The Fiscal Impact of Immigration: Labor Displacement, Wages, and the Allocation of Public Spending*

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Abstract

We reexamine the effect of immigration on public finances by accounting for second-order effects. We exploit exogenous variation in immigration across Colombian metropolitan areas between 2013 and 2018, resulting from the large increase in Venezuelan immigrants, and instrument immigrants' residential location using pre-existing settlement patterns and the distance between origin-destination flows. We find that immigration did not reduce natives' average fiscal contributions. Exploring the mechanisms in place, we document that immigration had no effect on employment, average wages in the upper half of the wage distribution, or hours worked that would have explained changes in labor-driven tax contributions. In addition, immigration did not trigger a decline in property values, and we find no evidence of changes in the composition of local public spending or costs being distributed among a larger population. This suggests that the composition of public expenditures is biased towards services that are rival in consumption and thus increase with population. Results in this paper indicate that, even when finding evidence of labor displacement or negative factor prices, immigration does not necessarily reduce natives' average tax contributions.

Keywords: immigration; public finances; public goods; employment; wages; property values.
JEL Codes: F22, H41, H72, J15, J61, R31.

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1. Introduction

With rising debt and deficits across many developed and developing countries, immigrants are often perceived to be a burden for public finances among native-born residents of immigrant-receiving countries. This has been the case even when the empirical evidence has shown for several countries that the net (direct) fiscal impact of immigration is usually less than 0.5% of GDP for a given year, regardless of whether it is positive or negative (OECD, 2013). For instance, total immigration in Colombia is estimated to generate a small fiscal benefit (Mesa-Guerra & Ramírez-Tobón, 2022), but consistently more than half of residents consider immigration to be a fiscal burden. Does the perception of natives reflect other indirect effects of immigration that affect their tax contributions and welfare benefits? Are these effects being omitted in the conventional fiscal accounting exercises, overestimating the positive fiscal impact of immigrants—or underestimating their effect when they don't do so well?

The conventional approach in the empirical literature that estimates the impact of immigration on public finances using an accounting approach has ignored, for example, the potential labor market equilibrium effects and interdependency in the level of public goods that individuals consume. Results derived from first-order fiscal effects, *i.e.*, the difference between the contributions immigrants make in the form of taxes and the government expenditures they receive, assume no impact on the wages and employment of natives, changes in the return to capital, fiscal benefits from economic growth, or lower per capita costs of providing public goods that are distributed over a larger population. By ignoring these second-order effects, it is implicitly assumed that the economy is able to absorb immigration without affecting the fiscal contributions of natives.

In this paper, we reexamine the effect of immigration on public finances by addressing these second-order effects. Specifically, we look at labor displacement, changes in factor prices (labor and capital), and changes in the allocation and cost of providing public services. To guide our empirical analysis, we present a simple theoretical framework that allows us to derive the implications of changes in population driven by increasing immigration on natives' net fiscal contributions. We use this framework to explain differences in fiscal contributions for natives and set the ground for exploring the mechanisms in place. We show that the effect of increasing immigrant inflows on the net fiscal contributions of native workers depends on three terms. First, a *factor price effect* that captures changes in taxes paid that result from both labor displacement and variations in wages, as part of the labor market adjustment, and changes in revenue from capital income. Second, the change in the cost of government service provision. This *cost effect* may be driven by the mix of goods and services provided (pure, congested, or private goods), changes in the composition of the population, and the overall increase in the local population. Third, changes in the *composition* of government expenditures and differences in the demand for government services between natives and immigrants.

Empirically, we exploit exogenous variation in immigration across Colombian cities with their metropolitan areas between 2013 and 2018, resulting from the significant increase in Venezuelan immigrants. This includes both native-born returnees and Venezuelan-born immigrants. Based on individual-level estimates of tax payments and benefits received from the use of public goods and services—such as education or health care—and welfare, we present reduced-form evidence of the effect of changes in the stock of immigrants on natives' per capita net fiscal contributions. We then explore the three mechanisms that are consistent with our theoretical framework.

To address the potential endogeneity from immigrants' self-selecting into local labor markets and measurement error from small sample sizes used to construct the fraction of immigrants at the regional level, we follow the literature and use two shift-share instruments. The first instrument uses predicted immigrant inflows based on pre-existing settlement patterns from the 2005 Census (Altonji & Card, 1991; Card, 2001). This instrument is constructed using historic settlement shares, using the well-known fact that immigrants tend to locate disproportionately near previous immigrants with similar backgrounds. The second instrument uses the distance between origin-destination flows. The instrument predicts the number of immigrants that would have arrived in a particular city and year based on the regional origin-distribution of aggregate inflows and the travel distance between the centroid of the capital for each Venezuelan province and the centroid of the capital city in each Colombian department (Del Carpio & Wagner, 2015; Caruso *et al.* 2019; Delgado-Prieto, 2022). Since Colombia and Venezuela share a 2,200 km border with multiple regular and irregular border crossings, distance is a key determinant of immigrants' location decisions.

Our results show no evidence that increasing immigrant inflows to Colombia lowered the average per capita contributions of natives between 2013 and 2018. These effects stay unchanged when we exclude revenues and expenditures not derived from individual behavior, such as corporate taxes, gross operating surplus, royalties, 'pure' public goods, and debt service. However, when we run yearly regressions separately, we see a small negative effect in 2017, when the pace of immigrant inflows increased exponentially. Nevertheless, the implied effects are relatively small, roughly equivalent to about 0.3% of GDP per capita.

We also find no indication of a significant effect in average tax contributions and a positive effect on expenditures only when we allow for differential trends in fiscal contributions across areas, although not distinguishable from zero at the 5% significance level based on the identification-robust Anderson–Rubin test. These estimates are robust to constructing the enclave instrument using 1993 immigrant shares, normalizing the fraction of immigrants in each area by the local population in 2013 to reduce potential bias from native supply changes (see Card & Peri, 2016), estimating the effects using the pooled sample, using the distance-based instrument, accounting for dynamic bias (see Jaeger *et al.*, 2019), and using a limited information maximum likelihood (LIML) estimator that has better small sample properties.

When we disaggregate the effects by types of revenues and expenditures, we find a slight drop in indirect tax contributions and an increase in health care benefits. The former is explained by a decline in wages for low-skilled workers, mainly at the bottom of the wage distribution. However, we believe these effects are too small to drive large changes in fiscal contributions, mainly since we do not find evidence of labor displacement. In addition, we find no evidence of costs being distributed among a larger population and no significant change in local spending that could be explained by increasing immigrant inflows. However, we find evidence that welfare take-up by natives increased by 1.7%, driven by a higher probability of receiving subsidized health. These results have three implications. First, the composition of public expenditures is biased towards services that tend to increase with population. Second, local governments have had to stretch their resources to meet the increasing demand as they face low budget flexibility. Third, the increase in demand for welfare benefits has put more pressure on resources coming from the National government.

This paper builds on the extensive literature on the fiscal effects of immigration. While the evidence on the direction of the effect is mixed, as one would expect from the analysis of very different settings, the evidence on the size of the effect has shown it to be fairly small compared to the size of the overall fiscal imbalance and the economy (OECD, 2013). Our paper is more closely related to the strand of research that has used a static fiscal accounting framework, *i.e.*, estimating the differences between the value of taxes and other contributions immigrants make to revenues and the value of expenditures received in the form of public services and welfare. Dustmann & Frattini (2014), using a repeated cross-sectional approach, show that immigrants to the UK since 2000 have contributed more to taxes than they have added to expenditures. In an extensive study of immigration in the U.S., the report by the National Academies of Sciences, Engineering, and Medicine (2017) shows that while first-generation immigrants (*i.e.*, foreign-born immigrants) have had lower fiscal contributions relative to the other generations, mainly at the state and local levels, second-generation immigrants (*i.e.*, immigrants' children) are among the larger fiscal contributors in the population. Recent evidence for developing countries finds considerable variation in the relative net fiscal contributions between immigrants and natives and shows that the per-capita effect is relatively large (OECD/ILO, 2018).

Using a similar approach, Mesa-Guerra and Ramírez-Tobón (2021) provide evidence of the fiscal effect of the recent Venezuelan immigration to Colombia. Their results indicate that the lower fiscal contributions of immigrants—relative to natives—are driven by recent arrivals who have less attachment to the labor market. However, immigrants that have been in the country for more than a year show equal or slightly higher per capita fiscal contributions than natives after controlling for differences in the age profile and other individual characteristics.

Our contribution to this literature is to account for second-order effects. We account for changes in natives' employment and wages and changes in the cost and allocation of public spending at the local level. While immigration can improve natives' fiscal position in the sense that 'pure' public goods can be provided at the same level while sharing the costs among the whole population—at least in the short run when the marginal cost is lower than the average cost, it can have a negative effect by reducing tax payments from natives as a result of job loss or lower wages. Our findings show that the wage response across skill groups is not uniform. We find negative and significant effects on wages between the 15th and 50th percentiles and for low-skilled and self-employed workers. However, because the tax contributions of these groups tend to be low, it is not unusual to see no change in the overall fiscal effect as a response to immigration.

Finally, a different literature has documented the relationship between states or cities' ethnic or racial composition and public spending. For example, Alesina *et al.* (1999) show that the allocation of spending to productive public goods (*e.g.*, education, roads, sewage, waste disposal) in U. S. cities is lower when the ethnic fragmentation of the city increases. More recently, Tabellini (2019) finds that the migration of blacks from southern cities to northern cities during the Great Migration had a negative impact on both public spending and tax revenues in northern cities, driven by a reduction in property values. However, while changes in the level of public spending do not necessarily translate into changes in the composition of spending, natives may be less willing to redistribute when facing an increase in ethnic diversity. This could potentially reduce the supply of local public goods (Dahlberg *et al.*, 2012; Munshi & Rosenzweig, 2018). Our work adds to this literature by analyzing the short-term effects of a large international migration event on the composition of local government expenditures, welfare take-up, and changes in property values and rents.

The remainder of the paper is organized as follows: Section 2 presents the theoretical framework. Section 3 presents the data and explains the context. Section 4 presents the empirical strategy and discusses the identification assumptions. Section 5 presents the reduced-form evidence of the effect of immigrant inflows on natives' fiscal contributions, including the robustness checks. Section 6 examines the mechanisms that explain our main findings. Section 7 discusses the implications of our results to the understanding of the overall fiscal effects of immigration. Section 8 concludes.

2. Theoretical Framework

We begin by laying out a theoretical framework to interpret the empirical results that follow. We present a simple model of equilibrium aggregate tax contributions and government services consumed by both natives and immigrants. The starting point is the static framework in Preston (2014).

Suppose the economy consists of an endogenous measure L of workers who each pay a tax $T(w)$ and consume a range of $k \in \{1, \dots, K\}$ government services G_k. Wages (w) are determined by equating aggregate demand for labor l from perfectly competitive firms with aggregate supply L. We don't model unemployment. Instead, we assume all non-hired labor exit the economy.

Denote the budget cost of providing the k public service by $E_k(G_k, L)$. This depends both on the nature of the service and size of the population. If the government service is a pure public good, then $\partial E_k/\partial L = 0$, and so its provision is independent of population size. If the service has either the characteristics of a private good or shows congestion in consumption, then $\partial E_k/\partial L > 0$. However, in the case of congested public goods, as opposed to private goods where the cost increases proportionately with the number of consumers, at low levels of consumption, the marginal cost of provision may be below the average cost, but at high levels of consumption (high population size), the cost of provision increases more than proportionately with population.

Suppose the population comprises both native workers (N) and immigrant labor (M). Denote the fraction of immigrants as $\theta = M/L$ so that overall population size can be written as $L = N/(1-\theta)$. The government budget balances if

$$NT(w) + MT(w) - \sum_k E_k(G_k, L) = 0 \tag{1}$$

and natives' net fiscal contributions can be written as

$$NT(w) - \sum_k s_k(\theta) \cdot E_k(G_k, L) = D, \tag{2}$$

where D is a resulting constant and $s_k(\theta)$ is the share of the k public service used by natives.

Assume now that the cost of provision of the kth public service is a function of the mean service use such that $G_k = (1-\theta)G_k^N + \theta G_k^M$, where G_k^N and G_k^M are the average service use by natives and immigrants, respectively. Differentiating Eq. (3) with respect to the fraction of immigrants (θ) shows that immigration is fiscally beneficial for natives if

$$N\frac{\partial T}{\partial w}\frac{\partial w}{\partial \theta} - \sum\nolimits_k \left[\frac{\partial s_k}{\partial \theta} E_k(G_k, L) + s_k\left(\frac{\partial E_k}{\partial G_k}\frac{\partial G_k}{\partial \theta} + \frac{\partial E_k}{\partial L}\frac{\partial L}{\partial \theta}\right)\right] > 0\,. \tag{3}$$

In other words, immigration can improve natives' fiscal position in the sense that either changes in natives' tax contributions compensate for higher costs of providing public services to a more diverse population or by taxes falling less than the spread in costs from an increase in population size. This can be seen more clearly by setting $\theta = 0$ and working around terms. We can now express natives' fiscal impact from an increase in immigration as

$$\underbrace{\left[\frac{\partial T}{\partial w}\frac{\partial w}{\partial \theta} - T\right]}_{factor\ price\ effect} + \underbrace{\sum_k \left[\frac{E_k(G_k, L)}{L} - \frac{\partial E_k}{\partial L}\right]}_{cost\ \ effect} + \underbrace{\frac{1}{N}\sum_k \left[\frac{G_k^M}{G_k^N} E_k(G_k, L) - \frac{\partial E_k}{\partial G_k}\left(G_k^M - G_k^N\right)\right]}_{composition\ and\ service\ use\ effect} \lesseqqgtr 0\,. \quad (4)$$

This expression depends on three terms:

(i) *Factor price effect.* This captures the change in taxes paid by natives from an increase in immigration. In this simplified version, the size and sign of this term will depend on the average wage effect. However, one could expect the overall effect to be different based on the distribution of the native population along the wage distribution. More broadly, this term captures labor marker equilibrium effects, both at the extensive and intensive margins, and changes in revenue from capital income.

(ii) *Cost effect.* This captures the change in the cost of government service provision from a change in the population size. Therefore, the direction of the effect depends on the combination of government services. Note that in the case of public goods—where the level of overall provision of public services is usually not affected by population growth, immigration is beneficial for natives by distributing the cost among a larger population.

(iii) *Composition and service use effect.* This term captures changes in the composition of government expenditures and differences in the demand for government services between natives and immigrants.

3. Data and Background

3.1. Data

Our main data comes from Colombia's Labor Force Survey (*Gran Encuesta Integrada de Hogares—GEIH)* for 2013–2018. The GEIH is a nationally representative household survey that collects information on labor market conditions. The survey is also representative for the 23 main metropolitan areas (MSAs), which account for up to 50% of the population and 65% of all immigrants. This survey has two crucial advantages. First, it contains information on the geographic distribution of Venezuelan immigrants across MSAs, allowing us to use empirical specifications that rely on variation across geographic units. Second, immigrants are surveyed regardless of their migratory status (regular or irregular).[1] We define *Venezuelan immigrants* as all individuals born in Venezuela plus all individuals born in Colombia who lived in Venezuela for 1 or 5 years before being surveyed.[2] We refer to the latter group as native-born returnees or just *returnees*. Therefore, throughout the document, *natives* correspond to all native-born, excluding returnees.

