Essays on the Economics of Immigration in Colombia

by

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Dissertation submitted in partial fulfillment of the
requirements for the degree of Doctor of Philosophy
in the Department of Economics
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ABSTRACT

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Abstract

Between 2015 and 2019, approximately 1.8 million Venezuelans fled into neighboring Colombia, increasing Colombia's population by almost 4%. In this dissertation, I study the effects of this large and unprecedented migration wave on Colombian labor market outcomes and attitudes towards foreigners. In Chapter 1, I study the economic effects of the migration using variation in the migration rate across 79 metropolitan areas, labor survey data, and an instrumental variable strategy based on historical migration rates. I find that Venezuelan migration caused a moderate decrease in the hourly wages of native Colombians that is most concentrated among low-wage and informal workers. Existing studies of this migration wave using similar methods and data have estimated different magnitudes for this wage effect, and I demonstrate the differences in specification that drive these discrepancies. In Chapter 2, I study the consequences of migrant occupational downgrading by estimating an aggregate production function that incorporates imperfect substitutability between migrants and natives and migrant occupational downgrading. I find that downgrading concentrates economic competition among less educated natives and decreases output in both the short- and long-term, thus affecting both wage equality and productivity. In Chapter 3, I study the effect of migration on trust towards foreigners using a nationwide survey on social preferences. While migration has no effect on trust on average, the effect is positive in municipalities that are more urbanized, have greater access to high-quality public goods, and where there is more residential integration between migrants and natives.

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Acknowledgements

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Introduction

Between 2015 and 2019, approximately 1.8 million Venezuelans fled into neighboring Colombia, largely fleeing poverty and violence induced by the economic and political crisis in Venezuela. This large and unprecedented migration wave increased Colombia's population by almost 4%, stimulated debate over the potential positive and negative economic consequences of the migration, and has become a focal point for economists interested in the labor market effects of migration in developing countries. The Colombian example is part of an alarming trend of increasing forced displacement around the world: the UNHCR estimates that the number of forcibly displaced worldwide increased from 41 million to 79.5 million between 2010 and 2019.

In Chapter 1 of this dissertation, I study the effects of this migration on the labor market outcomes of Colombian natives. I do this using labor force data combined with variation in the migration rate across 79 metropolitan areas, and I use an instrumental variable strategy based on the historical migration rate in a location to identify the causal effect of the migration. I find that the migration caused a moderate decrease in native hourly wages that is most concentrated among less educated and informal workers, and had no effect on native employment. Native movements across occupation skill groups and geography are small and do not play a meaningful role in mitigating local wage effects. Importantly, wage effects are larger in cities that have a higher baseline informality rate and lower ease of starting a business. The results motivate policies to support lower-income workers during large migration waves, as well as further research to better understand the mechanisms that drive the aggregate and distributional consequences of migration in the developing country setting.

The wage effects estimated in Chapter 1 are not as large as those from existing papers studying Venezuelan migration to Colombia that use similar data and methods. I explore the potential specification choices that drive this variation. Differences in how migration is measured are particularly important: when a subset of migrants is excluded from the migration measure, according to characteristics such as time of arrival, this amounts to an omitted variable bias that will tend to inflate the estimated wage effect. This presents an important lesson to immigration researchers.

In Chapter 2, I study the consequences of the occupational downgrading of Venezuelan migrants in Colombia: Despite being similarly educated as Colombian natives, Venezuelan migrants are disproportionately concentrated in lower-skill occupations such as restaurant work and domestic service. I estimate an aggregate production function that incorporates imperfect substitutability between migrants and natives and the occupational downgrading of migrants. As in Chapter 1, I estimate the model using variation in the migration rate and labor market outcomes across metropolitan areas and an instrumental variable strategy based on historical migration rates. I then use the model to calculate the total effect of migration on native hourly wages by education group, and I find results that are consistent with Chapter 1. Most importantly, in a counterfactual in which I undo migrant downgrading by reassigning migrants to compete with natives in their own education group, the hourly wages of less educated natives increase substantially in both the short- and long-term, while those of more educated natives are relatively unchanged. The increase in competition faced by more educated natives under the counterfactual is mitigated by lower migrant-native substitutability in high-skill occupations and counteracted by increases in total productivity that benefit all workers. The results

highlight the benefits of policies to reduce migrant downgrading for wage equality and productivity. This is especially true in the developing country setting, where the model indicates that the consequences of downgrading may be particularly severe, and where the majority of the world's forced displacement occurs.

Chapter 3 is a collaboration with coauthors Jonathan Moreno Medina (University of Texas at San Antonio) and Horacio Coral (Departamento Administrativo Nacional de Estadística en Colombia). We study the effect of the Venezuelan migration on native Colombians' trust towards foreigners. Given the recent emergence of anti-immigrant politics in many countries, this question has become pressing for policy-makers and researchers. Using data from the Colombian Survey of Political Culture and an instrumental variable strategy, we find that, while trust towards foreigners decreased in Colombia over this period, there is no evidence that proximity to migrants had a causal effect on trust. However, migration increases trust in foreigners in municipalities that are more urbanized, have better public goods provision, and where natives and foreigners are less residentially segregated. Given appropriate conditions, proximity to immigrants does not drive anti-immigrant sentiment and can in fact improve cooperative attitudes. Interestingly, migration does not induce a larger decrease in trust in foreigners for less educated and lower-earning natives, despite them facing a larger increase in economic competition.

Chapter 1 - The Labor Market Effects of Venezuelan Migration to Colombia

2.1 Introduction

In the canonical framework, an exogenous increase in labor supply induces a wage decrease when labor demand is downward-sloping, or a decrease in employment for natives whose wages fall below their reservation wage. Labor is distinguished by characteristics such as education, experience, or sector, and the effect of migration on any particular subgroup thus depends on the composition of migrants and their degree of substitutability with natives (Peri, 2016; Ottaviano & Peri, 2012; Borjas, 2003; Altonji & Card, 1991). Firms also react to increased labor supply by investing in additional capital, and we therefore expect persistence of economic effects to decrease in the speed of capital adjustment. Moreover, migrants boost consumer demand, transfer human capital and networks (Bahar *et al.* , 2019, 2020a), and can stimulate firm technological upgrading and native occupational upgrading (Foged & Peri, 2016). Thus, the effect of migration on native labor outcomes is not always expected to be negative, and will vary depending on the characteristics of the migration wave and the labor market. In general, when there are detrimental effects, we expect to see them most concentrated on natives with similar skills and working similar jobs as migrants.

The context of Venezuelan migration to Colombia is unique and interesting for various reasons. First, this is a developing country setting in which around 60% of natives and 90% of Venezuelan migrants are in the informal sector, defined according

to enrollment in mandatory health and pension schemes. The informal sector has no minimum wage and tends to have high turnover rates, increasing wage flexibility (Agudelo & Sala, 2017; Guriev *et al.* , 2019).[1] Furthermore, around 25% of natives and 30% of migrants are self-employed own-account (with no employees) workers, and thus compete directly over prices. Second, Venezuelan migrants and Colombian natives speak the same language and have a similar cultural background, which increases their substitutability in the workforce (Braun & Mahmoud, 2014). This also limits the scope for natives to respond to migration by upgrading to communication-intensive tasks (Peri & Sparber, 2009; Peri *et al.* , 2020). Third, Colombia has experienced extensive internal migration from decades of civil war and has an unemployment rate that has hovered between 8% and 11% since 2010, indicating limited capacity to mobilize capital to absorb an expanding workforce (Calderón-Mejía & Ibáñez, 2016; Morales, 2018). Finally, an important characteristic of this migration is the occupational downgrading of migrants: while Venezuelan migrants and Colombian natives have similar levels of education, Venezuelan migrants are heavily concentrated in occupations that typically require less education, such as restaurant work, construction, street vending, and domestic service. Because migrants in Colombia are mostly competing with less-educated workers in the informal sector, we expect economic effects to be most concentrated among these natives, with potential benefits for more educated natives (Dustmann *et al.* , 2013).

In the first part of this paper, I study the effects of the migration on native Colombians' economic outcomes. I do this using variation in the migration rate

[1]Many Venezuelans in Colombia have access to legal work status. However, in practice, the vast majority remain in the informal sector, where lack of work status is not a barrier to employment.

across 79 metropolitan areas constructed according to commuting patterns, data on the labor market outcomes of migrants and natives from the official labor survey of Colombia (the Colombian National Integrated Household Survey, or GEIH), and an instrumental variable strategy based on historical migration rates. I find that a 1 percentage point (pp) increase in the migrant share of the population decreases native hourly wages by 1.05%, and this decreases to -.59% after accounting for region-specific time trends. In 2019, the migrant share across metro areas varied from 1% at the 10th percentile to 8.6% at the 90th percentile, over which an effect size of -.59 is associated with a 4.5% wage decrease. While these wage effects do not vary significantly by age and gender, they are larger for less educated natives, especially those who are informal, self-employed, or working in low-skill occupations. These magnitudes are larger than typically observed in the literature and are consistent with evidence that the economic effects of migration tend to be largest in middle-income developing countries, especially for less educated workers in the informal sector (Verme & Schuettler, 2021).

Unlike with earnings, I find little evidence for effects on the employment margin, consistent with Colombian workers having low reservation wages. There is no effect of migration on unemployment. Among natives under age 25, migration causes a reduction in labor force participation that is partially explained by a reduction in school dropouts, but this is not robust to dropping metro areas close to the Venezuelan border with high migration rates.

This analysis is "non-structural" in that it makes no assumptions about mechanisms, which may include any of those discussed at the start of this introduction. This approach is the most common in the literature studying the labor market effects

of migration, and it is informative about the total effects of migration overall and for subgroups of natives, for example, by age, gender, education, or sector (Dustmann *et al.* , 2016).

After estimating this baseline specification, I conduct a sensitivity analysis to various specification choices, including choice of instrument, unit of geographic variation, migration data source, and definition of the migration share. The motivation for this is that a variety of studies have used a similar approach to study Venezuelan migration in Colombia (Caruso *et al.* , 2019; Delgado-Prieto, 2021; Penaloza-Pacheco, 2021; Santamaria, 2020; Bonilla-Mejía *et al.* , 2020a). While they are consistent in finding negative hourly wage effects concentrated on less educated natives, they find wildly different magnitudes for those wage effects, ranging from -0.5% to -7.6% in response to a 1pp increase in the migrant share.[2] I show that the smaller estimates can be explained by failing to use an instrument to account for endogenous sorting of migrants into locations, which biases the estimated wage effect towards zero. I show that among various factors that explain the larger estimates, the most important is related to implicit assumptions made while calculating the migrant share of the population. Specifically, by restricting migration to include only those who arrived over the past 12 months or by excluding Colombian-born return migrants, the estimated wage effect become substantially inflated due to what I argue is best understood as omitted variable bias.[3] This is because the location of previously arrived migrants

[2]Another paper, Rozo & Vargas (2021), studies the effects of Venezuelan migration on right-wing voting in Colombia. They also look at economic outcomes and find imprecise negative wage effects for natives. However, the magnitudes are not directly comparable with other papers because they estimate the effect of a change in predicted migrant share, rather than observed migrant share.

[3]Return migrants are those who were born in Colombia, migrated to Venezuela in the decades proceeding the Venezuelan crisis, and then returned to Colombia during the crisis. They make up around 20% of migrants who arrived from Venezuela between 2014-2019.

(or return migrants) is strongly correlated with that of recently arrived migrants (or Venezuelan-born migrants), and it is reasonable to expect that both groups affect the local economy.

To illustrate this concept, let "M1" represent the share of the population who are migrants who arrived in the past 12 months, and let "M5" represent the share who arrived in the past 13-60 months. Regress the average native log-wage in region k and year t on $M1_{kt}$ with region and year fixed effects[4]:

$$\ln W_{kt} = \beta M1_{kt} + \gamma_k + \delta_t + \epsilon_{kt} \tag{2.1}$$

Assume $M1_{kt}$ and ϵ_{kt} are uncorrelated conditional on the fixed effects. Let the true effect of $M1_{kt}$ and $M5_{kt}$ on $\ln W_{kt}$ be α_1 and α_5 respectively. Then β, the marginal effect of $M1_{kt}$ on $\ln W_{kt}$ (suppressing conditionality on the fixed effects from the notation for brevity), is

$$\hat{\beta} = \frac{\partial \ln W_{kt}}{\partial M1_{kt}} = \alpha_1 + \alpha_5 \frac{\partial M5_{kt}}{\partial M1_{kt}} \tag{2.2}$$

Thus, excluding $M5_{kt}$ from the regression generates an omitted variable bias equal to $\alpha_5 \frac{\partial M1_{kt}}{\partial M5_{kt}}$. Only if the excluded group is uncorrelated with the included group, or if the excluded group has no effect on native labor market outcomes, will this omission not generate a bias. If an instrument is being used for $M1_{kt}$, in which case $M1_{kt}$ can be replaced with $\widehat{M1}_{kt}$ in the above equations, then the problem persists so long as the instrument is also correlated with the excluded group. In this case, the

[4]The fixed effects are not necessary for this exercise, but I have included them to match the typical specification in the literature.

instrument based on historical migration rates is correlated with both 1-year and 5-year migrant shares, as well as with both foreign-born and return migrant shares. In the analysis in this paper, a regression of $M5_{kt}$ on the predicted $\widehat{M1}_{kt}$ and year and metro area fixed effects generates a coefficient of 2.6, such that even a small value of α_5 will substantially bias $\hat{\beta}$. For example, the true values of α_1 and α_5 could be -1.4 and -.8 respectively (consistent with a model in which the effects of migration dissipate over time), and this would generate a coefficient $\widehat{\beta} = 3.5$ (since $-1.4 - (.8 * 2.6) = -3.5$), much larger than the true α_1 of -1.4. As I will show, this is the estimate of $\widehat{\beta}$ when I only include past-year arrivals in the migrant share. As equation (2.2) makes clear, many other plausible effect sizes α_1 and α_5 would also be consistent with this estimate.

Of course, there are many theoretical reasons to believe that the economic effects of migration may differ according to migrant characteristics such as time of arrival, return-migrant status, or demographic characteristics. For example, the economic effects of migration tend to dissipate as capital mobilizes. In order to study this, one could include both $M1_{kt}$ and $M5_{kt}$ together on the right-hand-side, and this is a useful approach if there is sufficient independent variation in these variables to precisely estimate these coefficients. However, when an instrument is being used to account for the endogeneity of the migrant share, then this approach requires two instruments to generate independent variation in $M1_{kt}$ and $M5_{kt}$. In practice, such instruments are often difficult to find. If the groups are correlated, then absent independent exogenous variation in each group, one must either estimate the average effect of both groups jointly, assume that the excluded group has no effect on labor

market outcomes, or accept the bias that results from studying a group in isolation.[5]

It is worth noting that this discussion is closely related to one in the migration economics literature around the use of the "skill-cell" approach, in which data are divided into education-experience cells and labor outcomes are regressed on the cell-specific migrant share and cell fixed effects. This approach identifies the "partial effect" of migration on wages within an education-experience group given fixed supplies in other groups. It therefore does not account for potential effects on workers across cells, which is a necessary component of the total wage effect (Ottaviano & Peri, 2012; Dustmann *et al.* , 2016).

In the remainder of the paper, I conduct additional analysis that extends on the existing literature. I fail to find evidence for non-linear effects of migration on the logarithm of wages, though there is not enough variation in the data to identify non-linear effects at very high migration rates. I document a small internal migration response to the Venezuelan migration, and I confirm that native spatial arbitrage does not bias labor market estimates in this context (Borjas, 2003; Borjas & Katz, 2007; Monras, 2020). I study native employment across occupation skill groups ranked according the pre-migration mean years of schooling in each occupation. I find little average effect on occupational skill level and small movements among some demographic groups. Men with completed secondary schooling experienced minor upgrading from low- to middle-skill occupations, while men with post-secondary education experienced minor downgrading from high- to middle-skill occupations alongside in-

[5]Note that it is not satisfactory to simply use an instrument for one group, $M1_{kt}$, while controlling for the other, $M5_{kt}$. If $M5_{kt}$ is correlated with the instrument and the error term, this is an "endogenous controls" problem and $\widehat{\beta}$ will be inconsistent. See Frölich (2008) for a discussion of endogenous controls in OLS and 2SLS. Non-parametric methods may allow for consistent estimates in the absence of a second instrument for $M5_{kt}$, but this is demanding and requires extensive independent variation in the endogenous variables.

creases in self-reported under-employment. There is also a small movement of natives *out* of the formal sector in response to the migration. Thus, while some studies have found that migration stimulates native upgrading to higher-skill occupations (Peri & Sparber, 2009; Foged & Peri, 2016; Peri *et al.* , 2020), in this context this did not occur on a large scale, though there were winners and losers among some subgroups. Finally, I document that wage effects are slightly larger in locations with higher baseline informality rates and lower ease of starting a business as measured in the World Bank Doing Business report, indicating the local economic characteristics are relevant for the economic consequences of migration.

Importantly, the results from this paper are short-term, and both theory and existing empirical evidence predict that the effect of migration on native wages should recover and possibly become positive in the long-term (Edo, 2020; Verme & Schuettler, 2021). However, this is not necessarily true for the distributional consequences of migration, which often persist. The results motivate policies to support lower-income workers during large migration waves, as well as further research to better understand the mechanisms that drive the aggregate and distributional consequences of migration in the developing country setting, to enable policymakers to minimize the costs and maximize the benefits of migration.

Literature: The magnitude of the average wage effect found in this paper is large but not unheard of in the literature, in the context of a large, sudden migrant arrival and short-run outcomes. For example, Edo (2020) finds that the repatriation of Algerians to France led to corresponding native wage effects between -1.3% and -2%, though wages recovered after 10 years. In Dustmann *et al.* (2017), studying the 1991

inflow of Czech workers into Germany, the corresponding elasticity is a smaller .13% fall in native wages alongside reductions in employment. Studies of migration from the former Soviet Union to Israel in the 1990's also find small negative wage effects concentrated among less educated natives that disappear after 4-7 years (Cohen-Goldner & Paserman, 2011; Friedberg, 2001). For various other episodes of forced displacement in high-income settings, the literature has found little to no effects on native employment or wages. This includes Cuban refugees in Miami in the mid-1980s, refugee dispersal in the US between 1980-2000, migration from former Yugoslavia into the EU, and Puerto Ricans in Orlando after Hurricane Maria (Peri *et al.* , 2020; Peri & Yasenov, 2019; Clemens & Hunt, 2019; Mayda *et al.* , 2017; Card, 1990), though the lack of null results has been disputed in some cases (Borjas & Monras, 2017; Borjas, 2017). In other settings, effects are positive: Foged & Peri (2016) finds that late 1980s refugee dispersal in Denmark increased native low-skill wages and increased the complexity of native jobs. In a recent meta-analysis of the forced displacement literature, Verme & Schuettler (2021) finds that native wage and employment effects are typically insignificant, and when significant, they tend to be negative. These negative effects tend to be largest for less educated and informal workers, in middle-income countries, and whetn the migrant supply increase is large relative to the native workforce. They tend to dissipate after 5 years.

An emerging literature studies the economic effects of forced displacement in low- and middle-income countries. An important example is Syrian refugee migration to Turkey, where the supply increase was also mostly in the informal sector. Studies of this migration wave find negative effects on native informal employment, alongside positive effects on formal employment (Del Carpio & Wagner, 2015; Ceritoglu *et al.*

, 2017; Tumen, 2016; Altındağ *et al.* , 2020) and increasing native task complexity (Akgündüz & Torun, 2018). This could result from language and cultural barriers creating the potential for communication-intensive occupational upgrading by natives (Peri & Sparber, 2009). While many studies find negligible wage effects in Turkey, Aksu *et al.* (2018) finds a negative wage effect in the informal sector that is decreasing in education and Cengiz & Tekgüç (2021) finds imprecise negative wage effects in the informal sector using a synthetic control method. As discussed, one reason that wage effects may be larger in Colombia is that Venezuelan migrants speak the same language, increasing substitutability with natives. Another is that employment effects are smaller in Colombia, leaving effects concentrated along the earnings margin.[6] In contrast, Fallah *et al.* (2019) find no economic effects of Syrian refugees in Jordan, potentially explained by low refugee labor force participation alongside increases in EU aid and trade concessions.

Studies of the economic effect of Venezuelan migration in other countries in Latin America have also found evidence for negative native wage effects most concentrated on less educated and informal workers, in Ecuador (Olivieri *et al.* , 2021a), Brazil (Zago, 2020) and Peru (Morales & Pierola, 2020).[7] In Brazil, migration is found to affect participation rather than wages when using a synthetic control method (Ryu & Paudel, 2021). Finally, there is a literature studying the labor market consequences of

[6]In the low-income country setting, various papers in Africa have evaluated the impact of forced displacement on nearby communities. This setting can be characterized by refugees hosted in camps and large inflows of foreign aid, which differs heavily from the Colombian context as I discuss in Section 2.2. These studies have generally found positive effects on local employment and household consumption (Alix-Garcia & Saah, 2010; Maystadt & Verwimp, 2014; Ruiz & Vargas-Silva, 2015; Alix-Garcia *et al.* , 2018).

[7]More recent estimates from Peru show less clear evidence of negative wage effects (Boruchowicz *et al.* , 2021). A possible explanation is migrant selection - Venezuelans who go to Peru are even more educated on average than those who go to Colombia.

internal displacement during Colombia's civil war between the 1980's and the early 2000's. These papers have found negative wage effects for urban workers ranging from -.09% to -1.4%, which are consistently larger for low-skill and informal workers (Calderón-Mejía & Ibáñez, 2016; Morales, 2018). My results are consistent with these estimates.

This paper proceeds as follows: Section 2.2 gives background on Venezuelan migration to Colombia. Section 2.3 and 2.4 review the data and empirical specification, and Section 2.5 presents the baseline results. Section 2.6 tests sensitivity to various specification choices and uses these results to reconcile findings in the literature. Sections 2.7 and 2.8 conduct additional robustness tests and analysis, and Section 2.9 concludes.

2.2 Contextual Background

Between 2015 and 2019, around 4.5 million people fled Venezuela, making Venezuelans the second-largest internationally-displaced population after Syrians (UNHCR, 2019). The primary reasons for this migration were to escape poverty and violence induced by political and economic crisis. Following the sudden collapse of global oil prices in 2014, Venezuela entered an economic recession that led to hyperinflation by 2016. By 2018, GDP had contracted by 45% since 2013, and around 90% of the population was estimated to be living in poverty. Over 20% of the population was undernourished, access to water and electricity became increasingly scarce, and an estimated 85% of essential medicines were scarce. The murder rate also rose to one of the highest in the world (Wilson Center, 2019; Reina *et al.* , 2018; World Bank,

2018). The primary reasons for migration that Venezuelans cite include shortages of food and medicine, violence and insecurity, lack of access to social services and fear of political persecution (UNHCR, 2018).

Colombia, the neighbor closest to the population centers of Venezuela, received an estimated 1.8 million of these migrants, more than any other country. Figure 2.1 shows that this is the first time Colombia has received a large migration wave from another country: in the 1993 census, .13% of the population was Venezuelan born and .2% was born in a different foreign country, and these rates remained relatively constant until the onset of the Venezuelan migration in 2015.[8] The arrival rate increased in 2016 and again in 2017, with the majority of migrants arriving between 2018-2019. The results presented in this paper therefore reflect very short-run economic effects.

Figure 2.2 shows the migrant share of the population across 79 metro areas in 2019, where a migrant is defined as someone who was living in Venezuela 5 years ago. There is extensive variation in these migrant shares across Colombia. They tend to be largest closer to the Venezuelan border, in many cases exceeding 10% of the metro area population. In Cúcuta and Riohacha, two cities close to the primary entry points along the Venezuelan border, the migrant shares are around 16% and 11% respectively. In Bogotá, Medellín and Cali, the three largest cities in Colombia, the shares range between 4-5%. For other cities they remained below 1%.

The majority of migrants crossed at a handful of official border crossings, which

[8]The lack of migration into Colombia pre-2015 reflects the fact that Venezuela was historically a recipient, rather than a source, of immigrants. Favorable economic conditions and generous social programs attracted migrants from across Latin America, including Colombians fleeing the decades-long civil war in Colombia (Freitez, 2011).

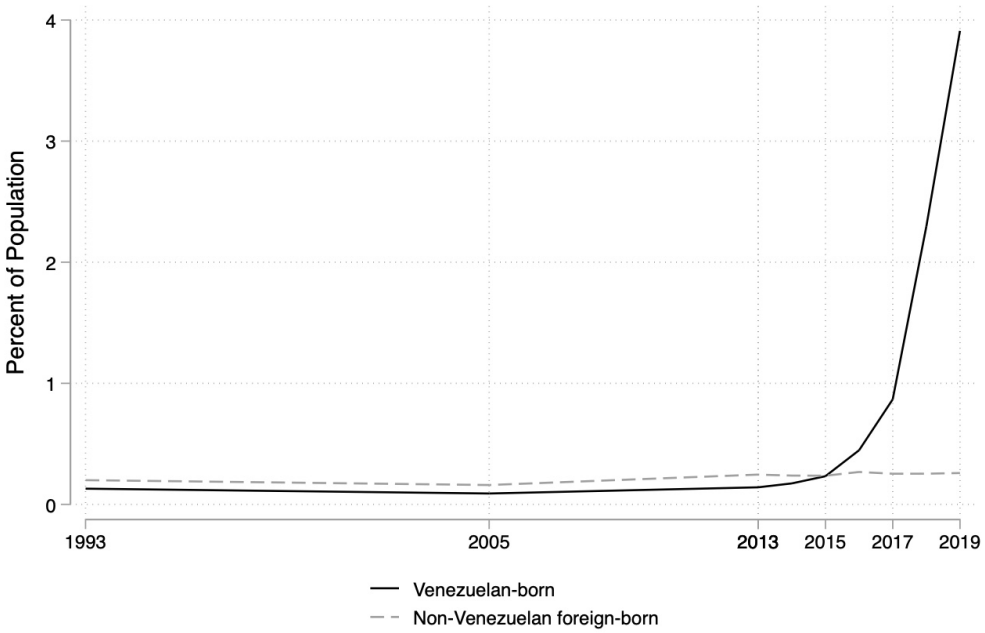


Figure 2.1: Foreign-Born Population in Colombia

Source: GEIH 2013-2019, 1993 and 2005 Census

required a passport or a VISA that allowed for short-term access to the border regions. However, those without legal documents could pass around border checkpoints on paths commonly known as "trochas". The Colombian government created a temporary resident VISA beginning in January 2017 (the Permiso Temporal de Permanencia, or PEP) that allowed documented migrants to access the formal labor force and education and health services. This status was offered to a large number of undocumented migrants starting in April 2018, in an attempt to regularize the growing number of migrants who had either entered illegally or overstayed their temporary permit (the Registro Administrativo de Migrantes Venezolanos, or RAMV). By the end of 2019, according to official numbers from the Colombian migration authorities, an estimated 754,000 Venezuelan migrants were regularized, corresponding to approximately 42% of the Venezuelan migrants estimated to be in the country (Migración

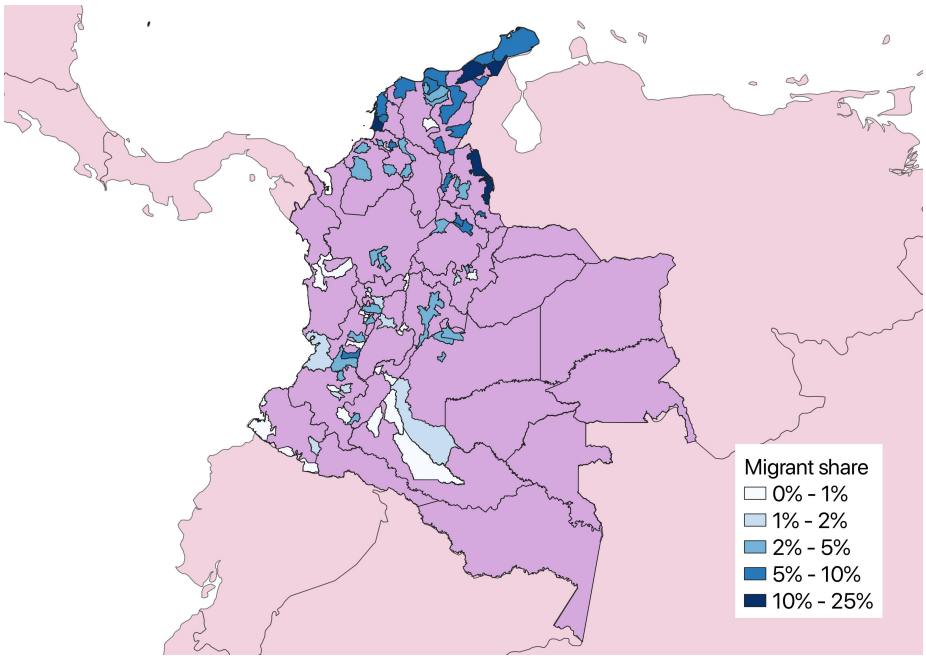


Figure 2.2: Migration Across Metropolitan Areas of Colombia

Source: GEIH 2019

Colombia, 2019). Furthermore, Bahar *et al.* (2021) show that the 2018 RAMV regularization had little to no effect on native labor outcomes, likely because registered migrants mostly remained in the informal sector. In summary, there was little control over who entered the country and where migrants went within Colombia, and despite many having access to regularization, the majority of migrants remained undocumented and informal. Both documented and undocumented migrants are included in my data and I do not observe documentation status.

Another important characteristic of this migration wave is that, unlike with many other global episodes of forced displacement, relatively little international aid has been dedicated to the reception of Venezuelan migrants. By 2019, international funding for the Venezuelan migration crisis was at $580 million, compared to $7.4 billion that went to displaced Syrians in the first 4 years of the Syrian crisis (Bahar

& Dooley, 2019). Furthermore, few migrants are living in camps, and the Colombian government has been unable to mobilize large-scale aid or investment into areas most impacted by migration (Migration Policy Institute, 2020).

Migration can also affect local economies by increasing local consumer demand, especially if migrants carry savings (Verme & Schuettler, 2021; Cortes, 2008). However, it is estimated that Venezuelan migrants' household expenditures in Colombia are less than half that of natives, reflecting migrants' low income in Colombia and the fact that savings for many Venezuelans was wiped away by inflation (Tribín-Uribe *et al.* , 2020). Delgado-Prieto (2021) studies the effect of migration on the consumer price index across 23 capital cities in Colombia, and finds precise null effects, though there is evidence for small increases in the cost of education and small decreases in the cost of healthcare. Overall, the demand effects of migration in this context appear to be modest, and I do not study affects on prices in this paper. The literature would benefit from a more detailed analysis of the effect of Venezuelan migration on local prices of different goods and services. To the extent that increased demand directly affects wages, this will be captured in the estimated wage effects.

2.3 Data and Descriptive Statistics

Labor market outcomes come from the Colombian National Integrated Household Survey (GEIH), which is a large-sample cross-sectional survey collected by the National Department of Statistics (DANE) and is the official source for labor market indicators in Colombia. Since 2013, this survey has included a migration module that can be used to identify migrants, and I therefore also use this survey to measure

migration shares across regions and over time. This module also allows me to restrict labor market outcomes to non-migrants, to avoid any compositional effects driven by arriving migrants.

It is important to note that the GEIH is not intended to be representative of Venezuelan migrants in Colombia. Another potential source of the migrant share across regions is the 2018 census, but this comes with two caveats.[9] First, security concerns and other unexpected logistical constraints led this census to undercount the Colombian population by 8.5%, generating scepticism over the ability of the census to accurately measure the migrant population (El Espectador, 2019). Second, this only measures a snapshot of the migrant population from January-September 2018, missing the dynamics of migrant arrival before 2018 and the large inflow of migrants in the last quarter of 2018 and 2019. For these reasons, the GEIH is increasingly being used to track the Venezuelan population across Colombia over time (Graham *et al.* , 2020; Tribín-Uribe *et al.* , 2020). In practice, migration rates under the census and the GEIH are closely correlated ($\rho = .77$ across metro areas in my sample in 2018). In Section 2.6, I test sensitivity to using the 2018 census to measure the migrant share, and I show that results are comparable but slightly larger in magnitude when using the census to estimate the effects of migration through 2018.[10]

I define migrants as anyone who was living in Venezuela 5 years ago. Importantly, this includes Colombian-born return migrants, who make up around 20% of

[9] Another possible source is the official estimate from the Colombia Migration Unit, which is imputed based on a combination of border flows, registration of undocumented Venezuelans, and the 2018 census. However, these numbers likely undercount undocumented migrants, especially before 2018 and in locations farther from the border (Tribín-Uribe *et al.* , 2020; Graham *et al.* , 2020).

[10] The complete 2005 and 2018 censuses were provided by DANE. I will also use the 1993 census 10% subsample which was accessed via IPUMS (2019).

all migrants from Venezuela during this period. Many of these returnees migrated to Venezuela during Colombia's decades-long civil war. They are on average older and less educated than Venezuelan-born migrants, and there are various reasons to believe that their effect on the labor market may differ.[11] However, many have been in Venezuela for decades (the first large migration to Venezuela was in the 1970s), and may be more comparable with Venezuelan-born migrants than with non-migrants in terms of characteristics such as networks and work experience. In practice, their location in Colombia is closely correlated with that of Venezuelan born-migrants ($\rho = .71$ across metro areas in 2019). Therefore, as I discussed in Section 2.1, excluding them from the migrant share could introduce bias. Indeed, I show in section 2.6 that the magnitude of the wage effect increases substantially when return migrants are excluded. While ideally one could study the effect of each group in isolation, these effects are empirically difficult to untangle due to a lack of independent variation and in the absence of a separate instrument for each group.

Colombia is divided into a capital district and 32 departments, which are further divided into 1,122 municipalities. In order to generate a geographic unit that represents a contiguous labor market in which workers compete, I group municipalities into metropolitan areas according to commuting patterns. Following Duranton (2015), I use a recursive algorithm based on a 10% commuting threshold using commuting data from the 2005 census, which is the latest year before the start of the migration period in which such data are available.[12] The algorithm results in 184

[11] In the working-age metro sample of the 2019 GEIH, the average age of return and Venezuelan-born migrants is 38.7 and 30.0 respectively. Completed years of schooling is 8.2 and 10.4 respectively.

[12] Specifically, a municipality is grouped with another if over 10% of its residents commute to work in that municipality. They are then treated as a single unit in the next round of the algorithm, and this is repeated until no more municipalities meet this threshold. In practice, the number

metro areas with at least 30,000 residents in 2005. These can be compared with the 23 metro areas officially defined by the GEIH: in some cases they are identical (for example, in the case of Medellín), and in other cases they are distinct (for example, the constructed areas of Bogotá, Calí, and Baranquilla all include substantially more municipalities than the administrative metro areas). Given that the administrative metro areas are politically determined in consideration of factors such as allocation of city resources and jurisdiction of city government activity, it is preferable to use metro areas constructed according to economic criteria (Duranton, 2015).

A concern about measurement error arises when the GEIH is used at such a fine geographic level, since it is only designed to be representative at the level of the department or the 23 official metro areas. I address this in various ways. First, I restrict analysis to the 79 metro area that contain at least 300 observations per year. These represent around 80% of the Colombian population and 90% of the Venezuelan migrant population. Second, I test the robustness of results to increasing the annual observation threshold to 1,000, essentially restricting analysis to the 23 metro areas for which the survey is officially representative. Third, in my analysis I use an instrument based on migration shares from the complete 2005 National Census, which will mitigate the effect of measurement error in the endogenous variable. Finally, I test robustness to using alternative geographic units (including the department level) in Section 2.6, and I find that choice of geographic unit only has a moderate effect on the results.

I now describe the characteristics of working-age (15-64) migrants and natives

of metro areas generated by this method does not depend on the choice of commuting threshold. See Duranton (2015) for details.

in the 79 metro areas of analysis in the 2019 GEIH using sampling weights, which has a national sample of 447,264 natives and 21,730 migrants. Table A1 shows that migrants are gender balanced and 5 years younger than natives on average. Figure A1 shows that migrants come most heavily from the middle of the education distribution: 44% have completed secondary relative to 35% of natives. However, over 20% of migrants have some post-secondary, indicating that this is not a low-skill migration wave, and the educational profile of migrants and natives is broadly similar. However, Figure A2 shows that the majority of migrants are concentrated in occupations that tend to employ less educated natives, by plotting the distribution of migrants and natives across occupations ranked according to mean years of completed schooling for natives.[13] In particular, they are severely over-represented in street vending, restaurant work, construction, domestic service, and beautician work. Thus, despite migrants and natives being similarly educated, we expect economic consequences to be larger for less educated natives working in heavily impacted occupations. In terms of economic outcomes, migrants have higher labor force participation than natives (79.4% relative to 71.7%) and higher unemployment rates (14.8% relative to 11.4%). They work more hours and have a median hourly wage that is around 70% of that of natives. They are also more likely to be own-account workers and almost 90% are informal.

[13]Occupations are recorded in the GEIH using the 82 classifications of the ISCO-68. They are ranked by the average years of schooling for natives between 2010-2015.

2.4 Empirical Specification

I estimate the following regression across metro areas (c) and years (t) from 2014-2019:

$$Y_{ct} = \beta M_{ct} + \gamma_c + \delta_t + \epsilon_{ct} \tag{2.3}$$

where M_{ct} is the migrant population as a share of the 2014 population. Metro fixed effects, γ_c, hold constant all time-invariant metro characteristics, and year fixed effects, δ_t, adjust for national-level changes in labor outcomes. Standard errors are clustered at the metro level to allow for the error to be correlated within metro areas over time, and observations are weighted by the population within each metro-year cell.[14]

Labor market outcomes, Y_{ct}, are averaged at the metro-year level and include labor force participation, unemployment, hours worked in a typical week and log hourly wage including all overtime, benefits and other transfers. This information is collected for all workers regardless of self-employment or formality status, and going forward I use hourly wage to refer to hourly earnings for wage workers or hourly profits for self-employed. These outcomes are residualized from a regression on gender, age, and education fixed effects, though this adjustment has little effect on the results.[15] Residual log wages are windsorized at the top and bottom 0.5% of observations to reduce the impact of extreme outliers.

There has been recent concern regarding bias in two-way fixed effects models

[14] I choose not to use the GEIH sampling weights since they are not designed to be representative at this level, but my results are not sensitive to this decision.

[15] Age is grouped into 5 year intervals, and education is grouped into less than primary, less than secondary, completed secondary, and post-secondary.

when there are dynamic treatment effects (Callaway & Sant'Anna, 2020; Goodman-Bacon, 2021; Borusyak & Jaravel, 2021; De Chaisemartin & d'Haultfoeuille, 2020). Intuitively, in two-way fixed effect models, groups that are "treated" later in the study period use previously treated groups as a control, even though they may still be experiencing a dynamic treatment effect. While these papers generally deal with models that have a binary treatment, the same concern in principle extends to a continuous treatment framework.[16] In the case of this paper, lagged economic effects of migration in preceding years can bias the estimate of the treatment effect in later years. However, I do not expect this to be a large problem because the majority of migrants arrive in 2018 or 2019, at the end of the study period. To confirm that this is not biasing β, I show a robustness specification in which I regress the 2014-2019 change in outcome on the change in the migrant share and I find very similar results. This implies that most of the estimation is driven by differences in total arrivals across metro areas and not by different timing of the arrival across metro areas.

One cannot simply compare labor market outcomes in metro areas that experienced different migration levels because migrants select destinations according to endogenous characteristics. For example, if migrants are more likely to settle in cities with greater expected growth in available jobs or wages, this would induce a positive correlation between ϵ_{ct} and M_{ct} and an upward bias in the estimate of β. I deal with

[16] One paper, De Chaisemartin & d'Haultfoeuille (2020), develops an estimator robust to heterogeneous treatment effects that works with a non-binary treatment. However, it only extends to discrete treatments (as opposed to continuous) and is not identified when treatment takes too many values. When I apply the estimator developed in this paper with migrant shares grouped into intervals, the resulting confidence intervals are not informative.

this endogeneity by constructing the following instrument:

$$Z_{ct} = M_{c,2005} * M^{-c}_{Nat,t} \tag{2.4}$$

where $M_{c,2005}$ is the 2005 Venezuelan share of the population in metro area c according to the complete 2005 population census, and $M^{-c}_{Nat,t}$ is the "leave-one-out" national share of migrants from Venezuela to all metro areas in year t, excluding migration into area c (Card, 2001; Tabellini, 2020). This "leave-out" factor reduces the correlation between national-level inflows and large inflows into certain cities, which may be correlated with time-changing characteristics of those cities.

