



**Essays on migration, labor market integration, and social
perceptions: Evidence from the Venezuelan Exodus in Colombia**

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Para Juan Martín.

Ser tu mamá es el título más importante que puedo tener.

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Resumen

Esta tesis reúne tres artículos empíricos que examinan dimensiones centrales del proceso de integración de los migrantes venezolanos en Colombia: el acceso a la autorización legal para trabajar y el persistente desajuste ocupacional, la prevalencia de creencias en la sociedad receptora que vinculan a los migrantes con la inseguridad, y la evolución de los efectos de la migración sobre el mercado laboral ante un choque de gran magnitud como la pandemia de COVID-19. El primer capítulo muestra que los permisos de trabajo fortalecen la vinculación al mercado laboral, en particular entre migrantes con menor inserción laboral antes de migrar, pero tienen efectos limitados sobre la movilidad ocupacional y la calidad del empleo. El segundo capítulo estudia si las personas locales perciben que los migrantes incrementan la inseguridad en la ciudad y, para identificar la prevalencia de esta creencia, utiliza un experimento de lista implementado en Cali, Colombia, que permite elicitarse actitudes socialmente sensibles de manera indirecta y mitigar el sesgo de deseabilidad social asociado con preguntas directas. El tercer capítulo muestra que la migración venezolana redujo el empleo y los salarios e incrementó la informalidad entre colombianos y migrantes previos, con efectos que varían en el tiempo y se intensifican durante la pandemia, mientras que los migrantes con mayor tiempo de residencia muestran una mayor resiliencia. En general, los resultados sugieren que la integración de los migrantes depende no solo del acceso legal al trabajo, sino también del entorno social y de las condiciones macroeconómicas bajo las cuales ocurre el ajuste.

Summary

This dissertation brings together three empirical papers that examine key dimensions of the integration process of Venezuelan migrants in Colombia: access to legal work authorization and persistent occupational mismatch, the prevalence of beliefs in the host society linking migrants to insecurity, and the evolution of the effects of migration on the labor market in the presence of a large shock such as the COVID-19 pandemic. The first chapter shows that work permits strengthen labor market attachment, particularly among migrants with weaker pre-migration labor force participation, but have limited effects on occupational mobility and job quality. The second chapter studies whether locals perceive that migrants increase insecurity in the city and, to identify the prevalence of this belief, implements a list experiment in Cali, Colombia, which allows for the indirect elicitation of socially sensitive attitudes and mitigates social desirability bias associated with direct questioning. The third chapter shows that Venezuelan migration reduced employment and wages and increased informality among locals and earlier migrants, with effects that vary over time and intensify during the pandemic, while migrants with longer residence exhibit greater resilience. Overall, the results suggest that migrant integration depends not only on legal access to work, but also on the social environment and the macroeconomic conditions under which adjustment takes place.

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Introduction

It has been nearly eight years since Venezuela's socio-economic and political crisis triggered the largest migration exodus in the recent history of Latin America. Colombia has absorbed a considerable share of this flow: by mid-2024, it hosted approximately 2.8 million Venezuelan refugees and migrants, accounting for close to 35% of the regional total ([Migración Colombia, 2024](#)). In principle, several features of the Venezuelan-Colombian migration corridor could reduce integration frictions: geographic proximity, a shared language, cultural similarities, and long-standing cross-border ties may facilitate communication, adaptation, and access to informal networks. However, the process appears to be more complex. Beyond standard labor market adjustments, the integration of migrants unfolds within a broader environment shaped not only by institutional constraints and economic conditions, but also by how migrants are perceived in receiving societies.

While a large body of work has examined the labor market effects of Venezuelan migration in Colombia on outcomes such as employment, wages, and informality ([Bahar, Ibáñez, & Rozo, 2021](#); [Bonilla-Mejía, Morales, Hermida, & Flórez, 2024](#); [Caruso, Gomez Canon, & Mueller, 2021](#); [Delgado-Prieto, 2024](#); [Lebow, 2022](#); [Otero-Cortés, Tribín-Uribe, & Mojica-Urueña, 2022](#); [Santamaria, 2019](#)), less is known about how these economic mechanisms interact with social perceptions to shape integration outcomes. This dissertation addresses this gap by jointly analyzing migrants' labor market trajectories, host-society beliefs, and the broader economic context in which migration takes place. In doing so, I show that the effects of migration cannot be understood solely through changes in labor supply and demand, but must also account for the role of perceptions that may condition both individual outcomes and aggregate responses.

To examine these mechanisms, the dissertation brings together three empirical papers that study complementary dimensions of the integration process. The first chapter focuses on migrants' economic incorporation, studying how access to legal work authorization shapes labor market outcomes. Using survey data that track migrants' labor market status before leaving Venezuela and after settling in Colombia, and combining a difference-in-differences framework with reweighting methods to improve the comparability of treatment and control groups, the paper shows that work permits strengthen labor market attachment, particularly among migrants with weaker pre-migration ties to employment, but have more limited effects on occupational upgrading and job quality. These findings highlight that legal access to work facilitates entry into the labor market but does not fully resolve frictions related to skill transferability and occupational sorting.

The second chapter shifts the focus to the social environment in which economic integration takes place. It focuses on measuring a socially sensitive dimension of host-society responses: the belief that migrants increase insecurity. To do so, it implements a survey experiment in Cali, Colombia, using a list experiment to elicit sensitive attitudes indirectly. By asking respondents to report only how many statements they agree with, rather than whether they agree with the sensitive statement itself, the design increases respondent anonymity and reduces misreporting due to social desirability bias. The results indicate that these beliefs are widespread, prevalent among nearly two-thirds of respondents, and closely associated with broader insecurity-related attitudes toward migrants. By providing a direct measure of these beliefs, the paper highlights that perceptions constitute an important dimension of the environment in which migrants integrate, even in contexts where administrative data do not support a strong link between migration and crime.

The third chapter extends the analysis to the aggregate consequences of migration. It examines how the Venezuelan inflow affected labor market outcomes for locals and earlier migrants,

and how these effects evolved in the presence of a large macroeconomic shock such as the COVID-19 pandemic. Using repeated cross-sectional labor market data and exploiting variation in migrant exposure across local labor markets, the paper shows that the effects of migration are shaped by the broader economic context, with impacts on employment, wages, and informality that change over time and intensify during periods of economic disruption.

This dissertation contributes to several strands of the literature. First, it provides new evidence on the labor market effects of forced migration in developing countries using novel and rich microdata that capture migrants' pre- and post-migration labor market trajectories. Second, it contributes to the literature on migration and social perceptions by directly measuring sensitive beliefs using an experimental approach. Third, it shows that the effects of migration depend on the macroeconomic environment, highlighting the role of large shocks in shaping adjustment dynamics. Taken together, these contributions provide an integrated framework to study migrant integration in high-informality settings.

Therefore, my findings highlight three main insights with direct policy implications: first, legal access to work facilitates labor market participation but does not ensure efficient job matching, suggesting that regularization policies should be complemented with interventions that address skill transferability, credential recognition, and access to higher-quality employment. Second, beliefs about migrants are widespread and form part of the environment in which integration takes place, underscoring the importance of the informational context and of policies aimed at reducing the gap between perceptions and administrative evidence. Third, the effects of migration depend on macroeconomic conditions and vary over time, with recently arrived migrants showing greater vulnerability during adverse shocks. Policy responses should therefore prioritize support for newly arrived migrants during economic downturns, for example through targeted employment assistance, income support, and integration programs that help accelerate labor market attachment and reduce the risk of persistent disadvantages.

Overall, the dissertation suggests that migrant integration should be understood as a dynamic process shaped by multiple interacting constraints, rather than as a single equilibrium outcome. Policies that focus on only one margin—such as legal status—may therefore have limited effects if other constraints remain binding. An important avenue for future research is to assess whether complementary policies targeting skill accumulation and transferability can improve occupational mobility and job quality over time.

Chapter 1

From exodus to employment: Labor market transitions and the role of work permits in Colombia

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Abstract

How do work permits shape labor market transitions among forced migrants in economies with pervasive informality? Using unique data that track Venezuelan migrants' labor market outcomes both before migration and after settlement in Colombia, this paper provides causal evidence on how legal work authorization affects migrants' labor market trajectories. The estimates indicate that access to a work permit is associated with substantial increases in labor-market participation and sustained employment, with the largest effects among migrants who were weakly attached to the labor market prior to migrating. In contrast, the estimates provide limited evidence that work permits improve occupational upgrading and the task content of jobs along the migration trajectory. These findings suggest that legal work authorization relaxes constraints on entry and participation while leaving frictions in skill transferability and occupational matching largely intact. More broadly, the results highlight that work permits can alter labor market trajectories at the extensive margin without necessarily improving the allocation of migrant human capital in segmented and informal labor markets.

Keywords: Migration, occupational downgrading, labor mobility, work permits.

JEL Codes: F22, J61, J24, J21.

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1.1 Introduction

Venezuela’s economic and political collapse has generated one of the largest migration flows in recent Latin American history. More than five million Venezuelans have relocated primarily to neighboring countries, with Colombia as the main destination (ACNUR, 2025; National Planning Department, 2022; Wolfe, 2021a). This inflow constitutes a major migration shock with first-order implications for labor-market outcomes, productivity, and the design of immigration policy. For migrants, a central margin of integration is economic incorporation into the host-country labor market. In Colombia, where informality is pervasive, employment-based indicators provide an informative but incomplete picture of migrant integration. A complementary perspective focuses on the quality of labor-market matching, namely whether migrants can access occupations consistent with their skills or whether they experience persistent occupational downgrading into lower-status jobs.

Such downgrading is relevant not only for migrant welfare, but also because it may reflect broader efficiency losses in the allocation of labor. To the extent that migrants face frictions preventing access to occupations commensurate with their skills, human capital is not employed in its most productive use, with potentially adverse effects on aggregate productivity in the short run (Akresh, 2008; Chiswick & Miller, 2009; Crollard, de Castro, & Tsai, 2012; Simón, Ramos, & Sanromá, 2014). This interpretation is consistent with recent work for Colombia showing that immigrant-specific frictions, including discrimination and barriers to occupational choice, can generate misallocation of labor across occupations and reduce aggregate productivity. In that setting, eliminating such frictions would increase Colombian productivity by 0.9 percent and induce roughly one-third of migrants to reallocate across occupations (Pulido & Varón, 2020).

Venezuelan migrants face substantial barriers to achieving skill-appropriate employment. These include limited recognition of academic and professional credentials, discrimination, and weak labor networks, all of which constrain access to formal jobs and higher-quality matches (Danzer & Dietz, 2014; Liu et al., 2019; Męczyński, 2016). In response to the massive migration, the Colombian government introduced work permits and broader regularization policies to facilitate economic integration.¹ The central program, the Permiso Especial de Permanencia (PEP), granted temporary legal status, formal work authorization, and access to public services. The PEP was issued in nine waves through March 2021 and reached more than 724,000 migrants, making it one of the largest regularization programs in the region and a natural setting to study how legal status shapes labor-market transitions in a high-informality context.

This paper examines whether access to legal status through the PEP improves Venezuelan migrants’ labor-market integration in Colombia. Beyond facilitating labor-force participation and employment, legal status may affect the quality of migrants’ labor-market matches by relaxing constraints that limit access to formal and skill-appropriate jobs. We estimate the causal effect of the permit on employment, unemployment, and labor-force participation, as

¹In the extant literature, this policy measure is also referred to interchangeably as an Amnesty Program for Undocumented Migrants, a Government Regularization Program, or Work Permits.

well as on migrants' occupational placement. We measure occupational upgrading using the probability of working in white-collar occupations. To assess whether legalization also alters the nature of work migrants perform, we examine changes in job task content using the Routine Task Intensity (RTI) index proposed by [Mihaylov and Tijdens \(2019\)](#), which distinguishes routine from non-routine tasks and captures the cognitive complexity of migrants' post-migration jobs.

Our analysis contributes to three strands of literature. First, we add to the evidence on legalization and regularization programs and their effects on immigrants' labor-market outcomes, including employment and labor-force participation ([Amuedo-Dorantes & De la Rica, 2007](#); [Bahar et al., 2021](#); [Monras, Vázquez-Grenno, & Elias Moreno, 2018](#)). We provide new evidence from a setting characterized by high informality and structural labor-market segmentation, where the role of legal status for labor-market attachment may differ from more formal labor markets.

Second, we contribute to the literature on immigrants' occupational downgrading and the determinants of skill utilization in host economies ([Akresh, 2008](#); [Chiswick & Miller, 2009](#); [Crollard et al., 2012](#); [Simón et al., 2014](#)). A key limitation in much of this work is that data often observe migrant labor trajectories only after arrival, making it difficult to measure changes relative to pre-migration occupations. By using data that record migrants' labor-market status and occupation both before leaving Venezuela and after settling in Colombia, we can directly assess whether access to legal status mitigates occupational downgrading and improves occupational sorting. Third, we connect these questions to a growing literature that measures job quality through task content and occupational complexity ([Autor & Dorn, 2013](#); [Autor, Levy, & Murnane, 2003](#); [Foged & Peri, 2016](#); [Lewandowski, 2025](#); [Peri & Sparber, 2009](#)). Using the Routine Task Intensity index, we examine whether legalization shifts migrants toward less routine and more cognitively intensive jobs.

Despite this growing body of evidence, it remains unclear whether legal status alone can reduce occupational downgrading or improve job quality in host labor markets characterized by high informality and strong segmentation. A key challenge is data availability. Most datasets observe migrants only after arrival, which limits researchers' ability to compare pre- and post-migration occupational trajectories for the same individuals. This paper addresses that limitation by leveraging data that record migrants' labor-market outcomes and occupations both before migration from Venezuela and after migration to Colombia. This feature allows us to directly study whether access to the PEP shapes occupational trajectories and the task content of jobs migrants perform.

We use the Encuesta Pulso de la Migración (EPM), collected by the National Administrative Department of Statistics (DANE, by its Spanish acronym) in partnership with Universidad del Rosario and the World Bank. The EPM provides retrospective information on migrants' labor-market status and occupation prior to migration, along with detailed outcomes in Colombia, including PEP status and timing. The survey spans four waves and covers 12,510 migrants (5,759 households), enabling the reconstruction of pre- and post-migration labor histories. Our empirical approach combines a difference-in-differences design with reweighting to address differences in observables between migrants who obtain the PEP and those who do not. Specifically, we

estimate difference-in-differences models comparing changes in outcomes from the pre-migration period to the post-migration period across treated and untreated migrants, controlling for individual characteristics and department fixed effects. Because permit uptake is not random, we complement this approach with inverse probability weighting to improve balance on observables, following recent work on robust DiD estimators (Sant’Anna & Zhao, 2020; Wei, Epland, & Liu, 2023).

We find that the PEP primarily strengthens migrants’ attachment to the labor market. In the full sample, legalization reduces the probability of being out of the labor force in Colombia by 11.9 percentage points, with effects concentrated among migrants who arrive with weaker baseline attachment, particularly those who were unemployed prior to migration. Heterogeneity analyses show that younger and less educated migrants exhibit the clearest improvements in labor-market participation. In contrast, legalization generates limited gains in access to white-collar employment and does little to curb occupational downgrading among migrants who were employed prior to migration. These findings point to persistent barriers to skill-based mobility in a high-informality labor market, even after legal work authorization is granted.

This paper contributes by using unique pre- and post-migration labor histories to distinguish between extensive-margin integration and improvements in job quality. The results suggest that work authorization can prevent labor-force detachment, but is not sufficient to avert occupational downgrading. The policy implication is that regularization programs may be effective at sustaining participation, but may need to be complemented with measures that reduce barriers to skill utilization, such as credential recognition, job-matching support, and interventions that expand access to higher-quality employment as it has been done in other countries (Brücker, Glitz, Lerche, & Romiti, 2021; Pecoraro & Tani, 2023; Pecoraro & Wanner, 2019), in order to fully harness migrant human capital in high-informality settings.

The remainder of the paper is organized as follows. Section 1.2 describes the institutional context of Venezuelan migration and the PEP. Section 1.3 presents the data and outcome measures. Section 1.4 details the empirical strategy. Section 1.5 reports the main results and heterogeneity patterns. Section 1.7 concludes.

1.2 Background and institutional context

Since 2014, Venezuela has experienced a severe economic and institutional collapse, characterized by a sharp contraction in output, accelerating inflation, and persistent shortages of essential goods (Alhadeff, 2021; Caruso et al., 2021; Lombardo, Martínez Correa, Peñaloza-Pacheco, & Gasparini, 2021; OECD, 2018; Ramoni Perazzi, Orlandoni Merli, Torres Rivas, & Zambrano, 2017). This crisis triggered an unprecedented displacement episode in Latin America. Between 2015 and 2019, approximately 5 million Venezuelans migrated to neighboring countries, with Colombia emerging as the primary destination in the region (Inter-American Development Bank & OECD, 2021; Wolfe, 2021a).

Colombia is a particularly informative setting to study migrant labor-market integration because it combines relatively low barriers to cultural and linguistic assimilation with a labor market structured around high informality. Geographic proximity and long-standing cross-border ties, including a shared language and similar cultural traditions, likely facilitate basic economic participation (Brill, 2017; Orozco & Campos, 2015). However, the Colombian labor market is characterized by segmented access to formal employment, which implies that employment gains do not necessarily translate into improvements in occupational status or job-task content.

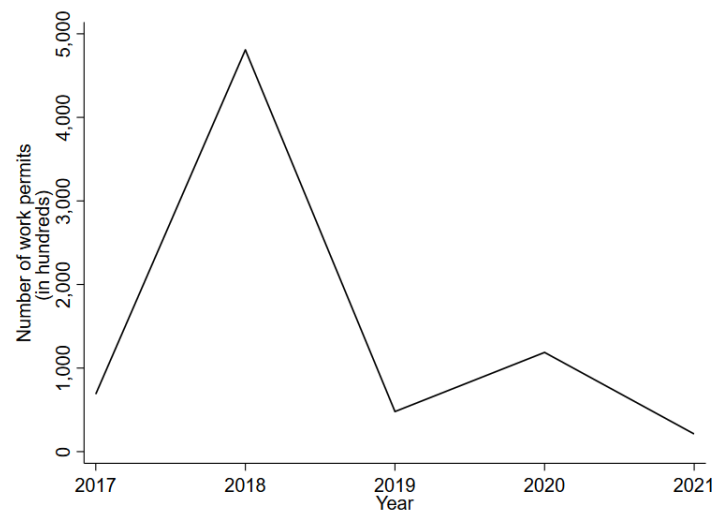
For the origin country, Venezuela, estimates suggest that a substantial portion of the labor force was also engaged in informal employment, even before mass migration outflows: several studies indicate that over 40% of employed individuals were in the informal sector (Ramoni, 2016). This high rate of informality in the origin country likely affects the human-capital and occupational structure of migrants, conditioning their labor-market insertion in the host country.

The Venezuela–Colombia comparison is particularly informative from a human-capital perspective. The two countries exhibit similar education-system architectures and broadly comparable curricula, and UNESCO and World Bank indicators suggest only modest differences in key outcomes such as literacy and completion rates (Bank, 2019; UNESCO, 2020). Under standard models in which schooling and experience are portable, migrants with comparable credentials should, on average, access occupations of similar status upon arrival. However, Colombian evidence departs from this benchmark. Using nationally representative survey data and an ORU models, Mora, Castillo Caicedo, and Gómez (2022) document substantial educational mismatch among Venezuelan migrants in Colombia, with systematic differences across arrival cohorts: earlier arrivals exhibit higher overeducation, while more recent arrivals are more likely to be undereducated.

To address the integration challenges associated with the migration surge, Colombia introduced a sequence of regularization programs. The Permiso Especial de Permanencia (PEP) was created in July 2017 as a temporary residence permit valid for two years that granted legal status, formal work authorization, and access to public services in health and education. Eligibility required that applicants did not hold a visa issued by the Ministry of Foreign Affairs, had no national or international criminal records, were not subject to expulsion or deportation measures, and had not previously had a PEP revoked. These requirements are central for empirical design because they imply that access to legal status is not randomly assigned and may be correlated with migrants' resources, information, and administrative capacity.

The PEP was issued in nine waves between July 2017 and March 2021, ultimately benefiting 724,083 migrants. Figure 1.1 documents the annual flow of permits granted between 2017 and 2019 and shows a pronounced issuance spike in 2018. This peak is consistent with expanded registration efforts and the relaxation of administrative constraints that increased access to the program.

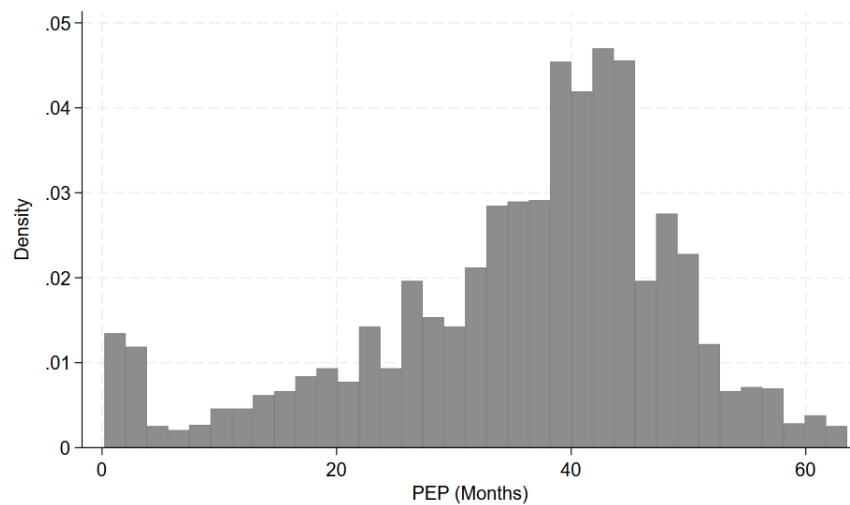
Figure 1.1: Total granted work permits in Colombia (2017–2021)



Notes: Data available until May 2021. Source: Own calculations, using the information provided by Migración Colombia.

Figure 1.2 complements this institutional evidence by describing the distribution of time since first PEP issuance among migrants in the EPM data. Most migrants with a valid PEP at the time of the survey report having held it for approximately 3 to 4 years (around 40 months), with a smaller mass of more recent issuances below 10 months. This distribution is consistent with the issuance spike observed in Figure 1.1 and provides a direct link between administrative policy timing and the survey-based measurement of treatment status. It also implies that, for most treated individuals in our sample, the PEP reflects medium-term legal status rather than very recent regularization, which has two implications. First, potential labor-market effects may reflect both access to formal work authorization and cumulative exposure to legal status. Second, the limited number of newly treated migrants suggests that short-run estimates may be less precisely identified than medium-run effects.

Figure 1.2: Time since WP issuance among venezuelan migrants in Colombia



Source: Calculations by the author using EPM. It uses the question about when the respondent first obtained their PEP. We only use information from those who had a valid PEP at the time of the survey, ensuring the continuity of the permit.

It is important to clarify that this study focuses on the type of permit that was in force during the period of analysis, namely the PEP, rather than the new regularization status introduced in early 2021, the Permiso por Protección Temporal (PPT), which was designed as a broader and more permanent framework.²

This institutional setting motivates an empirical strategy that compares labor-market trajectories before and after migration across migrants who obtain legal work authorization and those who do not. The multi-wave rollout of the PEP provides variation in access to legal status, while Figures 1.1 and 1.2 show that this variation is concentrated in specific issuance periods and implies heterogeneous treatment duration. At the same time, eligibility and take-up are potentially selective, which makes cross-sectional comparisons uninformative about causal effects. The structure of the EPM, which records labor outcomes both in Venezuela and in Colombia for the same individuals and identifies PEP status and timing, naturally supports a difference-in-differences framework combined with reweighting to address selection on observables.

1.3 Data and descriptive evidence

Our primary data source is the Encuesta Pulso de la Migración (EPM), a migrant-specific panel survey that provides unusually detailed labor-market information on Venezuelan migrants

²The PEP primarily targeted migrants who entered through regular channels, while the PPT expanded coverage to a larger population, including migrants who were not eligible for the PEP. Our analysis focuses on PEP holders because the EPM waves used in this paper span 2021 to early 2022, a period in which the PPT had only recently been implemented and is underrepresented in the sample. Consequently, our estimates capture labor-market outcomes associated with PEP status rather than the broader effects of the later PPT program.

in Colombia, including retrospective outcomes prior to migration. Collected by DANE and designed in collaboration with Universidad del Rosario and the World Bank, the EPM is especially well suited to studying labor-market integration because it records migrants' labor-market conditions both before migration, in Venezuela, and after migration, in Colombia. This structure allows us to trace occupational trajectories and transitions, rather than relying solely on cross-sectional comparisons observed in the host country.

The EPM targets households with at least one Venezuelan migrant. Each wave includes approximately 4,000 households, drawn from the Gran Encuesta Integrada de Hogares (GEIH) sampling frame. The survey covers Venezuelan migrants aged 15 or older, and the analysis excludes migrants born in Colombia and those with dual nationality, since these individuals have access to Colombian citizenship and therefore do not face the same legal constraints as other Venezuelan migrants.

The survey was implemented in four rounds. The pooled panel includes 12,510 migrants from 5,759 households. The first round draws from GEIH respondents interviewed between January and June 2021.³ To compensate for panel attrition and preserve sample size over time, subsequent rounds incorporated refresher households with demographic characteristics similar to those of the original sample. The second round draws on households interviewed between June and August 2021, the third on households interviewed between August and October 2021, and the fourth on households interviewed in November and December 2021. DANE reports attrition rates of 18.5%, 33.9%, and 47.3% in rounds 2, 3, and 4, respectively, and only 3,662 individuals completed all four survey rounds.

Our identification strategy relies on within-individual comparisons across migration. We therefore restrict attention to migrants for whom the EPM records labor-market outcomes both prior to migration in Venezuela and after migration in Colombia, together with information on PEP status and covariates. Since the module collecting pre-migration outcomes was introduced in Round 2, the resulting estimation sample includes approximately 4,300 migrants, which provides roughly 8,600 observations for the difference-in-differences design.

The EPM records whether a respondent holds a PEP and the month and year in which the permit was issued. The survey also contains the information needed to construct labor-market outcomes in both Venezuela and Colombia. We focus on two classes of outcomes. The first captures labor-market status. For Venezuela, we use the main activity performed for most of the time before migrating. For Colombia, we use labor-market status in the seven days prior to the interview. In both contexts, individuals are categorized as employed, unemployed, or out of the labor force.⁴

The second class of outcomes captures job quality and occupational allocation. We use

³The EPM follows the GEIH sampling frame, which is based on the 2018 Population and Housing Census. Since the Census was conducted during an early and highly mobile phase of the Venezuelan migration wave, the frame may reflect the settlement patterns observed at that stage rather than migrants' final locations in Colombia. This should be taken into account when interpreting the geographic composition of the sample.

⁴We use the response from the most recent round in which the respondent participated to ensure the most up-to-date information.

three complementary measures: an indicator for white-collar employment, a proxy for formal employment, and a task-based measure of job content. To construct the white-collar indicator, we map the occupation codes reported in the EPM into the one-digit ISCO-88 classification⁵ and define white-collar employment as ISCO categories 1 through 5. Since the EPM does not include a standard measure of informality, we use the presence of a written contract as a proxy for formal employment.

On the other side, to capture the task content of jobs, we link occupations to the Routine Task Intensity (RTI) index developed by [Mihaylov and Tijdens \(2019\)](#). The index is based on ISCO-08 occupations and measures the extent to which jobs involve tasks that are routine, codifiable, and therefore more susceptible to automation. It distinguishes between routine and non-routine tasks, with additional cognitive and manual components, allowing us to characterize the complexity, autonomy, and skill content of each occupation. In this framework, higher RTI values indicate more routine jobs, while lower RTI values are associated with occupations involving greater non-routine cognitive or manual content. Further details are provided in [Section 1.4](#).

The results indicate that migrants who held blue-collar jobs in Venezuela are the group most likely to remain in the same occupational category after arrival in Colombia, with 56.5 % staying in blue-collar employment. By contrast, only 40.5 % of those who held white-collar jobs in Venezuela remain in white-collar employment after migration. For this group, downgrading is pervasive. Approximately 59.5 % transition out of white-collar employment, with 26.0 % moving into blue-collar jobs, 11.4 % becoming unemployed, and 22.1 % exiting the labor force. These patterns suggest that migration is associated with a large decline in occupational status among previously high-skilled workers, which motivates the paper's focus on downgrading rather than employment alone.

The [Table 1.1](#) also shows meaningful mobility from lower initial states. Among migrants who were blue-collar workers in Venezuela, 17.6 % move into white-collar employment in Colombia, while 26 % transition into unemployment or nonparticipation. Among migrants who were unemployed in Venezuela, 56 % enter employment in Colombia, either white-collar or blue-collar, while 17.4 % become out of the labor force. Finally, among migrants who were out of the labor force in Venezuela, 54.3 % remain out of the labor force after migration, but a sizable share improves their status, with 19.1 % entering white-collar employment and 14.5 % entering blue-collar employment. Taken together, this table highlights that labor-market integration involves both employment transitions and occupational sorting, and it underscores that downgrading among previously white-collar migrants is a central empirical fact in this setting.

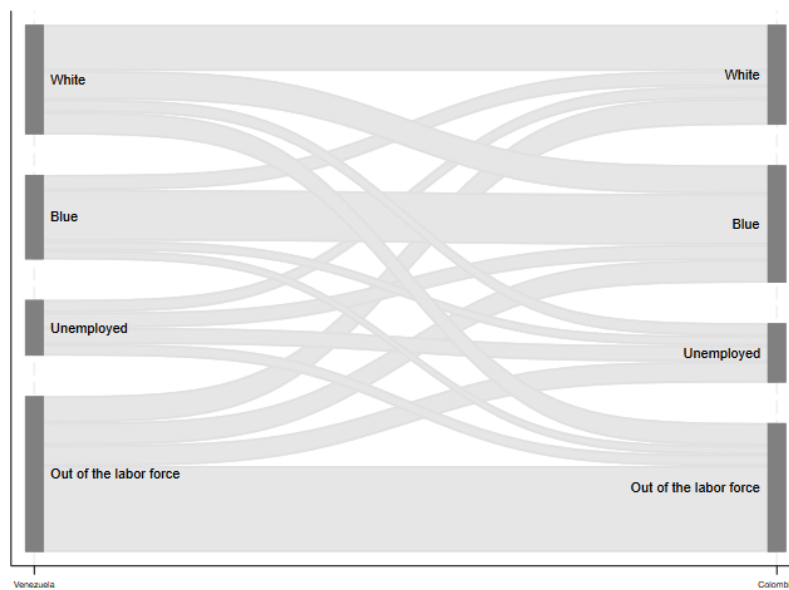
⁵Armed forces are excluded. Detailed information on the ISCO-88 classification can be found at [ILO \(2021\)](#). ISCO-88 considers the following 10 groups: (1) Legislators, senior officials, and managers; (2) Professionals; (3) Technicians and associate professionals; (4) Clerks; (5) Service workers and shop and market sales workers; (6) Skilled agricultural and fishery workers; (7) Craft and related trades workers; (8) Plant and machine operators and assemblers; (9) Elementary occupations; (10) Armed forces. The latter group is excluded in our analysis.

Table 1.1: Descriptive statistics of white and blue workers

Venezuela	Colombia				Total
	White-collar job	Blue-collar job	Unemployed	OLF	
White-collar job	40.5%	26.0%	11.4%	22.1%	100%
Blue-collar job	17.6%	56.5%	11.9%	14.0%	100%
Unemployed	23.8%	32.5%	26.2%	17.4%	100%
OLF	19.1%	14.5%	12.1%	54.3%	100%

Notes: The table uses data from Round 2, which contains the largest number of observations reporting pre-migration labor status in Venezuela and therefore maximizes follow-up comparability.

Figure 1.3: Change in occupational status from Venezuela to Colombia



Notes: The left axis reports the occupation in which the migrant worked most of the time in Venezuela, and the right axis reports the occupation of the current job in Colombia. Pre-migration occupation is measured using Round 2 responses, which have the lowest missingness. *Source:* Authors' calculations using EPM data.

Appendix A.1 reports complementary evidence on labor-market transitions that motivates the focus of the paper on job quality, entry dynamics, and heterogeneity across population groups. Appendix Figure A.1.2 shows a shift toward verbal contracts in Colombia relative to Venezuela, consistent with a decline in job formality and job quality. Appendix Figure A.1.1 focuses on migrants' first employment match in Colombia and indicates that occupational downgrading occurs at entry: 53 % of first jobs are blue-collar, and 41 % of migrants who were either unemployed or white-collar workers in Venezuela enter Colombia through blue-collar employment. Appendix Figures A.1.3–A.1.8 further document transitions by gender and age among permit holders, both across migration and around permit acquisition. Conditioning on permit status, downgrading remains common for both genders, but adjustment differs by gender: men's transitions out of pre-migration white-collar employment are more concentrated

in reallocations toward blue-collar work, whereas women exhibit a higher propensity to exit the labor force from pre-migration white-collar status.

