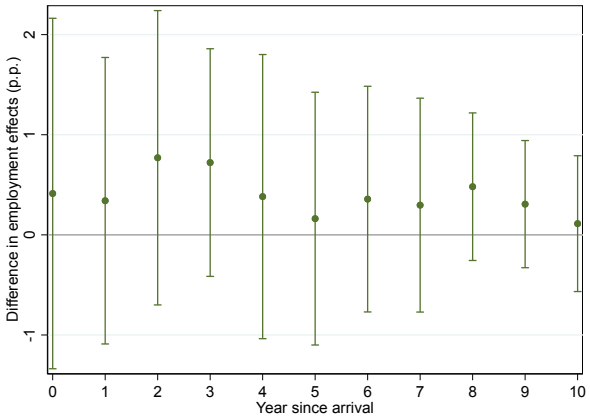
Figure A.18: Heterogeneous effects of initial unemployment rate shocks



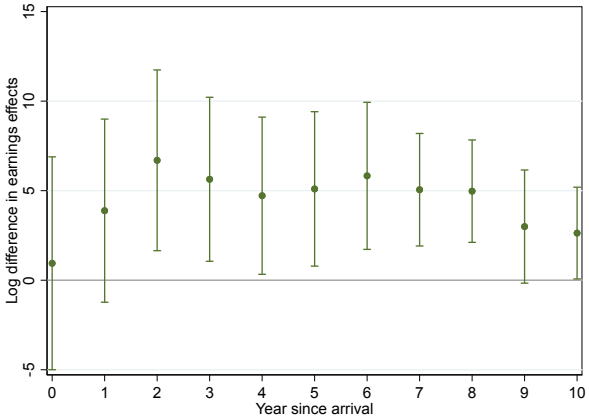
(a) IUR shock × older immigrant: employment



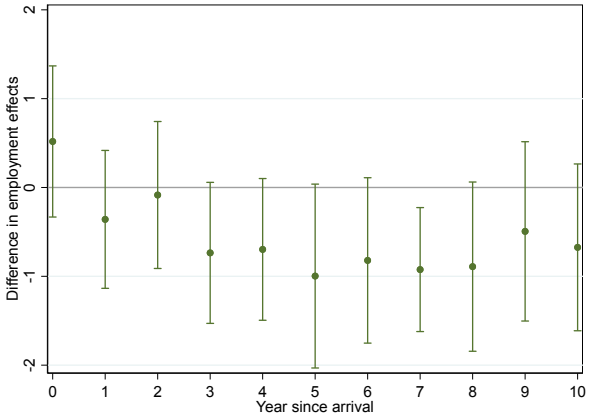
(b) IUR shock × older immigrant: earnings