This variable is strongly predictive of subsequent migrant shares, creating the strong first stage needed for consistent 2SLS estimates. Figure A3 displays the positive linear relationship between the 2005 Venezuelan share (which ranges from 0% to .9%) and the 2019 migrant share (which ranges from 0% to 25%). There is strong persistence over time in the distribution of Venezuelan migrants across metro areas, likely driven by the tendency of migrants to locate where they have migrant networks (Beaman & Magruder, 2012; McKenzie & Rapoport, 2007; Munshi, 2003). When the instrument in equation 2.4 is used with metro and year fixed effects, the first stage Kleibergen-Paap Wald statistic is 23.[17]

The variable $M^{-c}_{Nat,t}$ is assumed to be exogenous, driven primarily by push factors in Venezuela and uncorrelated with any changes occurring in Colombia. Unemployment in Colombia steadily increased from 8.5% to 10% from 2014-2019, indicating

[17]The Kleibergen-Paap LM test tests the null that the structural equation is underidentified. With a single endogenous regressor, this reduces to a standard first-stage F-statistic that is heteroskedasticity robust.

that there were not favorable labor market conditions attracting migrants over this period. Furthermore, migration did not increase from any other country. Of greater concern is the potential endogeneity of $V_{c,2005}$, which may be correlated with *changes* in economic outcomes between 2014-2019 in metro c. This concern is mitigated by the fact that historical migrant shares were determined two decades before the onset of the Venezuelan exodus, well before the election of Hugo Chávez. While I use the 2005 census to construct the instrument, I show that results using the 1993 census are similar. The correlation between the migrant share in these years is .89.[18] The instrument would also be invalid if migration before 2005 stimulated dynamic economic responses or subsequent migration correlated with current economic trends (Jaeger *et al.* , 2018). However, there was almost no migration into Colombia between 2005-2015, and the 2005 migrant share was miniscule relative to the current migration - in no metropolitan area in 2005 was the Venezuelan-born share greater than 1%.

It remains possible that migrants historically selected into cities that had differential economic trends between 2014-2019. While this cannot be formally tested, I conduct various checks and robustness tests recommended by the literature (Goldsmith-Pinkham *et al.* , 2020). First, I check for a correlation between the 2005 shares and pre-period economic outcomes. Table A2 shows a regression of the 2005 Venezuelan share on a set of metro area characteristics measured in 2014, before the onset of the migration. The results show that a 1% increase in wages is associated with a .02 SD decrease in the 2005 migrant share. Hours, labor force participation, unemployment, and total population are all insignificant and small in magnitude. Importantly, the

[18] I prefer to use the 2005 census to construct the instrument because I have access to the complete census in this year, which helps to minimize measurement error.

R^2 is small, indicating that only 9% of the variation in the 2005 migrant share can be explained by all of these variables together. The largest concern is that places with more immigration in 2005 had slightly lower wages in 2014.

These fixed metro characteristics are controlled for in the analysis. It is arguably more relevant to test if historical migrant shares are correlated with pre-trends in economic outcomes leading up to the migration. I run the following event-study model:

$$Y_{ct} = \sum_{y=2010}^{2014} [\sigma_y M_{c,2005} * (t = y)] + \sum_{y=2016}^{2019} [\sigma_y M_{c,2005} * (t = y)] + \gamma_c + \delta_t + \epsilon_{ct} \quad (2.5)$$

where σ_y measures the differential change in the outcome for cities that had a relatively higher 2005 migration share (in standard deviations) relative to the excluded year of 2015, the year proceeding the onset of the migration. The 95% confidence interval around the coefficient for each primary outcome is presented in Figure A4. Hourly wages show a decreasing trend from 2011-2013, but this levels off and is flat in the two years before the migration. After 2015 it drops precipitously, in line with the 2SLS results. Hours per week is stable in the pre-period and increases after 2015, also consistent with 2SLS results. However, pre-trends are observed for unemployment rates, which increase steadily during the pre-period and then flatten after the migration, and participation rates, which decreases between 2013-2014 and again after the migration. This motivates an adjustment for pre-trends when using this instrument to study unemployment and participation, which I include as a robustness check.

2.5 Results

The OLS and 2SLS results for the population average are presented in Table 4.3. According to Column 1, a 1pp increase in the migrant share is associated with a .73% decrease in residual hourly wages. After endogenous selection is accounted for in the 2SLS model, this magnitude increases to a 1.05% decrease, with a 95% CI that includes [-1.48,-.62].[19] At the -1.05 point estimate, a movement from the 10th to the 90th percentile of the migrant share across metro areas in 2019 (1% to 8.6%) is associated with a 7.98% decrease in wages. This effect is economically meaningful, considering that average wages for natives in the sample increase by around 4% over the 2014-2019 period. That the 2SLS is more negative than the OLS indicates that there was positive selection of migrants into locations that had bigger increases in wages, though this difference is not significant according to a Hausman test for endogenous regressors.

There is little effect on hours or unemployment, and a small negative effect on participation that is significant at the 10% level. Specifically, a 1pp increase in the migrant share causes a .21pp decrease in labor force participation, with a 95% CI that includes [-.43,.01]. This is associated with a 1.6pp decrease in participation from the 10th to the 90th percentile of the migrant share, relative to a 2014 sample mean of 72.6%. Thus, the majority of the effect on natives was realized on the earnings margin rather than the employment or participation margin.

[19] To place this in the literature, this elasticity is slightly smaller that the values of -1.3 to -2 found by Edo (2020) when looking at the repatriation of Algerians to France, but substantially larger than the -.13 found in Dustmann *et al.* (2017) regarding the cross-border flow of Czech workers into the German workforce.

Table 2.1: Labor Market Effects of Immigration

	(1) OLS	(2) 2SLS	(3) Test (1)=(2) (p-val)
Ln(Hrly Wage)	-0.73*	-1.05***	0.209
	(0.42)	(0.22)	
Ln(Hrs per Week)	0.08	0.27	0.399
	(0.26)	(0.27)	
Unemployment	0.01	-0.08	0.255
	(0.13)	(0.07)	
Labor Force Participation	-0.10	-0.21*	0.355
	(0.12)	(0.11)	
Kleibergen-Paap Wald Stat.		23.35	
N	474	474	
Year FE, City FE	X	X	

Outcomes are residualized and multiplied by 100. Observations weighted by city-year population. Column 3 presents a Hausman test for endogenous regressors with robust standard errors. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

In Table 2.2, I split the outcomes by gender, age, and education.[20] The results show that the effect on wages is slightly stronger for men, though the difference is not significantly significant. Though there are various examples of female participation being more responsive to changes in labor supply (Verme & Schuettler, 2021; Dustmann *et al.* , 2017), effects on participation are only slightly more negative for women and not significantly different. A-priori, one could also expect labor market effects to be more severe for younger natives, since migrants are younger on average. However, given that migrants work in occupations that tend to employ older natives, it is not surprising that negative wage effects are slightly stronger for older natives, though again the differences are insignificant.

[20]This is done by averaging the outcome within metro-year-group cells. The first stage F-statistic changes as the population-based cell weights change. Observations remain clustered at the metro level because treatment is assigned at this unit.

Table 2.2: 2SLS Estimates by Demographic Groups

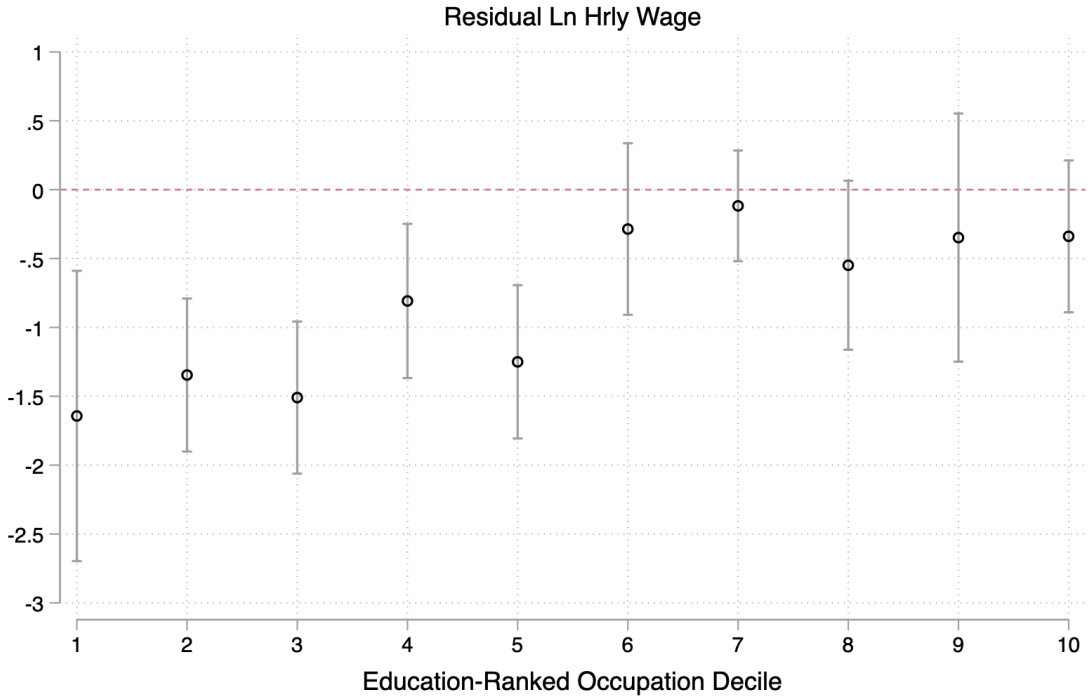
	(1) Ln(Hrly Wage)	(2) Ln(Hrs per week)	(3) Unemployment	(4) LFP	(5) K-P Wald Stat.
All	-1.05***	0.27	-0.08	-0.21*	23.35
	(0.22)	(0.27)	(0.07)	(0.11)	
Male	-1.16***	0.37**	-0.05	-0.18**	24.34
	(0.22)	(0.17)	(0.08)	(0.08)	
Female	-0.91***	0.19	-0.12	-0.22*	22.22
	(0.25)	(0.39)	(0.08)	(0.13)	
Age 15-24	-0.94***	0.14	-0.01	-0.54***	21.34
	(0.20)	(0.46)	(0.12)	(0.18)	
Age 25-34	-0.97***	0.31	-0.13	-0.08	22.11
	(0.22)	(0.21)	(0.10)	(0.07)	
Age 35-44	-1.10***	0.29	-0.06	-0.07	23.39
	(0.23)	(0.20)	(0.07)	(0.06)	
Age 45-54	-1.01***	0.34	-0.10**	-0.11	26.11
	(0.26)	(0.25)	(0.05)	(0.10)	
Age 55-64	-1.21***	0.24	-0.02	-0.10	23.71
	(0.30)	(0.38)	(0.05)	(0.11)	
Less than Secondary	-1.42***	0.50	-0.05	-0.23*	28.72
	(0.23)	(0.34)	(0.06)	(0.12)	
Secondary	-0.86***	0.32	-0.02	-0.12**	25.64
	(0.22)	(0.21)	(0.09)	(0.06)	
Post-Secondary	-0.75*	-0.02	-0.18*	-0.24	17.22
	(0.38)	(0.23)	(0.10)	(0.16)	

Outcomes are residualized and multiplied by 100. All models include Year FE and City FE. Observations weighted by city-year-group population. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

The results also show that the decrease in labor force participation is concentrated among workers under age 25, with a magnitude of -.54pp from a 1pp increase in the migrant share, likely reflecting lower labor force attachment for these workers. In Table A11, I show that this is partially driven by reduced school dropouts. Among workers under age 25, there is a .39pp increase in the share who are not working and are attending school, and a net .2pp increase in school attendance relative to a mean of 54.6. However, these are only significant at the 10% level, and results are not robust to dropping the metro areas located closest to the Venezuelan border, which are in the right tail of the migrant share distribution. I further discuss the importance of this robustness check in Section 2.7.

I expect the wage effect to be larger for less educated workers given the concentration of migrants in low-skill occupations, and the results confirm this. The

Figure 2.3: 2SLS Coefficient Plot



hourly wage effect, respectively for those with less than secondary, secondary and post-secondary education, is -1.42, -.86 and -.75 percent. This pattern of negative wages effects decreasing in native education is also observed when the sample is split by occupation groups, defined by ranking occupations according to mean pre-period native years of schooling and split into deciles. This is shown in Figure 2.3, where the coefficient hovers around -1.5 in the lowest three deciles, around -1 in deciles 4 and 5, and between -.5 and 0 for the top five deciles.[21]

I also split outcomes for workers by the "type" of work, grouped into 4 mutually exclusive categories: formal salaried, informal salaried, own-account and employer

[21]Estimates within occupation groups may be biased if workers switch occupations in respond to the migration. In Section 2.8.2, I show that there are very small effects on movements across these groups.

(representing 29%, 20%, 47% and 4% of native workers respectively). Self-employed workers may experience different effects of competition with migrants considering that they compete directly over prices rather than wages. Self-employed workers who hire employees, on the other hand, may experience increases in profits if the price of labor decreases. I split salaried workers according to formality status because we expect informal workers to face more direct competition with migrants and to have more flexible wages.[22] Figure A5 shows that the wage effects are driven by informal salaried and own-account workers. Formal salaried workers also experience a wage decrease, but the coefficient is 50% smaller and not statistically different from zero. As I show in Section 2.8.2, this may be partly explained by the small exit out of formal work, which could mitigate estimated wage effects for formal workers. The sample of employers is too small to generate precise estimates about the effects of migration on their profits.

2.6 Reconciling Results in the Literature

Various papers (Caruso *et al.* , 2019; Delgado-Prieto, 2021; Bonilla-Mejía *et al.* , 2020a; Santamaria, 2020; Penaloza-Pacheco, 2021) have also studied the effects of Venezuelan migration to Colombia on native employment and earnings. While they are generally consistent in finding null or small employment effects and negative wage effects for natives, the magnitude of the estimated wage effect varies drastically, with elasticities ranging from -.5% to -7.5% from a 1pp increase in the migrant share. This variation could be driven by various specification choices, including the geo-

[22]Over 90% of own-account workers are informal, so I do not split that group into formal and informal.

graphic unit of analysis, the instrument, and the formula and data used to measure the migrant share of the population. In this section, I explore the sensitivity of the magnitude of the wage effect to these factors, to explain the dispersion in the literature and to shed light on the specification choices that may be most consequential when estimating the economic effects of migration.

In Table A3, I start by cross-interacting three dimensions of sensitivity: the chosen instrument, the geographic unit of analysis, and whether or not return migrants (born in Colombia and living in Venezuela before the migration) are included in the migrant share. The first dimension that I vary is the choice of instrument. Table A3 shows that OLS consistently generates smaller wage effects, consistent with migrants positively sorting into high-wage locations. This already helps to explain some variation in the literature: the papers that get smaller coefficients of -.5, Penaloza-Pacheco (2021) and Santamaria (2020), use OLS and thus do not account for migrant positive sorting.[23][24]

The papers that use 2SLS differ in their choice of the "share" component of the shift-share instrumental variable (IV), using either historical migrant shares based on the 1993 or 2005 census or inverse driving distance to the Venezuelan border.[25] The

[23]The goal of this analysis is not to fully replicate the coefficients from each paper. There remain differences in how variables are calculated and analysis is conducted across papers. For example, Santamaria (2020) uses Google search keywords to measure migrant locations across departments. The goal is instead to identify the factors that explain the variation in results.

[24]In an alternate approach, Penaloza-Pacheco (2021) estimate the average effect for border departments La Guajira and Norte de Santander using a group of hand-selected departments as a control group or a synthetic control method. These methods estimate total wage effects of -13% and -9.4% respectively, or -1.03% and -.75% from a 1pp increase in the migrant share. These are more consistent with my own estimates, though they should be interpreted as the average effect specifically for these two department.

[25]Distance to the border can be replaced with a summed distance to each Venezuelan department weighted by the share of Colombian expatriates living in each department (Caruso *et al.* , 2019; Delgado-Prieto, 2021). Here, I simply use driving distance to the border, which is closely corre-

wage effects based on the 1993 and 2005 census IVs are similar in magnitude, driven by the high correlation in the migrant shares across these years. The IV based on distance to the border also produces similar results, though the magnitudes tend to be slightly larger. That these IVs generate similar results is not surprising given that border proximity is correlated with historical migrant shares. However, one may be concerned that border proximity is less likely to satisfy the exclusion restriction than historical migrant shares. These historical shares were small, determined decades prior, and induced little immigration before 2015, while border proximity has potential to be directly impacted by economic changes related to the crisis. In Section 2.7, I treat border proximity as an omitted variable, correlated with historical migrant shares and potentially with trends in the outcome, rather than as a source of exogenous variation. I will show that controlling for a time trend interacted with border proximity somewhat reduces the magnitude of the wage effect.

Second, these papers differ in their unit of analysis, either at the level of the 24 departments or 23 administrative metro areas. The primary advantage of conducting analysis at these levels, relative to the 79 commuting-zone based metro areas that I construct, is to mitigate measurement error concerns since the GEIH is representative at these levels. The disadvantage is that departments are large while most migrants are located in cities, and the official metropolitan areas are somewhat arbitrarily defined according to political considerations (Duranton, 2015). Table A3 shows that the wage effect tends to be smaller at the administrative metro area level and biggest

lated with this measure. Driving distance is calculated using Open Street Maps, from the central municipality of the metro area to the closest Venezuelan border crossing. I continue to use the "leave-out" instrument, which does not affect results.

at the department level, but results are generally similar across these specifications.[26] The geographic level of analysis is therefore not a dimension of variation that drives large differences across papers, though when the department level is combined with the distance IV the coefficient becomes notably larger (-1.27 relative to -1.05 using the 2005 census IV at the commuting-zone metro level, which I refer to going forward as the "baseline specification").[27]

Third, not all of these papers choose to include Colombian return migrants when calculating migration shares. Table A3 shows that the coefficients are inflated when return migrants are excluded. The coefficient increases from -1.05 to -1.24 in the baseline specification, and further to -1.48 using the distance IV at the department level. As discussed, Venezuelan-born and return migrant shares are correlated. Therefore, to exclude return migrants from the migrant share is to assume that they have no effect on local labor markets, and to attribute any changes in labor market outcomes to the Venezuelan-born migrant population. While it is possible that the increased effect size of -1.24 is driven by a larger effect of Venezuelan-born migration on wages, it is equally likely that it is driven by omitted variable bias, which will have a drastic effect on the estimated magnitude according to the framework laid out in Section 2.1. To see this, consider the framework outlined in Section 2.1. A regression of the foreign-born migrant share (predicted by the instrument) on the return migrant share

[26] The instrument is also adjusted to calculate both the historical shares and the national shift at each geographic level. Distance to the border for departments is calculated from the department capital.

[27] The remaining paper that estimates a smaller wage elasticity of around -.5 is Bonilla-Mejía *et al.* (2020a), using the 2005 census IV at the administrative metro area level. This result can be explained by the fact that they use total wages, not hourly wages, as the outcome. While this generates an effect size of -.78 in that baseline specification (as can bee seen in Table 4.3), it falls to -.5 using the administrative metro areas. As we have seen, part of the effect on native total income is mitigated by an increase in hours worked.

with metro and year fixed effects generates a coefficient of .2. Thus, if the effect of Venezuelan-born migration and return migration are both -1.05, one would expect a coefficient of -1.26 (since $-1.05 - 1.05 * .2 = -1.26$), very close to the observed result. It would also be possible for the effect of Venezuelan-born migration to be -1.1 and of return migration to be -.7 (since $-1.1 - .7 * .2 = -1.24$), or for the effect of Venezuelan-born migration to be -.9 and of return migration to be -1.7 (since $-.9 - 1.7 * .2 = -1.24$). Thus, a range of possible effects are consistent with the observed change in magnitude, including scenarios in which Venezuelan-born migration has a larger or smaller effect than return migration.

Delgado-Prieto (2021), using a specification in which the change in outcomes relative to the baseline period is regressed on the 2018 migrant share, finds a significant wage elasticity of -1.7. An important difference in this paper is the use of the 2018 census to calculate migration rates. As discussed in Section 2.3, there are two caveats with the census: it undercounted the Colombian population by 8.5%, and it only measures migration in the first three quarters of 2018. Given that the GEIH is also not designed to be representative of the migrant population, it is not obvious a-priori whether one is more desirable than the other. However, the fact that the GEIH can measure the migration rates in other years, rather than only in 2018, is a large advantage. To test sensitivity to the choice of dataset, I run this specification using both the GEIH and the census to measure the migrant share in 2018.[28] Specifically,

[28]There are a few additional changes in how I use the GEIH in this specification, to allow results to be comparable with those estimated using the 2018 census. First, the migrant share includes migrants from all countries as opposed to only those from Venezuela. This is because the census does not ask for country of origin. However, this does not add very much noise because, according to the GEIH, 95% of foreigners who arrived over this period came from Venezuela. Second, the migrant share is taken as a fraction of the current population rather than the 2014 population, and again this does not have a large effect on results.

the specification is

$$\Delta Y_{c,2014-2018} = \beta M_{c,2018} + \epsilon_c \tag{2.6}$$

First, the results show that, using a 2014-2018 difference model and using the GEIH to measure the migrant share, the results are very similar to the baseline specification. Second, once the 2018 census is used to measure the migrant share, the wage effect magnitude increases by around 20%, and this is true using each instrument and geographic unit of analysis. Thus, if we believe that the census is a more accurate measure of migrant locations, then the true wage effect becomes slightly larger, to -1.26 in the baseline specification. However, this is still not large enough to reconcile the primary results from Delgado-Prieto (2021): using the department unit (as in the paper), the wage coefficient ranges from -1.36 to -1.53 depending on which instrument is used.

The remaining difference in this paper's specification that explains the larger coefficient is the exclusion of return migrants in the migrant share, which as we have seen further inflates the wage coefficient. This is presented in Panel B of Table A4, where using the 2018 census the 2SLS wage coefficient now ranges from -1.46 to -1.77 using commuting zone metro areas. At the department level, it ranges between -1.72 and -2.02. To conclude, the large coefficient found in Delgado-Prieto (2021) is not driven by stopping the analysis in 2018. It can be explained by a combination of using the 2018 census rather than the GEIH, and more importantly, excluding return migrants from the migrant share. If the reader's preferred specification is to measure total migration with the 2018 census using the migrant enclave IV, then the proper coefficient is between -1.09 and -1.13 using the administrative metro areas, between

-1.21 and -1.26 using the commuting zone metro areas, or between -1.32 and -1.36 at the department level.[29]

A final paper is Caruso *et al.* (2019), which finds a negative wage effects of -7.6% from a 1pp increase in the migrant share, almost 5 times larger than the estimate from Delgado-Prieto (2021). One difference in this paper is that their analysis ends in 2017, before the majority of migrants arrived. They also conduct analysis at the department level combined with an inverse-border distance instrument, which we have seen tends to generate a larger wage effect. However, the most consequential difference is that they define the migrant share as the share of the population that arrived from Venezuela over the past 1 year, rather than 5 years. Similar to the case of excluding return migrants, this will generate omitted variable bias considering that these shares are highly positively correlated, and this will inflate the wage effect if previously arrived migrants also have a depressing effect on wages. In this example, this is also akin to using a migration flow rather than a migration stock on the right-hand-side.[30] In the two-way fixed effect framework, the resulting coefficient thus measures the effect of a change in the *flow* of migrants who arrived over the previous year. For example, in Bogotá, the 5-year migrant share was .48% in 2016 and 1.05% in 2017. Likewise, the 1-year migrant share was .24% in 2016 and .56% in 2017, the latter approximately reflecting the change in the 5-year migrant share over

[29] Another result unique to Delgado-Prieto (2021) is the large estimated negative employment effect of -1.7% from a 1pp increase in the migrant share. Using log total employment as the outcome in my primary specification, I find an insignificant negative effect of -.2. However, this increases to -.7 using the distance IV, and increases further to -1.5 when data is restricted to 2018. This is consistent with Delgado-Prieto (2021)'s result that employment effect of -1.7 coinciding with the 2018 migrant surge remains unchanged and in fact diminishes in 2019, despite large numbers of migrants continuing to arrive.

[30] According to the GEIH, only .23% of the population had come from Venezuela in the past 5 years in 2014. Thus, using the 5-year migration measure until 2019 essentially measures the stock of migrants in the country who arrived since 2014.

this period. When the model compares the change in the 1-year share, it attributes changes in the average wage in Bogotá between 2016-2017 to a change in migration of .32pp, rather than the .57pp increase in the migrant stock that actually occurred. In doing so, the researcher implicitly assumes that the economic effects of migration do not persist for more than 1 year.

In Table A5, I run the baseline specification using the migrant share defined in terms of 1-year flows, and I do this both through the full period and using data only through 2017 to test sensitivity to the period of analysis. The 1-year flow estimates are substantially larger across all specifications. In the baseline specification, the wage coefficient increases to -3.53.[31] Using the distance IV at the department level, the coefficient increases further to -4.17. When data are restricted to 2017, the coefficient inflates even further, to -4.80 in the baseline specification and -6.33 using the distance IV at the department level. Thus, the magnitude found in Caruso *et al.* (2019) can be explained primarily by the use of flows rather than stocks, second by the termination of analysis in 2017, and third by conducting analysis at the department level combined with an instrument based on distance to the border.[32]

To conclude, differences in these specification choices are able to explain the variation in the average wage effect reported in the literature. Choice among the candidate instruments and geographic unit of analysis lead to small differences in magnitudes,

[31]Returning again to the framework in Section 2.1, this would be consistent with, for example, 1-year migration and 5-year migration having an effect of -1.4 and -.8 respectively, considering that the regression of the predicted 5-year share on the 1-year share generates a coefficient of 2.5, and $-1.4 - .8 * 2.5 = -3.5$.

[32]Indeed, in Table A4 in the Online Appendix, Caruso *et al.* (2019) run a specification in which they replace the 1-year migration measure with a 5-year migration measure, and the results are comparable with what I find here. However, these are not the primary results reported in the abstract and introduction.

while choices relating to the measure of the migrant share are most consequential: when the migrant share is restricted to only a portion of the migrant population, or to those who arrived within the past year, the wage effect is substantially inflated. When the 2018 census is used to measure migration through 2018, the predicted wage effects also increase slightly. It is worth noting that, while the magnitude of the wage effect varies substantially, most of these papers find that the wage effect is larger for less educated natives.

2.7 Additional Robustness

Armed with a preferred specification that uses an instrument based on historical migrant shares, variation across commuting-zone metropolitan areas, and measuring the *total* migrant share using the GEIH, I now conduct various additional robustness tests for all four labor market outcomes, for the population average and by education group. The most important result from this section is that, after controlling for region-specific time trends, the wage effect falls to -.59.

I start by testing whether proximity to the Venezuelan border is a relevant omitted variable, since it is correlated with both the 2019 and historical migrant shares. In particular, cities very close to the border have experienced changes over this period in economic activity, commuting flows from Venezuela, and violent crime (Knight & Tribin, 2020). The cities with the highest migrant shares, including all of those with a migrant share larger than 15%, are located within 100km driving distance from the Venezuelan border. To ensure that these cities are not driving the results, I drop these six cities in Column 2 of Table 2.3. The instrument loses power (the first

stage Wald statistic falls to 13) and standard errors increase substantially. However, the coefficient values remain stable overall and within education group, implying that wage effects are not driven by these six metro areas. Effects on labor force participation, on the other hand, are eliminated when these cities are excluded.

Proximity to the Venezuelan border may remain an omitted variable if, among cities greater than 100km from the border, those closer to the border experienced decreases in trade with Venezuela over this period. While Venezuela used to be a top trading partner of Colombia, its trade shares steadily declined during the 2000s such that it represented a small share of imports and exports by 2010. However, trade persisted longer for departments closer to the border. In Column 3, I therefore control for total imports and exports with Venezuela at the department level, and the results are highly robust.[33]

To more flexibly account for distance to the border, I control for a linear trend interacted with inverse driving distance to the border. When I do this in Column 4, the estimated average wage elasticity falls to -.53, but the first-stage F-statistic becomes weak and the standard error increases drastically, such that the coefficient is not statistically different from the original specification. I conclude that it is not feasible to isolate variation in the predicted migrant share from this distance measure. Thus, it remains possible that 2SLS results are partially explained by trends associated with border proximity.[34]

[33]Trade data was downloaded from DANE, and measures the net weight of all Venezuelan imports and exports with origin or destination in each department.

[34]In all robustness checks with a linear trend, I can instead use interactions with year fixed effects and results are qualitatively similar, though the standard errors are less precise. I can also use linear distance instead of driving distance and results are similar. These are available upon request.

Table 2.3: 2SLS Estimates Robustness

	(1) Original 2SLS	(2) Drop <100km from Border	(3) Control Trade with Venezuela	(4) Year Trend X Inv. Distance to Border	(5) Year Trend X Region	(6) Year Trend X Pre-trend
			Ln(Hourly Wage)			
All	-1.05***	-1.02	-1.07***	-0.53	-0.59**	-1.06***
	(0.22)	(0.68)	(0.21)	(0.67)	(0.28)	(0.22)
Less than Secondary	-1.42***	-1.43**	-1.45***	-0.45	-0.57	-1.42***
	(0.23)	(0.64)	(0.22)	(0.57)	(0.40)	(0.23)
Secondary	-0.86***	-0.93	-0.86***	-0.54	-0.32	-0.87***
	(0.22)	(0.68)	(0.22)	(0.58)	(0.23)	(0.22)
Post-Secondary	-0.75*	-0.65	-0.76**	-0.33	-0.71**	-0.82**
	(0.38)	(0.99)	(0.35)	(1.18)	(0.30)	(0.36)
			Ln(Hours per Week)			
All	0.27	-0.50*	0.27	-0.68***	0.47*	0.28
	(0.27)	(0.28)	(0.28)	(0.24)	(0.24)	(0.26)
Less than Secondary	0.50	-0.58	0.51	-0.74**	0.67**	0.52
	(0.34)	(0.37)	(0.35)	(0.31)	(0.30)	(0.34)
Secondary	0.32	-0.29	0.32	-0.37	0.50**	0.32*
	(0.21)	(0.28)	(0.21)	(0.25)	(0.23)	(0.17)
Post-Secondary	-0.02	-0.52*	-0.04	-0.80***	0.16	-0.02
	(0.23)	(0.28)	(0.25)	(0.22)	(0.22)	(0.22)
			Unemployment			
All	-0.08	-0.15	-0.09	-0.38**	-0.25*	-0.12
	(0.07)	(0.20)	(0.07)	(0.16)	(0.13)	(0.08)
Less than Secondary	-0.05	-0.13	-0.06	-0.34**	-0.17*	-0.07
	(0.06)	(0.17)	(0.06)	(0.13)	(0.10)	(0.07)
Secondary	-0.02	-0.21	-0.02	-0.41***	-0.21	-0.04
	(0.09)	(0.19)	(0.08)	(0.16)	(0.14)	(0.08)
Post-Secondary	-0.18*	-0.21	-0.18**	-0.47**	-0.37**	-0.19*
	(0.10)	(0.24)	(0.09)	(0.24)	(0.18)	(0.10)
			LFP			
All	-0.21*	0.07	-0.20*	0.05	-0.20**	-0.13
	(0.11)	(0.18)	(0.11)	(0.23)	(0.09)	(0.09)
Less than Secondary	-0.23*	0.09	-0.23*	0.04	-0.20*	-0.17
	(0.12)	(0.23)	(0.12)	(0.31)	(0.11)	(0.11)
Secondary	-0.12**	-0.02	-0.09	0.00	-0.14***	-0.11
	(0.06)	(0.15)	(0.07)	(0.17)	(0.05)	(0.07)
Post-Secondary	-0.24	0.13	-0.25	0.17	-0.26**	-0.21
	(0.16)	(0.16)	(0.16)	(0.19)	(0.13)	(0.15)
Kleibergen-Paap Wald Stat.	23.35	13.07	28.14	13.95	88.40	23.61
Number of metro areas	79	73	79	79	79	79

Outcomes residualized and multiplied by 100. All models include Year FE and City FE. See text for description of robustness checks. Observations weighted by city-year-group population. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Considering this, another method to control for unobserved heterogeneity across broad geographic areas is to control for linear trends interacted with fixed effects for the four regions of Colombia defined by DANE: Pacific, Caribbean, Central, and Eastern, the last of which incorporates Bogotá. Accounting for regional time trends

was also found to be important in the case of Syrian migration to Turkey (Aksu *et al.* , 2018). I interact region dummies with a time trend rather than year fixed effects to preserve power in the first stage. The results in Column 5 show that the wage effect is mitigated to -.59 and remains statistically different from 0, though it is again not statistically different from the baseline estimate. This indicates that part of the 2SLS wage effect is driven by regional time trends, and after controlling for them the true wage effect decreases. None of the papers discussed in Section 2.6 complete this robustness check. If I estimate the model using the 2018 census to measure migration as in column 2 row 2 of Table A4, adding in region fixed effects brings the coefficient down from -1.26 to -.70.[35]

Next, motivated by the presence of pre-trends in unemployment and participation, in Column 6 I interact a linear year trend with the change in the outcome between 2013-2015, allowing linear changes over the migration period to vary flexibly with pre-period trends. As expected, this has little effect on the wage and hours estimates. Despite potential pre-trends for unemployment, the unemployment coefficients remain stable. The negative coefficient on participation decreases in magnitude and becomes insignificant for each education group, implying that the small decreases in participation are partially driven by trends that began before the Venezuelan exodus. This is consistent with evidence from Delgado-Prieto (2021) that native employment effects are mitigated after accounting for pre-trends.

Finally, in Table A6, I conduct various checks that have no effect on the estimated

[35]It is also notable that the wage effect for less-educated natives falls the most when region trends are included, but the standard errors also increase the most, such that the effect for less than secondary group is not significantly different from the baseline estimate and includes a wide range of plausible estimates. Thus, we are unable to isolate the effect from region trends for this subgroup.

coefficients. First, I drop Bogotá, which is the largest city in the sample, to ensure that it is not disproportionately driving results.[36] Next, I drop all metro areas with an annual sample size of less than 1,000 observations, resulting in 27 major cities closely overlapping with the 23 official areas for which the GEIH is representative. This is to ensure that measurement error within small areas is not driving results. Third, I show that a first-difference model, in which the change in the outcome between 2014-2019 is regressed on the change in the migrant share, instrumented with the change in the instrument, produces results very similar to the two-way fixed effects framework. This ensures that the bias of using two-way fixed effects in a context with potentially dynamic treatment effects is small, which was expected in this case considering that most migrants did not arrive until close to the end of the study period. Fourth, there is a concern that metro population, which is correlated over time, is in the denominator of both the endogenous variable and the instrument, and this may induce a spurious correlation that drives the first stage (Clemens & Hunt, 2019; Kronmal, 1993).[37] To test this, I replace the Venezuelan *share* of the population, M_{ct}, with a variable that is the total number of Venezuelans. I then flexibly control for baseline population by interacting it with year fixed effects. The results, in Column 10, show that the first stage remains strong, and that an increase of 100 migrants significantly reduces wages by .66%. At the mean population of 18,756, a 1% increase in the migrant share is therefore associated with a -1.23% wage effect, closely matching the initial estimates. Similar results are seen for other outcomes

[36] I can also drop all metro areas one-by-one, and this is available upon request.

[37] For example, as discussed in Clemens & Hunt (2019) citing Kronmal (1993), "One would find storks-per-woman to be a strong instrument for babies-per-woman even if storks are irrelevant to babies, and that framework could show spuriously that babies cause any regional outcome that is correlated with the number of women in the region."

and subgroups. The final robustness check, regarding native internal migration, is discussed in Section 2.8.3.

2.8 Additional Results

2.8.1 Non-Linear Effects

The analysis thus far assumes a log-linear relationship between wages and migration. To allow for non-linearity in the migrant share, in Table A7 I run a quadratic model in which I include the squared migrant share as an endogenous variable and the squared instrument as an additional instrument. Overall and within education groups, the curvature is positive. but it is insignificant and extremely small in magnitude. With every 1pp increase in the migrant share, the wage effect diminishes by .02 off a base effect of -1.4.

To visualize the wage effect across the distribution of the migrant share, I residualize the log-wage and migrant share predicted by the instrument from metro and year fixed effects, and I use them to estimate a LOWESS model (locally weighted scatterplot smoothing) in Figure A6. Focusing on the middle of the migrant share distribution, which contains the majority of observations, one sees that the slope is relatively constant, and if anything becomes more negative at higher migrant shares. In other words, the effect of migration on wages becomes slightly larger as the migrant share increases. However, at very high and low migrant shares, the wage effect becomes flat and potentially even positive, though these tails are being determined by a small number of observations. It is clear that outliers are not responsible for

driving the negative wage effect. To summarize, a linear model seems broadly appropriate, though it may be masking some negative curvature for small increases in migration, and without sufficient variation in the data to identify non-linear effects among very large or small migrant shares.

2.8.2 Occupation Skill Group and Informality

It is also possible to have natives respond to migration by changing their occupation or type of work. In particular, given that migrants are concentrated in low-skill occupations, natives may have benefited by upgrading to higher-skill occupations where their labor is more complementary (Foged & Peri, 2016). While work transitions cannot be observed directly in the GEIH because it is cross-sectional, in Table 2.4 I look at changes in total employment across occupation skill groups, again defined by ranking the mean native education in occupations before 2015, this time split into quintiles rather than deciles. These results are not conditional on working, so they reflect a combination of occupational movements and changes in employment (which is why rows do not sum to 1). This is to avoid coefficient changes in one group being driven by workers exiting from a different group.

The results show small movements into middle-skill occupations on average, coming from both the lowest and the highest skill groups. The analysis by demographic group reveals that the upgrading from low- to middle-skill occupations is concentrated among men with completed secondary education (but not post-secondary). Likewise, the downgrading from high- to middle-skill occupations occurred among men with post-secondary, especially between ages 35-44. However, these effects are small in magnitude: a 1pp increase in the migrant share causes around a .15pp shift

Table 2.4: 2SLS Effects on Employment by Occupation Skill Group

	(1) Occup Grp 1	(2) Occup Grp 2	(3) Occup Grp 3	(4) Occup Grp 4	(5) Occup Grp 5	(6) Under-Employed	(7) K-P Stat.
All	-0.06***	-0.03	0.07	-0.01	-0.09***	0.29**	25.69
	(0.02)	(0.08)	(0.04)	(0.02)	(0.02)	(0.13)	
Female	0.02	-0.05	0.02	-0.03	-0.07***	0.24*	25.35
	(0.03)	(0.05)	(0.06)	(0.02)	(0.02)	(0.12)	
Male	-0.15***	-0.00	0.12***	0.02	-0.11***	0.35**	26.10
	(0.03)	(0.10)	(0.02)	(0.03)	(0.02)	(0.14)	
Age 15-24	-0.16***	-0.19**	-0.01	-0.04	-0.01	0.15	24.77
	(0.03)	(0.07)	(0.07)	(0.05)	(0.02)	(0.11)	
Age 25-34	-0.11***	0.01	0.11**	0.15***	-0.11**	0.49***	23.15
	(0.03)	(0.13)	(0.05)	(0.04)	(0.05)	(0.16)	
Age 35-44	0.00	0.08	0.08**	-0.03	-0.18***	0.36**	25.02
	(0.03)	(0.09)	(0.04)	(0.04)	(0.04)	(0.14)	
Age 45-54	-0.04	0.07	0.12***	-0.08*	-0.04	0.35**	29.18
	(0.05)	(0.05)	(0.04)	(0.04)	(0.03)	(0.15)	
Age 55-64	0.09**	-0.12	0.02	-0.05	-0.06***	0.09	29.02
	(0.04)	(0.09)	(0.08)	(0.03)	(0.02)	(0.08)	
Less than Secondary	-0.09***	-0.04	-0.01	-0.02*	-0.01**	0.18	31.28
	(0.03)	(0.07)	(0.06)	(0.01)	(0.00)	(0.11)	
Secondary	-0.14***	-0.04	0.15***	-0.00	0.00	0.28*	26.91
	(0.03)	(0.06)	(0.05)	(0.05)	(0.01)	(0.15)	
Post-Secondary	-0.02	-0.02	0.12**	0.04	-0.20***	0.49***	19.22
	(0.02)	(0.09)	(0.05)	(0.04)	(0.07)	(0.17)	

All models are 2SLS linear probability models for probability of employment in a group (multiplied by 100), not conditional on working, with Year FE and City FE. Occupations ranked according to mean education of natives pre-2015 and grouped into quintiles. Workers are under-employed if they say they would like to change jobs to improve use of skills or trianing. Observations weighted by city-year-group population. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

out of low-skill occupations for men and people with completed secondary. Women do not experience any occupational shift but are more likely to exit employment out of high-skill occupations. The analysis also demonstrates that the workforce exit among workers under age 25 is mostly out of low-skill occupations.