Table 1.2: Descriptive statistics of white and blue workers

	White-collar job in Venezuela			Blue-collar job in Venezuela		
	PEP	Non-PEP	Diff.	PEP	Non-PEP	Diff.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Outcomes in Colombia						
Employed [=1] [†]	73.8	65.2	8.6***	82.7	74.2	8.5***
White-collar [=1] [†]	59.3	62.7	-3.4	27.2	22.5	4.6
Formal job [=1] [†]	5.8	3.5	2.3	6.5	3.1	3.4
Unemployed [=1] [†]	10.2	11.7	-1.6	8.9	12.1	-3.2
Out of the labor force [=1] [†]	16.0	23.1	-7.1***	8.4	13.8	-5.3**
Hourly wage [†]	4.82	3.60	1.22***	4.34	3.63	0.71***
Average RTI	-0.579	-0.646	0.067	-0.582	-0.582	0.000
Routine Cognitive	0.098	0.100	-0.002	0.158	0.165	-0.008
Routine Manual	0.112	0.077	0.036*	0.051	0.044	0.008
Non-Routine Analytic	0.028	0.037	-0.009	0.098	0.064	0.035**
Non-Routine Interactive	0.082	0.093	-0.011	0.139	0.168	-0.028**
Non-Routine Manual	0.677	0.692	-0.015	0.564	0.566	-0.001
Panel B: Sociodemographic characteristics						
Female [=1]	55.9	58.2	-2.2	23.8	28.2	-4.3
Age	35.6	36.7	-1.1*	37.0	38.4	-1.4
Married or cohabiting [=1]	70.5	70.5	0.0	74.3	71.2	3.1
High education [=1]	46.9	26.0	20.9***	15.9	10.7	5.2*
Urban [=1]	97.1	95.9	1.2	95.8	92.2	3.6*
Years in Colombia	3.8	3.7	0.1	3.7	3.6	0.1
Migrated because of family reasons [=1]	10.1	12.2	-2.1	7.9	8.7	-0.8
Difficulty finding a job [=1]	65.4	69.1	-3.7	71.1	67.4	3.7
Job training [=1]	6.8	1.5	5.3***	5.2	0.8	4.4***
N	413	1,303	-890	214	1,076	-862
% N	24.07	75.93	-51.86	16.59	83.41	-66.82

Notes: Data are from EPM Wave 2. [†] Hourly wages are in USD, using the average monthly TRM for the survey months. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. Source: Authors' calculations based on EPM data.

Results indicate that across both groups, PEP holders exhibit higher employment rates in Colombia. Among migrants who were white-collar workers in Venezuela, the employment rate is 73.8 % for PEP holders compared to 65.2 % for non-holders. Among those who were blue-collar workers in Venezuela, employment is 82.7 % for PEP holders versus 74.2 % for non-holders. Labor-force participation also differs by permit status. The probability of being out of the labor force is lower among PEP holders in both groups, from 23.1 to 16.0 % for the previously

white-collar group and from 13.8 to 8.4 % for the previously blue-collar group. Hourly wages in Colombia are higher among PEP holders in both groups, rising from 3.60 to 4.82 USD for those previously white-collar and from 3.63 to 4.34 USD for those previously blue-collar.

In contrast, differences in occupational allocation and job formality in Colombia are not statistically distinguishable across legal status. Within the previously white-collar group, the share employed in white-collar jobs is 62.7 % for non-holders and 59.3 % for PEP holders, and within the previously blue-collar group it is 22.5 versus 27.2 %; neither difference is statistically significant. Similarly, differences in the probability of holding a formal job, proxied by a written contract, and differences in unemployment are not statistically significant in either stratum.

Results also show RTI index and its components that will be details in Section 1.4. Mean differences in the overall RTI index are not statistically significant. Some task components differ by legal status. Among migrants who were white-collar in Venezuela, the routine manual task share is higher for PEP holders (0.112 versus 0.077, $p < 0.10$). Among migrants who were blue-collar in Venezuela, non-routine analytic tasks are higher for PEP holders (0.098 versus 0.064, $p < 0.05$), while non-routine interactive tasks are lower (0.139 versus 0.168, $p < 0.05$).

The Panel B also documents differences in observables consistent with selective take-up. PEP holders have higher education and job-related training in both groups. For migrants previously white-collar, the share with high education is 46.9 % among PEP holders versus 26.0 % among non-holders, and job training is 6.8 versus 1.5 %. For migrants previously blue-collar, high education is 15.9 versus 10.7 % and job training is 5.2 versus 0.8 %. Differences in gender, marital status, and years in Colombia are not statistically significant.

Among those who remained in blue-collar jobs, only 10% have an education level beyond high school. In contrast, among those who experienced an upgrading from blue-collar to white-collar jobs, the proportion of individuals with education beyond high school rises to 16%. This higher educational attainment among those who transitioned to white-collar occupations may indicate that education plays a crucial role in facilitating upward mobility in the labor market.

Table 1.3 shows that Venezuelan migrants who were blue-collar in Venezuela sort into Colombia's labor market in a highly concentrated manner across a small set of industries, with a clear predominance of the tertiary sector, particularly trade and services. Notably, wholesale and retail trade is the main sector both for migrants who remain in blue-collar jobs (19.13%) and for those who upgrade into white-collar jobs (19.01%), suggesting that occupational mobility often occurs within the same sector, through reassignment toward higher-status tasks and positions.

At the same time, upgrading is accompanied by a non-trivial sectoral reallocation: migrants who transition into white-collar employment are disproportionately represented in tertiary industries with greater scope for occupational progression—such as transport and logistics (17.14% vs. 13.62%), domestic services (8.55% vs. 5.22%), information and communications (2.87% vs. 1.77%), and public-oriented activities including public administration, health, and education. Taken together, this pattern highlights the central role and internal heterogeneity of the tertiary

sector in migrants' labor market integration, motivating empirical specifications that allow the effects of the PEP to vary by sector. In particular, estimating differential impacts for workers with exposure to services is crucial to assess whether regularization translates into stronger upgrading and formalization in segments where documentation is more binding and mobility opportunities seems to be larger.

Table 1.3: Sector distribution of workers in Venezuela who remained in blue-collar jobs and those who upgraded from blue-collar to white-collar jobs in Colombia

Sector	Remained in blue-collar job	Upgraded to white-collar job
Wholesale and retail trade	19.13	19.01
Transportation and storage	13.62	17.14
Manufacturing industries	17.87	14.94
Construction	20.94	12.37
Accommodation and food services	7.73	7.27
Household activities	5.22	8.55
Administrative services activities	2.71	3.38
Agriculture, livestock, hunting, forestry	9.79	2.43
Information and communications	1.77	2.87
Public administration and defense; planning	0.39	2.55
Health care activities	0.53	1.91
Education	0.62	1.91
Artistic and entertainment activities	0.29	1.15
Real estate activities	0.35	1.15
Mining and quarrying	0.82	0.64
Water distribution; wastewater and waste treatment	0.45	0.64
Electricity, gas, steam supply	1.64	0.64
Other service activities	2.63	0.90
N	4,862	1,568

Notes: *Source:* Authors' calculations using EPM.

1.4 Empirical Strategy

To quantify the causal effect of legal work authorization on Venezuelan migrants' labor-market integration, we exploit a key feature of the EPM: it records each individual's main labor-market status and occupation prior to migration in Venezuela and their contemporaneous outcomes after migration in Colombia. This structure enables a within-individual comparison across origin and destination that is not feasible in conventional host-country cross-sections. We implement a canonical difference-in-differences design that compares the change in outcomes for migrants who hold a work permit (PEP) relative to those who do not.

We estimate the effect of legal work authorization using a difference-in-differences specification that contrasts outcomes before migration (in Venezuela) and after migration (in Colombia)

for migrants with and without a PEP. The baseline outcome equation is

$$Y_{idst} = \alpha + \beta_1 (PEP_i \times Post_t) + \beta_2 PEP_i + \beta_3 Post_t + \gamma_d + \eta_s + \phi_k + \delta' X_{idst} + \varepsilon_{idst}, \quad (1.1)$$

where i indexes individuals, d denotes the Colombian department of residence, s denotes the Venezuelan state of origin, k denotes the sector of employment, and t indicates whether the observation refers to the pre-migration period in Venezuela ($Post_t = 0$) or the post-migration period in Colombia ($Post_t = 1$). The vector X_{idst} includes predetermined individual characteristics: indicators for being female, older than 34, having completed at least high school, and residing in an urban area; the log of months in Colombia; an indicator for migrating for family reasons; the state of origin; and the sector of employment in Colombia. The coefficient of interest, β_1 , captures the impact of legal work authorization on the change in outcomes across migration.

Because PEP take-up is not randomly assigned, we address selection on observables by implementing inverse probability of treatment weighting (IPTW) based on the estimated propensity score. This IPTW-DiD approach follows recent weighting-based estimators that combine reweighting and differencing to improve robustness to selection into treatment and time-invariant unobserved heterogeneity (Sant'Anna & Zhao, 2020; Wei et al., 2023).

Then, let $e(X_i) = \Pr(PEP_i = 1 \mid X_i)$ denote the propensity score, estimated in a first step as a function of the covariates in X_i .

$$w_i = \begin{cases} 1, & \text{if } PEP_i = 1, \\ \frac{\hat{e}(X_i)}{1 - \hat{e}(X_i)}, & \text{if } PEP_i = 0, \end{cases} \quad (1.2)$$

which reweight non-PEP migrants so that, in expectation, the distribution of observables matches that of PEP holders. The DiD parameters are then obtained by weighted least squares, namely

$$\hat{\theta} = \arg \min_{\theta} \sum_{i,t} w_i (Y_{idst} - \alpha - \beta_1 (PEP_i \times Post_t) - \beta_2 PEP_i - \beta_3 Post_t - \gamma_d - \eta_s - \phi_k - \delta' X_{idst})^2, \quad (1.3)$$

where $\theta = (\alpha, \beta_1, \beta_2, \beta_3, \gamma_d, \eta_s, \phi_k, \delta)$.

We assess the credibility of the weighting strategy by examining overlap and covariate balance. Specifically, we verify common support in the propensity score distributions for treated and control individuals and evaluate balance using standardized mean differences before and after weighting. Inference is conducted using procedures appropriate for settings with few clusters. Since the number of clusters is limited because of the 23 cities the EPM is implemented,

we report wild cluster bootstrap p-values clustered by department, following [Cameron, Gelbach, and Miller \(2008\)](#) and [Roodman, Nielsen, MacKinnon, and Webb \(2019\)](#) and also used by [Bahar et al. \(2021\)](#).

IPTW strengthens the empirical design by reducing selection on observed characteristics, but it does not by itself make treatment assignment as good as random. In this setting, reweighting makes non-PEP migrants comparable to PEP holders in terms of observed pre-treatment covariates, while the difference-in-differences structure absorbs time-invariant unobserved heterogeneity. A causal interpretation therefore requires a conditional parallel-trends assumption: after conditioning and reweighting, untreated migrants must provide a valid counterfactual for the evolution of treated migrants in the absence of legal status. Since the data contain only one retrospective pre-migration observation, this assumption cannot be directly tested. The estimates should thus be interpreted as conditional difference-in-differences estimates, not as causal effects identified by IPW alone.

We therefore evaluate the credibility of the identification strategy using alternative diagnostics suitable for a two-period structure. Specifically, we examine covariate balance and common support in the propensity score distributions after IPTW, and we implement placebo and robustness exercises to assess whether the estimated effects could be driven by differential selection or compositional changes rather than by legal work authorization.

To characterize migrants' labor-market dynamics more directly, we therefore report estimates separately by pre-migration labor-market status and occupation in Venezuela. This allows us to assess whether migrants who were unemployed or out of the labor force prior to migration experience larger gains in employment once regularized, and whether work authorization mitigates occupational downgrading among those who held white-collar jobs in Venezuela. We further examine heterogeneity by gender and age, given that constraints and returns to regularization may differ across demographic groups. Taken together, this transition-based perspective clarifies that the impact of the PEP operates through reallocations across labor-market states and occupational categories, and it provides a disciplined way to identify the specific margins along which legal work authorization improves migrants' labor-market integration.

1.4.1 Job quality and task content

To complement occupational classifications based on broad collar categories, we examine whether legal work authorization affects the *task type* of migrants' jobs. Even within the same occupational category, job quality may differ substantially depending on the extent to which tasks are routine, non-routine, cognitive, or manual. We therefore use the *Routine Task Intensity* (RTI) index to assess whether legal access to work influences the task content of migrants' jobs.

We use RTI at the four-digit occupation level calculated by [Mihaylov and Tijdens \(2019\)](#). The RTI measure summarizes the balance between routine and non-routine tasks and is defined as in equation 1.4:

$$RTI_k = RC_k + RM_k - NRA_k - NRI_k - NRM_k, \quad (1.4)$$

where RC_k denotes routine cognitive task intensity, RM_k routine manual task intensity, and NRA_k , NRI_k , and NRM_k correspond to non-routine analytic, non-routine interactive, and non-routine manual tasks, respectively. Higher RTI values indicate a greater concentration of routine tasks and a lower intensity of non-routine tasks, and the index is normalized to lie in $[-1, 1]$, where values closer to -1 indicate less routine tasks, and values closer to 1 indicate more routine tasks.

We estimate the impact of work authorization on RTI using the same difference-in-differences specification described above, replacing the labor-market outcome with the RTI score associated with the individual's occupation (see equation 1.5).

$$RTI_{idst} = \alpha + \beta_1 (PEP_i \times Post_t) + \beta_2 PEP_i + \beta_3 Post_t + \gamma_d + \eta_s + \phi_k + \delta' X_{idst} + \varepsilon_{idst}. \quad (1.5)$$

As in the baseline analysis, we address selection into treatment by implementing inverse probability of treatment weighting (IPTW), and we estimate the model in the resulting weighted sample. This allows us to evaluate whether regularization shifts migrants toward occupations characterized by lower routinization and higher non-routine content.

Finally, because changes in the RTI index can arise from distinct margins, we estimate analogous specifications separately for each RTI component. This decomposition clarifies whether the effects of legal work authorization operate primarily through reductions in routine manual tasks, increases in non-routine analytic or interactive tasks, or other changes in task content. In this way, the RTI framework provides a disciplined characterization of how regularization affects not only the probability of employment and occupational upgrading, but also the underlying nature of work performed by migrants.

To aid interpretation of the RTI results, Appendix Table A.2.1 in Appendix A.2 summarizes the five task components underlying the index and provides illustrative examples. RTI increases with routine cognitive and routine manual task intensity and decreases with non-routine analytic, interactive, and manual tasks. Routine cognitive tasks capture rule-based clerical activities, while routine manual tasks reflect repetitive physical tasks. In contrast, the non-routine components capture problem-solving and analytical work, interpersonal and communication-intensive activities, and manual tasks requiring adaptability and creativity. In the empirical analysis, we estimate the effect of work authorization on the overall RTI index as well as on each component, which allows us to identify the specific task margins through which regularization affects job content.

1.4.2 Limitations

The main limitation of the empirical strategy is that parallel pre-trends cannot be directly assessed. The EPM provides retrospective information on migrants' labor-market outcomes before leaving Venezuela and post-migration outcomes in Colombia, but it does not contain multiple pre-treatment observations for the same individual. The identifying assumption is therefore that, conditional on observed characteristics and after reweighting, migrants without a work permit approximate the counterfactual evolution of migrants with a permit in the absence of legal status. The estimates should be interpreted as conditional difference-in-differences estimates, rather than as evidence from a design in which pre-treatment trends can be directly verified.

1.5 Main results

1.5.1 Baseline effects on employment and occupational outcomes

To assess whether legal access to work mediates migrants' labor-market integration, we estimate Equation 1.1 separately by migrants' pre-migration labor-market status in Venezuela. This approach allows us to evaluate whether the PEP affects post-migration outcomes primarily through facilitating employment entry, sustaining participation, or enabling occupational mobility.

Table 1.4 examines whether access to a work permit (PEP) alters migrants' labor-market outcomes in Colombia, conditional on their pre-migration occupational status in Venezuela. The estimates reveal highly concentrated effects: across most origin groups, the PEP does not produce statistically detectable changes in employment, occupational status, formality, or unemployment. Instead, the primary effect operates through labor-force attachment among migrants who were unemployed before migrating.

Among migrants who were unemployed in Venezuela (Panel C), holding the PEP reduces the probability of being out of the labor force in Colombia by 11.9 percentage points (column 6), and this is the only coefficient that is precisely estimated. This result is economically meaningful and directly speaks to the research question: rather than shifting migrants into better occupations or formal jobs, legal work access appears to facilitate integration mainly by preventing labor-market detachment among those who are most weakly attached at baseline. Importantly, for this group we do not observe a corresponding significant increase in employment or formality (columns 1 and 3), which suggests that the main margin affected by legalization is participation rather than job quality.

For migrants who were white-collar or blue-collar in Venezuela (Panels A and B), the coefficients are small and statistically indistinguishable from zero across outcomes, including transitions into white-collar work. This pattern indicates that regularization alone is unlikely to mitigate occupational downgrading or to improve skill-appropriate matching in the short run.

Similarly, migrants who were already out of the labor force in Venezuela (Panel D) show no detectable response to the PEP, consistent with persistent barriers to entry for individuals with low baseline labor attachment.

Overall, the evidence in Table 4 suggests that the mediating role of work permits is limited and asymmetric: legalization affects labor-market integration primarily by increasing labor-force attachment among migrants who were unemployed prior to migration, while leaving occupational mobility and formalization largely unchanged.

Table 1.4: Impact of work permits on labor market outcomes of migrants

	Employed (1)	White-collar job (2)	Formal (3)	Unemployed (4)	Out of the labor force (6)
Panel A. Migrants who were <i>white-collar</i> in Venezuela					
PEP x T	0.018 (0.021)	-0.031 (0.033)	-0.020 (0.037)	0.011 (0.015)	-0.029 (0.025)
Observations	2,813	2,197	2,139	2,813	2,813
R-squared	0.352	0.471	0.303	0.113	0.278
Panel B. Migrants who were <i>blue-collar</i> in Venezuela					
PEP x T	0.036 (0.040)	0.008 (0.050)	0.002 (0.046)	0.000 (0.027)	-0.036 (0.038)
Observations	2,070	1,701	1,644	2,070	2,070
R-squared	0.282	0.285	0.208	0.122	0.257
Panel C. Migrants who were unemployed in Venezuela					
PEP x T	0.151 (0.094)	-0.036 (0.066)	-0.031 (0.089)	-0.032 (0.049)	-0.119** (0.053)
Observations	937	699	651	937	937
R-squared	0.605	0.526	0.411	0.796	0.265
Panel D. Migrants who were out of the labor force in Venezuela					
PEP x T	0.025 (0.041)	0.000 (0.072)	-0.011 (0.073)	-0.018 (0.032)	-0.007 (0.030)
Observations	3,274	2,087	1,054	3,274	3,274
R-squared	0.446	0.684	0.315	0.101	0.490

Notes: The sample of these estimates corresponds to the migrants who had the occupational status indicated in the title. The estimates include the same controls described in Equation 1.1. Cluster-robust wild bootstrap p-values are used in these estimates to adjust for potential clustering effects. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. *Source:* Authors' calculations using EPM.

1.5.2 Heterogeneous effects

1.5.2.1 Across demographic and human-capital groups

To assess whether the effects of work permits are concentrated in specific segments of the migrant population, we estimate Equation 1.1 separately by gender, age, and education within each pre-migration labor-market status in Venezuela. This heterogeneity analysis speaks directly to the integration mechanism implied by regularization: if work permits relax constraints related to job search, hiring, or labor-force attachment, impacts should be strongest for groups facing higher baseline barriers (e.g., women, younger, and less-educated migrants), and weaker where structural frictions to occupational mobility remain binding.

Tables A.3.1 - A.3.4 show a clear pattern: the effects of the PEP are not broad-based, and are instead concentrated on labor-force attachment and employment among migrants who were weakly connected to the labor market prior to migration, particularly those who were unemployed in Venezuela. In contrast, we find little evidence that work permits systematically prevent occupational downgrading or induce upgrading into white-collar jobs.

Among migrants who held white-collar jobs in Venezuela (Table A.3.1), the estimates point to heterogeneous responses by gender and education. Notably, for men in this group, PEP access reduces the likelihood of formal employment by 10.6 percentage points, suggesting that regularization may reshape the composition of employment rather than improving job quality for higher-skilled male migrants. At the same time, among less-educated white-collar migrants, the PEP reduces non-participation by 5.2 percentage points, consistent with a participation margin effect for individuals facing greater entry barriers despite prior white-collar status.

Results for migrants who were blue-collar workers in Venezuela (Table A.3.2) highlight persistent gender disparities. For women, holding the PEP decreases the probability of obtaining a white-collar job in Colombia by 22.3 percentage points, indicating that legalization does not translate into upward occupational mobility for this group and may interact with labor-market segmentation in ways that limit access to higher-skilled positions. By contrast, younger blue-collar migrants experience employment gains: the PEP increases employment by 8.2 percentage points and reduces unemployment by 6.8 percentage points, pointing to improved labor-market entry for younger workers.

The most pronounced and policy-relevant heterogeneity emerges among migrants who were unemployed in Venezuela (Table A.3.3). In this group, PEP access increases formal employment for women by 14.2 percentage points, raises employment for men by 11.2 percentage points, and reduces men's probability of being out of the labor force by 3.2 percentage points. Effects are even larger for groups with lower human capital: among less-educated migrants, the PEP increases formal employment by 21.4 percentage points and reduces non-participation by 16.6 percentage points. Similarly, among younger migrants, legalization reduces non-participation by 17.8 percentage points. Together, these results indicate that regularization primarily improves outcomes by easing constraints to labor-market entry and sustained participation for migrants

who are marginally attached at baseline.

Finally, among migrants who were out of the labor force in Venezuela (Table A.3.4), we find little evidence of systematic effects. The only precisely estimated impact appears for men, for whom PEP access reduces unemployment by 8.5 percentage points, again consistent with limited effects concentrated on the extensive margin.

Overall, the heterogeneity results reinforce two main conclusions. First, regularization improves labor-market integration primarily through participation and employment entry, with the largest gains accruing to migrants who were unemployed prior to migration and to those with lower education. Second, the persistence of null (or adverse) effects on transitions into white-collar employment suggests that occupational mobility remains constrained even after legalization, consistent with binding frictions such as credential recognition, segmented labor demand, and limited transferability of origin-country experience. These patterns imply that while work permits can facilitate labor-market inclusion, complementary policies may be required to improve skill-appropriate matching and reduce human-capital misallocation.

In terms of external validity, our findings are consistent with evidence that legal status can strengthen attachment and expand access to formal opportunities through improved job-search and workplace channels (e.g., Saari, Leinonen, and Tapanila (2022); Schwerdt, Ichino, Ruf, Winter-Ebmer, and Zweimüller (2008)). They also complement prior evidence on the Venezuelan regularization context. While Bahar et al. (2021) emphasize limited aggregate effects on labor-force outcomes and highlight formalization responses, our results suggest that formal-employment gains are concentrated among specific high-barrier groups—particularly migrants who were unemployed pre-migration and those with lower education—rather than uniformly distributed across the migrant population.

1.5.2.2 Does duration of legal status matter?

The labor market consequences of regularization may unfold gradually. Gains from legal status can depend on complementary inputs, such as accumulated host-country experience, labor market networks, and familiarity with local institutions and job search technologies, that evolve over time (Chiswick, 1978; Chiswick & Miller, 2009; Dustmann & Fabbri, 2003; Kossoudji & Cobb-Clark, 2002). Motivated by this dynamic adjustment, Table 1.5 examines whether the duration of legal work authorization matters by redefining treatment as holding a work permit (PEP) for at least three years ($PEP_{3y} = 1$) and comparing these migrants to individuals who never held a permit ($PEP_{3y} = 0$). Migrants with more recent permits, defined as less than three years, are excluded from the estimation sample. The coefficient on $PEP_{3y} \times Post$ therefore captures the reduced-form effect of sustained exposure to legal work authorization.

The estimates suggest limited aggregate effects, with some evidence on the extensive margin. In the pooled results, prolonged permit holding is associated with a 5.6 percentage-point increase in employment. Effects on white-collar employment and formal employment are not statistically

different from zero, and we do not detect changes in unemployment or labor force participation. This pattern is consistent with regularization primarily facilitating access to employment rather than generating measurable job upgrading or formalization over the horizon studied, and it aligns with evidence of negligible short-run impacts of work permits (Bahar et al., 2021). The stronger employment response among migrants previously employed in white-collar occupations is consistent with skilled migrants facing larger initial adjustment costs and benefiting more from sustained access to legal work as they rebuild host-country-specific capital (Akresh, 2008; Chiswick & Miller, 2009; Kossoudji & Cobb-Clark, 2002).

Table 1.5: Impact of holding a work permit for more than 3 years on the labor market outcomes

Variables	Employed	White-collar job	Formal	Unemployed	Out of the labor force
	(1)	(2)	(3)	(4)	(5)
A. Migrants who were <i>white-collar</i> workers in Venezuela					
PEP3y $\times$ Post	0.056*	-0.035	0.036	-0.014	-0.042
	(0.028)	(0.043)	(0.051)	(0.019)	(0.030)
Observations	2,530	1,972	1,967	2,530	2,530
R-squared	0.323	0.500	0.294	0.125	0.250
B. Migrants who were <i>blue-collar</i> workers in Venezuela					
PEP3y $\times$ Post	-0.010	-0.004	0.027	0.037	-0.026
	(0.047)	(0.059)	(0.059)	(0.041)	(0.042)
Observations	1,959	1,604	1,569	1,959	1,959
R-squared	0.329	0.300	0.248	0.181	0.287
C. Migrants who were <i>unemployed</i> in Venezuela					
PEP3y $\times$ Post	-0.014	-0.016	-0.053	0.024	-0.010
	(0.045)	(0.081)	(0.143)	(0.033)	(0.045)
Observations	881	654	622	881	881
R-squared	0.668	0.578	0.472	0.841	0.238
D. Migrants who were <i>out of the labor force</i> in Venezuela					
PEP3y $\times$ Post	0.045	-0.081	-0.031	-0.002	-0.043
	(0.035)	(0.064)	(0.102)	(0.029)	(0.034)
Observations	3,097	1,966	996	3,097	3,097
R-squared	0.473	0.696	0.381	0.122	0.507

Notes: The sample of these estimates corresponds to the migrants who had the occupational status indicated in the title. The estimates include the same controls described in Equation 1.1, except time with PEP due to endogeneity concerns. Cluster-robust wild bootstrap p-values are used in these estimates to adjust for potential clustering effects. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. *Source:* Authors' calculations using EPM.

1.5.2.3 Effects of work permits by sector

The Colombian evidence underscores the central role of the services sector in the labor market absorption of Venezuelan migrants. This pattern is consistent with the global distribution of migrant employment, where approximately 68% of migrants work in services (UN, 2024). In Colombia, the concentration is even more pronounced. Close to 80% of employed Venezuelan migrants are in the tertiary sector (see Table 1.3), largely in low-entry and high-turnover activities such as restaurants, retail trade, domestic services, beauty services, and call centers. Together, these figures suggest that the services sector constitutes the primary short-run channel through which migrants are absorbed into the host-country labor market.

Given the sector's quantitative importance, we next assess whether the effects of work permits differ when restricting the sample to individuals linked to the tertiary sector. Table 1.6 reports estimates of equation 1.1 under two alternative restrictions. Panel A conditions on migrants whose pre-migration job in Venezuela was in the tertiary sector, while Panel B conditions on migrants whose current job in Colombia is in the tertiary sector. This distinction is informative because it allows us to separately examine whether work authorization operates through prior sector-specific experience in Panel A or through the segment of the Colombian labor market that most intensively absorbs migrants in Panel B.

Two main results emerge. First, for migrants with prior tertiary-sector experience in Venezuela, whether previously in white-collar or blue-collar occupations, the estimated effects of work permits on employment, occupational upgrading, formalization, unemployment, and labor-force participation are small and statistically indistinguishable from zero. This mirrors the baseline results reported in Table 1.4 and indicates that, within the service sector, legal status alone does not generate measurable improvements in labor market outcomes for workers with pre-existing employment trajectories.

Second, the estimates point to a different pattern for migrants who were unemployed in Venezuela. In the baseline specification, work permits were associated with an economically meaningful reduction in the probability of being out of the labor force, approximately 11 percentage points. When focusing on those currently employed in Colombia's tertiary sector in Panel B, this effect is slightly larger, with a 12 percentage-point decline. Moreover, work permits are now also associated with a statistically significant increase in the probability of employment for this group, approximately 14 percentage points. These magnitudes suggest that work permits may matter most for individuals entering the labor market from non-employment, particularly within the service sector segment that offers the lowest barriers to entry.

Table 1.6: Impact of work permits for those with experience in the service sector

Variables	Employed	White-collar job	Formal	Unemployed	Out of the labor force
	(1)	(2)	(3)	(4)	(5)
A. Previous experience in the tertiary sector (in Venezuela)					
A.1 Migrants who were <i>white-collar</i> in Venezuela					
WP x T	0.016	-0.001	-0.045	0.016	-0.031
	(0.027)	(0.042)	(0.042)	(0.016)	(0.028)
Observations	2,528	1,977	1,924	2,528	2,528
R-squared	0.256	0.348	0.251	0.090	0.207
A2. Migrants who were <i>blue-collar</i> in Venezuela					
WP x T	0.010	-0.018	0.076	0.012	-0.022
	(0.060)	(0.060)	(0.068)	(0.029)	(0.060)
Observations	1,196	959	920	1,196	1,196
R-squared	0.242	0.270	0.227	0.121	0.235
B. Current job in the tertiary sector (in Colombia)					
B1. Migrants who were <i>white-collar</i> in Venezuela					
WP x T	0.016	-0.001	-0.045	0.016	-0.031
	(0.027)	(0.042)	(0.042)	(0.016)	(0.028)
B2. Migrants who were <i>blue-collar</i> in Venezuela					
WP x T	0.010	-0.018	0.076	0.012	-0.022
	(0.060)	(0.060)	(0.068)	(0.029)	(0.060)
B3. Migrants who were <i>unemployed</i> in Venezuela					
WP x T	0.140*	-0.088	-0.032	-0.019	-0.121**
	(0.074)	(0.080)	(0.097)	(0.033)	(0.050)
B4. Migrants who were <i>out of the labor force</i> in Venezuela					
WP x T	0.053	-0.015	-0.016	-0.020	-0.033
	(0.042)	(0.084)	(0.076)	(0.030)	(0.032)

Notes: The sample of these estimates corresponds to the migrants who had the occupational status indicated in the title of the Panel. Panel A excludes A3 (unemployed) and A4 (out of the labor force) because prior occupation and sector information are unavailable for these groups. The estimates include the same controls described in Equation 1.1, except sector due to collinearity concerns. Cluster-robust wild bootstrap p-values are used in these estimates to adjust for potential clustering effects. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. *Source:* Authors' calculations using EPM.

1.5.3 Task content and job quality

This section examines whether legal access to work affects not only migrants' employment status, but also the task content of the jobs they hold in Colombia. Because broad occupational categories (white- versus blue-collar) can mask substantial within-category variation, we complement the previous analysis by studying changes in Routine Task Intensity (RTI) and its five

components. This allows us to assess whether regularization shifts migrants toward jobs that are more routinized—and therefore potentially more exposed to automation—or toward jobs with more non-routine cognitive and interpersonal content.

Table 1.7 yields no detectable average effect of PEP access on overall RTI for migrants coming from either white-collar or blue-collar occupations in Venezuela. The estimated effects are close to zero, indicating no broad reallocation toward jobs with higher or lower routine intensity. Consistent with this pattern, Figure A.4.1 (Appendix A.4) shows no discernible differences in RTI across gender, age, or education groups. Nonetheless, component-level estimates reveal that task-content effects are concentrated in specific margins and subgroups.