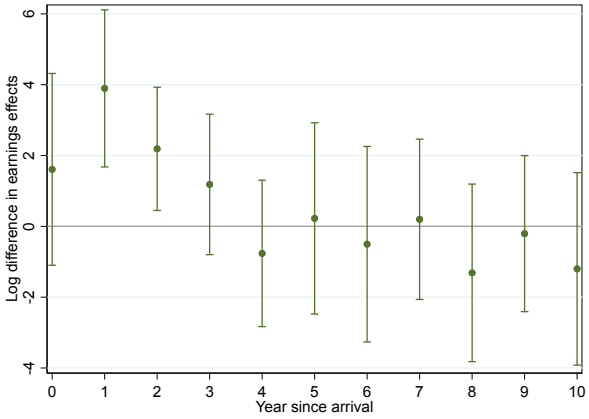
(c) IUR shock × recession: employment



(d) IUR shock × recession: earnings



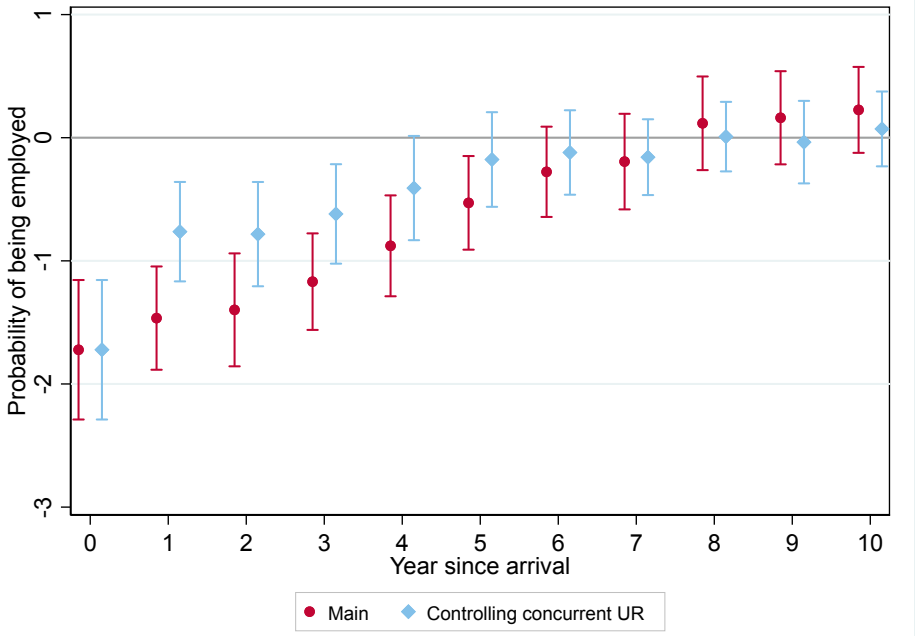
(e) IUR shock × advantaged origin: employment



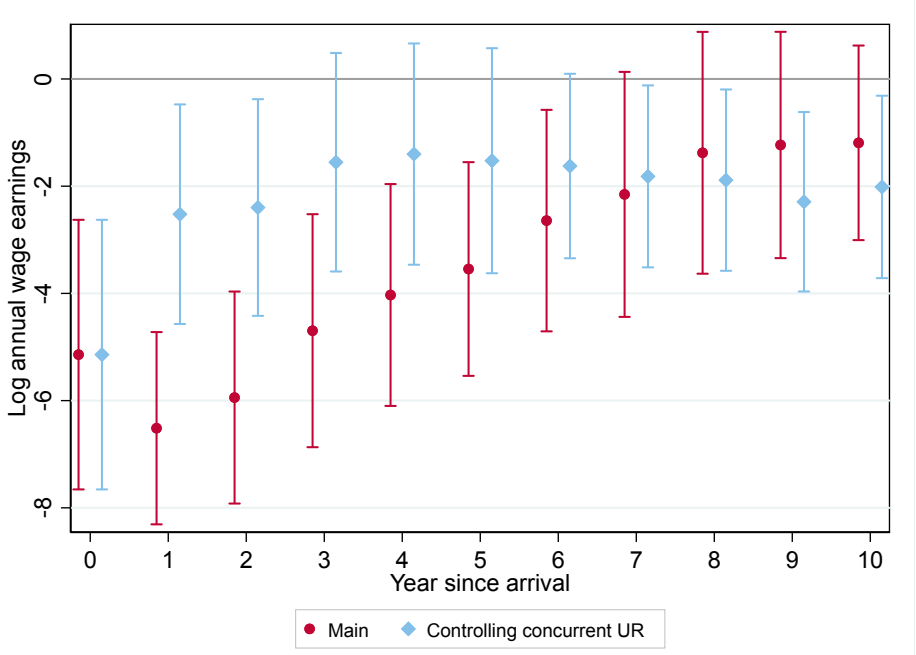
(f) IUR shock × advantaged origin: earnings

Notes: The figure reports heterogeneous effects of initial unemployment rate (IUR) shocks by landing age, expected business-cycle conditions at arrival, and country of origin. Each panel plots the coefficient on the interaction between the IUR shock and the corresponding subgroup indicator, together with its 95% confidence interval.