Another measure indicative of occupational downgrading or upgrading is self-reported under-employment. The GEIH includes a question that asks workers if they would like to change their job in order to improve their use of skills or training. In Column 6, I show that a 1pp shift in the migrant share caused an increase in under-

Table 2.5: 2SLS Effects on Employment by Type of Work

	(1) Formal Salaried	(2) Informal Salaried	(3) Own-Account	(4) Employer	(5) K-P Stat.
All	-0.09***	-0.02	-0.04	0.01	25.69
	(0.02)	(0.07)	(0.09)	(0.02)	
Female	-0.09***	-0.00	-0.03	0.01	25.35
	(0.03)	(0.06)	(0.10)	(0.01)	
Male	-0.08**	-0.03	-0.05	0.02	26.10
	(0.03)	(0.10)	(0.09)	(0.03)	
Age 15-24	-0.10***	-0.02	-0.31***	-0.01	24.77
	(0.03)	(0.08)	(0.11)	(0.00)	
Age 25-34	-0.02	-0.01	0.07	-0.00	23.15
	(0.05)	(0.08)	(0.11)	(0.02)	
Age 35-44	-0.09**	-0.04	0.10	0.04	25.02
	(0.04)	(0.08)	(0.09)	(0.03)	
Age 45-54	-0.10**	-0.00	0.02	0.03	29.18
	(0.04)	(0.06)	(0.08)	(0.03)	
Age 55-64	-0.10**	-0.03	-0.01	0.05**	29.02
	(0.05)	(0.05)	(0.11)	(0.02)	
Less than Secondary	-0.10***	-0.05	-0.06	0.02	31.28
	(0.02)	(0.08)	(0.10)	(0.03)	
Secondary	-0.05	-0.07	0.06	-0.00	26.91
	(0.04)	(0.07)	(0.11)	(0.02)	
Post-Secondary	-0.08*	0.01	-0.04	0.03	19.22
	(0.05)	(0.06)	(0.10)	(0.02)	

Models are 2SLS linear probability models for probability of employment in a group (multiplied by 100), not conditional on working, with Year FE and City FE. Wage-sector workers are split by formality, defined according to compliance with mandatory health and pension schemes. Self employed workers are split into own-account (with no employees, predominantly informal) and employer. Observations weighted by city-year-group population. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

employment of .29pp on average. This effect is strongest for the demographic groups that the previous analysis indicates experienced occupational downgrading: men and workers with post-secondary. Interestingly, these magnitudes are larger than those for the occupation groups. This indicates that Venezuelan migration induced a change in self-reported under-employment among workers who did not change occupation skill group, which could reflect changing task requirements within an occupation group or decreased satisfaction with work caused by migration.

Finally, I study whether natives changed their formality status or shifted into own-account employment. In Table 2.5, I divide jobs into the same 4 mutually exclusive categories already discussed (formal salaried, informal salaried, own-account and employer). There is evidence that natives left the formal market-wage sector and went into non-employment. As discussed in Delgado-Prieto (2021), this may have resulted from firms substituting formal native labor for informal labor. This effect is persistent across all demographic groups. Thus, there is no evidence for native upgrading to the formal sector. There are also no significant shifts of natives into informal wage work, own-account work or becoming an employer. The workforce exit unique to people under age 25 that was primarily out of lower-skill occupations is driven by a reduction in own-account work.

In Table A8, I conduct each of the robustness checks from the previous section for this analysis. This shows that, while the downgrading is robust to each check, the upgrading goes away once I include the linear trend interacted with inverse distance to the border or region fixed effects. The increase in self-reported underemployment and decrease in formal salaried work are robust to each check.

2.8.3 Internal Migration

An extensive literature documents that natives often respond to immigration by migrating away from impacted areas, perhaps because of changing labor market conditions or direct disutility from migrant exposure (Borjas, 2003; Borjas & Katz, 2007; Monras, 2020). This can occur through both increases in out-migration or reductions in in-migration, and it might bias geography-based estimates of labor market impacts in two ways. First, it creates a compositional change in the inhabitants of a metro

area. Second, it could remove from each city individuals who experienced differential labor market effects of the migration; for example, if those who out-migrated would have experienced large counterfactual wage decreases had they stayed, this would bias upward the estimated wage effect.

A major strength of the GEIH is the ability to see where each individual was living 5 years ago, including their previous municipality of residence in Colombia. Using this information, I can calculate the number of migrants who left or entered a metro area over a 5-year period. I then take this as a share of the 5-year lagged population to get the out- and in-migration rate for each metro area. Using the same 2SLS framework, I can study the causal effect of Venezuelan arrivals on this in- and out-migration. I restrict analysis to 5-year migration flows because they are less likely to be driven by measurement error or short-term temporary movements than 1-year flows. I only include data from 2014 and 2019 (thus identical to the difference model in Table A6) and thus measure the change in the out- and in-migration rate from 2014 to 2019 induced by the arrival of migrants over this period.

Table 2.6 shows that, on average, Venezuelan arrivals cause a small increase in out-migration and decrease in in-migration, but both effects are insignificant. Out-migration becomes significant and larger in magnitude among people over age 45 and with secondary education or less. Among those with completed secondary, a 1pp increase in Venezuelan arrivals causes a .1pp increase in out-migration over 2015-2019 relative to 2010-2014, or 1.5% of the base migration rate for this group. Moving from the 10th-90th percentile of metro areas, this is associated with an increase in out-migration of .76pp. The magnitudes for changes in in-migration are slightly larger, and also concentrated among people with completed secondary, but they are

Table 2.6: Effects on Native Internal Migration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Out-Migration			In-Migration			
	OLS	2SLS	Sample Mean	OLS	2SLS	Sample Mean	K-P Stat.
All	0.06	0.03	6.58	-0.04	-0.08	10.17	17.78
	(0.06)	(0.03)		(0.25)	(0.11)		
Male	0.08	0.06	6.76	0.01	-0.04	10.16	18.13
	(0.06)	(0.04)		(0.26)	(0.12)		
Female	0.05	0.01	6.42	-0.07	-0.11	10.17	17.48
	(0.06)	(0.03)		(0.23)	(0.11)		
Age 15-24	0.07	0.05	7.67	-0.02	-0.06	14.88	16.69
	(0.08)	(0.06)		(0.32)	(0.16)		
Age 25-34	0.05	-0.03	9.49	-0.09	-0.13	13.02	16.74
	(0.08)	(0.05)		(0.32)	(0.15)		
Age 35-44	0.05	0.03	6.64	-0.06	-0.10	9.03	17.02
	(0.09)	(0.05)		(0.25)	(0.11)		
Age 45-54	0.07	0.07**	3.97	-0.04	-0.09	5.62	20.18
	(0.05)	(0.03)		(0.14)	(0.06)		
Age 55-64	0.04	0.05*	3.09	-0.04	-0.09	4.24	20.09
	(0.03)	(0.03)		(0.13)	(0.07)		
Less than Secondary	0.10*	0.07**	5.54	-0.01	-0.04	8.87	21.90
	(0.06)	(0.04)		(0.25)	(0.12)		
Secondary	0.08	0.10***	6.62	-0.08	-0.17	10.04	17.97
	(0.08)	(0.03)		(0.23)	(0.11)		
Post-Secondary	0.00	-0.07	7.70	-0.02	-0.01	11.71	13.30
	(0.08)	(0.05)		(0.28)	(0.14)		

Sample includes 2014 and 2019. In- and out-migration rates taken as a share of 5-year lag population and multiplied by 100. All models include Year FE and City FE. Obsevations weighted by city-year-group population. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

not significantly different from zero using a 10% test size. These OLS and 2SLS results are also similar, implying little selection of Venezuelans into areas that had differential trends in internal migration. Finally, Table A9 shows that these results also become very imprecise and insignificant after dropping metro areas close to the border. The effect on out-migration is actually reversed, and become significantly negative, after including region-specific time trends.

To ensure that this does not bias my primary results, in Column 6 of Table A6 I assign all individuals to their metro area of residence five years before the survey year, and the results are stable. This robustness check holds constant the composition of the sample, thus eliminating any potential for compositional change

to drive results. Furthermore, if one makes the assumption that out-migrants earn in their new location as much or more than they would have had they stayed (or in the case of diverted in-migrants, had they come to the city), then this also generates a lower bound on the magnitude of the true wage effect. Thus, under the assumption of weakly improving wages for internal migrants, these estimates provide an upper-bound on the true negative wage effect. Given the small magnitude of the internal migration response, this bias is likely to be small.

2.8.4 Regional Heterogeneity

Thus far, results have been reported for the national average. Yet the economic effects of migration may vary according to regional economic characteristics. Cities with high unemployment may have less capacity to absorb incoming migrants. The 2014 unemployment rate varies considerably across metro areas in Colombia, from 4% at the 5th percentile to 15% at the 95th percentile. The rate of informal and own-account work may also be relevant considering that migrants compete more directly with natives in these sectors. Business climate considerations are also important, both for documented migrants who would like to register a business and for natives who may respond to the increase in labor supply by forming or expanding a business. Consistent with these hypotheses, Aracı *et al.* (2021) find that the labor market consequences of Syrian refugees in Turkey are smaller in more developed regions of Turkey.

In Table 2.7, I interact both the endogenous migrant share and the instrument with various pre-period metro-level economic characteristics measured in the GEIH, GDP per-capita at the department level measured by DANE, as well as business cli-

Table 2.7: 2SLS Wage Effects Interacted with Regional Characteristics

	Ln(Hrly Wage)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Migrant Share	-0.97***	-1.14**	-1.02***	-0.55	-1.28***	-0.94***	-1.23***	-1.01***	-1.01***
	(0.26)	(0.55)	(0.23)	(0.39)	(0.23)	(0.30)	(0.20)	(0.37)	(0.30)
Migrant Share X 2014 Mean Ln(Hrly Wage)	-0.36								
	(0.46)								
Migrant Share X 2014 Unemployment Rate		0.07							
		(0.29)							
Migrant Share X 2014 Informal Rate			-0.62*						
			(0.34)						
Migrant Share X 2014 Own-Account Rate				-0.75**					
				(0.35)					
Migrant Share X 2017 WB DB (Starting a Business)					0.29*				
					(0.16)				
Migrant Share X 2017 WB DB (Construction Permits)						-0.31			
						(0.50)			
Migrant Share X 2017 WB DB (Registering Property)							-0.36		
							(0.43)		
Migrant Share X 2017 WB DB (Paying Taxes)								-0.06	
								(0.23)	
Migrant Share X 2014 Per-Capita GDP									-0.13
									(0.32)
Kleibergen-Paap Wald Stat.	12.38	9.51	14.11	13.27	36.46	11.55	8.73	9.03	13.31
N	474	474	474	474	474	474	474	474	474

Both the endogenous variable and the instrument are interacted with the interaction variable indicated in each row. All interaction variables are centered around 0 and divided by the standard error. 2014 metro-level economic indicates measured using the GEIH. Doing Business measures at the department level were taken from the World Bank 2017 Doing Business report. 2014 per-capita GDP at the department level downloaded from DANE. All models include Year FE and City FE. Observations weighted by city-year population. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

mate indicators at the department level taken from the 2017 World Bank Doing Business report.[38] Importantly, these results may not reflect the causal effect of regional heterogeneity since metro areas may differ according to unobservable characteristics. Furthermore, in some cases the first-stage F-statistic falls below 16. Nonetheless, the results are indicative of the importance of regional heterogeneity for the economic effects of migration.

There is no significant heterogeneity in the wage effect according to baseline hourly wages, unemployment, or per-capita GDP. However, the effect does become significantly more negative as the baseline informality and own-account rates increase, which may be driven by the fact that migrants compete more directly with natives

[38]This report evaluates the regulatory environment across Colombia's department capitals for four areas: ease of starting a business, obtaining construction permits, registering property, and paying taxes. See World Bank (2017). I assume non-capital metro areas face the same regulatory environment as the capital. Results are similar when analysis is restricted to administrative metro capitals (available upon request).

in these sectors. Also as hypothesized, the wage effect becomes less negative as the ease of starting a business increases, though this is only significant at the 10% level. For departments 1 standard deviation above and below the mean score in ease of starting a business, the wage effect is -.99 and -1.57 respectively. None of the other Doing Business indicators are significant.

Table A10 shows that the result of wage effects increasing in informality and decreasing in ease of doing business are robust to the same list of robustness checks previously considered, though in some cases the standard error increases and the first-stage F-statistic becomes weak. The informality interaction coefficient becomes even larger and more significant with the inclusion of region-specific time trends, while the ease of doing business interaction coefficient remains stable. Overall, the results are suggestive that the ease of doing business and the size of the informal sector are relevant contributors to the wage effects of migration. From a policy perspective, this may indicate that, not only is it desirable to reduce formality and facilitate business formation, but also to encourage or subsidize migrant relocation to areas that are better prepared according to these characteristics (as discussed regarding the Colombian setting, for example, in Bahar *et al.* (2018)).

2.9 Conclusion

The migration from Venezuela to Colombia presents a unique opportunity to better understand the short-term effects of mass migration on native labor market outcomes in a developing country and in a context where natives and migrants share a similar culture and language. The results show little effects on the employment margin:

there was a small drop in participation and increase in school attendance among people under age 25, but this was driven entirely by cities close to the Venezuelan border and mitigated after adjusting for pre-trends. However, there were negative and robust effects on native hourly wages most pronounced for informal and less-educated workers, consistent with low reservation wages and high wage flexibility. These results are not biased by the small increase in native internal migration that occurred in response to the Venezuelan arrival, or the small shifts in occupational skill group that benefited some workers and harmed others. They are consistent with a pattern in the literature in which the economic consequences of migration are most pronounced in developing countries, especially for less-educated and informal workers, and clearly motivate policy responses to mitigate the economic consequences of migration. They also motivate additional research to better understand the drivers of these economic consequences in developing countries. That these wage effects are moderately stronger in metro areas with higher baseline informality rates and lower ease of starting a business indicates that local economic conditions are a determinant of the labor market effects of migration, and motivate policies to facilitate business formation or encourage migrant relocation according to local economic conditions. The role of migrants' occupational downgrading is explored extensively in Chapter 2, and the results indicate that migrant downgrading plays an important role in concentrating wage effects among lower-income natives.

The robustness and sensitivity analysis identified two important caveats for the estimated average native wage effect of -1.05% from a 1pp increase in the migrant share. First, when the 2018 census rather than the GEIH is used to measure the migrant share, the magnitude of the average wage effect increases to -1.26%. Second,

after controlling for regional time trends, the wage effect decreases to -.59% (or -.70% using the 2018 census), with a 95% confidence interval ranging from [-1.14,-.04].

Finally, I demonstrated the specification choices that explain the variation in estimated wage effects in the literature studying Venezuelan migration to Colombia. The papers that find small or insignificant wage effects do not use an instrument and thus fail to account for the positive sorting of migrants into locations. Larger wage effect magnitudes of -1.7% and -7% found in the literature are only partially explained by differences in the instrument, geographic unit, time period of analysis, or data used to measure migration. The most important determinant is that, when a subset of the migrant population is excluded from the migrant share, or when the migrant share is defined to only include recently arrived migrants, the wage effect inflates substantially. This is likely driven by omitted variable bias and presents an important lesson for migration researchers.

Chapter 2 - Immigration and Occupational Downgrading

3.1 Introduction

Around the world, migrants are typically over-educated in their occupation relative to natives, which is a common definition of occupational downgrading (Piracha *et al.* , 2012; Chiswick & Miller, 2009). This is particularly true for large and sudden migration waves driven by push-factors in the home country, in part because such migrants are less likely to arrive with a pre-organized job or plan for employment (Brell *et al.* , 2020; Fasani *et al.* , 2018). In addition to reducing earnings and economic assimilation for migrants themselves, this downgrading may have important economic consequences for natives in the host country. First, migrant occupational downgrading could increase inequality by concentrating economic competition among poorer natives. Second, it could hinder increases in total economic output - a potential benefit of migration - if migrants are concentrated in a narrow set of occupations and mismatched according to skill and experience. Both of these outcomes have the potential to encourage social and political backlash against migrants and migration policy. This is particularly worrying given the increasing global number of forcibly displaced persons, which doubled from 41 million to 82 million between 2010-2020 (UNHCR, 2020). If this trend continues, the economic and political consequences of downgrading may become increasingly relevant in the coming decades.

The extent to which downgrading increases wage inequality depends on the degree of imperfect substitutability between migrants and natives (Ottaviano & Peri, 2012;

Manacorda *et al.* , 2012; Card, 2009). Migrants and natives generally work different jobs, complete different tasks, and have different skills, and the extent to which they are substitutes or complements is an empirical question that will vary across contexts and occupations. If migrants and natives are more substitutable in low-skill occupations, perhaps because they are more routine or less specialized, then downgrading will concentrate migrants into occupations where they put greater downward pressure on native wages.[1] Furthermore, if substitutability across skill groups is low, for example, because there is little capacity for natives to upgrade to higher-skill occupations, then this amplifies the consequences of migrant downgrading for less educated natives. In this paper, I estimate a model of labor demand that captures these essential elements by incorporating migrant occupational downgrading and imperfect substitutability between migrants and natives and across skill groups.

I do this in the context of Venezuelan migration to Colombia. Between 2015-2019, approximately 1.8 million migrants fled Venezuela's economic and political crisis into neighboring Colombia, increasing the Colombian population by almost 4%. Despite being similarly educated as Colombian natives on average, these migrants are highly concentrated in occupations that typically employ less-educated workers: over 25% of working Venezuelan migrants in Colombia in 2019 had post-secondary education, and of them, around 75% were in occupations where the modal native did not go to college, relative to around 30% for college-educated natives. The four most common occupations for migrants are street vending, restaurant work, construction, and domestic service. This situation has stimulated debate over the consequences of

[1]Throughout this paper, I define the occupation skill distribution according to the average completed schooling of natives in each occupation, even though, strictly speaking, jobs can require little education but important skills.

migrant downgrading for the Colombian economy.[2]

There are several reasons why this setting is interesting. First, the Venezuelan out-migration was driven by a drastic increase in poverty, violence, and inadequate quality of life, stimulated by the sudden collapse of oil prices in 2014, and unrelated to changes happening in Colombia over this period (Migration Policy Institute, 2020). Thus, the endogeneity of migration timing is less of a concern relative to a setting in which migrants are attracted by economic pull-factors and time their migration over many years. There is also broad geographic dispersion of migrants across Colombia, alongside high-quality household survey data on labor market outcomes for both migrants and natives, resulting in a sample of over 1.5 million natives and 30,000 migrants over the study period. Together, these factors create a unique opportunity to study the causal effect of migration on the Colombian economy.

Second, the developing country setting has received less attention in the literature studying the economic effects of migration and forced displacement, despite the fact that 86% of people displaced across borders are hosted in a developing country (UNHCR, 2020). Developing countries tend to have large informal sectors - in Colombia, around 60% of native and over 90% of Venezuelan migrants remain informal, defined according to enrollment in mandatory health and pension schemes. These jobs are characterized by a lack of minimum wages, ease of entry, low hiring costs, and high turnover rates, implying that substitutability between migrants and natives in these disproportionately low-skill jobs may be particularly large (Guriev

[2]For example, the think-tank Global Americans stated "the Colombian economy can benefit from the vast talent pool of Venezuelan professionals who have migrated to Colombia and currently rely on informal work when they could be more productive in other sectors" (Guzmán & Marmolejo, 2021). A Brookings report stated "If properly integrated into the labor force, [Venezuelan migrants] represent a largely productive cohort which could contribute to economic growth and productivity gains" (Bahar *et al.* , 2018).

et al. , 2019; Agudelo & Sala, 2017). The substitutability between education groups also tends to be lower in developing countries - in particular, the literature estimates much smaller substitutability between those with and without a high school degree in Latin America than in the U.S. or the U.K. (Manacorda *et al.* , 2010; Fernández & Messina, 2018; Acosta *et al.* , 2019). Both of these characteristics may amplify the economic consequences of migrant downgrading for low-skill natives in the developing country context.

Third, an important characteristic of this migration wave is that Venezuelans and Colombians share a similar culture and language, thus mitigating any effects of cultural or linguistic barriers and highlighting that migrant downgrading can still occur in the absence of linguistic barriers. This may increase migrant-native substitutability (Braun & Mahmoud, 2014). Furthermore, migration can sometimes benefit natives by inducing upgrading into communication-intensive occupations, and this is mitigated if there are no linguistic barriers, thus decreasing substitutability across skill groups (Peri & Sparber, 2009; Peri *et al.* , 2020).

Fourth, as I document in Chapter 1, there is existing evidence that this migration wave decreased hourly wages for Colombian natives and especially for less educated natives. I also find little evidence for changes in employment, implying that most of the economic effect is realized along the hourly wage margin. The results from Chapter 1 make no assumptions about migrant-native substitutability and are informative about the total effects of migration on native economic outcomes (Dustmann *et al.* , 2016). Migrant downgrading has indeed been proposed as a mechanism behind this unequal wage effect, in Colombia and in other countries in Latin America (Olivieri *et al.* , 2021b; Lombardo *et al.* , 2021). The primary advantage of the more structural

approach taken in this chapter is to look under the hood at what drives the relationship between downgrading and native labor market outcomes, to disentangle the role of migrant downgrading, and to understand in what settings migrant downgrading is likely to be most consequential.

My research design exploits geographic variation in the arrival of migrants across 79 metropolitan areas constructed according to commuting patterns (Duranton, 2015). I address two fundamental challenges highlighted in the migration economics literature. First, to account for endogenous sorting of migrants into destinations within Colombia, I use an instrumental variable strategy based on the historical settlement patterns of Venezuelans (Altonji & Card, 1991; Card, 2001, 2009). The strengths of the instrument in this context mirror the discussion in Chapter 1: The historical location of Venezuelans was determined two decades before the onset of the Venezuelan exodus, well before the election of Hugo Chávez, and was for the most part uncorrelated with levels or trends in economic outcomes leading up to the current migration (Goldsmith-Pinkham *et al.* , 2020). Furthermore, the historical number of Venezuelans was miniscule relative to the recent inflow, and there was very little immigration from Venezuela or any other country before 2014, mitigating the concern highlighted in Jaeger *et al.* (2018) regarding serial correlation in economic outcomes. At the same time, these historical flows strongly predict the current location of Venezuelans, making this instrument uniquely appropriate in this setting. Second, I confirm that the spatial arbitrage of natives does not bias my estimation by endogenously changing the composition of native workers (Borjas, 2003; Borjas & Katz, 2007; Monras, 2020). In Chapter 1, I showed that the Venezuelan migration caused a very small increase in native out-migration. Therefore, as a robustness check, I exploit the migration

histories in the labor market data by assigning natives to their pre-migration metro residence, and the results are not impacted.

My primary methodological contribution is to incorporate migrant occupational downgrading into CES demand models of the labor market. These models have long been used to study the relationship between wage inequality and the relative supply of skilled and unskilled workers (Katz & Murphy, 1992; Katz *et al.* , 1999; Goldin & Katz, 2009). They have also been applied to study the effects of migration on native wages while incorporating migrant-native imperfect substitutability (Ottaviano & Peri, 2012; Manacorda *et al.* , 2012; Card, 2009). However, these models traditionally assign migrants to education groups according to their observed education, creating a misclassification problem if migrants downgrade to compete with workers in education groups different from their own (Dustmann *et al.* , 2013, 2016).[3] Therefore, when estimating the model, I iteratively randomly assign migrants to education groups with probability weights equal to the native education composition of their occupation in Colombia. I thus estimate the substitutability between natives within an education group and the migrants they compete with.

Another interesting question is whether migrants who downgrade carry skills and experiences into low-skill occupations that increase their productivity. This would increase their effective labor supply and amplify their effect on wages in these occupations. To evaluate this, I estimate wage regressions to identify the education premia for migrants in low-skill occupations, and I find small wage benefits associated with schooling for migrants who downgrade. I use these coefficients to adjust the effective

[3]One can still interpret the substitutability parameters estimated in these models as incorporating the effect of migrant downgrading. However, if the goal is to simulate the effects of migration under counterfactual degrees of downgrading, then we need a framework that incorporates downgrading separately from the other mechanisms that determine migrant-native substitutability.

labor supplied by migrants in low-skill occupations and this has little effect on the estimated parameters.

I estimate migrant-native elasticities of substitution of around 6 for workers with post-secondary education, implying levels of short-run substitutability comparable with that of high-skill migrants recently arrived in the UK (Manacorda *et al.* , 2012). Importantly, this elasticity increases to 15 for workers without completed secondary schooling. This elasticity is relatively large considering that most Venezuelans have been in Colombia for only a few years, and it is comparable in magnitude with the average long-run migrant-native substitutability estimated in the U.S. of around 20 (Ottaviano & Peri, 2012). As discussed, the higher substitutability between migrants and natives in low-skill occupations could be driven by the fact that these jobs tend to be more routine and less specialized or because they are more likely to be informal. I observe that migrant-native substitutability is larger in the informal sector, lending some support to this hypothesis. Finally, I estimate across-education substitutability parameters that are consistent with the literature in Latin America (Manacorda *et al.* , 2010; Fernández & Messina, 2018; Acosta *et al.* , 2019), and I estimate low substitutability between workers with and without secondary education.

I then use the estimated model to calculate the total effect of migration on native wages over this period. This generates total predicted wage effects of around -1% overall and -4% for native workers without a high-school degree. Considering the short time-frame of the migration (the majority of migrants did not arrive until 2018), I hold capital fixed for this analysis, such that there are diminishing returns in the labor aggregate. However, I can also use the model to show that, under long-term capital adjustment, the average wage effect is eliminated but the distributional

consequences of downgrading persist. These effects on wage inequality are amplified by high migrant-native substitutability in low-skill jobs and low substitutability across education groups. As discussed, these are both common characteristics of the developing country context, highlighting that the distributional consequences of downgrading may be larger in developing countries.

Finally, I simulate a "no downgrading" counterfactual in which I reallocate migrants to compete with natives in their observed education group.[4] This reallocation reduces the wage effect for native workers without a high school degree from -4% to -3.1%. At the same time, despite facing increased within-group competition, the wage effects for natives with completed secondary schooling is unchanged, and natives with post-secondary are only slightly negatively affected. These results reflect both the higher complementary between migrants and natives in higher-skill occupations, as well as the increase in total output that results from moving migrants into more productive and relatively under-supplied jobs, which increases everyone's marginal product and counteracts the greater competition faced by more educated natives. In other words, improved migrant-occupation matching reduces labor market competition for low-skill natives and increases productivity for all workers.

This counterfactual assumes that the quality of education in Venezuela and Colombia are comparable. In the Appendix, I evaluate this assumption. I first note that a 2009 cross-country evaluation of 15-year-olds ranked Venezuela and Colombia extremely close in reading, math, and scientific literacy, with Venezuela slightly outperforming Colombia in every category. I then study the education earnings premia

[4]This counterfactual takes the migrant inflow as given. In the absence of barriers to occupational choice, more high-skill foreigners may choose to migrate, and I do not consider this general equilibrium effect here.

for Colombian and Venezuelan migrants in the U.S. who were educated in their home country. I find that the earnings premium for post-secondary education is slightly higher for Venezuelans, and this is not likely to be driven by different migrant selection out of each country. The evidence indicates that, if anything, Venezuelan education is higher quality than Colombian education, in which case the model underestimates the true distributional consequences of downgrading.

I treat migrant downgrading as exogenous rather than as an equilibrium outcome under migrant occupational sorting. The primary factors driving migrant downgrading in Colombia include barriers to education recognition and occupational licensing, gaps in legal status, lack of employer networks, and discrimination (Graham *et al.* , 2020; Chaves-González *et al.* , 2021). Thus, the assumption of exogenous downgrading is motivated by the fact that these factors have effectively closed off access to high-skill occupations for many migrants. As discussed in Sections 3.3, an implication of this assumption is that migrants who do and do not downgrade are exchangeable. I therefore conduct sensitivity tests to show that, even when migrants who downgrading are negatively selected according to unobserved skill, there are large benefits to undoing downgrading.

The results highlight the importance of policies that match migrants to jobs according to education and experience, not only to directly benefit migrant workers, but also to mitigate negative wage effects for the most vulnerable natives and to maximize the economic gains from migration. While linguistic barriers are often a focus in the discussion of refugee integration, this is less likely to be relevant in the Colombian setting (Arendt *et al.* , 2020; Lochmann *et al.* , 2019). Work permits have also been available to many migrants, though there are significant gaps in coverage. An

important question is whether the 2021 expansion of 10-year legal status to nearly 1 million Venezuelans will mitigate downgrading. Other factors likely to be relevant are barriers in educational recognition and occupational licencing, lack of networks with employers, and employer discrimination. Programs that certify migrant education and skills and directly connect migrants with employers, including assistance relocating to cities with well-matched job vacancies, are promising candidates to reduce migrant occupational downgrading in Colombia (Graham *et al.* , 2020).

Literature and Contributions: A vast literature studies the causal effects of sudden migration or forced displacement on native employment and earnings and this was discussed in Chapter 1. A recent review by Verme & Schuettler (2021) finds that negative effects on native economic outcomes are most likely to be observed in middle-income developing countries, and to be most pronounced for less-educated natives working in the informal sector. An example of such a setting is Syrian refugee migration to Turkey. Like in Colombia, this migration caused a large and sudden increase in labor supply mostly in the informal sector, and the literature has found evidence for negative employment effects and potentially small negative wage effects among lower-skill and informal workers (Altındağ *et al.* , 2020; Aksu *et al.* , 2018; Ceritoglu *et al.* , 2017; Tumen, 2016; Del Carpio & Wagner, 2015). By taking a more structural approach, this paper sheds light on the mechanisms driving differences in the economic effects of migration observed across settings. The results highlight the disproportionately large effects of migrant downgrading on wage inequality in the developing country setting.

I also contribute to the literature studying the economic effects of Venezuelan

migrants in Latin America (Caruso *et al.* , 2019; Delgado-Prieto, 2021; Penaloza-Pacheco, 2021; Bonilla-Mejía *et al.* , 2020a; Santamaria, 2020; Olivieri *et al.* , 2021a; Zago, 2020; Morales & Pierola, 2020). These papers have found evidence for negative wage effects concentrated on less educated natives in the informal sector in Ecuador, Brazil, and Peru in addition to Colombia. Many have proposed migrant downgrading as a mechanism driving this effect on wage inequality, which I test directly in this paper (Lombardo *et al.* , 2021; Olivieri *et al.* , 2021b). I also relate to a literature documenting the systematic over-education of migrants relative to natives across many countries and its consequences for migrant earnings and assimilation (Leuven & Oosterbeek, 2011; Piracha *et al.* , 2012; Chiswick & Miller, 2009; Eckstein & Weiss, 2004; Friedberg, 2000). I contribute to this with a focus on the effects of downgrading for native outcomes, which is less studied.

Methodologically, this paper relates to the literature using nested-CES models of labor demand with migrant-native imperfect substitutability to study the effects of migration on wages (Ottaviano & Peri, 2012; Manacorda *et al.* , 2012; Card, 2009). As discussed, I adapt this framework to directly incorporate migrant occupational downgrading, which allows me to estimate the counterfactual effects of migration absent downgrading. I also apply this framework for the first time in a developing country setting, and the context of mass, sudden arrival allows for cleaner identification of substitutability parameters relative to the literature. Dustmann *et al.* (2013) also avoids the mismatch problem of assigning migrants to education groups by estimating wage effects along percentiles of the native wage distribution, and finds that wage effects of migration to the UK were negative below the 20th percentile, where migrants were over-represented, and positive above the 20th percentile, where

migrants were under-represented. This approach is similar in spirit to this paper. However, I allow for imperfect substitutability between migrants and natives within skill groups, as well as a nested-CES structure that allows for substitutability parameters to vary across different levels of education, which as I demonstrate both have important implications for the predicted wage effects. A few existing structural models of the labor market have also modeled migrant occupational downgrading (Llull, 2018; Muysken *et al.* , 2015). Unlike these papers, I take migrant downgrading as given, I incorporate imperfect substitutability between migrants and natives on the demand side, and I use this extended model of labor demand to conduct counterfactuals in which there is no migrant downgrading.

This paper proceeds as follows: Section 3.2 reviews the data and characterizes migrant downgrading. Section 3.3 outlines the model and estimation. Sections 3.4, 3.5, and 3.6 respectively present the results, counterfactuals, and conclusion.

3.2 Data and Migrant Characteristics

As in Chapter 1, data for this analysis come primarily from the GEIH. Since 2013, this survey has included a migration module that records where a person was living 1 and 5 years ago, and can therefore be used to measure both migration rates and labor market outcomes for natives and migrants across locations and over time. This is rare in the developing country setting and necessary to estimate the model.

Also as in Chapter 1, analysis is conducted at the metropolitan area level. These are constructed according to commuting patterns following the recursive algorithm developed in Duranton (2015). The GEIH does not survey people in all of these

metro areas and in others has a very small sample. I therefore restrict my analysis to the 79 metro area that contain at least 300 GEIH observations per year over the study period, representing around 80% of the Colombian population and 90% of the Venezuelan migrant population. The GEIH is designed to be representative of a smaller group of 23 metro areas that are loosely overlapping with my largest 23 metro areas, but not for areas finer than this. One might therefore be concerned about measurement error in the smaller metro areas, and I address this in two ways. First, I test the robustness of results to increasing the 300 annual observation threshold to 1,000, similar to restricting analysis to the 23 metro areas for which the survey is officially representative. Second, in my analysis I use an instrumental variable based on migration shares from the complete 2005 National Census, which will isolate measurement error in the yearly metro-level migration rates.

I define migrants as anyone who was living in Venezuela 5 years ago.[5] In Table A12, I display the characteristics of working-age migrants and natives in my metro area sample over time. In 2019, migrants and natives had a similar gender composition and migrants were moderately younger, with an average age of 32.1 compared to 39.4 for natives. The migrant profile also became slightly younger and more female over this period, and these demographic characteristics are controlled for in the analysis.

[5]Because so few migrants were in Colombia before 2014, this measure is approximately the stock of migrants arrived since 2014. This measure also includes Colombian-born return migrants, who make up around 20% of all migrants from Venezuela during this period. Many of these migrants left for Venezuela during Colombia's decades-long civil war, some as early as in the 1970s, and many have lived in Venezuela for decades. These migrants also increase the local labor supply and are closely correlated with the locations of Venezuelan born-migrants, so excluding them could cause an overestimation of the labor market effects of migration. Ideally, one could study the effects of each group separately, but they are empirically difficult to untangle due to the close correlation in the location of foreign-born and return migrants.

In 2019, migrants were more likely to have completed secondary education but less likely to have some post-secondary. Still, 24.5% of migrants, relative to 27.7% of natives, have some post-secondary, indicating that this is not a low-skill migration wave. Migrants who arrived later were slightly more educated, but the last row of Table A12 shows that they continued to enter occupations that require similarly low levels of education. Thus, the changing rate of migration by occupation skill group over the sample period, which drives estimation in my analysis, are driven mostly by changes in the total number of migrants arriving to each metro area and not by the changing occupation composition of migrants. However, migrants were increasingly more likely to be own-account self-employed and less likely to be in the formal sector. By 2019, 46.8% of migrants were own-account, relative to 39% of natives, and 12.4% were in the formal sector relative to 47.2% of natives.[6]

Despite having relatively similar levels of completed education, migrant hourly wages were 45% lower than native wages, and this gap falls to 27% controlling for occupation fixed effects. In the estimation, this wage gap will impact the gap in migrant-native productivity parameters, and only changes in relative wages will impact the substitutability parameters. While native wages increased steadily by around 4% over this period, migrant wages decreased by around 27%, and this magnitude does not change after controlling for the changing migrant demographic composition. This suggests that imperfect substitutability between migrants and natives may cause migrant wages to decrease disproportionately in response to an increase in migrant labor supply.

[6]Formality in this paper is defined according to enrollment in mandatory health and pension programs. Own-account workers are self-employed with no employees.

Table 3.1: Occupation Skill Groups by Education

	2014 Non-Migrant Population				2019 Migrant Population			
	Total	< Secondary	Secondary	Post-Secondary	Total	< Secondary	Secondary	Post-Secondary
	266,095	110,665 (41.6%)	77,138 (29.0%)	78,292 (29.4%)	13,971	4,904 (35.1%)	5,355 (38.3%)	3,713 (26.6%)
Percent in occupation with:								
Mode < Secondary	64.9%	90.0%	68.8%	25.6%	81.7%	91.8%	83.1%	66.5%
Mode Secondary	8.2%	5.5%	14.5%	5.9%	8.1%	5.5%	10.7%	7.9%
Mode Post-Secondary	26.9%	4.5%	16.7%	68.6%	10.1%	2.7%	6.2%	25.7%
	Total	< Secondary	Secondary	Post-Secondary				
Change in total labor supply under:								
Downgrading (assigned by occupation)	6.2%	8.0%	6.1%	3.8%				
No Downgrading (assigned by education)	6.2%	5.2%	8.2%	5.6%				

Sample restricted to working residents of 79 primary metro areas, age 15-64, with completed education. A migrant is defined as anyone who was living in Venezuela 5 years before the survey. Occupations grouped according to the mode education of native workers in that occupation pre-2014. Migration-induced changes in labor supply under downgrading calculated by splitting each migrant observation into education groups according to the pre-2014 native distribution in their occupation. Source: GEIH, population weights applied.

Table 3.1 shows the occupation skill distribution within education groups among workers in the sample, among non-migrants in 2014 and migrants in 2019. For this Table, I define occupation groups according to the modal education of natives in that occupation between 2010-2014. Among all education groups, migrants are less likely to be observed in post-secondary occupations. Among those with post-secondary education, 74% of migrants and 31% of natives are in occupations where the modal native has post-secondary, demonstrating the striking level of downgrading among well-educated migrants. Similar downgrading, though less severe, is observed for migrants with a secondary degree.

To compare downgrading in Colombia with downgrading in other migration waves around the world, I calculate the share of migrants who have a level of education greater than the mode in their occupation.[7] This results in an estimate of 52% of migrants, relative to 34% of natives, who are over-educated. These estimates are large relative to immigrant over-education numbers calculated using the same method in other countries, which range from 16% in Denmark to 24% in the UK and 28% in

[7]The mode is preferred over the mean for this exercise because it has been shown to generate over-education rates more comparable with those based on classifications of occupation skill requirements and self-reported worker over-skilling when such data are available (Leuven & Oosterbeek, 2011).

the US (Piracha *et al.* , 2012). However, they are not unheard of - estimates are as high as 39% in Spain, 41% among established male migrants in New Zealand and 52% among those who arrived in the past five years (Sanroma *et al.* , 2008; Poot & Stillman, 2010). Therefore, the extent of migrant over-education in Colombia is large but within the range of estimates observed in other countries.

The bottom of Table 3.1 presents the group-specific increase in labor supply from migration as a share of the total 2014 population. To understand the extent to which migrant downgrading impacts labor competition by skill group, I first assign migrants to education groups based on the native distribution between 2010-2014 in their observed occupation. For example, 14% of migrants are in the "restaurant workers" occupation, which includes waiters, bartenders, and chefs. Among natives in this occupation, 61% did not complete secondary, 27% completed secondary, and 12% have post-secondary. Migrants in this occupation are thus assigned to education groups with probability weights equal to these proportions. This is what I refer to as the change in labor supply "under downgrading". As I discuss in Section 3.3, this is the assignment mechanism that I use when I estimate the elasticity of substitution between migrants and natives within education group, thus capturing the effect of competition between natives and migrants who compete within their education group. I then show how labor supply would change "under no downgrading", in which migrants are assigned to education groups according to their actual level of education.