We use the self-reported information in the GEIH to estimate tax contributions to and benefits received from the Central Government, the social security sector, and local governments for all individuals aged 15 to 64. The raw data on all revenues and expenditures of the General Government between 2013 and 2018 comes from Mesa-Guerra and Ramírez-Tobón (2022).[3] This data includes information on all levels of government for 14 revenue groups (*e.g.*, income tax, wealth tax, indirect taxes, property tax) and 15 expenditure groups (*e.g.*, 'pure' public goods, health services, compulsory education, social protection). In the Online Appendix, we describe in

[1] We follow the terminology used by the Colombian government to classify immigrants. Undocumented or irregular immigrants do not satisfy the requirements established by the host country to enter or remain in the country.
[2] As is standard in the literature on the fiscal effects of immigration, we classify all native-born dependents (*e.g.*, children aged 18 or less) as immigrants if the head of the household (*e.g.*, parent) is classified as an immigrant. Because we restrict our sample to all those aged 15 to 64 and since our immigration shock is quite recent, excluding native-born dependents does not affect our estimates.
[3] The General Government consist of the National government, regional and local governments, and the social security sector.

detail how we estimate the fiscal contribution for each individual observation in the GEIH. We now provide a summary.

Revenues. To estimate personal income tax, we apply year-specific tax rates to reported income, including capital income. Social insurance and payroll taxes are estimated by applying the tax schedule based on an individual's affiliation status to a pension fund and a health regime. Contributions from VAT and other indirect taxes are estimated using decile-specific effective tax rates from Mesa-Guerra and Ramírez-Tobón (2022) and applying them to gross household income. Property tax and wealth tax contributions are estimated using proxies for asset ownership and estimates of property values. Other contributions such as motor vehicle, industry, and phone taxes are allocated using the information of ownership of a vehicle (car or motorcycle), registered business, or landline. In the case of the financial transaction tax, we apply estimates of the expenditures-to-income ratio by decile to individual income reported in the GEIH and use the tax exemption threshold to allocate revenues. All other sources of revenues are allocated on a per capita basis using eligibility information in the GEIH, as described in detail in the Online Appendix.

Expenditures. To allocate government expenditures, we followed a simple rule: all self-reported income or subsidies from government agencies are taken as such, and the rest are assigned on a per capita basis using individual eligibility. In the first group lies all spending on welfare or social protection programs: sickness and disability, old age, family and children, unemployment, housing, and vulnerable population. The second group comprises public goods (rival and non-rival in consumption), law courts and prisons, water supply, health, education, and debt service. Public goods are assigned per capita; law and prison spending is assigned using the share of the prison population by origin country; water spending is allocated using the population with access to the water supply system; health services are allocated using the individual health cost by age group for each regime; education services are allocated based on enrollment, differentiating between compulsory education, job training, and higher education; debt service is allocated per capita but conditioning on the year of arrival to the country.

Table 1 shows some descriptive statistics for natives and Venezuelan immigrants aged 15 to 64, divided by group. It stands out the fact that Venezuelan immigrants went from representing around 0.2 percent of the population in 2013 to representing 3.1 percent of the population by 2018. A first look at the demographic characteristics of natives and Venezuelan immigrants aged 15 to 64 suggests that the latter are younger and have more years of schooling. However, this is true for Venezuelan-born immigrants who are, on average, seven years younger and have an additional year of education relative to natives. In contrast, as of 2018, Colombian-born returnees were slightly older and had lower schooling than natives.

Table 1
Descriptive statistics for 2013 and 2018

Characteristics	Natives		Venezuelan immigration			
			Returnees		*Venezuelan-born*	
	2013	2018	2013	2018	2013	2018
Demographics						
1. Share of the population	99.8	96.9	0.1	0.9	0.1	2.2
2. Percent Male	48.7	48.8	55.2	51.6	48.3	50.8
3. Average Age (years)	35.7	36.3	34.2	37.5	31.2	29.4
4. Avg. years of schooling (age 15+)	9.0	9.5	8.9	7.9	10.5	10.5
Labor Market						
5. Share of the labor force	99.7	96.5	0.2	1.0	0.1	2.5
6. Employment Rate	66.5	66.3	72.3	70.7	66.3	70.0
7. Unemployment Rate	9.9	9.9	12.7	12.2	14.0	14.6
8. Percent Self-Employed	46.2	45.9	52.3	55.0	43.2	48.8
9. Avg. Monthly Labor Income *(K)*	998.4	1,023.4	870.2	678.1	2,479.8	740.3
10. Percent Earning below min. wage	45.5	44.1	47.4	61.5	40.0	63.5

Notes: The Table reports for 2013 and 2018 descriptive statistics for natives, Venezuelan-born immigrants, and returnees aged 15 to 64. The employment rate is defined as the ratio of the employed to the working-age population. Average real labor income (adjusted using the 2018 Consumer Price Index as base year) includes all labor income for wage and salary workers and self-employed, excluding business owners. *Source:* Own estimates using data from the GEIH.

Regarding labor market conditions, Venezuelan immigrants contributed substantially to the overall increase in the labor force between 2013 and 2018. Immigrants have both a higher employment and unemployment rate compared to natives. Now, as rows 8 to 10 suggest, immigrants are more likely to be self-employed and are potentially downgrading upon their arrival, which would be consistent with the decrease in the average monthly labor income and the increase in the share of workers with earnings below the legal minimum wage.

3.2. Background

According to the Interagency Coordination Platform for Refugees and Migrants (R4V), more than 7 million people have emigrated from Venezuela following the social, economic, and political crisis that has plunged the country.[4] Over 80% of all outflows from Venezuela have found refuge in Latin American countries. Colombia has been the primary recipient of immigrants. Between 2013 and 2018, the total stock of Venezuelan immigrants in Colombia (including returnees) went from 0.1 to 1.5 million, a fifteen-fold increase. For that same period, the stock of those aged 15 to 64 increased by 925 thousand. As of 2018, around 63% of the working-age immigrants from Venezuela were living in urban areas (see Figure 1). Immigrants aged 15 to 64 explained 67% of the overall increase in Venezuelan immigrants. To get a sense of the magnitude of the supply shock, immigrants from Venezuela went from representing only 0.3% of the native working-age population in 2013 to 3.8% in 2018.

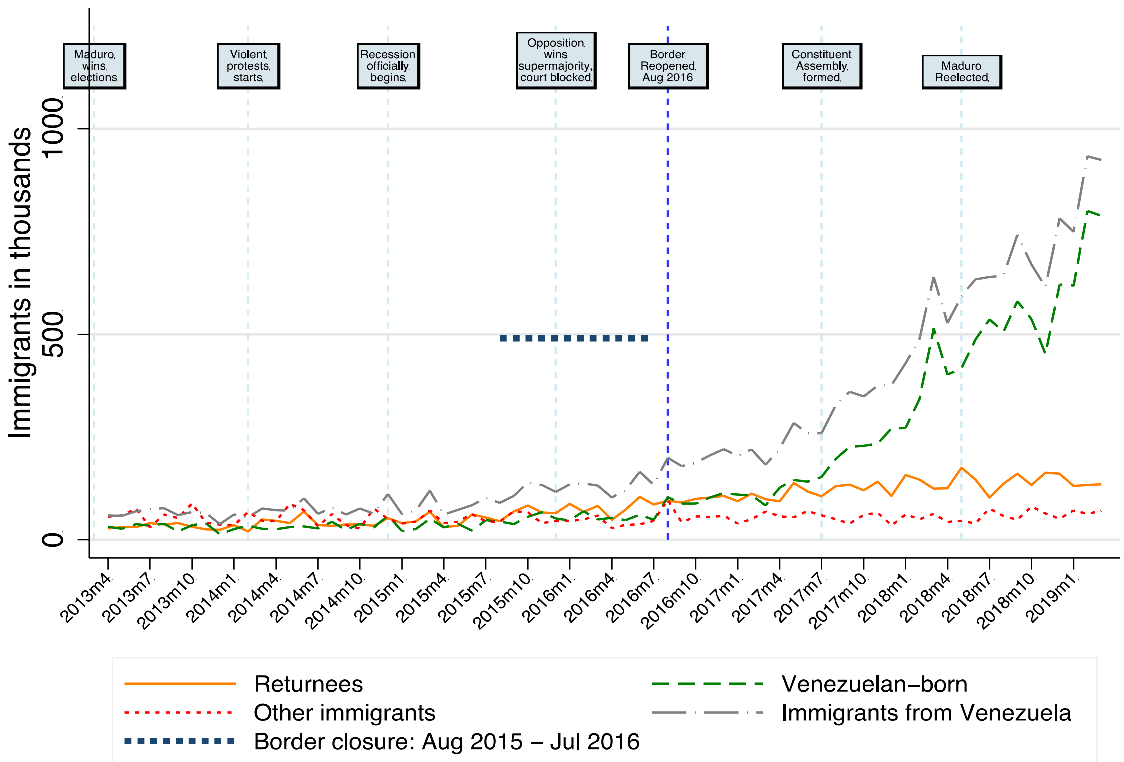
However, Venezuela's economic and political crisis also forced many Colombians to return to their country. The phenomenon was particularly intense between August 2015 and August 2016 when the Venezuelan government unliterally decided to close the border between the two countries and expel many Colombians living in the border regions in Venezuela. During that period, more than 23,000 Colombians were either expelled or voluntarily returned to Colombia. With more than 1,700 being deported.[5] As shown in Figure 1, after the border was reopened on August 12, 2016, immigration

[4] In a span of 5 years (2013 to 2018), the Venezuelan economy contracted by half. According to the 2021 ENCOVI survey (*Encuesta Sobre Condiciones de Vida en Venezuela*), conducted by three private universities in Venezuela, 94.5% of the population was living in poverty and 76.6% was living in extreme poverty.

[5] https://www.humanitarianresponse.info/sites/www.humanitarianresponse.info/files/documents/files/151015_informe_de_situacion_no_12_situacion_de_frontera_final.pdf

from Venezuela dramatically increased. For our estimations, we treat 2013 and 2014 as pre-shock periods as we observed very low inflows from Venezuela.

Figure 1
Migrant stock, April 2013 - March 2019



Notes: The Figure depicts the evolution of the monthly stock (in thousands) of working-age (15-64 years old) immigrants from Venezuela and other immigrant groups living in urban areas between April 2013 and March 2019. The gray dashed line corresponds to the sum of all migrants coming from Venezuela, including returnees and Venezuelan-born. The horizontal blue dashed line indicates the period when Venezuelan President Nicolas Maduro unilaterally decided to close the main border crossings between the two countries. The vertical dashed blue line indicates the date when the border was reopened. Light-blue boxes on the upper part of the graph show important political and economic events in Venezuela. Returnees are defined as Colombian-born individuals returning from Venezuela. *Source:* Own estimates using data from the GEIH 2013-2019.

Following the significant increase in immigrant inflows, at the beginning of 2017, the government created a two-year special permit (*Permiso Especial de Permanencia—PEP*) that allowed Venezuelan-born immigrants with regular status to stay and work in the country. However, at that moment, it was estimated that two out of every three Venezuelan-born immigrants in the country were under an irregular status (Reina *et al.*, 2018), either because they overpassed their authorized stay or because they entered the country through unauthorized border crossings. As a result, the Colombian government expanded this program in 2018 to cover around 440.000 undocumented immigrants that had voluntarily registered at the time using the Administrative Register of Migrants from Venezuela (RAMV). This legal mechanism was intended to improve the assimilation of this population, allowing immigrants to work and access health and education services.

4. Empirical Framework

4.1. Estimation Strategy

Considering our theoretical framework, an analysis of the impact of immigration on natives' fiscal position should relate the changes in natives' net fiscal contributions over time to the corresponding change in population due to immigration (see Card & Peri, 2016). As immigration is distributed across local labor markets and a large share of government finances is accrued at the regional and local level, one can use the variation across geographical units. Let M_{jt} and N_{jt} represent the number of immigrants and natives in area j in year t, respectively. Then, the empirical specification that derives directly from the relationship of interest is:

$$\Delta y_{jt} = \beta \Delta m_{jt} + \Omega_t + \varepsilon_{jt} , \tag{5}$$

where

$$\Delta m_{jt} = \frac{M_{jt} - M_{jt-1}}{M_{jt-1} + N_{jt-1}} = \frac{\Delta M_{jt}}{L_{jt-1}} , \tag{6}$$

and Δy_{jt} is the residual change in the average net fiscal contributions of natives. We also include year-fixed effects (Ω_t) and control for changes in the demographic and skill composition of the native population. Instead of relying on averages at the aggregate level, we account for changes in demographic characteristics by exploiting individual-level data and estimating fiscal contributions net of individual characteristics. For each year between 2013 and 2018, we regress individual fiscal contributions on age, age squared, sex, and education. We then use the resulting residuals to estimate equation (5). Finally, ε_{jt} is a random error term. We restrict our sample to all individuals aged 15 to 64.[6]

We allow cities to be on differential trends by interacting year dummies with a variety of 2013 city characteristics such as the share of local revenues that accrue to transfers from the central government, the share of spending on public goods, the share of working age population in the city, the share of college workers, and the share of workers employed in manufacturing. These are meant to pick up any potential correlation between natives' change in fiscal contributions and a city's fiscal or demographic composition. By fixing controls in a baseline year and not conditioning on variables measured in the post-treatment period, we avoid potential pitfalls from controlling for intermediate outcomes that may have been affected by the shock.

Since the economy also experienced a shock from the fall in commodity prices starting in late 2014, which considerably affected the fiscal balance of the General Government, by including year-fixed effects, we control for common changes that may have affected local budgets. Figure A1 shows the evolution of average fiscal contributions compared to the immigrant shock (*panel A*) and the cyclical fiscal balance for the energy sector (*panel B*).[7] While fiscal contributions seem to follow the energy cycle, because this is a common shock, it will not affect our results. Therefore, the variation used for identification in equation (5) comes from changes in the fraction of immigrants within areas over time.

[6] To alleviate the potential effect of outliers, specially coming from the allocation of corporate taxes to resident households which were allocated equally to those that reported to have received individual dividend or interest income, our main results are computed by trimming the distribution of net fiscal contributions each year for observations below the 1st percentile and above the 99th percentile. Corporate and capital taxes assigned to resident households' range between 25% and 34% of total government revenues between 2013 and 2018.

[7] The energy cyclical balance measures the change in fiscal revenues caused by the difference between the observed price and the long-term price of crude oil of the previous period.