Figure A.19: Effects of initial unemployment rate shocks on employment and earnings net of concurrent unemployment rates



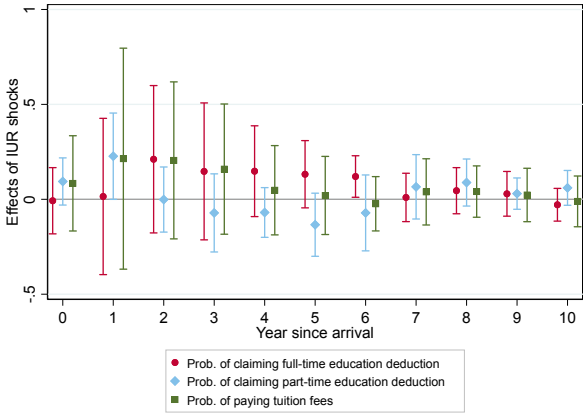
(a) Employment probability



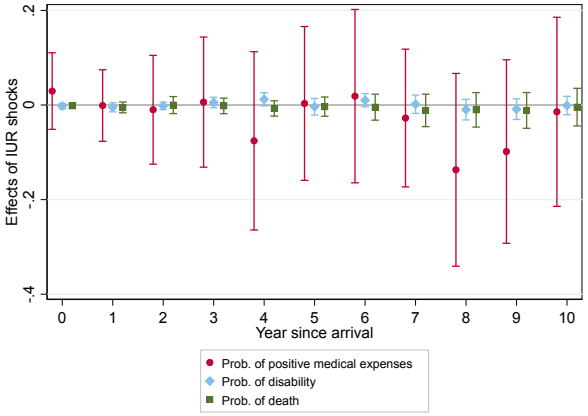
(b) Real annual earnings

Notes: The figure reports the estimated effects of IUR shocks on employment and earnings conditional on concurrent unemployment rates. The two series correspond to: (1) the baseline specification; and (2) the specification controlling for concurrent unemployment rates. Vertical bars denote 95% confidence intervals. The sample consists of male immigrants who landed between 1982 and 2012.

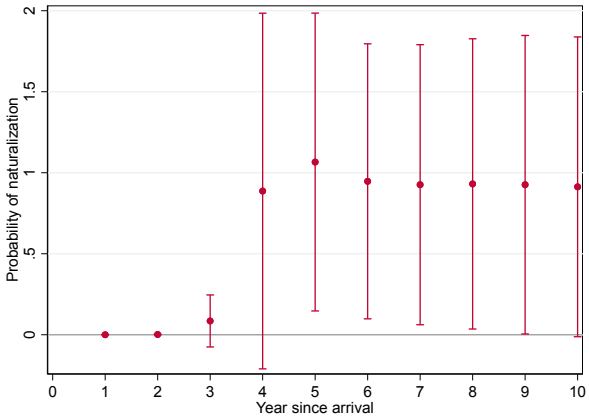
Figure A.20: Dynamic effects of initial unemployment rate shocks on education, health, political, and family outcomes