The bottom of Table 3.1 shows that the total growth in the population due to migration over this period is 6.2% when restricted to the sample of urban workers age 15-64. Under downgrading, natives in each education group experience an 8%, 6.1%,

and 3.8% increase in labor supply respectively, demonstrating the disproportionate impact of migration on supply of less educated workers. If migrants were instead competing with workers of their own education, the supply shifts heavily towards completed secondary and post-secondary workers - both experience around a 2pp increase in labor supply. One of the key questions I explore is whether such a reallocation would simply shift the economic effects of competition to better-educated workers, or whether there are aggregate gains in wages driven by the structure of the production function.

When I define "no downgrading" as migrants competing with workers of similar education, I implicitly assume that schooling quality in Colombia and Venezuela are comparable. This assumption is not necessary for the model's estimation or use to predict the economic effects of immigration. However, if Venezuelan education is less (more) valuable than Colombian education, then the "no downgrading" counterfactual places Venezuelans too high (low) in the native education distribution. I compare the quality of the Colombian and Venezuelan education systems in two ways. First, I look at the 2009 Programme for International Student Assessment (PISA), which evaluates 15-year-olds' knowledge and skills in reading, mathematical and scientific literacy and included both Colombia and the Venezuelan state of Miranda (which includes around 10% of the population). Out of 74 countries, Colombia and Venezuela ranked extremely close in every category, though Venezuela ranked slightly higher in all categories and this difference is statistically significant for both math and science literacy (see Table A14).

Second, I conduct an analysis of the education earnings premium for Venezuelan and Colombian migrants in a common external labor market: the U.S. For this

analysis, I use the 2015 U.S. American Community Survey (ACS), and I restrict the sample to Colombian-born and Venezuelan-born migrants who were educated in their home country and have been in the U.S. for at least 5 years. In Table A13, I show that the earnings premia for secondary and post-secondary education are similar for migrants from each country, and the post-secondary earnings premium is in fact higher for Venezuelans, controlling for differences in age, gender, and time since arrival. This is especially true for younger migrants who completed their education and migrated after 1990, which is the group most comparable with the migrant population in Colombia.[8] Thus, the evidence indicates that the quality of secondary and post-secondary education is similar across countries and if anything higher in Venezuela, which would lead the counterfactual to underestimate the distributional impact of undoing downgrading.

3.3 Model

3.3.1 Nested CES Framework

To model the economic response to the migration, I consider a model of CES labor demand disaggregated by education group and nativity status. Even in a basic disaggregation (as I use here) with only six cells, a fully flexible production function with

[8]It is possible that this gap in earnings premia could be driven by differences in migrant selection. However, the share of the home-country population within each education group that migrated to the U.S., and the share of those with post-secondary who migrated to the U.S., relative to those without secondary, is larger for Venezuela than it is for Colombia. Under a simple model of migrant selection, a higher share of the home country migrating to the U.S. is indicative of a lower latent ability threshold above which people migrate. Thus, we would expect these regressions to underestimate the true gap in the post-secondary earnings premium between Venezuelans and Colombians.

minimal structure would require estimating 30 cross-group elasticities. To reduce the number of parameters estimated, I use a nested CES structure, following an extensive literature in labor economics that has used this framework to study relative changes in labor supply and inequality (Katz & Murphy, 1992; Card & Lemieux, 2001). Following the examples of Ottaviano & Peri (2012) and Manacorda *et al.* (2012), I extend this framework to allow for imperfect substitutability between migrants and natives, which was shown to be very relevant for estimates of the total wage effects of immigration.

Firms produce output according to a constant-returns-to-scale Cobb-Douglas production function, which is widely applied in the macro growth literature:

$$Y = AK^{1-\zeta}L^{\zeta} \tag{3.1}$$

where K is capital, ζ is the labor share of income, and A is a skill-neutral technology parameter. Labor is a CES aggregate of three education types: less than high school ($e = 1$), high school graduates ($e = 2$), and any post-high school ($e = 3$)[9]:

$$L = \left(\alpha_3 L_3^{\frac{\sigma_3 - 1}{\sigma_3}} + \alpha_{-3} L_{-3}^{\frac{\sigma_3 - 1}{\sigma_3}} \right)^{\frac{\sigma_3}{\sigma_3 - 1}} \tag{3.2}$$

$$L_{-3} = \left(\alpha_2 L_2^{\frac{\sigma_2 - 1}{\sigma_2}} + \alpha_1 L_1^{\frac{\sigma_2 - 1}{\sigma_2}} \right)^{\frac{\sigma_2}{\sigma_2 - 1}} \tag{3.3}$$

where σ_3 measures the elasticity of substitution between workers with and without a college degree, and σ_2 measures the elasticity of substitution between workers with

[9]I use these broad education groups to maintain a large enough sample size within each cell. If I separate out those who did not complete primary or who are college dropouts, the confidence intervals on the substitutability parameters become uninformative.

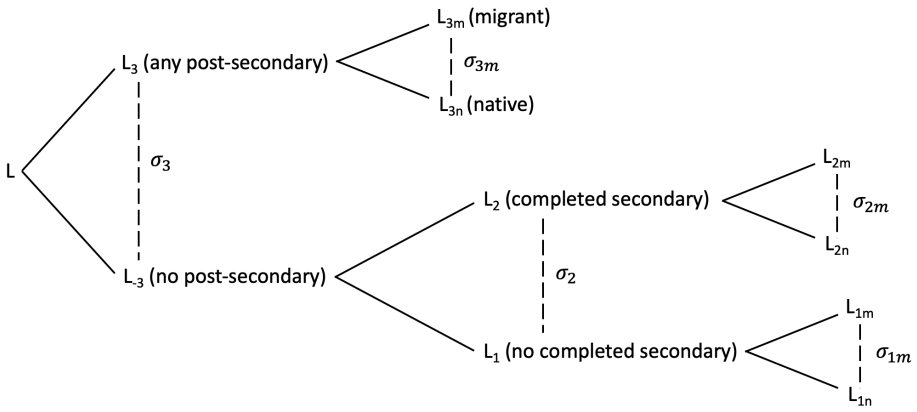


Figure 3.1: CES Nesting Structure

and without a high school degree. α_e is the relative productivity of group e, standardized so that $\alpha_1 + \alpha_2 = \alpha_3 + \alpha_{-3} = 1$.

I then assume that each education-specific cell is a CES combination of native (n) and migrant (m) labor:

$$L_e = \left(\alpha_{em} L_{em}^{\frac{\sigma_{em}-1}{\sigma_{em}}} + \alpha_{en} L_{en}^{\frac{\sigma_{em}-1}{\sigma_{em}}} \right)^{\frac{\sigma_{em}}{\sigma_{em}-1}} \tag{3.4}$$

where the elasticity of substitution between migrants and natives σ_{em} is allowed to vary by education group. The productivity parameters that are again standardized such that $\alpha_{em} + \alpha_{en} = 1$. The nesting structure is presented in Figure 3.1.

I assume perfect competition, such that wages are set equal to the marginal product of labor, and this generates the following wage equation by nativity status $j \in \{m, n\}$ and education groups $e \in \{1, 2\}$:

$$\begin{aligned} \ln W_{ej} = \ln\left(AK^{1-\zeta}\zeta\right) + \ln\alpha_{-3} + \ln\alpha_e + \ln\alpha_{ej} + \left(\zeta - 1 + \frac{1}{\sigma_3}\right)\ln L \\ + \left(\frac{1}{\sigma_2} - \frac{1}{\sigma_3}\right)\ln L_{-3} + \left(\frac{1}{\sigma_{em}} - \frac{1}{\sigma_2}\right)\ln L_e - \frac{1}{\sigma_{em}}\ln L_{ej} \end{aligned} \tag{3.5}$$

and for $e = 3$:

$$\ln W_{3j} = \ln\left(AK^{1-\zeta}\zeta\right) + \ln\alpha_3 + \ln\alpha_{3j} + \left(\zeta - 1 + \frac{1}{\sigma_3}\right)\ln L + \left(\frac{1}{\sigma_{3m}} - \frac{1}{\sigma_3}\right)\ln L_3 - \frac{1}{\sigma_{3m}}\ln L_{3j} \tag{3.6}$$

According to these equations, wages are a function of group-specific productivity and the labor supplies in each group. The final term in each equation captures the fact that wages are decreasing in the labor supply in ones own skill-nativity group (L_{ej}), and less so as the substitutability between natives and migrants in skill group e increase. In this sense, greater substitutability between migrants and natives "spreads out" the wage impact across both groups. Likewise, greater substitutability across skill groups "spreads out" the wage impact across skill groups, and wages are decreasing in skill-specific labor supply (L_e) as long as the within-skill group migrant-native substitutability is greater than the across-skill group substitutability with the next group in the nest (in the case of $e \in \{1, 2\}$, when $\sigma_{em} > \sigma_2$). Finally, the term $\frac{1}{\sigma_3}\ln L$ reflects the fact that, in a model with imperfect substitutability, all workers benefit from increases in the total labor supply. This benefit is mitigated by the term $(\zeta - 1)\ln L$, which captures the diminishing returns in the labor aggregate when capital is held fixed. In Section 3.5, I will relax this assumption to observe what happens to predicted wage effects when capital adjusts in the long-term.

A major take-away from this framework is that, because of imperfect substitutability, native wages depend on the labor supply in all skill groups, not just one's own. This implies that if migrants are moved to compete with more educated natives, this does more than mechanically shift competition from low- to high-skill groups; it also adjusts the benefits that workers receive from complementarities across groups.

Whether the wage effect of migration is negative or positive for a skill groups depends on the tradeoff between these across-group and within-group effects.

Finally, note that the migration-driven change in labor supply within an education group can be expressed as a function of the total migrant share of the population M:[10]

$$d\ln L_{em} = \frac{s_e^M}{s_e^N} M \tag{3.7}$$

where s_e^M is the share of migrants in education group e and s_e^N is the share of the native population in group e. Furthermore, in the nested CES framework, the change in labor supply for an aggregate group can be expressed as sum of the supply change in each subgroup weighted by each group's wage bill share. Thus, the wage equations can be expressed as a linear function of M, with a coefficient that is a function of the share of the migrant population going to each education group, the wage bill of each group, and the model parameters. In this sense, the non-structural estimate of the total wage effect estimated in Chapter 1 makes no assumptions about migrant-native substitutability.

3.3.2 Estimation

Estimating Substitutability Parameters

Taking the ratio of $\ln W_{em}$ and $\ln W_{en}$ using equations (3.5)-(3.6) yields the following:

$$\ln\left(\frac{W_{em}}{W_{en}}\right) = \ln\left(\frac{\alpha_{em}}{\alpha_{en}}\right) - \frac{1}{\sigma_{em}} \ln\left(\frac{L_{em}}{L_{en}}\right) \tag{3.8}$$

[10] A similar modelling approach is taken in Edo (2020) and Dustmann *et al.* (2017).

This equation can be used to estimate σ_{em} using changes in the relative wages and labor supplies of migrants and natives. Intuitively, as migrants and natives become increasingly substitutable, wages equalize across groups as the relative labor supplies change, and the sensitivity of the wage ratio to the labor ratio approaches zero.

This is estimated via OLS and 2SLS with the following regression:

$$\ln\left(\frac{W_m}{W_n}\right)_{ect} = \lambda_{ec} - \frac{1}{\sigma_{em}}\ln\left(\frac{L_m}{L_n}\right)_{ect} + \epsilon_{ect} \tag{3.9}$$

where e, c, and t represent education groups, metro areas and years respectively. Mean log wages and number of workers within each of the 1,422 education-year-metro groups (3 education groups X 79 metro area X 6 years covering 2014-2019) are calculated among migrants and natives age 15-64, where a migrant is defined as anyone who was living in Venezuela 5 years ago.[11] Labor supply is defined as the number of workers in an education-metro-year cell. Each regression is run separately by education group, so the migrant-native substitutability parameter is allowed to vary by education group. Cells are weighted by sample size to downweight small-sample cells. Finally, I hold the native population fixed to its 2014 value to mitigate any effects driven by changes in this variable. In order to account for endogenous migrant locational sorting, I use an instrument based on the historical settlement of

[11] Hourly wages are calculated for the primary job as past-month income over 4.2 times hours worked in a typical week in the past month, including all overtime, benefits and other transfers. This is calculated for all workers regardless of self-employment or formality status, and I use the term hourly wages to refer to the hourly earnings for the self-employed. Hourly wages are adjusted for yearly inflation at the national level and residualized from a regression on gender and 10-year-wide age group fixed effects, to absorb effects driven by the demographic composition of workers within education groups. Finally, they are winsorized at the top and bottom .5% within each year. I choose not to use the GEIH sampling weights since the survey is not designed to be representative at this level, but results are not sensitive to this decision. I use mean log wages, rather than log mean wages, following the discussion in Borjas *et al.* (2012).

Venezuelans in Colombia that is discussed extensively in Section 3.3.2.

Geographic variation is essential in my context, considering the large geographic spread and short time period of the Venezuelan migration to Colombia. There has been debate over the value of cross-city comparisons with this framework, considering that migrants and natives can endogenously sort across locations in response to changing labor market outcomes (Borjas *et al.* , 1997). Nonetheless, after carefully accounting for geographic sorting, cross-city comparisons have proved useful for identifying labor market impacts of migration and produce estimates that are generally consistent with time series-based estimates (Card, 2009; Ottaviano & Peri, 2012). As mentioned, because of the unique data that match labor market outcomes to migration histories in Colombia, I confirm in my robustness checks that endogenous native internal migration does not bias my results.

The education-metro fixed effects absorb the relative wage factors that are constant within metro area and can be used to construct α_{em} and α_{en}.[12] Thus, identification is driven by changes in the wage and labor ratios within metro area over time, and anything that affects the wage ratios across metro areas (for example, wage discrimination against Venezuelans) will not impact estimates of $\widehat{\sigma_{em}}$ as long as it does not vary over time. Importantly, because equation (3.9) uses ratios and not levels, any changes in wages that equally affect migrants and natives within the same education-metro-year cell (for example, aggregate productivity shocks) will also not impact estimates of $\widehat{\sigma_{em}}$.[13]

[12] $\widehat{\alpha_{em}} = \frac{e^{\widehat{\lambda_{ec}}}}{1+e^{\widehat{\lambda_{ec}}}}$ and $\widehat{\alpha_{en}} = \frac{1}{1+e^{\widehat{\lambda_{ec}}}}$

[13] Ideally, this regression could also include a time trend to capture trends in demand that differentially affect migrants and natives. In practice, adding year fixed effects substantially reduces power. In Table A16, I show that, when estimating (3.9) at the aggregate level (grouping education groups together), the inclusion of a linear year trend has a minimal effect on the point

In the final steps of the estimation, I estimate σ_2 and σ_3. Armed with the estimates of $\widehat{\sigma_{em}}$, $\widehat{\alpha_{em}}$ and $\widehat{\alpha_{en}}$, I use equation (3.4) to calculate the education-specific "effective" labor supplies and estimate the next step of the model, which is derived using the population-weighted mean of migrant and native waves within each group:

$$\ln\left(\frac{W_2}{W_1}\right)_{ct} = \lambda_{2,c} + \lambda_{2,t} - \frac{1}{\sigma_2}\ln\left(\frac{L_2}{L_1}\right)_{ct} + \epsilon_{2,ct} \tag{3.10}$$

Likewise, the estimates from regressions (3.9) and (3.10) can now be used to calculate the terms needed to estimate σ_3:

$$\ln\left(\frac{W_3}{W_2}\right)_{ct} = \lambda_{3,c} + \lambda_{3,t} - \frac{1}{\sigma_3}\ln\left(\frac{L_3}{L_2}\right)_{ct} + \epsilon_{3,ct} \tag{3.11}$$

The same intuition applies for these equations, in that variation in the ratio of wages and labor supplies is used to identify the substitutability parameters, holding constant metro and year fixed effects. In this case, I can compare my estimates of σ_2 and σ_3 to other estimates from the literature in Colombia and Latin American.

Incorporating Occupational Downgrading

As it stands, this framework does not explicitly account for migrant occupational downgrading because it classifies migrants according to their education rather than their occupation (Dustmann *et al.* , 2013, 2016). I therefore develop a method to assign migrants to education groups according to their observed occupation. Within

estimates but increases the standard errors by a factor of 2.5. In the 2SLS model, the first stage Kleibergen-Paap Wald statistic becomes weak. Thus, while it is not possible to include these controls in the full model, I can at least be confident that they do not change the OLS point estimates (which are very similar to the 2SLS results) in the aggregate sample.

each occupation, I calculate the share of natives in each education group between 2010-2014. Next, I randomly assign migrants into education groups with probability equal to these shares for their observed occupation. Intuitively, if a high-skill migrant is working in a job that is mostly worked by natives in education group 1, they will be assigned to this group with high probability. I then calculate cell-specific average wages and labor supplies and I estimate the entire model. I repeat this for 100 random assignments and take the average parameter values. Standard errors are bootstrapped with 100 samples and clustered at the metro level.[14]

An assumption of this framework is that educated and uneducated migrants have the same productivity within a given occupation. This would be violated if, for example, migrant restaurant workers with post-secondary were more productive than those without post-secondary. I can investigate this empirically by estimating the education wage premium for migrants and natives across occupation skill levels. Table A15 shows that, controlling for gender, age, year fixed effects and metro fixed effects, migrants face a substantially smaller education premium than natives in occupations where the modal native worker has no secondary: 8.8% for having completed secondary and 15.1% for any post-secondary, relative to 19.7% and 41.9% respectively for natives. The education wage premia are also smaller for migrants than for natives in higher skill occupation groups. Though small, these wage premia indicate that the effective labor provided by educated migrants may be greater than that of less educated migrants, which would add bias to my estimates of σ_{em}. I therefore adjust the migrant labor supplies using the estimates from Column 2 of Table A15. For

[14] I drop the very small share of resample where a first-stage Kleibergen-Paap Wald statistic is weak, to ensure that this does not bias downstream steps in the estimation.

example, if a migrant with post-secondary is assigned to the "less than secondary" group, then I multiply their labor supply by 1.151, reflecting the wage premium for post-secondary migrants in low-skill occupations. In practice, this has little impact on the results.

Another assumption of this framework is that migrant occupation choice is exogenous, rather than an equilibrium sorting outcome. This is motivated by the fact that, in this setting, a variety of institutional factors have effectively closed off access to high-skill occupations for many migrants, such as gaps in legal status, educational recognition, and occupational licensing. Nonetheless, the counterfactual may be wrong if migrants who do and do not downgrade have different unobserved ability. In Section 3.5.2, I evaluate sensitivity of the counterfactual wage effects to variation in this unobserved skill wedge, and I show that the distributional benefits from undoing downgrading remain large even when migrants who downgrade are negatively selected on ability.

Instrumenting for Migration

Equation (3.9) is biased if migrant labor supply $L_{m,ect}$ is correlated with the error term. Specifically, the concern regards migrant sorting into locations according to the migrant-native wage ratio, as opposed to the traditional concern of sorting according to native wages. To deal with this potential endogeneity, I construct an instrument similar to that of Chapter 1, which is based on the 2005 Venezuelan share of the population. In Chapter 1, I showed that this share was strongly predictive of subsequent migrant locations, and I discussed the exclusion restriction and the appropriateness of this instrument in this setting extensively. I adapt the instrument to match the

functional form of the new endogenous variable:

$$Z_{ect} = \ln\left(\frac{L_{Ven}}{L_{Col}}\right)_{e,c,2005} * \ln\left(\frac{L_m}{L_n}\right)_{e,Nat^{-c},t} \tag{3.12}$$

The first term is the log ratio of Venezuelan-born to Colombian-born in the complete 2005 census in education group e and metro area c. The second term is the national log ratio of migrants to natives in education group e and year t, excluding migration into metro area c.[15] I use education-specific migrant shares to generate independent variation in predicted labor ratios by education group, exploiting the fact that migrants are more likely to have networks with migrants of similar levels of education.

3.4 Results

3.4.1 Parameter estimates

Table 3.2 shows the estimated elasticities of substitution between migrants and natives within each education group. The OLS and 2SLS results are almost identical, indicating little sorting of migrants according to migrant-native wage ratios. The first-stage Kleibergen-Paap Wald statistic (averaged over the 100 migrant random assignments) remains over 50 for each education group, and the coefficients from the

[15] By matching the functional form of the endogenous variable, the log transformation gives me substantially more power in the first stage regression, but it is not necessary to replicate my primary results. I also normalize the 2005 log-share to be positive by adding it to its minimum value (this ensures that the migrant share is increasing in the instrument), and I add one to the 2005 Venezuelan population to make the log-ratio non-missing in places with no Venezuelans in 2005.

first-stage regression are all positive and significant.[16] Below each 2SLS coefficient, I also present the negative inverse of each coefficient which corresponds to the elasticity of substitution, using the Delta method to calculate the standard errors.[17]

The estimates imply substitutability parameters of 15.2, 13.0 and 6.5 for those without secondary, completed secondary and any post-secondary respectively. The elasticity between migrants and post-secondary natives is comparable with the estimated elasticity of around 5 for recently arrived high-skill workers in the UK (Manacorda *et al.* , 2012). However, the elasticity with less educated natives is substantially larger. The magnitudes approach the long-term migrant-native substitutability estimated in the U.S. of around 20, estimated over only a few years rather than decades (Ottaviano & Peri, 2012). This will inflate the distributional consequences of occupational downgrading.

Why are migrants and natives more substitutable in low-skill work? This may reflect the fact that low-skill occupations tend to be more routine and require less skill specialization. These occupations are also more likely to be informal, which as discussed may increase substitutability with migrants. I test this directly by dividing the sample according to formality status. Unlike education, formality status is not fixed for native workers and endogenous sorting by natives could therefore bias these estimates. With this caveat in mind, I show in Table A17 that for the population average, migrant-native substitutability is 17 in informal jobs and 12 in formal jobs. Within education groups, substitutability is also higher among informal workers,

[16]The Kleibergen-Paap LM test with a single endogenous regressor is a first-stage F-statistic that is heteroskedasticity robust.

[17]Directly bootstrapping the inverse function is not possible because it is discontinuous at zero, which generates negative elasticities in extreme resamples.

Table 3.2: Migrant-Native Substitutability Parameters

	$-\frac{1}{\sigma_{1m}}$	$-\frac{1}{\sigma_{2m}}$	$-\frac{1}{\sigma_{3m}}$
OLS	-0.067 (0.011)	-0.077 (0.008)	-0.135 (0.027)
2SLS	-0.066 (0.009)	-0.077 (0.009)	-0.155 (0.035)
2SLS (σ)	15.17 (2.01)	12.95 (1.50)	6.46 (1.47)
Mean Kleibergen-Paap Wald Stat.	59.7	70.7	92.7
Mean First-Stage Coef.	0.33 (0.03)	0.37 (0.03)	0.40 (0.03)

Means presented from 100 random assignments of migrants to education groups. Metro-clustered bootstrapped standard errors with 100 resamples in parenthesis. Standard errors for the elasticities calculated using the Delta method. Kleibergen-Paap Wald statatic is clustered at the metro level. See paper for estimation details.

though the standard errors become substantially larger due to the small sample size within cells, especially within low-skill formal jobs. Though the difference across groups are not statistically significant, this indicates that formality may explain at least part of the higher migrant-native substitutability for low-skill workers.

In Table 3.3, I present the across-education group substitutability parameters. Because I hold native labor supply fixed, these estimates are driven by the arrival of migrants across metropolitan areas and years.[18] They account for differential trends in labor demand with the inclusion of year fixed effects. The elasticity of substitution between workers with completed secondary and less than secondary, σ_2, and between workers with secondary and post-secondary, σ_3, are 3.5 and 1.2 respectively. The estimates are very close to existing estimates from Colombia and other countries in Latin America, despite being estimated with a unique framework based on migrant

[18]This time, I do not use historical migration shares as an instrument, due to the compounding uncertainty introduced by using 2SLS in the second step of the estimation.

Table 3.3: Substitutability Across Education Groups

	$-\frac{1}{\sigma_2}$	$-\frac{1}{\sigma_3}$
OLS	-0.290	-0.868
	(0.140)	(0.361)
OLS Negative Inverse (σ)	3.45	1.15
	(1.67)	(0.48)

SEs bootstrapped, 100 resamples clustered at metro level, inverted using Delta method.

arrivals across locations. Across 16 countries in Latin America between 1991-2013, Acosta *et al.* (2019) estimate values of σ_2 and σ_3 of 3.5 and 2 respectively. Over the same period, in Argentina Brazil and Chile, Fernández & Messina (2018) estimate values of 2.3 and 1.5. While to my knowledge there are no existing estimates of σ_2 specifically in Colombia, various papers have estimated values of σ_3 in Colombia that consistently land between 1.3-1.5 (Medina & Posso, 2010; Santamaría *et al.* , 2004; Núñez & Sánchez, 1998). Thus, these estimates are very reasonable given the existing literature.

It is notable that σ_2 is much smaller than estimates of this parameter in the US, which range from around 30 to perfect substitutability (Ottaviano & Peri, 2012; Card, 2009). That σ_2 is lower in Latin America may reflect the large socioeconomic gap in secondary school completion, as well as the barriers to entering the formal sector faced by workers without secondary. This implies that, in the developing country context, wage effects of migration among workers without secondary will remain concentrated among these workers.

Finally, in Table 3.4 I present the estimated productivity parameters (averaged across metro areas). Migrants are relatively less productive than natives, reflected

Table 3.4: Productivity Parameters

α_{1m}	α_{2m}	α_{3m}	α_2	α_3
0.439	0.371	0.209	0.547	0.710
(0.008)	(0.008)	(0.023)	(0.007)	(0.010)

SEs bootstrapped, 100 resamples clustered at metro level, inverted using Delta method.

by the fact that α_{em} are all less than .5. This wage gap is most severe in high-skill occupations, where migrants have a relative productivity weight of .21, reflecting the larger wage penalty that migrants face in these occupations. α_2 is .55 and α_3 is .71, reflecting the relatively higher productivity of high-skill labor. One benefit of shifting migrants from low- to high-skill occupations is an increase in total productivity, which will increase the total effective labor supply governed by equations (3.2) and (3.3).

3.4.2 Robustness

In Table 3.5, I conduct various robustness checks for the migrant-native substitutability parameters. First, one may be concerned that the instrument is correlated with proximity to the Venezuelan border, which may in turn be related to economic trends during the period of analysis. In particular, locations very close to the Venezuelan border, which have high predicted migrant shares, also underwent changes during this period in economic activity, daily commuting from Venezuela, and crime. I therefore show that results are robust to dropping the six metro areas within 100 km driving distance of the Venezuelan border, which also include all of the outliers with a migrant share greater than 15% that have the potential to disproportionately

drive results.[19] To ensure that changing exposure to trade with Venezuela is not an omitted variable, I also control for net imports and exports to Venezuela at the department level and results do not change.[20] I also drop Bogotá, the largest city in the sample, to ensure it is not single-handedly driving any results.[21] Finally, I drop all metro areas with an annual sample size of less than 1,000 observations, resulting in 27 major cities closely overlapping with the 23 official areas for which the GEIH is representative. This is to ensure that measurement error within small areas is not driving results. In each case, the results remain very stable.

Another concern already discussed is native internal migration. According to analysis in Chapter 1, there were very small increases in out-migration among natives without post-secondary education, with no effects on in-migration rates. This could bias the estimation by endogenously changing the composition of natives across metro areas. Though I already hold native labor supply fixed, this compositional change may impact native wages. To account for this, I exploit the fact that I observe where natives lived 5 years before the survey, and I can assign all natives to their lagged metro area, to hold native composition fixed to the pre-migration period. The results remain unchanged.[22]

[19]Driving distance was calculated using Open Street Maps software, from the central municipality of the metro area to the closest crossing point along the Venezuelan border.

[20]While Venezuela used to be a top trading partner of Colombia, its trade shares steadily declined during the 2000s such that Venezuela represented a very small share of imports and exports in 2010. However, trade may have persisted longer or had lagged economic effects closer to the border. Trade volumes by department origin and destination are collected from DANE.

[21]I can also drop all metro areas one-by-one, and this is available upon request.

[22]If natives who would have experienced a migration-induced drop in wages leave their initial cities, then internal out-migration would also bias the substitutability parameters towards 0 (greater complementarity). However, this effect is likely small given the scale of the internal migration response. Furthermore, the education groups that experienced out-migration, groups 1 and 2, are those with higher migrant-native substitutability.

Table 3.5: 2SLS Migrant-Native Substitutability - Robustness

	$-\frac{1}{\sigma_{1m}}$	$-\frac{1}{\sigma_{2m}}$	$-\frac{1}{\sigma_{3m}}$
		Drop <100km from Border	
2SLS	15.94	13.64	6.52
	(2.53)	(2.00)	(1.75)
Mean K-P Stat.	101.0	102.1	117.6
		Drop Bogota	
2SLS	16.48	15.05	8.72
	(2.53)	(2.26)	(1.67)
Mean K-P Stat.	56.7	67.1	73.2
		Drop Annual Sample <1,000	
2SLS	14.88	13.12	6.46
	(1.97)	(1.63)	(1.46)
Mean K-P Stat.	56.5	75.4	100.2
		Control State Imports/Exports	
2SLS	16.19	13.38	6.11
	(2.49)	(1.49)	(0.93)
Mean K-P Stat.	44.2	54.3	71.2
		Natives Assigned to Past Metro	
2SLS	14.99	13.37	6.52
	(2.12)	(1.73)	(1.63)
Mean K-P Stat.	59.8	71.1	91.3
		Drop Wages of Recent Migrants	
2SLS	23.79	20.79	8.59
	(6.77)	(4.75)	(2.91)
Mean K-P Stat.	58.3	68.8	94.1

SEs bootstrapped, 100 resamples clustered at metro level, inverted using Delta method.

There may also be a concern that changing composition of migrants arriving over this period is driving changes in the migrant-native wage ratio. I have already residualised wages from age and gender composition. To see if other unobserved characteristics are driving a compositional effect, I set the wages of migrants who arrived in the past year to missing, so that very recently arrived migrants impact the labor supply but not the migrant wage in each year. When I do this, the standard errors increase substantially, and the substitutability parameters increase, especially for

workers in the lower two education groups. Therefore, absent compositional change, the true migrant-native substitutability is if anything larger than I estimate, and the gradient across education groups even steeper.

In Appendix Table A18, I conduct various additional robustness tests. The estimates do not change when I restrict analysis to full-time workers. One may be worried that labor supply is more elastic for women or younger workers. I can drop either group from the analysis and results remain unchanged, indicating that endogenous labor supply decisions among these groups are not influential. Migrant-native substitutability in low-skill occupations fall slightly when the sample is restricted to workers over age 22, but results are qualitatively similar. Results are also similar when analysis is restricted to workers over age 28, who clearly completed schooling in Venezuela before the election of Maduro and the onset of the crisis. I can also replace employment on the right-hand-side with the total working-age population of migrants and natives, again mitigating concern that endogenous labor supply is driving estimates. Finally, I can replace the 2005 census IV with one based on the 1993 census, and results do not change, consistent with the fact that metro-area migrant shares are highly correlated across these years.

3.5 Total Wage Effects of Migration

In this section, I use the estimated parameters to calculate the total wage effects of migration for natives at a national scale. We can use equations (3.5) and (3.6) to express the wage change experienced by natives as a function of the change in migrant

supply while holding the native labor supply fixed. This results in, for $e \in \{1, 2\}$:

$$d\ln W_{en} = \left(\zeta - 1 + \frac{1}{\sigma_3}\right) d\ln L + \left(\frac{1}{\sigma_2} - \frac{1}{\sigma_3}\right) d\ln L_{-3} + \left(\frac{1}{\sigma_{em}} - \frac{1}{\sigma_2}\right) d\ln L_e \quad (3.13)$$

and for $e = 3$:

$$d\ln W_{3n} = \left(\zeta - 1 + \frac{1}{\sigma_3}\right) d\ln L + \left(\frac{1}{\sigma_{3m}} - \frac{1}{\sigma_3}\right) d\ln L_3 \quad (3.14)$$

To calculate changes in labor supply, using the appropriate survey weights, I calculate the number of Venezuelan migrants who arrived to Colombia between 2014-2019 and the 2014 native population by education group, and insert these into the CES equations for effective labor supply (3.2)-(3.4). As in the model estimation, migrants are assigned to education groups according to the education distribution of natives in their observed occupation.

An important component of equations (3.13)-(3.14) is the term $(\zeta - 1)\,\Delta \ln L$, which captures the role of diminishing returns in the labor aggregate when capital is held fixed. In this model, if capital is fully flexible, it is generally not possible for migration to generate a non-trivial wage effect for the population average, because output becomes linear in the labor aggregate.[23] By holding capital fixed, I allow for negative average wage effects, which is what we observe in the 2SLS estimates of the total wage effect in Chapter 1. This can be motivated by the short-term nature of this immigration, since the majority of migrants did not arrive until 2018. I use the value of $\zeta = .49$, which is the preferred labor share for Colombia reported by the

[23] See Ottaviano & Peri (2008) for an extensive discussion of the role of capital flexibility in this framework.

Penn World Tables (averaged across 2014-2019, though it is relatively stable over this period).[24] This parameter determines the magnitude of the average wage effect. The distribution of wage effects, on the other hand, are driven by the differences in competition by education group combined with the within- and across-group elasticity parameters, and are not sensitive to the choice of ζ. I show this in a sensitivity analysis by varying ζ within plausible ranges of .4-.6, and the benefits of undoing downgrading by education group remain relatively stable.

Column A of Table 3.7 shows the total changes in observed labor supply, "effective" labor supply at each level of the nest, and predicted wage changes for natives by education group. The results show predicted wage effects of -4.0%, -3.4% and -0.3% for those without secondary, with secondary, and with post-secondary respectively.[25] These effects broadly mirror the non-structural wage effects estimated in Chapter 1, indicating that the model with the labor share $\zeta = .49$ does a reasonably good job of replicating the non-structural results.

I now consider a counterfactual in which I assign migrants to their observed education group rather than the one assigned according to their observed occupation. This can be thought of as a "no downgrading" scenario in which migrants compete with natives of the same education group. The results are presented in Panel B of Table 3.7. Without downgrading, the labor supply increase shifts substantially

[24]The biggest challenge with measuring the labor share in Colombia is that self-employment income includes a combination of labor and capital income. The method used here assumes that self-employed workers use labor and capital in the same proportion as the rest of the economy. Methods that impute labor shares of self employed workers according to observable characteristics predict very similar values over this period (Gomis, 2019).

[25]Standard errors for the wage effect are generated by simulating 1,000 draws from a joint normal distribution with the estimated means and standard errors for the migrant-native substitutability and productivity parameters.

Table 3.6: Total Effect of Immigration on Native Wages

Table 3.7: Total Native Wage Effects of Immigration

	Observed Change in Labor Supply (1)	ΔL_e $(e \in \{1,2,3\})$ (2)	ΔL_e $(e \in \{m3,3\})$ (3)	ΔL (4)	$\Delta \ln(W_{en})$ (5)
Education Group 1	7.98	7.52	6.07	3.09	-4.09
Education Group 2	6.07	4.63	6.07	3.09	-3.39
Education Group 3	3.84	1.89	1.89	3.09	-0.24
Total	6.21			3.09	-1.05

Model predictions for change in native wages from the change in the population due to Venezuelan migration from 2014-2019. See paper for details.

towards education groups 2 and 3: it decreases by 2.7pp in group 1, and increases by 2.1pp and 1.8pp in groups 2 and 3 respectively. The wage decrease faced by natives in group 1 reduces from -4% to -3.1%. Interestingly, the wage effect for natives with completed high school remains almost unchanged, despite this group facing a substantial increase in competition absent occupational downgrading. There is a small increase in the wage effect for post-secondary workers, from -.3% to -.6%, which in percentage points is around 2/5 of the gains faced by workers without completed secondary. For a visualization, these effects are plotted with 95% confidence intervals in Figure A7a.

Why does the increase in within-group labor supply not substantially reduce wages for workers with completed secondary and post-secondary education? First, migrants and natives are less substitutable in high-skill occupations, such that migrants in high-skill occupations put less downward pressure on native wages than migrants in low-skill occupations. Second, there are increases in total "effective" labor supply under the reallocation, from 3.1% to 3.5%, which increases everyone's marginal product working through the first term in equations (3.13) and (3.14). This reflects the higher productivity of workers in high-skill occupations, as well as the returns to spreading

labor supply across skill groups in the CES framework. Because σ_3 is relatively small, there are large gains in total output from moving migrants into group 3. This increase in aggregate marginal product counteracts the increase in within-group competition faced by medium- and high-skill workers.

3.5.1 Effects on Wages and Total Output Under Capital Adjustment

The model can also be used to predict the evolution of wages as capital adjusts in the long-term. In Figure A8, I adjust skill-neutral capital to grow at a rate proportional to the growth in aggregate labor supply, ranging from 0 (no capital adjustment) to 1 (full capital adjustment). Full capital adjustment can be thought of as taking around 5-10 years, since existing evidence indicates that average wages tend to recover from supply shocks by this time (Verme & Schuettler, 2021; Edo, 2020). Under full capital adjustment, the average wage effect is slightly positive. This will always be true in a long-run CRS economy with imperfect substitutability between migrants and natives (Amior & Manning, 2020; Borjas, 1995). Figure A8 shows that wage effects change linearly in capital adjustment within each education group. Under full capital adjustment, the wage effect is around -2.5%, -1.8%, and 1.4% for workers without completed secondary, with completed secondary, and with post-secondary respectively. Absent downgrading, these numbers change to -1.4%, -1.5%, and 1.2% (also presented in Figure A7b). There is still inequality in the wage effect, driven by higher substitutability in low-skill occupations and the larger benefit from complementarities accruing to post-secondary natives, but this long-run

inequality is mitigated by matching migrants to occupations fit for their education. It is also notable that the reduction in wage inequality under the counterfactual increases in capital adjustment, because total effective labor supply, and thus the increase in capital, is slightly larger under no downgrading.

The model can also be used to calculate the increase in total output driven by migration. Figure A9 shows that, with fixed capital, migration increased total output by around 1.5%. This increases to 1.7% without downgrading. As capital fully adjusts, these rise to 3.1% and 3.5% respectively, which, because this is a constant-returns-to-scale model, is proportional to the increase in effective labor supply over this period. Again, because there is more capital growth to match the larger increase in effective labor supply under no downgrading, the benefits of undoing downgrading for total output increase in the long-term.[26]

3.5.2 Alternative Parameter Values and Sensitivity

In Figure A10, I explore alternate values of σ_2 and σ_3. These parameters impact the wage effects under both downgrading and no downgrading, and I display the percentage point wage gains going from downgrading to no downgrading by education group. The results are similar using various values of these parameters within the plausible ranges observed in the literature of 2.5-3.5 for σ_2 and 1.5-2 for σ_3. As σ_2 increases, the gains for workers with and without completed secondary equalize very slowly, and only at a value of 15 is the gap in gains eliminated. This remains true at

[26] While the standard errors are large such that the percent effect on total output is not statistically different between the downgrading and no downgrading scenarios, it can be seen in panel B that the simulated standard errors of the difference between these effects is statistically different from zero.

$\sigma_2 = 30$, approximately the value estimated in the U.S. and the U.K., highlighting that low σ_2 amplifies the cost of migrant downgrading for workers without secondary. Results are somewhat more sensitive to increases in σ_3, which passes wage increases under the counterfactual from workers without post-secondary to workers with post-secondary. However, the consequences of downgrading persist for workers without completed secondary. For migrant downgrading to have no effect on the distribution of native wages, both σ_2 and σ_3 would have to be substantially larger than their estimated values, such as 30 and 5 respectively. Figure A10 also shows that, as mentioned, varying the labor share parameter from .4 to .6 has little consequence for the effects of downgrading.

A remaining concern regards the assumption that migrants and natives have comparable skill within an education group. If this assumption is wrong, for example, if the Venezuelan education system is higher- or lower-quality, then the counterfactual reassignment effectively matches migrants to the wrong education group, under a policy that matches migrants to jobs according to skill rather than observed education. In Figure A11, I therefore explore sensitivity to moving a share of the migrants in each education group in the counterfactual into neighboring groups according to the degree of skill mismatch. For example, when the degree of skill mismatch is .2, 20% of the migrants in education groups 1 and 2 are shifted up to groups 2 and 3 respectively. When it is -.2, 20% of migrants in groups 2 and 3 are shifted down to 1 and 2 respectively. We can see that, even if migrants are under-skilled such that 20% should match to a lower education group, there are still benefits to undoing downgrading for natives without secondary, on the scale of a .5pp gain rather than a 1pp gain. If Venezuelan education is worth slightly more than Colombian education,

then the benefits of undoing downgrading for workers without secondary may be even larger, on the scale of 1.5pp if 20% of migrants are shifted up an education group.