4.2. Identification

If the fraction of immigrants in the local labor market population is correlated with local demand shocks, the coefficient of interest (β) will be biased. Given that the location of immigrants is the result of decisions that depend on conditions at the destination, a simple comparison between high- and low-immigration areas would result in biased estimates. In other words, if immigrants may endogenously select themselves into areas experiencing positive demand shocks, this would cause β in equation (5) to be upward biased. In addition, because labor force surveys are not initially intended to be a representative sample of all immigrant populations but of the overall population, this could lead to measurement error and, therefore, attenuation bias (Aydemir & Borjas, 2011).

To deal with the sorting of immigrants into local labor markets, we follow the empirical literature and estimate Δm_{jt} using historical settlement patterns of Venezuelan immigrants in Colombia. The instrument takes advantage of the well-known fact that new immigrants tend to locate near earlier immigrants from the same origin country, creating a source of exogenous variation (Altonji & Card, 1991; Card, 2001). We use census data to construct this instrument of predicted inflows to each area as a fraction of the corresponding local population in 2013:

$$IV_{jt}^{Past} = \left(\frac{M_{j,2005}}{M_{2005}}\right) \times \left(\frac{1}{L_{j,2013}}\right) \times \Delta M_t \ . \tag{7}$$

Here, M_{2005} is the number of Venezuelan-born immigrants in Colombia in 2005, $M_{j,2005}$ is the number of Venezuelan-born immigrants in location j in 2005, $L_{j,2013}$ is the working-age population of location j in 2013, and ΔM_t is the number of working-age individuals arriving in Colombia each year from Venezuela.[8] Notice that

[8] Inflows from Venezuela include individuals classified as Venezuelan-born, native-born returnees, and other foreign-born who previously lived in Venezuela. For 2005 we use total counts derived directly from the complete Census as published online by DANE. For 1973 and 1993 we use the 10% census samples from IMPUS.

the share $\left(\frac{M_{j,2005}}{M_{2005}}\right)$ includes all Venezuelan-born immigrants in Colombia, while the shift (ΔM_t), restricts inflows to all working-age Venezuelans arriving to the country.

Identification comes from exogenous exposure (given by the shares) to a common shock (given by the shift), as discussed by Goldsmith-Pinkham *et al.* (2020). The validity of the instrument depends on the exogenous variation in the national inflow rates from Venezuelan to local conditions in a specific city. Because increasing inflows of Venezuelan immigrants during our period of analysis are driven primarily by push factors in Venezuela that are uncorrelated with specific conditions in Colombia, this condition is very likely to hold.[9] Goldsmith-Pinkham *et al.* (2020) show that if the pre-period shares are correlated with unobserved local conditions, even if the national inflow rates are not correlated to those conditions, the instrument might not satisfy the exclusion restriction. Following equations (5) and (7), our empirical strategy tests whether differential exposure to the migration shock leads to differential changes in fiscal contributions for working-age natives. By focusing on changes, we can control for unobserved level differences. As long as there are no time-varying omitted local characteristics that are correlated both with our instrument and the outcome variable, our empirical strategy is thus valid.

The directed acyclic graph (DAG) in Figure A2 illustrates our setup, where the reduced-form relationship between natives' fiscal contributions and immigrant inflows is confounded by unobserved local demand factors, thus the need for an instrument. In addition, it highlights some of our assumptions. First, the only way in which immigration influences natives' fiscal contributions is through changes in factor prices (*e.g.*, labor market effects), the cost of providing public services, or the allocation of public spending, as presented in Eq. (4). Second, because the relationship between immigration and the mediating channels is confounded by unobserved local factors, this can be uncovered by ways of an instrument.

[9] A fact supporting this assumption is that immigration from other countries did not significantly change between 2013 and 2018 as depicted in Figure 1.

To build credibility for our empirical design, we follow the three-step approach suggested by Goldsmith-Pinkham *et al*. (2020). First, we look at the correlation between the exposure shares and local characteristics in 2005 and 2013. Table 2 indicates that local characteristics explain a good amount of the cross-sectional variation in the shares, with over half of the variation being explained when accounting for observed characteristics in 2013. In addition, we do not find a significant relationship between our local characteristics and the exposure shares or the instrument, except for self-employment in 2013. Although the share of self-employed workers in 2013 is not a mediator of the effect of immigration on natives' fiscal contributions (see Figure A2), including this as a control in Eq. (5) would potentially bias our estimates. First, it would be equivalent to partially controlling for labor market outcomes. Second, the fact that the share of self-employed is a strong predictor of the distribution of immigrants would amplify the bias created by the unobserved local demand factors (Pearl, 2013). The results presented in Table 2 suggest no other channels through which the shares affect outcomes in the relevant period except by predicting where the new inflows of immigrants locate within the country.

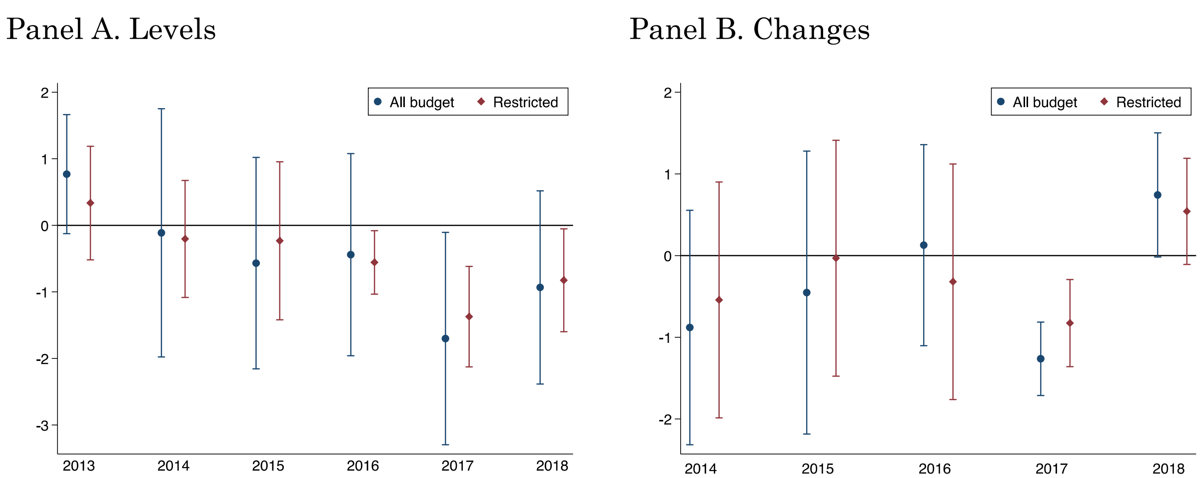
As a second step, we test for parallel pre-trends for our baseline period 2013-2014 (*differences-in-differences* logic), following our discussion in Section 3.2. We construct our pre-trend figures by estimating for each year a reduced-form regression of natives' fiscal contributions on the share of Venezuelan immigrants in 2005 and including the set of controls in Table 2, panel B. We do this for both levels and changes in the dependent variable. Figure 2 shows that the variation in the share of Venezuelan immigrants in 2005 did not predict lower net fiscal contributions for natives between 2013 and 2014, conditional on local characteristics. The figures suggest that a large enough change in the stock of immigrants starting in 2015 led to lower fiscal contributions for working-age natives. The immigrant enclave instrument seems to capture the well-defined labor supply shock. As a third step, in Section 5.2. we consider an alternative instrument to corroborate our main results.

Table 2
Relationship between exposure shares and local characteristics

Characteristics	*Exposure share*				*Instrument*	
	Coef.	SE	Coef.	SE	Coef.	SE
Panel A. Local characteristics in 2005						
Share of population age 15 to 64	0.596	(0.717)				
Share of college-educated population	0.240	(0.375)				
Share of workers self-employed	–0.397	(0.489)				
Share of employment in manufacturing	–0.848	(0.711)				
Panel B. Local characteristics in 2013						
Share of transfers from the central gov.			–0.279	(0.147)	–0.015	(0.194)
Share of expenditures in public goods			0.110	(0.082)	0.120	(0.107)
Log labor income			0.114	(0.060)	0.044	(0.079)
Share of population age 15 to 64			–0.755	(0.670)	0.020	(0.882)
Share of college-educated population			0.369	(0.455)	–0.153	(0.598)
Share of workers self-employed			0.516	(0.192)	0.742	(0.253)
Share of employment in manufacturing			0.506	(0.457)	0.480	(0.602)
Obs.	23		23		23	
R^2	0.365		0.669		0.550	

Notes: Each column reports results of a single regression of the 2005 Venezuelan share on a set of local characteristics in 2005 and 2013. The final column uses as dependent variable the immigrant enclave instrument constructed using the change in immigrant inflows from 2013 to 2018. Results are weighted by the 2013 population. Standard errors in parentheses.

Figure 2
Pre-trends using the exposure shares in 2005



Notes: The figures report year-by-year estimates of a reduced-form regression of natives' fiscal contributions on the share of Venezuelan immigrants in 2005. We present pre-trend results for the dependent variable both in levels (Panel A) and first differences (Panel B). Regressions include the set of controls described in Table 2, panel B. Results are computed by trimming the distribution of net fiscal contributions each year at 1% and 99%. Estimates are weighted by the working-age population in 2013.

An issue raised by Jaeger *et al*. (2019) is that when using immigrant enclave instruments, if local characteristics at the base period influence future levels of migration inflows and thus persist over time, the coefficient of interest will be biased. This could be particularly relevant when estimating effects on labor market outcomes as settlement patterns of immigrants are persistent. For example, estimates of the impact of immigration on wages may confound the partial equilibrium wage impact of recent immigration inflows (presumably negative), with the local labor adjustment to previous immigrant supply shocks (presumably positive), leading to a 'dynamic adjustment bias. Jaeger *et al*. (2019) show that the dynamic adjustment bias is reduced in settings in which the overall rate of immigration has temporally increased and where origin-specific push factors change the migration rate of a particular origin country. Our empirical setting closely resembles these conditions. In addition, the literature has shown, at least in the developed world, that the effects of previous immigrant supply shocks tend to die out within ten years (National Academies of Sciences, Engineering, and Medicine, 2017). In Section 5.2. we show estimates allowing for dynamic adjustment to corroborate our findings.

Finally, instrumental variables could also account for the attenuation bias generated by the low sampling of Venezuelan immigrants observed in the GEIH in its early years. This will be the case if the measurement error in the selected instrument is uncorrelated with the measurement error in the spatial distribution of Venezuelans for the period 2013–2018.

5. Results

5.1. Effect on Natives' Fiscal Contributions

We begin by estimating the effect of changes in relative immigrant inflows on the change in natives' fiscal contributions net of individual characteristics, following Eq. (5). In Table 3, we report the results of the change in immigrant inflows expressed as a fraction of the size of the total local population in the previous year. In addition, we present our results using the complete fiscal contributions [panel *(a)*] and using a restricted budget that excludes revenues and expenditures that are not derived from individual behavior [panel *(b)*]. In the latter, we exclude corporate taxes, gross operating surplus, royalties, 'pure' public goods, and debt service.

Columns 1–3 present the estimates using an ordinary least squares (OLS) estimator, while columns 4–6 use a two-stage least square (2SLS) estimator based on the immigrant enclave instrument from equation (7). Columns 2 and 5 include only pretreatment covariates, while columns 3 and 6 include pretreatment covariates interacted with year dummies to allow cities to be on differential trends. As described above, we exclude from our set of controls the share of self-employed workers as this variable is highly correlated with the exposure shares and will thus introduce bias to our estimates.

Overall, we find that the effect of immigration on natives' average net fiscal contributions is small and not distinct from zero. This is the case when using either the complete budget or our restricted definition. Looking at OLS results in columns 1–3, if immigrants were positively sorting into high-wage areas, one would expect larger estimates (in absolute terms) than those produced from 2SLS regressions. As we mentioned before, the potential for measurement error would lead to attenuation bias in the estimated effect under OLS, which we expect to correct with the inclusion of our instrument.

As suggested by Andrews *et al.* (2019), we report the Anderson–Rubin (AR) test of structural parameters that is robust to the presence of weak instruments. Based on the AR test, identification-robust confidence intervals would include zero at any standard confidence level.

The recurring negative coefficients presented in Table 3 and the fact that the pace of immigrant inflows increased exponentially from 2017 would indicate the possibility of finding effects at the end of our sampled period. To corroborate this intuition, we present year-by-year estimates using Eq. (5) in Figure A3. Panel A suggests a small negative effect in 2017. According to these estimates, a 1 percentage point increase in the inflow of immigrants relative to the working-age population in the previous year would lead to a decrease in average net fiscal contributions for natives of about COP$0.1 million, but with no effect on the following year.

Table 3
Effect of immigration on net fiscal contributions – first differences
(COP$ million, 2018 equivalent)

	OLS			*2SLS*		
Explanatory variable	(1)	(2)	(3)	(4)	(5)	(6)
Panel (a): Contributions using the complete budget						
Immigrant inflows (m_{jt})	–0.009	–0.008	–0.022	–0.006	–0.000	0.007
	(0.027)	(0.032)	(0.029)	(0.031)	(0.035)	(0.036)
AR test (*p-value*)				0.860	0.996	0.849
Panel (b): Contributions using the restricted budget						
Immigrant inflows (m_{jt})	0.002	0.008	–0.005	–0.018	–0.019	–0.000
	(0.026)	(0.029)	(0.028)	(0.024)	(0.030)	(0.031)
AR test (*p-value*)				0.441	0.529	0.988
Kleibergen-Paap F-stat				13.841	17.343	24.212
Year FE	✓	✓	✓	✓	✓	✓
MSA-level controls		✓			✓	
Controls x Year FE			✓			✓
Obs.	115	115	115	115	115	115

Notes: The Table reports the coefficients obtained by regressing the change in natives' net fiscal contributions net of individual characteristics on the change in immigrant inflows (Δm_{jt}) between 2013–2018 and year fixed effects. Individual characteristics include a dummy for sex, age, and education. Education groups are classified as: *(i)* less than HS, *(ii)* HS graduate, *(iii)* some college, *(iv)* bachelor's degree, *(v)* any post-bachelors. Residuals for observation with no information on education are obtained from conditioning only on sex and age. MSA-level controls include the share of local revenues that accrue to transfers from the central government, the share of expenditures in public goods, the share of working age population in the city, the share of college workers, and the share of workers employed in manufacturing. We report the Anderson–Rubin test of structural parameters that is robust to the presence of weak instruments. AR test the hypothesis $H_0: \beta = 0$ by estimating a reduced form equation for the dependent variable with the full set of instruments as regressors and testing that the coefficients of the excluded instruments are jointly equal to zero. Results are computed by trimming the distribution of net fiscal contributions each year at 1% and 99%. Estimates are weighted by the working-age population in 2013. Robust standard errors are reported in parentheses. *** Denotes significance at 1%, ** significance at 5% and * significance at 10%.