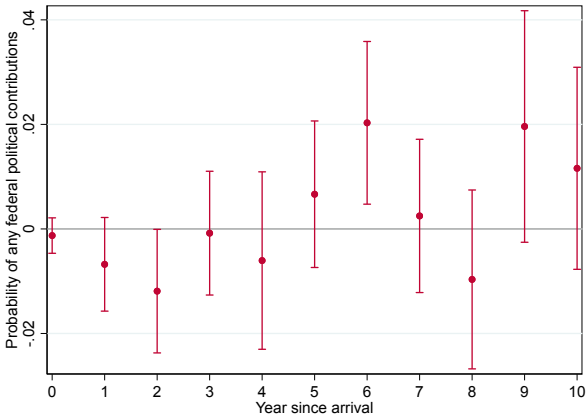
(a) Education



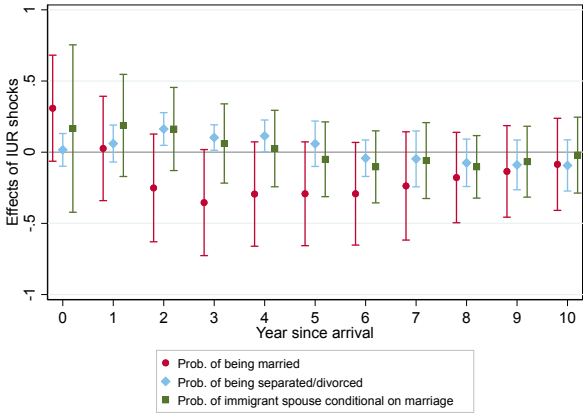
(b) Health



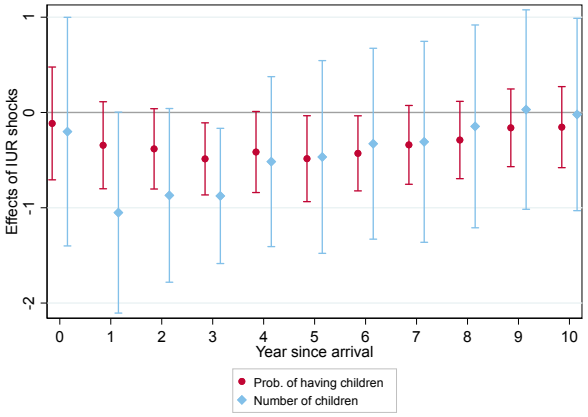
(c) Naturalization



(d) Probability of positive federal political contribution



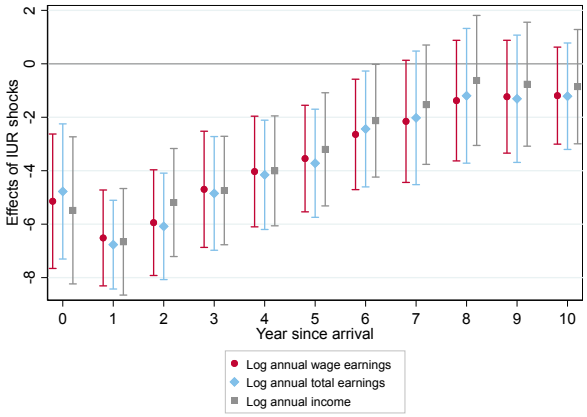
(e) Marriage



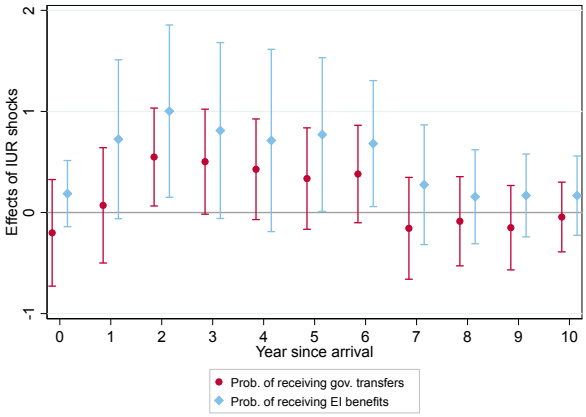
(f) Children

Notes: The figure reports the dynamic effects of initial unemployment rate (IUR) shocks on education (panel (a)), health (panel (b)), naturalization (panel (c)), federal political contribution (panel (d)), marriage (panel (e)), and children outcomes (panel (f)). Vertical bars denote 95% confidence intervals.

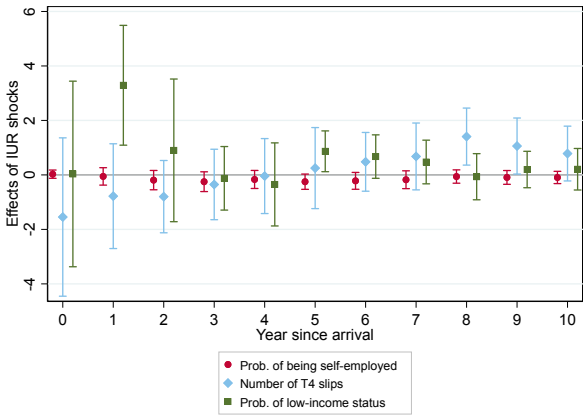
Figure A.21: Dynamic effects of initial unemployment rate shocks on additional IMDB outcomes



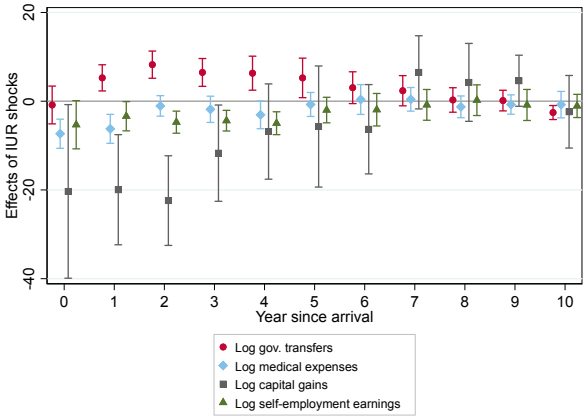
(a) Wage earnings, total earnings, and total income