In a related issue, the model assumes that migrants who downgrade are comparable with those who do not. If this is wrong, then a share of the reallocated migrants should instead be placed in a neighboring education group. This is analogous to the previous exercise, except now only migrants who downgraded, as opposed to all migrants, are shifted to a neighboring group according to the degree of skill mismatch. Thus, as shown in Figure A12, counterfactual results are much less sensitive to migrants being placed in the wrong group. In an extreme case in which 30% of downgrading migrants are reallocated to the education group below their observed education, the benefits of undoing downgrading are still .7pp for workers without secondary.

3.5.3 Effect on Migrant Wages

Given the imperfect substitutability between migrants and natives, migration will have a larger detrimental effect on the wages of migrants relative to natives. This is visible in Table A19. The effect of migration on hourly wages for migrants in occupations that tend to employ less than secondary, completed secondary, and post-secondary workers is -24.6%, -26.6%, and -45.9% respectively. That the consequences are so much larger for migrants in higher-skill occupations reflects the lower substitutability between migrants and natives in higher-skill occupations, which benefits natives at the expense of migrants. In the counterfactual, hourly wage effects for migrants shift further towards higher-skill occupations. Unlike the results for natives, migrants in high-skill occupations lose relatively more (-6.0pp) than migrants in low-

skill occupations gain (+3.6pp), again driven by the relatively lower migrant-native substitutability in high-skill occupations.

The average wage gains for migrants under the counterfactual will depend on both the direct wage gains from moving into more productive occupations, as well as the equilibrium wage effects discussed in the previous paragraph. By simply calculating the difference in the average migrant wage under the counterfactual and the baseline, I find a large increase in the average migrant wage of 10.9% under the counterfactual. If I turn off the endogenous wage response by holding wages fixed within skill groups, this gain increases to 14.2%, highlighting the importance of considering general equilibrium wage effects when calculating the effects of combating downgrading on migrant wages.

3.6 Conclusion

In this paper, I estimate a nested CES production function with imperfect substitutability between migrants and natives and migrant occupational downgrading, and I used the results to calculate the counterfactual wage effect of Venezuelan migration on native Colombian hourly wages in the absence of occupational downgrading. I find that, absent downgrading, the increase in labor supply faced by workers without completed secondary falls from 8% to 5.2%, and their reduction in wages falls from -4% to -3.1%. At the same time, though it significantly increased competition faced by natives with secondary and post-secondary education, there was little change in the wage effects experienced by these groups. This reflects the increasing complementarity between migrants and natives in high-skill occupations, as well as the benefits

that all workers experience from increased productivity under the reallocation. These effects on wage equality and total output persist under long-term capital adjustment.

The model helps to explain why the effect of migration on labor market outcomes for low-skill workers may be larger in the developing country setting. I estimate high short-run migrant-native substitutability in low-skill occupations and showed that this is partially explained by the fact that these jobs are more likely to be informal. The low substitutability between workers with and without completed secondary schooling in Latin America also inflates the consequences of migrant downgrading for workers without completed secondary.

The results highlight the importance of properly matching migrants to jobs given their education and experience. My analysis excludes various factors that may further increase the benefits of reduced downgrading, such as transfer of sector-specific human capital and networks (Bahar *et al.* , 2020a, 2019), or the increasing labor demand and taxable income that would accompany larger migrant salaries and consumption (Graham *et al.* , 2020). I therefore may underestimate the total economic benefits of reducing occupational downgrading.

Chapter 3 - Immigration and Trust

4.1 Introduction

The recent increase in global refugee flows has brought issues of xenophobia and discrimination into the spotlight for researchers and policy-makers, in part because they present major hurdles to refugees' ability to economically integrate and promote economic growth in the host country. Related and less rigorously studied is the relationship between immigration and trust, which we define as the optimistic expectation or belief regarding other peoples' behavior, and which has long been considered by social and political scientists as essential to a functioning economy and society.[1] In this paper, we address two key open questions in this discussion: Does mass immigration affect trust towards the incoming group and other natives? If so, what drives this effect, and how does it depend on regional characteristics such as economic conditions, public goods provision or crime?

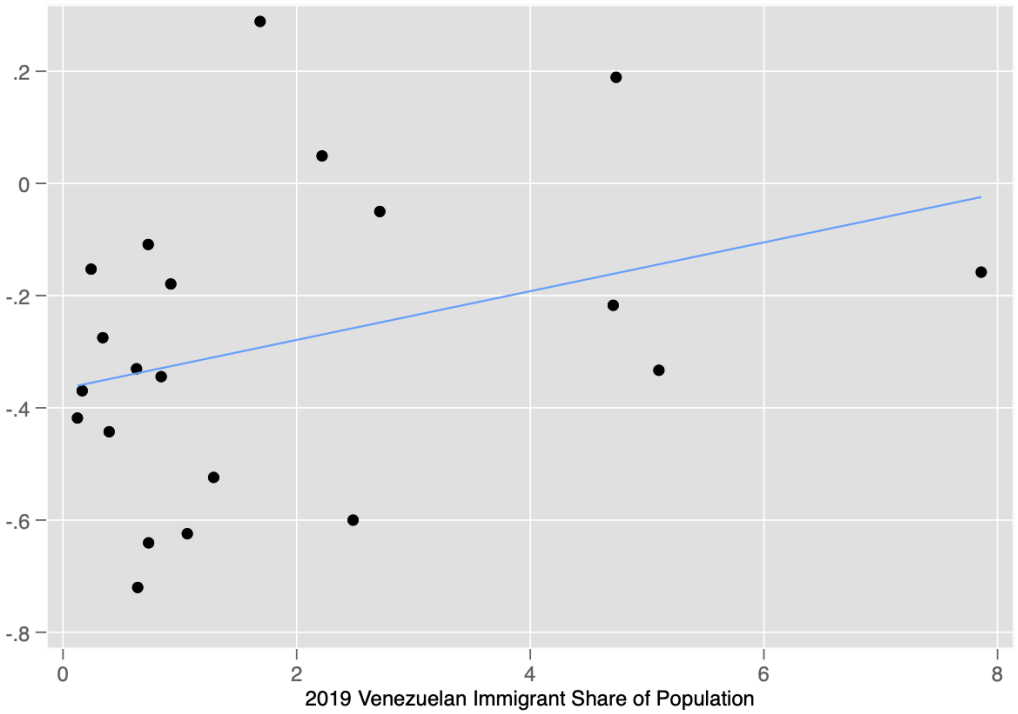
The answer depends on the relative importance of various mechanisms driving the relationship between immigration and trust, all which have been discussed heavily in the social sciences literature. One hypothesis, based on evolutionary psychology, is the tribalism hypothesis, which posits that human beings are quick to identify an "in-group", with which they cooperate more often, and an "out-group", with which they do not (Tajfel, 1982; Greene, 2013). According to this theory, so long

[1]Classic references in this literature are Banfield (1967); Arrow (1972); Fukuyama (1995); Knack & Keefer (1997). A growing body of work shows that trust has a causal impact on financial development, entrepreneurship, economic exchanges and other economic outcomes (Guiso *et al.* , 2004, 2006, 2009), is deeply related to functioning institutions (Algan & Cahuc, 2009; Aghion *et al.* , 2010) and may explain part of the difference in historical growth rates across countries (Tabellini, 2010; Algan & Cahuc, 2010).

as the population bases their identity according to the native-migrant division, the presence of incoming migrants will decrease trust in foreigners and increase trust in other natives. A second hypothesis, the contact hypothesis, predicts that intergroup contact can, under the right conditions, reduce prejudice and improve cooperation between groups. Finally, immigration can heighten concerns about labor market competition, scarcity of public goods, or crime. This can directly decrease trust towards foreigners or make it more difficult for the necessary conditions for the contact hypothesis to emerge. We shed light on these hypotheses in the early stages of a major migration episode: that of Venezuelans migrating to Colombia.

In Colombia, the sudden and unprecedented surge of migration from Venezuela has stimulated an outburst of anti-immigrant sentiment, visible in political dialogue, protest, negative media portrayal and in some cases violence and death threats against Venezuelan immigrants. Survey evidence confirms that Colombians' trust towards foreigners has decreased over this period: comparing responses to the nationally representative and identically-worded 2012 World Values Survey (WVS) and 2019 Survey of Political Culture (or Encuesta de Cultura Política - from now on ECP), trust towards foreigners on a scale from 1-5 decreased by around 1 SD between 2012 and 2019. Moreover, Figure 4.1 demonstrates that the largest decreases in trust towards foreigners occurred in the departments that received *fewer* migrants. Of course, this pattern could be explained by selection of migrants into locations that were more receptive to immigrants. One of the primary contributions of this paper is to identify the average *causal* impact of local Venezuelan in-migration on trust.

Figure 4.1: Department-level Change in Trust in Foreigners Index, 2012-2019



Calculated from the 2012 WVS and 2019 ECP using survey weights at the department-level. Index ranges from 1-5. See Data Appendix for specific questions.

To evaluate the effect of migration on trust, we added a module on social capital with questions related to trust and attitudes towards foreigners to the 2019 wave of the ECP. The large sample and granular geographic variation of the survey allow us to explore the effect of municipality-level Venezuelan inflows on reported levels of trust both towards foreigners and towards other native Colombians. We address concerns about endogenous sorting of migrants into municipalities using an instrumental variable: the driving distance between the municipality capital and the Venezuelan border, which is a strong predictor of the arrival of Venezuelan immigrants.[2] Our

[2]Unlike in Chapters 1 and 2, driving distance is now used as an instrument rather than historical migration because it is a much stronger predictor of Venezuelan migration in the subset of municipalities included in the ECP. Given that historical presence of migrants is very likely to directly

results can be summarized by two key findings. First, we find no causal effect of immigration on trust on average: While OLS results show a positive relationship between immigration and trust towards both groups, this relationship disappears in our 2SLS estimates. Second, we find that this average null effect masks important municipality-level heterogeneity: in municipalities that are more urbanized, have better public goods provision, and where migrants and natives are less residentially segregated, the causal impact of immigration on trust towards foreigners is in fact positive. These results are mirrored, though less precisely estimated, for a different measure of attitudes towards foreigners: preferences to not have foreigners as neighbors.

Various elements of this migration wave make it particularly suited for this empirical analysis. First, this migration wave was large, sudden and unprecedented. As discussed in Chapter 1, between 2015-2019 Venezuelans rapidly increased from 0.2% to 4% of the population. Prior to 2015, the foreign-born share of the population was consistently smaller than 0.2%, making it unlikely that any changes induced by the recent migration spell are confounded by previous exposure to immigrants. Second, this migration wave has spread across vast regions of the country that were previously unexposed, creating the geographic variation that we exploit in our analysis: in 2014, most Venezuelans were concentrated in the departments along the border, and in 2019 they made up more than 1% of the population in most departments and over 2% of the population in more than half of departments. Third, the forces driving the migration were unanticipated and unrelated to the economic and social conditions of

affect present attitudes towards migrants, driving distance may also be more likely to satisfy the exclusion restriction when trust is the outcome.

the host country. Thus, the timing of the Venezuelan migration is largely exogenous, leaving only sorting into locations within Colombia as an important endogeneity concern.

The essential identification assumption is that driving distance to Venezuela is not correlated with trust via any mechanism unrelated to the migration. Due to the lack of trust outcomes at the municipality level before the inclusion of our ECP module in 2019, we are unable to fully control for pre-period values through municipality fixed effects. We therefore devote considerable effort to defending the exclusion assumption of our instrument by presenting five pieces of evidence. First, we use the 2012 WVS to show that proximity to the border is uncorrelated with pre-period trust in foreigners at the department level. This geographic unit is large and may mask finer geographic correlation between pre-period trust and border proximity. However, if this endogeneity concern were relevant one would at least expect to see a positive correlation, and we find that this relationship is very precisely zero. We further show that our main results are replicated when we look at changes over time at the department level. Second, given that our primary analysis is at the municipality level, we show that there is little correlation between proximity to the border and an extensive collection of pre-period municipality socioeconomic characteristics, which we also include as controls in our analysis. We also control for a set of pre-period municipality-level variables correlated with trust in foreigners observed in previous rounds of the ECP, such as trust in state actors, political participation and community involvement, and use them to construct a placebo test to confirm that our model doesn't predict changes in these variables before the onset of the migration.

There may also be a concern that municipalities very close to the border have a

long history of migratory flows and cultural exchange with Venezuela, which could make them more trusting of Venezuelans and generate an upward bias in our estimates. In our third piece of evidence, we confirm that results are robust to dropping municipalities close to the border. We also include fixed effects for the geographic regions of Colombia to control for regional variation in social preferences. Fourth, though there was relatively little immigration pre-2015, the historical Venezuelan share of the population across municipalities may be an important omitted variable, and we corroborate that our results are robust to controlling for the share of Venezuelans in 2005 as well. Finally, we verify that our results are not driven by a native internal migration response by restricting our sample to respondents who were living in the same municipality five years prior.

Our heterogeneity results are most consistent with the contact hypothesis, which according to Allport *et al.* (1954)'s conditions is only operational when there is equal status, shared common goals, intergroup cooperation, and acknowledgement of authority.[3] We show that the effect of immigration on foreign trust is more positive in more urbanized municipalities. This may be because interaction with foreigners is more common in these places, since population density is higher and according to the ECP natives in these municipalities are almost twice as likely to have an immigrant in their social network (conditional on the foreign population share). An alternative possibility is that more urbanized municipalities have historically had stronger state presence as a form of authority that maintains rule-of-law and arbitrates conflict resolution, which is one of Allport's conditions for contact to reduce prejudice. We

[3]There is debate as to how and when each of these requirements are necessary for contact to improve intergroup cooperation. As long as some combination of them are met, contact appears to be a powerful force for reducing discrimination (Pettigrew & Tropp, 2000; Bertrand & Duflo, 2017).

also use census microdata to compute an "interaction index" measuring the extent to which natives and foreigners live in the same neighborhoods, and we find that the trust effect is larger in less segregated urban areas. This is direct evidence that, in areas where contact is more likely, trust increases in response to immigration.

The effect of migration on trust in foreigners is also increasing in quality of public goods, measured using high school test scores and infant mortality rates. Availability of health and education resources have been at the forefront of the Venezuelan immigration debate, and concern over access to these resources may drive anti-immigrant sentiment and restrain the ability for the contact hypothesis to operate.[4]

This evidence is somewhat suggestive, considering that urbanization, public goods provision, and residential integration are correlated with each other and possibly with other unobserved variables. However, we can divide the sample in other ways to provide evidence against other competing theories, such as economic competition and fears about crime. Competition over jobs could erode trust between groups by increasing the economic penalties of the trusting party being taken advantage of. While there is evidence that labor market competition from Venezuelans negatively impacted low-income workers in the informal sector, we find no evidence that the effect of local exposure to immigration makes these workers less trusting of foreigners - the effect of immigration on trust is no different for individuals who are predicted to work in industries where Venezuelans are concentrated or for unemployed workers, and only slightly more negative for lower-income workers. Regarding crime, per-

[4]Venezuelans have the right to access basic healthcare and to attend public school in Colombia. The public healthcare system is available to all regardless of immigration status, and regularized immigrants can join the public insurance system for access to additional services. In August of 2019, there were around 200,000 migrant students enrolled in schools across Colombia (Migration Policy Institute, 2020).

ceptions of criminal behavior by Venezuelans may directly affect beliefs about their trustworthiness, despite the fact that there is little evidence that Venezuelans actually increased crime in Colombia (Knight & Tribin, 2020; Bahar *et al.* , 2020b; Mora, 2020). However, we find little evidence that the trust effect depends on crime rates or changes in crime rates over the migration period. Of course, we cannot rule out that economic competition and concern over crime are related to national decreases in trust in foreigners. We show here that they do not drive decreases in trust due to local immigration. Lastly, we fail to find evidence for the *tribalism* hypothesis. Although we find some evidence that the migration increased trust in other natives, we do not observe a simultaneous decrease in trust in foreigners.

Literature: Our paper joins the small number of existing studies of the causal effect of immigration on attitudes towards foreigners outside of a randomized setting, thus picking up the aggregate effects of naturally-occurring interactions between natives and foreigners. Bursztyn *et al.* (2021) finds that decades-long exposure to Arab-Muslims in the U.S. decreased implicit prejudice against Muslims and increased interaction between non-Muslim Whites and Arab-Muslims, pointing to the contact hypothesis as a potential mechanism. Bazzi *et al.* (2019) finds that the resettlement of ethnically diverse Indonesians in the 1980s led to long-run increases in integration and national identity in fractionalized communities (with many small groups) and decreases in these outcomes in polarized communities (with a few large groups). Bazzi *et al.* (2019) includes, to our awareness, the only existing causal estimate of the effect of mass migration on trust, and finds that diversity decreases trust towards neighbors in polarized but not fractionalized communities. However, these estimates

are imprecise (possibly because they are based on a limited sample for which this data was available) and do not measure trust specifically towards the out-group.

Our paper differs from these two papers in three important ways. First, our paper shows that changes in attitudes can occur in the short term. While these papers point towards potential long-run increases in positive attitudes and integration resulting from non-polarizing increases in migration, we find effects on attitudes just after a few years of integration. There is evidence that intensive close intergroup contact can improve trust over a short period of only a year or less, for example, after random assignment in military barracks (Finseraas *et al.* , 2019) or in college dorm rooms (Boisjoly *et al.* , 2006). To our awareness, this has not been documented in the context of a real-world migration wave, where the frequency and scale of intergroup interaction is much smaller than in a contrived and randomized setting. Second, Colombians and Venezuelans are very similar ethnically, linguistically and culturally; both populations are majority Catholic, Spanish-speaking and with a similar genetic composition.[5] By studying culturally similar groups, we can remove "cultural threat" from the set of plausible mechanisms altering the trusting behavior of natives. Having a common language and cultural background may also increase the propensity for the contact hypothesis to operate.[6] This characteristic is relevant for external validity, considering that almost 75% of the world's refugees are estimated to reside in countries neighboring the country of origin, many of which share a common

[5]Autosomal DNA in Colombia is 62.50% European, 27.4% Native American and 9.2% African; in Venezuela it is 60.6% European, 23% Native American and 16.3% African. Colombia is 79% Catholic and 13% Protestant; Venezuela is 71% Catholic and 17% Protestant (Pew).

[6]Consistent with this idea, Tabellini (2020) finds that the hostile political reaction to immigrants in the United States was historically lower for immigrant groups who were culturally similar to natives.

language and cultural characteristics (UNHCR, 2019). Finally, we specifically study trust towards foreigners and can differentiate it from effects on limited and generalized trust. Generalized trust (towards neighbors, coworkers, and acquaintances) is very distinct from trust in foreigners, and distinguishing them allows us to study attitudes specifically towards the out-group and to look for evidence of tribalism.

This paper also relates to a large literature across the social sciences studying the causal relationship between contact and prejudice.[7] Paluck *et al.* (2019) provides a recent review of the lab experiment literature and emphasizes the lack of evidence on the effects of inter-group contact beyond more than a single day after treatment. Other papers exploit natural experiments such as random assignment to dorm rooms, classrooms, peer groups, military units or sports teams.[8] These papers generally find that inter-group exposure cause greater willingness to interact, less implicit prejudices and discrimination, and more egalitarian attitudes and prosocial behavior. Many also find that, consistent with Allport's hypothesis, context matters; for example, Lowe (2020) finds that cross-caste attitudes among Indian men improved when they were placed on the same cricket team but not on opposing teams, and Carrell *et al.* (2019) finds that white male freshman in the U.S. Air Force Academy randomly assigned to diverse peer groups were more likely to choose a black roommate in their sophomore year, but only if they were exposed to black students from the middle or the top of

[7]There is an extensive social sciences literature on the *correlational* relationship between immigration and trust. Some studies have found that immigration and diversity are associated with lower trust and cooperative behavior (Knack & Keefer, 1997; Alesina & La Ferrara, 2002; Costa & Kahn, 2003; Putnam, 2007; Dincer, 2011; Dinesen & Sønderskov, 2012; Ziller, 2015; Laurence & Bentley, 2016), while others have found no effect, a positive effect, or that the relationship disappears when appropriate economic variables are controlled for (Letki, 2008; Gesthuizen *et al.* , 2009; Hooghe *et al.* , 2009; Kesler & Bloemraad, 2010; Stichnoth & Van der Straeten, 2013a; Van der Meer *et al.* , 2014; Pottie-Sherman & Wilkes, 2017).

[8]See Dahl *et al.* (2021); Lowe (2020); Mousa (2020); Corno *et al.* (2019); Finseraas *et al.* (2019); Rao (2019); Carrell *et al.* (2019); Scacco & Warren (2018); Boisjoly *et al.* (2006).

the high school performance distribution.

Finally, our findings complement a large literature studying the effect of immigration on political preferences and voting for far-right parties. Many studies have found that local exposure to immigration reduces support for redistribution, public goods expenditure, and immigration-friendly border policies (Alesina *et al.* , 2019; Hangartner *et al.* , 2019; Choi *et al.* , 2019; Enos, 2014). However, there is mounting evidence that this effect is highly context-specific and in some cases positive, consistent with the notion that contact is only beneficial under the appropriate conditions. For example, Dustmann *et al.* (2019) finds that refugees decrease support for anti-immigrant parties specifically in urban areas in Denmark, and Steinmayr (2020) finds a similar effect in Austria in areas where refugees and natives have regular interaction with each other. Other papers have found that increases in refugee allocation decreased far-right support in France (Vertier & Viskanic, 2019; Schneider-Strawczynski, 2020), Italy (Gamalerio *et al.* , 2020), and Finland (Lonsky, 2020), and in all cases the authors find evidence to support the contact hypothesis driving their results.[9] We contribute to this literature by presenting a likely underlying mechanism for their findings: contact with migrants can improve trusting attitudes and cooperative behavior under the right set of circumstances, which translates into decreased support for anti-migrant platforms. Finally, in Colombia, Rozo & Vargas (2021) finds that Venezuelan immigration increased political participation and voting for the right-wing party. However, conservative parties in Colombia have not taken a clear anti-immigrant stance as in many other countries. The authors explain

[9]Other examples include Mayda *et al.* (2020), who find that the effect of low-skill immigration on right-wing voting in the U.S. is stronger in rural and and resource-scarce counties, and Alesina *et al.* (2019), who find that migrant-native residential segregation is associated with lower preferences for redistribution.

that their findings likely have less to do with anti-immigrant sentiment and more to do with anti-socialism, which has largely been viewed as the source of economic decline in Venezuela and contrasts starkly with the market-friendly approach of modern Colombian politics. Thus, while extremely interesting, their paper has less to say about the effects of immigration on attitudes towards foreigners.

The remainder of the paper proceeds as follows: Section 4.2 presents a short background on the history of Venezuelan-Colombian relations. Section 4.3 reviews the data. Section 4.4 reviews the empirical framework and identification. Section 4.5 presents the primary results. Section 4.6 presents the heterogeneity results. Section 4.7 presents the robustness checks. Section 4.8 concludes.

4.2 Background on Colombian-Venezuelan Relations

Since the dissolving of Gran Colombia in the mid-19th century and Colombia's and Venezuela's subsequent consolidations as independent states, the two countries have had close relations, characterized by regular trade exchanges and migratory flows. Importantly, before 2010 migration flows were largely in the opposite direction, from Colombia to Venezuela. Over the course of the decades-long civil war in Colombia, millions of Colombians are believed to have fled to Venezuela, where they benefited from relatively low poverty rates and generous social programs funded by high oil revenues. Now, as the migration flow has reversed drastically, Colombia's immigration policy and rhetoric has been widely perceived as more accepting relative to other countries receiving Venezuelans such as Ecuador, Peru and Brazil, in part because of

this historic relationship.[10]

However, there are indications that the initial positive reception by Colombians is changing. There is concern about availability of resources in the communities that have been most impacted, especially regarding healthcare and education systems. Colombia's unemployment rate hovers around 10% and the informal sector is large, and as discussed in Chapter 1, there is evidence that competition with Venezuelan workers has reduced wages for low-skill and informal workers. Large protests in Bogotá in 2019 demanded, among many things, stricter border controls with Venezuela, and there has been an increase in threats and violent attacks against Venezuelans across the country (Colombia Reports, 2018; Reuters, 2019). This adds important context to our study because it implies that Colombia was in a state of low anti-Venezuelan sentiment before 2015, and since then anti-Venezuelan sentiment has visibly increased.

4.3 Data

Data on social preferences come from the Survey of Political Culture (ECP) which is collected by DANE. The ECP is a nationally-representative survey with the objective of characterizing key aspects of Colombian political culture, perceptions and participation. The most recent wave of the ECP was completed in July and August of 2019, generating a total of 41,850 people over age 18 in our sample, in 15,392 urban households and 3,974 rural households across 118 municipalities.

[10]For example, while Colombian President Iván Duque has repeatedly referred to Venezuelans as "our Venezuelan brothers" and announced before the UN in 2018 that the two countries were "united by fraternity", Brazilian President Jair Bolsonaro once referred to Venezuelan migrants as "the scum of the earth" (Financial Times, 2019).

In the 2019, ECP we included a module on social capital with questions related to trust and attitudes towards foreigners. While the ECP trust question is similar to one included in the WVS and various other social preferences surveys globally, it has the important advantage of distinguishing between trust in different groups. Specifically, the survey asks respondents to rate, on a scale of 1 to 5, how much they trust various different groups (family, co-workers, strangers, neighbors, friends and foreigners). The survey also asks "who would you not like to have as a neighbor" and lists people of another nationality as one of the options, which we refer to for the remainder of the paper as "preferences for segregation". To facilitate comparison across outcomes, each outcome is centered around the mean and divided by the standard deviation.[11]

Table 4.1 displays summary statistics for our main social preference outcomes before they are normalized into Z-scores. As we would expect, trust is highest for family, lower for friends and co-workers, again lower for neighbors, and is lowest for strangers and foreigners. Regarding preferences for segregation, 6.5% of respondents report not wanting to live near a foreigner. These measures are all positively correlated (and negatively correlated with preferences for segregation). Throughout our analysis, we reduce the number of outcomes by using "limited trust" as the standardized average of trust in family and friends, and "generalized trust" as the standardized average of trust in co-workers, neighbors and strangers.

[11]These questions refer to foreigners in general, and not specifically Venezuelans. Because almost 95% of foreigners in Colombia in 2019 were Venezuelan, we believe that respondents have Venezuelans in mind when responding.

Table 4.1: Social Preferences Summary Statistics

	Mean	**(SD)**	**(Min)**	**(Max)**
Trust in:				
Family	4.8	(0.6)	1	5
Friends	3.2	(1.1)	1	5
Co-workers	3.1	(1.2)	1	5
Neighbors	2.7	(1.1)	1	5
Strangers	1.6	(1.0)	1	5
Foreigners	1.7	(1.0)	1	5
Not want as a neighbor:				
Foreigner	.065	(.246)	0	1

Means presented. Standard errors in parentheses. Sample includes 41,850 observations. See Data Appendix for questions as written in the survey. Source: 2019 ECP

As can be seen in Figure A13, this measure of trust in foreigners is strongly positively related to other ECP measures of political and community participation and trust in the state. While these relationships are not causal, they are consistent with a large literature showing that trust is deeply related to social involvement and faith in government. In Table 4.2, we see that trust in foreigners is higher for men and more educated people.[12] It is higher in urban areas until demographic characteristics are controlled for, and higher for older people conditional on gender and education. There are also important regional differences in trust, which is highest in the Caribbean and lowest in the Central and Pacific, that are held fixed in our analysis with region fixed effects.

[12]These correlations are also consistent with those of the 2012 WVS in Colombia.

Table 4.2: Correlates of Trust in Foreigners

	Trust Foreigner					
Female	-0.05***					-0.06***
	(0.01)					(0.01)
Age		-0.000				0.003***
		(0.000)				(0.000)
Yrs Ed			0.02***			0.03***
			(0.00)			(0.00)
Urban				0.16***		0.02
				(0.01)		(0.01)
Region:Caribe					(excluded)	(excluded)
Region:Oriental					-0.08***	0.00
					(0.01)	(0.00)
Region:Central					-0.04***	-0.11***
					(0.02)	(0.01)
Region:Pacifica					-0.29***	-0.03**
					(0.01)	(0.02)
Observations	41,770	41,770	41,770	41,770	41,770	41,770
R-Squared	0.001	0.000	0.016	0.004	0.014	0.031

Trust outcomes are standardized into Z-scores. Municipality-clustered standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

To measure migration, we calculate the municipality-level share of the population that is a Venezuelan immigrants using the 2018 full population census. Using the census allows us to conduct our analysis using variation at the municipality level. In our primary specification, we define a migrant as anyone who was born in a foreign country who was not living in Colombia five years prior. One limitation of the census is that it does not distinguish country of origin. However, 95% of all immigrants who arrived in the five years proceeding 2018 were Venezuelan, giving us confidence that our Venezuelan immigrant shares are close to the shares of all foreigners.[13]

[13]Data from the GEIH. As a robustness check, we replace our estimate of the migration share with

Finally, we use the 10% sub-sample of the 2005 census, which is the most recent census before 2018, to control for the Venezuelan-born share of the municipality in 2005 (IPUMS, 2019). In the next Section we discuss the various additional datasets used as inputs for the construction of municipality-level controls.

4.4 Empirical Strategy

For all non-migrant ECP respondents over age 18, we run the following regression of outcomes for individual i in municipality m on the migrant share of the population (M_m)[14]:

$$Y_{im} = \beta M_m + X_i\gamma + Z_m\rho + \delta_r + \epsilon_{im} \tag{4.1}$$

We include individual-level controls (X_i) for age (including separate indicators for age groups in 4-year intervals), gender, level of completed education and urban-rural status. Results are presented with and without these controls, as well as a long list of municipality-level controls (Z_m) measured before the start of the migration. These include the log-2013 population, the driving distance from the municipality capital to Bogotá, the share who have completed secondary school, 2015 per-capita value added (DANE), a measure of severity of conflict exposure between 2000-2012 calculated by the Conflict Analysis Resource Center (CERAC), and the following list of pre-period covariates publicly provided by the Center for the Study of Economic Development at

an estimate provided to us by Migración Colombia, based on an imputation formula combining information from the census and registrations for legal status. The correlation between this variable and our census-based variable is .8, and results are indifferent to which variable is used. These results are available upon request.

[14]The migrant share of the population is calculated as the number of foreign-born who entered Colombia in the 5-years proceeding 2018 divided by the 2013 municipality population.

the Universidad de Los Andes (CEDE): share urban, infant mortality rate, secondary school test scores, electricity coverage, trash collection coverage, homicide rate and crime rate.[15] We also include fixed effects (δ_r) for the four regions of Colombia as designated by DANE: Pacific, Caribbean, Central and Eastern (since we do not have the power to include department fixed effects, which substantially increase the standard errors). Our identification is therefore driven by differences across municipalities within the same region of Colombia. Given that our main outcome is an average of a discrete ranking, we confirm that results are not sensitive to using an ordered logit model instead. Standard errors are clustered at the municipality level.

We can take a causal interpretation of β if M_m is independent of ϵ_{im}. This would be violated if immigrants choose where to locate based on unobserved characteristics that are correlated with social preferences. For example, if immigrants choose to settle in municipalities that have greater levels of trust towards foreigners, then this would generate an upward bias in our estimates. Alternatively, if immigrants choose to locate in areas where jobs are more available, and these areas also have higher levels of trust after controlling for municipality characteristics, then this would again cause an upward bias in β. By instrumenting for M_m, we eliminate this bias by isolating variation in M_m that is uncorrelated with ϵ_{im}.

The instrument that we use is the log of the minimum driving distance from the municipality capital to the closest of the four entry points along the Colombian-

[15] Whenever possible, we take the average across 2014-2016 to minimize measurement error. In a few cases, missing values are imputed using non-missing variables.

Venezuelan border.[16][17] These four entry points are depicted in Figure 4.2, along with the 118 municipalities included in the 2019 ECP sample.[18] We find that the minimum driving distance to these entry points, which is identical to the minimum driving distance to any roads that cross the Venezuelan border, is a strong predictor of the number of immigrants living in a municipality. In particular, a 100% increase in the driving distance to the border is associated with a 1.075 percentage point decrease in the foreign share of the population, controlling for total population and region fixed effects (presented below results in Table 4.3). The Kleibergen-Paap first stage F-statistics is 51 and increases to 139 after individual and municipality controls are included. The relevance of travel distance is intuitive considering that many Venezuelans have entered Colombia travelling on foot or via public transportation.

[16]Driving distance is calculated using Open Street Maps algorithms in QGIS3.8. For 3 municipalities, the capital was isolated such that no driving distances could be calculated. In this case, the nearest city with a driving distance was used, and results are robust to dropping these municipalities.

[17]Results do not depend on whether or not we use the minimum driving distance to the closest entry point or the average driving distance to all four points. They are also invariant to whether or not we use driving distance or driving time.

[18]More than 99% of the Venezuelans who legally crossed the Colombian border in 2018 entered through one of these checkpoints, and legal entries represented 72% of the Venezuelans in Colombia according to migration authority estimates in March 2019 (though some of them subsequently overstayed) (Migración Colombia, 2019). Many illegal entries are via 'trochas", or paths that simply pass around legal entry points.

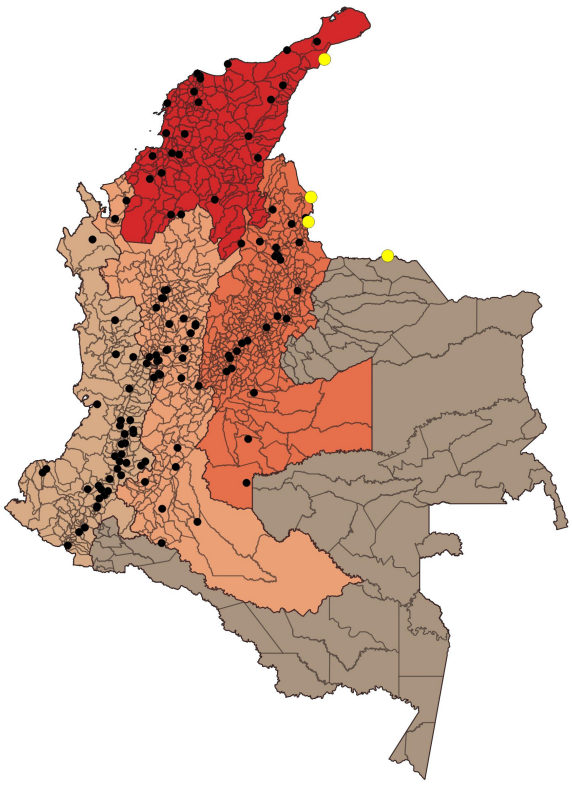


Figure 4.2: Municipalities of the ECP

Capitals of the 118 municipalities in the ECP sample represented by black dots. Border crossing points represented by yellow dots. Regions used in analysis shaded in different colors.

In order for the instrument to satisfy the exclusion restriction, driving distance to the Venezuelan border must only affect trust through the channel of increased immigration from Venezuela. There are various potential threats to this assumption's validity that we attempt to address. To start, we re-emphasize a point made in the introduction: that this is a refugee wave driven by push factors rather than by pull factors. The timing of this migration wave is therefore unlikely to be related to any economic changes occurring in Colombia. As noted, the majority of Venezuelans in Colombia report violence and poverty as the primary reasons for leaving Venezuela,

and Colombia's unemployment rate was almost 9% in 2015. Various concerns remain about the geographic sorting of migrants that our instrument may fail to solve, and we attempt to address these in our analysis.

First and foremost, one may be concerned that baseline levels of trust in Venezuelans are positively correlated with proximity to the border, which would generate an upward bias in our 2SLS estimates. To our knowledge, no surveys of trust in foreigners before 2019 exist to enable a pre-period balance check at the municipality level. However, the 2012 WVS asks a trust question identical to the ECP, allowing us to conduct this balance check at the department level. Figure A14 shows that in 2012 there was no relationship between driving distance from the department capital to the Venezuelan border and trust in foreigners across Colombia. A 500 km increase in distance from the border, which covers more than a third of the driving distance across the country, is associated with a .02 SD decrease in trust in 2012. In Table A21, we run this regression with and without region controls. To ensure that these results are not sensitive to the starting point within the department, we also use driving distance from the department centroid and from the population-weighted centroid of ECP municipalities in the department. In each case, the coefficient remains very close to zero and statistically insignificant. It is still possible that, within department, proximity to the border is correlated with baseline foreigner trust. But if this concern were relevant, one would at least expect to see some evidence of a relation at the department level, which we fail to find evidence for. We can also use this data to estimate our main model using *changes* in trust at the department level, using the change between the 2012 WVS and 2019 ECP as we did in Figure 4.1, and weighting each observation by the department population. We see in column

1 of Table A20 that a 1pp increase in the foreign share predicts a .27 SD increase in the change in trust in foreigners, consistent with the correlation shown in Figure 4.1. When we instrument the foreign share with the distance measure, the coefficient becomes substantially smaller and insignificant. This demonstrates that our main results are replicated at a larger geographic unit when looking at changes over time rather than cross-sectional variation.

Second, we check for balance using a long list of pre-period socioeconomic variables at the municipality level, which is the finer geographic level we use in our analysis. In particular, one may be concerned that areas closer to the border are wealthier because they benefited from trade with Venezuela.[19] To investigate this, we regress all of our pre-period municipality covariates on the log-distance instrument, to see if driving proximity to the border is predictive of observable outcomes before the migration wave. These results are presented in Table A22. The only significant result is that a 100% increase in driving distance is associated with a 15.5% increase in per-capita value added, and this is no longer significant once we account for multiple hypotheses testing using sharpened false discovery rate q-values (Anderson, 2008). There is no statistically significant relationship with other pre-period municipality characteristics, including urbanization, education rates and secondary test scores, infant mortality, coverage of electricity and waste collection, or crime and historical violence exposure. Importantly, considering the sample size of 118 municipalities, these coefficients are also small in magnitude - the largest coefficient indicates that a 100% increase in distance is associated with a .16 SD decrease in the urban share of the population.

[19]While Venezuela used to be a top trading partner of Colombia, its trade shares steadily declined during the 2000s such that it represented around 3.6% of exports and less than .1% of imports in 2010 (COMTRADE). However, proximity to the border may have historically benefited economic growth.

We conclude that, controlling for population and region fixed effects, municipalities in our sample that are closer to the border are moderately less productive in terms of per-capita output, but not noticeably different in terms of other socioeconomic outcomes or violence. We also show in Table A23 that distance to the border is not significantly related to any individual-level demographic characteristics or urban-rural status in 2019 (the significant coefficient on female is very small in magnitude and is not robust to the adjustment for multiple hypothesis testing). We control for all of these variables in our analysis and this does not significantly impact the results. The remainder of our robustness tests are conducted in Section 4.7.

4.5 Results

Table 4.3 displays the main results for each trust measure. In an OLS regressions that controls only for the log-population, there is a significant and positive correlation between migrant population and trust towards foreigners (column 1). This is robust to including region fixed effects as well as all of our individual and municipality controls. Column 3 indicates that, within regions and conditional on observables, a 1 percentage point increase in the migrant share of the population is associated with a statistically significant .051 SD increase in trust towards foreigners. Across the 10th-90th percentile of municipalities in the sample, the migrant share of the population increases from 0.04% to 2.72%, associated with a .14 SD increase in trust towards foreigners. For context, this is similar to the raw difference in trust towards foreigners reported in urban versus rural areas.