Considering that the average increase in the size of the working-age immigrant population over the period studied was around 0.72 percentage points per year, the actual impact is close to COP$0.07 million. This is equivalent to about $23 US dollars in 2018, or 0.3% of GDP per capita. Not a large effect. We also present estimates of natives' change in fiscal contributions relative to our baseline year (2013) in Figure A4. Results in Panel A presents evidence of significant negative effects starting in 2015 that decline over the following years.

We go further and estimate the effect separately for tax contributions and benefits received in the form of expenditures. Results displayed in Table 4 suggest no significant effect on average tax contributions and a positive effect on expenditures only when we allow for differential trends in fiscal contributions across areas. Based on the identification-robust AR test, the effect is not distinguishable from zero at the 5% level.

Table 4
Effect of immigration on revenues and expenditures
(COP$ million, 2018 equivalent)

	Revenues			*Expenditures*		
Explanatory variable	(1)	(2)	(3)	(4)	(5)	(6)
Panel (a): Contributions using the complete budget						
Immigrant inflows (m_{jt})	–0.020	–0.012	0.069	–0.016	–0.017	0.049*
	(0.039)	(0.042)	(0.042)	(0.026)	(0.029)	(0.026)
AR test (*p-value*)	0.590	0.778	0.051	0.510	0.521	0.064
Panel (b): Contributions using the restricted budget						
Immigrant inflows (m_{jt})	–0.045	–0.053	0.045	–0.023	–0.031	0.038*
	(0.045)	(0.047)	(0.043)	(0.026)	(0.027)	(0.021)
AR test (*p-value*)	0.267	0.224	0.268	0.300	0.189	0.069
Kleibergen-Paap *F*-stat	13.841	17.343	24.212	13.841	17.343	24.212
Year FE	✓	✓	✓	✓	✓	✓
MSA-level controls		✓			✓	
Controls x Year FE			✓			✓
Obs.	115	115	115	115	115	115

Notes: The Table reports the coefficients obtained by regressing the change in natives' tax contributions and benefits net of individual characteristics on the change in immigrant inflows (Δm_{jt}) between 2013–2018 and year fixed effects. Individual characteristics include a dummy for sex, age, and education. MSA-level controls include the share of local revenues that accrue to transfers from the central government, the share of expenditures in public goods, the share of working age population in the city, the share of college workers, and the share of workers employed in manufacturing. We report the Anderson–Rubin test of structural parameters that is robust to the presence of weak instruments. Results are computed by trimming the distribution of net fiscal contributions each year at 1% and 99%. Estimates are weighted by the working-age population in 2013. Robust standard errors are reported in parentheses. *** Denotes significance at 1%, ** significance at 5% and * significance at 10%.

5.2. Robustness Checks

To assess the robustness of our estimates, we conduct additional exercises using alternative regression specifications. In Table 5, we report results using the restricted budget definition. Table A1 presents the results using the complete budget. We report all coefficients with their corresponding standard errors, the first-stage F-statistic, and identification-robust Anderson–Rubin confidence intervals.

In row *(a)*, we use the shares from the 1993 census. In row *(b)*, we modify Eq. (5) and normalize the fraction of immigrants from Venezuela in each area by the local population in 2013.[10] By fixing the population in a base year, we intend to avoid potential spurious correlation between fiscal contributions and changes in the fraction of immigrants that may have been induced by changes in the native population—which may carry over with the lagged terms.

In row *(c)*, we exploit the microdata from the GEIH and estimate the following modified version of Eq. (5) using individual observations:

$$y_{ijt} = \beta \widetilde{m}_{jt} + \gamma \mathbf{X}_{ijt} + \phi \mathbf{Z}_{j2013} \cdot \mathbf{1}[\Omega_t = t] + \Omega_j + \Omega_t + \varepsilon_{ijt} \, , \tag{8}$$

where the fraction of immigrants is defined as $\widetilde{m}_{jt} = M_{jt}/L_{j,2013}$.[11] We also include a vector of individual-specific characteristics, $\mathbf{X}_{ijt}$, interact time-invariant fiscal controls $\left(\mathbf{Z}_{j2013}\right)$ with time fixed effects (Ω_t), and include area fixed effects, Ω_j. Including area-fixed effects is approximately equivalent to estimating a first difference model in which the variable of interest is the change in the fraction of immigrants in a given metropolitan area. Since we fixed the total population in the area in the immigrant ratio to the population level in the baseline period (2013), a

[10] The correlation between $\Delta M_t/L_{t-1}$ and $\Delta M_t/L_{2013}$ is 0.9983.

[11] Following Eq. (7) we construct an enclave instrument using as "shift" the total stock of Venezuelan immigrants in Colombia in each year.

first-order approximation of the change in the fraction of immigrants in a given area results in $\Delta M_t / L_{2013}$.[12]

In row *(d)*, we construct a distance-based instrument in the spirit of Del Carpio and Wagner (2015) and Delgado-Prieto (2021). The instrument predicts the number of immigrants that arrived in each city j in year t as follows:

$$IV_{jt}^{Dist} = \sum_{v} \frac{1}{T_{vd}} \theta_v \cdot \Delta M_t \,, \qquad (9)$$

where T_{vd} is the road distance between the centroid of the capital city in each Venezuelan province v and the centroid of the capital city in each Colombian department d;[13] θ_v is the distribution of Venezuelan-born immigrants by region of origin in Venezuela from the voluntary registry of undocumented migrants (Administrative Register of Migrants from Venezuela—RAMV) conducted in 2018;[14] ΔM_t corresponds to the new inflows of Venezuelan immigrants to Colombia in year t. Ideally, one would want information on the distribution of Venezuelan immigrants by city/region of origin before the shock. However, the RAMV is the only available source of information that we know of that contains the region of origin of Venezuelan immigrants. We assume that the share of immigrants from each sending region has remained stable over the years as it is a product of information networks that have been built—at least—over the last three decades. In the Online Appendix, we re-estimate our results using the pre-shock population share for each province in Venezuela to construct the distance-based instrument.[15] Our qualitative results remain unchanged.

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[12] Fixing the denominator to the baseline population eliminates the bias induced by changes in the number of native workers as a result of changes in immigrant inflows. Since we are interested in estimating the effect of immigration on the fiscal outcomes of natives (left-hand side of Eq. 8), changes in the number of native workers that appear on the right-hand side of Eq. (8) can lead to bias in the partial correlation between native outcomes and immigrant inflows as showed by Card and Peri (2016).
[13] The distance is calculated using the package *georoute* in Stata 17 (Weber & Péclat, 2017).
[14] The information can be accessed here: https://data.unhcr.org/en/documents/details/64101
[15] Venezuelan population numbers come from the 2011 population census.

Table 5
Robustness estimates of the effect of immigration on natives' fiscal contributions – restricted budget

Estimates of immigrant inflows (m_{jt})	Coef.	SE	*F*-stat	Anderson–Rubin CI
(1) Net fiscal contributions				
(a) Using the shares from the 1993 census	0.005	0.028	23.707	[–0.040, 0.078]
(b) Normalizing ΔM_{jt} by the local pop. in 2013	0.000	0.030	22.672	[–0.053, 0.074]
(c) Using individual pooled data ($\widetilde{m}_{jt}$)	0.006	0.014	25.710	[–0.022, 0.033]
(d) Using a distance-based instrument	–0.008	0.027	66.698	[–0.056, 0.045]
(e) Controlling for dynamic bias				
– Contemporaneous term: m_{jt}	0.105	0.083	30.433	[–0.069, 0.336]
– Lagged term: m_{jt-1}	–0.157	0.115	74.634	[–0.396, 0.082]
(f) LIML	0.000	0.031	24.212	[–0.047, 0.075]
(2) Revenues				
(a) Using the shares from the 1993 census	0.052	0.038	23.707	[–0.010, 0.153]
(b) Normalizing ΔM_{jt} by the local pop. in 2013	0.044	0.042	22.672	[–0.031, 0.146]
(c) Using individual pooled data ($\widetilde{m}_{jt}$)	0.024	0.014	25.825	[–0.007, 0.050]
(d) Using a distance-based instrument	0.026	0.038	66.698	[–0.041, 0.106]
(e) Controlling for dynamic bias				
– Contemporaneous term: m_{jt}	0.132	0.109	30.433	[–0.094, 0.443]
– Lagged term: m_{jt-1}	–0.131	0.143	74.634	[–0.428, 0.166]
(f) LIML	0.045	0.043	24.212	[–0.027, 0.149]
(3) Expenditures				
(a) Using the shares from the 1993 census	0.039	0.019	23.707	[0.005, 0.086]
(b) Normalizing ΔM_{jt} by the local pop. in 2013	0.037	0.020	22.672	[–0.002, 0.083]
(c) Using individual pooled data ($\widetilde{m}_{jt}$)	0.017	0.007	25.432	[–0.002, 0.027]
(d) Using a distance-based instrument	0.028	0.018	66.698	[–0.004, 0.067]
(e) Controlling for dynamic bias				
– Contemporaneous term: m_{jt}	0.023	0.050	30.433	[–0.116, 0.127]
– Lagged term: m_{jt-1}	0.023	0.048	74.634	[–0.078, 0.124]
(f) LIML	0.038	0.021	24.212	[–0.002, 0.084]

Notes: The Table reports various estimates of the effect of changes in the fraction of immigrants on natives' net fiscal contributions, tax contributions, and expenditures. All regressions include year dummies and interactions of MSA-level controls with year dummies. Results are net of individual-level controls (sex, age, education) and computed by trimming the distribution of contributions each year at 1% and 99%. We report 5%-level identification-robust Anderson–Rubin confidence sets. Results are expressed as 2018 equivalent COP$ million.

Since the two countries share a border of more than 2,000 kms., the instrument is based on the idea that distance creates time and economic costs for new immigrants, and therefore, it is a crucial determinant of immigrants' location decisions in Colombia.[16] Identification comes from comparing cities in areas close to the border with those further away. A major threat to identification with this instrument is that cities closer to the border could have been more affected by

[16] The location of Colombians in Venezuela before the shock also serves an information mechanism that conveys information about where to locate in Colombia.

economic shocks arising from changes in trade patterns between Colombia and Venezuela which could be correlated with changes in the economic outcomes of natives and affect their fiscal contributions (Bonilla *et al.*, 2020; Delgado-Prieto, 2021). In the Online Appendix, we show that results are robust to the exclusion of cities along the border and the inclusion of an indicator of bilateral trade at the department level, measured as the sum of imports and exports from/to Venezuela for each department.

To control for a potential dynamic bias, row *(e)* adds a term for lagged immigrant inflows and instruments with a lagged version of our enclave instrument from Eq. (7), as suggested by Jaeger *et al.* (2019). Finally, row *(f)* estimates Eq. (5) using a limited information maximum likelihood (LIML) estimator. 2SLS and LIML yield identical estimates of $\hat{\beta}$ in the exactly identified case, but LIML has better small sample properties.

The results presented in Table 5 suggest, as stated before, that natives' net fiscal contributions were not affected by immigrant inflows. A key takeaway from the dynamic analysis, even though the results are not distinct from zero, is that contemporaneous inflows have a positive coefficient while lagged inflows have a negative coefficient. This seems to point in the direction of immigrants improving natives' fiscal position by spreading costs in the short-term, but possibly offsetting this with an effect on factor prices in the medium- to long-term. We still do not find any effects on average tax contributions and clear evidence of an increase in average benefits received—except when using the 1993 immigrant shares.

5.3. Differences by Revenue and Expenditure Group

To understand what is driving the effect in both revenues and expenditures, we look at changes in the different types of contributions. Figure 3 presents a detailed picture in each case by plotting the point estimates and the respective confidence intervals of our OLS and 2SLS coefficients obtained by regressing natives' individual contributions on the fraction of immigrants, conditioning on baseline city characteristics. The Online Appendix provides a complete description of how we defined and computed each revenue and expenditure category.

Results presented in Figure 3 suggest that changes in immigrant inflows over the period had only a small negative effect on indirect taxes on the revenues side and a positive effect on health services in the case of expenditures. These findings have two implications. First, since we are not seeing effects on personal income tax and social security contributions, changes in indirect taxes seem to be driven primarily by changes in household income. Second, a positive effect on health services could reflect one or both of the following: an increase in the unit cost of health services from changes in the epidemiological and demographic profile of the population or an increase in coverage of the immigrant population through demand subsidies which are then borne to a certain extent by the native population. We explore these different mechanisms in detail next.

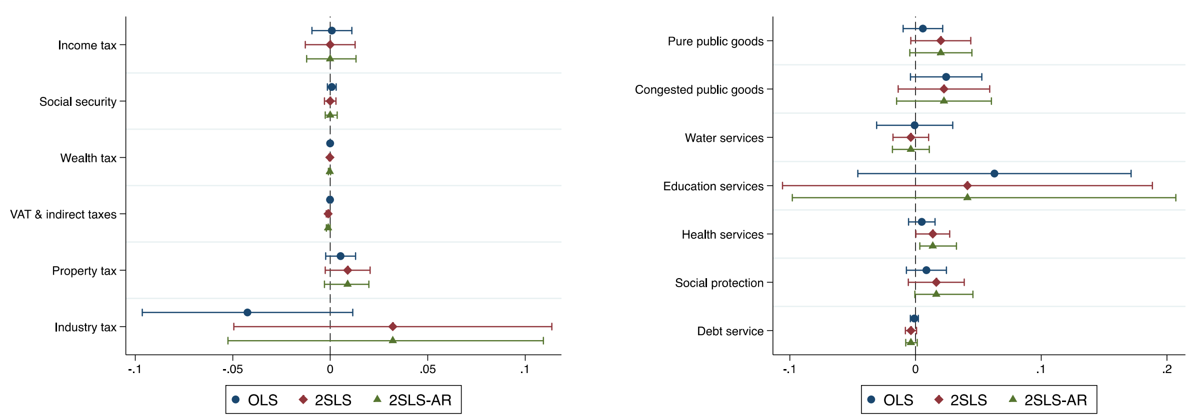
Figure 3

Change in natives' fiscal contributions by revenue and expenditure group

(COP$ million, 2018 equivalent)

Panel A. Revenues

Panel B. Expenditures



Notes: The Figure reports the point estimates and the respective 95% confidence intervals of OLS and 2SLS coefficients obtained by regressing the change in natives' individual contributions on the change in immigrant inflows between 2013–2018 (Δm_{jt}). We also report Anderson–Rubin robust confidence sets for 2SLS estimates using the geographic distribution of immigrants in 2005. Results are computed by trimming the distribution of contributions each year above the 99th percentile. All regressions include year dummies and interactions of MSA-level controls with year dummies. MSA controls include the share of local revenues that accrue to transfers from the central government, the share of working age population in the city, and the share of college workers. Estimates are weighted by the working-age population in 2013.

6. Mechanisms

In this section, we explore the three mechanisms outlined in Section 2. We start by discussing the extent to which the fiscal effects presented before are mediated by changes in labor market outcomes for natives or changes in the price of non-tradable goods, particularly changes in property values and rents. We then discuss changes in the cost of provision and use of government services and the overall allocation of expenditures.