(b) Government transfers and employment insurance benefits



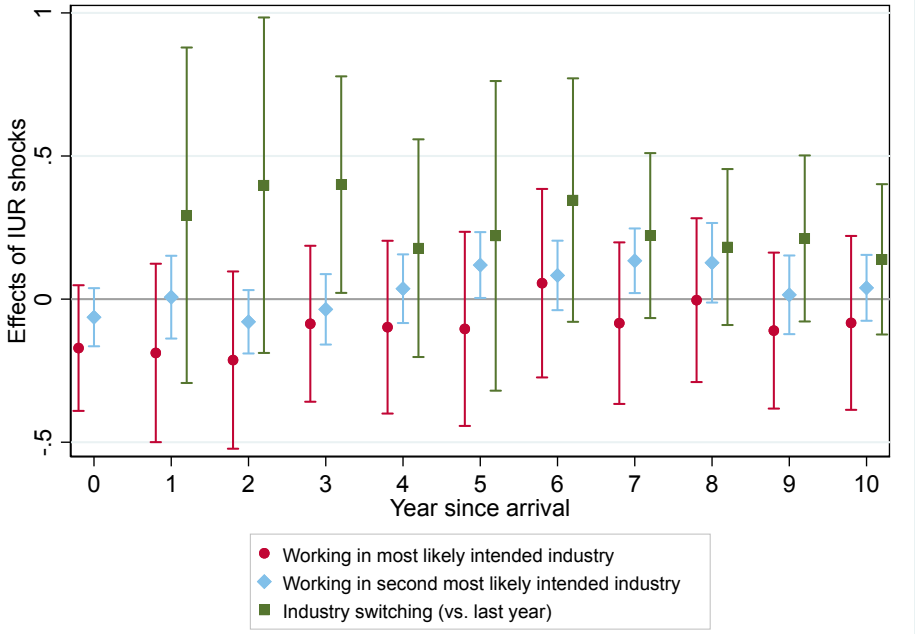
(c) Self-employment, T4 slip numbers, low-income status