Table 4.3: Impact of Immigration on Trust and Preferences for Segregation

	(1) OLS	(2) OLS	(3) OLS	(4) 2SLS	(5) 2SLS
	Trust Foreigner				
Foreign Share	0.058**	0.043**	0.051**	0.004	0.003
	(0.022)	(0.022)	(0.023)	(0.019)	(0.023)
Hausman Test P-val:				0.065*	0.008***
Reduced-form Coef.:				-0.005	-0.003
				(0.021)	(0.026)
	Trust Generalized				
Foreign Share	0.063**	0.028	0.025	-0.004	-0.029
	(0.028)	(0.024)	(0.030)	(0.020)	(0.031)
Hausman Test P-val:				0.238	0.027**
Reduced-form Coef.:				0.004	0.032
				(0.022)	(0.034)
	Trust Limited				
Foreign Share	0.050**	0.009	-0.014	0.009	-0.014
	(0.020)	(0.018)	(0.020)	(0.021)	(0.029)
Hausman Test P-val:				0.989	0.986
Reduced-form Coef.:				-0.010	0.016
				(0.023)	(0.032)
	Not Want Nbr Immigrant				
Foreign Share	-0.718**	-0.217	0.624	-0.573	0.145
	(0.303)	(0.371)	(0.438)	(0.596)	(0.566)
Hausman Test P-val:				0.474	0.331
Reduced-form Coef.:				0.616	-0.162
				(0.670)	(0.633)
Region FE		X	X	X	X
Ind/Mun Controls			X		X
Observations	41,850	41,850	41,850	41,850	41,850
Kleibergen-Paap F				51.018	138.603
First-Stage Coef.				-1.075***	-1.116***
				(0.151)	(0.095)

All models include log-population. Trust outcomes are standardized into Z-scores. Municipality-clustered standard errors in parenthesis. See Data Appendix for full list of individual and municipality controls. P-value for Durbin-Wu-Hausman test robust to clustered standard errors is reported below each IV coefficient. * p<0.10, ** p<0.05, *** p<0.01.

The 2SLS model is applied in columns 4 and 5 of Table 4.3 (alongside the first stage and reduced form coefficients). The effect of immigration on trust becomes small and insignificant, and this is true both with and without municipality controls. This indicates that migrants sort into areas that are more trusting towards foreigners. This may be because foreigners directly sort into areas according to trust, or according to characteristics that are correlated with trust such as economic development. However, the fact that this sorting is robust to including the full set of municipality controls implies that migrants may choose destinations specifically according to natives' attitudes towards foreigners. In the full model with all controls, a Durbin-Wu-Hausman test rejects the exogeneity of the migrant share with 95% confidence, showing that the positive bias in the OLS model is statistically significant. While the coefficient in Column 4 is very close to zero, the 95% confidence interval ranges from -.033 to .041. Thus, at the lower bound of this confidence interval, an increase in the Venezuelan share across the 10th-90th percentile of municipalities causes a .088 SD decrease in trust towards foreigners, which is more than 10 times smaller than the national decrease in foreigner trust observed over this period.

We next turn to results for trust in other native Colombians, presented in rows 2 and 3. The unconditional OLS coefficient in column 1 is positive and significant for generalized trust, indicating that immigrants select into destinations that are more trusting towards other native Colombians as well as foreigners. It is also significant for limited trust, indicating that immigrants also select into areas that are more trusting towards family and friends. However, both of these correlations are only true across regions, as they become smaller and insignificant when region fixed effects are included in column 2, as well as when the individual and municipality controls

are included in column 3. Overall, there is moderate positive sorting according to generalized trust, where the Durbin-Wu-Hausman test is significant only in the model with municipality controls. There is no sorting according to limited trust, which can be thought of a as a placebo test because one would not expect this to respond in response to a migration wave. Again, the 2SLS estimates indicate no statistically significant change in trust towards any group in response to a large immigrant inflow.

Finally, row 4 shows the impact on preferences to not have an immigrant as a neighbor. The raw correlation in column 1 is negative, indicating that immigrants live in areas that are *more* willing to have an immigrant neighbor on average. Similar to the results for trust in foreigners, this relationship becomes insignificant when region fixed effects and other controls are added, and remains insignificant when 2SLS is used.

Because our outcome is a discrete ranking, as an additional check we replicate analysis with an ordered logit model, using a control function approach with clustered bootstrapped standard errors. These results are presented in Table A24. They are qualitatively similar to those presented in Table 4.3, showing a statistically significant increase in trust towards foreigners that decreases in magnitude and becomes insignificant when the instrument is applied. Results for the other primary outcomes also echo what we observe in our main analysis.

4.6 Heterogeneity

We have established that, while there is selection of migrants into locations that are more trusting, there is little evidence that Venezuelan inflows effect trust or segrega-

tion preferences on average. This may mask important heterogeneity in the response to immigration that could also shed light on mechanisms through which migration might effect trust. In Tables 4.4, 4.5 and 4.6, we fully interact the model (including the migration share, the instrument and all covariates) with a variety of municipality and individual characteristics along which the potential mechanisms predicts differential effects.[20] To account for the fact that we test multiple hypotheses in this section, we compute sharpened false discovery rate q-values for the interaction terms, which can be interpreted as the expected portion of rejections that are type-I errors (Anderson, 2008). These q-values are presented below each pair of coefficients.[21] In all cases, the first-stage F-statistic remains above the recommended critical value of 7.56 for a test with two endogenous regressors and two instruments (Stock & Yogo, 2002).

4.6.1 Socioeconomic Characteristics and Interaction

In column 1 of Table 4.4 we interact the model with the pre-period urbanization rate in the municipality, which was converted into a Z-score so that the "Foreign Share" intercept can be interpreted as the impact of migration on trust at the mean municipality. Here we see that, at the mean urbanization rate, the effect of migration on trust is significantly negative. However, the coefficient on the interaction term is significantly positive, with a multiple hypothesis adjusted q-value of .011. For a

[20] Results are similar when we split the sample over and under the median values rather than interact the model with a continuous variable.

[21] For each outcome, we consider that the model is being interacted with ten different variables, amounting to ten tests. We do not further adjust for the fact that we are looking at four outcomes, because we consider limited trust to be a placebo test, and the remaining outcomes we expect to be correlated with each other.

municipality 1 SD above the mean urbanization rate, a 1pp increase in migration increases trust in foreigners by .076 SDs, associated with a .2 SD increase in trust towards foreigners moving from the 10th to the 90th percentile of migration rates. This is larger than the difference in trust in foreigners between urban and rural areas, and comparable with the difference between a person with a college degree and with no education. As expected, these results are not replicated for limited trust, which we consider a placebo outcome. In general, the effect is in a similar direction for both generalized trust and segregation preferences, both in the interaction with urbanization rate and with other heterogeneity variables. However, these results are generally not statistically significant and smaller in magnitude, so we do not interpret them as reflecting a meaningful pattern. It is possible that exposure to foreigners could also increase generalized trust, but the results are not consistent enough to convincingly make this claim.

Table 4.4: Heterogeneity by Municipality Socioeconomic Characteristics

Heterogeneity By:	(1) Share Urban (SDs) (Pre-Period)	(2) Value Added Per-Capita (SDs) (Pre-Period)	(3) Public Goods Index (SDs) (Pre-Period)	(4) Interaction Index † (SDs) (2018 Census)
	Trust Foreigner			
Foreign Share	-0.139**	0.015	-0.017	-0.379**
	(0.062)	(0.022)	(0.038)	(0.174)
Foreign Share × Heterogeneity Variable	0.215***	0.066	0.110**	0.136**
	(0.063)	(0.056)	(0.052)	(0.053)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 0.011]	[q = 0.488]	[q = 0.103]	[q = 0.048]
	Trust Generalized			
Foreign Share	-0.084	0.009	-0.066	-0.251
	(0.066)	(0.022)	(0.042)	(0.158)
Foreign Share × Heterogeneity Variable	0.119*	0.140***	0.126**	0.050
	(0.063)	(0.050)	(0.062)	(0.048)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 0.142]	[q = 0.053]	[q = 0.133]	[q = 0.319]
	Trust Limited			
Foreign Share	0.015	0.006	-0.045*	-0.041
	(0.059)	(0.021)	(0.024)	(0.160)
Foreign Share × Heterogeneity Variable	-0.038	0.091*	0.012	-0.074
	(0.059)	(0.049)	(0.045)	(0.053)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 1.000]	[q = 0.396]	[q = 1.000]	[q = 0.803]
	Not Want Nbr Immigrant			
Foreign Share	0.542	0.309	0.913	-4.466
	(1.684)	(0.529)	(0.809)	(3.945)
Foreign Share × Heterogeneity Variable	-0.723	-1.208	-2.132*	-0.503
	(1.767)	(1.459)	(1.253)	(1.071)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 1.000]	[q = 1.000]	[q = 0.669]	[q = 1.000]
Ind/Mun Controls	X	X	X	X
Observations	41,850	41,850	41,850	30,116
Kleibergen-Paap F	10.429	20.367	13.525	16.799

All regressors fully interacted with heterogeneity variable. All of the heterogeneity variables are standardized into Z-scores, such that "Foreign Share" measures the treatment effect at the mean. See Data Appendix for description of heterogeneity variables and full list of controls. Municipality-clustered standard errors in parenthesis. * p<0.10, ** p<0.05, *** p<0.01.
† Sample restricted to urban areas

Why would the effect of immigration on trust be positive in more urbanized municipalities and negative in less urbanized municipalities? One possibility is that urbanized municipalities are simply wealthier and have higher quality public goods. Poverty or public good scarcity in less urbanized areas may inflate concerns about resource competition and increase tensions between natives and foreigners, reducing the propensity for cooperation and restricting the ability for positive contact to occur. In columns 2 and 3, we interact the model with pre-period per-capita value added and an index for public goods quality. To measure public goods, we standardize and average measures of secondary test scores and (negative) infant mortality rates, as measures of the quality of education and health resources respectively. Over-crowding of education and health resources have been at the forefront of the debate surrounding Venezuelans in Colombia. The results show that the effect of immigration on trust becomes more positive as wealth and the public goods index increases. However, the interaction terms is significant only for the public goods index, and has a q-value of .1 adjusted for multiple hypothesis testing.[22] As can be seen in the correlation matrix in Table A25, per-capita value added and the public goods index are both positively correlated with urbanization rate (.3 and .6 respectively), highlighting that these results may be driven by other characteristics of urban areas. Nonetheless, they are indicative that public goods provision in particular may be an important part of the story.

A second possibility is that in urbanized municipalities, contact between foreigners and natives is more likely to take place, which promotes increased cooperation via

[22]The results are similar when we calculate them individually for test scores and infant mortality rates. We prefer to group them into a single index to reduce the number of tests conducted in this analysis.

the contact hypothesis. Greater population density may increase the frequency with which foreigners and natives interact.[23] It is also possible that immigrants and natives in urbanized municipalities are more likely to live near each other. To investigate this, we construct an "interaction index" using the 2018 census at the level of the census block, which contains an average of 33 households and 101 individuals. This measure takes the following form (Lieberson, 1981):

$$Int_m = \sum_{i \epsilon m} \left(\frac{N_i}{N_m} \right) \left(\frac{F_i}{P_i} \right)$$

where N_i, F_i and P_i are the number of natives, foreigners and total residents respectively in census block i, and N_m is the number of natives in municipality m. This index can be interpreted as the average share of foreigners in each natives' block, which can be seen as a proxy for the average probability of interacting with a foreigner.[24][25] This analysis is restricted to urban areas because the census block identifier is only available for urban areas. The interaction index and urbanization rate have a correlation of .14, implying that urban areas in more urbanized munic-

[23] The ECP data also indicate that, in municipalities over the median urbanization rate, people are almost twice as likely to have foreigners in their close social network, and this persists conditional on the migrant share of the municipality. In this question, the network is defined to include anyone with whom they have lent or borrowed money, as well as anyone who they have helped find a job or who has helped them find a job. However, fewer than 2.5% indicate that they have a foreigner in their network, and over 30% report a network size of zero, implying that this question is not reliable for capturing information outside of a very close social circle.

[24] We get similar results when we instead compute the "dissimilarity index", which is an alternative measure common in the literature and has a .7 correlation with our interaction index measure.

[25] Variation in the size of census blocks and number of migrants can generate substantial segregation according to random allocation alone. We therefore generate the integration index relative to random allocation, by randomly assigning observations to census blocks within each municipality, holding their size constant. We then taking the average assignment from 500 repetitions and calculate the percent difference of the original measure from this benchmark (Carrington & Troske, 1997; Dustmann *et al.* , 2019). This generates similar results that are available upon request. We prefer our original measure as our primary specification, since what is most relevant for contact is the probability of interaction regardless of the probability under random allocation.

ipalities are slightly more residentially integrated, but that this measure generates substantial variation independent of urbanization.

The results in column 4 show that the interaction term with this measure is significant and positive. On the other hand, at the mean value of the interaction index, the effect is significantly negative. The results imply that the effect of immigration on trust is significantly more positive where natives and immigrants live closer to each other. This is the most direct evidence we have that the contact hypothesis is important for building trust between foreigners and natives in areas that have received more migrants.

Because the interaction index is measured in 2018, it is possible it was impacted by immigration, which could bias our interpretation of the results. However, note that an endogenous relationship between trust and the index would have to be increasing in the migrant share to drive the interaction term. For example, one might worry about reverse causality, in which Venezuelans choose to live near natives in places that are more trusting towards foreigners. However, our result is that the effect of migration on trust is increasing in residential integration, and therefore the reverse causality effect would have to be increasing in the migrant share to drive this interaction result. While still possible, this is less likely than a simple sorting of migrants into native neighborhoods in more trusting locations.

An alternative hypothesis is that individuals in urbanized municipalities are more educated, which may make them more open-minded to the arrival of immigrants or skeptical of claims that immigrants are increasing crime or economic competition. We find some evidence for this in the data. In Figures A15 and A16 we split the sample according to achieved levels of education at the individual level as measured in the

ECP, and display the coefficients and their confidence intervals from the full 2SLS model with controls, separately for trust in foreigners and preferences to not have an immigrant neighbor. The trust effect is substantially more negative specifically for those with no education, and there is no difference in impact across those with primary, secondary, and post-secondary education. The fact that those in more urbanized municipalities are less likely to have no education (4% compared to 9%) may partially drive to the result that migration decreases trust more so in urbanized municipalities, but is unlikely to explain a substantial fraction of this gap.[26]

4.6.2 Crime and Violence

Another mechanism to explore is concern over increasing crime and violence, which has been at the forefront of the Venezuelan migrant dialogue. While there is limited evidence that Venezuelans actually increased crime, many natives continue to associate Venezuelans with crime (Knight & Tribin, 2020; Bahar *et al.* , 2020b; Mora, 2020). The effect of migration on attitudes towards immigrants may be larger in municipalities with higher baseline crime rates, where crime is more of a concern. Municipalities with a history of exposure to the internal conflict in Colombia may have also become more distrusting towards outsiders, inflating their negative reaction to a newly arriving out-group. However, while it is interesting to study heterogeneity according to pre-migration crime and violence, the most direct characteristics we expect to be relevant are *changes* in crime and violence over the migration period. In municipalities where crime and violence increase, individuals may associate wors-

[26] We also split the sample according to two other time invariant individual characteristics, age and gender, and we see little difference across these groups. In general, the relevant variation in the impact of immigration on trust seems to be across municipalities rather than across individuals.

ening outcomes with the Venezuelan arrival, even if the migration was not in fact responsible for these changes.

These heterogeneity results are presented in Table 4.5. In column 1, we interact the model with pre-period crime rates, and we do not find any meaningful differences in the treatment effect across this dimension. In column 2, we do this for the *change* in crime rates between 2013 and 2017 (the most recent year for which municipality homicide data is available). We find that the effect of migration on foreigner trust is more negative in places that had larger increases in crime, but that this interaction term is not statistically significant. Finally, in column 5 we interact the model with a binary variable measuring whether the municipality experienced "strong and persistent violence" between 2000-2012 (CERAC), and as in column 1 we find no evidence that historical violence is relevant. Our primary conclusion is that baseline and historical experiences with crime and violence have no relationship with the impact of migration on trust, and that while people may associate increases in crime with migrants, this effect is not as relevant as urbanization or residential integration.

Table 4.5: Heterogeneity by Crime and Violence Characteristics

Heterogeneity By:	(1) Crime Rate (SDs) (Pre-period)	(2) Δ Crime Rate (SDs) (2014-2017)	(3) Heavy Violence (Binary) (2000-2012)
	Trust Foreigner		
Foreign Share	0.002	0.004	-0.031
	(0.028)	(0.031)	(0.034)
Foreign Share × Heterogeneity Variable	0.018	-0.080	0.060
	(0.040)	(0.059)	(0.061)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 0.880]	[q = 0.446]	[q = 0.558]
	Trust Generalized		
Foreign Share	-0.051*	-0.044	-0.083**
	(0.029)	(0.035)	(0.035)
Foreign Share × Heterogeneity Variable	0.046	-0.166**	0.064
	(0.047)	(0.071)	(0.054)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 0.319]	[q = 0.094]	[q = 0.319]
	Trust Limited		
Foreign Share	-0.052**	-0.015	-0.060*
	(0.023)	(0.023)	(0.031)
Foreign Share × Heterogeneity Variable	0.028	-0.127**	-0.041
	(0.034)	(0.051)	(0.068)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 1.000]	[q = 0.150]	[q = 1.000]
	Not Want Nbr Immigrant		
Foreign Share	0.590	0.089	0.626
	(0.739)	(0.626)	(0.969)
Foreign Share × Heterogeneity Variable	-0.152	1.935	-1.622
	(1.092)	(1.295)	(1.559)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 1.000]	[q = 0.681]	[q = 1.000]
Ind/Mun Controls	X	X	X
Observations	41,850	41,850	41,850
Kleibergen-Paap F	24.650	25.959	27.591

All regressors fully interacted with heterogeneity variable. All of the heterogeneity variables are standardized into Z-scores, such that "Foreign Share" measures the treatment effect at the mean. See Data Appendix for description of heterogeneity variables and full list of controls. Municipality-clustered standard errors in parenthesis. * p<0.10, ** p<0.05, *** p<0.01.
† Sample restricted to urban areas

4.6.3 Labor Market Competition

The last mechanism that we explore is labor market competition, which could also amplify the impact of local migration on trust. A variety of studies find that the negative wage effects of Venezuelan competition have been highly concentrated on low-income native workers in the sectors where Venezuelans are heavily concentrated, in particular, sales, services, construction, and manufacturing. While we do not observe industry in the ECP, we observe other individual characteristics, such as unemployment status or income, that indicate vulnerability to economic competition. We also use the GEIH to construct a measure of "industry exposure" at the department level, which is the finest geographic unit for which industry information is available, for workers of a specific demographic. We calculate:

$$IndExp_{dg} = \sum_{k} NativeShare_{kdg,2014} * VenShare_{k,2015-2019} \tag{4.2}$$

Within each department d and each of the 2-by-3-by-4 gender-age-education groups g, we multiply the share of working natives with the share of working Venezuelans in industry k, summed over 8 core industries. The industry shares for natives are calculated in 2014 to prevent the statistics from being influenced by industry movements in response to the Venezuelan immigration. The Venezuelan industry shares are calculated at the national level using data from 2015-2019 to maximize sample

size.[27][28]

In column 1 of Table 4.6, we interact the model with $IndExp_{dg}$, restricting the sample to those who are working or unemployed. There are no differences in trust according to predicted industry exposure. The only significant result is that natives predicted to face more competition are *less* likely to say that they do not want an immigrant neighbor in response to more immigration. One explanation for this could be that industry overlap actually improves cooperative attitudes by increasing contact between natives and migrants. However, this result is only significant at the 5% level and is no longer significant once multiple hypothesis testing is considered. In columns 2 and 3, we interact with unemployment status and income as measured in 2019. Neither of these interaction terms are significant or large in magnitude for trust in foreigners or for any other outcome, implying that effects are not larger for those who were more likely to be economically threatened by this migration.[29]

[27]We do not break Venezuelans into demographic groups for two reasons. First, the sample of Venezuelans within fine demographic divisions becomes small in the GEIH. Second, the demographic groups are less relevant for Venezuelans in terms of labor market competition, since many young and more educated Venezuelans are working jobs traditionally filled by older and less educated natives. Finally, for Venezuelans we use national industry shares, rather than department-level industry shares, again considering the small sample size, and to mitigate concerns about endogenous sorting of workers in certain industries into specific departments. Our results are not sensitive to these simplifications: they do not change if we break Venezuelans into demographic groups or if we use department-level Venezuelan industry shares.

[28]Industry exposure is largest for women under age 30 with completed secondary education, in part reflecting the large concentration of Venezuelans working in sales, hotels and restaurants.

[29]Labor outcomes in 2019 may have themselves changed in response to Venezuelan immigration. If anything, we expect this to increase the differences across groups, as people who became unemployed or lower-income because of the Venezuelan migration should react more negatively in terms of trust. Likewise, those who may have benefited from increased employment or earnings should react more positively.

Table 4.6: Heterogeneity by Industry Exposure and Labor Characteristics

Heterogeneity By:	(1) Industry Exposure (SDs) (GEIH 2014-2019)	(2) Unemployed (Binary) (ECP 2019)	(3) Income† (SDs) (ECP 2019)
	Trust Foreigner		
Foreign Share	-0.017	-0.014	-0.006
	(0.024)	(0.026)	(0.023)
Foreign Share × Heterogeneity Variable	-0.004	0.032	-0.002
	(0.012)	(0.034)	(0.018)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 0.880]	[q = 0.558]	[q = 0.880]
	Trust Generalized		
Foreign Share	-0.045	-0.041	-0.032
	(0.032)	(0.031)	(0.030)
Foreign Share × Heterogeneity Variable	0.007	0.037	0.008
	(0.015)	(0.034)	(0.014)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 0.638]	[q = 0.319]	[q = 0.638]
	Trust Limited		
Foreign Share	-0.020	-0.018	-0.017
	(0.028)	(0.024)	(0.027)
Foreign Share × Heterogeneity Variable	-0.010	-0.012	0.004
	(0.011)	(0.042)	(0.014)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 1.000]	[q = 1.000]	[q = 1.000]
	Not Want Nbr Immigrant		
Foreign Share	0.406	0.100	0.108
	(0.606)	(0.587)	(0.609)
Foreign Share × Heterogeneity Variable	-0.603**	-0.014	0.158
	(0.281)	(0.948)	(0.447)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 0.471]	[q = 1.000]	[q = 1.000]
Ind/Mun Controls	X	X	X
Observations	31,103	31,103	29,015
Kleibergen-Paap F	59.079	68.376	48.949

Sample restricted to working or unemployed. All regressors fully interacted with heterogeneity variable. All of the heterogeneity variables are standardized into Z-scores, such that "Foreign Share" measures the treatment effect at the mean. See Data Appendix for description of heterogeneity variables and full list of controls. Municipality-clustered standard errors in parenthesis. * p<0.10, ** p<0.05, *** p<0.01.

† Income is zero if unemployed, missing if unknown

The relative lack of importance of industry composition could be for various reasons. Individuals that work in immigrant-heavy industries may not be concerned about labor market competition negatively impacting them, or if they are, this may not translate into lower trust or preferences for segregation. Alternatively, as discussed, positive interactions between natives and immigrants in the workplace could counteract the negative impact generated by concerns over labor force competition. Overall, there is little evidence for concerns over economic competition driving a negative relationship between immigration and trust. This is consistent with existing work showing that anti-immigrant sentiment is driven more by concerns over public goods availability than over wages (Card *et al.* , 2012).

4.7 Robustness

The validity of the instrument depends on the extent to which it is correlated with other characteristics related to trust in foreigners, including baseline values of trust. We have already shown that, at the department level, distance to the border is uncorrelated with pre-period trust in foreigners, and that a 2SLS model using changes over time at the department level replicates our main results of positive sorting with no causal effect. We also showed the trust was uncorrelated with a long list of municipality-level pre-period characteristics related to socioeconomic outcomes, crime and historical violence. These were included as controls in our main analysis and this did not significantly impact the results. We now present the remainder of the robustness checks.

Pre-period differences in trust across municipalities: As mentioned before, there is no data that allows us to evaluate trust by municipality before 2019, when we included the social capital module in the ECP. Nonetheless, we can leverage variables from the 2015 wave of the ECP that are correlated with trust in 2019: an index of trust in state actors (including local, state and national legislative and executive bodies), whether they voted in the most recent presidential election, whether they are politically involved (such as being a member of a political party, attending town hall meetings or being involved in any popular initiatives or campaigns) and an index for involvement with community organizations (such as advocacy groups and community action committees).[30] In column 2 of Table A26, we include these variables as controls using the 101 municipalities that were surveyed in both waves. Neither the OLS nor the 2SLS results change. In row 2 of Table A27, we show that this also doesn't impact our heterogeneity results across the three dimensions that we found to be relevant - urbanization, public goods provision and residential integration.

As an additional check, in Table A28 we run the 2SLS model using these 2015 ECP variables as placebo outcomes. For each of the four placebo outcomes, the coefficient is small and insignificant. We repeat this with our key heterogeneity variables in Table A29. In only one case is the interaction term significant - in places with more residential integration, migration predicted lower trust in the state in the pre-period - and this significance goes away when multiple hypothesis testing is considered (adjusting for the fact that this outcome was tested in three interacted

[30]Details on the construction of each variable can be found in the Data Appendix. Figure A13 shows that they are all closely correlated with trust in foreigners in 2019. For example, a 1 SD increase in trust in state actors in 2019 is associated with a .13 SD increase in trust in foreigners, and people who are politically involved have .16 SD more trust in foreigners. The relationship between foreigner trust and community participation is smaller but statistically significant at the 5% level.

models). When we controlled for this variable, it also did not impact the finding that migration increases trust in more integrated municipalities. That the instrument does not predict any effect of immigration on these pre-period measures helps to build confidence that it also uncorrelated with pre-period trust in foreigners.

Previous exposure to migrants and proximity to the border: One may be concerned that municipalities closer to the border have had past exposure to Venezuelan immigration or Venezuelan culture, and this is not reflected in the department measure of trust from the 2012 WVS. To address this, we control for the share of the municipality that was Venezuelan-born in the 2005 census, to absorb effects that are driven by the historical presence of Venezuelan residents, which as we previously noted is very small relative to the current migration wave. One could also be concerned that municipalities very close to the border are driving the results, as these had the most substantial presence of Venezuelans both before and during the crisis. They may also be more likely to be influenced by changes happening along the Venezuelan border, such as changes in daily commuting or economic activity. We therefore drop municipalities within 200km driving distance from the Venezuelan border, including all of municipalities in the department of Norte de Santander, which is the only department that had any municipalities with a Venezuelan-share larger than 1% in 2005.

These results are presented in columns 3 and 4 of Table A26. For both checks, the OLS and IV results are maintained. When we drop municipalities within 200km of the border (column 4), the 2SLS coefficient in fact becomes larger, implying that these municipalities were not creating an upward bias, as would have been the case

if municipalities closer to the border had more baseline trust towards Venezuelans. They are presented in rows 3 and 4 of Table A27 for the heterogeneity results. The results for urbanization are not impacted. When we control for the 2005 Venezuelan share, the heterogeneity results regarding public goods and residential integration become a bit smaller in magnitude and no longer statistically significant, implying that this variable is at least partly responsible for driving the results. However, they remain qualitatively similar. The most significant impact comes from dropping municipalities close to the border on the interaction with the "interaction index" - the interaction term falls to zero. So it does appear that places very close to the border, that also have a higher measure of residential integration, are responsible for the result that places with more integration react more positively to migrants. In the same tables, we also confirm that these results are not driven by Bogotá, which is the largest municipality in our sample and is the municipality that has had the largest historical presence of immigrants.

Internal migration: If native Colombians internally migrate in response to the Venezuelan migration, then this could bias estimates based on geographic variation. Chapter 1 shows evidence for small increases in out-migration and decreases in-migration in places that receives more migrants. In particular, if less trusting individuals leave municipalities that receive more Venezuelans, this would generate an upward bias. We therefore drop the 8% of respondents who lived in a different municipality five years before 2019, since this information was collected from all ECP respondents in 2019. This is presented in column 6 of Table A26 and row 6 of Table A27. None of our results change, implying that they are not driven by selective

internal migration.

Geographic isolation: One may be concerned that driving distance is endogenous to local isolation, which may be an important determinant of trust. To mitigate this concern, we have already included driving distance to Bogotá in our main set of controls, which should absorb any factors related to local economic isolation. We further show here that the results do not change when we use straight-line distance to the closest border crossing, which reduces the correlation between the instrument and economic isolation.[31]

Return migrants: Finally, we run a specification where we include all migrants from Venezuela in our right-hand-side migration share, including Colombian-born migrants. As mentioned, they make up almost 20% of all Venezuelan migrants, and are excluded because we are most interested in studying the impact of foreign-born immigrants on social preferences. Including this population in the migration share does not impact the results.

Alternative measures of trust: We follow the wording of the World Value Survey in our measurement of trust so it could be compared, even coarsely, over time. Nonetheless, recent work using the Global Preference Survey (GPS) has suggested that other possible wordings of the trust question can predict behavior in the lab with precision (Falk *et al.* , 2018). We included the question suggested by (Falk *et al.* , 2018) in a second phone survey carried out in the summer of 2020, and asked again the ECP question to verify their correlation. The answer to the ECP trust question

[31]The results also do not change if we use the average driving distance or straight-line distance to all border crossings, or the straight-line distance to the closest point along the border.

is highly correlated with the one proposed in the GPS. In particular, for individuals who respond that they do not 'assume that foreigners have the best intentions', trust in foreigners on the 1 to 5 scale is heavily concentrated at 1 (mean value 1.27), whereas for those who responded 'yes' the values are distributed in an almost normal fashion (mean 2.42). We take this as evidence that our instrument is also a good proxy for trusting behavior, as the question from GPS has shown to be.

4.8 Conclusion

The recent growth in global refugee flows, combined with the emergence of anti-immigrant politics in many countries, has created a widespread fear that immigration fuels anti-immigrant sentiment. This has also been true of the unprecedented mass migration of Venezuelans to Colombia, which coincided with a decrease in native trust towards foreigners. However, in this paper we provide causal evidence that, on average, municipality-level in-migration did not cause local decreases in trust towards foreigners. We were able to identify a causal impact because of the unexpected size and timing of the migration, combined with the strong negative correlation between the migrant share and distance to the Venezuelan border. This border stretches across northeast Colombia and this proximity, we show, is largely uncorrelated with pre-period department-level trust or other relevant municipality-level characteristics.

We then show that, in municipalities that are more urbanized, have higher quality public goods, and where migrants live closer to natives, local in-migration actually increased trust in foreigners. These results point to the contact hypothesis as an important mechanism - only where natives and foreigners are more likely to interact

and where there is less competition over public goods does immigration increase trust in foreigners. On the other hand, neither fears about increasing economic competition nor increasing crime appear to drive a significant relationship between local migration and trust.

There is by now mounting evidence across many literatures, such as those from lab-based experiments or those studying electoral outcomes, of the potential benefits of intergroup contact. Fewer papers have studied causal effects on attitudes towards foreigners in the context of a real-world migration wave, where native-migrant interaction is substantially smaller in scale and frequency. We present optimistic evidence that, under the right conditions, these interactions can promote greater trust in foreigners. We also present a warning that, under the strains of public good scarcity or a lack of contact between immigrants and natives, immigration may decrease trust. We observe this happening over a very short time frame and in a context in which natives and foreigners speak a common language and are culturally and ethnically similar. It may be that trust is less likely to respond positively to immigration, and more likely to respond negatively, when this condition does not hold.

Conclusion

In this dissertation, I documented the effects of Venezuelan migration to Colombia between 2015-2019 on the labor market outcomes of native Colombians. I estimated minimal effects on employment and significant decreases in hourly wages that are most concentrated among less educated, low-wage, and informal workers. This was found under both a non-structural instrumental variable approach and by estimating a structural aggregate production function. Using the structural approach, I documented what many immigration scholars have conjectured - that the occupational downgrading of migrants has played a role in increasing the wage effects faced by low-wage natives. This indicates that policies to minimize migrant occupational downgrading, such as reducing barriers to licensing and education recognition or directly matching migrants to employers, will reduce inequality and increase productivity.

At the same time, the analysis indicates that negative wage effects persist in the absence of migrant downgrading. This is partially because of high substitutability between migrants and natives in low-skill occupations, in part reflecting high flexibility in the informal sector. Evidence from Chapter 1 also indicates that low capital mobility amplifies the wage effect - something that migration economists have emphasized in their models for decades, but for which there is scant empirical evidence. These are both characteristics of the developing country setting that increase the short-term wage consequences of migration. Therefore, expanding the formal sector, where minimum wages and other regulations are enforced, and increasing investment and feasibility of doing business in places with declining wages, are development goals that coincide with minimizing short-term costs of migration for native workers.

Importantly, this analysis is short-term, within only a few years of migrant arrival, and it is important that scholars continue to study the long-term effects Venezuelan migration in Colombia over the next decade. Many of the benefits of migration take time to accrue. For example, migrants transfer human capital and industry networks, and they will consume more as migrant incomes rise in Colombia. Capital will also adjust in response to the increase in labor supply, and my dynamic analysis in Chapter 2 indicates that this by itself will lead migration to benefit many workers in the long-term.

Finally, the effects of migration are also sociopolitical. Globally, increases in migration and forced displacement have coincided with increases in anti-immigrant dialogue and politics. Colombia is no exception, as general attitudes and trust towards foreigners have decreased over the period of Venezuelan migration. However, Chapter 3 indicates that this is not driven by the municipalities that are receiving the most migrants. In some municipalities - those that are more urbanized, have higher quality health and education services, and where there is more residential integration between migrants and natives - migration actually increases trust towards foreigners. Interestingly, there is little evidence that trust decreases more among those who experience the most economic competition with migrants, highlighting the complexity of the formation of immigrant sentiment. Much more research is needed to understand exactly when, where, and how anti-immigrant sentiment forms. Particular attention should be paid to the roles of media portrayal of migrants and social media, which was not explored in this dissertation.

Appendix - Additional Tables and Figures

Table A1: Characteristics of Migrants and Non-Migrants

	Non-Migrants	Migrants
Male (%)	48.5 (50)	49.6 (50)
Age	36.5 (13.9)	31.5 (11.3)
Labor Force Participation (%)	71.7 (45)	79.4 (40.5)
Unemployment (%)	11.4 (31.7)	14.8 (35.5)
Median Hrly Wage (2010 USD)	2.3 (6.1)	1.6 (4.8)
Hrs per Week	45.2 (15.9)	49.6 (17.4)
Own-Account (%)	25.3 (43.5)	32.1 (46.7)
Informal (%)	56 (49.6)	88.3 (32.1)
N	447264	21730

Means presented, standard deviations in parentheses. Restricted to urban residents age 15-64. Migrant defined as anyone in 2019 who was living in Venezuela 5 years ago. Population weights applied. Source: GEIH 2019

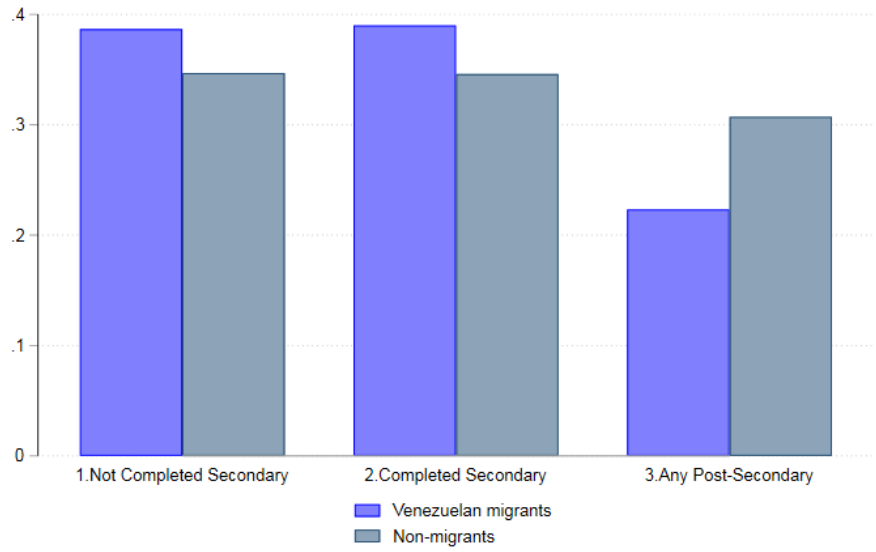


Figure A1: Education by Migrant Status
Restricted to urban residents age 15-64. Source: GEIH 2019.

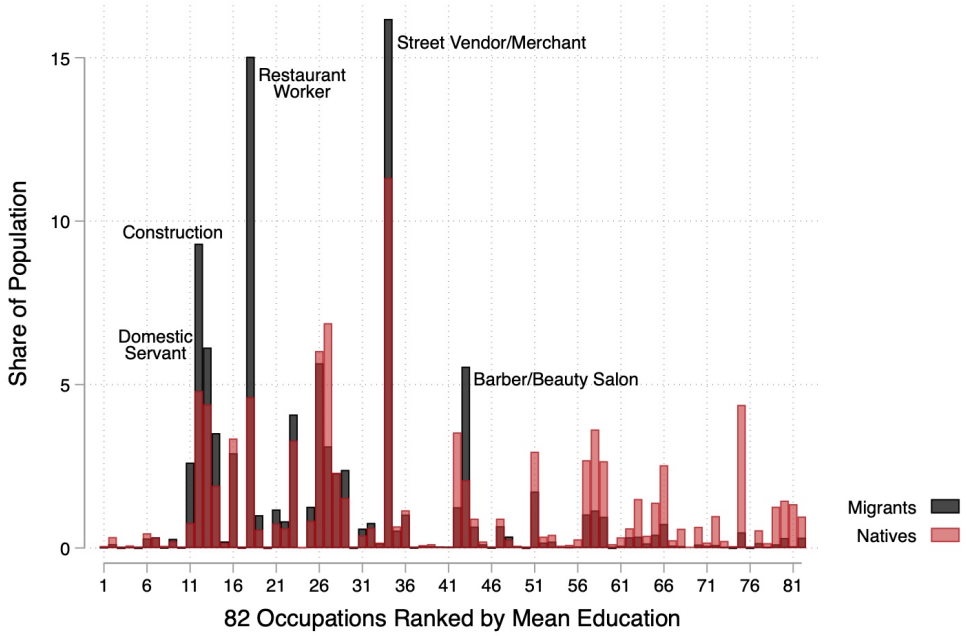


Figure A2: Concentration of Migrants and Natives Across Occupations
Restricted to urban residents age 15-64. Occupations ranked according to mean years of completed schooling for natives in the GEIH between 2010-2015. Source: GEIH 2019.