6.1. Labor Market Effects

As part of labor market equilibrium effects, we explore three possible mechanisms: changes in aggregate labor market outcomes (extensive margin), changes along the distribution of wages, and changes in the number of hours of work (intensive margin) and the quality of employment, measured by changes in the probability of being employed in the informal sector.

6.1.1. Labor displacement and average wages

We start by testing if the increase in immigrant inflows generated labor displacement among native workers or affected average wages. This could explain our fiscal results, especially the negative effect on indirect taxes. We follow the 'pure spatial approach' literature and estimate the effect of immigration on natives' labor market outcomes.[17] The baseline model regresses labor market outcomes (the probability of being employed, the probability of being unemployed, the probability of being active in the labor force, and log hourly wages) for natives on the fraction of immigrants in the working-age population of 2013, leveraging the pooled sample. We run a similar specification as in Eq. (8) but without including interactions of area-level characteristics in 2013 with time dummies.

[17] Our approach is similar to those implemented by Card (2007), Dustmann *et al.* (2013), and Peri & Yasenov (2019). In contrast to those papers which aggregate the outcomes of interest at the region by time level and regress differences over time in those outcomes in a particular skill group on changes in the fraction of immigrants, we exploit individual-level data.

Figure 4 presents OLS and 2SLS estimates for the impact of Venezuelan immigration on natives' labor market outcomes. 2SLS estimates are presented for both the past settlements instrument (IV1) following Eq. (7) and the distance instrument (IV2) as in Eq. (9).[18] Our specification does not suffer from a weak instrument problem since our instruments can predict immigration shares effectively. The Kleibergen-Paap F-stat for weak instruments ranges from 22.2 to 47, with a higher predictive capacity for the distance instrument.

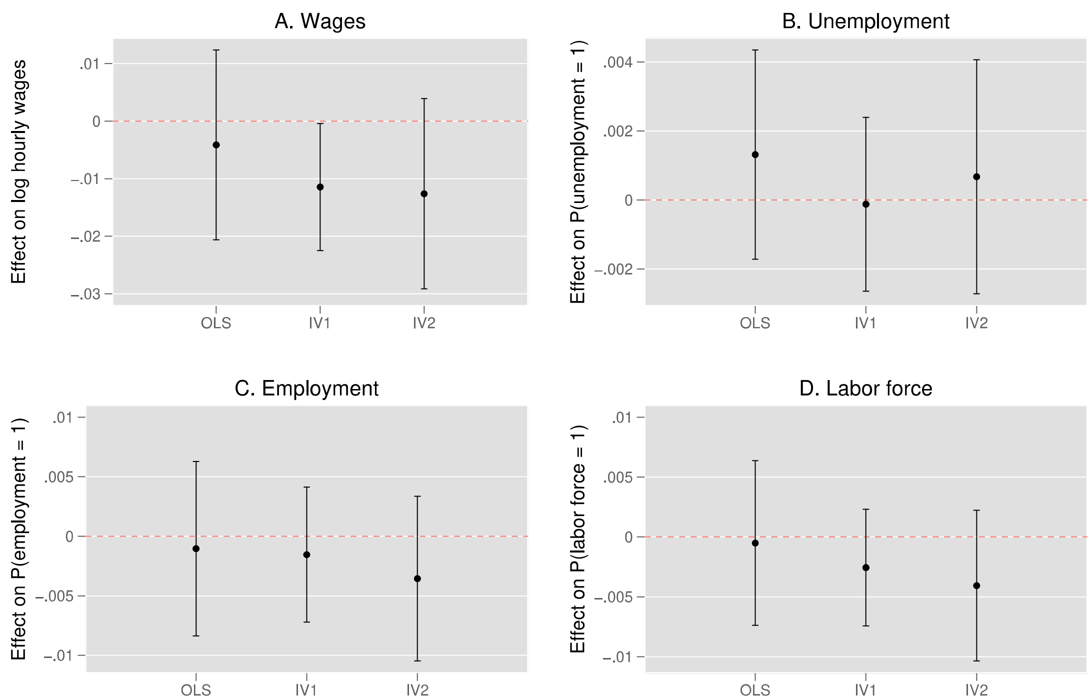
The OLS estimates in Figure 4 suggest that immigration from Venezuela is negatively correlated with natives' hourly wage and the probability of being employed and participating in the labor force, and positively correlated with the probability of being unemployed. However, these effects are statistically not distinct from zero. After accounting for the endogenous sorting of immigrants into areas and potential measurement error in the shares by instrumenting the fraction of immigrants with the past settlement and the distance-based instruments, the results show a negative and significant effect on wages only using the past settlement instrument (Panel A, IV1 estimate). On average, a 1 percentage point increase in the share of immigrants in the working-age population (for instance, going from 1% to 2%) is associated with a reduction in native wages of 1.1%. Placing the wage response into perspective, since the real hourly wage growth for working-age native workers between 2015 and 2018 (the high immigration period) was about 1.4% per year and the fraction of immigrants increased by 1 percentage point on average, the negative wage effect does not necessarily imply a decline in natives' real wages.

On the other hand, 2SLS estimates suggest that immigration from Venezuela does not seem to have influenced natives' aggregate employment and labor participation outcomes between 2013 and 2018 (see IV1 and IV2 estimates on panels B, C, and D). These findings are robust to alternative specifications. We get comparatively similar results when we do not use sample weights, when controlling

[18] Since we don't estimate regressions using first differences, in Eq. (7) we use as "shift" the total stock of Venezuelan immigrants in Colombia in each year.

for a proxy of local economic activity (state GDP), and when building confidence intervals that are robust to weak instruments á la Anderson-Rubin as suggested by Andrews *et al.* (2019) (results available upon request).[19]

Figure 4
Effect of immigration on main labor market outcomes



Notes: The Figure reports OLS and 2SLS coefficients and the respective 95% confidence intervals obtained by regressing natives' log wages (Panel A), unemployment (Panel B), employment (Panel C), and labor force participation status (Panel D) on the fraction of immigrants ($\widetilde{m}_{jt}$). IV1 instruments $\widetilde{m}_{jt}$ with the past settlement instrument following Eq. (7). IV2 instruments $\widetilde{m}_{jt}$ with the distance instrument as defined in Eq. (9). All regressions include year and area fixed effects, individual controls (sex, age, age squared), and dummies for education achievement (less than high school, high school, some college, college graduates, and graduate degrees). Wages are computed for wage and salary workers and include the labor income of self-employed workers. The sample is restricted to natives aged 15 to 64 living in metropolitan areas (MSAs) from 2013-2018. To alleviate the potential impact of outliers, wages were computed by trimming the wage distribution by year at 0.5% and 99.5%. Wages are expressed in 2018 equivalent COP. Estimates are weighted by sampling weights. Standard errors are clustered at the metropolitan area level. The Kleiberger-Paap F-statistic ranges between 22.2 and 47.5 depending on the outcome variable, with higher predictive capacity for the distance instrument.

[19] Our results are not directly comparable with some of the recent evidence of the effect of Venezuelan immigration on natives' labor market outcomes in Colombia as studies tend to differ in the definition of the fraction of immigrants, the period of analysis, the definition of Venezuelan immigrants, and the unit of analysis. Just as a reference, Carusso *et al.*, (2019) found large negative effects on wages and on the probability of being employed, and positive effects on the probability of being unemployed. Lebow (2021), using aggregate specifications at the MSA level between 2014-2019, only found negative effects on wages, and no effects on employment and unemployment. Delgado-Prieto (2021), looking at changes at the department-level, found negative effects on wages and employment in 2018 relative to 2015.

In Figures A5 and A6, we explore whether labor market effects differ by workers' sex and skill, respectively. Results suggest that the impact on wages reported in Panel A, Figure 4 is mainly driven by the reduction in wages of low-skill males. For instance, the first panel in Figure A6 shows a meaningful negative and statistically significant effect on the wages of low-skill workers (those with less than a high school diploma) using both the past settlement and the distance-based instruments. A 1 percentage point increase in the share of immigrants in the working-age population is associated with a reduction in real wages for low-skill natives between 1.8% and 2%, depending on the instrument used. The point estimates are in line with previous work from Lebow (2021) and Delgado-Prieto (2021), who estimate low-skill native responses to immigration from Venezuela between 1.4% and 2%.

Similarly, the first panel in Figure A5 shows a negative and significant effect on native males using the past settlement instrument. The coefficient implies a negative impact of immigration close to 2% for each 1 percentage point increase in immigration across metropolitan areas. Figures A5 and A6 also show no heterogeneity in the effects of immigration on natives' probabilities of employment, unemployment, and labor force participation by sex and skill. The effects of immigration seem to be concentrated on the wages of the low-skill males.

In Figures A7 and A8, we further explore the effect on wages by economic sector and type of job (salaried, self-employed, government, and domestic workers), respectively. Figure A8 shows no effects on wages of salaried workers (Panel A) but a negative and statistically significant effect on self-employed workers (Panel B). The effect on the wages of those self-employed could be explained by the fact that 59% are also low educated, and almost 90% are in the informal sector.[20] As a robustness check, we find, as expected, no effect on the wage of workers employed by the government. However, we do not see a decline in the wages of domestic workers. Because many

[20] Informality is defined as those in jobs with limited to no access to social security (pension or health benefits).

immigrants are employed as domestic workers (see Lebow, 2021), we may have expected to find an effect for this group.

Taken together, the results for low-skill and self-employed natives are consistent with a story of lack of downward wage rigidities and low reservation wages: natives and immigrants are competing for jobs over prices in the informal sector. These wage effects explain almost entirely the small decline in indirect taxes presented in Section 5.3.

6.1.2. Effects along the wage distribution

We move to study the effects of immigration from Venezuela across natives' wage distribution. The fact that immigrants from Venezuela downgrade upon arrival, as depicted in Figure A9, would lead one to think that the wage effect found in the previous section might not be constant across the wage distribution and would be particularly burdensome in segments of the labor market where the density of Venezuelan immigrants is higher than that of native workers.

We start by estimating log wages net of individual characteristics. Using the entire sample between 2013-2018 at the individual level, we regress log wages on sex, age, age squared, and dummies for education achievement (less than high school, high school, some college, college graduates, and graduate degrees). We then get residual log hourly wages for different percentiles $(5, 10, 15, \ldots, 95)$ for each MSA and year $(\widetilde{w}_{pjt})$ and estimate: [21]

$$\widetilde{w}_{pjt} = \beta_p \widetilde{m}_{jt} + \Omega_j + \Omega_t + \varepsilon_{pjt}, \tag{10}$$

[21] In a similar application to the UK context, Dustmann *et al.* (2013) regress differences over time in percentiles (p) of log wages across different regions (j) on changes in the fraction of immigrants to natives (m_{jt}), year dummies (Ω_t), and changes in the average age of immigrant and native workers in the regions, and the ratio of high (or intermediate) to low-educated native workers (X_{pjt}). Our empirical strategy differs from Dustmann *et al.* in two ways. First, we exploit individual-level data and use residual wages net of individual characteristics. Second, instead of using a first-difference estimator, we exploit variability across time within areas by using a two-way fixed effects estimator as defined in Eq. (10).

where Ω_t and Ω_j are year and area fixed effects, respectively; $\widetilde{m}_{jt}$ is defined as in Eq. (8) and it's instrumented with the past-settlement and distance-based instruments described before.[22]

Our specification is robust to immigrants' downgrading because it does not allocate immigrants into skill groups.[23] Downgrading is indeed an important issue when estimating the effects of immigration on natives' wages. Even though Venezuelan immigrants are, on average, more educated than natives, a large fraction of them remained undocumented and thus unable to work in the formal sector until the end of 2018 when the Colombian government extended the enrollment for the special working permit—PEP.

Figure 5 presents OLS and 2SLS estimates using the past settlement instrument (IV1) and distance-based instruments (IV2). The effects across the natives' wage distribution seem to mirror where immigrants from Venezuela are overrepresented. Figure A9 shows that Venezuelan immigrants are overrepresented below the 35th percentile in contrast to what one would have expected if they were compensated similarly to natives after accounting for immigrants' sex, age, and education (see green dashed line). Effects across the wage distribution using the distance-based instrument (Figure 5, Panel C) show a negative and significant effect on native wages between the 15th and 50th percentiles. The effects are larger at the lower part of the distribution. For instance, a 1 percentage point increase in the share of immigrants in the working-age population leads to a reduction of 2.4% and 1.9% in native hourly wages at the 20th and 50th percentile, respectively. Results are comparatively similar but slightly lower in magnitude using the past-settlement

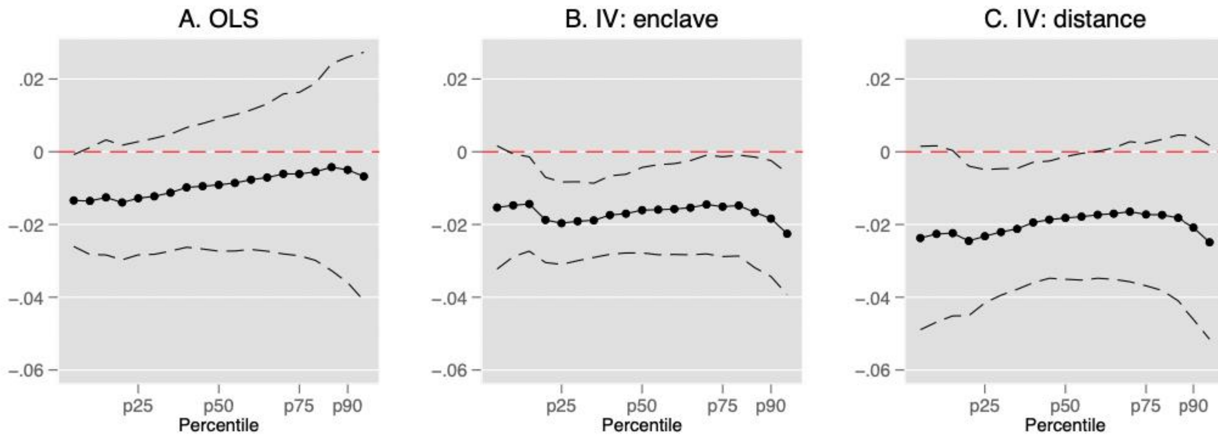
[22] Our approach, as in Dustmann *et al.* (2013), assumes the same effect of immigrants in any other segment in the wage distribution (or with different skills) on natives' wages. See Ottaviano & Peri (2012) for an estimate of the *total wage effect* of immigrants on natives' wages using a skill-cell approach. The skill-cell approach implies different effects depending not only on total immigration but also on the distribution of immigrants across skill groups. Such an approach, however, exploits variation in wages and immigrants across groups of workers with different skills at the national level, and not across local labor markets.

[23] For an example of the skill cell approach see Borjas (2003). For research using a mix approach (a combination of skill cell and spatial variation) see Dustmann & Glitz (2015).

instrument (Figure 5, Panel B). In the case of the enclave instrument, there also appear to be effects in the upper part of the distribution. However, the effects above the 50th percentile are not distinct from zero at the 1% significance level.