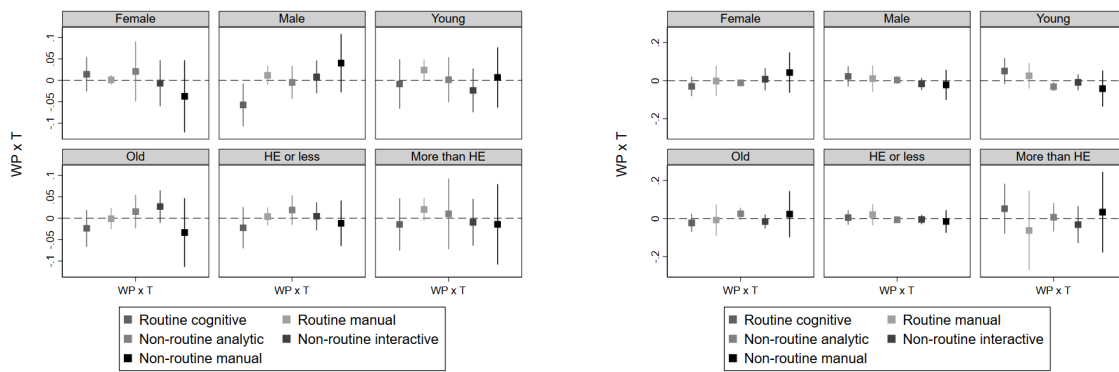
Table 1.7: Impact of PEP on RTI in Colombia

Variables	RTI	Routine		Non-routine		
		Cognitive	Manual	Analytic	Interactive	Manual
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Migrants who had a <i>white-collar</i> job in Venezuela						
PEP x T	-0.012 (0.045)	-0.015 (0.022)	0.010 (0.007)	0.015 (0.021)	0.001 (0.018)	-0.015 (0.030)
Observations	2,175	2,175	2,175	1,843	2,175	2,175
R-squared	0.207	0.245	0.121	0.349	0.260	0.403
Panel B. Migrants who had a <i>blue-collar</i> job in Venezuela						
PEP x T	0.043 (0.057)	0.012 (0.025)	0.009 (0.034)	-0.002 (0.010)	-0.010 (0.016)	-0.010 (0.033)
Observations	1,630	1,630	1,630	1,607	1,630	1,630
R-squared	0.301	0.267	0.264	0.215	0.253	0.276

Notes: The sample consists of migrants whose pre-migration occupational status is indicated in the table title. All specifications include the controls described in Equation 1.1. Reported p-values are computed using cluster-robust wild bootstrap procedures. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. *Source:* Authors' calculations using EPM.

However, the disaggregated evidence in Figure 1.4 indicates that this null average masks heterogeneity across demographic and human-capital groups (See data in A.4.1 - A.4.2). For this outcome, we do not observe systematic increases in non-routine components, suggesting that the adjustment reflects a change in task composition rather than broad upgrading in task complexity. In contrast, among migrants who held blue-collar jobs in Venezuela, the main heterogeneity arises among younger migrants: PEP access reduces non-routine analytic task intensity by 3.2 percentage points. This pattern indicates that legalization may facilitate labor-market entry without improving access to analytic, higher-complexity task content for younger workers coming from lower-skilled occupations, consistent with persistent barriers to mobility into skill-intensive roles even after legal status is obtained.

Figure 1.4: Impact of work permit on the five components of RTI

(a) *White-collar job in Venezuela*(b) *Blue-collar job in Venezuela*

Notes: *Source:* Authors' calculations using EPM data.

Overall, the task-based evidence is consistent with the paper's broader findings. Legalization improves labor market integration primarily through increased labor-force attachment and entry into employment, while generating limited average changes in job task content. In particular, we do not find systematic shifts toward less routine or more complex work, in line with the limited evidence of occupational upgrading documented above. These results suggest that legal status relaxes constraints to labor market access, but does not substantially alter the allocation of migrants across tasks. Where task-content effects do arise, they are not broad-based and instead appear concentrated in specific subgroups. Taken together, the evidence indicates that legalization operates mainly along the extensive margin, while frictions in occupational assignment and task allocation remain largely unaffected.

1.5.4 Mechanisms: Job search, training, and assimilation

This section explores mechanisms that may help interpret the labor market patterns documented in the previous sections. Work authorization may plausibly ease constraints to labor market access by facilitating contact with formal employment opportunities, expanding access to training programs, and supporting broader integration processes. As a result, migrants with permits may be more likely to receive information about formal jobs, invest in employability-enhancing skills, and develop stronger links with host-country institutions, factors that are typically associated with improved labor outcomes.

Using the EPM survey, we examine intermediate outcomes related to job search, job training, and cultural assimilation. We compare migrants with and without work permits using OLS regressions that include the same set of controls as in the main empirical specification. While this exercise is descriptive, it provides a consistent way to assess whether permit status is systematically associated with these mechanisms.

Table 1.8: Mechanisms explaining the impact of work permits

Variables	Difficulty finding a job	Information on formal jobs	Formal job seeking mechanism	Work in street	Work in online apps	Employer was a friend	Job training	Degree recognition	Identify with colombian culture
PEP	-0.022 (0.029)	0.111* (0.063)	-0.251 (0.294)	-0.030 (0.027)	0.005 (0.017)	-0.017 (0.018)	0.054** (0.025)	0.028 (0.018)	0.053** (0.022)
Observations	4,596	920	6,914	6,914	6,910	6,882	6,718	2,752	4,066
R-squared	0.301	0.589	0.275	0.397	0.238	0.193	0.257	0.584	0.304

Notes: The estimates include the same controls described in Equation 1.1. Cluster-robust wild bootstrap p-values are used in these estimates to adjust for potential clustering effects. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. *Source:* Authors' calculations using EPM.

Results in Table 1.8 indicate that work permits are positively correlated with a higher likelihood of receiving information about formal employment and participating in job training programs. These correlations are particularly relevant in a setting where migrants' job search often relies on informal networks—such as friends and relatives—and where migrant labor informality remains extremely high. In this context, greater exposure to formal job information and training opportunities may help rationalize the higher labor force participation and improved employment outcomes observed among some groups of permit holders, as well as their lower likelihood of engaging in street-based work.

Beyond job-search and human-capital channels, permits may also be correlated with broader integration dynamics. Legal recognition can facilitate social assimilation by strengthening migrants' attachment to the host society and improving their ability to navigate local norms and institutions, which may indirectly support labor market outcomes (Zhao & Tang, 2022). Consistent with this interpretation, permit holders are more likely to report identifying with Colombian culture, suggesting stronger social incorporation among migrants with work authorization.

At the same time, the occupational downgrading documented above suggests that work permits alone may be insufficient to remove barriers to occupational mobility. In line with this, Table 1.8 does not show a robust association between permit status and degree recognition, pointing to the continued relevance of credential-related constraints. This is consistent with a broader literature emphasizing that occupational upgrading often depends on frictions such as credential recognition and the limited transferability of origin-country skills, which may persist even after legal access improves (Chiswick & Miller, 2009; Friedberg, 2000).

Overall, the descriptive evidence suggests that permit holders are more connected to formal job information, more likely to invest in training, and more likely to report cultural identification with the host country—mechanisms that align with the paper's main findings. At the same time, the persistence of downgrading indicates that complementary interventions—particularly those aimed at skill recognition and job matching—may be needed to improve the allocation of migrants' skills in the labor market.

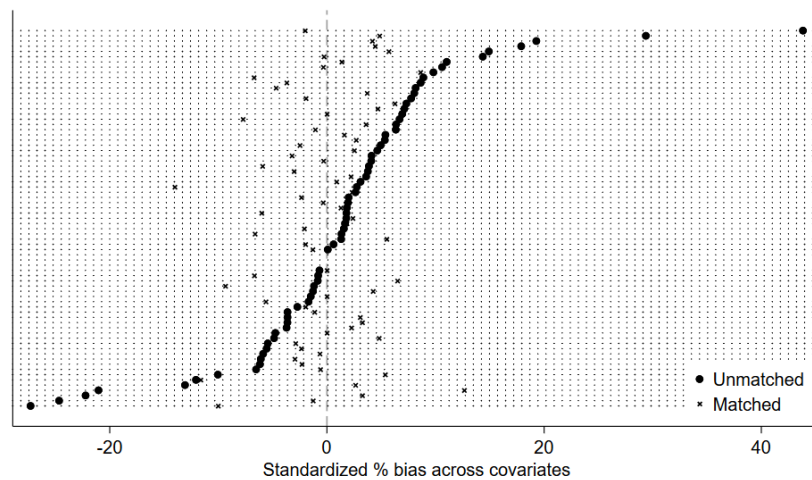
1.6 Robustness checks

A standard event study pre-trends test is not feasible in our setting because the data do not provide multiple observations in the pre-treatment period. The “pre” outcomes are measured retrospectively as respondents’ main activity before migration, while the “post” outcomes correspond to their current activity in Colombia. Consequently, we cannot estimate lead effects or inspect dynamic pre-treatment coefficients as in conventional panel Difference-in-Differences designs. Instead, we assess the credibility of our identification through (i) balance and overlap checks in the pre-migration period and (ii) placebo and falsification exercises described below, including permutation-based placebo assignments.

1.6.1 Balance checks

Figure 1.5 reports standardized differences in baseline covariates before and after matching. Several covariates exhibit substantial imbalance in the raw sample, but reweighting/matching markedly reduces standardized bias across all covariates, with post-adjustment differences concentrated near zero.

Figure 1.5: Covariate balance before and after weighting (for age as covariate)



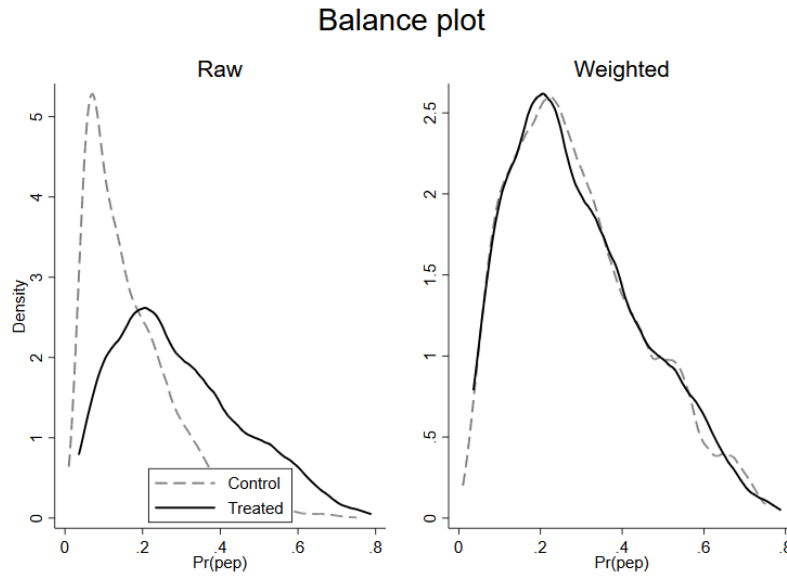
Notes: The figure plots standardized %age differences in baseline covariates between treated and control units in the raw sample (unadjusted) and after applying ATT inverse-probability weights.

1.6.2 Common support

We assess the plausibility of the reweighting strategy by examining covariate balance and common support between treated and control individuals. Figure 1.6 reports standardized mean differences (SMDs) for baseline covariates before and after applying ATT inverse-probability weights. Prior to reweighting, several covariates exhibit meaningful imbalance (e.g., education

and location). After reweighting, all standardized differences shrink markedly and fall close to zero, indicating that the weighted control group is highly comparable to the treated group in observed characteristics. Figure 1.6 further shows that reweighting substantially improves common support: while the propensity-score distributions differ in the raw data, they overlap closely in the weighted sample. Consistent with this, the ATT weights are well behaved ($p99 = 1.16$; $\max = 3.02$), suggesting that results are not driven by extreme reweighting.

Figure 1.6: Distribution of estimated propensity scores by PEP Status: Common support before and after reweighting



Notes: The figure plots kernel densities of estimated propensity scores for treated and control individuals in the raw sample (left panel) and after applying ATT inverse-probability weights (right panel).

1.6.3 Falsification test: Randomized treatment assignment

As a falsification exercise, we randomly permute the work permit indicator within the group of migrants who explicitly report not having a PEP, while preserving the overall treatment share (17 percent). Since this placebo assignment breaks any systematic link between legal status and outcomes, the interaction $\text{Random PEP} \times \text{Post}$ should be close to zero.

Table 1.9 confirms this prediction. The estimates are small and statistically indistinguishable from zero across all outcomes, indicating that randomly assigned “PEP status” does not generate spurious effects. This supports the interpretation that the main results are not driven by the empirical design or the weighting procedure. This pattern holds across the subsamples used above.

Table 1.9: Placebo treatment: Randomized PEP assignment

	Employed	White-collar job	Formal	Unemployed	Out of the labor force
Variables	(1)	(2)	(3)	(4)	(5)
Random PEP $\times$ Post	0.011 (0.015)	0.033 (0.028)	-0.001 (0.035)	-0.010 (0.017)	-0.001 (0.021)
Observations	7,699	5,491	4,579	7,699	7,699
R-squared	0.487	0.411	0.252	0.109	0.412

Notes: This table reports a placebo test in which PEP status is randomly reassigned within the sample that reports not having a PEP, preserving the treatment share (17%). Estimates use the baseline difference-in-differences specification and controls. Cluster-robust wild bootstrap p-values are reported. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Source: Authors' calculations using EPM.

1.6.4 Placebo outcomes: Predetermined outcomes

As an additional falsification exercise, we estimate the baseline difference-in-differences specification using outcomes that should not plausibly respond to work authorization. Specifically, we consider (i) gender, (ii) age, and (iii) whether the respondent reports having migrated for health-related reasons. These variables are either predetermined or reflect fixed characteristics and motivations that should not be altered by receiving a work permit.

If the identifying assumptions underlying our main estimates are valid, the interaction $PEP \times Post$ should be close to zero for these placebo outcomes. Consistent with this prediction, the estimates show no statistically significant effects on gender, age, or health-related migration, supporting the view that our results are not driven by differential trends, compositional changes mechanically induced by the weighting procedure, or spurious correlations.

Table 1.10: Placebo outcomes: Predetermined outcomes

Variables	Female	Education years	Migrated for health reasons
	(1)	(2)	(3)
PEP $\times$ Post	0.024	0.060	-0.015
Observations	0.146	0.168	0.095

Notes: Estimates use the baseline difference-in-differences specification and controls, except gender and age when corresponds. Cluster-robust wild bootstrap p-values are reported. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Source: Authors' calculations using EPM.

1.6.5 Exclusion of border areas

We examine whether our results are sensitive to excluding cities in border departments, following [Bahar et al. \(2021\)](#). Specifically, we drop the departments of Norte de Santander and Cesar from the sample. Table A.5.1 in Appendix A.5 re-estimates the program effects and confirms the robustness of our results.

1.7 Conclusions

This paper studies how legal work authorization shapes the labor-market integration of forced migrants in a high-informality setting. Leveraging unique data that track Venezuelan migrants' labor-market outcomes both before migration and after settlement in Colombia, we provide new evidence on the role of regularization policies in mediating employment, occupational allocation, and job-task content. Exploiting the rollout of the *Permiso Especial de Permanencia* (PEP) and combining a difference-in-differences design with inverse probability weighting, we identify the causal effects of access to legal work authorization on migrants' labor-market trajectories.

Three main findings emerge. First, under the identifying assumptions of the reweighted difference-in-differences design, access to the PEP is estimated to strengthen labor-market attachment, mainly by reducing non-participation. These effects are concentrated among migrants who were unemployed prior to migration and among younger and less educated individuals. The results are consistent with regularization relaxing barriers to labor-market entry and sustained participation for migrants who are weakly attached at baseline.

Second, the estimates provide limited evidence that legal status mitigates occupational downgrading or improves skill-appropriate matching. Migrants who held white-collar jobs in Venezuela continue to experience substantial downgrading upon arrival in Colombia, and access to the PEP is not associated with significant increases in white-collar or formal employment on average. Task-based measures corroborate this pattern: legalization does not appear to induce broad shifts toward less routine or more cognitively intensive jobs.

Third, the results highlight a distinction between extensive-margin integration and improvements in job quality. Work permits appear to facilitate attachment to the labor market, but they do not substantially alter the allocation of migrants' skills across occupations or task complexity. A natural interpretation is that PEP operates partly through improved access to complementary institutions that support labor supply. Consistent with this view, [Ibáñez, Moya, Ortega, Rozo, and Urbina \(2022\)](#) show that regularization among Venezuelan forced migrants in Colombia increases access to social protection and public services, including SISB'EN enrollment, as well as access to financial services. These channels can relax liquidity and insurance constraints and reduce the costs of participation, even if occupational upgrading remains limited over the horizon we study. At the same time, our findings speak to work highlighting downgrading as a key margin shaping the broader labor market consequences of the Venezuelan inflow in Colombia, including its distributional implications (e.g., [Lombardo et al., 2021](#)).

Taken together, the evidence indicates that regularization policies increase labor force participation—particularly among previously unemployed migrants—but the estimates provide limited evidence that work permits improve occupational upgrading, job quality, or transitions into formal employment. This suggests that legal access to work, while necessary for labor market inclusion, is not sufficient to overcome the structural determinants of occupational downgrading.

A natural implication is that Colombia's system of special permits should be embedded

within a broader integration policy framework. In particular, complementary interventions targeting skill accumulation and transferability are likely to be critical. Consistent with international experience, this includes policies that expand access to education and vocational training, as well as targeted upskilling and reskilling initiatives.

Institutions such as the OECD [Organisation for Economic Co-operation and Development \(OECD\) \(2025\)](#) and the European Commission [European Commission \(2025\)](#) highlight a set of policy instruments that can guide the design of migrant labor market integration policies in Colombia. First, micro-credential programmes—implemented in countries such as Austria, Canada, France, and Germany—provide modular pathways for migrants to address skill gaps without requiring full requalification. Second, policies that facilitate the recognition of foreign qualifications and the assessment and validation of skills have been shown to improve both skill utilization and employment outcomes, as documented for Switzerland and Germany ([Brücker et al., 2021](#); [Pecoraro & Tani, 2023](#); [Pecoraro & Wanner, 2019](#)). Third, digital tools, including AI-based systems, can enhance the matching of migrant skills to local labor market demand by predicting settlement needs and identifying skill shortages.

An important avenue for future research is to evaluate whether these complementary policies generate measurable effects on migrants' upskilling and reskilling trajectories, and whether such gains translate into sustained improvements in occupational mobility and job quality.

Chapter 2

Measuring perceived migrant threat: Evidence from a list experiment in Cali, Colombia

Angie A. Mondragón-Mayo*

Abstract

Beliefs linking migrants and insecurity are politically salient but difficult to measure due to potential misreporting in surveys with direct questions. This paper employs a list experiment within the 2024 SISBEN Scientific Survey conducted in Cali, Colombia, to estimate the prevalence of perceived threats from migrants. Analyzing data from approximately 2,000 respondents, findings indicate that around two-thirds of the surveyed individuals believe that foreign immigrants increase insecurity in the city, with results remaining consistent across various specifications and resilient to controls that capture geography, demographics, and exposure. Additionally, the study reveals that this underlying belief is strongly correlated with other insecurity-related attitudes: respondents who endorse the sensitive belief are significantly more likely to expect future victimization, rationalize violence, and perceive low probabilities of punishment. These results underscore the effectiveness of indirect elicitation methods in uncovering socially sensitive beliefs that are critical in migration and security policy discussions.

Keywords: List experiment, migration, insecurity, social desirability bias.

JEL Codes: C83, F22, D91.

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2.1 Introduction

Large migration inflows are frequently framed as a public safety problem. A central dimension of migrant integration in this context is the social environment in which migrants are received, including the beliefs and perceptions held by the host population. When citizens believe that immigrants increase crime, these beliefs can shift political support toward enforcement, weaken support for public integration policies, and shape tolerance for punitive or coercive responses. These dynamics are particularly salient in urban settings with longstanding security challenges. The central empirical object in these debates, the perception of migrant threat, is difficult to measure. Political and socially sensitive attitudes that associate immigrants with criminal activity may be routinely misreported in direct survey questions. As a result, public narratives attribute insecurity to migration even in contexts where their observed participation in crime is limited.

This study examines these beliefs in the context of the Venezuelan migration exodus, one of the largest episodes in Latin America's history. Since 2014, approximately 7.7 million Venezuelans have left their country, with Colombia being the primary destination. In this country, Cali stands out as the fifth-largest receiving city for Venezuelan migrants in Colombia, hosting roughly 129,000 migrants as of January 2024 (*Bitácora Migratoria*, 2024). At the same time, Cali has long faced elevated levels of urban violence compared to other large Colombian cities (*Policía Nacional de Colombia*, 2024; UNODC, 2023). The combination of visible demographic change and persistent insecurity makes Cali a salient setting in which narratives linking migration to crime can emerge and circulate.

Despite the prominence of migration in public debate, administrative evidence does not indicate that Venezuelan migrants are disproportionately responsible for criminal activity (*Bahar, Dooley, & Selee*, 2020; *Knight & Tribin*, 2022). Nationality-disaggregated records show that foreigners account for a minority share of arrests, and Venezuelans constitute only a fraction of that already limited proportion between 2017 and 2024 (*Fiscalía General de la Nación*, 2024; *Policía Nacional de Colombia*, 2024). The contrast between these administrative patterns and the persistent narratives linking migration to insecurity motivates a central question: i) How widespread is the belief that migrants increase insecurity? and ii) how is this belief connected to attitudes about victimization, violence, and punishment?

To answer this question, I estimate the prevalence of perceived migrant threat using a list experiment embedded in the 2024 SISBEN Scientific Survey, administered to nearly 2,000 respondents interacting with local public institutions in Cali. The survey collects detailed information on perceptions of safety, victimization experiences and expectations, and views about punishment and impunity. By embedding an item-count technique within a large urban field survey, the paper obtains an estimate of the prevalence of agreement with a socially sensitive statement without requiring respondents to reveal their position directly.

I find that almost two-thirds of respondents agree that foreign migrants make the city more

insecure. The estimate is measured, stable across specifications, and robust to geographic, demographic, and exposure-related controls. Beyond documenting prevalence, I show that perceived migrant threat is systematically associated with other insecurity-related beliefs. Respondents classified as endorsing the sensitive statement are 17 percentage points more likely to expect future victimization, 11 percentage points more likely to justify violence, and 17 percentage points more likely to perceive low probabilities of punishment.

This paper makes two contributions. It provides a credible measurement of a politically salient belief in a context where direct survey responses may be distorted by social desirability. In doing so, it shifts attention from whether migration increases crime to the extent to which the belief in such a link is widespread. In addition, the study documents that the perception of migrant threat is embedded in a broader configuration of insecurity-related attitudes, suggesting that migration narratives may interact with preexisting concerns about victimization and institutional weakness. In public policy environments where perceptions may shape preferences independently of objective crime patterns, understanding and measuring these beliefs is central to the study of migration and public policy security.

The remainder of the paper proceeds as follows. Section 2.2 reviews the related literature. Section 2.3 describes the data. Section 2.4 outlines the experimental design and identification strategy. Section 2.5 presents the main results and the heterogeneity analysis. Section 2.6 examines the relationship between the perception of migrant threat and other insecurity beliefs. Section 2.7 reports robustness checks and discusses external validity. Section 2.8 concludes.

2.2 Related literature

This paper relates to three strands of literature: the economics of immigration and crime, the political economy of immigration attitudes, and the measurement of socially sensitive beliefs.

A large literature examines whether immigration increases crime using administrative data and quasi-experimental variation. The emerging consensus is that the relationship is neither mechanical nor uniform across contexts. In many settings, immigration has small or null effects on crime, and when effects arise, they are often outcome-specific and heterogeneous. For example, [Bell, Fasani, and Machin \(2013\)](#) study large immigration waves in the United Kingdom and finds limited impacts on crime. Evidence from Colombia reinforces this pattern. Exploiting the temporary closure and reopening of the Colombia–Venezuela border, [Knight and Tribin \(2022\)](#) show that increases in violence in border municipalities are driven primarily by Venezuelans as victims rather than perpetrators. Complementary evidence finds modest short-run increases in selected property crimes but no effects on violent crime ([Franco, 2021](#)). Using nationality-disaggregated data from Colombia, Peru, and Chile, [Bahar et al. \(2020\)](#) conclude that Venezuelans are not systematically overrepresented in crime relative to their population. Taken together, this literature documents that objective crime patterns do not generally support claims of migrant-driven criminality.

Other literature studies the formation of immigration attitudes. Political economy models emphasize that social heterogeneity shapes preferences over public goods and redistribution (Alesina, Baqir, & Easterly, 1999). Empirical work shows that immigration attitudes are often driven by concerns more related to identity and culture than by narrowly defined labor market competition (Hainmueller & Hopkins, 2014). Experimental evidence further complicates purely economic explanations. Hainmueller and Hiscox (2010) show that respondents tend to prefer high-skilled immigrants, and Alesina, Miano, and Stantcheva (2023) expose systematic misperceptions about immigrant population size and composition that correlate strongly with policy preferences. These results imply that perceptions about immigration could respond to identity concerns, information frictions, and salient narratives even when objective economic or crime outcomes do not change.

Related work provides direct evidence of divergence between objective conditions and perceived insecurity. In Chile, Ajzenman, Domínguez, and Undurraga (2022) find no detectable effect of immigration on victimization, yet substantial increases in crime-related concerns and defensive behaviors. Similarly, Lebow, Moreno-Medina, Mousa, and Coral (2024) reveal that anti-migrant sentiment deteriorated nationally across major receiving countries during the Venezuelan exodus, while subnational areas with greater migrant exposure did not exhibit more negative attitudes. These patterns are consistent with beliefs shaped by national narratives rather than local crime exposure. This paper contributes to this strand by indirectly measuring perceived migrant threat in a major receiving city and linking it to broader insecurity attitudes.

Finally, the paper contributes to the methodological literature on measuring socially sensitive attitudes. When beliefs are normatively charged, direct survey questions may induce misreporting due to social desirability bias. The item-count technique or list experiment addresses this concern by eliciting latent agreement without requiring respondents to disclose specific positions. Imai (2011) and Blair and Imai (2012) formalize the identification assumptions and estimation strategies for list experiments, and early applications show that indirect elicitation can uncover substantially higher levels of prejudice than direct questioning (Kuklinski, Cobb, & Gilens, 1997). Building on this framework, this paper implements a list experiment in a large urban field survey to recover a design-based estimate of perceived migrant threat and to examine how this belief varies with expectations of victimization, tolerance for violence, and perceived impunity.

2.3 Data

The analysis uses data from the Security Module of the 2024 SISBEN Scientific Survey (SSS2024), collected between mid-July and mid-August 2024 at SISBEN service points in Cali. The SSS2024 is implemented by the District Secretariat of Social Development as part of a broader municipal data initiative. While the Secretariat routinely administers the standard SISBEN questionnaire (Colombia's main targeting instrument for classifying households ac-

according to their socioeconomic conditions and determining eligibility for social programs)¹ the Security Module was designed specifically for this project as an additional survey component. It collects detailed information on multiple dimensions of urban insecurity, including perceptions of safety in public spaces, retrospective and expected victimization, beliefs about the likelihood of punishment, preventive behaviors, and attitudes toward the use of violence.

The security module was designed to measure multiple dimensions of urban insecurity, including perceptions of safety in several public spaces, retrospective and expected victimization, beliefs about the likelihood of punishment, preventive behaviors, and attitudes toward the use of violence. These measures allow insecurity to be characterized not only as fear of crime but also as a broader set of beliefs about social order and institutional effectiveness. The 2024 wave represents the first implementation of this module. The results reported in this study correspond to this deployment of the survey.

The list experiment sample consists of 2,051 local Colombian respondents, of whom 975 were assigned to the control condition and 1,076 to the treatment condition. After excluding non-responses to the list experiment item, the estimation sample includes approximately 1,985 observations.

The sampling frame imposes an important restriction on the external validity of the estimates. Since respondents were recruited through SISBEN service points, the results should be interpreted as applying to socioeconomically vulnerable populations in Cali rather than to the city as a whole. This distinction is relevant because beliefs about insecurity and migration may vary systematically with income, neighborhood characteristics, exposure to crime, and access to public goods. Thus, the estimates are best understood as documenting perceived migrant threat in a policy-relevant segment of the urban population, but they should not be read as citywide prevalence estimates.

Trained interviewers conducted fieldwork during regular weekday operations at SISBEN service points. Treatment assignment was randomized on the day of the interview, rotating treatment status throughout the fieldwork period to avoid systematic assignment on weekdays. Descriptive statistics indicate that the treatment and control groups are balanced in terms of demographic and socioeconomic characteristics.

Following CIDSE (2011), and to capture spatial heterogeneity, respondents are grouped into five geographic zones, defined by contiguity and shared socioeconomic characteristics: east (communes 7, 13, 14, 15, and 21), center-east (communes 8, 11, 12, and 16), Center-North (communes 3, 4, 5, 6, 9, and 10), hillside (communes 1, 18, and 20), and north-south (communes 2, 17, 19, and 22). Nearly half of respondents (48.7 percent) reside in the Eastern zone, followed by the Hillside (17.5 percent) and Center-North (14.7 percent). This spatial distribution reflects the location of SISBEN service points and the populations covered, which is central to interpreting heterogeneity across neighborhoods.

¹The population size corresponds to the number of individuals registered in the SISBÉN database of Santiago de Cali as of August 2024 (7,863 individuals).

2.4 Experimental design

2.4.1 List experiment

To measure the perception of migrant threat, the survey includes a list experiment, also known as the item count technique, designed to mitigate misreporting when attitudes are socially sensitive (Imai, 2011). Attributing insecurity to migrants is normatively charged in this context and may be understated with direct questioning. The list experiment provides an indirect measure of agreement with the sensitive statement.

Treatment and control groups received a list of safety-related statements and were asked to indicate only the total number of statements with which they agreed. The control list contained three neutral statements about local insecurity. The treatment list included the same three statements plus a fourth sensitive item: “Foreign immigrants make the city more insecure.” The experiment was administered exclusively to local Colombian respondents.

Under standard identifying assumptions, the difference in mean item counts between treatment and control groups allows measure the sample prevalence of agreement with the sensitive statement (Blair & Imai, 2012). The mean item count equals 3.29 in the treatment group and 2.63 in the control group, implying substantial latent agreement. Table 2.1 reports the exact wording of the experiment.

Table 2.1: Questions to evaluate perception about immigrants increase insecurity.

Control group	Treatment group
1. Walking alone at night is dangerous in some neighborhoods.	1. Walking alone at night is dangerous in some neighborhoods.
2. Insecurity is a major problem in the city.	2. Insecurity is a major problem in the city.
3. Youth gangs affect security.	3. Youth gangs affect security.
	4. Foreign immigrants make the city more insecure.

Notes: Participation in the survey was voluntary, and respondents were allowed to skip any question. In the list experiment, 3.22 percent of eligible respondents chose not to provide a response. Source: Security module of SSS2024.

Table 2.2 reports descriptive statistics for the control and treatment groups in the list experiment. Random assignment produced groups that are highly comparable across observable characteristics: differences in gender, age, educational attainment, marital status, ethnic minority status, employment, and neighborhood tenure are small and statistically insignificant. The only notable imbalance is residence in the eastern zone of the city, which is approximately three percentage points higher in the control group, motivating the inclusion of geographic controls in the empirical analysis. Consistent with the experimental design, the mean item count is higher in the treatment group (3.29) than in the control group (2.63). This statistically significant difference indicates substantial support for the sensitive statement that foreign immigrants make the city more insecure.

Table 2.2: Descriptive statistics

Variable	Control	Treatment	Diff.
	(1)	(2)	(1) - (2)
N	975	1,076	-101
Mean (item count)	2.63	3.29	-0.66***
SD (item count)	0.63	0.95	N/A
Share at minimum (%) [†]	0.08	0.01	0.07
Share at maximum (%) [‡]	71	57	14
Female (%)	77	74	3.00
Age (years)	41.92	42.18	-0.26
High college	71	69	2.00
Married or cohabiting (indicator)	0.30	0.28	0.02
Ethnic minority (indicator)	0.37	0.37	0.00
Employed (indicator)	0.19	0.20	-0.01
Resides in eastern zone (indicator)	0.52	0.49	0.03 *
Neighborhood tenure (years)	2.35	2.36	0.00
Expects to be a future robbery victim	0.67	0.69	-0.02
Does not expect offenders to be sanctioned (indicator)	0.71	0.72	-0.01
Feels unsafe in the city (indicator)	0.50	0.53	-0.03

Notes: [†] Minimum item count equals 0 in both groups. [‡] Maximum item count equals 3 in the control group and 4 in the treatment group. Source: Security Module, SSS2024.

2.4.2 Design checks

I conduct a series of diagnostic checks to assess the internal validity of the list experiment following the approach proposed by [Blair and Imai \(2012\)](#). We begin by examining the distribution of item-count responses in the treatment and control groups in [Table 2.3](#). Responses in both groups span the full support of the scale, indicating that the respondents understood the task. At the same time, a large share of respondents reports the maximum possible count: 71.2 percent in the control group (maximum of three items) and 57.1 percent in the treatment group (maximum of four items). This concentration at the upper tail of the distribution suggests a ceiling effect.

However, this feature is mechanically expected in settings where baseline agreement with non-sensitive insecurity items is already high. Importantly, the ceiling effect does not invalidate identification under the standard assumptions of the list experiment, since the estimates relies on differences in mean counts across randomized conditions. If anything, compression at the upper bound is likely to attenuate the estimated treatment effect, implying that the estimated prevalence should be interpreted as a conservative lower bound of latent agreement. The stability of the treatment coefficient across specifications further suggests that the estimated magnitude

is not driven by distributional artifacts but reflects a broad upward shift induced by the sensitive item.