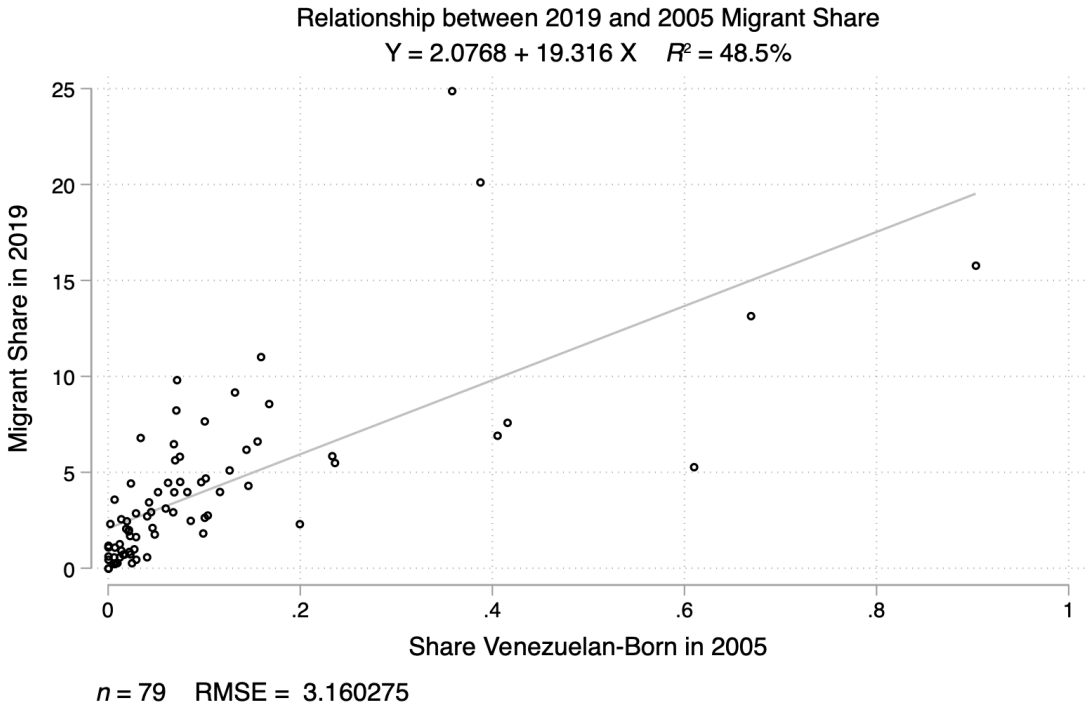


Figure A3: Relationship between 2019 and 2005 Migrant Share

Table A2: Pre-Period Correlates of 2005 Venezuelan Share of Population

2014 Metro Characteristic:	(1) $V_{c,2005}$	(2) $V_{c,2005}$	(3) $V_{c,2005}$	(4) $V_{c,2005}$	(5) $V_{c,2005}$
Population (100,000)	-0.001	0.005	0.004	0.004	0.002
	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)
100*Ln(Hrly Wage)		-0.017**	-0.018*	-0.018*	-0.024*
		(0.008)	(0.009)	(0.009)	(0.014)
100*Ln(Hrs per Week)			0.021	0.021	0.026
			(0.017)	(0.017)	(0.019)
100*Unemployment Rate				0.047	0.049
				(0.125)	(0.126)
100*LFP Rate					0.041
					(0.047)
N	79	79	79	79	79
R^2	1.6e-04	.051	.073	.073	.09

$V_{c,2005}$ is the 2005 Venezuelan share of the metro area, which ranges from 0% to .9%, and has been normalized to a mean of 0 and SD of 1. Observations weighted by 2014 population. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

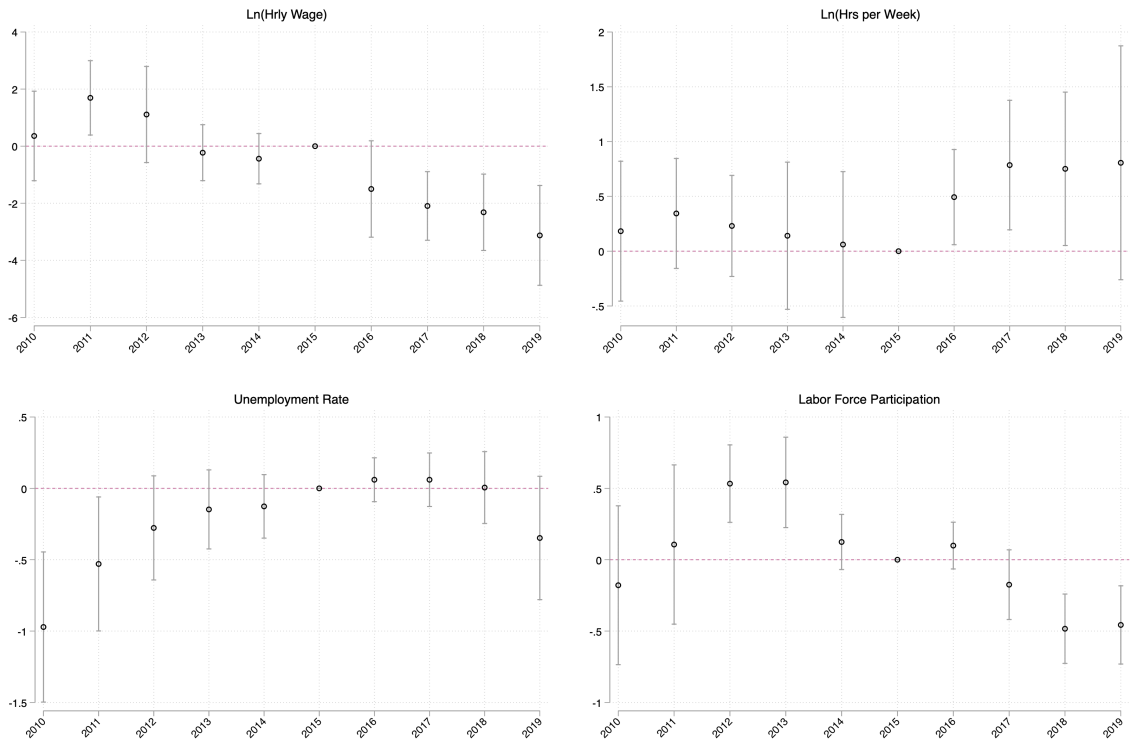


Figure A4: Pre-Trends in Labor Market Outcomes
Estimated according to Equation (2.5) in Section **??**.

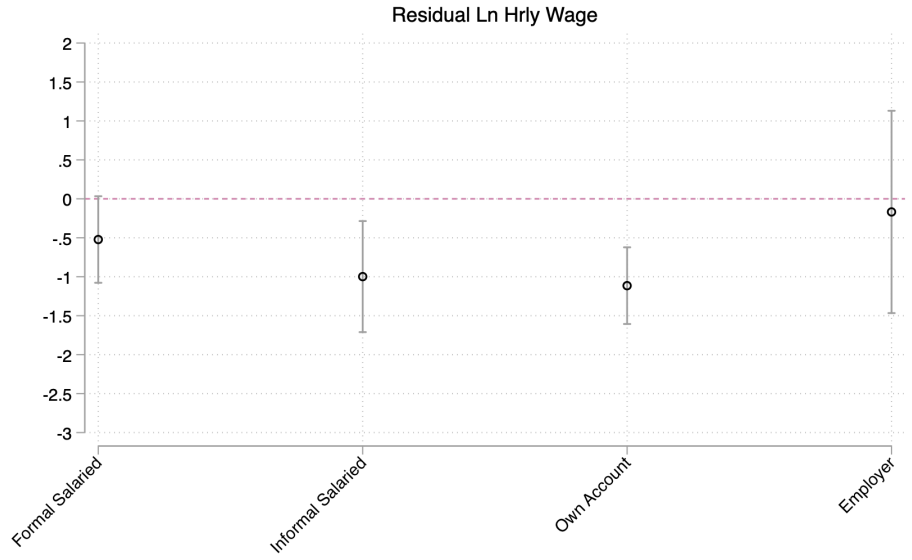


Figure A5: 2SLS Coefficient Plot - By Work Sector

Table A3: Wage Estimate Sensitivity

	Panel A: Including Return Migrants			
	(1)	(2)	(3)	(4)
	OLS	IV: 2005 Census	IV: 1993 Census	IV: Inverse Distance
	Geographic Unit: CZ Metro Areas			
Migrant share	-0.73*	-1.05***	-1.13***	-1.16***
	(0.42)	(0.22)	(0.21)	(0.17)
K-P Wald Stat.		23.35	21.78	28.54
Number of units	79	79	79	79
	Geographic Unit: Administrative Metro Areas			
Migrant share	-0.67	-0.94***	-1.03***	-1.08***
	(0.49)	(0.26)	(0.20)	(0.16)
K-P Wald Stat.		29.58	19.88	46.11
Number of units	23	23	23	23
	Geographic Unit: Department			
Migrant share	-0.83	-1.19***	-1.21***	-1.27***
	(0.51)	(0.28)	(0.30)	(0.18)
K-P Wald Stat.		19.07	20.90	35.20
Number of units	24	24	24	24
	Panel B: Excluding Return Migrants			
	(5)	(6)	(7)	(8)
	OLS	IV: 2005 Census	IV: 1993 Census	IV: Inverse Distance
	Geographic Unit: CZ Metro Areas			
Migrant share	-0.87**	-1.24***	-1.32***	-1.34***
	(0.42)	(0.26)	(0.25)	(0.21)
K-P Wald Stat.		26.42	21.52	29.50
Number of units	79	79	79	79
	Geographic Unit: Administrative Metro Areas			
Migrant share	-0.79	-1.09***	-1.18***	-1.24***
	(0.48)	(0.30)	(0.23)	(0.18)
K-P Wald Stat.		30.17	17.76	39.35
Number of units	23	23	23	23
	Geographic Unit: Department			
Migrant share	-0.97*	-1.41***	-1.44***	-1.48***
	(0.52)	(0.34)	(0.35)	(0.21)
K-P Wald Stat.		19.55	19.38	31.37
Number of units	24	24	24	24

The outcome, log hourly wage, is residualized and multiplied by 100. All models include Year FE and Unit FE. Panel A includes Colombian-born migrants in the migrant share, while Panel B excludes them. Column 2-4 use different share components of the shift-share IV, based respectively on the complete 2005 census (as in the main analysis), the 10% subsample of the 1993 census, and the inverse minimum driving distance from the metro area or department capital to the closest Venezuelan border crossing. Geographic units include the 79 commuting zone-defined metro areas (as in the main analysis), the 23 official metro areas of which the GEIH is representative, and the 24 departments of Colombia. Observations weighted by unit-year population. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A4: Wage Estimate Sensitivity: 2014-2018 Difference Model

	Panel A: Including Return Migrants			
	(1)	(2)	(3)	(4)
	OLS	IV: 2005 Census	IV: 1993 Census	IV: Inverse Distance
	Geographic Unit: CZ Metro Areas			
Foreigner Share (2018 GEIH)	-0.26	-1.07***	-1.05***	-1.24***
	(0.60)	(0.39)	(0.35)	(0.24)
Foreigner Share (2018 Census)	-0.54	-1.26***	-1.21***	-1.45***
	(0.49)	(0.46)	(0.42)	(0.28)
Number of units	79	79	79	79
	Geographic Unit: Administrative Metro Areas			
Foreigner Share (2018 GEIH)	-0.09	-0.87**	-0.89***	-1.07***
	(0.63)	(0.43)	(0.33)	(0.23)
Foreigner Share (2018 Census)	-0.39	-1.13**	-1.09**	-1.35***
	(0.54)	(0.57)	(0.43)	(0.29)
Number of units	23	23	23	23
	Geographic Unit: Department			
Foreigner Share (2018 GEIH)	-0.25	-1.02***	-0.97**	-1.19***
	(0.59)	(0.39)	(0.41)	(0.23)
Foreigner Share (2018 Census)	-0.46	-1.36***	-1.32**	-1.53***
	(0.79)	(0.50)	(0.53)	(0.29)
Number of units	24	24	24	24
	Panel B: Excluding Return Migrants			
	(5)	(6)	(7)	(8)
	OLS	IV: 2005 Census	IV: 1993 Census	IV: Inverse Distance
	Geographic Unit: CZ Metro Areas			
Foreigner Share (2018 GEIH)	-0.62	-1.58***	-1.53***	-1.80***
	(0.75)	(0.58)	(0.52)	(0.34)
Foreigner Share (2018 Census)	-0.65	-1.55***	-1.46***	-1.77***
	(0.55)	(0.56)	(0.51)	(0.34)
Number of units	79	79	79	79
	Geographic Unit: Administrative Metro Areas			
Foreigner Share (2018 GEIH)	-0.36	-1.27**	-1.26***	-1.54***
	(0.76)	(0.63)	(0.48)	(0.32)
Foreigner Share (2018 Census)	-0.48	-1.38**	-1.31**	-1.65***
	(0.61)	(0.70)	(0.53)	(0.36)
Number of units	23	23	23	23
	Geographic Unit: Department			
Foreigner Share (2018 GEIH)	-0.55	-1.54***	-1.43**	-1.77***
	(0.75)	(0.59)	(0.61)	(0.33)
Foreigner Share (2018 Census)	-0.55	-1.81***	-1.72**	-2.02***
	(0.99)	(0.67)	(0.70)	(0.38)
Number of units	24	24	24	24

See notes to previous table. This model is a regression of the change in residual wages from 2014 to 2018 on the 2018 foreigner share, calculated using the GEIH or Census as a share of the 2018 population. The instrument is created analogously (interacted with the national migration rate from the GEIH or census). In no case does the K-P Wald stat fall below 10.

Table A5: Wage Estimate Sensitivity: 1-Year Migration Measure

	Panel A: Including Return Migrants			
	(1)	(2)	(3)	(4)
	OLS	IV: 2005 Census	IV: 1993 Census	IV: Inverse Distance
	Geographic Unit: CZ Metro Areas			
1-Year Migrant Share (Data until 2019)	-1.45	-3.53***	-3.96***	-3.82***
	(1.29)	(0.80)	(0.90)	(0.67)
1-Year Migrant Share (Data until 2017)	-2.18	-4.80***	-5.15***	-5.74***
	(2.35)	(1.36)	(1.25)	(0.91)
Number of units	79	79	79	79
	Geographic Unit: Administrative Metro Areas			
1-Year Migrant Share (Data until 2019)	-1.38	-3.05***	-3.60***	-3.58***
	(1.67)	(0.89)	(0.82)	(0.59)
1-Year Migrant Share (Data until 2017)	-2.76	-4.40**	-5.03***	-5.65***
	(3.03)	(1.74)	(1.35)	(0.96)
Number of units	23	23	23	23
	Geographic Unit: Department			
1-Year Migrant Share (Data until 2019)	-1.80	-4.01***	-4.31***	-4.17***
	(1.65)	(1.01)	(1.19)	(0.63)
1-Year Migrant Share (Data until 2017)	-3.58	-5.38***	-5.29***	-6.33***
	(3.15)	(1.59)	(1.63)	(0.92)
Number of units	24	24	24	24
	Panel B: Excluding Return Migrants			
	(5)	(6)	(7)	(8)
	OLS	IV: 2005 Census	IV: 1993 Census	IV: Inverse Distance
	Geographic Unit: CZ Metro Areas			
1-Year Migrant Share (Data until 2019)	-1.74	-3.75***	-4.28***	-3.94***
	(1.29)	(0.85)	(1.06)	(0.75)
1-Year Migrant Share (Data until 2017)	-3.64	-5.51***	-5.92***	-6.64***
	(2.85)	(1.48)	(1.30)	(0.96)
Number of units	79	79	79	79
	Geographic Unit: Administrative Metro Areas			
1-Year Migrant Share (Data until 2019)	-1.58	-3.03***	-3.55***	-3.55***
	(1.57)	(0.87)	(0.79)	(0.59)
1-Year Migrant Share (Data until 2017)	-3.78	-4.85**	-5.49***	-6.42***
	(3.23)	(1.88)	(1.40)	(1.02)
Number of units	23	23	23	23
	Geographic Unit: Department			
1-Year Migrant Share (Data until 2019)	-1.97	-4.14***	-4.45***	-4.30***
	(1.63)	(1.03)	(1.21)	(0.67)
1-Year Migrant Share (Data until 2017)	-4.82	-6.68***	-6.50***	-8.07***
	(3.41)	(1.93)	(1.93)	(1.08)
Number of units	24	24	24	24

See notes to previous table. This is the original two-way fixed effects model, replacing the 5-year migrant share with the 1-year migrant share (the share of the population that arrived from Venezuela in the past 12 months), and varying the end of the period of analysis. In no case does the K-P Wald stat fall below 10.

Table A6: 2SLS Estimates Additional Robustness

	(1) Original 2SLS	(2) Drop Bogotá	(3) Drop Metro Yrly Sample <1,000	(4) 2014-2019 Difference Model	(5) Kronmal Specification	(6) Assign Natives to Past Metro
			Ln(Hourly Wage)			
All	-1.05***	-1.03***	-1.04***	-1.09***	-0.66***	-1.04***
	(0.22)	(0.22)	(0.23)	(0.27)	(0.15)	(0.20)
Less than Secondary	-1.42***	-1.41***	-1.41***	-1.33***	-0.95***	-1.38***
	(0.23)	(0.24)	(0.25)	(0.27)	(0.17)	(0.22)
Secondary	-0.86***	-0.87***	-0.85***	-0.79***	-0.54***	-0.80***
	(0.22)	(0.22)	(0.23)	(0.28)	(0.15)	(0.21)
Post-Secondary	-0.75*	-0.69*	-0.75*	-0.96**	-0.46**	-0.80**
	(0.38)	(0.37)	(0.39)	(0.48)	(0.23)	(0.36)
			Ln(Hours per Week)			
All	0.27	0.28	0.27	0.27	0.17	0.29
	(0.27)	(0.27)	(0.27)	(0.32)	(0.17)	(0.27)
Less than Secondary	0.50	0.52	0.51	0.51	0.34	0.51
	(0.34)	(0.34)	(0.34)	(0.39)	(0.22)	(0.34)
Secondary	0.32	0.32	0.30	0.26	0.20	0.32
	(0.21)	(0.21)	(0.21)	(0.26)	(0.12)	(0.21)
Post-Secondary	-0.02	-0.02	-0.00	0.01	-0.01	0.02
	(0.23)	(0.22)	(0.22)	(0.25)	(0.12)	(0.22)
			Unemployment			
All	-0.08	-0.08	-0.09	-0.07	-0.05	-0.07
	(0.07)	(0.07)	(0.08)	(0.07)	(0.04)	(0.07)
Less than Secondary	-0.05	-0.05	-0.06	-0.03	-0.04	-0.06
	(0.06)	(0.07)	(0.07)	(0.06)	(0.04)	(0.06)
Secondary	-0.02	-0.01	-0.03	0.02	-0.01	-0.01
	(0.09)	(0.08)	(0.09)	(0.09)	(0.05)	(0.08)
Post-Secondary	-0.18*	-0.18*	-0.17*	-0.20*	-0.11*	-0.15
	(0.10)	(0.10)	(0.10)	(0.10)	(0.06)	(0.10)
			LFP			
All	-0.21*	-0.21**	-0.21**	-0.17*	-0.13**	-0.20**
	(0.11)	(0.11)	(0.10)	(0.10)	(0.07)	(0.10)
Less than Secondary	-0.23*	-0.24*	-0.24**	-0.16	-0.16*	-0.24**
	(0.12)	(0.12)	(0.11)	(0.10)	(0.08)	(0.12)
Secondary	-0.12**	-0.12**	-0.12**	-0.10	-0.07**	-0.12**
	(0.06)	(0.06)	(0.06)	(0.07)	(0.04)	(0.05)
Post-Secondary	-0.24	-0.25	-0.23	-0.27	-0.15*	-0.21
	(0.16)	(0.16)	(0.17)	(0.18)	(0.08)	(0.15)
Kleibergen-Paap Wald Stat.	23.35	23.41	19.28	30.89	24.17	24.47
Number of metro areas	79	78	27	79	79	79

Outcomes are residualized and multiplied by 100. All models include Year FE and City FE. Column 4 regresses the change in outcomes from 2014-2019 on the change in the migrant share. Column 5 replaces the migrant share with the number of migrants (divided by 100) and controls for baseline population interacted with year fixed effects. Column 6 assigns natives to their 5-year lagged metro area of residence. Observations weighted by city-year-group population. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A7: 2SLS Estimates with Migrant Share Quadratic

	(1)	(2)	(3)	(4)
		Ln(Hrly Wage)		
	All	Less than Secondary	Secondary	Post-Secondary
Migrant Share	-1.40	-2.37**	-1.14	-0.79
	(1.12)	(1.11)	(1.02)	(1.67)
Migrant Share^2	0.02	0.05	0.02	0.00
	(0.05)	(0.05)	(0.05)	(0.08)
Joint F-Statistic	62.36	55.46	39.73	25.46
Kleibergen-Paap Wald Stat.	58.02	35.66	33.97	89.30

Linear and quadratic migrant shares are instrumented with a linear and quadratic of the IV. Wages are residualized and multiplied by 100. All models include Year FE and City FE. Observations weighted by city-year-group population. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

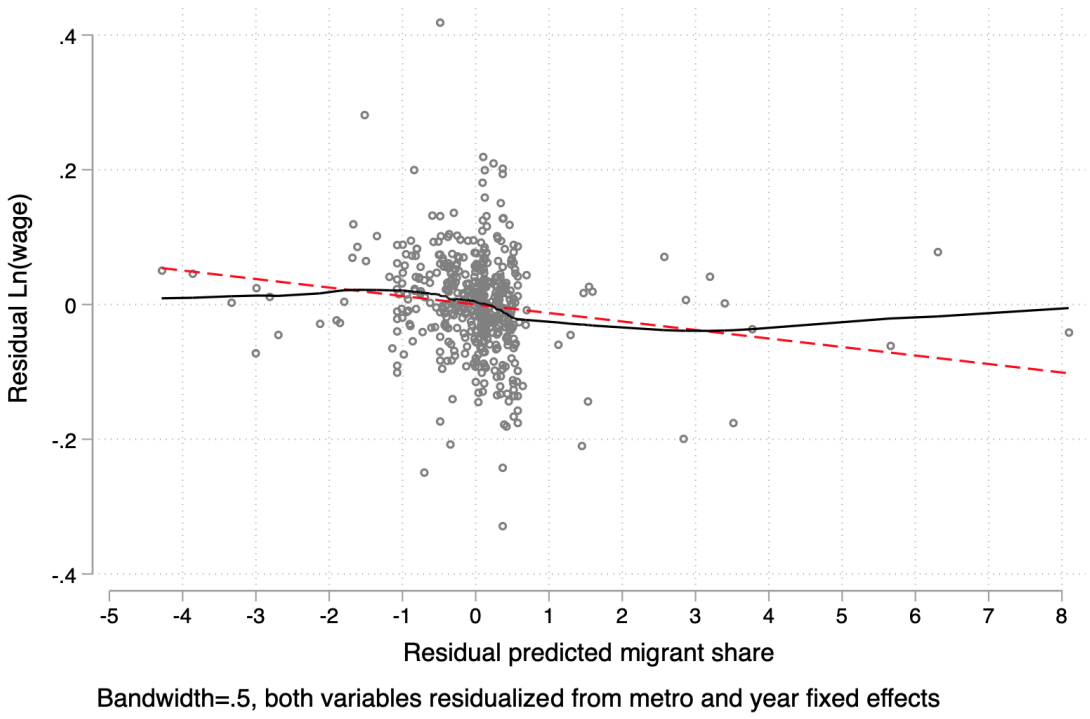


Figure A6: LOWESS Plot

Table A8: 2SLS Effects on Occupation Skill Group and Type of Work - Robustness

	(1) Original 2SLS	(2) Drop <100km from Border	(3) Control Trade with Venezuela	(4) Year Trend X Inv Distance to Border	(5) Year Trend X Region	(6) Year Trend X Pre-trend	(7) Drop Bogotá	(8) Drop Metro Yrly Sample <1,000	(9) 2014-2019 Difference Model	(10) Kronmal Specification	(11) Assign Natives to Past Metro
Occup Skill Group 1	-0.06***	-0.05	-0.05**	0.04	-0.00	-0.06**	-0.07***	-0.07***	-0.06***	-0.04***	-0.06**
	(0.02)	(0.08)	(0.03)	(0.07)	(0.04)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Occup Skill Group 2	-0.03	0.15	-0.05	0.21**	0.03	-0.01	-0.03	-0.03	-0.04	-0.02	-0.03
	(0.08)	(0.10)	(0.07)	(0.10)	(0.08)	(0.08)	(0.08)	(0.08)	(0.07)	(0.04)	(0.08)
Occup Skill Group 3	0.07	0.16**	0.08	0.21***	0.05	0.09**	0.06	0.06	0.08*	0.04	0.06
	(0.04)	(0.08)	(0.05)	(0.07)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)	(0.03)	(0.04)
Occup Skill Group 4	-0.01	0.03	0.00	0.04	0.02	-0.01	-0.01	-0.00	-0.00	-0.01	-0.01
	(0.02)	(0.05)	(0.03)	(0.06)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Occup Skill Group 5	-0.09***	-0.11*	-0.09***	-0.14**	-0.11***	-0.09***	-0.08***	-0.09***	-0.09***	-0.06***	-0.09***
	(0.02)	(0.06)	(0.02)	(0.07)	(0.04)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
Under-Employed	0.29**	0.52	0.26**	0.57*	0.31**	0.29**	0.28**	0.30**	0.33**	0.18**	0.28**
	(0.13)	(0.32)	(0.12)	(0.32)	(0.12)	(0.13)	(0.13)	(0.13)	(0.13)	(0.08)	(0.12)
Formal Salaried	-0.09***	-0.10**	-0.08***	-0.07	-0.09*	-0.09***	-0.08***	-0.09***	-0.07***	-0.06***	-0.06***
	(0.02)	(0.05)	(0.03)	(0.04)	(0.05)	(0.02)	(0.02)	(0.02)	(0.03)	(0.01)	(0.02)
Informal Salaried	-0.02	0.07	0.01	0.18	0.09	-0.04	-0.03	-0.01	-0.07	-0.01	0.00
	(0.07)	(0.16)	(0.07)	(0.15)	(0.06)	(0.06)	(0.07)	(0.07)	(0.08)	(0.05)	(0.06)
Own-Account	-0.04	0.09	-0.06	0.13	-0.08	-0.04	-0.04	-0.05	0.01	-0.03	-0.07
	(0.09)	(0.21)	(0.08)	(0.22)	(0.13)	(0.09)	(0.09)	(0.09)	(0.07	(0.05)	(0.09)
Employer	0.01	0.04	0.02	0.07	0.06***	0.02	0.01	0.02	0.02	0.01	0.01
	(0.02)	(0.05)	(0.02)	(0.04)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02	(0.01)	(0.02)
Kleibergen-Paap Wald Stat.	25.69	12.80	30.99	14.50	102.73	24.59	25.77	20.69	33.17	26.38	26.37
Number of metro areas	79	73	79	79	79	79	78	27	79	79	79

Outcomes are multiplied by 100. All models are 2SLS linear probability models for probability of employment in a group, not conditional on working, with Year FE and City FE. Observations weighted by city-year-group population. See paper for description of robustness checks. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A9: 2SLS Effects on Native Internal Migration - Robustness

	(1) Original 2SLS	(2) Drop <100km from Border	(3) Control Trade with Venezuela	(4) Year Trend X Inv Distance to Border	(5) Year Trend X Region	(6) Drop Bogotá	(7) Drop Metro Yrly Sample <1,000	(8) Kronmal Specification
				Out-Migration				
All	0.03	0.01	0.03	-0.09	-0.11**	0.05*	0.03	0.02
	(0.03)	(0.08)	(0.05)	(0.12)	(0.05)	(0.03)	(0.03)	(0.02)
Less than Secondary	0.07**	0.07	0.07	-0.04	-0.09	0.10***	0.04	0.05**
	(0.04)	(0.10)	(0.06)	(0.14)	(0.06)	(0.03)	(0.03)	(0.03)
Secondary	0.10***	0.05	0.13***	0.04	0.04	0.10***	0.07**	0.06***
	(0.03)	(0.10)	(0.04)	(0.13)	(0.05)	(0.03)	(0.03)	(0.02)
Post-Secondary	-0.07	-0.06	-0.13	-0.16	-0.24***	-0.04	-0.04	-0.04
	(0.05)	(0.08)	(0.08)	(0.15)	(0.08)	(0.04)	(0.05)	(0.03)
				In-Migration				
All	-0.08	0.03	-0.08	0.26	-0.03	-0.09	-0.07	-0.05
	(0.11)	(0.28)	(0.12)	(0.32)	(0.09)	(0.12)	(0.12)	(0.07)
Less than Secondary	-0.04	0.20	-0.05	0.44	-0.01	-0.05	-0.02	-0.02
	(0.12)	(0.29)	(0.12)	(0.30)	(0.10)	(0.12)	(0.13)	(0.08)
Secondary	-0.17	-0.04	-0.19*	0.11	-0.08	-0.18*	-0.16	-0.11*
	(0.11)	(0.30)	(0.10)	(0.36)	(0.08)	(0.11)	(0.11)	(0.06)
Post-Secondary	-0.01	-0.06	0.03	0.26	0.07	-0.02	-0.04	-0.00
	(0.14)	(0.30)	(0.15)	(0.41)	(0.14)	(0.14)	(0.13)	(0.09)
Kleibergen-Paap Wald Stat.	17.78	8.88	38.19	6.30	61.57	17.79	14.00	19.49
Number of metro areas	79	73	79	79	79	78	27	79

Sample includes 2014 and 2019. Migration rates taken as a share of 5-year lag population. All models include Year FE and City FE. Obsevations weighted by city-year-group population. See paper for description of robustness checks. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A10: 2SLS Wage Effects Interacted with Regional Characteristics - Robustness

Interaction Variable:	(1) Original 2SLS	(2) Drop <100km from Border	(3) Control Trade with Venezuela	(4) Year Trend X Inv Distance to Border	(5) Year Trend X Region	(6) Year Trend X Pre-trend	(7) Drop Bogotá	(8) Drop Metro Yrly Sample <1,000	(9) 2014-2019 Difference Model	(10) Kronmal Specification	(11) Assign Natives to Past Metro
2014 Mean Ln(Hrly Wage)	-0.36	-0.69	-0.36	-0.31	-0.20†	-0.36	-0.47	-0.68	-0.48†	-0.63**	-0.35
	(0.46)	(0.60)	(0.46)	(0.45)	(0.42)	(0.46)	(0.49)	(0.62)	(0.50)	(0.29)	(0.40)
2014 Unemployment Rate	0.07	0.35	0.11	0.58**	-0.29	0.08	0.08	0.04	0.08	0.11	0.07
	(0.29)	(0.82)	(0.31)	(0.25)	(0.33)	(0.29)	(0.30)	(0.33)	(0.33)	(0.18)	(0.26)
2014 Informal Rate	-0.62*	-0.80**	-0.63*†	-0.56*	-0.76***	-0.60*	-0.62*	-0.64	-0.62†	-0.34	-0.53*
	(0.34)	(0.38)	(0.34)	(0.34)	(0.27)	(0.34)	(0.37)	(0.40)	(0.42)	(0.48)	(0.31)
2014 Own-Account Rate	-0.75**	-1.15**	-0.81**†	-0.73**	-0.86***†	-0.73**	-0.75**	-0.80*	-0.71*†	-0.74*	-0.61*
	(0.35)	(0.45)	(0.38)	(0.33)	(0.33)	(0.35)	(0.37)	(0.42)	(0.43)	(0.40)	(0.33)
WB DB (Starting a Business)	0.29*†	0.43	0.34*†	0.41*	0.28†	0.29*†	0.28*†	0.27*†	0.26†	0.16†	0.26*†
	(0.16)	(0.35)	(0.18)	(0.24)	(0.25)	(0.16)	(0.16)	(0.15)	(0.16)	(0.11)	(0.14)
WB DB (Construction Permits)	-0.31	-0.31	-0.31	-0.29	-0.08†	-0.28	-0.36	-0.30	-0.35	-0.18	-0.26
	(0.50)	(0.51)	(0.51)	(0.52)	(0.38)	(0.49)	(0.51)	(0.53)	(0.61)	(0.23)	(0.47)
WB DB (Registering Property)	-0.36	-0.37	-0.38	-0.34	-0.38†	-0.37	-0.50	-0.33	-0.34	-0.20	-0.30
	(0.43)	(0.43)	(0.50)	(0.39)	(0.37)	(0.43)	(0.45)	(0.45)	(0.54)	(0.16)	(0.39)
WB DB (Paying Taxes)	-0.06	-0.28	-0.05	0.30	-0.49*	-0.05	-0.06	-0.07	-0.11	0.01	-0.05
	(0.23)	(0.53)	(0.25)	(0.25)	(0.27)	(0.24)	(0.23)	(0.24)	(0.27)	(0.12)	(0.22)
2014 Per-Capita GDP	-0.13	-0.15	-0.14†	-0.20	0.09†	-0.11	-0.18	-0.12	-0.18†	-0.14	-0.14
	(0.32)	(0.33)	(0.31)	(0.33)	(0.37)	(0.31)	(0.30)	(0.33)	(0.35	(0.15)	(0.30)
N	474	438	474	474	474	474	468	162	79	474	474

Each row presents the interaction coefficient from a separate regression using the indicated interaction variable. † indicates that the Kleibergen-Paap Wald Statistic is greater than 16. See notes to Table 2.7. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A11: School Attendance 2SLS Linear Probability Models

	(1)	(2)	(3)
	In School	In School: In Labor Force	In School: Out of Labor Force
	All Ages (Including Border Locations)		
Migrant Share	0.07**	-0.03	0.11*
	(0.03)	(0.08)	(0.06)
Sample Mean	18.76	7.14	11.62
	Age≤24 (Including Border Locations)		
Migrant Share	0.20*	-0.19	0.39*
	(0.10)	(0.24)	(0.23)
Sample Mean	54.60	14.43	40.17
	All Ages (Excluding Border Locations)		
Migrant Share	0.13	0.20*	-0.07
	(0.11)	(0.10)	(0.09)
Sample Mean	18.82	7.17	11.65
	Age≤24 (Excluding Border Locations)		
Migrant Share	0.28	0.49*	-0.21
	(0.34)	(0.27)	(0.33)
Sample Mean	54.69	14.45	40.24

All outcomes are multiplied by 100. All models include Year FE and City FE. Observations weighted by city-year-group population. Excluding border locations drops cities within 100km driving distance of the Venezuelan border. Cluster-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A12: Sample Statistics By Year

	Non-Migrants						Migrants					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	2014	2015	2016	2017	2018	2019	2014	2015	2016	2017	2018	2019
	All Working-Age (Age 15-64)											
N	380,468	381,579	378,484	373,135	368,902	360,745	1,262	1,891	3,635	6,537	13,690	20,554
Male (%)	48.0	48.0	48.1	48.1	48.1	48.0	58.7	52.9	53.6	50.8	50.9	49.6
Age	38.7	38.8	38.9	39.0	39.2	39.4	34.5	35.3	34.2	33.2	32.2	32.1
Completed Secondary (%)	29.2	31.0	32.4	33.7	34.2	35.1	30.3	28.5	28.1	33.5	36.8	37.7
Any Post-Secondary (%)	26.1	25.5	26.1	26.2	27.3	27.7	18.1	20.3	19.0	24.3	25.2	24.5
Labor Force Participation (%)	79.4	79.7	79.8	79.5	79.1	78.8	79.4	81.0	81.0	82.2	84.2	81.8
Unemployment Rate (%)	9.4	9.3	9.6	10.0	10.2	10.9	12.6	10.9	17.0	14.9	15.0	14.7
Employed (%)	72.0	72.3	72.1	71.5	71.0	70.3	69.5	72.2	67.2	70.0	71.6	69.8
	Employed											
Male (%)	56.4	56.3	56.4	56.4	56.5	56.6	69.5	64.9	65.0	61.2	61.4	60.7
Age	38.5	38.6	38.7	38.8	39.0	39.3	33.5	35.3	34.3	33.6	32.7	32.4
Completed Secondary (%)	29.2	31.3	32.6	33.7	34.1	34.9	29.2	26.8	28.9	33.2	36.9	38.2
Any Post-Secondary (%)	28.7	27.8	28.3	28.3	29.5	30.7	20.5	19.0	20.9	26.3	26.8	26.8
Ln(Hourly Earnings)	8.38	8.37	8.37	8.39	8.41	8.42	8.24	8.21	8.13	8.09	7.97	7.97
Ln(Hours per week)	3.77	3.76	3.77	3.77	3.76	3.76	3.84	3.73	3.80	3.84	3.85	3.84
Own-Account (%)	40.3	39.6	39.1	38.9	39.4	39.0	44.4	50.5	49.7	47.1	48.1	46.8
Formal (%)	41.6	42.8	44.4	45.7	46.4	47.2	34.0	22.1	18.1	21.2	13.3	12.4
Mean Yrs Educ in Occup	9.6	9.6	9.6	9.6	9.7	9.8	8.7	8.7	8.6	8.7	8.6	8.6

Sample restricted to residents of 79 primary metro areas, age 15-64, with completed education. A migrant is defined as anyone who was living in Venezuela 5 years before the survey. Source: GEIH, population weights applied.

Table A13: Migrant Education Earnings Premia in 2015 U.S. ACS

	Colombian-Born		Venezuelan-Born	
	(1)	(2)	(3)	(4)
	Ln(Income)			
Not Completed Secondary	(excluded)	(excluded)	(excluded)	(excluded)
Completed Secondary	0.06	0.10	0.06	0.09
	(0.08)	(0.07)	(0.24)	(0.19)
Any Post-Secondary	0.47***	0.52***	0.68***	0.66***
	(0.07)	(0.07)	(0.22)	(0.17)
N	1,791	2,477	628	846
Individual Controls		X		X

Sample restricted to migrants age 15-64, who have been in the U.S. for at least 5 years and who migrated after age 18 or after age 21 if they have any post-secondary. Controls include gender, age group (in 10-year bins), and years since arrival (bins: <1, 1-4, 5-9, ≥10 years). Population weights applied. Robust SEs in parenthesis. Source: 2015 U.S. Census 1% subsample (IPUMS)

Table A14: Colombian and Venezuela 2009 PISA Score

	Colombia			Venezuela			Sample Range
	Mean	(SD)	Rank	Mean	(SD)	Rank	
Reading Literacy	413	(3.7)	56th	422	(5.3)	52nd	314-556
Math Literacy	381	(3.2)	64th	397	(4.3)	60th	331-600
Science Literacy	402	(3.6)	60th	422	(4.9)	53rd	314-556

Results from PISA 2009+. Source: Walker (2011).

Table A15: Education Wage Premia by Occupation Skill Group

	Non-Migrants	Migrants
	Occupation Mode: Not Completed Secondary	
Not Completed Secondary	(excluded)	(excluded)
Completed Secondary	19.67***	8.76***
	(0.25)	(1.42)
Any Post-Secondary	41.87***	15.08***
	(0.38)	(1.87)
	Occupation Mode: Completed Secondary	
Not Completed Secondary	(excluded)	(excluded)
Completed Secondary	28.40***	18.00***
	(0.75)	(4.90)
Any Post-Secondary	54.04***	19.61***
	(0.93)	(6.27)
	Occupation Mode: Any Post-Secondary	
Not Completed Secondary	(excluded)	(excluded)
Completed Secondary	34.17***	18.29**
	(0.80)	(7.75)
Any Post-Secondary	94.51***	59.41***
	(0.76)	(7.49)

Outcome is 100 times log hourly wage. Controlling age group (binned into 10-year intervals), gender, year FE and metro FE. Restricted to urban workers age 15-64. Source: GEIH, population weights applied.

Table A16: Population Average Migrant-Native Substitutability with Time Trend

	OLS		2SLS	
	(1)	(2)	(3)	(4)
Migrant-Native Log-Ratio	-0.115	-0.108	-0.112	-0.522
	(0.020)	(0.050)	(0.016)	(0.792)
Kleibergen-Paap Wald stat			88.84	0.46
Metro FE	X	X	X	X
Year Trend		X		X

Coefficient corresponds to $-\frac{1}{\sigma_m}$ estimated for the total population (not broken into education subgroups). Each metro-year cell is population-weighted. Metro-clustered standard errors in parenthesis.

Table A17: Migrant-Native Substitutability by Formality Status

	Pop Average	σ_{1m}	σ_{2m}	σ_{3m}
		Formal		
2SLS	12.05	19.97	18.42	8.10
	(3.93)	(18.09)	(19.55)	(6.33)
Mean Eff. F	64.4	31.5	47.7	53.0
		Informal		
2SLS	17.28	20.80	21.37	13.53
	(3.95)	(8.76)	(10.73)	(7.50)
Mean Eff. F	75.0	56.2	63.3	78.7

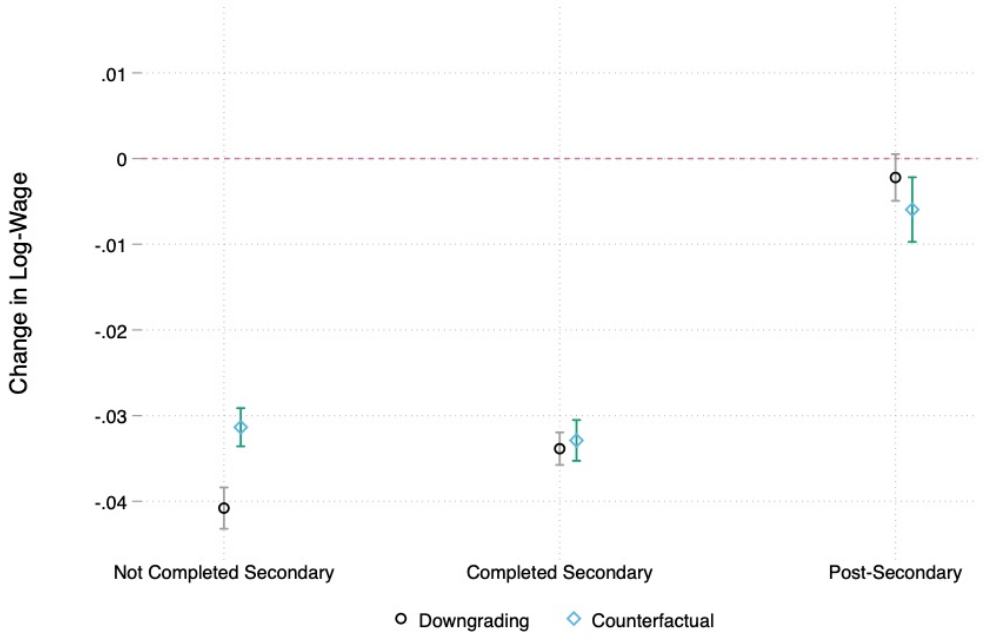
Migrants and natives restricted to indicated formality status, defined according to compliance with mandatory pension and health insurance requirements.