Does it make sense to find effects above the 50th percentile? As a reference, in our setting, the 50th percentile in 2018 is equivalent to the minimum wage, while the 75th percentile is equivalent to 1.5 times the minimum wage. Therefore, finding some effects in the upper half of the wage distribution is not unreasonable.[24]

Figure 5
Effect of Venezuelan immigration along natives' wage distribution



Notes: The Figure reports OLS and 2SLS coefficients and the respective 95% confidence intervals obtained by regressing residual log hourly wages for each percentile of the native wage distribution on the fraction of immigrants ($\widetilde{m}_{jt}$) following Eq (10). The enclave instrument is defined as in Eq. (7). The distance-based instrument is defined as in Eq. (9). Regressions are weighted by the number of observations in each quantile and area in 2013.

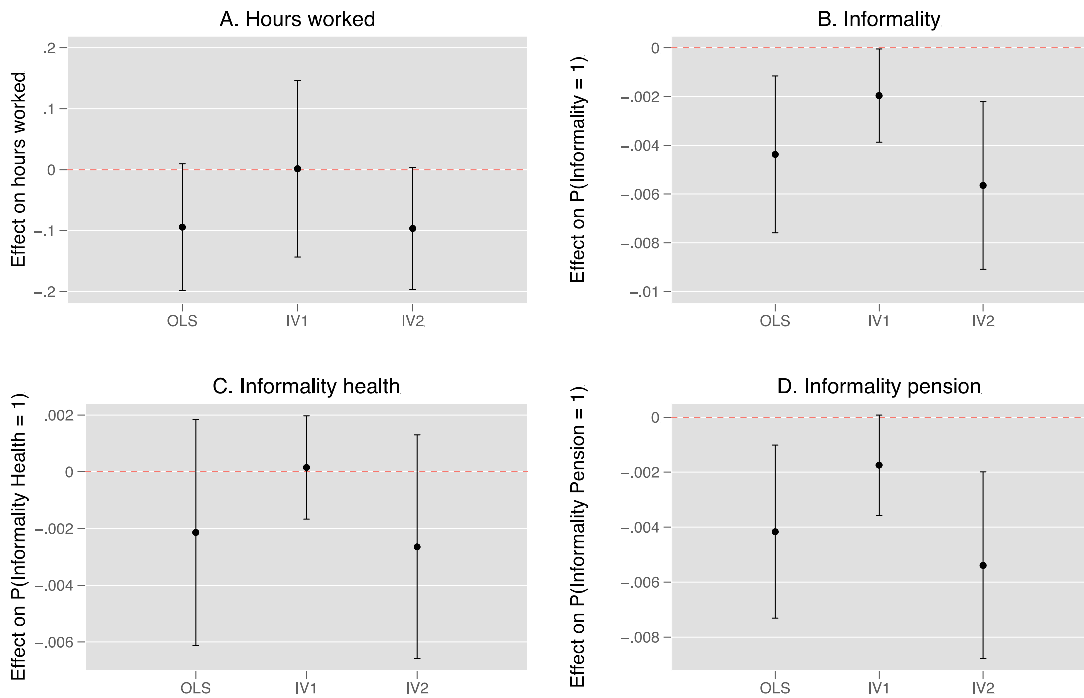
6.1.3 Intensive margin and labor quality

We explore two other possible sources of adjustment to increasing immigrant inflows from Venezuela: the intensive margin (by working fewer hours) or through changes in the quality of employment (changes in the probability of being employed in the informal sector). We estimate regressions using as dependent variables the hours worked by natives in the main occupation and different indicators of informality

[24] As a comparison, Dustmann *et al.* (2013) find a negative impact of immigration below the 1st decile for the UK, which was at the same time around the minimum wage, but no significant effect up to about the 35th percentile. However, they find positive wage effects between the 40th and 90th percentile.

status.[25] Specifically, we use three related definitions of informality: 1) workers do not report pension contributions; 2) workers do not report health contributions; and 3) workers do not report both pension and health contributions.[26]

Figure 6
Effect of immigration on the intensive margin and the quality of jobs



Notes: The Figure reports OLS and 2SLS coefficients and the respective 95% confidence intervals obtained by regressing natives' hours worked in the previous week in the main occupation and three measures of informality status on the fraction of immigrants ($\widetilde{m}_{jt}$). IV1 instruments $\widetilde{m}_{jt}$ with the past settlement instrument following Eq. (7). IV2 instruments $\widetilde{m}_{jt}$ with the distance instrument as defined in Eq. (9). Informality status in Panel B refers to workers who do not report pension and health contributions. In panels C and D, informal is defined as not contributing to the health or pension system, respectively. All regressions include year and area fixed effects and individual controls (sex, age, age squared, and education). The sample is restricted to natives aged 15 to 64 living in metropolitan areas (MSAs) for 2013-2018. Estimates are weighted by sampling weights. Standard errors are clustered at the MSA-level. The F-statistic is 21.1 and 35.3 for the past settlement and distance instruments, respectively. The sample size is 1,328,858.

Figure 6 presents the results on the margins of adjustment. Panel A shows the effect on hours worked for native workers. We see a positive coefficient but not

[25] We run a similar specification as in Eq. (8) but without including interactions of MSA-level characteristics in 2013 with time dummies.

[26] We classify as informal all those self-employed workers that don't make social security contributions and wage and salary workers for whom their employer is not making the legal contributions.

statistically distinct from zero, even after using both the past settlement and distance-based instrument. Regarding the probability of being employed informally, we see a small negative and significant effect on informality (Panel B). A 1 percentage point increase in the fraction of immigrants reduces the probability of natives being employed in the informal sector between 0.2 and 0.5 percentage points, depending on the instrument used. These results do not necessarily imply a transition into the formal sector and out of the informal jobs. However, it could suggest that natives are finding jobs where contributions to the pension system are more likely (Panel D). As most of the job opportunities for immigrants before the PEP was extended were in the informal sector, increasing competition might have induced some selection of natives into formal jobs. Indeed, among Venezuelan immigrants, 61% were working in the informal sector in 2014, increasing to 77% by 2017. These results add to our evidence on the effect on average wages for self-employed workers presented in Section 6.1.2.

6.2. Property Values and Rents

Table 6 examines the effect of increasing immigrant inflows on property values (section *(i)*) and rents (section *(ii)*). We look at variation across cities (Panel A) and neighborhoods within cities (Panel B). We use the geographic equivalent of a census tract (*sector urbano*) as our definition of neighborhood. Although only property values are used to determine the property tax, the change in rents provides an additional measure of the market value of housing. Based on findings in the literature studying the Age of Mass Migration (Tabellini, 2020) and the Great Migration (Tabellini, 2019), it is not clear that one would find an effect on property values and rents, or that the effect goes in a particular direction, even in the short run. While large inflows of immigrants may increase the demand for housing, potentially increasing prices, at the same time, it could reduce the demand for housing among natives who, unwilling to live in larger immigrant enclaves, chose not to move to the city or move out of neighborhoods.

We regress $ln(PRICE)_{kt}$ on the fraction of immigrants ($\widetilde{m}_{kt}$), where $PRICE$ indicates the mean real value (rent) of owner-occupied (rental) units in MSA or neighborhood in each year, net of observed housing quality characteristics.[27] As before, we instrument the fraction of immigrants across cities using our enclave instrument. Now, because immigrants sort into neighborhoods, we also need to account for the endogenous allocation of immigrants within the city. We instrument the immigrant share in the tract using a modified version of our previous instrument. We interact predicted inflows of immigrants at the metro level for each year with the share of rooms in each tract in 2013.[28] The idea behind is that tracts with a higher number of rooms will eventually accommodate a larger share of immigrants.

Focusing on the 2SLS results reported in Panel A, it seems that immigration had a negative and statistically significant impact on both property values and rents. A 1 percentage point increase in the share of immigrants in the area—relative to the local population in 2013—reduces housing prices and rents by 2.3% and 2.9%, respectively. When considering variation across neighborhoods (column 3, Panel B), these negative effects are more pronounced, reducing property values by 4.7% and rents by 3.9%. One crucial piece of information to keep in mind is that housing prices and rents were growing on average by 6.5% and 2.4% in real terms, respectively. At the same time, the fraction of immigrants increased by 0.66 percentage points on average between 2013 and 2018. Therefore, the negative effect does not necessarily imply a decline in prices and rents across areas. A more compelling interpretation of our results is that, at least in the case of property values, in the absence of immigration, areas that faced higher inflows would have experienced higher price

[27] We start by regressing for each year log housing values on a set of housing quality characteristics (type of unit, number of rooms, structure and flooring material, utilities) and use the residual value as the dependent variable in our main regressions. These hedonic-type regressions account for changes in the quality-mix of properties over time. Since a household can rent either a whole unit or rooms within a unit, our rent price is defined as the average rent per room in the unit. So, the same unit can be owner-occupied and rent rooms. One drawback of the data is that housing values are based on owner self-reports.

[28] Because the neighborhood identification in the GEIH is an orthogonal construction of the real, we are not able to use Census information for previous years to match to our data.

growth. Despite the positive sign, this slower growth might explain why immigration did not significantly affect property tax revenues (Section 5.3).

Table 6
Effect of immigration on property values and rents

	Panel A. Variation across metropolitan areas	
	OLS	2SLS
(i) Property values		
Immigrant share ($\widetilde{m}_{jt}$)	0.015	–0.023*
	(0.014)	(0.012)
Kleibergen-Paap F-stat		18.327
(ii) Rents		
Immigrant share ($\widetilde{m}_{jt}$)	–0.004	–0.029***
	(0.008)	(0.039)
Kleibergen-Paap F-stat		19.999
Observations	138	138

	Panel B. Variation across neighborhoods (*tracts*)			
	OLS	OLS	2SLS	2SLS
(i) Property values				
Immigrant share ($\widetilde{m}_{njt}$)	0.002	0.001	–0.047*	0.056
	(0.005)	(0.003)	(0.026)	(0.035)
Kleibergen-Paap F-stat			28.535	29.568
Observations	8,108	8,108	8,108	8,108
(ii) Rents				
Immigrant share ($\widetilde{m}_{njt}$)	–0.002	0.000	–0.039***	0.012
	(0.000)	(0.001)	(0.011)	(0.011)
Area by Year FE		✓		✓
Kleibergen-Paap F-stat			53.822	31.080
Observations	8,178	8,178	8,178	8,178

Notes: The Table reports the coefficients obtained by regressing the log of mean real property values (section (*i*)) and rents (section (*ii*)) on the fraction of immigrants ($\widetilde{m}_{kt}$) between 2013–2018. Regressions in columns 1 and 3 include year and area (tract) fixed effects. Regressions in columns 2 and 4 control for tract and area by year fixed effects. The fraction of immigrants ($\widetilde{m}_{njt}$) in Panel B is constructed by averaging immigrants in the tract using 2-year moving averages. Values are deflated using the price index for used housing from Colombia's Central Bank and the rent index from the CPI reported by DANE. Results are computed by trimming the distribution of property values and rents each year at 0.5% and 99.5%. In all regressions, observations are weighted by the number of owner-occupied (or rental) units in the area or tract in 2013. Columns 1 and 3 report in parentheses robust standard errors. Columns 2 and 4 report robust standard errors clustered at the area level. *** Denotes significance at 1%, ** significance at 5% and * significance at 10%.

Now, when we control for overall trends in the area, the negative neighborhood effect disappears (column 4, Panel B). By conditioning on tract and area by year fixed effects, the variation used for identification comes from changes in the fraction of immigrants in the tract over time, as compared to other tracts in the same area in a given year. In contrast to the results in Tabellini (2020), who finds that immigration from the Age of Mass Migration did not have a significant effect on property values in the US, some of our findings in the context of Venezuelan immigration suggest otherwise.[29]

6.3. Cost Effect

As discussed in Section 2, an increase in population size can affect the cost of government service provision. While we do not directly observe the unit cost of the multiple goods and services provided by local governments, we can use our estimates of the effect of immigrant inflows on the different expenditure groups to infer whether or not this mechanism is in place.

The fact that we do not see an increase in the per capita spending of public goods classified as non-rival in consumption or 'pure' (see Figure 3, Panel B) suggests no sign of costs being distributed among the larger population. On the contrary, spending classified as 'pure' public goods seems to share the characteristics of public goods that are rival in consumption and therefore tend to increase with population size. This may be the case of spending on general public administration and transfers of a general character between different levels of government.

In the case of health expenditures, as we show in Section 6.4., immigration did not significantly change the allocation of expenditures. Thus, part of the increase in natives' health benefits may reflect an increase in the cost of providing the service. Changes in the cost could be driven by changes in the epidemiological profile of the population, explained by increasing immigrant inflows. For example, immigration

[29] The literature looking at the effects of black in-migration and racial desegregation has found also negative effects on property values and rents. For example, Boustan (2012) finds a decline in urban house prices and rents by 6% following the desegregation of public schools in the 1970s. Similarly, Tabellini (2019) finds that black in-migration to the North of the US between 1910 and 1930 reduced per capita property values by roughly 10%.

could have impacted the cost of providing health services through changes in the composition of the population. Public records show that a significant fraction of Venezuelan immigrants are minors and many immigrant women arriving in Colombia are pregnant. This, combined with limited information about the health records of immigrants before arriving in the country, could possibly increase the health cost overall.

6.4. Composition and Service Use Effect

We now turn our attention to analyzing the effect that the increase in immigrant inflows has on the allocation of local government spending and service use. If we follow the findings in the literature, one may expect a negative effect for those expenditure items where interactions between groups are more salient, or that imply larger transfers, such as productive public goods, education, or social protection. However, this is not necessarily true in our setting for at least three reasons. First, many local governments have been lenient—at least in their discourse—in helping in the humanitarian assistance and assimilation of immigrants. Second, local authorities tend to face less pushback from residents considering that Venezuelan immigrants are culturally not that different from natives. Third, local governments face limitations on local taxation combined with low budget flexibility, limiting the discretion of local authorities to change the allocation of public expenditures in the short run.

In Figure 7, we ask whether the change in the stock of Venezuelan immigrants affected the composition of local spending and the likelihood that natives rely on government-funded services or social programs. Panel A presents the change in the share of local spending by group (*e.g.*, public goods, health, education, social protection, etc.). We estimate a version of Eq. (5) for each type of expenditure, and instead of using variation in the share of working-age migrants, we use the total population. Because some expenditures executed at the local level tend to be approved at higher levels of government (*e.g.*, transfers from the National government to pay for education and health), limiting the autonomy of local governments, Panel B provides results excluding transfers from the national government.