Table 2.3: Distribution of item-count responses in the list experiment

Item count	Control		Treatment	
	Quantity	%	Quantity	%
0	8	0.84	1	0.10
1	55	5.78	78	7.55
2	211	22.16	125	12.10
3	678	71.22	239	23.14
4	-	-	590	57.12
Total	952	100	1,033	100

Notes: The maximum count is 3 in the control group and 4 in the treatment group. Source: Security module of SSS2024.

To further assess whether the inclusion of the sensitive item distorts responses to the non-sensitive items, we implement formal distributional tests. Pearson chi-squared and Kolmogorov–Smirnov tests comparing item-count distributions over the common support reject equality between treatment and control groups (Pearson $\chi^2(3) = 43.33$, $p < 0.001$; KS $D = 0.090$, $p = 0.001$), with differences concentrated in the upper tail of the distribution.

We also implement a targeted diagnostic that focuses on responses that cannot include the sensitive item. Specifically, we compare the probability of reporting an item count below the maximum number of non-sensitive items across treatment and control conditions. Treatment respondents are significantly less likely to report counts below this threshold than control respondents (19 percent versus 28 percent; Pearson $\chi^2(1) = 23.92$, $p < 0.001$), indicating that the treatment shifts responses toward higher item counts.

Taken together, these patterns indicate that the inclusion of the sensitive item moves mass toward the upper end of the item-count distribution. Following Glynn (2013), we interpret this evidence as reflecting high agreement with the sensitive statement rather than misunderstanding of the task or strategic distortion of responses to the non-sensitive items. While the presence of a ceiling effect may reduce precision, it does not invalidate the difference-in-means estimator underlying the list experiment. A central identifying assumption of the item count technique is the absence of design effects, meaning that the inclusion of the sensitive item does not alter responses to the non-sensitive items. While the upward shift in the distribution is consistent with endorsement of the sensitive item, formal distributional tests do not indicate systematic distortions inconsistent with additive behavior. Moreover, the invariance of treatment effects across subgroups and specifications provides indirect evidence that the inclusion of the sensitive statement does not fundamentally change how respondents evaluate the non-sensitive items. Although the assumption cannot be tested directly, the available diagnostics are consistent with the standard additive framework underlying the estimator.

Taken together, the design checks suggest that the estimate should be interpreted conservatively. Because agreement with the non-sensitive items is high, the list experiment may be vulnerable to ceiling effects. Treated respondents who also agree with the sensitive item may need to report the maximum count, which can weaken perceived anonymity and induce downward adjustment in reported counts. This mechanism would attenuate the estimated treatment-control difference. The estimate of approximately 66 percent should therefore be read as a lower bound on the prevalence of perceived migrant threat in the surveyed population. Future survey waves will address this limitation by introducing control items with more heterogeneous expected agreement—such as items on traffic, public transportation, parks, or neighborhood amenities—which preserve the urban-policy frame while reducing the likelihood of maximum item counts.

2.4.3 Identification strategy

Our identification strategy exploits randomized assignment to a list experiment to measure agreement with a socially sensitive statement regarding migration and insecurity. The outcome is the item-count response, C_i , defined as the number of statements with which respondent i agrees. Respondents were randomly assigned to either a control condition, which included only non-sensitive items, or a treatment condition, which included the same list plus the sensitive item stating that *foreign immigrants make the city more insecure*.

Under the standard assumptions of the list experiment, the difference in mean item counts between treatment and control groups identifies the prevalence of agreement with the sensitive statement in the study population. We implement this estimate using the following regression specification:

$$C_i = \alpha + \tau T_i + X_i' \beta + \varepsilon_i, \quad (2.1)$$

where T_i is an indicator equal to one if respondent i was assigned to the treatment condition and zero otherwise, and X_i is a vector of pre-determined demographic and socioeconomic characteristics. The coefficient τ measures the average change in the item count attributable to the inclusion of the sensitive item, and therefore corresponds to the share of respondents who agree with the sensitive statement.

Treatment assignment was randomized at the interview-day level for operational feasibility. Because observations may be correlated within days, inference relies on wild cluster bootstrap procedures suited to a small number of clusters. Results are very similar when adding day fixed effects, suggesting the estimated treatment effect is not driven by day-specific shocks or shifts in the composition of respondents across interview dates.

Randomization implies that T_i is orthogonal to both observed and unobserved respondent characteristics, so τ should be unbiased even without controls. Nevertheless, I include covari-

ates to improve precision and to demonstrate robustness. X_i includes gender, age, educational attainment (an indicator equal to one if the respondent has completed secondary education or more), marital or cohabitation status, ethnic minority status, employment status, residence in the eastern zone of the city, neighborhood tenure, expectations of future victimization, perceived likelihood of punishment for criminal activity, and perception of security at the city-level.

To identify the perception of migrant threat within a broader set of insecurity-related beliefs, I examine whether individuals who express *sure agreement* with the sensitive statement also differ systematically in related perceptions and behaviors. This analysis is intended to characterize how such perception varies with other dimensions of insecurity, rather than to establish causal relationships.

Specifically, I estimate a *logit* model for the probability of observing insecurity-related outcomes, $P(Y_i = 1 \mid X_i)$, where Y_i is a dichotomous indicator capturing perceptions or behaviors related to citizen security:

$$\Pr(Y_i = 1 \mid X_i) = \Lambda(\alpha + \beta \text{Threat}_i + X_i' \gamma), \quad (2.2)$$

Where $\Lambda(z) = \frac{\exp(z)}{1+\exp(z)}$ is the logistic cdf. The vector X_i includes the controls employed in Equation 2.1, except for those considered outcome variables.

To link these outcomes to perceived migrant threat, I construct a conservative proxy using the list experiment. I restrict the analysis to respondents assigned to the treatment condition and define:

$$\text{Threat}_i = \mathbb{1}\{C_i = 4\}, \quad (2.3)$$

Where C_i denotes the item-count response. Reporting the maximum count implies agreement with the sensitive statement that foreign immigrants increase insecurity. Respondents reporting three items in the treatment group may or may not agree with the sensitive statement and are therefore coded as $\text{Threat}_i = 0$ under this definition.

I report *average marginal effects* for ease of interpretation. The parameter β is descriptive and should not be interpreted causally; it shows conditional associations between the perception of migrant threat and the attitudes related to insecurity.

2.5 Main results

Table 2.4 presents the baseline difference in item counts between the treatment and control lists. Across all columns, assignment to the treatment list increases the reported count by about 0.66 items (s.e. around 0.055 to 0.059), with no meaningful variation across specifications. Interpreted through the standard item count design, this coefficient corresponds to the estimated share of respondents who endorse the sensitive statement that foreign immigrants make the city

more insecure, implying prevalence on the order of two-thirds in this sample.

A key feature of the table is the invariance of the treatment coefficient to progressively richer adjustment sets. The point estimate is already large in the unadjusted specification and remains essentially identical after adding the balance adjustment for Eastern-zone residence and then layering in demographics, measures of migrant exposure, prior victimization, and local attachment. This stability is consistent with randomized assignment and suggests that the estimated prevalence is not an artifact of chance imbalances in observables, but rather reflects a broad-based shift in responses induced by the inclusion of the sensitive item.

The added covariates have limited incremental explanatory power and do not change the substantive conclusion. Eastern-zone residence and commune-level migrant LFPR enter with small coefficients, while prior victimization is modestly negative in the richer specifications. Overall, the baseline results provide a stable estimate of the prevalence of perceived migrant threat in a context where baseline agreement with non-sensitive insecurity statements is already high.

Table 2.4: Baseline estimates: List Experiment outcomes (N ~ 1907)

	Item count outcomes					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.659*** (0.058)	0.658*** (0.058)	0.657*** (0.059)	0.661*** (0.055)	0.661*** (0.055)	0.662*** (0.055)
Eastern zone		-0.026 (0.029)	-0.023 (0.031)	-0.061 (0.036)	-0.051 (0.036)	-0.036 (0.034)
Female			-0.005 (0.043)	0.002 (0.039)	0.005 (0.039)	0.003 (0.039)
High School or More			-0.058 (0.048)	-0.051 (0.048)	-0.047 (0.046)	-0.049 (0.050)
Age			-0.012 (0.008)	-0.012 (0.007)	-0.011 (0.007)	-0.011 (0.008)
Age squared			0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Commune-Level Migrant LFPR				-0.004 (0.007)	-0.004 (0.007)	-0.004 (0.007)
Any victimization					-0.088* (0.044)	-0.085* (0.043)
Internal migrant						-0.005 (0.050)
Neighborhood tenure						0.029 (0.021)
Constant	2.638*** (0.024)	2.651*** (0.033)	2.901*** (0.176)	3.161*** (0.525)	3.200*** (0.526)	3.124*** (0.506)
N	1,985	1,985	1,985	1,907	1,907	1,899
R-squared	0.140	0.140	0.143	0.150	0.152	0.153
Balance adjustment control	No	Yes	Yes	Yes	Yes	Yes
Individual controls	No	No	Yes	Yes	Yes	Yes
Migrant exposure controls	No	No	No	Yes	Yes	Yes
Crime exposure controls	No	No	No	No	Yes	Yes
Local attachment controls	No	No	No	No	No	Yes

Notes: The dependent variable is the number of items affirmed in a list experiment (item count technique). Treatment exposure indicates assignment to the list experiment condition that includes the sensitive statement. Coefficients are reported in item units. Standard errors are clustered at the interview-day level, and p-values are based on wild cluster bootstrap inference with Rademacher weights.

Table 2.5 reports the main estimates from the list experiment in a parsimonious format. The implied effect of adding the sensitive item is 0.661 items on average (ATE) and 0.653 items

for respondents assigned to the treatment condition (ATT), with inference based on standard errors clustered at the interview-day level. Both estimates are obtained from specifications that include Eastern-zone residence, commune-level migrant LFPR, and prior victimization, which address the small baseline imbalance and capture salient exposure channels. The near equality of ATE and ATT is what one would expect under randomized assignment and indicates that selection into treatment does not mechanically shape the estimated prevalence.

Table 2.5: Estimated Treatment Effects (ATE and ATT)

	ATE	ATT
	(1)	(2)
Value	0.661***	0.653***
	(0.036)	(0.036)

Notes: The table reports ATE and ATT estimates. Standard errors clustered at the interview-day level are reported in parentheses. All specifications control for Eastern zone residence, commune-level Migrant LFPR, and prior crime victimization, included to address baseline imbalance and for theoretical relevance. Coefficients are expressed in item units.

These estimates provide a disciplined benchmark for the level of latent agreement with the statement linking migrants to insecurity in the study population, and they suggest that the average response to the sensitive item is not driven by a narrow subset of respondents. Having established a precise average effect, the analysis naturally turns to heterogeneity. The next section asks whether attribution is systematically higher in specific social or spatial environments, and whether differences across ethnic identity and neighborhood context align with variation in migrant exposure and insecurity-related experiences.

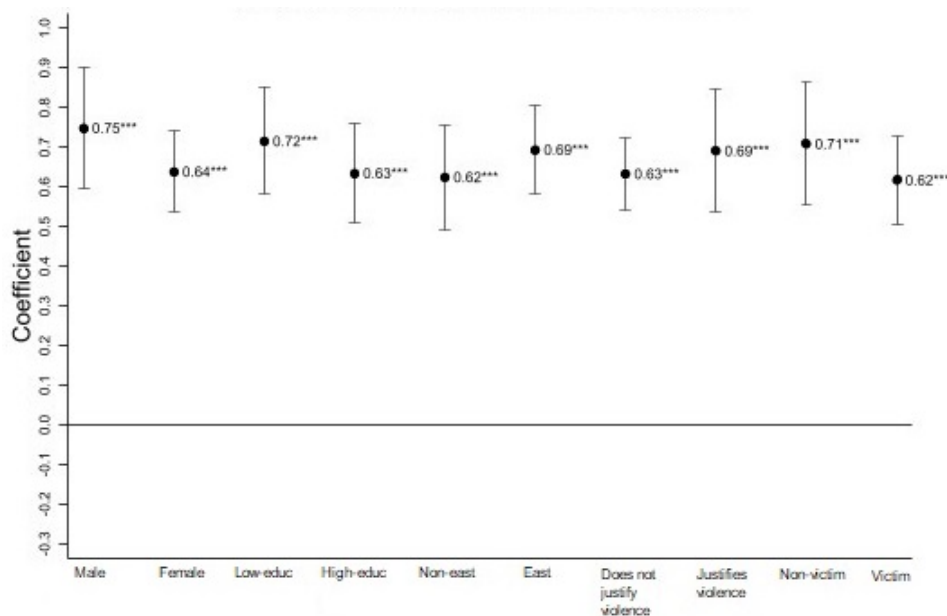
An important distinction is whether attribution of the sensitive statement reflects migrant-specific hostility or broader insecurity and pessimism. Baseline responses to the non-sensitive items indicate that perceived insecurity is already high in this population, and the list experiment isolates the marginal endorsement of the migration–insecurity link on top of that baseline. The fact that agreement remains substantial even among respondents without recent victimization and varies across demographic and spatial groups is consistent with the view that perceived migrant threat is not driven solely by direct crime exposure. This interpretation aligns with evidence showing that immigration attitudes are often shaped by sociotropic concerns and information frictions rather than narrowly defined personal experiences (Alesina et al., 2023; Hainmueller & Hopkins, 2014). It also echoes work documenting that immigration can increase crime-related concerns and defensive behaviors even when victimization does not change, highlighting a gap between objective conditions and perceived insecurity (Ajzenman, Dominguez, & Undurraga, 2023a). More generally, when beliefs are normatively charged, public narratives and shifts in perceived social acceptability can sustain or amplify expressed concerns independently of local exposure (Bursztyn, Egorov, & Fiorin, 2020).

2.5.1 Heterogeneity in the prevalence of perceived migrant threat

Figure 2.1 reports subgroup-specific estimates of the treatment effect of the list experiment, which, under the identifying assumptions, can be interpreted as the estimated prevalence of agreement with the statement that foreign immigrants make the city more insecure. The point estimates are uniformly large across groups. Among men, the implied prevalence is approximately 0.75, compared with women (0.64). By education, the estimate is about 0.72 for respondents with low education and 0.63 for those with college. Among residents of the Eastern zone, the coefficient is close to 0.69, compared to roughly 0.62 outside the East. In all cases, the estimates are statistically and economically significant.

The Figure 2.1 also shows that the perception studied is strongly present across the attitudinal and exposure-based categories. Respondents who report justifying violence exhibit an estimated prevalence of about 0.71, while even among those who do not justify violence, the estimate remains around 0.63. Similarly, individuals reporting prior victimization display an estimated prevalence of 0.62. Confidence intervals overlap across most subgroups, indicating that although point estimates vary between roughly 0.62 and 0.75, the differences are not sharply distinguished statistically.

Figure 2.1: Heterogenous effects



Source:

2.6 Perceived migrant threat and insecurity beliefs

Next, we examine whether individuals who endorse the sensitive statement in the list experiment differ systematically in other insecurity-related attitudes. Because the list experiment does not reveal individual-level agreement with the sensitive item, I restrict my attention to respondents

assigned to the treatment condition and relate the list experiment-based measure of perceived migrant threat to three binary outcomes using probit models. Table 2.6 reports probit estimates and average marginal effects linking this proxy for perceived migrant threat to insecurity-related outcomes.

Agreement with the sensitive statement is positively and statistically significantly associated with each outcome. Respondents who agree with the statement are 17 percentage points more likely to expect future victimization ($p < 0.001$). They are 11 percentage points more likely to justify the use of violence ($p < 0.001$). They are also 17 percentage points more likely to believe that criminal offenses are unlikely to be punished ($p < 0.001$).

These associations are statistically precise and remain significant in specifications that include geographic controls and demographic characteristics. The magnitude of the marginal effects is stable across specifications. However, we emphasize that these estimates are descriptive. The list experiment identifies agreement with a sensitive belief, but the regressions capture conditional associations between that belief and other attitudes related to insecurity. The results do not establish a causal relationship.

Table 2.6: Probit estimates

	Expected future victimization	Justifies violence	Perceived impunity
	(1)	(2)	(3)
Revealed agreement with sensitive statement	0.172*** (0.029)	0.109*** (0.031)	0.169*** (0.029)
N	1033	1033	1033
% Correct answers	74.92	61.83	71.07

Notes: Agreement is defined as reporting the maximum item count in the treatment condition, which implies agreement with the sensitive statement. This constitutes a conservative measure of revealed agreement. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

However, as mentioned before, the proxy used in 2.6 is intentionally conservative: among treated respondents, only those reporting the maximum item count are classified as endorsing the sensitive statement, while all others are coded as non-endorsers. This coding rule minimizes false positives but necessarily discards information contained in intermediate counts, since treated respondents who report one less than the maximum may still agree with the sensitive item.

To make fuller use of the list-experiment responses, I complement the maximum-count proxy with a respondent-specific probability of endorsement in the following subsection.

Respondent-specific probability of endorsement

Using a likelihood-based model for item-count data, I estimate latent agreement with the sensitive statement as a function of observed covariates and compute, for each treated respondent, the implied probability of endorsing the sensitive item conditional on the observed count. Estimation follows the regression framework for the item count technique developed by Imai (2011)

and Blair and Imai (2012), and employs the respondent-level probabilistic measures documented by Glynn (2013).

The resulting measure is continuous on $[0,1]$, with a mean of 0.78 (s.d. 0.29) in the treated group, and it is strongly correlated with the maximum-count proxy $\rho = 0.87$, while not being mechanically identical to it. I then use this respondent-specific probability in place of the conservative indicator and re-estimate the probit models for the three insecurity outcomes in the treated sample.

Table 2.7 reports the corresponding marginal effects and shows that the respondent-specific probability of endorsement is positively and significantly associated with expectations of future victimization and perceived impunity (0.286 and 0.300; $p < 0.001$). The association with justification of violence is positive but not statistically significant (0.103).

Table 2.7: Heterogeneous treatment estimates

	Expected future victimization	Justifies violence	Perceived impunity
	(1)	(2)	(3)
Respondent-specific probability of endorsement	0.286*** (0.067)	0.103 (0.070)	0.300*** (0.061)
N	985	985	985

Notes: *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

The results are consistent with an interpretation in which perception of migrant threat is systematically associated with broader insecurity-related beliefs, including pessimistic expectations related to victimization and low perceived punishment. While the analysis is descriptive, the fact that endorsement is widespread and only modestly related to direct victimization is consistent with the idea that salient narratives can shape perceptions beyond objective exposure. This interpretation is aligned with evidence from Latin America documenting gaps between administrative crime patterns and crime-related misperceptions.

2.7 Robustness

I test the stability of the results. First, Likelihood ratio tests reject the joint null hypothesis that control variables are collectively insignificant ($p < 0.001$), indicating that their inclusion improves model fit. Estimating heteroskedastic probit specifications yields no consistent evidence that the variance of the latent index varies systematically with demographic characteristics; likelihood ratio tests fail to reject homoskedasticity. RESET-type functional form checks do not indicate substantial misspecification of the index. The results are also robust to alternative link functions, with logit and probit models yielding similar marginal effects. These exercises suggest that the findings are not driven by the functional form assumptions or the inclusion of particular controls.

External validity

In this first version of the instrument, it was not possible to include a direct question asking whether migrants increase insecurity. As a result, the study cannot internally quantify the gap between a direct measure and the indirect estimate produced by the list experiment, nor can it directly calculate the magnitude of social desirability bias for this specific item. Nevertheless, the estimated prevalence can be assessed through external validation by comparing it with surveys and prior studies that document attitudes toward Venezuelan migration and its perceived link to crime and insecurity in Colombia.

Available evidence suggests that perceptions of threat associated with Venezuelan migration have been substantial in Colombia. In a survey conducted in Cúcuta by Invamer in 2019, roughly two-thirds of respondents viewed migrants as a threat to the city.² They also cite national survey evidence indicating that 62% of Colombians held unfavorable views of Venezuelan migrants. Also [Oxfam International \(2019\)](#) reporting that 74 percent of Colombians believed that immigration increases crime and insecurity. These figures provide a useful benchmark for interpreting the magnitude of agreement recovered by the list experiment. More broadly, regional evidence from Latinobarómetro indicates that 54 percent of Latin Americans perceive migrants as having negative effects on their country and link migration to rising crime, underscoring the salience of migrant-related insecurity perceptions in the region ([IADB, 2024](#); [Latinobarometro Corporation, 2023](#)).

Taken together, these external references place the prevalence estimate reported in this paper within a plausible empirical range for Colombia in recent years. Future waves of the instrument will incorporate a direct question on the migration–insecurity link, which will allow us to explicitly measure the gap between direct and indirect responses, assess the magnitude of social desirability bias in this context, and more tightly harmonize comparisons with national surveys and the broader literature documenting divergences between objective crime patterns and perceived insecurity associated with immigration.

2.8 Conclusion

This paper provides evidence on the prevalence and correlates of perceived migrant threat in a major urban receiving context. Using a list experiment embedded in the 2024 SISBEN Scientific Survey in Cali, and based on survey data from nearly 2,000 respondents interacting with local public institutions, I estimate that approximately two-thirds of surveyed individuals agree with the statement that foreign immigrants make the city more insecure. The estimate is stable across specifications and robust to the inclusion of geographic, demographic, and exposure-related controls. Under standard identifying assumptions, this magnitude can be interpreted as a credible estimate of latent agreement with a socially sensitive belief within the surveyed population.

The estimate should nevertheless be interpreted conservatively. The design checks indicate

²See [Semana \(2019\)](#).

that the list experiment may be vulnerable to ceiling effects, as agreement with the non-sensitive insecurity items is high. In this setting, treated respondents who also agree with the sensitive item may have limited scope to reveal this agreement without reporting the maximum count, potentially attenuating the treatment-control difference. The estimated prevalence of approximately 66 percent should therefore be read as a lower bound on perceived migrant threat within the surveyed population, rather than as a citywide prevalence estimate.

The paper also shows that perceived migrant threat is positively associated with other insecurity-related attitudes. Individuals classified as endorsing the sensitive statement are substantially more likely to expect future victimization, to justify the use of violence, and to perceive low probabilities of punishment. These associations are economically large and robust across specifications. While descriptive, they suggest that the migration–insecurity link is embedded in a broader configuration of insecurity-related beliefs rather than operating as an isolated attitude.

At the same time, heterogeneity analysis further indicates that the belief is not confined to a narrow subgroup within the sample. Estimated prevalence exceeds 60 percent across gender, education, and spatial categories, with moderately higher levels among men, lower-educated respondents, residents of the Eastern zone, and individuals who justify violence. Although confidence intervals overlap, the results suggest broad-based endorsement within the surveyed population.

These findings have implications for local integration policy. The relevant policy lesson is not that migrants increase crime, but that beliefs linking migration to insecurity are prevalent among socioeconomically vulnerable respondents. Policies that focus only on legal status or access to services may therefore be incomplete if they do not also address the informational and institutional environment in which migration is interpreted. A natural policy response is to combine transparent communication about crime and migration data with local trust-building strategies and visible institutional responses to insecurity that benefit both migrants and host communities. Evidence from Chile shows that immigration can increase crime-related concerns even when victimization does not rise, suggesting that perceptions may diverge from objective security conditions ([Ajzenman, Dominguez, & Undurraga, 2023b](#)). Experimental evidence further suggests that information can shift attitudes toward immigrants, but that these effects are heterogeneous and depend on the content, credibility, and target population of the intervention ([Facchini, Margalit, & Nakata, 2022](#); [Grigorieff, Roth, & Ubfal, 2020](#); [Haaland & Roth, 2020](#)).

More broadly, the findings underscore the value of indirect measurement in contexts where attitudes linking migrants to crime are normatively sensitive and may be misreported under direct questioning. The design checks also point to a concrete improvement for future survey waves: using control items with more heterogeneous expected agreement, such as items related to traffic congestion, public transportation, parks, or neighborhood amenities, to reduce maximum item counts and improve variation in the sensitive belief. A natural next step is to move from measuring perceived threat to measuring misperceptions by combining administrative data on crime and migrant presence at the neighborhood or comuna level with survey measures of beliefs about migrant involvement in crime, victimization, and local security trends. Embedding

randomized information interventions in this design would allow future work to distinguish general insecurity concerns, anti-migrant attitudes, and factual misperceptions, and to test whether correcting inaccurate beliefs affects attitudes toward migrants, support for integration policies, or demand for punitive responses.

Chapter 3

Venezuelan migration and Colombian labor market dynamics: Does COVID-19 shape the migration impacts?

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Abstract

The ongoing Venezuelan migration crisis has had a profound impact on labor markets in host countries, particularly Colombia, which has seen a significant influx of migrants since 2016. While existing research has explored the effects of migration on local labor markets, the intersection between migration and external economic shocks, such as the COVID-19 pandemic, remains under-examined. This study aims to fill this gap by analyzing the effects of Venezuelan migration on Colombia's labor market from 2016 to 2021, focusing on how the pandemic has amplified these impacts. Using an event study design combined with instrumental variables to

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address potential endogeneity, we find that migration increased labor informality, reduced employment, and decreased wages for local workers, with these effects diminishing over time. The pandemic further exacerbated these vulnerabilities, though migrants with longer residency in Colombia showed greater economic resilience. Additionally, the study highlights heterogeneous impacts across various population groups, with young workers, women, and the less educated facing disproportionately adverse effects. This research contributes to the literature on migration and labor market adaptation, offering insights into the evolving dynamics of migration shocks in the context of global crises.

Keywords: Venezuelan migration, labor market, COVID-19 pandemic.

JEL Codes: F22, O15, R23.

3.1 Introduction

Massive migration exerts significant pressure on social and economic systems, increasing the demand for services and employment opportunities. Effective assimilation is crucial to alleviating these pressures by facilitating migrants' access to essential services and their integration into the labor market. This process also benefits local populations by reducing labor market distortions and contributing to economic growth and public finances. Furthermore, successful assimilation enhances resilience to economic shocks by fostering a more inclusive and adaptive economy. The COVID-19 pandemic, as a large-scale shock, has likely driven resource reallocation across various sectors, disproportionately affecting vulnerable populations. These dynamics are particularly critical in countries with high immigration rates, where populations are more exposed to economic and social vulnerabilities.

Venezuelan migration to Colombia represents one of the most significant demographic shifts in recent Latin American history, profoundly impacting the host country's labor market dynamics. Since 2016, millions of Venezuelans have crossed into Colombia seeking better opportunities, introducing new challenges and complexities to employment, informality, and wage structures. While migration has often been associated with both opportunities and disruptions, its effects are particularly critical in economies with high unemployment rates and pronounced labor informality, as is the case in Colombia, the country that hosts 2.5 million of Venezuelan migrants ([National Planning Department, 2022](#)).

Existing research has highlighted diverse outcomes of Venezuelan migration, such as its influence on local wages, employment, and labor market participation. However, little is known about how these effects evolve over time or how external shocks, like the COVID-19 pandemic, reshape these dynamics. The pandemic, a global crisis with far-reaching economic and social implications, intensified vulnerabilities within labor markets, disproportionately affecting migrants due to their precarious employment conditions and limited access to social safety nets. This knowledge gap necessitates an in-depth examination of how migration and crises intersect

to shape labor market outcomes.

This study seeks to address the existing research gap by examining the impacts of Venezuelan migration on Colombia's labor market between 2016 and 2021, with a particular emphasis on how the COVID-19 pandemic influenced these dynamics. Utilizing data from household surveys, national censuses, and an innovative migration survey, the analysis focuses on the short- and medium-term effects of migration on employment, wages, labor force participation, and informality. Furthermore, the study delves into the social and economic assimilation of migrants, shedding light on the mechanisms that mitigate their labor market vulnerabilities over time.

This research provides a comprehensive analysis of the dynamic interactions between migration, labor markets, and external shocks, contributing significantly to both academic discourse and policy development. It highlights the critical need for resilient labor market policies designed to address the specific challenges faced by migrants and vulnerable populations, particularly during times of economic instability or disruption.

The findings of the study demonstrate substantial effects of migration on the labor market of both local workers and prior migrants. Utilizing instrumental variable (IV) estimates to address endogeneity, it is ascertained that an augmentation of 1 percentage point in the migration rate results in a diminution of 0.76 percentage points in the employment probability of locals, accompanied by an escalation of 3.9 percentage points in the informality rate. Furthermore, the study found a 3.88% decrease in the wages of locals in 2017, with a progressive decline observed in subsequent years. For those who had previously migrated, the initial arrival of new migratory waves led to an increase in their probability of employment (0.57 percentage points in 2017). However, this effect was reversed in 2018 (-3.25 percentage points), a year during which the Venezuelan exodus intensified, accompanied by an increase in unemployment by 2.55 percentage points and a wage decline of 7.96%.

Concerning the process of assimilation, it has been observed that each additional year of residence in Colombia has been associated with an increase in labor force participation and employment among migrants, leading to a reduction in unemployment and informality. However, wage disparities compared to local workers persist. Furthermore, migrants who have spent more time in the country have demonstrated greater economic resilience during the pandemic, better adjusting their finances and experiencing a reduced impact on their employment from the crisis. The heterogeneous analysis indicates that youth, women, less educated individuals, and informal workers are the most affected by migration, with more pronounced reductions in employment and wages, especially during the pandemic.

Our results show that the impact of migration varies significantly over time, with additional effects on unemployment, working hours, and informality as consequence of the pandemic. These effects are heterogeneous among population groups, which underscore the relevance of considering disparities between groups in the policy design. Moreover, earlier migrants face adverse effects, but the labor market has adapted over time, mitigating some of the impacts.

Although the impacts of migration are significant, they diminish over time, indicating a certain level of labor market adaptation to the migration shock. This can be explained by a process of migrant assimilation, for which we present evidence. Specifically, migrants have significantly improved their labor market outcomes and social indicators since their year of arrival.

The rest of the paper is organized as follows. Section 3.2 briefly presents related literature, while Section 3.3 provides a general context on the immigration from Venezuela to Colombia. Section 3.4 describes the data and presents the empirical strategy. We present results in Section 3.5. Finally, concluding remarks are presented in Section 3.6.

3.2 Related literature

The effects of migration on labor markets have been widely studied. However, to gain a comprehensive understanding of how labor markets are affected, it is essential to examine the process of migrant assimilation. Higher levels of assimilation lead to lower distortions in local labor markets. Consequently, our article contributes to both the literature on labor market impacts and the studies focused on the factors that determine migrant assimilation. Furthermore, migrant assimilation plays a key role in ensuring resilience to economic shocks. Therefore, our findings are also relevant to the literature examining the impacts of aggregate shocks in high-migration contexts, particularly in the case of the COVID-19 pandemic. Our paper provides insights into the intersection of migration, economic shocks, and labor market assimilation.

Therefore, our literature review covers three branches of literature. First, we examine foundational studies on migration and labor markets, emphasizing methodological approaches and key findings. Second, we describe studies on the determinants of migrant assimilation. Finally, the emerging literature on the effects of the COVID-19 pandemic on labor markets, focusing on its disproportionate impacts on vulnerable populations, including migrants is also reviewed.

Pioneering work on the impact of migration on labor markets focuses on its influence on low-skilled local workers. For instance, Card (1990) analyzed the impact of the Mariel Boatlift in Miami on the labor market outcomes of locals, which concluded that large migratory inflows had no significant effects on wages or unemployment among low-skilled local workers. However, subsequent research suggested that migrants might directly compete with low-skilled locals, leading to wage reductions while benefiting high-skilled workers (Borjas & Katz, 2007; Ottaviano & Peri, 2012). In developing countries, the impacts of migration are diverse and tend to be concentrated among vulnerable groups, such as informal workers, young women, and those with lower levels of education (Aksu, Erzan, & Kırdar, 2022; Atta, Chort, & Senne, 2022; Ceritoglu, Yunculer, Torun, & Tumen, 2017; Tumen, 2016).

The significant exodus from Venezuela has also prompted numerous studies, most of which analyze South-to-South migration. For Peru, Ecuador, and the US, minimal effects have been found on hours worked, employment status, wages, and labor force participation (Boruchow-

icz, Martinelli, & Parker, 2021; Gunadi, 2021; Olivieri, Ortega, Rivadeneira, & Carranza, 2022; Vera & Jiménez, 2022). In contrast, in the case of Brazil, Shamsuddin, Acosta, Schwengber, Fix, and Pirani (2021) and Ryu and Paudel (2022) report declines in employment, particularly concentrated among women and those in self-employment. Colombia has been a focal point of research on the effects of Venezuelan migration, with studies showing mixed results. Research by Caruso et al. (2021); Delgado-Prieto (2020); Santamaria (2019), and Lebow (2021) indicates that migration has had negative impacts on both wages and employment. Delgado-Prieto (2020) suggests that these effects result from a reallocation of workers between the formal and informal sectors. In contrast, Bonilla-Mejía, Morales, Hermida-Giraldo, Flórez, et al. (2020) and Muñoz-Mora, Aparicio, Martinez-Moya, and Urbano (2022) find no significant impact on the unemployment rate among local workers. Lastly, when examining gender-specific effects, Pedrazzi and Peñaloza-Pacheco (2023) finds that Venezuelan migration decreased employment for low-skilled local women while increasing employment for high-skilled locals.