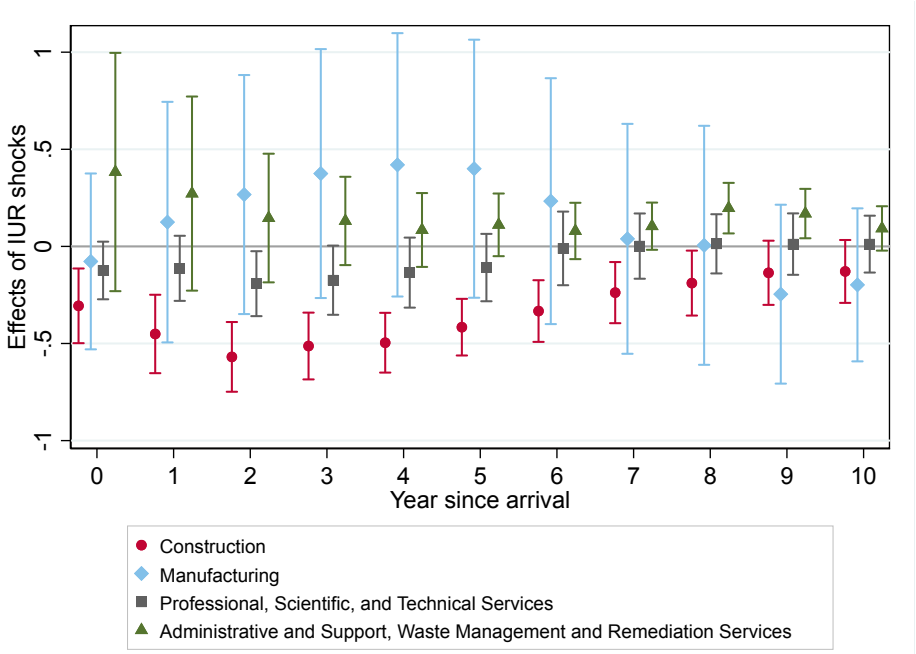
(d) Other income components

Notes: The figure reports the dynamic effects of initial unemployment rate (IUR) shocks on additional IMDB outcomes. In panel (a), the red, blue, and gray series report the effects on annual wage earnings, total annual earnings, and total annual income, respectively. In panel (b), the red and blue series report the effects on the probability of receiving any government transfers and any Employment Insurance (EI) benefits, respectively. In panel (c), the red, blue, and green series report the effects on the probability of being self-employed, the number of T4 slips, and the probability of having low-income status, respectively. In panel (d), the red, blue, gray, and green series report the effects on government transfer income, medical expenses, capital gains, and self-employment earnings, conditional on having positive amounts of each income component, respectively. Vertical bars denote 95% confidence intervals.

Figure A.22: Dynamic effects of the initial unemployment rate shocks on industry sorting and mobility



(a) Industry matching and mobility



(b) Employment in selected industries

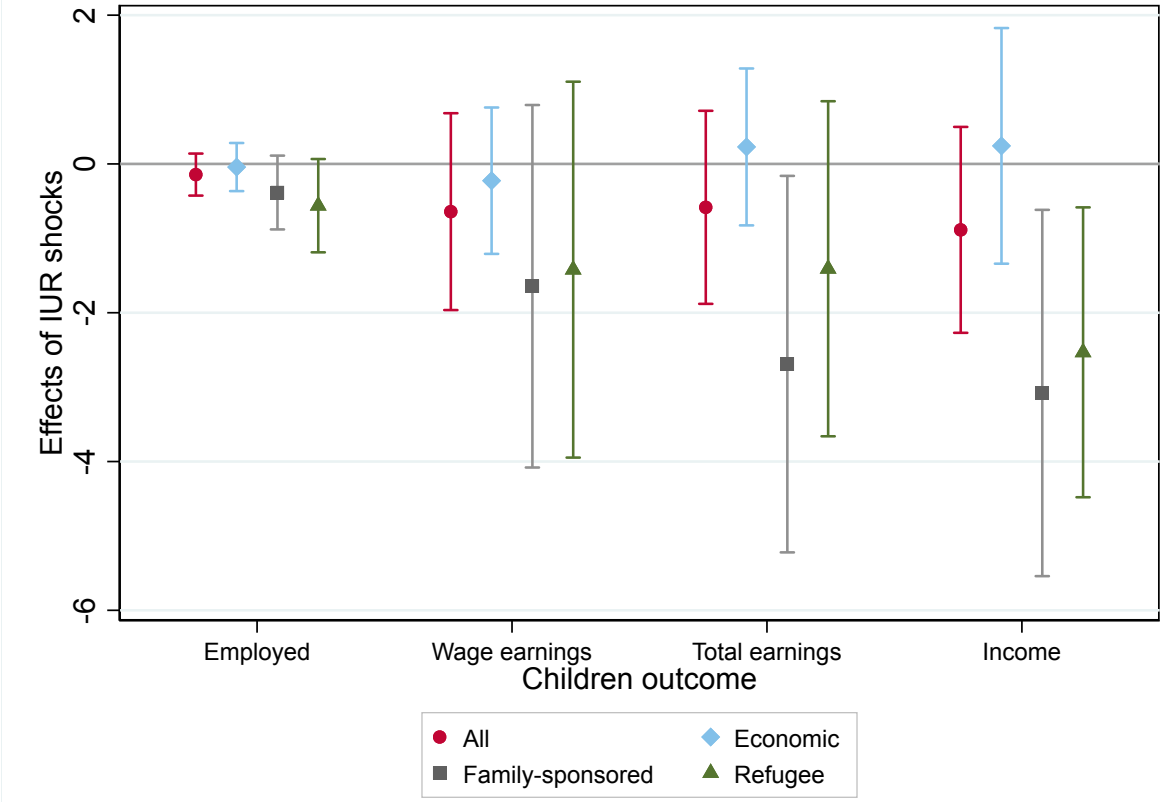
Note: The figure reports the dynamic effects of initial unemployment rate (IUR) shocks on industry sorting and industry mobility. In panel (a), the red and blue series report the effects on the probability of working in the most likely and second most likely intended industries, respectively, while the green series reports the probability of switching industries relative to the previous year. In panel (b), the red, blue, gray, and green series show the estimated effects on the probabilities of working in construction; manufacturing; professional, scientific, and technical services; and administrative and support, waste management, and remediation services, respectively. Vertical bars denote 95% confidence intervals.