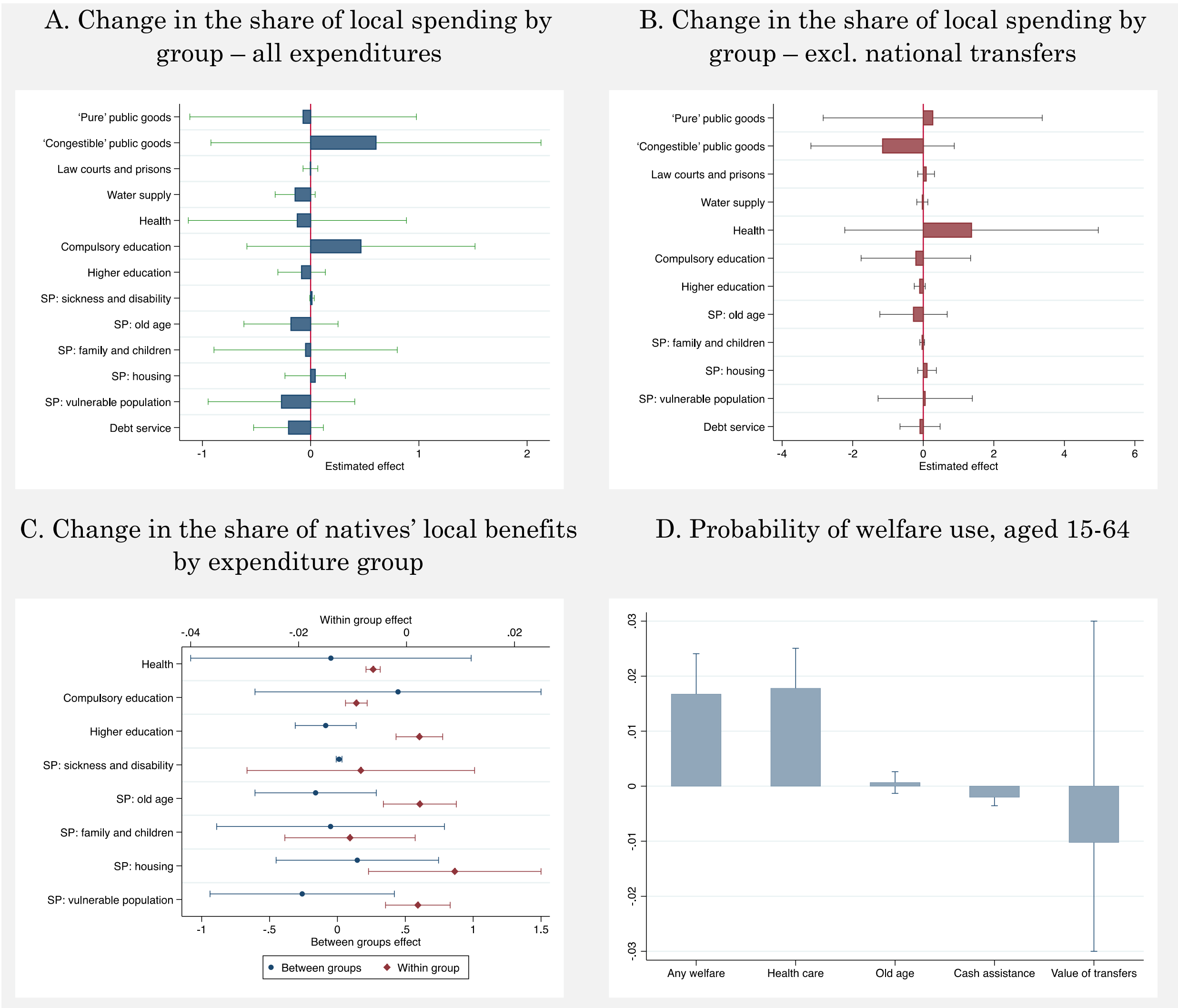
The results in Figure 7 show no significant change in local spending that could be explained by increasing immigrant inflows. However, a couple of things can be highlighted from the sign of some of the coefficients. First, after excluding transfers from the National government, the large and positive sign in health expenditures by local governments allows us to think that the increasing pressure on the health system from immigration, as many public officials have pointed out,[30] has required the fiscal effort of municipalities. Second, the shift in signs in both 'congested' public goods (*e.g.*, police services, transportation, housing development) and compulsory education, between Panel A and B, shows the large weight transfers have on local budgets, especially for smaller cities. Third, most of the humanitarian assistance for the vulnerable population, under which immigrants from Venezuela are beneficiaries, is being provided by the National government.

These results probably reflect low budget flexibility at the local level, which reduces the autonomy to reassign resources between expenditure groups. Therefore, in Panel C, we examine if, instead of shifts in overall spending, immigration might have affected the allocation of natives' local benefits. To that end, we exclude expenditures not derived from individual behavior and focus on health care, education, and social protection. We look at changes *between* expenditure groups and changes in spending *within* groups. Consistent with the results in Panel A and B, we do not see a significant change in the allocation of public spending across expenditure groups. However, when we look at the shares within groups, we do see a decline in natives' share in health and compulsory education. Comparing the *between* and *within* effects suggest that local governments have had to stretch their resources to meet the increasing demand. As Reina *et al.* (2018) pointed out, in the case of spending on compulsory education, the overall resources transferred to regional and

[30] According to the Office of the Comptroller General, the debt with public hospitals only for urgent care provided to Venezuelan citizens exceeded $407 billion in the last three years.

local governments are allocated essentially based on the cost of the payroll, which initially reduces the need for additional spending.

Figure 7
Composition and service use effect



Notes: The Figure reports the point estimates and the respective 95% confidence intervals of 2SLS coefficients for the fraction of immigrants ($\widetilde{m}_{jt}$). The dependent variable in Panel A is the share of local public spending in each expenditure group. In Panel B, the shares exclude transfers from the national government. In Panel C, only information on spending allocated to natives is used to construct the *between* and *within* shares. All regressions include year dummies and interactions of MSA-level controls at their 2013 level with year dummies. MSA controls include the share of local revenues that accrue to transfers from the central government, the share of the working-age population in the city, the share of college-educated workers, and the log of mean labor income. All estimates are weighted by the total population in the area in 2013. Panel D reports the probability of taking-up welfare, using as dependent variables dummies that indicate self-reported information on receiving any welfare, subsidized health, old age benefits, or cash assistance, and the real value of cash transfers. Regressions in Panel D control for year and area fixed effects, age, sex, family size, monthly wage, nonlabor and non-transfer related income.

Finally, in Panel D, we estimate whether natives are more or less likely to receive welfare due to increasing immigrant inflows. We examine whether individuals claim welfare benefits, such as subsidized health care, conditional cash transfers or other cash assistance, and benefits for older adults. We also check if immigration changed the cash value of transfers received. We estimate a linear probability model and control for observable individual characteristics that may influence welfare take-up, as well as area and year fixed effects.

Immigration appears to have increased welfare take-up by natives. For instance, a 1 percentage point increase in the fraction of working-age immigrants, relative to the local population in 2013, raises the probability of claiming welfare by 1.7 percentage points. This effect is driven by a higher probability of being enrolled in the subsidized health system. Although the results show a slight drop in the probability of receiving any type of cash assistance, the cash value does not seem to have changed. These findings are consistent with a decline in the probability of working informally; however, we believe these effects are too small to explain any effect on fiscal contributions.

The effects on subsidized health take-up are somewhat puzzling, especially since we do not find an effect on informality measured by contributions to the health system. We provide one interpretation for these findings. Considering that between 2013 and 2018 the total health coverage went from 96% to 94.7%, and the subsidized regime did not grow in the number of affiliates, these results suggest that areas with higher inflows of immigrants experienced a slower decline—perhaps even a slight increase—in coverage. Overall, the increase in the probability of natives relying upon subsidized health care is consistent with the increase in per capita health expenditures shown in Figure 3.

7. Discussion

What do these results imply for the effect of immigration on public finances? In this paper, we have tried to address a valid concern of traditional accounting estimates of the net fiscal effect of immigration, which estimate individual tax contributions and benefits, ignoring labor market equilibrium effects, changes in the price of non-tradable goods, and assuming no interdependency in the level of public goods that both natives and migrants consume. If these effects are present, not accounting for them in traditional fiscal estimates will most likely underestimate the fiscal effects if immigrants' fiscal position is negative or overestimate their effect if they generate a fiscal surplus.

Overall, our reduced-form findings indicate that immigration did not significantly change natives' average net fiscal contributions between 2013 and 2018. This result is supported by our analysis of the mechanisms potentially affecting natives' contribution. Not only do we not find labor displacement effects, but there is also no evidence of changes in the allocation of public expenditures, widespread increase in housing values, or increase in the cost of public services. Even when we show that increasing immigrant inflows did induce a decline in the wages at the bottom of the wage distribution, particularly for relatively low-skilled workers, this group of workers is more likely not to meet the threshold required to pay income taxes or wealth tax. They are also more likely to be self-employed or work in the informal sector, meaning they usually contribute less to the health and pension systems. Because these add up to be a large share of overall contributions, results showing a slight decline in contributions to value-added and other indirect taxes due to lower wages at the bottom of the distribution do not significantly change natives' average contributions.

Estimates that adjust for aggregate general equilibrium effects that do not consider the heterogeneity in the labor market and the rate at which natives use government services may induce a more considerable bias in fiscal estimates. Even in contexts that have experienced a decline in employment, the job loss does not mean that natives would have paid more taxes and received fewer benefits in the absence of immigration. In addition, looking at changes in average wages does not truly capture the potential impact on fiscal estimates. Differential responses in the bottom and upper parts of the wage distribution might have very different effects on fiscal contributions.

Finally, it is essential to acknowledge that these effects are static and backward-looking in nature. Any intent to obtain a complete picture of the fiscal effects of immigration would require an evaluation—under a set of assumptions—about the future life trajectories of immigrants and their descendants. In addition, we do not directly account for changes in revenues from capital income caused by increasing immigrant inflows because this is addressed in Mesa-Guerra and Ramírez-Tobón (2022). The argument is that profit-maximizing firms hiring additional workers would have had to employ additional capital, creating additional capital income. Omitting this effect has important implications for fiscal estimates and is a usual source of bias in cash flow accounting exercises, as shown by Clemens (2022). This would affect both natives' personal and corporate income tax contributions.

8. Concluding Remarks

While immigration in Colombia is estimated to generate a small fiscal benefit, consistently, more than half of residents consider immigration to be a burden for public finances. Even when we look only at Venezuelan immigrants, who have a negative fiscal position, their effect as a fraction of total economic activity is rather small. Now, a relatively common critique of these types of estimates that are produced using a static fiscal accounting analysis is that they usually miss general equilibrium effects that are mediated through factor markets (labor and capital), changes in the cost of providing public services, and mean service use by both natives and immigrants.

As a result, the estimates produced by this partial-equilibrium approach might likely be underestimating the effect of Venezuelan immigration to Colombia between 2013 and 2018. Using variation in the inflows of immigrants received by Colombian metro areas during this period, we find no evidence that increasing immigrant inflows to Colombia lowered the average per capita contributions of natives. This is consistent with no significant decline in employment, average wages in the upper half of the wage distribution, or hours worked (as part of labor market adjustment), and no real decline in property values or change in the composition of local public spending. In addition, we find no evidence of costs being distributed among a larger population, suggesting that the composition of public expenditures is biased towards services that tend to increase with population size.

The findings in this paper may be specific to the Colombian context and may not hold necessarily after 2018. However, they may still be relevant for understanding the fiscal effects of immigration in developing countries and how local governments adjust to immigrant shocks in the short run. The results presented here also show the need to account for second-order effects to provide a complete view of immigrants' fiscal effects.

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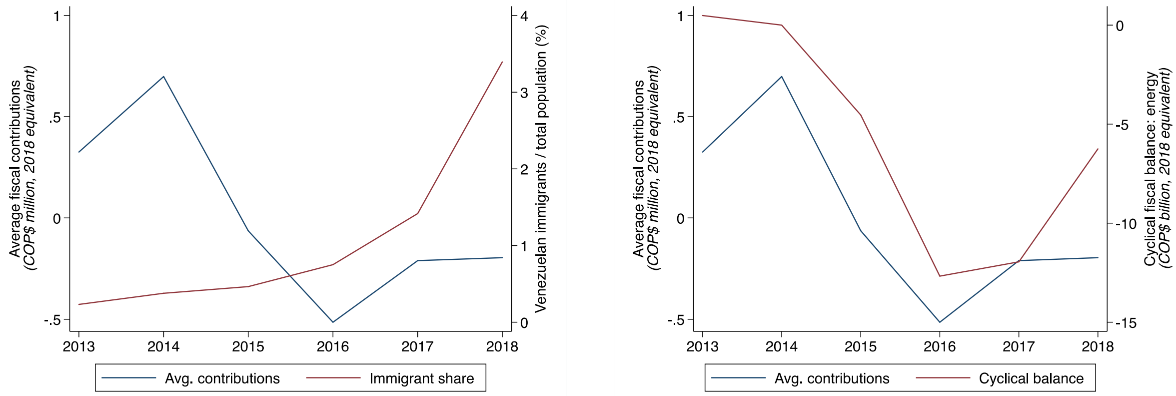
Appendix A. Supplementary Tables and Figures

Figure A1

Fiscal contributions, immigrant shock, and energy cycle

Panel A. Immigrant shock

Panel B. Energy Cycle



Notes: The Figure compares the evolution between 2013 and 2018 of average net fiscal contributions for working-age natives to the immigrant share (Panel A) and the cyclical fiscal balance for the energy sector (Panel B). The data for the energy cyclical balance comes from public records published by the Ministry of Finance and measures the change in fiscal revenues caused by the difference between the observed price and the long-term price of crude oil of the previous period.