Our second strand of related literature focuses on the assimilation process of migrants. Studies in this line analyze the convergence over time of economic and social outcomes between migrants and locals (Albert, Glitz, & Llull, 2021; Borjas, 1985; Cadena, Duncan, & Trejo, 2015; Fernández & Ortega, 2008; LaLonde & Topel, 2007). For instance, Chiswick (1978) and Borjas (1985) present evidence of wage convergence between immigrants and locals in the U.S. over a span of 10 to 15 years. In turn, Fernández and Ortega (2008), Demirci and Kırdar (2023) and Lee, Peri, and Viarengo (2022) extend the analysis to participation and employment, demonstrating that the gaps in these indicators are relatively small.

Some studies have focused on understanding the magnitude of the gaps between locals and migrants. On one hand, it has been argued that this magnitude depends on the size of the migration shock and the substitution between local and immigrant labor (Albert et al., 2021; LaLonde & Topel, 2007). On the other hand, it is also crucially influenced by the time since migrants' arrival (Aydemir & Duman, 2021; Bacolod & Rangel, 2017; Friedberg, 1992). Other factors determining assimilation have also been studied country or region of origin (Amuedo-Dorantes & De la Rica, 2007; Rendall, Rabinovich, Janta, Rubin, & Tsang, 2008), the economic conditions at the time of arrival (Barsbai, Steinmayr, & Winter, 2022), and the regularization and citizenship policies (Gathmann & Keller, 2018).

While previous research has primarily focused on wages and employment, it has often overlooked the impact of economic shocks on labor market outcomes. This paper seeks to address this gap by examining the evolving effects of migration within the context of such unprecedented disruptions. In this regard, the third body of related literature focuses on studies assessing the pandemic's impact on labor markets, particularly within migrant populations. These studies have predominantly explored the effects on migrants, rather than local workers. Research by International Labour Organization (2020), Béland, Brodeur, and Wright (2023), and Morando (2021) indicates that the COVID-19 pandemic had a substantial negative impact on informal migrants, exacerbating issues such as employment rates, working hours, and wages, which can be attributed to occupational segregation (for further discussion see Atkeson, 2020; Dustmann,

Glitz, & Vogel, 2010; Jordá, Singh, & Taylor, 2020; Orrenius & Zavodny, 2010; Ramelli & Wagner, 2020; Sinha, Narain, & Bhanjdeo, 2022).

3.3 Context

The Venezuelan migration crisis represents one of the most significant demographic and socioeconomic shifts in recent Latin American history. Beginning in 2015, the collapse of the Venezuelan economy, compounded by political instability and hyperinflation, led millions of Venezuelans to seek refuge in neighboring countries, with Colombia becoming the primary destination. The most significant increase in the share of Venezuelan migrants occurred in 2018, marking the peak of the exodus as the crisis deepened. By 2020, Colombia was home to approximately 1.8 million Venezuelan migrants, creating substantial challenges and opportunities for both the migrants and the host country. The scale and pace of this migration, particularly after 2016, reshaped the Colombian labor market, raising questions about labor market integration, social assimilation, and the resilience of migrant populations to external shocks, such as the COVID-19 pandemic.

The COVID-19 pandemic, which began to affect Colombia in March 2020, further complicated the integration process for migrants. With strict lockdown measures and widespread economic disruption, the pandemic disproportionately impacted vulnerable groups, including migrant populations. This context provides an essential backdrop for examining the intersections between migration, labor market outcomes, and external shocks in Colombia. As this study will explore, the effects of Venezuelan migration on the Colombian labor market evolved over time, with significant implications for both local workers and migrants themselves, especially during the pandemic.

3.3.1 Immigration from Venezuela to Colombia

In the last two decades, the Venezuelan government raised expenditures based on oil revenues. However, in 2014, the oil price declined, and consequently the Venezuelan economy contracted dramatically (Alhadeff, 2021; Lombardo et al., 2021; OECD, 2018). This shock led to a rapid increase in the inflation rate, a shortage in basic needs, and reduced GDP growth (Caruso et al., 2021; Ramoni Perazzi et al., 2017).¹ This was coupled with political issues between the Colombian and Venezuelan governments. In 2015, the president of Venezuela, Nicolás Maduro, closed the border between Colombia and Venezuela, causing significant economic losses. However, in July 2016, it was re-opened due to protests and international pressures.

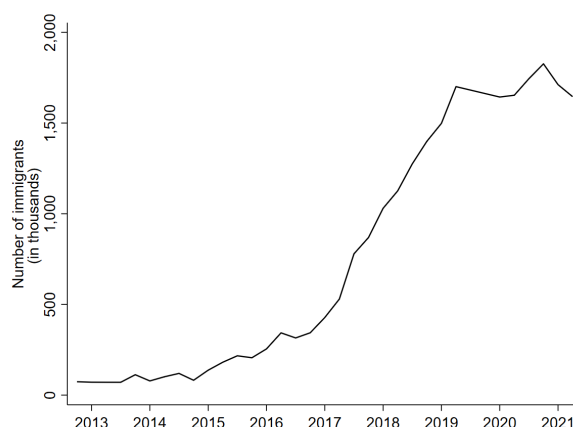
As a result of a worsening socioeconomic and political situation, emigration from Venezuela increased substantially until it became the largest exodus in Latin American history and the second-largest worldwide after the Syrians. Between 2015 and 2019, around 5 million Venezue-

¹Venezuelan inflation rate was 929.8% in 2019 (Shamsuddin et al., 2021).

lans migrated to neighboring countries such as Colombia, Peru, Ecuador, Chile, Brazil, and Argentina (Wolfe, 2021b). The two main destinations for Venezuelan immigrants outside Latin America in 2018 were Spain and the U.S. (Inter-American Development Bank & OECD, 2021). Figure 3.1 illustrates the stock of Venezuelan immigrants in Colombia from 2015 to 2020, showcasing the dynamics of migration over this period. A notable trend in the data is the sharp increase in the stock of Venezuelan migrants around 2018, which stands out as the year with the most significant growth. Prior to this year, the number of Venezuelan immigrants remained relatively stable, with gradual increases. However, starting in 2018, the figures surge dramatically, reflecting the intensification of the Venezuelan exodus due to the deepening of the economic and political crises in Venezuela.

This year marks the peak of the migration wave, as millions of Venezuelans were forced to leave their country in search of better opportunities and refuge in neighboring nations, particularly Colombia. The rapid increase in migration during 2018 substantially altered the demographic and labor dynamics in Colombia. This shift forms the basis of our hypothesis, which posits that it had a profound impact on both the local population and the newly arrived migrants, with distinct differences observed during the pandemic.

Figure 3.1: Stock of Venezuelan immigrants in Colombia



Notes: The sample is restricted to working age-immigrants. We apply linear interpolation techniques for the first and second quarters of 2020 at the department-quarter level to address the missing data issues that affected household survey collection during the COVID-19 pandemic confinement. *Source:* Authors' calculations using the national household survey (GEIH).

In 2020, the migration trend experienced a slight decline, largely due to the global COVID-19 pandemic. The pandemic severely disrupted migration patterns, causing many Venezuelans to return to Venezuela. Between March and August 2020, approximately 95,000 Venezuelans returned home, largely driven by job losses, economic insecurity, and restrictions on mobility (Graham & Guerrero, 2020; Migración Colombia, 2020). Despite this dip, the stock of Venezuelan migrants in Colombia remained substantial, and the long-term impacts of the 2018 migration wave continued to shape the socio-economic landscape in the country.

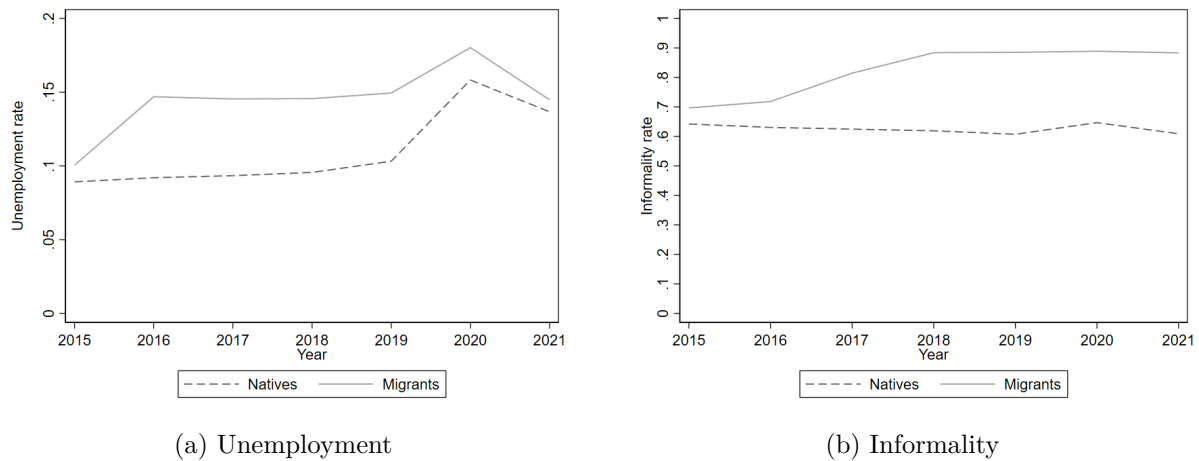
3.3.2 COVID-19 in Colombia

The COVID-19 pandemic began globally in December 2019, but Colombia did not report its first confirmed case until March 6, 2020 (Rosselli, 2020).² On March 23, the Colombian government implemented a nationwide mandatory lockdown, which included school closures, a ban on mass events, and various other restrictions. These measures were part of a broader wave of social distancing policies implemented across Latin America in March 2020, including lockdowns, school closures, and the cancellation of public events (Aromí et al., 2023). Colombia's lockdown measures were among the strictest globally (Cárdenas, Beltrán, et al., 2020).. After 150 days, on August 30, the government began a phased reopening of public establishments. By October 2020, Colombia had the second-highest cumulative case count in Latin America and the fifth-highest globally (World Health Organization, 2020) and economic losses are estimated to range between 5% and 6.1% of GDP (Bonet-Morón et al., 2020). Throughout the pandemic, the Colombian government introduced several economic relief strategies, including cash transfers, tax deferrals, and subsidies for businesses and low-income households to mitigate the adverse economic effects. Immigrants were also included as beneficiaries of cash transfers, healthcare services, and access to COVID-19 testing and treatment at public hospitals.

The COVID-19 pandemic exerted substantial pressure on both the healthcare system and the labor market. In the labor market, its effects were especially severe for workers in informal employment, many of whom were unable to transition to remote work and lacked access to social protection mechanisms (Rodríguez-Lesmes, Jaramillo, Londoño, & García-Suaza, 2020). Figure 3.2 illustrates the evolution of unemployment and informality rates for both locals and immigrants between 2015 and 2021. As shown in the figure, unemployment and informality rates were similar in 2015, but disparities began to widen after 2016. This trend persisted until 2020, when the pandemic narrowed the gap between locals and immigrants. Notably, the increase in unemployment was more pronounced among immigrants compared to locals. Conversely, while immigrants maintained relatively stable informality rates, locals experienced a significant rise in informality in 2020. These patterns suggest that the impact of COVID-19 on employment status differed between locals and immigrants.

²COVID-19 is a viral infection that can lead to severe respiratory syndrome, including pneumonia (Coccia, 2020). While some infected individuals remain asymptomatic, the majority exhibit symptoms such as fever, cough, loss of taste and smell, muscle pain, fatigue, diarrhea, and difficulty breathing (Malagón-Rojas et al., 2021).

Figure 3.2: Unemployment and informality rates of locals and immigrants



Source: Authors' calculations using the national household survey (GEIH).

In 2019, around 90% of employed Venezuelans worked as informal compared to the 60% of Colombians. An important fact is that sectors with high informality tend to be the most affected by COVID-19 (Dempster et al., 2020). According to Graham and Guerrero (2020), using data from Colombian household surveys of 2020, it was found that 64% of Venezuelan immigrant workers were employed in the sectors that were hit hardest by the pandemic. These sectors included accommodation and food services, manufacturing, real estate, and wholesale and retail trade. In comparison, only 47% of Colombians were working in these sectors. Moreover, immigrants made up only 3% of the workforce in sectors that were least affected by the pandemic, such as education, human health, social work, public administration, defense, and utilities, compared to the 13% of locals who worked in these sectors.

3.4 Data and empirical strategy

3.4.1 Data

Our analysis draws on three primary data sources: the National Household Survey (Gran Encuesta Integrada de Hogares, GEIH), the 2005 Colombian National Census, and a novel migration survey. The GEIH, administered by DANE, is a monthly cross-sectional survey that includes data from both rural and urban areas. It provides comprehensive information on population demographics, housing characteristics, employment conditions, and migration patterns. For this study, we use GEIH data spanning from 2016 to 2021, covering the significant migration wave of 2017 as well as the pre- and post-pandemic periods.

The key labor market outcomes examined in this study include employment, unemployment,

labor force participation, weekly hours worked, hourly wages, and informality.³ Second, we construct an instrumental variable based on data from the 2005 National Census, utilizing historical settlement patterns to predict the current immigration rate. This methodology, inspired by [Card \(2001\)](#) and [Caruso et al. \(2021\)](#), uses the proportion of immigrants in a given department (state) as a predictor for the contemporary geographical distribution of immigrants. The relevance and validity of this instrument are discussed in detail in the following section.

Furthermore, we incorporate data from the Migration Pulse (EPM, for its Spanish acronym), a newly established survey specifically targeting the immigrant population. EPM is a bimonthly, nationally representative phone survey, structured as panel data, which covers 23 of Colombia's largest cities. The sample includes immigrant respondents identified in the GEIH (Gran Encuesta Integrada de Hogares) during the first semester of 2021. Since its launch in July 2021, the survey has been administered by DANE. EPM offers comprehensive insights into various dimensions of immigration, such as the migration process, integration into Colombian society, and future outlooks. This dataset is especially valuable for investigating the mechanisms of social and economic assimilation.

We assess the impact of immigration on labor market outcomes by analyzing annual cohorts from the GEIH dataset. Table 3.1 presents descriptive statistics for our sample, highlighting key labor market indicators and individual characteristics of both locals and immigrants across three distinct periods: 2016 (prior to the largest wave of immigration), 2018 (following the immigration shock but before the pandemic), and 2020 (during the pandemic). The data reveal a substantial increase in immigration, with the number of working immigrants growing tenfold between 2016 and 2020.

In general, Venezuelan immigrants exhibit poorer labor market outcomes compared to local workers. Specifically, immigrants face higher unemployment and informality rates and receive lower wages. These disparities are consistent with a significant 43% decrease in average real labor income over the analysis period. Notably, the differences between locals and immigrants are statistically significant, as confirmed by the results of a mean difference test.

³Wages are considered for salaried workers, while labor income is analyzed for employers and self-employed individuals. Labor force participation is defined as a binary variable, with a value of one if the respondent is either employed or unemployed. For calculations of hourly labor income, domestic workers, unpaid family workers, and unpaid employees in businesses or households are excluded. Informality is defined according to the criteria outlined in [OECD \(2019\)](#).

Table 3.1: Socioeconomic characteristics of locals and immigrants, before and after the massive immigration

	2016			2018			2020		
	Locals	Immigrants	Mean Diff.	Locals	Immigrants	Mean Diff.	Locals	Immigrants	Mean Diff.
Panel A. Labor market outcomes									
Employment rate	68.56	63.39		67.07	69.89	***	61.24	66.57	***
Unemployment rate	8.49	17.64	***	9.38	14.93	***	14.17	14.98	*
Labor participation	74.93	76.97	**	74.01	82.16	***	71.35	78.29	***
Weekly hours worked	44.92	51.66	***	44.48	50.40	***	44.41	48.80	***
Monthly labor income	949,732	1,177,981	**	1,001,135	659,331	***	967,845	631,604	***
Informality rate	61.57	86.56	***	60.52	89.15	***	59.91	89.79	***
Panel B. Individual characteristics									
Age	35.94	27.10	***	36.31	29.76	***	36.61	30.64	***
Female	51.17	48.28	**	51.24	49.50	***	51.05	50.46	**
Education									
% None	3.59	1.63	***	3.45	1.33	***	2.82	1.17	***
% Primary	21.98	8.54	***	21.01	7.05	***	19.36	12.16	***
% Secondary	48.09	58.77	***	49.34	63.45	***	50.29	64.56	***
% Higher education	26.30	31.06	*	26.19	28.16	***	27.53	22.08	***
Observations	128,713	486		122,372	3,165		118,505	5,097	
Population	30,936,054	125,890		31,056,281	811,015		31,067,146	1,448,191	

Notes: Calculations are based on working age-population. We use information from the fourth quarter of every year. Monthly labor income is at constant prices of 2018. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. *Source:* Authors' calculations using the national household survey (GEIH).

Between 2016 and 2018, there was a marked shift in the educational profile of Venezuelan immigrants. However, a comparison of immigrant characteristics between 2018 and 2020 reveals no significant changes in age, gender, or educational attainment. By 2020, as anticipated, unemployment increased for both immigrants and locals, with the latter group experiencing a more pronounced rise. Additionally, average labor income decreased by 1.5% for locals and 3.4% for immigrants, suggesting different mechanisms of labor market adjustment during the pandemic. While locals were primarily affected by a "quantity effect", evidenced by rising unemployment, immigrants appeared to experience a "price effect," reflected in a sharper decline in labor income.

To further investigate the pandemic's impact, we replicated Table 3.1 for the period from 2019 to 2021. Table 3.2 provides a detailed examination of the labor market adaptation of recent and earlier-term immigrants. This distinction is relevant because descriptive evidence for Colombia suggests that more recently arrived Venezuelan migrants were already less integrated into the labor market prior to the pandemic (Guataquí et al., 2017). In 2019, recent immigrants faced unemployment rates that were twice as high as those of both locals and earlier-term immigrants, along with substantial income disparities. Their monthly labor income was 20% lower than that of earlier-term immigrants and nearly 50% lower than that of locals. The onset of COVID-19 significantly exacerbated these disparities. While the unemployment rate for locals doubled, earlier-term immigrants saw a 16.4 percentage point increase in unemployment—nearly three times the 2019 rate. In contrast, the unemployment rate for recent immigrants rose more modestly by 12.1 percentage points. E

The pandemic also triggered significant shifts in employment-intensive margins. Average weekly hours worked declined by approximately five hours for both categories of immigrants, whereas locals showed only minimal variation. These reductions in work hours were consistent with a notable drop in labor income, further highlighting the disproportionate economic impact of the pandemic on immigrants.

Table 3.2: Socioeconomic characteristics of locals and immigrants, before, during and after pandemic

	2019			2020			2021		
	Locals	Recent	Earlier-term	Locals	Recent	Earlier-term	Locals	Recent	Earlier-term
	Immigrants	Immigrants		Immigrants	Immigrants		Immigrants	Immigrants	
Panel A. Labor market outcomes									
Employment rate	65.59	59.07	71.88	51.86	38.46	50.62	54.84	51.15	62.69
Unemployment rate	10.07	23.77	10.98	20.80	35.88	27.42	15.19	31.55	16.67
Labor participation	72.94	77.49	80.75	65.48	59.98	69.74	69.38	74.73	75.23
Weekly hours worked	44.09	48.37	51.44	44.47	43.47	46.36	44.50	50.27	50.38
Monthly labor income	994,744	519,292	643,195	842,068	415,312	474,315	949,345	536,480	628,804
Informality	58.25	94.74	86.96	61.94	95.45	90.74	58.25	96.14	88.93
Panel B. Individual characteristics									
Age	36.49	30.33	29.67	36.56	29.90	29.97	36.73	29.25	30.80
Female	51.04	53.34	49.06	50.95	51.68	53.41	51.00	51.54	50.53
Education									
% None	3.44	0.93	1.37	2.89	1.30	1.23	3.06	0.36	1.02
% Primary	20.24	10.66	9.27	18.88	12.15	7.62	18.89	7.80	7.83
% Secondary	48.31	62.06	64.30	51.18	63.84	66.62	49.34	77.07	69.37
% Higher education	28.01	26.34	25.05	27.05	22.72	24.53	28.70	14.77	21.75
Observations	81,848	1,268	1,885	83,888	399	2,810	76,074	394	3,165
Population	30,834,871	502,306	660,722	31,003,338	191,849	1,059,776	31,043,477	172,579	1,495,555

Notes: Data used for estimations corresponds to the May and June for every year. Calculations are based on the working-age population. Monthly labor income is in 2018 at constant prices. Source: Authors' calculations using the GEIH, DANE.

In 2021, all three populations experienced a significant reduction in unemployment rates, with earlier-term immigrants showing the most pronounced recovery, reducing their unemployment rate by more than 10 percentage points. This suggests that a longer duration of residence may enhance labor market adaptation, although their response to the economic shock was marked by considerable volatility. Furthermore, there was a notable improvement in the intensive margin of employment for immigrants, with the average weekly hours worked surpassing 50 in both groups. Finally, labor income exhibited an overall increase, with earlier-term immigrants experiencing the greatest benefit.

3.4.2 Identification strategy

Our primary objective is to assess whether the impact of migration on labor market outcomes varies over time, with a particular focus on determining whether this impact shifts as a result of the COVID-19 pandemic. Specifically, we aim to analyze how the pandemic, beginning in 2020, altered pre-existing migration impacts on labor outcomes. By 2019, migration had already influenced the Colombian labor market, but the arrival of the pandemic likely introduced

additional distortions, affecting labor market adaptation. To address this, we employ an event study specification combined with an instrumental variable strategy to mitigate endogeneity between migration influxes and labor market performance.

Our analysis uses 2017 as the baseline period, which represents an inflection point preceding the substantial influx of Venezuelan migrants in 2018, prompted by Venezuela's escalating economic and social crisis. By 2018, the migrant population in Colombia had exceeded one million and continued to grow rapidly, providing a context for assessing the combined effects of migration and the pandemic. Moreover, our analysis extends beyond 2019, providing complementary insights to [Caruso et al. \(2021\)](#) and [Delgado-Prieto \(2020\)](#).

In particular, we estimate Equation 3.1 given by:

$$Y_{idrt} = \alpha + \beta_0 M_{dt} + \sum_{\tau=2016, \tau \neq 2017}^{2021} \beta_\tau M_{dt} D_{\{\tau=t\}} + \gamma_t + \gamma_r + \delta' X_{idt} + \epsilon_{idrt} \quad (3.1)$$

where i denotes an individual, d represents a department, r corresponds to a region, and t refers to a specific year. The variable Y_{idrt} represents the labor market outcomes of interest, including labor status (employment, unemployment, and labor force participation) and job quality indicators (logarithm of weekly hours worked, logarithm of hourly wage, and informality). The immigration rate M_{dt} is defined as the proportion of working-age immigrants in department d during period t , relative to the working-age population in the same department.

β_0 represents the labor market effects of immigration in 2017, while β_τ captures the time-varying impact of immigration rates relative to 2017. The variable X_{idt} denotes a set of individual characteristics, including gender, age, age squared, education, a dummy variable for head of household status, a dummy for rural residence, the household dependency ratio, GDP growth at the department level between 2015 and 2020, and the unemployment rate. The last two variables control for pull factors influencing immigration settlement decisions (see [Delgado-Prieto, 2020](#)). Additionally, γ_t and γ_r represent year-fixed effects and region-fixed effects, respectively, while ϵ_{idrt} is the error term.

The immigration rate is potentially endogenous due to immigrants' self-selection of settlement locations based on unobservable factors such as economic opportunities, social networks, violence, or political conditions (see [Bohnet, Peralta, & dos Santos, 2021](#); [Card, 1990](#); [Caruso et al., 2021](#); [Del Carpio & Wagner, 2015](#); [Di Maio, Sciabolazza, & Molini, 2023](#)). To estimate the causal impact of immigration, we construct an instrumental variable that combines historical immigrant settlement patterns, the shift in the total migrant population and the shortest distance to an official Venezuelan entry point.⁴ This type of instrument is known as a shift-share

⁴Colombia has eight official entry points for Venezuelans: Paraguachón (La Guajira - Zulia), international bridges: La Unión (Norte de Santander - Táchira), Francisco de Paula Santander (Norte de Santander - Táchira), Tienditas (Norte de Santander - Táchira), Simón Bolívar (Norte de Santander - Táchira), José Antonio Páez (Arauca - Apure), Puerto Carreño river port, and Inírida fluvial immigration checkpoint. Using coordinates provided by Migración Colombia, we calculate the distance from the centroid of each department to each entry point and use the minimum distance as the relevant measure.

or Bartik instrument (see [Bartik, 1991](#)). The intuition behind a shift-share instrument in the context of migration is to use a combination of two key components. First, a shift, representing spatial variations in immigration flows driven by external shocks or exogenous factors. And second, a share, reflecting the historical distribution of immigrants across locations. By interacting these elements, the instrument isolates variation in immigration rates that is plausibly exogenous to local labor market conditions.

In our case, the instrumental variables is defined as:

$$IV_{dt} = \frac{\theta_{d2005}}{T_{bd}} P_t \quad (3.2)$$

Here, θ_{d2005} is the share of Venezuelans in each department based on the 2005 national census, and P_t is the total Venezuelan immigrant stock in Colombia in year t , b refers to the Venezuelan border, T_{bd} is the minimum distance (in kilometers) between the department's centroid and an official entry point. Thus, IV_{dt} can be seen as a prediction of the number of immigrants arriving in department d in year t .

In the context of shift-share IVs, concerns regarding endogeneity depend on the 'share' component of the instrument. [Goldsmith-Pinkham, Sorkin, and Swift \(2020\)](#) argue that Bartik instruments assume pooled exposure, where the shares reflect differential exposure to the treatment ([Borusyak, Hull, & Jaravel, 2022](#)). Regarding the shifts, the Venezuelan migration wave was primarily driven by economic and political crises in Venezuela rather than labor demand conditions in Colombia. To ensure exogeneity, historical settlement patterns must be unrelated to other factors affecting immigrants' decisions, such as serially correlated labor market demand shocks ([Cho, 2019](#); [Jaeger, Ruist, & Stuhler, 2018](#)). Such shocks could introduce endogeneity if unobservable factors influence both economic conditions and immigrant settlement patterns ([Costas-Fernández, 2018](#); [Del Carpio & Wagner, 2015](#)). In our case, the shares are based on Venezuelan settlement patterns from the 2005 Census, which, while predating the recent migration crisis, are potentially uncorrelated with current economic conditions. However, our specification also includes controls for regional business cycles and labor demand (e.g., GDP growth and unemployment rates), as well as year-fixed and region-fixed effects to mitigate any additional concerns.

The instrument's relevance is grounded in the tendency of immigrants to settle in areas with established immigrant communities, as discussed in [DaVanzo \(1983\)](#); [Hoover, Giarratani, et al. \(1999\)](#); [Rodríguez-Pose and von Berlepsch \(2020\)](#). This persistence is also consistent with evidence for Colombia showing that migration flows are shaped by geographic proximity, regional characteristics, and existing population distributions ([Galvis-Aponte, 2002](#)). Appendix C.1, Figure C.1.1, compares the distribution of Venezuelan immigrants in 2005 and 2018, showing that regions near the border host significant proportions of immigrants, along with major cities such as Bogotá, Medellín, Cali, and Barranquilla. The correlation between the share of Venezuelan immigrants in 2005 and 2018 is 0.87, supporting the hypothesis that past settlement patterns predict immigration flows.

To test the instrument's relevance, Table 3.3 presents first-stage estimates for recent and earlier-term immigration rates, while Appendix C.2, Table C.2.1, reports F-tests for all endogenous variables.⁵ The estimates show a statistically significant positive association (p-value < 0.01) between the instrument and immigration rates for most years, except for 2020 interactions with recent immigration rates. High F-test values (above 10) confirm the instrument's relevance.

Table 3.3: First stage estimates

Variables	(1)	(2)
	Recent immigration rate	Earlier -term immigration rate
IV Migration	122.0546*** (7.8548)	155.3636*** (19.1751)
D2016 $\times$ Total migration rate	14.7256*** (5.2430)	6.5602 (14.0571)
D2018 $\times$ Total migration rate	-35.1923*** (4.6867)	-57.8447*** (11.5475)
D2019 $\times$ Total migration rate	-84.5915*** (5.3985)	-36.1020** (14.8940)
D2020 $\times$ Total migration rate	-98.0188*** (6.4412)	-28.2201** (13.3029)
D2021 $\times$ Total migration rate	-105.5378*** (6.6919)	-31.1290** (13.8410)
Observations	2,825,912	2,825,912
KP-stat	23.59	17.27

Notes: The set of covariates are those described in Equation 3.1. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. Standard errors are clustered at the department level. *Source:* Authors' calculations using the national household survey (GEIH).

3.5 Results

In this section, we present three sets of results. First, we assess the impact of migration on labor market outcomes for both local workers and earlier migrant cohorts, with particular attention to how these effects may have evolved in response to the pandemic. To better understand the labor market's adaptation to migration shocks, we next examine migrant assimilation by analyzing changes in key labor market outcomes and selected social indicators over time since migrants' arrival in Colombia. Finally, we explore heterogeneous effects across different population groups to identify factors that may explain varying levels of sensitivity to migration shocks.

⁵Additional first-stage estimates are available upon request.

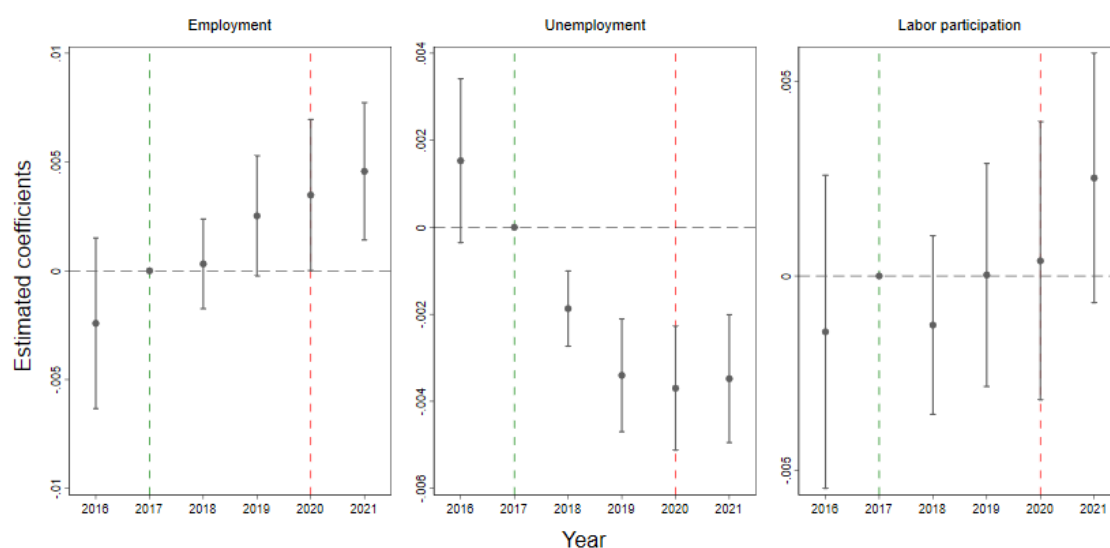
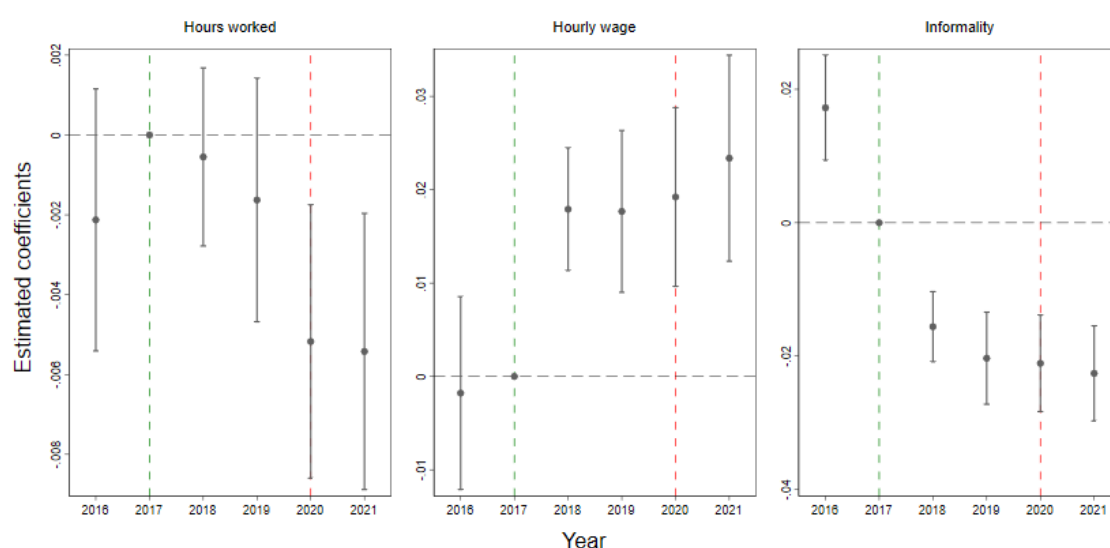
3.5.1 Impacts of migration on labor market outcomes

To study the impacts of migration on the labor market, we estimate the specification described earlier. As part of the analysis, a comparative assessment of the results is conducted using both ordinary least squares (OLS) and instrumental variables (IV). Table C.2.2 (Appendix B) presents the OLS estimates (Panel A) and IV estimates (Panel B) of the impact of total migration on local workers. Columns 1–3 report outcomes related to labor market status, while columns 4–6 focus on employment quality outcomes.