Table A18: 2SLS Migrant-Native Substitutability - Additional Robustness

	$-\frac{1}{\sigma_{1m}}$	$-\frac{1}{\sigma_{2m}}$	$-\frac{1}{\sigma_{3m}}$
		Only Full-Time	
2SLS	14.76	12.52	6.14
	(2.73)	(1.96)	(1.34)
Mean K-P Stat.	63.0	73.9	98.4
		Only Male	
2SLS	15.28	13.01	5.71
	(1.88)	(1.56)	(1.12)
Mean K-P Stat.	58.6	68.9	94.6
		Only Age>22	
2SLS	12.84	11.33	5.93
	(1.80)	(1.48)	(1.37)
Mean K-P Stat.	59.7	70.8	93.8
		Only Age>28	
2SLS	11.55	10.51	5.11
	(1.23)	(1.35)	(1.20)
Mean K-P Stat.	60.4	68.7	87.8
		All Working Age	
2SLS	15.45	13.71	6.50
	(2.18)	(1.91)	(1.59)
Mean K-P Stat.	55.5	72.4	89.9
		1993 Census IV	
2SLS	14.45	11.98	5.90
	(1.87)	(1.30)	(1.24)
Mean K-P Stat.	53.9	63.1	60.1

SEs bootstrapped, 100 resamples clustered at metro level, inverted using Delta method.

(a) No Capital Adjustment



(b) Full Capital Adjustment

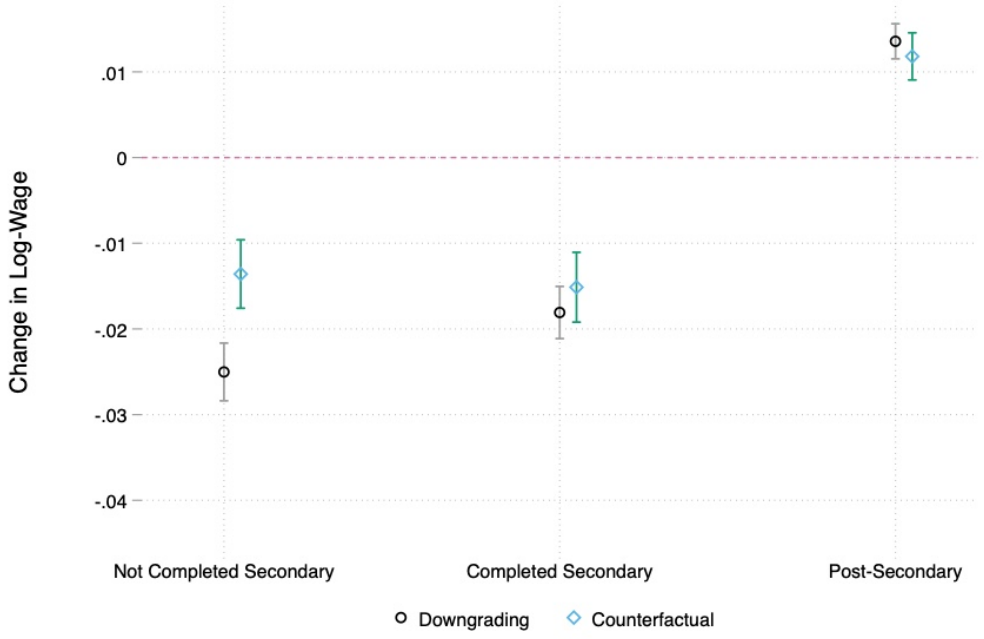


Figure A7: Total Wage Effects of Immigration

Visualizing coefficients from Table 3.7. 95% confidence intervals presented. Standard errors are calculated by simulating 1,000 draws from a joint normal distribution with the average and standard error for each parameter, and calculating the standard error for the total wage effects. σ_2 and σ_3 are taken as given.

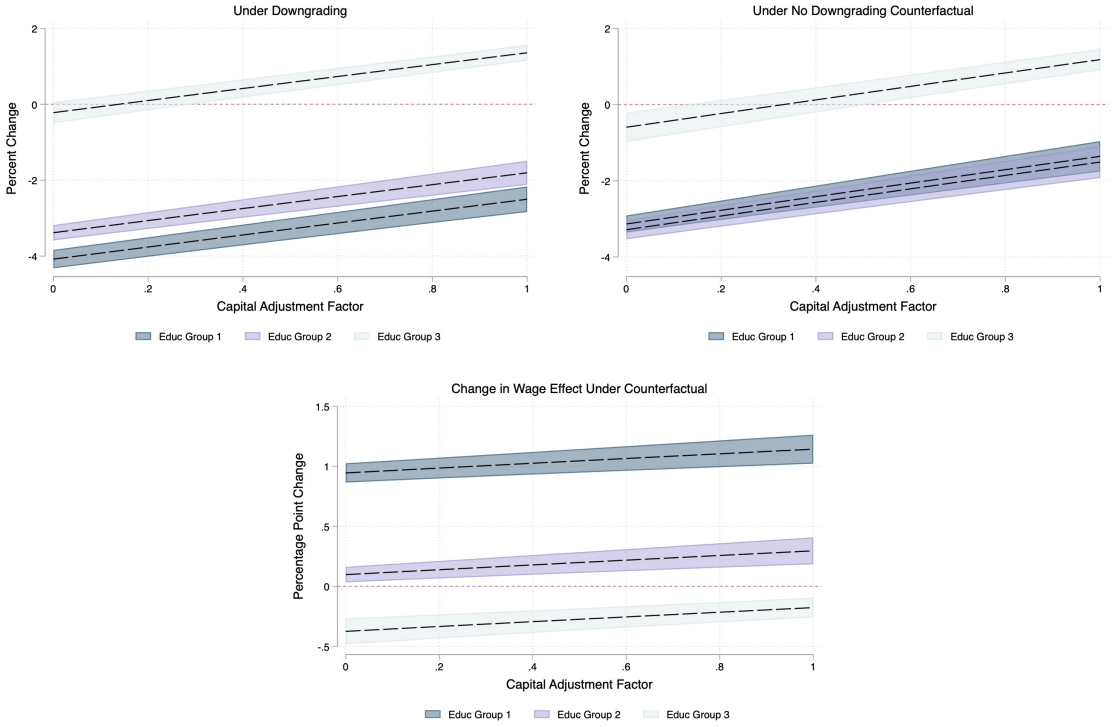


Figure A8: Wage Effects with Capital Adjustment

95% confidence intervals presented. Capital adjustment ranges from 0 (fixed capital) to 1 (grows proportionately to aggregate labor supply). Standard errors are calculated by simulating 1,000 draws from a joint normal distribution with the average and standard error for each parameter, and calculating the standard error for the total wage effects. σ_2 and σ_3 are taken as given.

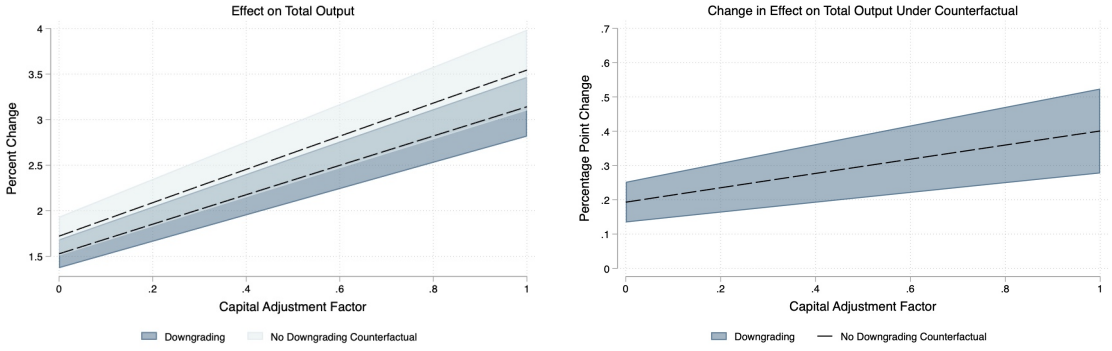


Figure A9: Effects on Total Output with Capital Adjustment

95% confidence intervals presented. See notes to previous Figure.

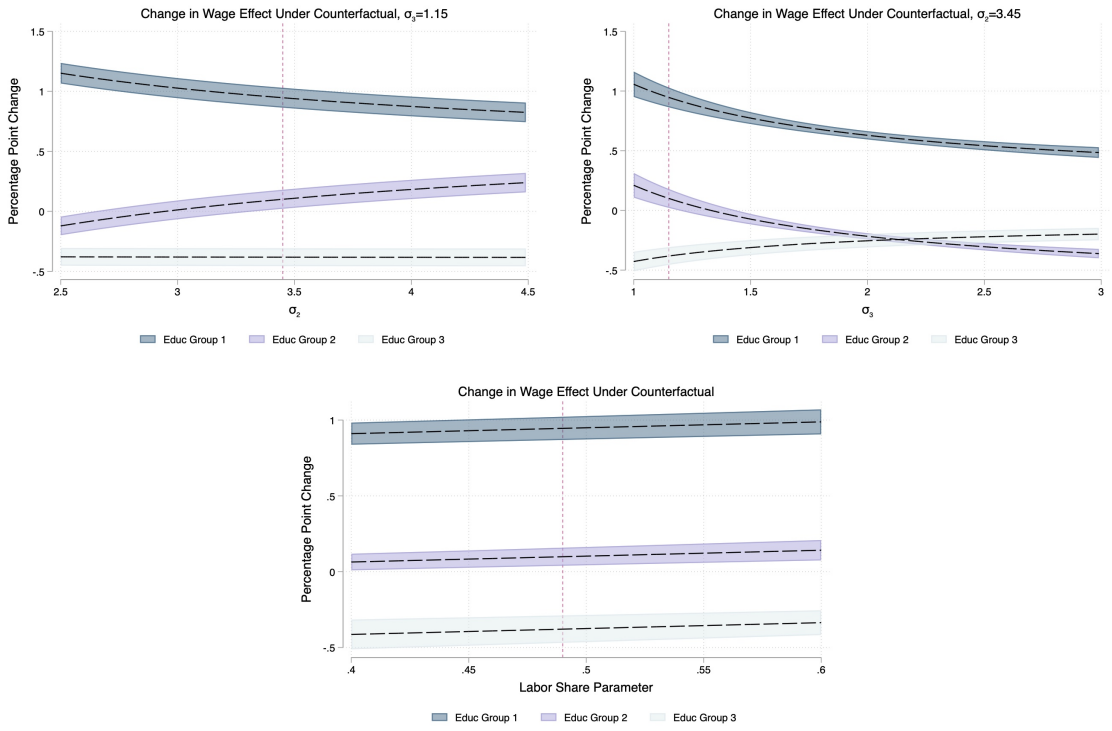


Figure A10: Total Wage Effect Sensitivity

95% confidence intervals presented. Standard errors are calculated by simulating 1,000 draws from a joint normal distribution with the average and standard error for each parameter, and calculating the standard error for the change in total wage effect going from the downgrading to the no downgrading counterfactual. σ_2 and σ_3 are fixed unless varying on the x-axis. Red vertical lines mark values from the original simulation.

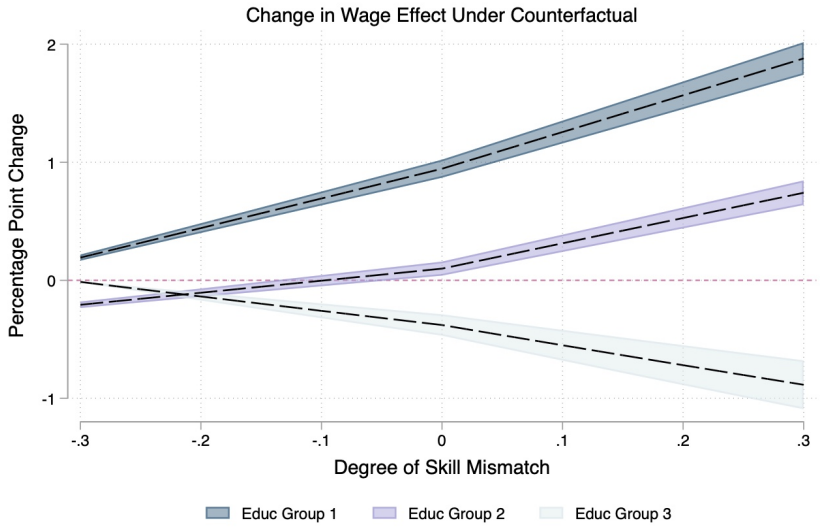


Figure A11: Sensitivity to Within-Education Migrant-Native Skill Differences

95% confidence intervals presented. Degree of skill mismatch indicates share of migrants in each education group shifted up an education group in the counterfactual (1-to-2 and 2-to-3), or down an education group (3-to-2 and 2-to-1) if negative. Standard errors are calculated by simulating 1,000 draws from a joint normal distribution with the average and standard error for each parameter, and calculating the standard error for the counterfactual wage effects. σ_2 and σ_3 are fixed.

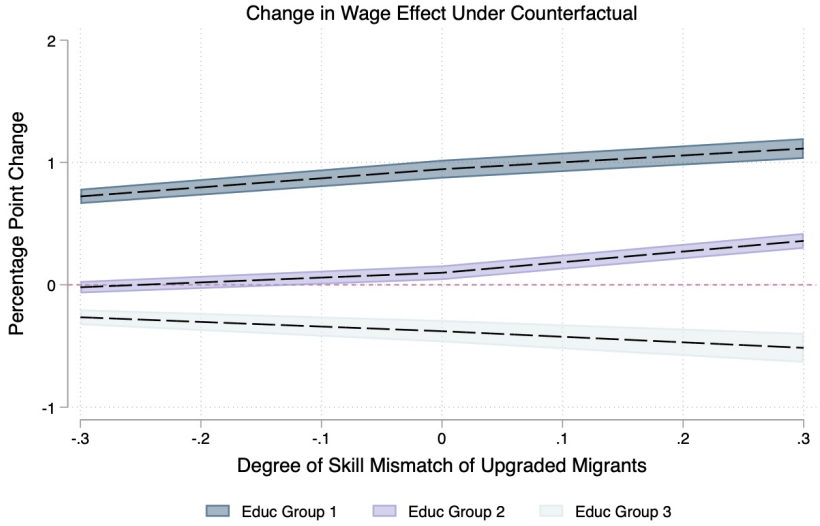


Figure A12: Sensitivity to Skill Differences Only Among Reallocated Migrants

See notes to previous Figure. Now, degree of skill mismatch indicates share of migrants *who were reallocated* to each education group who are shifted up an education group (2-to-3 if reallocated to 2), or down an education group (3-to-2 if reallocated to 3 and 2-to-1 if reallocated to 2) if negative.

Table A19: Total Effect of Immigration on Migrant Wages

	$\Delta \ln(W_{en})$ Under Downgrading	$\Delta \ln(W_{en})$ Under No Downgrading	Difference
Less than Secondary	-24.61	-21.04	3.57
	(2.82)	(2.47)	(0.35)
Completed Secondary	-26.63	-28.77	-2.14
	(2.75)	(3.01)	(0.26)
Post-Secondary	-45.90	-51.89	-6.00
	(15.11)	(16.94)	(1.83)

Calculated percent change in migrant wages from the change in population due to migration from Venezuela between 2014-2019. In parenthesis are the standard deviation of 1,000 simulations, with parameters drawn from a joint normal distribution using their estimated means and standard errors.

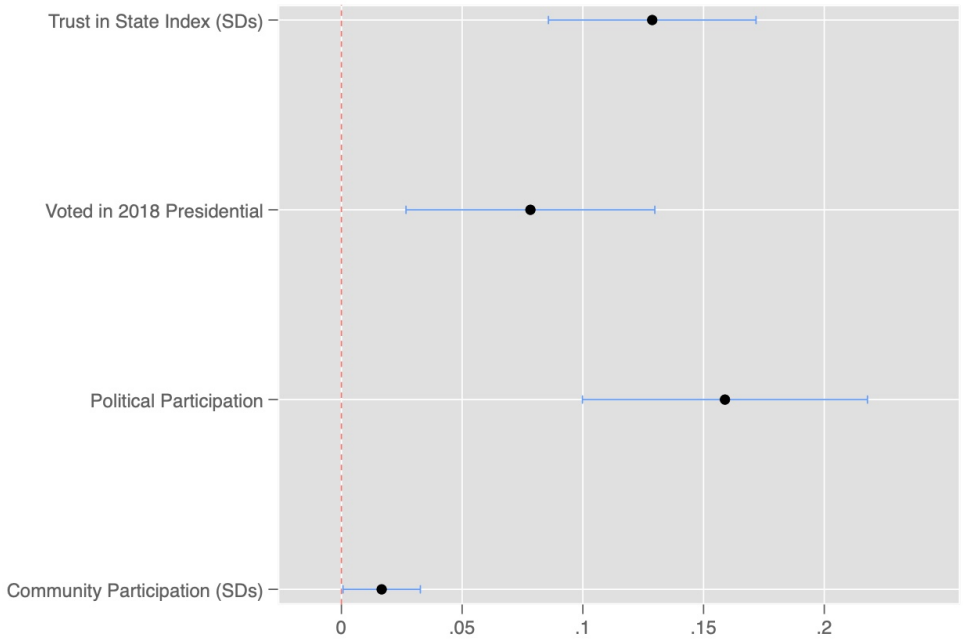


Figure A13: Foreigner Trust on 2019 ECP Outcomes with Panel

Regression of trust in foreigners on ECP variables available in previous years. Models include municipality fixed effects. Standard errors clustered at the municipality level. 95% robust CIs presented. See Data Appendix for details on variable construction.

Table A20: Effect of Immigration on Department-Level Changes in Trust

	(1) OLS	(2) 2SLS
	Δ Trust in Foreigners, 2012-2019	
Foreign Share	0.270*	0.102
	(0.144)	(0.138)
Observations	21	21
Kleibergen-Paap F		85.978

2012 (WVS) and 2019 (ECP) trust averaged at the department level using appropriate population weights. Change in trust is scaled by the standard deviation of trust in 2012. Robust standard errors in parenthesis. * p<0.10, ** p<0.05, *** p<0.01.

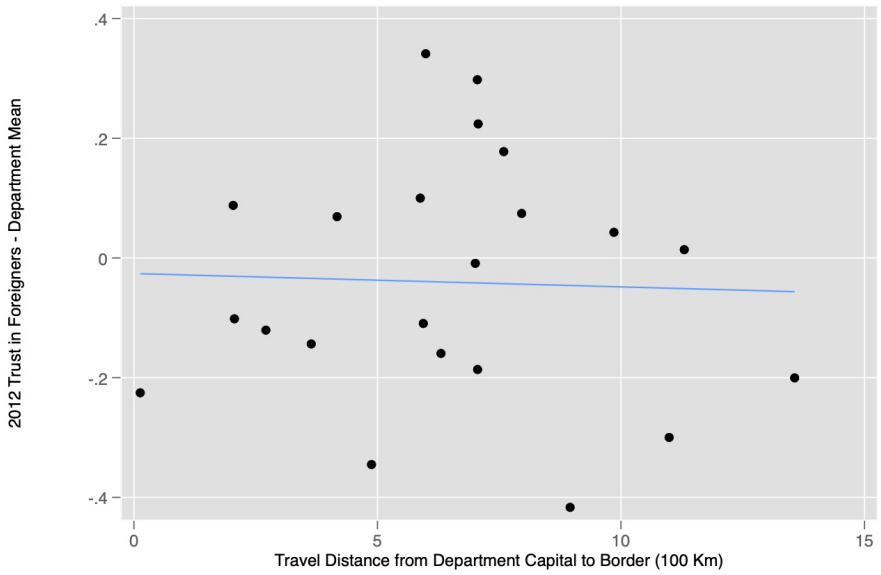


Figure A14: Instrument Exclusion - 2012 Department Trust in Foreigners (WVS)

Table A21: Pre-Period IV Balance Check - 2012 Department Trust

	(1)	(2)	(3)	(4)	(5)	(6)
			Trust in Foreigners (2012 WVS)			
Driving distance (100km) to border from:						
Deptartment Capital	-0.004			0.029		
	(0.014)			(0.024)		
Deptartment Centroid		-0.006			0.030	
		(0.016)			(0.030)	
Deptartment Pop-Weighted Centroid			-0.005			0.029
			(0.014)			(0.025)
Region FE				X	X	X
Observations	1,431	1,431	1,431	1,431	1,431	1,431

Explanatory variables are the minimum driving distance in units of 100km from the department capital, centroid or population weighted centroid of ECP municiplaities to the closest Venezuelan border crossing. Trust is measured on a scale from 1-5 and standardized to a mean of 0 and SD of 1. Department-clustered standard errors in parenthesis. * p<0.10, ** p<0.05, *** p<0.01.

Table A22: IV Balance Check - Pre-Period Municipality Characteristics

	(1) Ln(PC Value Added)	(2) Share Compl. Secondary	(3) Share Urban	(4) Infant Mort. Rate	(5) Secondary Test Scores	(6) Trash Coverage	(7) Electricity Coverage	(8) Homicide Rate	(9) Crime Rate	(10) Strong Violence (2000-2012)
Instrument	0.155**	-0.031	-0.160	0.074	0.072	-0.097	0.083	-0.036	0.110	0.009
	(0.061)	(0.105)	(0.100)	(0.152)	(0.083)	(0.145)	(0.211)	(0.113)	(0.079)	(0.040)
	[q = 0.150]	[q = 1.000]	[q = 1.000]	[q = 1.000]	[q = 1.000]	[q = 1.000]	[q = 1.000]	[q = 1.000]	[q = 1.000]	[q = 1.000]
Observations	118	118	118	118	118	118	118	118	118	118

One observation per municipality. Explanatory variable is the log driving distance from the municipality capital to the closest Venezuelan border crossing. All models include population and region fixed effects to mimic the main specification. All outcomes standardized into Z-scores except for Ln(Per-Capita Value Added and the binary indicator for whether the municipality experienced strong violence between 2000-2012. See Data Appendix for description and source of municipality-level variables. Robust standard errors in parenthesis. Sharpened false discovery rate q-values to account for multiple hypothesis testing (of 10 outcomes) are presented below each standard error. * p<0.10, ** p<0.05, *** p<0.01.

Table A23: IV Balance Check - 2019 ECP Individual Characteristics

	(1) Female	(2) Age (SDs)	(3) Yrs Educ (SDs)	(4) Completed Secondary	(5) Completed Primary	(6) In Municipality Capital	(7) Rural
Instrument	-0.007**	0.014	0.016	0.007	0.005	0.015	-0.009
	(0.003)	(0.010)	(0.025)	(0.010)	(0.010)	(0.017)	(0.020)
	[q = 0.301]	[q = 1.000]	[q = 1.000]	[q = 1.000]	[q = 1.000]	[q = 1.000]	[q = 1.000]
Observations	42,723	42,723	42,620	42,723	42,723	42,723	42,723

Explanatory variable is the log driving distance from the municipality capital to the closest Venezuelan border crossing. All models include population and region fixed effects to mimic the main specification. Municipality-clustered standard errors in parenthesis. Sharpened false discovery rate q-values to account for multiple hypothesis testing (of 7 outcomes) are presented below each standard error. * p<0.10, ** p<0.05, *** p<0.01.

Table A24: Ordered Logit

	(1) OLS	(2) OLS	(3) OLS	(4) 2SLS	(5) 2SLS
	Trust Foreigner				
Foreign Share	1.150***	1.108**	1.124*	1.022	1.015
	(1.051)	(1.044)	(1.067)	(1.071)	(1.070)
	Trust Generalized				
Foreign Share	1.128***	1.061	1.054	1.012	0.959
	(1.030)	(1.048)	(1.125)	(1.046)	(1.105)
	Trust Limited				
Foreign Share	1.101*	1.014	0.965	1.000	0.942
	(1.052)	(1.050)	(1.056)	(1.066)	(1.089)
	Not Want Nbr Immigrant				
Foreign Share	0.870	0.957	1.170	0.910	1.101
	(1.095)	(1.090)	(1.132)	(1.251)	(1.226)
Region FE		X	X	X	X
Ind/Mun Controls			X		X
Observations	41,850	41,850	41,850	41,850	41,850

Odds ratios presented. Standard errors bootstrapped and clustered by municipality. For 2SLS estimates, a control function approach is used. * p<0.10, ** p<0.05, *** p<0.01.

Table A25: Correlation Matrix: Variables Used in Heterogeneity Analysis

	Share Urban	Value Added Per-Capita	Public Goods Index	Interaction Index	Crime Rate	Change in Crime Rate (2014-2017)	Heavy Violence (2000-2012)	Mean Industry Exposure
Share Urban	1.00							
Value Added Per-Cap	0.30	1.00						
Public Goods Index	0.60	0.48	1.00					
Interaction Index	0.14	-0.20	-0.02	1.00				
Crime Rate	0.63	0.43	0.55	-0.03	1.00			
Change Crime Rate	-0.56	-0.20	-0.38	0.03	-0.73	1.00		
Heavy Violence	0.38	0.18	0.20	0.07	0.49	-0.48	1.00	
Mean Ind. Exposure	0.43	0.12	0.32	0.21	0.14	-0.14	0.07	1.00

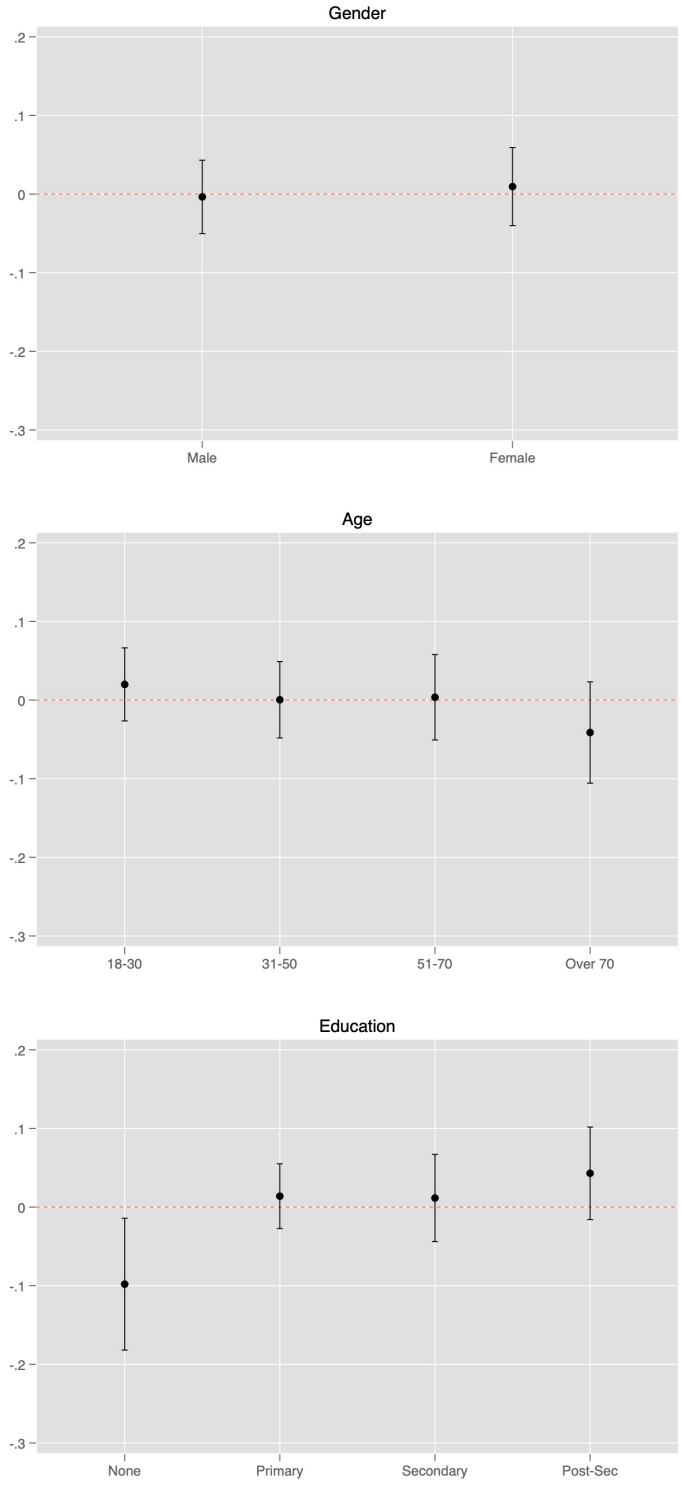


Figure A15: Heterogeneity - Trust in Foreigners

2SLS linear probability model coefficients and 95% municipality-clustered CIs presented. All controls included, trust is standardized into a Z-score.

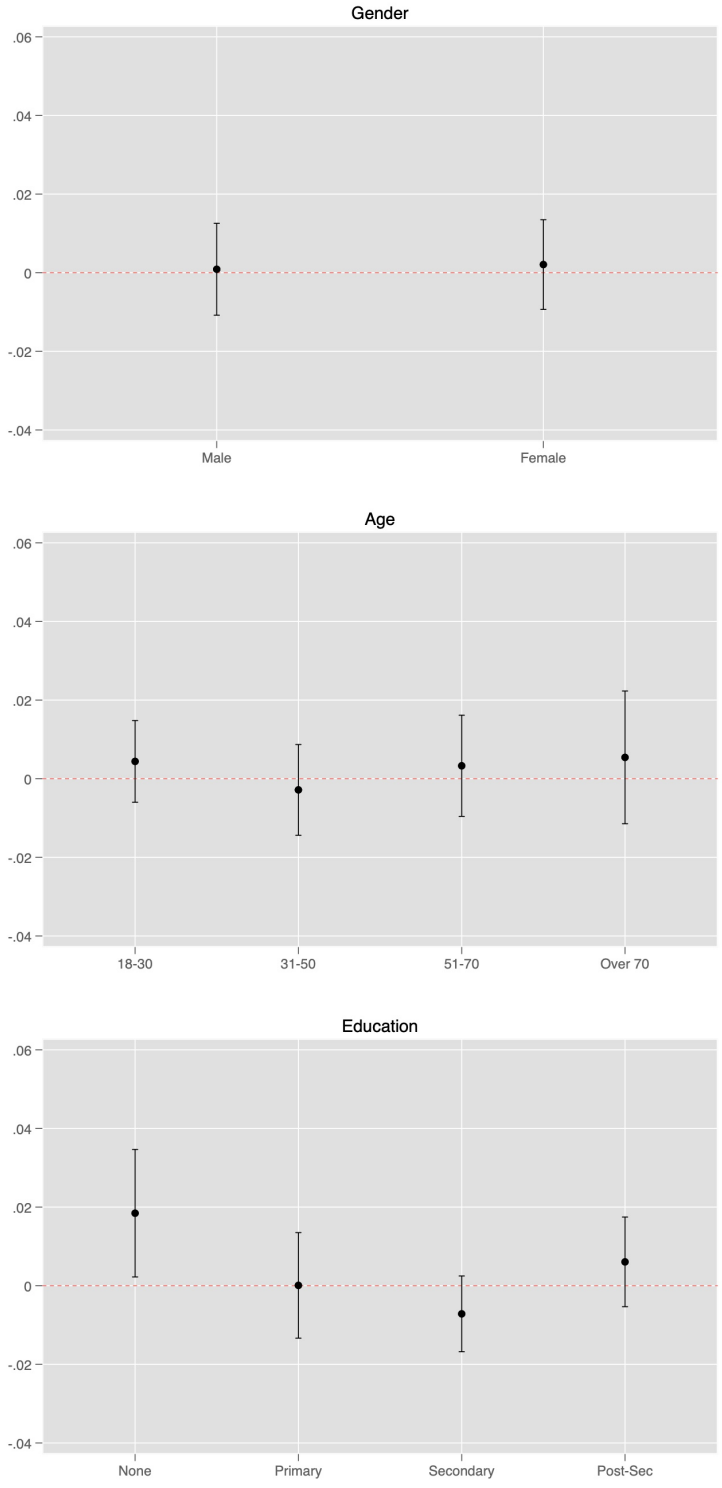


Figure A16: Heterogeneity - Preference to Not Have an Immigrant Neighbor

2SLS linear probability model coefficients and 95% municipality-clustered CIs presented. All controls included, trust is standardized into a Z-score.

Table A26: Robustness Checks

	(1) Original Model		(2) Control 2015 ECP Outcomes		(3) Control 2005 Venezuelan Share		(4) Drop <200km From Border	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
	Trust Foreigner							
Foreign Share	0.051**	0.003	0.055***	-0.002	0.049*	-0.028	0.085***	0.014
	(0.023)	(0.023)	(0.020)	(0.022)	(0.025)	(0.030)	(0.031)	(0.047)
Observations	41,850	41,850	37,417	37,417	41,850	41,850	39,048	39,048
Kleibergen-Paap F		139.540		164.685		74.393		25.729
	(5) Drop Bogota		(6) Drop Internal Migrants		(7) Straight-Line Distance IV		(8) Include Col-Born	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
	Trust Foreigner							
Foreign Share	0.046**	0.008	0.051**	0.001	0.051**	0.004	0.042**	0.002
	(0.021)	(0.022)	(0.024)	(0.023)	(0.023)	(0.028)	(0.018)	(0.018)
Observations	36,582	36,582	38,390	38,390	41,850	41,850	41,850	41,850
Kleibergen-Paap F		136.296		145.918		75.028		167.334

Trust outcomes are standardized into Z-scores. Municipality-clustered standard errors in parenthesis. All models include log-population, region fixed effects and individual and municipality controls (see Data Appendix for full list). * p<0.10, ** p<0.05, *** p<0.01.

Table A27: Heterogeneity Robustness Checks: Trust in Foreigners

Heterogeneity By:	(1) Share Urban	(2) Public Goods Index	(3) Interaction Index†
		Original Model	
Foreign Share	-0.139**	-0.017	-0.379**
	(0.062)	(0.038)	(0.174)
Foreign Share × Heterogeneity Variable	0.215***	0.110**	0.136**
	(0.063)	(0.052)	(0.053)
Test: Foreign Share × Het. Var. = 0	[p = 0.001]	[p = 0.035]	[p = 0.010]
		Control 2015 ECP Outcomes	
Foreign Share	-0.207***	-0.117**	-0.459***
	(0.073)	(0.053)	(0.166)
Foreign Share × Heterogeneity Variable	0.284***	0.212***	0.088**
	(0.071)	(0.074)	(0.043)
Test: Foreign Share × Het. Var. = 0	[p = 0.000]	[p = 0.004]	[p = 0.042]
		Control 2005 Venezuelan Share	
Foreign Share	-0.191**	-0.023	-0.460***
	(0.081)	(0.041)	(0.175)
Foreign Share × Heterogeneity Variable	0.272***	0.090	0.099
	(0.092)	(0.071)	(0.067)
Test: Foreign Share × Het. Var. = 0	[p = 0.003]	[p = 0.203]	[p = 0.140]
		Drop <200km From Border	
Foreign Share	-0.227	0.038	-0.596**
	(0.162)	(0.062)	(0.250)
Foreign Share × Heterogeneity Variable	0.414**	0.309***	-0.205
	(0.193)	(0.117)	(0.131)
Test: Foreign Share × Het. Var. = 0	[p = 0.032]	[p = 0.008]	[p = 0.117]
		Drop Bogota	
Foreign Share	-0.142**	-0.028	-0.383**
	(0.062)	(0.036)	(0.183)
Foreign Share × Heterogeneity Variable	0.212***	0.115**	0.145**
	(0.062)	(0.051)	(0.060)
Test: Foreign Share × Het. Var. = 0	[p = 0.001]	[p = 0.025]	[p = 0.015]
		Drop Internal Migrants	
Foreign Share	-0.149**	-0.012	-0.377**
	(0.065)	(0.038)	(0.176)
Foreign Share × Heterogeneity Variable	0.226***	0.098*	0.133**
	(0.066)	(0.051)	(0.054)
Test: Foreign Share × Het. Var. = 0	[p = 0.001]	[p = 0.055]	[p = 0.014]
		Straight-Line Distance IV	
Foreign Share	-0.150*	-0.070	-0.630**
	(0.081)	(0.068)	(0.302)
Foreign Share × Heterogeneity Variable	0.241***	0.194**	0.186**
	(0.082)	(0.093)	(0.089)
Test: Foreign Share × Het. Var. = 0	[p = 0.003]	[p = 0.037]	[p = 0.036]
		Include Colombian-Born Migrants on RHS	
Foreign Share	-0.068*	-0.016	-0.215*
	(0.036)	(0.032)	(0.113)
Foreign Share × Heterogeneity Variable	0.122***	0.088**	0.090**
	(0.035)	(0.042)	(0.040)
Test: Foreign Share × Het. Var. = 0	[p = 0.000]	[p = 0.037]	[p = 0.023]
Ind/Mun Controls	X	X	X

All regressors fully interacted with heterogeneity variable. All of the heterogeneity variables are standardized into Z-scores, such that "Foreign Share" measures the treatment effect at the mean. See Data Appendix for description of heterogeneity variables and full list of controls. Municipality-clustered standard errors in parenthesis. * p<0.10, ** p<0.05, *** p<0.01.

† Sample restricted to urban areas

Table A28: Placebo Test: 2SLS Estimates for 2015 ECP Outcomes

	(1) Trust in State Index (SDs)	(2) Voted in 2014 Presidential	(3) Political Participation	(4) Community Participation (SDs)
Foreign Share	-0.001	0.010	0.004	0.022
	(0.034)	(0.007)	(0.004)	(0.024)
Observations	31,805	32,441	32,441	32,441

ECP 2015 sample restricted to urban areas. Controls include all municipality controls and region fixed effects. Municipality-clustered standard errors in parenthesis. * p<0.10, ** p<0.05, *** p<0.01.

Table A29: Heterogeneity Placebo Test: 2SLS Estimates for 2015 ECP Outcomes

Heterogeneity By:	(1) Share Urban	(2) Public Goods Index	(3) Interaction Index
	Trust in State Index (SDs)		
Foreign Share	-0.070	-0.112**	0.022
	(0.074)	(0.053)	(0.231)
Foreign Share × Heterogeneity Variable	0.062	0.160	-0.123**
	(0.082)	(0.100)	(0.059)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 0.194]	[q = 0.122]	[q = 0.122]
	Voted in 2014 Presidential		
Foreign Share	0.027*	0.009	0.036
	(0.015)	(0.012)	(0.054)
Foreign Share × Heterogeneity Variable	-0.012	0.031	-0.004
	(0.016)	(0.025)	(0.015)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 1.000]	[q = 1.000]	[q = 1.000]
	Political Participation		
Foreign Share	0.014*	0.016***	0.014
	(0.008)	(0.006)	(0.030)
Foreign Share × Heterogeneity Variable	-0.014	-0.016	0.003
	(0.010)	(0.012)	(0.009)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 0.411]	[q = 0.411]	[q = 0.411]
	Community Participation (SDs)		
Foreign Share	-0.029	0.023	-0.046
	(0.072)	(0.046)	(0.142)
Foreign Share × Heterogeneity Variable	0.076	0.034	0.074
	(0.078)	(0.092)	(0.047)
Test: Foreign Share × Het. Var. = 0 (FDR Sharpened q-values)	[q = 0.541]	[q = 0.904]	[q = 0.541]
Mun Controls	X	X	X
Observations	31,805	31,805	31,461
Kleibergen-Paap F	8.403	15.469	16.147

All regressors fully interacted with heterogeneity variable. All of the heterogeneity variables are standardized into Z-scores, such that "Foreign Share" measures the treatment effect at the mean. See Data Appendix for description of heterogeneity variables and full list of controls. Municipality-clustered standard errors in parenthesis. * p<0.10, ** p<0.05, *** p<0.01.

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Biography

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