Figure A.23: Effects of initial unemployment rate shocks on 2016 Census outcomes



Notes: The figure reports coefficient estimates on the initial unemployment rate (IUR) shock and 95% confidence intervals from regressions of 2016 Census outcomes on both the IUR shock and the predicted provincial IUR, controlling for destination-province and landing-year fixed effects. The upper half reports estimates for immigrants in the main sample, while the lower half reports estimates for their children.

Figure A.24: Effects of mothers' initial unemployment rate shocks on adult children's labor market outcomes



Notes: The figure reports the estimated effects of mothers' initial unemployment rate (IUR) shocks on adult children's labor market outcomes measured between ages 30 and 34. Outcomes include employment, annual wage earnings, total annual earnings, and total annual income. Estimates are reported separately for all immigrants, economic immigrants, family-sponsored immigrants, and refugees. Vertical bars denote 95% confidence intervals. The sample consists of the 1.5-generation children of female immigrants who landed between 1982 and 2012.

Table A.4: Initial labor market conditions, COPR issuance, and PR admissions

	Log COPR issued		Log PR admissions		Difference	
	(1)	(2)	(3)	(4)	(5)	(6)
Real IUR	-0.190*		-0.188**		-0.002	
	(0.068)		(0.053)		(0.015)	
IUR shock		-0.107		-0.106		-0.000
		(0.099)		(0.082)		(0.018)
IUR prediction		-0.220*		-0.218**		-0.003
		(0.073)		(0.059)		(0.015)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Destination province FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	40	40	40	40	40	40

Notes: The dependent variable is the logarithm of the total number of Confirmation of Permanent Residence (COPR) documents issued in columns (1)–(2), the logarithm of the total number of Permanent Resident (PR) admissions in columns (3)–(4), and the difference between the logarithm of COPR documents issued and the logarithm of PR admissions in columns (5)–(6). Columns (1), (3), and (5) use the realized provincial IUR as the measure of initial labor market conditions. Columns (2), (4), and (6) include both the provincial IUR shock and the predicted IUR. All regressions include destination-province and landing-year fixed effects. The data are from the Immigration, Refugees and Citizenship Canada (IRCC) and cover the period from 2021 to 2024. Standard errors, clustered at the landing-year level, are reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

Table A.5: Labor market conditions and firm hiring

	Any new hires		Log new hires		Immigrant share of new hires	
	(1)	(2)	(3)	(4)	(5)	(6)
IUR shock	-1.341***	-1.047***	-5.744***	-5.360***	-0.134	-0.0907
	(0.243)	(0.197)	(1.041)	(0.897)	(0.146)	(0.152)
Shock × firm effect	-0.718**	-0.232	-5.100***	-3.891***	-0.756*	-0.241
	(0.235)	(0.155)	(1.237)	(0.665)	(0.380)	(0.273)
IUR prediction	-0.910***	-0.763***	-4.775***	-4.889***	-0.357***	-0.381***
	(0.112)	(0.0865)	(0.412)	(0.348)	(0.0941)	(0.103)
Prediction × firm effect	-0.104	0.102	-1.170	-1.737*	-0.287	0.118
	(0.131)	(0.161)	(1.104)	(0.790)	(0.261)	(0.213)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No	Yes	No
Year × industry FE	No	Yes	No	Yes	No	Yes

Notes: This table examines the relationship between provincial initial labor market conditions and firms' contemporaneous hiring outcomes, allowing the effects to vary with estimated AKM firm wage premiums. Columns (1)–(2) report effects on an indicator for any new hires, columns (3)–(4) on the log number of new hires, and columns (5)–(6) on the immigrant share of new hires. All specifications include firm fixed effects. Odd-numbered columns include year fixed effects, while even-numbered columns include industry-by-year fixed effects (four-digit NAICS). Standard errors are in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.