In general, the systematic difference between the OLS and IV results indicates the presence of significant bias. These differences are further highlighted by the statistically significant results in the IV estimates. Focusing on β_0 (see Equation 3.1), the OLS coefficients are positive for employment and labor force participation. This suggests that higher migration rates are associated with regional economic conditions. Specifically, migration tends to be higher in regions that attract migrants due to better economic conditions, characterized by higher employment levels and greater labor force participation. IV strategy corrects for this bias by isolating the exogenous variation, thus revealing the true negative impact. In the case of hourly wages, the difference between OLS and IV lies in the larger magnitude of the latter. Self-selection in this context implies that higher migration rates lead OLS to capture the positive relationship between migration rates and higher-wage labor markets, making the marginal effect less negative. Once endogeneity is corrected, this impact becomes more negative and statistically significant.

The IV estimates address these limitations by leveraging an instrumental variable that isolates exogenous variation in migration flows, ensuring that the estimated effects are not confounded by migrants' settlement preferences. By accounting for this endogenous selection, the IV provides valid estimates of migration's impact. Figure 3.3 illustrates the coefficients presented in Panel B of Table C.2.3 (Appendix B), which correspond to the interaction between the total migration rate and the year dummy variables. The dashed green line marks the start of the pre-treatment period, preceding the massive Venezuelan migration, while the dashed red line indicates the year of the pandemic in 2020.

Figure 3.3: Impact of migration on labor market outcomes of local workers

(a) Labor market *status*(b) Labor market *quality*

Notes: Calculations are based on working age-population. The plotted coefficients correspond to the time-varying effects in Equation 3.1, and standard errors are clustered at the department level. A 95% confidence interval is computed based on standard errors. *Source:* Authors' calculations using GEIH.

Using 2017 as the reference year, the estimated coefficients capture the relative changes in the dependent variables relative to this baseline. Table C.2.3 demonstrates that a one percentage point increase in the migration rate decreases the employment probability of locals by 0.76 percentage points. The substantial influx of migrants in 2018 further amplifies this effect by an additional 0.0003 percentage points. However, this increase lacks statistical significance. From 2019 onwards, the coefficients turn positive and display an upward trend. This pattern suggests that, relative to 2017, the initial adverse effect of migration on local employment vanishes over time, indicating a convergence toward a neutral or negligible impact.

On the other hand, while migration increased unemployment by 0.42 percentage points in 2017, this impact was reduced by nearly half in 2018 and continued to decrease, though at a slower pace, between 2019 and 2021. The diminishing negative effect on unemployment reflects the labor market's adaptation of local workers to the influx of migrants. Moreover, migration also contributed to a reduction in the labor force participation of local workers, with a recorded decrease of 0.6 percentage points in 2017. Notably, this negative effect is the only labor market status outcome that has not shown significant variations throughout the analyzed period.

Regarding job characteristics, we analyzed hours worked, hourly wages, and informality status. As for hours worked, there was a slight increase of 0.6% in 2017, which did not show statistically significant differences until 2020, when this effect was significantly reduced by more than 0.5%. This suggests that the labor market reorganization caused by the pandemic countered the increase in the intensive margin of employment. The increase in hours as a result of migration is likely a response to wage reductions.

Our results also show that migration reduced hourly wages for local workers by 3.88% in 2017. By 2018, this negative effect was halved (with a 1.79 percentage point decrease) and continued to diminish. This suggests that massive migration has not significantly affected the labor market adaptation in terms of wages, although a smaller impact still persists in 2021. Lastly, migration increased the informal employment rate of locals by 3.9 percentage points. However, this effect diminished over the years. A signal of assimilation is that migrants integrate into the labor market in sectors that align with their skills. This labor market reallocation can also benefit locals by reducing the pressure to transition into the informal sector (see Figure 3.3).

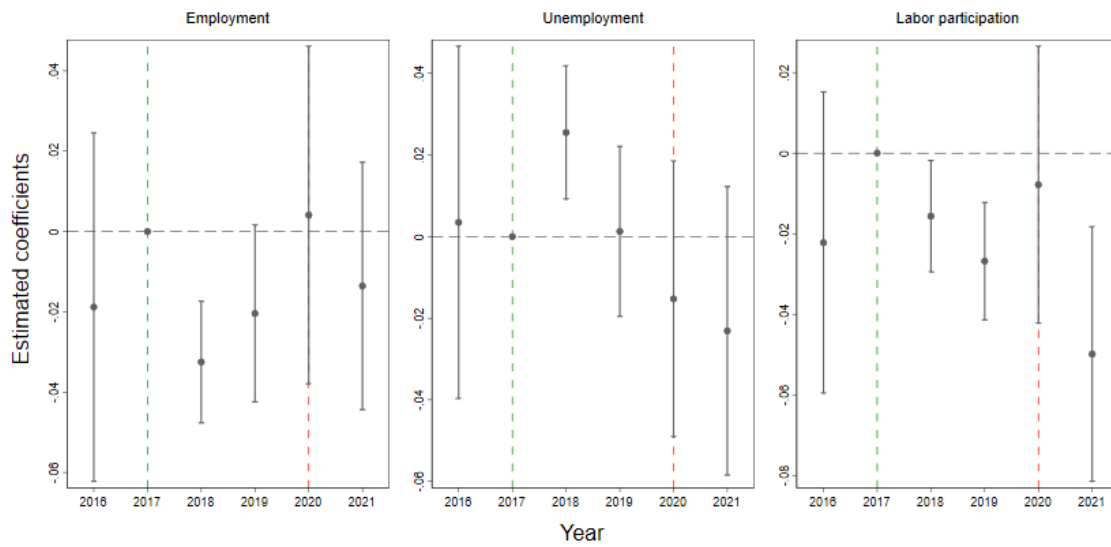
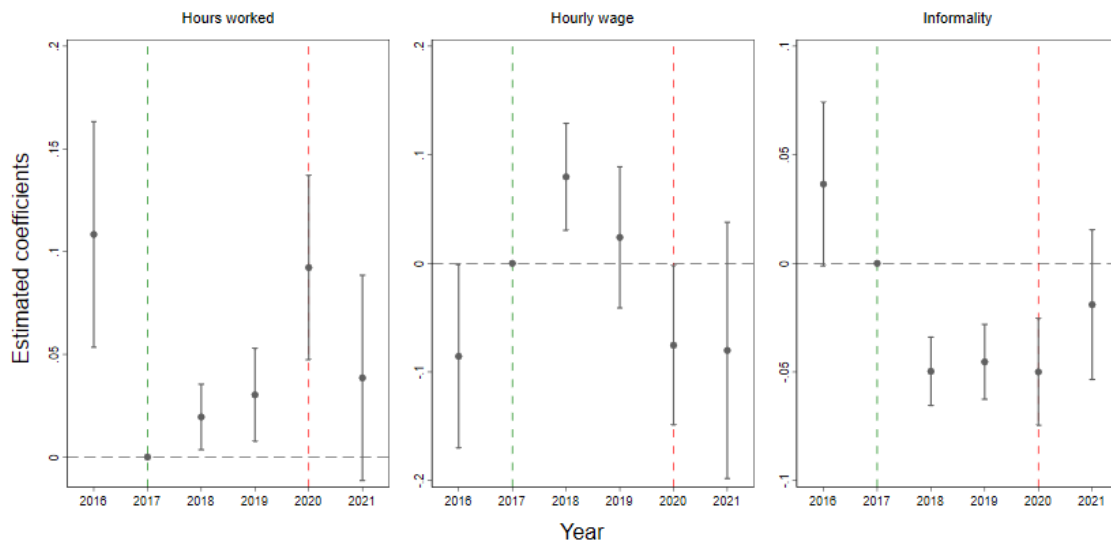
An important characteristic of the validity of our estimates is the existence of parallel trends. This means that if mass migration represented a shock, in the absence of it, the impacts of migration should not vary. In practice, this implies that the effects in 2016 and 2017 should be statistically similar. Interestingly, our results, except in the case of informality, support this hypothesis. In the case of informality, it can be argued that the effects of labor force reorganization are more pronounced with small variations in the migration rate, as nearly all migrants integrate into the labor market through informal jobs, with an informality rate exceeding 90%.

A relevant aspect of migration's impact, for which there is little evidence, is whether a migration shock affects earlier migrants. To address this, we examine the impacts of recent migration waves on earlier migrant cohorts. Understanding how newly arrived migrants influence those who migrated earlier is crucial for assessing the broader implications of migration on economic integration and social cohesion. Results presented in Figure 3.4 and Table C.2.3 show that recent migration only had significant effects on hourly wages and informality rates in 2017. Specifically, one percentage point increase in the recent immigration rate raised earlier migrants' employment by 0.57 percentage points, although this coefficient was not statistically significant. However, in 2018, this effect turned negative and became statistically significant, reducing earlier migrants' employment by 3.25 percentage points.

Similarly, in the case of unemployment, the effects are not significant except after the massive

migrant influx, where an increase of 2.55 percentage points is observed. This suggests that the pandemic did not influence the effect of recent migration on earlier migrants' outcomes and that the impacts of the massive influx appear to be temporary. The labor force participation of earlier migrants also tended to decline in 2018 and 2019, with this effect intensifying in 2021, reaching a reduction of up to 5 percentage points. This may be the result of a tighter labor market, as experienced after the pandemic, where migrants compete for a limited pool of available jobs, particularly in sectors that are more accessible to both groups, such as informal or low-skilled employment. Furthermore, the pandemic may have intensified pre-existing vulnerabilities among earlier migrants, such as limited social capital and restricted access to formal labor markets. This dynamic underscores the compounded challenges they face when labor markets come under additional pressure from new migration waves.

Figure 3.4: Impact of recent migration on labor market outcomes of earlier migrants

(a) Labor market *status*(b) Labor market *quality*

Notes: Calculations are based on the working-age population. The plotted coefficients correspond to the time-varying effects in Equation 3.1, and standard errors are clustered at the department level. A 95% confidence interval is computed based on standard errors. *Source:* Authors' calculations using GEIH.

Among the employment quality indicators, wages and informality among earlier migrants are particularly sensitive to new waves of migration. While wages declined by 20.56% in 2017, informality increased by 7 percentage points. Additionally, wages continued to decrease during the pandemic, which appears to be consistent with an increase in the number of hours worked. This phenomenon can be attributed to structural challenges faced by earlier migrants, including limited mobility across sectors or geographic areas, weak social networks in higher-wage industries, and restricted opportunities for upskilling or adapting to evolving labor market conditions (Ghimire, Nepal, Crowley, Ghimire, & Guragain, 2022; Sen et al., 2023).

3.5.2 Economic and social assimilation of migrants

The assimilation of migrants is essential for understanding their economic and social integration into the host country. In the case of Colombia, the large influx of Venezuelan migrants raises important questions about their adaptation and the extent to which their labor market outcomes and social network formation improve over time. The information from EPM provides a unique opportunity to study how the migrant assimilation process in Colombia has evolved. In particular, it allows to explore how the length of residence in Colombia influences migrants' outcomes. Interestingly, EPM data provide insights into how immigrants responded to the pandemic, offering valuable data to assess their vulnerability to this shock. This analysis helps determine whether higher levels of assimilation enhance migrants' ability to mitigate risk and resilience to shocks.

A first approach to providing evidence of assimilation is to study the behavior of labor market outcomes based on the length of time since arrival. In particular, we compute the gap between immigrants and the local population using data from GEIH and EPM. Figure C.3.1 shows the labor market outcomes observed in 2021 for both migrants and locals. In the case of locals, we present the average outcome, while for migrants, the average outcome is differentiated according to the year of arrival in Colombia. Immigrants who arrived before 2018 exhibit a lower unemployment rate than locals, and this indicator grows sharply for the most recent immigrants. In turn, the employment rate appears to have a decreasing trend, showing that earlier immigrants have higher employment levels. Both indicators show that the longer the time spent in Colombia, the better the labor market outcomes. Moreover, the higher employment rate among immigrants reflects their greater opportunity cost of unemployment and increased need for income generation. (see Lee et al., 2022).

Job conditions are also strongly related to the time of arrival. The higher employment rate is consistent with a lower informality rate. Moreover, in relative terms, informality is the variable with the largest gap between locals and immigrants. This gap is permanent and might be the result of the segregation into occupations of immigrants, as documented by (Alcobendas & Rodríguez-Planas, 2009; Fernández, Forero, Núñez, Ruiz, et al., 2022). Finally, previous findings translate into lower average labor income for immigrants. This analysis shows that, at first glance, there is a process of assimilation that allows immigrants to improve their labor market conditions; however, in terms of informality status and labor income, the gaps seem to be more permanent.

To assess whether migrants' labor market conditions improve and converge to those of local workers, we estimate the following regression models:

$$y_{id} = \alpha + \beta A_i + \gamma_d + \delta' X_{id} + \epsilon_{id} \quad (3.3)$$

where y_{id} is the outcome of interest, A_i is the time since arrival, γ_d is a set of department fixed effects and X_{id} is a set of covariates including gender, age, age squared, education, and whether individual lives in rural or urban areas. Estimated coefficients shown in Table C.3.1 in Appendix

C.3 reveal that one additional year since arrival increases labor participation and employment by 0.4 and 0.17 percentage points, respectively. And, it also reduces unemployment and informality by 1.3 and 3.3 percentage points, respectively. There is a remarkable increase in labor income of 4.8%. To further analyze this, we use a measure of the labor income gap between local workers and migrants, which consists of a ratio between the average labor income of locals relative to migrants. Estimates indicate that time in Colombia reduces the gap by 0.07, i.e., a initial gap of 50% would vanish to zero after approximately 7.1 ($0.5/0.07$) years. These results reinforce the idea that the labor market has absorbed the migration shock, facilitating the integration of migrants into jobs with better conditions.

To the extent that immigrants improve their labor outcomes, it is also expected to observe greater social assimilation. For example, under better labor market conditions households consider investing in housing and establishing long-term plans. Considering a range of outcomes, we analyze whether arrival time also influences social assimilation. The social assimilation process turns out beneficial for the economy since it promotes investment decisions, human capital accumulation, and tax payments, among others (see Cai & Zimmermann, 2020; Gathmann & Keller, 2018).

Using the same specification, we estimate regression models for the following variables contained in EPM: whether the immigrant expects to stay in Colombia, whether the immigrant feels identified with the local culture, whether the immigrant has non-immigrant friends, whether immigrant participates in organizations,⁶ and whether immigrants receives government assistance.⁷ The first outcome is related to long-term expectations, which depend on the level of economic security in the host country. The next three refer to building social networks, and the last one to formal relationships with the government.

Results in Table C.3.2 (see Appendix C.3) imply that the longer time in Colombia favors social assimilation. In particular, it increases identification with the local culture, promotes the construction of social networks, and improves access to government social programs. It is worth noting that each additional year in Colombia increases the probability of receiving social assistance by 2.5 percentage points and the probability of staying in Colombia by one percentage point.

A question of interest is whether the assimilation process is also related to the capacity to mitigate aggregate shocks. Combining information from GEIH and EPM, it is possible to perform this analysis. That is, we provide evidence of how the assimilation process may have improved the capacity to face the COVID-19 shock (see Table C.3.3 in Appendix C.3). In particular, we have data available on households decisions during the pandemic. This includes whether the

⁶Number of activities within organizations in which the respondent or his family have participated, such as recreation, religious, youth, older adults, community action, ethnic, political, worker unions, among others

⁷If the respondent received money from government programs such as Familias en Acción, Jóvenes en Acción, Programa de Alimentación Escolar, Ingreso Solidario, Devolución del IVA, and Colombia Mayor.

household changed labor market decisions,⁸ migration,⁹ reduced spending,¹⁰ reduced savings,¹¹ used borrowing mechanisms,¹² and received aid.¹³

According to Table C.3.3, the longer time in Colombia is associated with greater household financial adjustment in response to the pandemic. That is, household spending and savings are reduced. Specifically, one additional year from arriving would reduce savings by 1.4 percentage points and spending by 0.7 percentage points, a pattern consistent with the findings of Tang and Li (2021) in China. This suggests that immigrants who have spent more time in Colombia may have accumulated more assets and face fewer liquidity constraints, providing them with greater flexibility to manage household finances.

Furthermore, adjustments in household financial management are more prevalent than shifts in employment status among household members. Specifically, the likelihood of migration to another city within Colombia or returning to Venezuela decreases by 0.3 percentage points, and the probability of increasing work hours or changing jobs decreases by 0.1 percentage points. This phenomenon may be attributed to the higher long-term costs of shifting activities to the home environment. For instance, increased household labor may entail sacrifices in human capital accumulation, so that immigration could incur substantial misallocation costs. Moreover, prolonged residence in Colombia correlates with an increase in the number of support networks and access to institutional resources.

3.5.3 Heterogeneous impacts of migration

This section explores the heterogeneous effects of migration on the labor market outcomes. In this analysis we consider different population groups by gender, education level¹⁴, age¹⁵, and informality status. Tables C.4.1 to C.4.4 and Figures C.4.1 to C.4.4 in Appendix C.4 present the results. Our findings suggest that women, young, those with the least education and informal workers are the most affected by migration. In particular, local women face more pronounced reductions in employment and labor participation. Similarly, the increase in unemployment is nearly double for women compared to men. While no statistically significant changes in labor participation are observed for men, local women experience a substantial decline.

⁸If the immigrant got another job or worked more hours.

⁹If the immigrant moved to another city or returned to Venezuela.

¹⁰Reduced spending is measured by moving to cheaper housing and reduced remittances to Venezuela.

¹¹Reduction in savings means that household has sold of assets and spent savings.

¹²If the immigrant requested loans from the family or a bank.

¹³The aid received could be from foundations or the government.

¹⁴Education levels are classified as follows: high-education respondents have over 12 years of education, while low-education respondents have 12 or fewer years.

¹⁵We employ 33 as the cutoff of young population because first, research indicates that migration decisions are closely linked to life-cycle stages, with younger individuals (typically under 30) being more mobile due to fewer familial obligations and greater adaptability to new environments (Millington, 2000; Plane & Heins, 2003). Conversely, as individuals approach their mid-thirties, they often exhibit a decline in mobility due to increased responsibilities, such as home ownership and family commitments, which can anchor them to specific locations for purposes of studying prolonged education, delayed family formation, and evolving career patterns across diverse societies. Besides, and most important, it allows us to compare the results by age groups with studies about Venezuelan migration such as Bahar et al. (2021)

Interestingly, the national recovery in employment and unemployment rates since mid-2020 has been led primarily by women, who were also the most affected during the initial impact. Conversely, men experience sharper declines in job quality. They face significant reductions in hours worked, although this effect has dissipated since 2018. Hourly wages decline for both, but the effect is slightly larger for men. Migration also raises informality rates for both groups, with a greater increase observed among men.

On the other hand, migration reduces employment among young by 1.41 percentage points, while unemployment rises by 0.68 percentage points. This population group also experienced the largest initial impact, they have led the national recovery in employment and unemployment rates since 2019. In terms of labor participation, they face a decline of 1.30 percentage points and large wage reductions. The impact of migration also varies significantly by education level. Workers with lower education levels face the most severe declines in job quality, with sharper reductions in wages and higher informality rates.

Finally, regarding the formality status, we observe that immigration primarily affected informal workers, leading to a notable 3.96% decrease in hourly wages and a 2.20% increase in working hours. In addition, during pandemic, migration decreased the hours worked by 1.4 percentage points for informal workers. This indicates the presence of a price effect, i.e., the observed adjustments were exclusively related to income and linked to the informality status.

3.6 Conclusions

The migration flow from Venezuela to Colombia represents one of the most significant demographic shifts in recent decades. This shift is especially relevant in the context of the COVID-19 pandemic, which has heightened the vulnerability of immigrants and may lead to substantial disruptions in the labor market. This study examines the direct impact of migration on both local Colombians and earlier migrants, focusing on their labor market outcomes, including status and quality, during and after the pandemic. Using an event study framework combined with instrumental variables to address endogeneity concerns, our analysis reveals evolving effects over time.

Our results indicate that the impact of migration varies significantly over time, and that factors such as unemployment, working hours, and informality were notably influenced by the pandemic, affecting the magnitude of these outcomes. Furthermore, the effects differ across population groups. Variables such as unemployment, wages, and informality are particularly sensitive to the shock of massive migration observed in 2018. Additionally, adverse effects are estimated for earlier migrants. Overall, there are varying effects over time, and it is observed that the labor market has adapted to the migration shock, mitigating its impacts over time.

Furthermore, our research underscores the persistent adverse effects during the pandemic. We find that migration increases informality among locals, alongside reduces employment rates, labor participation, and hourly wages. Regarding migrants, we assess the influence of new

arrivals on the labor market outcomes of earlier migrants, revealing a rise in informality and a decline in labor participation and hourly wages.

Our findings reveal that women, young, those with lower levels of education, and informal local workers experience the most pronounced effects. When formulating migration and labor market policies, policymakers should account for these disparities among different population segments.

The adaptation of the labor market may be a response to the assimilation of migrants. If migrants integrate more easily into the labor market and improve their conditions, fewer effects are expected to be observed for local workers. Analyzing whether there has been an assimilation process, our results suggest that longer time in Colombia is associated with better labor market and social indicators, including higher employment, lower unemployment, increased identification with the local culture, and the promotion of social network construction.

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Appendix A

Chapter 1

A.1 Complementary graphs of occupational transitions

Figure A.1.1: Change in occupational status from Venezuela to the first job in Colombia.



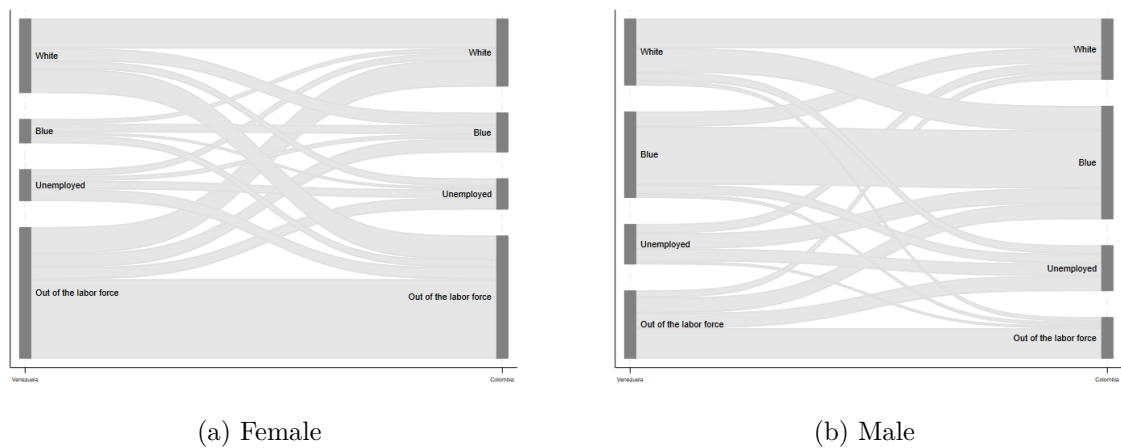
Notes: The left axis corresponds to the occupation in which the migrant worked most of the time in Venezuela. The right axis refers to the occupation of the first job in Colombia. EPM survey asks for the migrants' first job only if the migrant indicated he/she is employed at the time of the survey. Then, the right axis considers the first job of those who were employed. *Source:* Authors' calculations using EPM data.

Figure A.1.2: Change of formal status from Venezuela to Colombia



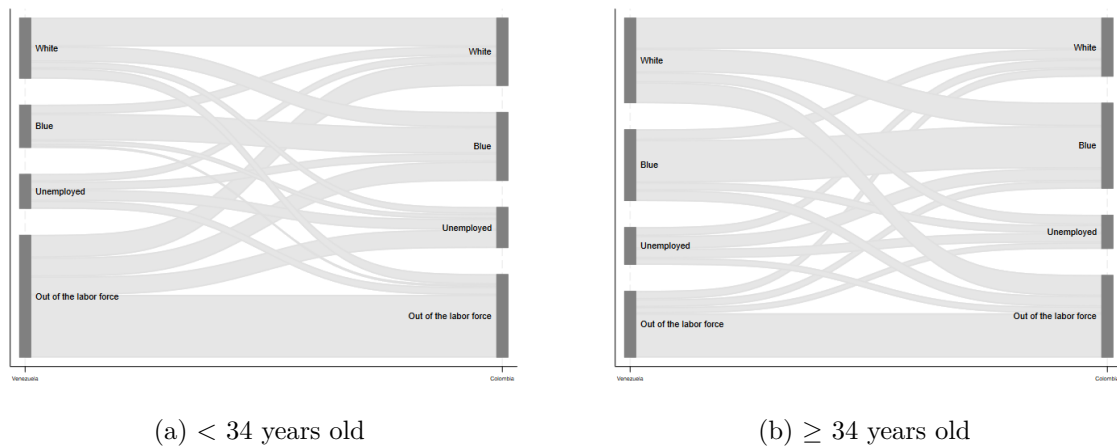
Notes: The left axis corresponds to whether the migrant had a written or verbal contract in their best job in Venezuela. The right axis indicates whether the migrant had a written or verbal contract in Colombia (at the time of their most recent survey). *Source:* Own calculations using EPM data.

Figure A.1.3: Change of occupational status from Venezuela to Colombia, by gender



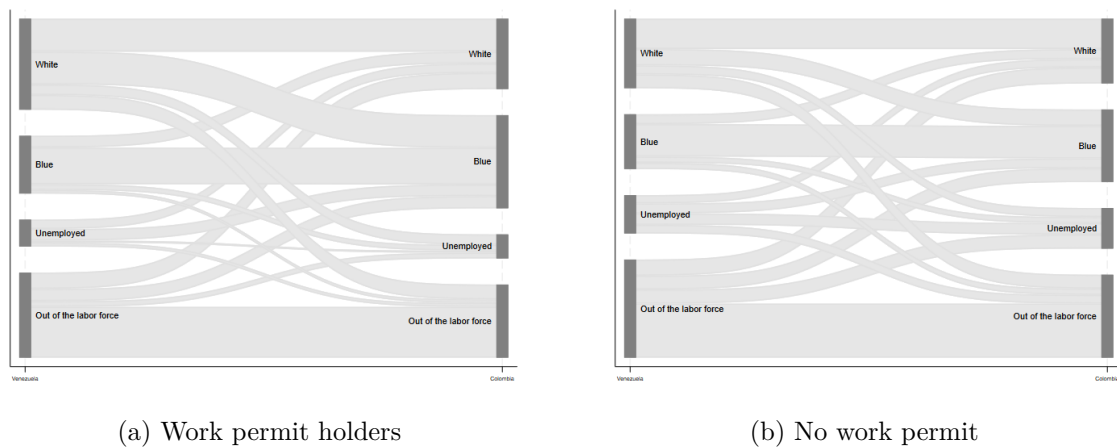
Notes: In each panel, the left axis corresponds to the occupation in which the migrant worked most of the time in Venezuela. The right axis refers to the occupation of the current job in Colombia. For the former variable, we used answers from round 2 which has less missing data. *Source:* Authors' calculations using EPM data.

Figure A.1.4: Change of occupational status from Venezuela to Colombia, by age group



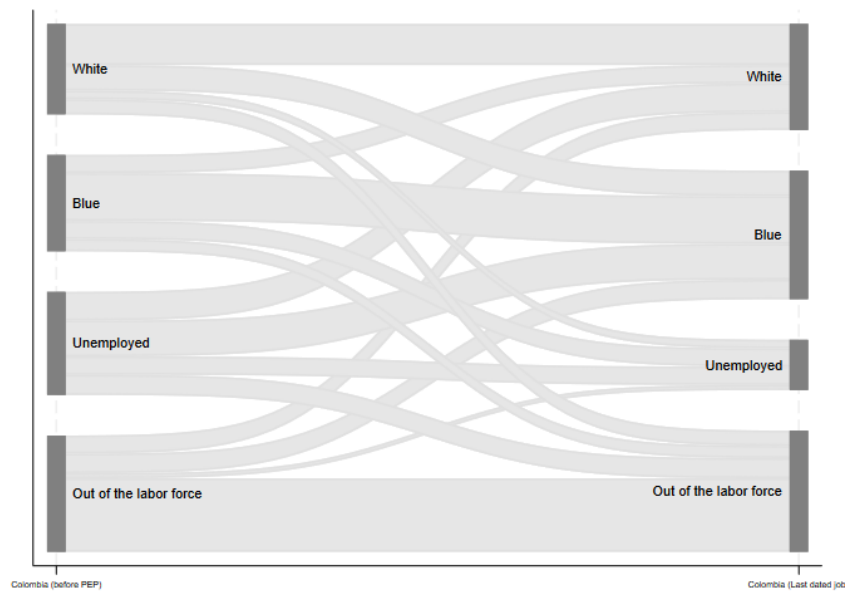
Notes: In each panel, the left axis corresponds to the occupation in which the migrant worked most of the time in Venezuela. The right axis refers to the occupation of the current job in Colombia. For the former variable, we used answers from round 2 which has less missing data. *Source:* Authors' calculations using EPM data.

Figure A.1.5: Change of occupational status from Venezuela to Colombia, by legal status



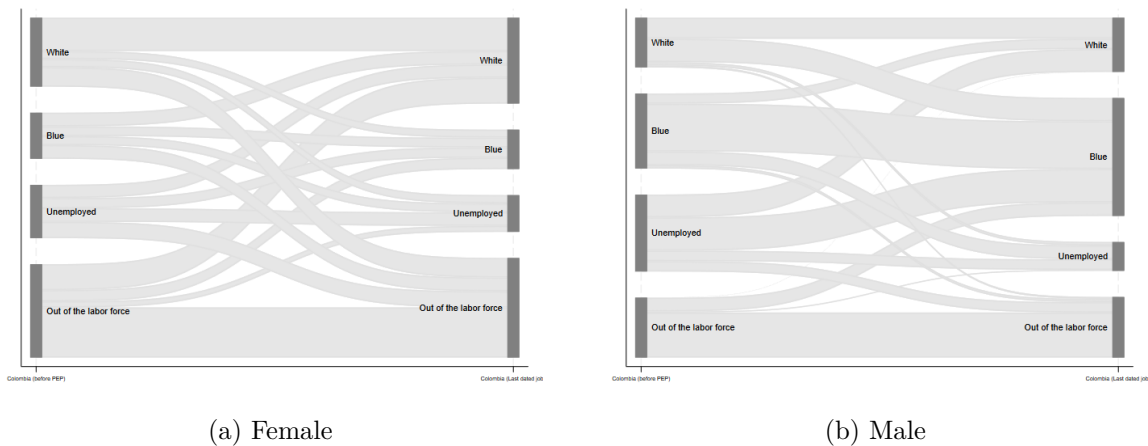
Notes: In each panel, the left axis corresponds to the occupation in which the migrant worked most of the time in Venezuela. The right axis refers to the occupation of the current job in Colombia. For the former variable, we used answers from round 2 which has less missing data. *Source:* Authors' calculations using EPM data.

Figure A.1.6: Change of occupational status before and after work permit



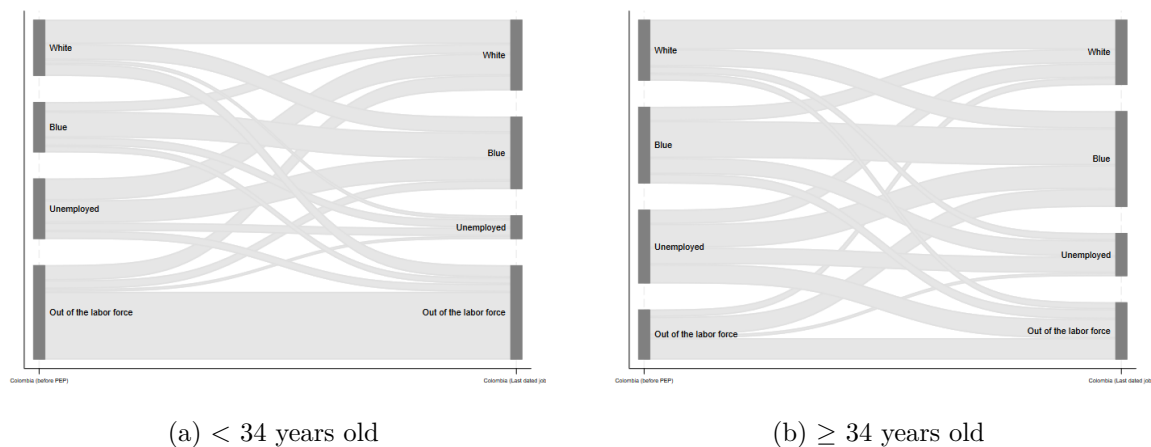
Notes: This sample considers only migrants with work permit. In each panel, the left axis corresponds to the occupation of the last job in which the migrant worked. The right axis refers to the occupation of the current job in Colombia. For the former variable, we used answers from round 2, which has less missing data. *Source:* Authors' calculations using EPM data.

Figure A.1.7: Change of occupational status from Venezuela to Colombia of migrants with work permits, by gender



Notes: This sample considers only migrants with work permit. In each panel, the left axis corresponds to the occupation of the last job in which the migrant worked. The right axis refers to the occupation of the current job in Colombia. For the former variable, we used answers from round 2 which has less missing data. *Source:* Authors' calculations using EPM data.

Figure A.1.8: Change of occupational status from Venezuela to Colombia of migrants with work permits, by age group



Notes: This sample considers only migrants with work permit. In each panel, the left axis corresponds to the occupation of the last job in which the migrant worked. The right axis refers to the occupation of the current job in Colombia. For the former variable, we used answers from round 2 which has less missing data. *Source:* Authors' calculations using EPM data.

A.2 RTI index

Table A.2.1: Tasks required and example for each component of the RTI

Task groups	Sign	Tasks related	Example
RC	+	Following well-defined rules and procedures, attention to detail, organization.	Data entry, record keeping, filing documents, ordering supplies, handling incoming calls.
RM	+	Following established procedures, physical skills, repetitive actions	Operating machinery, assembling products, packing and labeling, cleaning, and maintenance, tasks operating vehicles, performing repetitive
NRA	–	Critical thinking, analytical reasoning, problem-solving and decision-making skills that cannot be easily automated or programmed.	Fesearch and analysis, strategic planning, project management, data interpretation, problem-solving
NRI	–	Interpersonal skills, communication abilities, and emotional intelligence	Advising and counseling, negotiating, teaching, customer service
NRM	–	Physical or hands-on skills, creativity, and adaptability that are not easily automated or standardized	Building and construction, repair and maintenance, personal care services, customized production culinary arts, manual therapy

Notes: This table describes the five task components used to construct the RTI index at the four-digit occupation level following Mihaylov and Tijdens (2019), as defined in equation 1.4. The table provides brief descriptions and illustrative examples of tasks for each component.

A.3 Impact of PEP on occupational mobility by population groups

Table A.3.1: Heterogeneous impacts of work permits on labor market outcomes of migrants who had a *white-collar* job in Venezuela

	(1)	(2)	(3)	(4)	(5)
Variables	Employed	Employed	Formal	Unemployed	Out of the
	<i>White-collar</i>				labor force
Panel A. Female					
PEP x T	0.045	-0.007	0.083	0.019	-0.064
	(0.053)	(0.066)	(0.056)	(0.024)	(0.049)
Observations	1,603	1,141	1,118	1,603	1,603
R-squared	0.447	0.395	0.356	0.137	0.349
Panel B. Male					
PEP x T	-0.021	-0.045	-0.106**	0.001	0.021
	(0.035)	(0.045)	(0.041)	(0.030)	(0.018)
Observations	1,210	1,056	1,021	1,210	1,210
R-squared	0.181	0.554	0.351	0.152	0.119
Panel C. Age < 34 years old					
PEP x T	0.031	-0.054	-0.004	0.007	-0.038
	(0.032)	(0.052)	(0.057)	(0.023)	(0.028)
Observations	1,352	1,072	1,011	1,352	1,352
R-squared	0.391	0.530	0.339	0.168	0.273
Panel D. Age ≥ 34 years old					
PEP x T	0.006	-0.009	-0.022	0.013	-0.020
	(0.031)	(0.051)	(0.053)	(0.022)	(0.035)
Observations	1,461	1,125	1,128	1,461	1,461
R-squared	0.358	0.477	0.328	0.135	0.315
Panel E. Education ≤ Highschool					
PEP x T	0.042	-0.017	-0.026	0.010	-0.052*
	(0.028)	(0.047)	(0.051)	(0.022)	(0.029)
Observations	1,930	1,482	1,460	1,930	1,930
R-squared	0.369	0.521	0.288	0.153	0.280
Panel F. Education > Highschool					
PEP x T	-0.013	-0.040	0.029	0.011	0.001
	(0.036)	(0.057)	(0.075)	(0.025)	(0.032)
Observations	883	715	679	883	883
R-squared	0.382	0.458	0.347	0.121	0.338

Notes: The sample of these estimates corresponds to the migrants who had the occupational status indicated in the title. The estimates include the same controls described in Equation 1.1. Cluster-robust wild bootstrap p-values are used in these estimates to adjust for potential clustering effects. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. *Source:* Authors' calculations using EPM.

Table A.3.2: Heterogeneous impacts of work permits on labor market outcomes of migrants who had a *blue-collar job* in Venezuela

Variables	(1) Employed	(2) Employed	(3) formal	(4) Unemployed	(5) Out of the labor force
<i>White-collar</i>					
Panel A. Female					
PEP x T	0.015 (0.104)	-0.223*** (0.076)	0.008 (0.130)	-0.045 (0.031)	0.030 (0.090)
Observations	563	389	359	563	563
R-squared	0.464	0.549	0.397	0.170	0.430
Panel B. Male					
PEP x T	0.037 (0.033)	0.040 (0.045)	-0.011 (0.057)	0.014 (0.031)	-0.050*** (0.009)
Observations	1,507	1,312	1,285	1,507	1,507
R-squared	0.223	0.327	0.231	0.169	0.182
Panel C. Age < 34 years old					
PEP x T	0.082* (0.043)	0.087 (0.070)	-0.040 (0.066)	-0.068*** (0.024)	-0.014 (0.035)
Observations	917	779	731	917	917
R-squared	0.316	0.368	0.249	0.156	0.331
Panel D. Age ≥ 34 years old					
PEP x T	-0.008 (0.062)	-0.084 (0.075)	0.038 (0.066)	0.055 (0.049)	-0.047 (0.054)
Observations	1,153	922	913	1,153	1,153
R-squared	0.328	0.330	0.291	0.191	0.270
Panel E. Education ≤ Highschool					
PEP x T	0.044 (0.046)	0.010 (0.049)	0.016 (0.050)	0.002 (0.027)	-0.046 (0.042)
Observations	1,832	1,495	1,452	1,832	1,832
R-squared	0.307	0.293	0.215	0.143	0.275
Panel F. Education > Highschool					
PEP x T	0.019 (0.100)	0.011 (0.147)	-0.070 (0.172)	-0.018 (0.076)	-0.001 (0.042)
Observations	238	206	192	238	238
R-squared	0.367	0.533	0.471	0.295	0.396

Notes: The sample of these estimates corresponds to the migrants who had the occupational status indicated in the title. The estimates include the same controls described in Equation 1.1. Cluster-robust wild bootstrap p-values are used in these estimates to adjust for potential clustering effects. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. *Source:* Authors' calculations using EPM.

Table A.3.3: Heterogeneous impacts of work permits on labor market outcomes of migrants who were *unemployed* in Venezuela

Variables	(1) Employed	(2) Employed <i>white-collar</i>	(3) Formal	(4) Unemployed	(5) Out of the labor force
Panel A. Female					
PEP x T	0.124 (0.157)	0.040 (0.110)	0.142** (0.051)	0.008 (0.071)	-0.132 (0.110)
Observations	430	291	264	430	430
R-squared	0.495	0.739	0.418	0.828	0.396
Panel B. Male					
PEP x T	0.112* (0.058)	-0.008 (0.081)	-0.138 (0.138)	-0.080 (0.057)	-0.032* (0.018)
Observations	507	408	387	507	507
R-squared	0.735	0.474	0.516	0.801	0.259
Panel C. Age < 34 years old					
PEP x T	0.174 (0.106)	-0.050 (0.110)	0.036 (0.047)	0.005 (0.075)	-0.178*** (0.051)
Observations	571	423	371	571	571
R-squared	0.658	0.637	0.516	0.824	0.333
Panel D. Age ≥ 34 years old					
PEP x T	0.096 (0.101)	0.028 (0.076)	-0.090 (0.194)	-0.056 (0.050)	-0.040 (0.086)
Observations	366	276	280	366	366
R-squared	0.618	0.564	0.474	0.815	0.310
Panel E. Education ≤ Highschool					
PEP x T	0.214** (0.101)	0.014 (0.091)	-0.117* (0.067)	-0.048 (0.057)	-0.166*** (0.047)
Observations	735	545	501	735	735
R-squared	0.649	0.514	0.467	0.818	0.312
Panel F. Education > Highschool					
PEP x T	0.051 (0.145)	-0.035 (0.138)	0.109 (0.176)	-0.026 (0.083)	-0.025 (0.100)
Observations	202	154	150	202	202
R-squared	0.630	0.741	0.517	0.848	0.369

Notes: The sample of these estimates corresponds to the migrants who had the occupational status indicated in the title. The estimates include the same controls described in Equation 1.1. Cluster-robust wild bootstrap p-values are used in these estimates to adjust for potential clustering effects. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. *Source:* Authors' calculations using EPM.

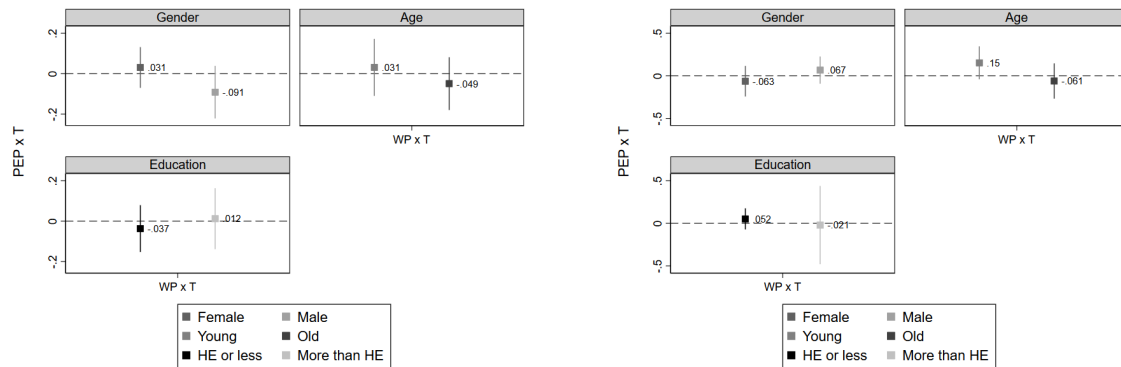
Table A.3.4: Heterogeneous impacts of work permits on labor market outcomes of migrants who were *out of the labor force* in Venezuela

Variables	(1) Employed	(2) Employed <i>white-collar</i>	(3) Formal	(4) Unemployed	(5) Out of the labor force
Panel A. Female					
PEP x T	0.036 (0.047)	-0.013 (0.047)	-0.030 (0.076)	0.008 (0.035)	-0.044 (0.034)
Observations	2,390	1,495	755	2,390	2,390
R-squared	0.431	0.762	0.338	0.108	0.460
Panel B. Male					
PEP x T	0.015 (0.086)	-0.000 (0.167)	-0.003 (0.162)	-0.085* (0.043)	0.070 (0.067)
Observations	884	592	299	884	884
R-squared	0.540	0.593	0.508	0.176	0.630
Panel C. Age < 34 years old					
PEP x T	-0.000 (0.046)	0.005 (0.117)	-0.006 (0.095)	-0.025 (0.041)	0.025 (0.040)
Observations	2,388	1,541	674	2,388	2,388
R-squared	0.442	0.698	0.382	0.115	0.499
Panel D. Age ≥ 34 years old					
PEP x T	0.074 (0.053)	0.057 (0.085)	-0.004 (0.093)	-0.012 (0.031)	-0.063 (0.049)
Observations	886	546	380	886	886
R-squared	0.526	0.736	0.349	0.148	0.547
Panel E. Education ≤ Highschool					
PEP x T	0.017 (0.041)	0.026 (0.081)	-0.075 (0.085)	-0.013 (0.032)	-0.004 (0.033)
Observations	2,850	1,792	867	2,850	2,850
R-squared	0.433	0.684	0.300	0.097	0.478
Panel F. Education > Highschool					
PEP x T	0.058 (0.077)	-0.058 (0.136)	0.180 (0.194)	-0.031 (0.067)	-0.027 (0.071)
Observations	424	295	187	424	424
R-squared	0.583	0.791	0.512	0.280	0.643

Notes: The sample of these estimates corresponds to the migrants who had the occupational status indicated in the title. The estimates include the same controls described in Equation 1.1. Cluster-robust wild bootstrap p-values are used in these estimates to adjust for potential clustering effects. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. *Source:* Authors' calculations using EPM.

A.4 Impact of PEP on routinization

Figure A.4.1: Effect of work permit on RTI: by pre-migration occupation and demographic group



(a) Previously *white-collar* in Venezuela

(b) Previously *blue-collar* in Venezuela

Notes: *Source:* Authors' calculations using EPM data.

Table A.4.1: Heterogeneous impacts of work permits on RTI who were *white-collar* in Venezuela.

	(1)	(2)	(3)	(4)	(5)	(6)
	RTI	Routine		Non-routine		
Variables		Cognitive	Manual	Analytic	Interactive	Manual
Panel A. Female						
PEP x T	0.031 (0.049)	0.014 (0.023)	0.001 (0.007)	0.021 (0.041)	-0.007 (0.031)	-0.037 (0.049)
Observations	1,134	1,134	1,134	908	1,134	1,134
R-squared	0.272	0.283	0.130	0.393	0.273	0.523
Panel B. Male						
PEP x T	-0.091 (0.063)	-0.057* (0.029)	0.012 (0.013)	-0.005 (0.023)	0.008 (0.022)	0.040 (0.040)
Observations	1,041	1,041	1,041	935	1,041	1,041
R-squared	0.261	0.336	0.162	0.413	0.326	0.412
Panel C. Age < 34 years old						
PEP x T	0.019 (0.072)	-0.016 (0.036)	0.025 (0.016)	0.001 (0.033)	-0.018 (0.030)	0.008 (0.042)
Observations	1,033	1,033	1,033	857	1,033	1,033
R-squared	0.246	0.277	0.162	0.356	0.262	0.436
Panel D. Age ≥ 34 years old						
PEP x T	0.031 (0.068)	-0.009 (0.034)	0.024 (0.014)	0.001 (0.031)	-0.023 (0.030)	0.007 (0.041)
Observations	1,064	1,064	1,064	878	1,064	1,064
R-squared	0.254	0.279	0.162	0.376	0.276	0.453
Panel E. Education ≤ Highschool						
PEP x T	-0.037 (0.056)	-0.022 (0.028)	0.004 (0.012)	0.019 (0.020)	0.005 (0.019)	-0.012 (0.031)
Observations	1,467	1,467	1,467	1,215	1,467	1,467
R-squared	0.240	0.282	0.149	0.302	0.300	0.400
Panel F. Education > Highschool						
PEP x T	0.012 (0.073)	-0.014 (0.036)	0.021 (0.016)	0.010 (0.048)	-0.009 (0.032)	-0.014 (0.055)
Observations	708	708	708	628	708	708
R-squared	0.264	0.286	0.173	0.414	0.304	0.475

Notes: The sample of these estimates corresponds to the migrants who had the occupational status indicated in the title. The estimates include the same controls described in Equation 1.1. Cluster-robust wild bootstrap p-values are used in these estimates to adjust for potential clustering effects. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. Source: Authors' calculations using EPM.

Table A.4.2: Heterogeneous impacts of work permits on RTI who were *blue-collar* in Venezuela

	(1)	(2)	(3)	(4)	(5)	(6)
	RTI	Routine		Non-routine		
Variables		Cognitive	Manual	Analytic	Interactive	Manual
Panel A. Female						
PEP x T	-0.063	-0.030	-0.002	-0.011	0.008	0.043
	(0.086)	(0.030)	(0.046)	(0.008)	(0.034)	(0.062)
Observations	385	385	385	377	385	385
R-squared	0.476	0.444	0.433	0.430	0.538	0.418
Panel B. Male						
PEP x T	0.067	0.023	0.011	0.004	-0.017	-0.022
	(0.077)	(0.031)	(0.041)	(0.012)	(0.019)	(0.046)
Observations	1,245	1,245	1,245	1,230	1,245	1,245
R-squared	0.294	0.262	0.279	0.226	0.209	0.289
Panel C. Age < 34 years old						
PEP x T	0.153	0.051	0.026	-0.032**	-0.009	-0.042
	(0.093)	(0.040)	(0.039)	(0.013)	(0.024)	(0.055)
Observations	756	756	756	741	756	756
R-squared	0.355	0.290	0.271	0.259	0.317	0.358
Panel D. Age ≥ 34 years old						
PEP x T	-0.061	-0.023	-0.008	0.025	-0.015	0.023
	(0.100)	(0.028)	(0.048)	(0.018)	(0.022)	(0.071)
Observations	874	874	874	866	874	874
R-squared	0.359	0.357	0.353	0.284	0.315	0.323
Panel E. Education ≤ Highschool						
PEP x T	0.052	0.005	0.021	-0.006	-0.005	-0.015
	(0.060)	(0.022)	(0.032)	(0.009)	(0.015)	(0.035)
Observations	1,430	1,430	1,430	1,412	1,430	1,430
R-squared	0.323	0.296	0.286	0.234	0.276	0.292
Panel F. Education > Highschool						
PEP x T	-0.021	0.052	-0.062	0.007	-0.032	0.034
	(0.219)	(0.076)	(0.121)	(0.043)	(0.056)	(0.122)
Observations	200	200	200	195	200	200
R-squared	0.447	0.439	0.429	0.529	0.467	0.491

Notes: The sample of these estimates corresponds to the migrants who had the occupational status indicated in the title. The estimates include the same controls described in Equation 1.1. Cluster-robust wild bootstrap p-values are used in these estimates to adjust for potential clustering effects. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. *Source:* Authors' calculations using EPM.

A.5 Robustness checks

Table A.5.1: Dropping the border cities

	Employed (1)	White-collar job (2)	Formal (3)	Unemployed (4)	Out of the labor force (6)
Panel A. Migrants who were <i>white-collar</i> in Venezuela					
WP x T	0.022 (0.022)	-0.054 (0.034)	-0.030 (0.032)	0.021 (0.015)	-0.042** (0.020)
Observations	2,226	1,749	1,698	2,226	2,226
R-squared	0.351	0.476	0.296	0.124	0.278
Panel B. Migrants who were <i>blue-collar</i> in Venezuela					
WP x T	0.019 (0.042)	-0.011 (0.054)	-0.005 (0.055)	0.010 (0.029)	-0.029 (0.042)
Observations	1,579	1,302	1,269	1,579	1,579
R-squared	0.291	0.279	0.201	0.139	0.269
Panel C. Migrants who were unemployed in Venezuela					
WP x T	0.156* (0.088)	-0.045 (0.074)	-0.061 (0.089)	-0.034 (0.043)	-0.122** (0.053)
Observations	919	685	637	919	919
R-squared	0.605	0.525	0.399	0.795	0.271
Panel D. Migrants who were out of the labor force in Venezuela					
WP x T	0.018	-0.043	-0.037	-0.009	-0.009
Observations	2,642	1,680	877	2,642	2,642
R-squared	0.434	0.657	0.330	0.107	0.492

Notes: Estimates use the baseline difference-in-differences specification and controls. Cluster-robust wild bootstrap p-values are reported. *** p<0.01, ** p<0.05, * p<0.10. Source: Authors' calculations using EPM.

Appendix B

Chapter 2

B.1 Respondent-specific probability calculation

I define the following notation. Let i index respondents.

- $T_i \in \{0, 1\}$ denotes treatment assignment in the list experiment, where $T_i = 1$ indicates that the respondent received the list including the sensitive item and $T_i = 0$ indicates the control list without the sensitive item.
- $K \in \mathbb{N}$ denotes the number of *non-sensitive* items in the control list. Hence, the treated list contains $K + 1$ items (the K non-sensitive items plus one sensitive item).
- Y_i denotes the observed item count reported by respondent i . Its support is

$$Y_i \in \{0, 1, \dots, K\} \text{ if } T_i = 0, \quad Y_i \in \{0, 1, \dots, K + 1\} \text{ if } T_i = 1.$$

- C_i denotes the (latent) number of non-sensitive items endorsed by respondent i :

$$C_i \in \{0, 1, \dots, K\}.$$

- A_i denotes latent endorsement of the sensitive item:

$$A_i \in \{0, 1\},$$

where $A_i = 1$ indicates agreement with the sensitive statement.

- X_i denotes a vector of observed covariates (e.g., geographic indicators and victimization history).
- $\Lambda(\cdot)$ denotes the logistic cumulative distribution function,

$$\Lambda(z) = \frac{1}{1 + \exp(-z)}.$$

- π_i denotes the probability of endorsing a non-sensitive item, modeled as

$$\pi_i = \Lambda(X_i' \beta),$$

where β is a parameter vector.

- θ_i denotes the probability of endorsing the sensitive item, modeled as

$$\theta_i = \Lambda(X_i' \gamma),$$

where γ is a parameter vector.

The observed count satisfies

$$Y_i = \begin{cases} C_i, & \text{if } T_i = 0, \\ C_i + A_i, & \text{if } T_i = 1. \end{cases}$$

I model

$$C_i \mid X_i \sim \text{Binomial}(K, \pi_i), \quad A_i \mid X_i \sim \text{Bernoulli}(\theta_i),$$

and estimate (β, γ) by maximum likelihood.

For treated respondents ($T_i = 1$), I compute the respondent-specific probability of endorsing the sensitive item conditional on the observed count and covariates:

$$\Pr(A_i = 1 \mid Y_i = y, T_i = 1, X_i) = \frac{\theta_i \Pr(C_i = y - 1 \mid X_i)}{(1 - \theta_i) \Pr(C_i = y \mid X_i) + \theta_i \Pr(C_i = y - 1 \mid X_i)}, \quad y \in \{0, 1, \dots, K+1\}.$$

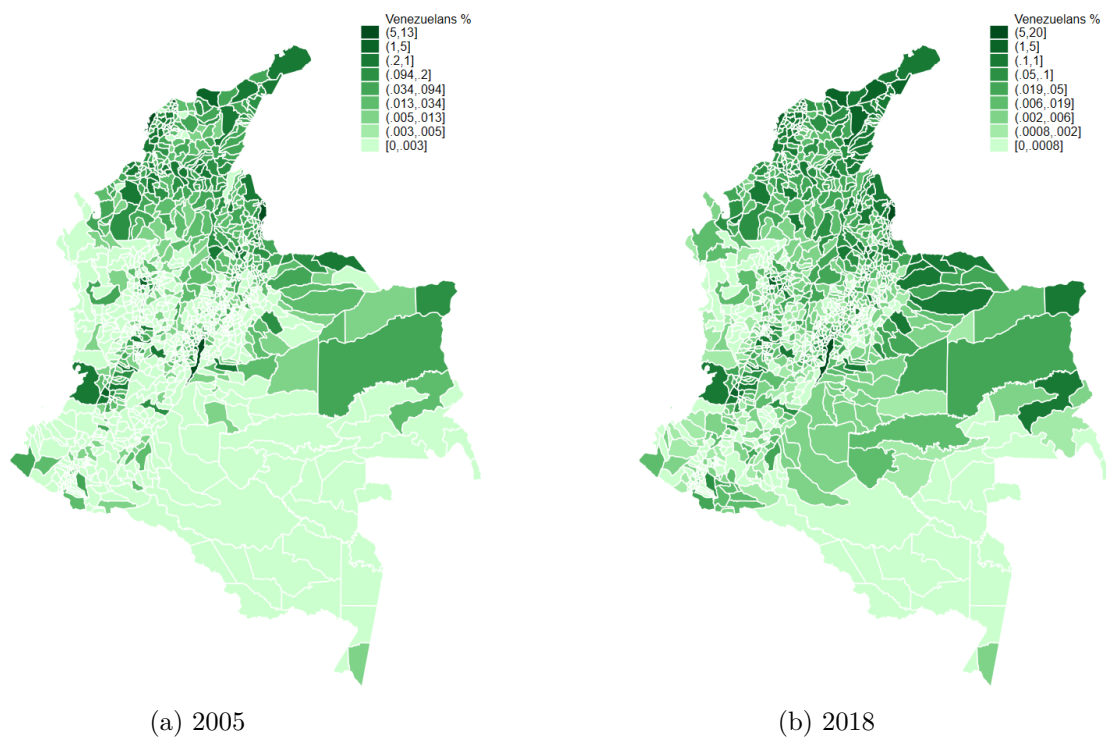
By construction, this probability equals 0 when $y = 0$ and equals 1 when $y = K + 1$.

Appendix C

Chapter 3

C.1 Geographical distribution of Venezuelans in Colombia

Figure C.1.1: Percentage of Venezuelan immigrants in Colombia, 2005 and 2018



Source: Authors' calculations using the national census of 2005 and 2018.

C.2 Main results

Table C.2.1: First stage regressions

Variable	F-stat	p-val
Panel A. Impact of total immigration		
Total Immigration rate	76.76	0.00000
D2016 $\times$ Total immigration rate	175.32	0.00000
D2018 $\times$ Total immigration rate	124.91	0.00000
D2019 $\times$ Total immigration rate	83.62	0.00000
D2020 $\times$ Total immigration rate	94.94	0.00000
D2021 $\times$ Total immigration rate	61.22	0.00000
Panel B. Impact of recent immigration		
Recent Immigration rate	66.59	0.00000
D2016 $\times$ Recent Immigration Rate	331.79	0.00000
D2018 $\times$ Recent Immigration Rate	105.31	0.00000
D2019 $\times$ Recent Immigration Rate	94.01	0.00000
D2020 $\times$ Recent Immigration Rate	66.02	0.00000
D2021 $\times$ Recent Immigration Rate	56.51	0.00000

Notes: The set of covariates are those described in Equation 3.1. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. Standard errors are in parentheses. *Source:* Authors' calculations using the national household survey (GEIH).

Table C.2.2: Impact of migration on labor market outcomes of local workers, OLS and IV estimates

Variables	Status			Quality		
	(1) Employment	(2) Unemployment	(3) Labor Participation	(4) Hours worked	(5) Hourly wage	(6) Informality
Panel A. OLS estimates						
D2016 $\times$ Total migration rate	0.0014 (0.0036)	-0.0011 (0.0013)	0.0011 (0.0037)	-0.0051 (0.0043)	0.0100* (0.0054)	0.0040 (0.0047)
Total migration rate	0.0038 (0.0056)	0.0003 (0.0018)	0.0050 (0.0061)	0.0061 (0.0047)	-0.0026 (0.0145)	0.0117 (0.0121)
D2018 $\times$ Total migration rate	-0.0020 (0.0026)	0.0004 (0.0009)	-0.0023 (0.0027)	-0.0017 (0.0030)	0.0037 (0.0073)	-0.0060 (0.0056)
D2019 $\times$ Total migration rate	-0.0011 (0.0032)	-0.0009 (0.0011)	-0.0022 (0.0033)	-0.0042 (0.0038)	0.0013 (0.0079)	-0.0076 (0.0066)
D2020 $\times$ Total migration rate	-0.0019 (0.0038)	-0.0015 (0.0014)	-0.0037 (0.0037)	-0.0063* (0.0033)	-0.0030 (0.0091)	-0.0080 (0.0064)
D2021 $\times$ Total migration rate	0.0004 (0.0037)	-0.0018* (0.0010)	-0.0010 (0.0042)	-0.0059 (0.0035)	-0.0049 (0.0112)	-0.0086 (0.0072)
Constant	-1.0074*** (0.0619)	0.5393*** (0.0259)	-1.0466*** (0.0676)	3.0099*** (0.0591)	6.1085*** (0.0973)	1.5102*** (0.0907)
Observations	2,745,117	1,892,207	2,745,117	1,629,717	1,466,000	1,629,745
Adjusted R-squared	0.2320	0.0549	0.2627	0.0872	0.3251	0.2262
Panel B. IV estimates						
D2016 $\times$ Total migration rate	-0.0024 (0.0020)	0.0015 (0.0010)	-0.0014 (0.0033)	-0.0021 (0.0017)	-0.0018 (0.0053)	0.0172*** (0.0040)
Total migration rate	-0.0076*** (0.0027)	0.0042*** (0.0013)	-0.0060*** (0.0015)	0.0062*** (0.0022)	-0.0388*** (0.0090)	0.0396*** (0.0071)
D2018 $\times$ Total migration rate	0.0003 (0.0011)	-0.0019*** (0.0004)	-0.0013 (0.0006)	-0.0005 (0.0011)	0.0179*** (0.0034)	-0.0156*** (0.0027)
D2019 $\times$ Total migration rate	0.0025* (0.0014)	-0.0034*** (0.0007)	0.0000 (0.0006)	-0.0016 (0.0016)	0.0177*** (0.0044)	-0.0203*** (0.0035)
D2020 $\times$ Total migration rate	0.0035** (0.0018)	-0.0037*** (0.0007)	0.0004 (0.0008)	-0.0052*** (0.0017)	0.0192*** (0.0049)	-0.0211*** (0.0037)
D2021 $\times$ Total migration rate	0.0046*** (0.0016)	-0.0035*** (0.0007)	0.0025 (0.0016)	-0.0054*** (0.0018)	0.0234*** (0.0056)	-0.0226*** (0.0036)
Constant	-0.9852*** (0.0715)	0.5325*** (0.0244)	-1.0236*** (0.0797)	3.0076*** (0.0602)	6.1953*** (0.0886)	1.4518*** (0.1143)
Observations	2,745,117	1,892,207	2,745,117	1,629,717	1,466,000	1,629,745
Adjusted R-squared	0.2308	0.0547	0.2611	0.0871	0.3227	0.2211
KP-stat	23.54	24.02	23.54	23.85	24.88	23.85
Mean of 2017	0.6234	0.1172	0.7061	3.7363	8.3251	0.5731

Notes: The set of covariates are those described in Equation 3.1. The mean of weekly hours worked and hourly wage for 2017 is in logs. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. Standard errors are clustered at the department level. *Source:* Authors' calculations using the national household survey (GEIH).

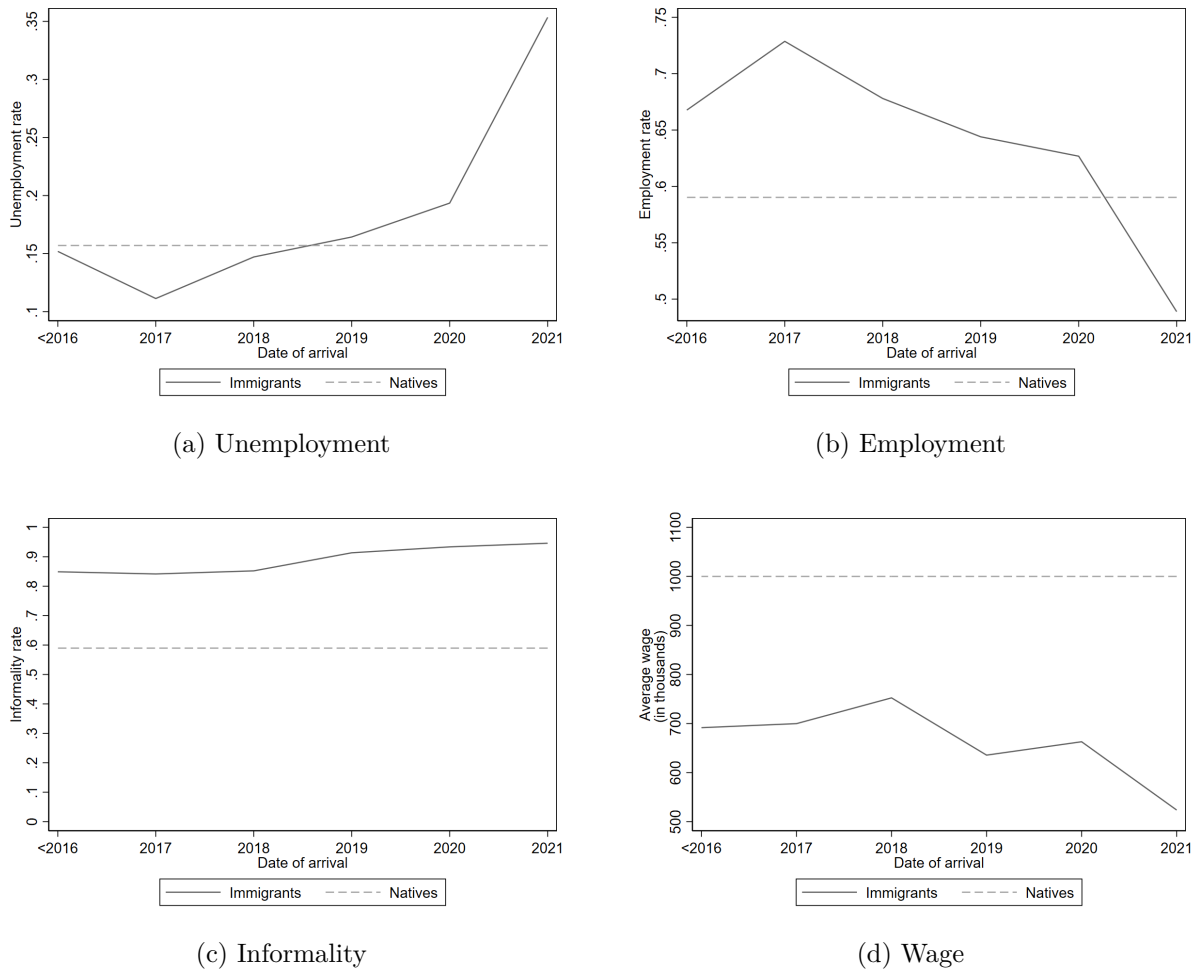
Table C.2.3: Impact of recent migration on labor market outcomes of earlier migrants, OLS and IV estimates

Variables	Status			Quality		
	(1) Employment	(2) Unemployment	(3) Labor Participation	(4) Hours worked	(5) Hourly wage	(6) Informality
Panel A. OLS estimates						
D2016 $\times$ Recent Immigration Rate	0.0012 (0.0257)	0.0138 (0.0274)	0.0170 (0.0245)	0.0480 (0.0295)	-0.0411 (0.0523)	0.0514 (0.0343)
Recent Immigration Rate	0.0157 (0.0132)	-0.0166 (0.0139)	0.0021 (0.0078)	-0.0117 (0.0121)	-0.1416*** (0.0324)	0.0479*** (0.0141)
D2018 $\times$ Recent Immigration Rate	-0.0203* (0.0108)	0.0246* (0.0128)	0.0001 (0.0065)	0.0235* (0.0125)	0.0651*** (0.0216)	-0.0340** (0.0128)
D2019 $\times$ Recent Immigration Rate	-0.0317** (0.0141)	0.0223 (0.0166)	-0.0176** (0.0073)	0.0134 (0.0145)	0.0310 (0.0315)	-0.0433*** (0.0123)
D2020 $\times$ Recent Immigration Rate	-0.0290 (0.0297)	0.0245 (0.0308)	-0.0123 (0.0176)	0.0627** (0.0249)	-0.0761*** (0.0237)	-0.0434*** (0.0153)
D2021 $\times$ Recent Immigration Rate	0.0178 (0.0247)	-0.0108 (0.0295)	0.0164 (0.0215)	-0.0055 (0.0331)	-0.0258 (0.0746)	-0.0358* (0.0184)
Constant	-0.4682*** (0.0513)	0.5637*** (0.0554)	-0.4048*** (0.0781)	3.4843*** (0.0558)	6.8818*** (0.1701)	1.0821*** (0.0651)
Observations	23,115	17,742	23,115	13,738	12,209	13,738
Adjusted R-squared	0.1885	0.0494	0.2211	0.0827	0.0964	0.0607
Panel B. IV estimates						
D2016 $\times$ Recent Immigration Rate	-0.0188 (0.0221)	0.0035 (0.0220)	-0.0222 (0.0191)	0.1083*** (0.0280)	-0.0856** (0.0431)	0.0365* (0.0192)
Recent Immigration Rate	0.0057 (0.0086)	-0.0101 (0.0098)	-0.0031 (0.0089)	-0.0121 (0.0138)	-0.2056*** (0.0304)	0.0702*** (0.0074)
D2018 $\times$ Recent Immigration Rate	-0.0325*** (0.0077)	0.0255*** (0.0083)	-0.0157** (0.0070)	0.0195** (0.0081)	0.0796*** (0.0251)	-0.0497*** (0.0080)
D2019 $\times$ Recent Immigration Rate	-0.0204* (0.0112)	0.0013 (0.0106)	-0.0268*** (0.0074)	0.0303*** (0.0115)	0.0239 (0.0332)	-0.0454*** (0.0088)
D2020 $\times$ Recent Immigration Rate	0.0041 (0.0214)	-0.0153 (0.0172)	-0.0079 (0.0175)	0.0922*** (0.0229)	-0.0754** (0.0374)	-0.0500*** (0.0125)
D2021 $\times$ Recent Immigration Rate	-0.0136 (0.0157)	-0.0231 (0.0180)	-0.0499*** (0.0161)	0.0385 (0.0255)	-0.0802 (0.0602)	-0.0190 (0.0176)
Constant	-0.4371*** (0.0712)	0.5463*** (0.0535)	-0.3825*** (0.0917)	3.4919*** (0.0543)	7.0128*** (0.1497)	1.0482*** (0.0657)
Observations	23,115	17,742	23,115	13,738	12,209	13,738
Adjusted R-squared	0.1873	0.0488	0.2193	0.0819	0.0915	0.0580
KP-stat	8.431	9.990	8.431	7.546	6.597	7.546
Mean of 2017	0.6166	0.2165	0.7871	3.8111	7.8615	0.9255

Notes: The set of covariates are those described in Equation 3.1. The mean of weekly hours worked and hourly wage for 2017 is in logs. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. Standard errors are clustered at the department level. *Source:* Authors' calculations using the national household survey (GEIH).

C.3 Explaining assimilation of earlier migrants

Figure C.3.1: Labor market outcomes in 2021, according to the arriving year of immigrants



Notes: Comparison of labor market outcomes is based on GEIH for the period July to November 2021. Source: Authors' calculations using the national household survey (GEIH) and the Migration Pulse (EPM).

Table C.3.1: Relation between arrival time and economic assimilation outcomes

Variables	(1) Labor participation	(2) Unemployment	(3) Employment	(4) Informality	(5) Labor income	(6) Labor income gap
Years in Colombia	0.004*** (0.000)	-0.013*** (0.000)	0.017*** (0.000)	-0.033*** (0.000)	0.048*** (0.001)	-0.071*** (0.010)
Observations	7,751	5,910	7,712	4,354	4,072	4,072
Adjusted R-squared	0.0350	0.0350	0.0350	0.0350	0.0350	0.0350

Notes: The number of observations changes from one regression to another due to the existing attrition between rounds 1 and 2 of the immigration survey. The set of covariates are gender, age, age-squared, education, a dummy for rural areas, and department-fixed effects. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. Standard errors are in parentheses. We use sampling weights for calculations. Source: Authors' calculations using the Migration Pulse (EPM).

Table C.3.2: Relation between arrival time and social assimilation outcomes

Variables	(1) Staying in Colombia	(2) Identification with the culture	(3) Non-immigrant friends	(4) Activities/organizations	(5) Government assistance
Years in Colombia	0.010*** (0.000)	0.012*** (0.000)	0.083*** (0.002)	0.003*** (0.000)	0.025*** (0.000)
Observations	7,946	5,989	5,988	7,946	7,946
Adjusted R-squared	0.0979	0.0979	0.0979	0.0979	0.0979

Notes: The number of observations changes from one regression to another due to the existing attrition between rounds 1 and 2 of the immigration survey. The set of covariates is gender, age, age-squared, education, a dummy for rural areas, and department-fixed effects. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. Standard errors are in parentheses. We use sampling weights for calculations. *Source:* Authors' calculations using the Migration Pulse (EPM).

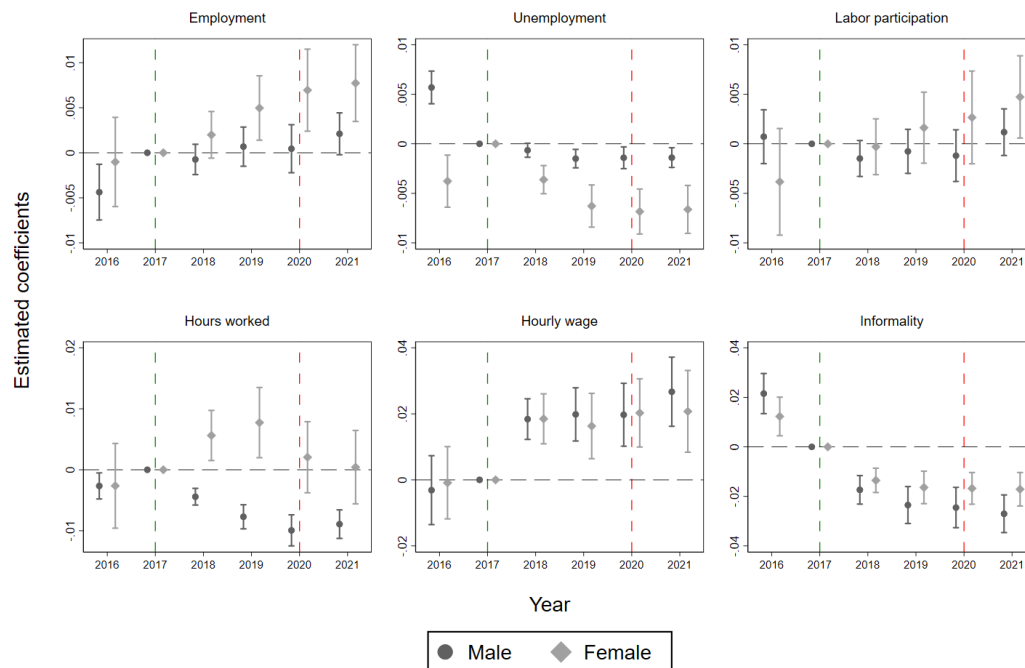
Table C.3.3: Relation between arrival time and coping mechanism to face pandemic

Variables	(1) Labor	(2) Migrate	(3) Reduce spending	(4) Reduce savings	(5) Apply for loans	(6) Receive aid
Years in Colombia	-0.001*** (0.000)	-0.003*** (0.000)	0.007*** (0.000)	0.014*** (0.000)	-0.009*** (0.000)	0.013*** (0.000)
Observations	7,946	7,946	7,946	7,946	7,946	7,946
Adjusted R-squared	0.0483	0.0483	0.0483	0.0483	0.0483	0.0483

Notes: The number of observations changes from one regression to another due to the existing attrition between rounds 1 and 2 of the immigration survey. The set of covariates is gender, age, age-squared, education, a dummy for rural areas, and department-fixed effects. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. Standard errors are in parentheses. We use sampling weights for calculations. *Source:* Authors' calculations using the Migration Pulse (EPM).

C.4 Heterogeneous effects

Figure C.4.1: Impact of migration on labor market outcomes of local workers, by gender



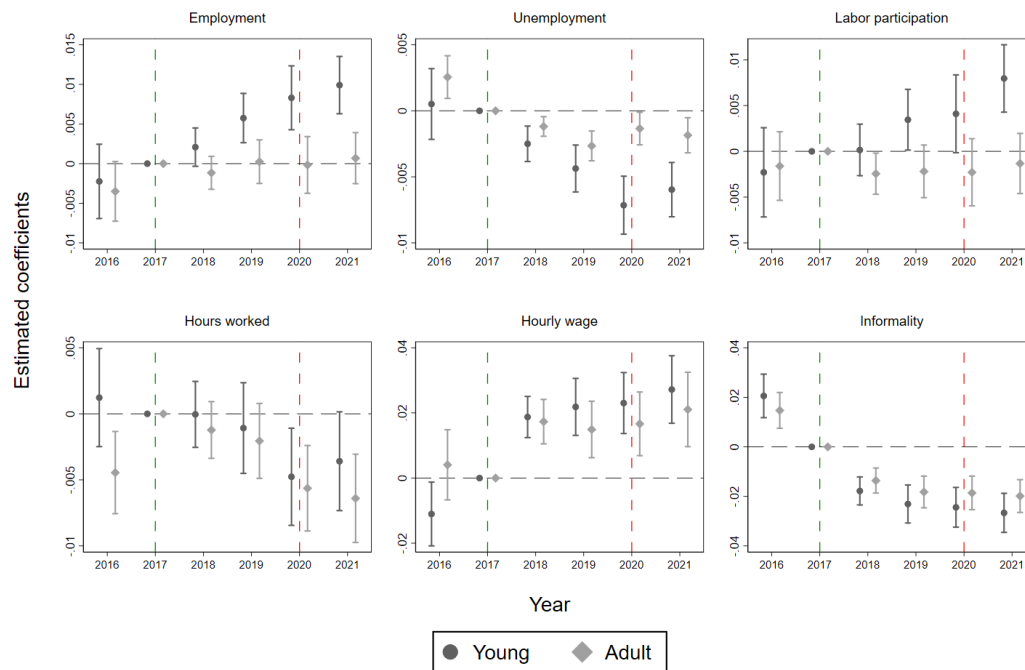
Notes: Calculations are based on the working-age population. The plotted coefficients correspond to the time-varying effects in Equation 3.1, and standard errors are clustered at the department level. A 95% confidence interval is computed based on standard errors. *Source:* Authors' calculations using the National Household Survey (GEIH).

Table C.4.1: Impact of migration on labor market outcomes of local workers, IV estimates by gender

Variables	Status			Quality		
	(1) Employment	(2) Unemployment	(3) Labor Participation	(4) Hours worked	(5) Hourly wage	(6) Informality
Panel A. Male						
D2016 × Total migration rate	-0.0044*** (0.0016)	0.0057*** (0.0008)	0.0007 (0.0014)	-0.0027** (0.0011)	-0.0031 (0.0053)	0.0215*** (0.0041)
Total migration rate	-0.0042** (0.0020)	0.0032*** (0.0009)	-0.0019 (0.0018)	0.0134*** (0.0014)	-0.0428*** (0.0082)	0.0463*** (0.0075)
D2018 × Total migration rate	-0.0007 (0.0009)	-0.0007* (0.0004)	-0.0015 (0.0009)	-0.0044*** (0.0007)	0.0184*** (0.0031)	-0.0174*** (0.0029)
D2019 × Total migration rate	0.0007 (0.0011)	-0.0015*** (0.0005)	-0.0008 (0.0011)	-0.0077*** (0.0010)	0.0198*** (0.0041)	-0.0235*** (0.0038)
D2020 × Total migration rate	0.0004 (0.0014)	-0.0014** (0.0006)	-0.0012 (0.0013)	-0.0099*** (0.0013)	0.0197*** (0.0049)	-0.0245*** (0.0042)
D2021 × Total migration rate	0.0021* (0.0012)	-0.0014*** (0.0005)	0.0012 (0.0012)	-0.0089*** (0.0012)	0.0267*** (0.0053)	-0.0270*** (0.0039)
Constant	-0.7737*** (0.0584)	0.4085*** (0.0250)	-0.7737*** (0.0616)	3.2449*** (0.0548)	6.3947*** (0.0940)	1.4674*** (0.1397)
Observations	1,291,840	1,038,190	1,291,840	925,616	828,572	925,631
Adjusted R-squared	0.2595	0.0425	0.3151	0.0484	0.3131	0.1968
KP-stat	23.56	24.14	23.56	23.62	24.89	23.62
Mean of 2017	0.7447	0.0879	0.8165	3.8446	8.3646	0.5826
Panel B. Female						
D2016 × Total migration rate	-0.0010 (0.0025)	-0.0038*** (0.0013)	-0.0038 (0.0027)	-0.0026 (0.0035)	-0.0009 (0.0056)	0.0123*** (0.0040)
Total migration rate	-0.0120*** (0.0035)	0.0063*** (0.0019)	-0.0109*** (0.0035)	-0.0059 (0.0043)	-0.0359*** (0.0101)	0.0313*** (0.0067)
D2018 × Total migration rate	0.0020 (0.0013)	-0.0036*** (0.0007)	-0.0003 (0.0014)	0.0056*** (0.0021)	0.0185*** (0.0039)	-0.0135*** (0.0025)
D2019 × Total migration rate	0.0050*** (0.0018)	-0.0063*** (0.0011)	0.0016 (0.0018)	0.0077*** (0.0029)	0.0163*** (0.0050)	-0.0164*** (0.0033)
D2020 × Total migration rate	0.0069*** (0.0023)	-0.0068*** (0.0012)	0.0027 (0.0024)	0.0021 (0.0030)	0.0203*** (0.0053)	-0.0168*** (0.0033)
D2021 × Total migration rate	0.0077*** (0.0022)	-0.0066*** (0.0012)	0.0047** (0.0021)	0.0004 (0.0009)	0.0207*** (0.0022)	-0.0171*** (0.0008)
Constant	-0.9580*** (0.0922)	0.6104*** (0.0359)	-1.0416*** (0.1037)	2.9619*** (0.0698)	6.1661*** (0.0924)	1.3845*** (0.0847)
Observations	1,453,277	854,017	1,453,277	704,101	637,428	704,114
Adjusted R-squared	0.1646	0.0590	0.1997	0.0442	0.3472	0.2643
KP-stat	23.51	23.74	23.51	24.02	24.75	24.02
Mean of 2017	0.5149	0.1524	0.6075	3.5962	8.2748	0.56087

Notes: The set of covariates are those described in Equation 3.1. The mean of weekly hours worked and hourly wage for 2017 is in logs. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. Standard errors are clustered at the department level. *Source:* Authors' calculations using the national household survey (GEIH).

Figure C.4.2: Impact of migration on labor market outcomes of local workers, by age group



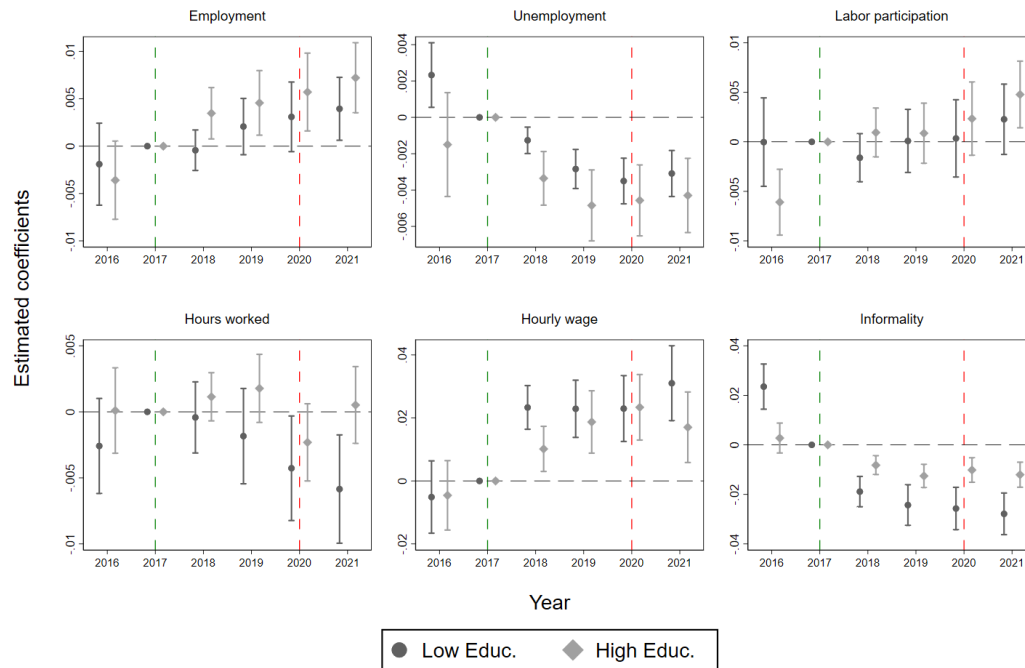
Notes: Adults are individuals aged 33 or older, and young respondents are younger than 33. Calculations are based on the working-age population. The plotted coefficients correspond to the time-varying effects in Equation 3.1, and standard errors are clustered at the department level. A 95% confidence interval is computed based on standard errors. *Source:* Authors' calculations using the national household survey (GEIH).

Table C.4.2: Impact of migration on labor market outcomes of local workers, IV estimates by age group

Variables	Status			Quality		
	(1) Employment	(2) Unemployment	(3) Labor Participation	(4) Hours worked	(5) Hourly wage	(6) Informality
Panel A. Age < 33						
D2016 × Total migration rate	-0.0022 (0.0024)	0.0005 (0.0014)	-0.0023 (0.0025)	0.0012 (0.0019)	-0.0110** (0.0050)	0.0206*** (0.0045)
Total migration rate	-0.0141*** (0.0032)	0.0068*** (0.0017)	-0.0130*** (0.0032)	0.0069*** (0.0026)	-0.0462*** (0.0090)	0.0447*** (0.0082)
D2018 × Total migration rate	-0.0021* (0.0012)	-0.0025*** (0.0007)	0.0001 (0.0014)	-0.0000 (0.0013)	0.0187*** (0.0032)	-0.0178*** (0.0029)
D2019 × Total migration rate	0.0058*** (0.0016)	-0.0044*** (0.0009)	0.0035** (0.0017)	-0.0011 (0.0018)	0.0218*** (0.0045)	-0.0231*** (0.0039)
D2020 × Total migration rate	0.0083*** (0.0021)	-0.0071*** (0.0011)	0.0041* (0.0022)	-0.0048** (0.0019)	0.0230*** (0.0048)	-0.0244*** (0.0041)
D2021 × Total migration rate	0.0099*** (0.0018)	-0.0060*** (0.0010)	0.0080*** (0.0019)	-0.0036* (0.0019)	0.0272*** (0.0053)	-0.0266*** (0.0040)
Constant	-2.0191*** (0.1121)	0.7753*** (0.0819)	-2.8056*** (0.1176)	1.5184*** (0.1310)	5.6703*** (0.1139)	2.5443*** (0.2018)
Observations	1,266,992	771,142	1,266,992	617,502	564,265	617,510
Adjusted R-squared	0.2729	0.0506	0.3456	0.0823	0.2726	0.2030
KP-stat	22.34	24.86	22.34	24.76	25.83	24.76
Mean of 2017	0.5163	0.1736	0.6248	3.7262	8.2875	0.53290
Panel B. Age ≥ 33						
D2016 × Total migration rate	-0.0035* (0.0019)	0.0026*** (0.0008)	-0.0016 (0.0019)	-0.0045*** (0.0016)	0.0041 (0.0055)	0.0147*** (0.0037)
Total migration rate	-0.0027 (0.0025)	0.0026** (0.0011)	-0.0007 (0.0026)	0.0060*** (0.0019)	-0.0342*** (0.0089)	0.0360*** (0.0064)
D2018 × Total migration rate	-0.0012 (0.0011)	-0.0012*** (0.0004)	-0.0024** (0.0011)	-0.0012 (0.0011)	0.0173*** (0.0035)	-0.0136*** (0.0026)
D2019 × Total migration rate	0.0002 (0.0014)	-0.0027*** (0.0006)	-0.0022 (0.0015)	-0.0021 (0.0014)	0.0149*** (0.0044)	-0.0182*** (0.0033)
D2020 × Total migration rate	-0.0002 (0.0018)	-0.0013** (0.0006)	-0.0023 (0.0019)	-0.0056*** (0.0017)	0.0166*** (0.0050)	-0.0186*** (0.0035)
D2021 × Total migration rate	0.0007 (0.0016)	-0.0019*** (0.0007)	-0.0013 (0.0017)	-0.0064*** (0.0017)	0.0211*** (0.0058)	-0.0199*** (0.0034)
Constant	-0.4350*** (0.0618)	0.1964*** (0.0169)	-0.3701*** (0.0662)	3.3381*** (0.0530)	6.5608*** (0.0900)	1.0287*** (0.1063)
Observations	1,478,125	1,121,065	1,478,125	1,012,215	901,735	1,012,235
Adjusted R-squared	0.1544	0.0234	0.1937	0.1039	0.3460	0.2379
KP-stat	24.58	23.42	24.58	23.28	24.27	23.28
Mean of 2017	0.7186	0.07697	0.7785	3.7427	8.3498	0.5988

Notes: The set of covariates are those described in Equation 3.1. The mean of weekly hours worked and hourly wage for 2017 is in logs. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. Standard errors are clustered at the department level. *Source:* Authors' calculations using the national household survey (GEIH).

Figure C.4.3: Impact of migration on labor market outcomes of local workers, by education level



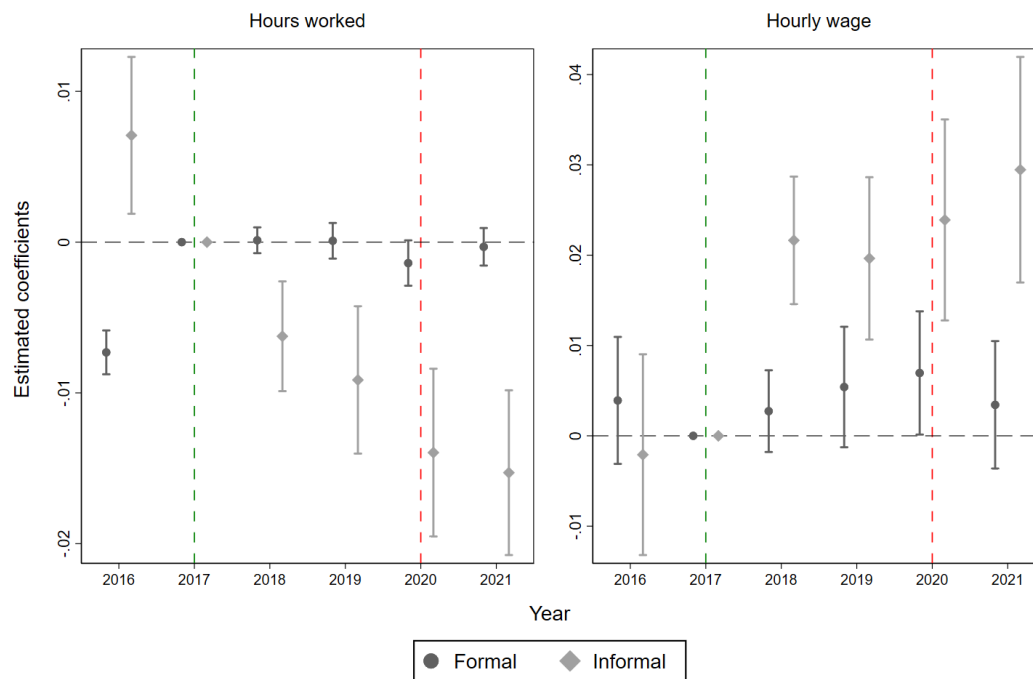
Notes: High-education respondents have more than 12 years of education. Low-education respondents are individuals with 12 or fewer years of education. Calculations are based on the working-age population. The plotted coefficients correspond to the time-varying effects in Equation 3.1, and standard errors are clustered at the department level. A 95% confidence interval is computed based on standard errors. *Source:* Authors' calculations using the national household survey (GEIH).

Table C.4.3: Impact of migration on labor market outcomes of local workers, IV estimates by educational level

Variables	Status			Quality		
	(1) Employment	(2) Unemployment	(3) Labor Participation	(4) Hours worked	(5) Hourly wage	(6) Informality
Panel A. Low education						
D2016 × Total migration rate	-0.0019 (0.0022)	0.0023** (0.0009)	-0.0000 (0.0023)	-0.0026 (0.0018)	-0.0051 (0.0059)	0.0235*** (0.0046)
Total migration rate	-0.0064** (0.0028)	0.0030*** (0.0010)	-0.0056* (0.0029)	0.0077*** (0.0026)	-0.0477*** (0.0092)	0.0477*** (0.0084)
D2018 × Total migration rate	-0.0004 (0.0011)	-0.0013*** (0.0004)	-0.0016 (0.0012)	-0.0004 (0.0014)	0.0233*** (0.0035)	-0.0189*** (0.0031)
D2019 × Total migration rate	0.0021 (0.0015)	-0.0028*** (0.0005)	0.0001 (0.0016)	-0.0018 (0.0018)	0.0229*** (0.0046)	-0.0243*** (0.0042)
D2020 × Total migration rate	0.0031* (0.0019)	-0.0035*** (0.0006)	0.0003 (0.0020)	-0.0043** (0.0020)	0.0230*** (0.0053)	-0.0257*** (0.0044)
D2021 × Total migration rate	0.0039** (0.0017)	-0.0031*** (0.0006)	0.0023 (0.0018)	-0.0058*** (0.0021)	0.0310*** (0.0061)	-0.0279*** (0.0043)
Constant	-0.9645*** (0.0733)	0.4974*** (0.0252)	-1.0434*** (0.0843)	2.9479*** (0.0610)	6.6356*** (0.1234)	1.3692*** (0.1549)
Observations	1,946,488	1,245,955	1,946,488	1,077,764	980,046	1,077,786
Adjusted R-squared	0.2605	0.0644	0.2814	0.1253	0.1297	0.1199
KP-stat	25.08	26.53	25.08	25.85	26.80	25.85
Mean of 2017	0.5863	0.1128	0.6609	3.7474	8.0738	0.7058
Panel B. High education						
D2016 × Total migration rate	-0.0036* (0.0021)	-0.0015 (0.0015)	-0.0061*** (0.0017)	0.0001 (0.0016)	-0.0046 (0.0056)	0.0028 (0.0031)
Total migration rate	-0.0115*** (0.0034)	0.0064*** (0.0021)	-0.0085*** (0.0025)	-0.0015 (0.0018)	-0.0374*** (0.0103)	0.0227*** (0.0045)
D2018 × Total migration rate	0.0035** (0.0014)	-0.0034*** (0.0008)	0.0009 (0.0013)	0.0011 (0.0009)	0.0102*** (0.0037)	-0.0082*** (0.0019)
D2019 × Total migration rate	0.0046*** (0.0017)	-0.0048*** (0.0010)	0.0009 (0.0015)	0.0018 (0.0013)	0.0187*** (0.0051)	-0.0126*** (0.0024)
D2020 × Total migration rate	0.0057*** (0.0021)	-0.0046*** (0.0010)	0.0023 (0.0019)	-0.0023 (0.0015)	0.0234*** (0.0053)	-0.0102*** (0.0025)
D2021 × Total migration rate	0.0072*** (0.0019)	-0.0043*** (0.0010)	0.0048*** (0.0017)	0.0005 (0.0015)	0.0170*** (0.0057)	-0.0121*** (0.0026)
Constant	-1.1226*** (0.0741)	0.7048*** (0.0283)	-0.8935*** (0.0863)	3.1653*** (0.0532)	5.0774*** (0.0748)	1.3467*** (0.0754)
Observations	798,629	646,252	798,629	551,953	485,954	551,959
Adjusted R-squared	0.1364	0.0447	0.1462	0.0352	0.3205	0.0540
KP-stat	18.72	18.29	18.72	18.71	18.85	18.71
Mean of 2017	0.7164	0.1260	0.8197	3.7134	8.8699	0.3001

Notes: The set of covariates are those described in Equation 3.1. The mean of weekly hours worked and hourly wage for 2017 is in logs. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. Standard errors are clustered at the department level. *Source:* Authors' calculations using the national household survey (GEIH).

Figure C.4.4: Impact of migration on labor market outcomes of local workers by formal status



Notes: Informality outcome is not calculated due to collinearity. Informality is defined following [OECD \(2019\)](#). Calculations are based on the working-age population. The plotted coefficients correspond to the time-varying effects in Equation 3.1, and standard errors are clustered at the department level. A 95% confidence interval is computed based on standard errors. *Source:* Authors' calculations using the national household survey (GEIH).

Table C.4.4: Impact of migration on labor market outcomes of local workers, IV estimates by formal status

Variables	(1) Hours worked	(2) Hourly wage
Panel A. Formal		
2016 $\times$ Total Immigration Rate	-0.0073*** (0.0007)	0.0039 (0.0036)
Total immigration rate	0.0005 (0.0010)	-0.0110 (0.0067)
2018 $\times$ Total Immigration Rate	0.0001 (0.0004)	0.0027 (0.0023)
2019 $\times$ Total Immigration Rate	0.0001 (0.0006)	0.0054 (0.0034)
2020 $\times$ Total Immigration Rate	-0.0014* (0.0008)	0.0070** (0.0035)
2021 $\times$ Total Immigration Rate	-0.0003 (0.0006)	0.0034 (0.0036)
Constant	3.7847*** (0.0296)	6.9024*** (0.0908)
Observations	708,509	650,541
Adjusted R-squared	0.1104	0.4211
KP-stat	28.89	30.02
Mean of 2017	3.8534	8.7386
Panel B. Informal		
2016 $\times$ Total Immigration Rate	0.0071*** (0.0027)	-0.0021 (0.0057)
Total immigration rate	0.0220*** (0.0039)	-0.0396*** (0.0083)
2018 $\times$ Total Immigration Rate	-0.0062*** (0.0019)	0.0216*** (0.0036)
2019 $\times$ Total Immigration Rate	-0.0091*** (0.0025)	0.0196*** (0.0046)
2020 $\times$ Total Immigration Rate	-0.0140*** (0.0028)	0.0239*** (0.0057)
2021 $\times$ Total Immigration Rate	-0.0153*** (0.0028)	0.0295*** (0.0064)
Constant	2.8286*** (0.0860)	6.5064*** (0.0950)
Observations	921,208	815,459
Adjusted R-squared	0.1225	0.1421
KP-stat	19.21	19.40
Mean of 2017	3.6491	8.0053

Notes: The set of covariates are those described in Equation 3.1. The mean of weekly hours worked and hourly wage for 2017 is in logs. *** significant at the 1%, ** significant at the 5%, * significant at the 10%. Standard errors are clustered at the department level. *Source:* Authors' calculations using the national household survey (GEIH).