Figure A2

Directed Acyclic Graph (DAG) of the Effect of Immigration on Natives' Fiscal Contributions

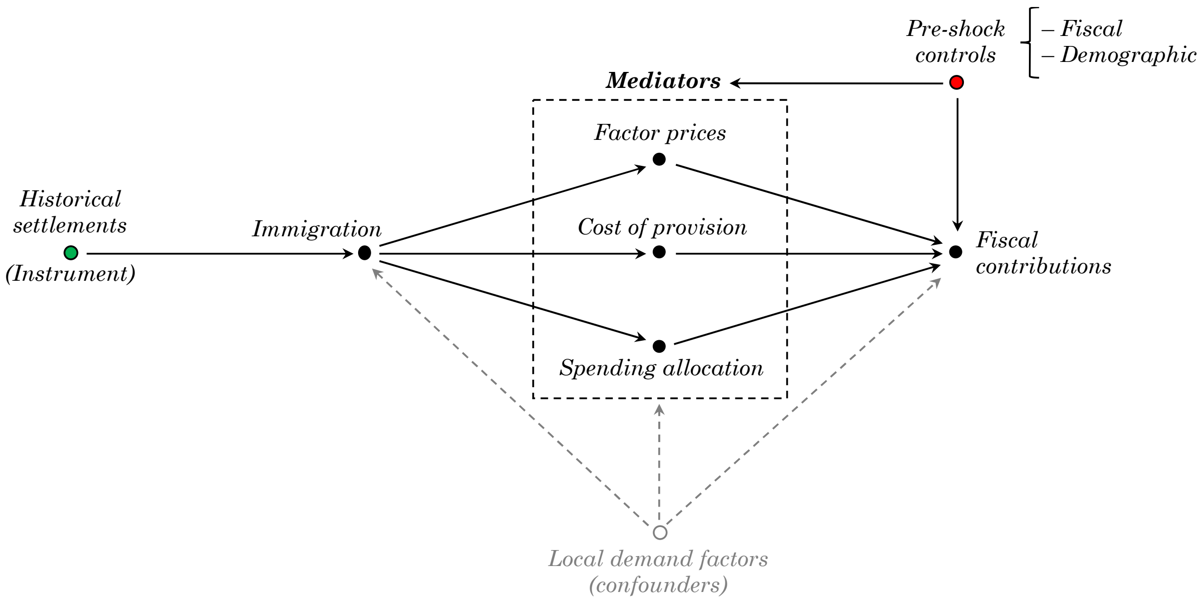
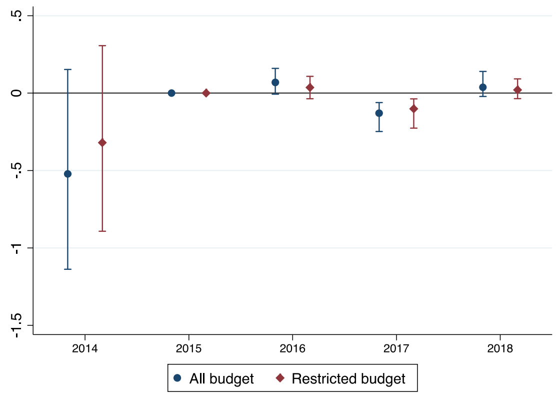


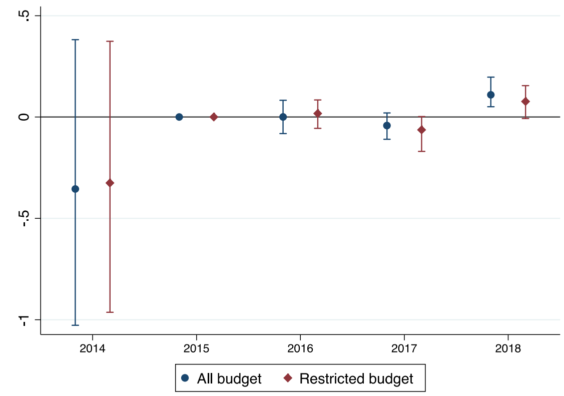
Figure A3

Year-by-year estimates of the effect of immigration on natives' fiscal contributions

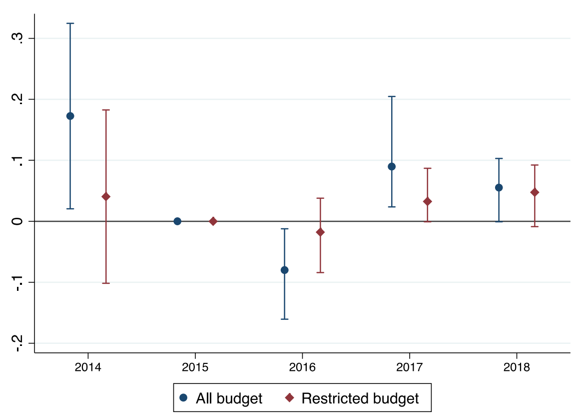
Panel A. Net Fiscal Cost



Panel B. Total Revenues



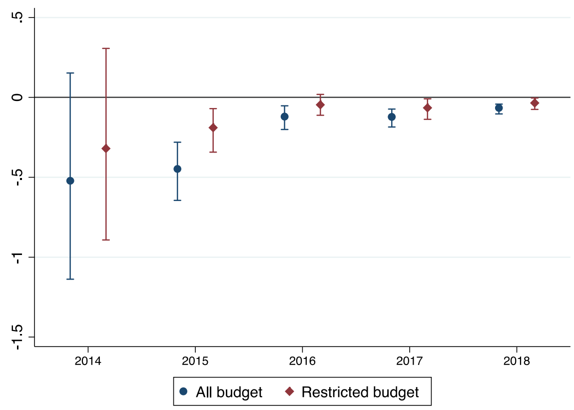
Panel C. Total Expenditures



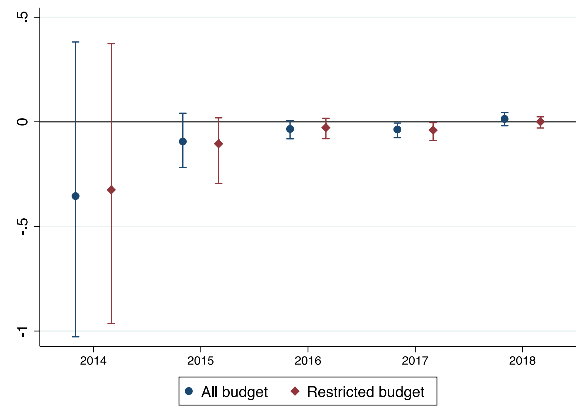
Notes: The Figure reports the year-by-year coefficients obtained by regressing the change in net fiscal contributions (panel A), revenues (Panel B), and expenditures (Panel C) on the change in immigrant inflows (Δm_{jt}). All regressions control for the share of local revenues that accrue to transfers from the central government, the share of expenditures in public goods, the share of working age population in the city, the share of college workers, and the share of workers employed in manufacturing. Anderson–Rubin confidence sets are presented. Coefficients in 2015 are set to zero as a result of weak instruments. Estimates are weighted by the working-age population in 2013.

Figure A4
Estimates of the effect of immigration on natives' fiscal contributions relative to 2013

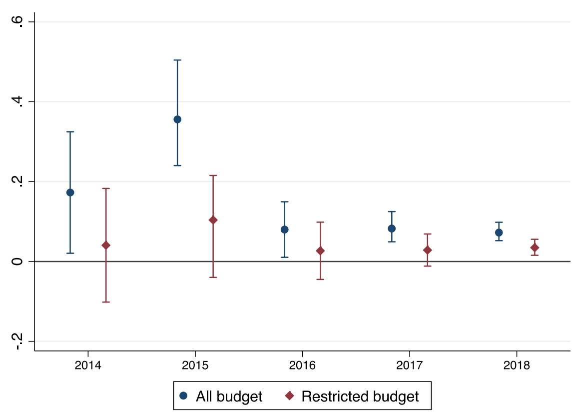
Panel A. Net Fiscal Cost



Panel B. Total Revenues



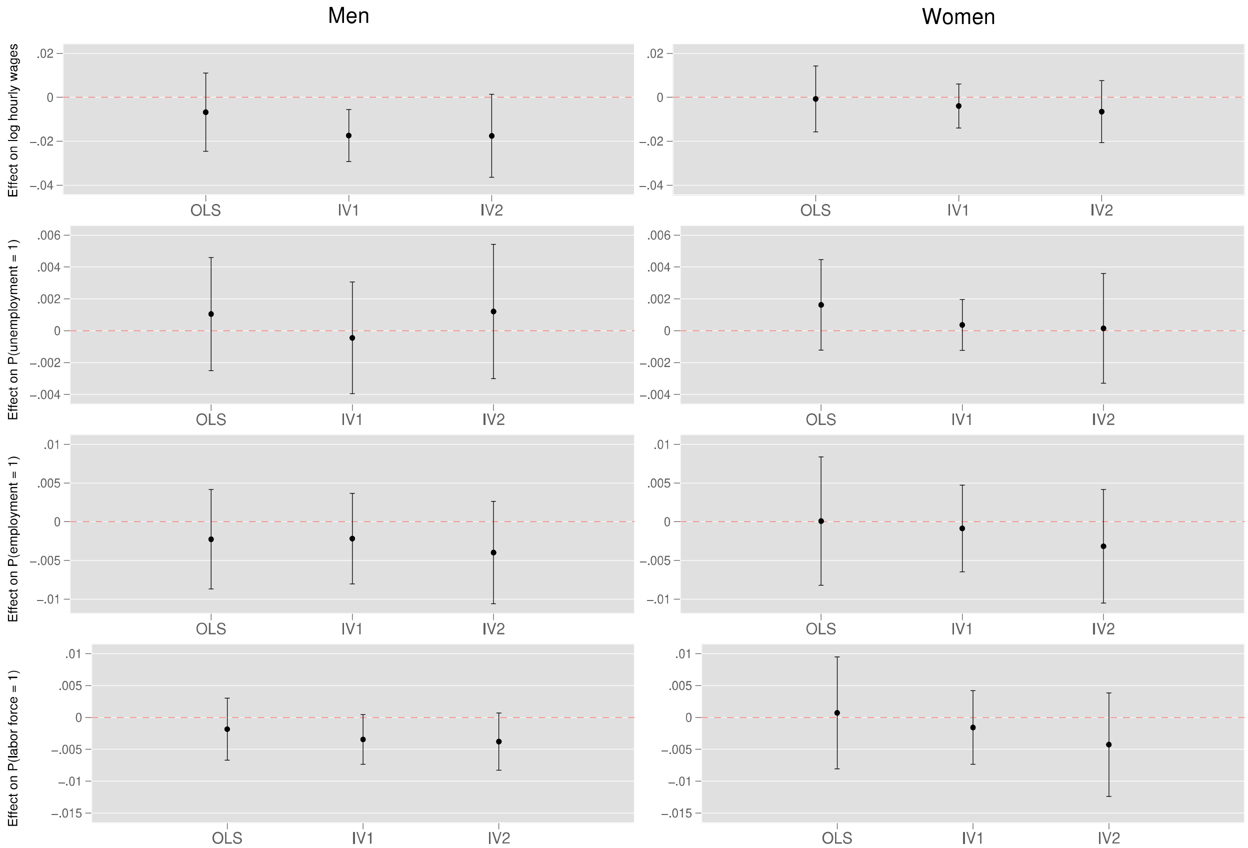
Panel C. Total Expenditures



Notes: The Figure reports the coefficients obtained by regressing the change in net fiscal contributions (panel A), revenues (Panel B), and expenditures (Panel C) on the change in immigrant inflows relative to the local population in 2013. All regressions control for the share of local revenues that accrue to transfers from the central government, the share of expenditures in public goods, the share of working age population in the city, the share of college workers, and the share of workers employed in manufacturing. Anderson–Rubin confidence sets are presented. Estimates are weighted by the working-age population in 2013.

Figure A5

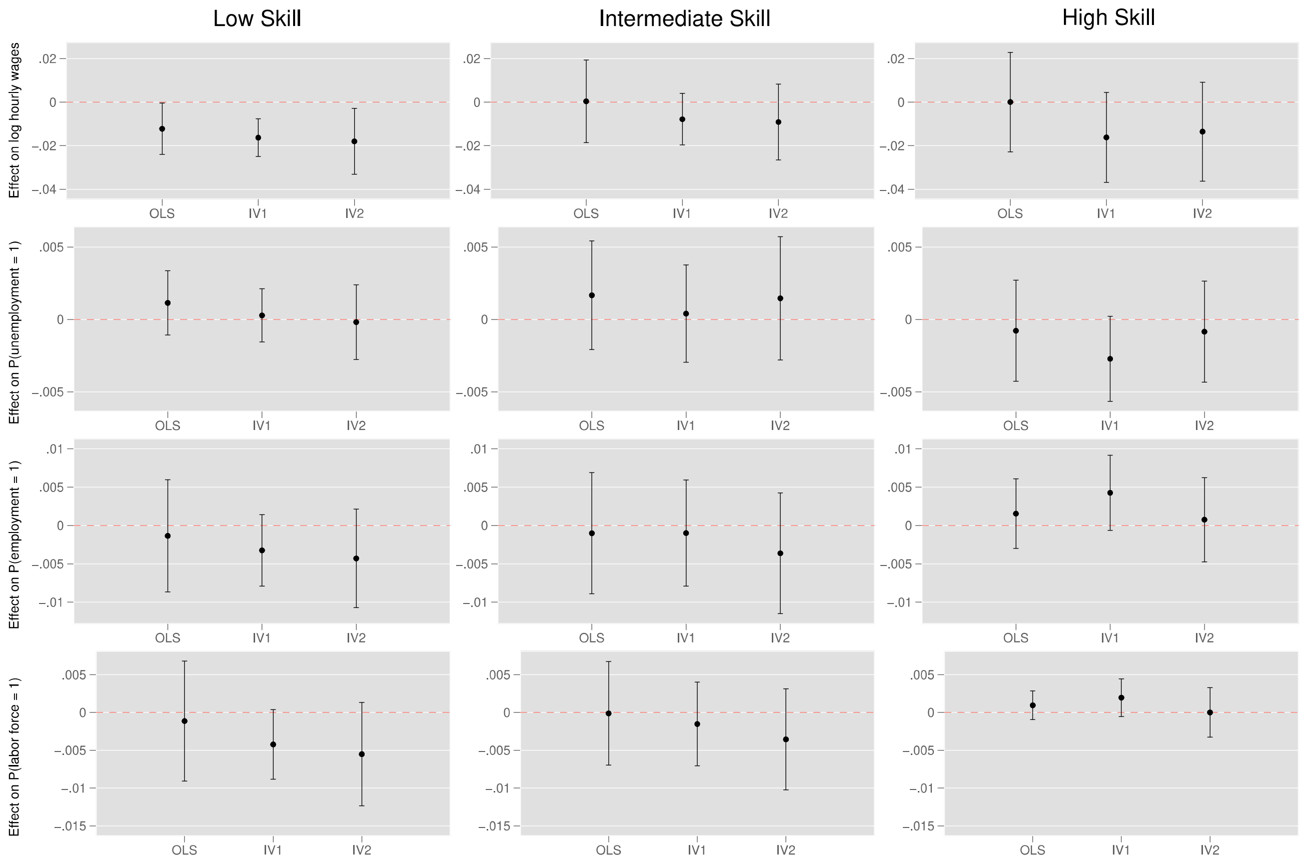
Effect of immigration on main labor market outcomes by sex



Notes: The Figure reports OLS and 2SLS coefficients and the respective 95% confidence intervals obtained by regressing natives' log wages, unemployment, employment, and labor force participation status on the fraction of immigrants ($\widetilde{m}_{jt}$) separated by sex. IV1 instruments $\widetilde{m}_{jt}$ with the past settlement instrument as defined in Eq. (7). IV2 instruments $\widetilde{m}_{jt}$ with the distance instrument as defined in Eq. (9). All regressions include year and area fixed effects, individual controls (sex, age, age squared), and dummies for education achievement (less than high school, high school, some college, college graduates, and graduate degrees). Wages are computed for wage and salary workers and include the labor income of self-employed workers. The sample is restricted to natives aged 15 to 64 living in metropolitan areas (MSAs) from 2013-2018. To alleviate the potential impact of outliers, wages were computed by trimming the wage distribution by year at 0.5% and 99.5%. Wages are expressed in 2018 equivalent COP. Estimates are weighted by sampling weights. Standard errors are clustered at the metropolitan area level.

Figure A6

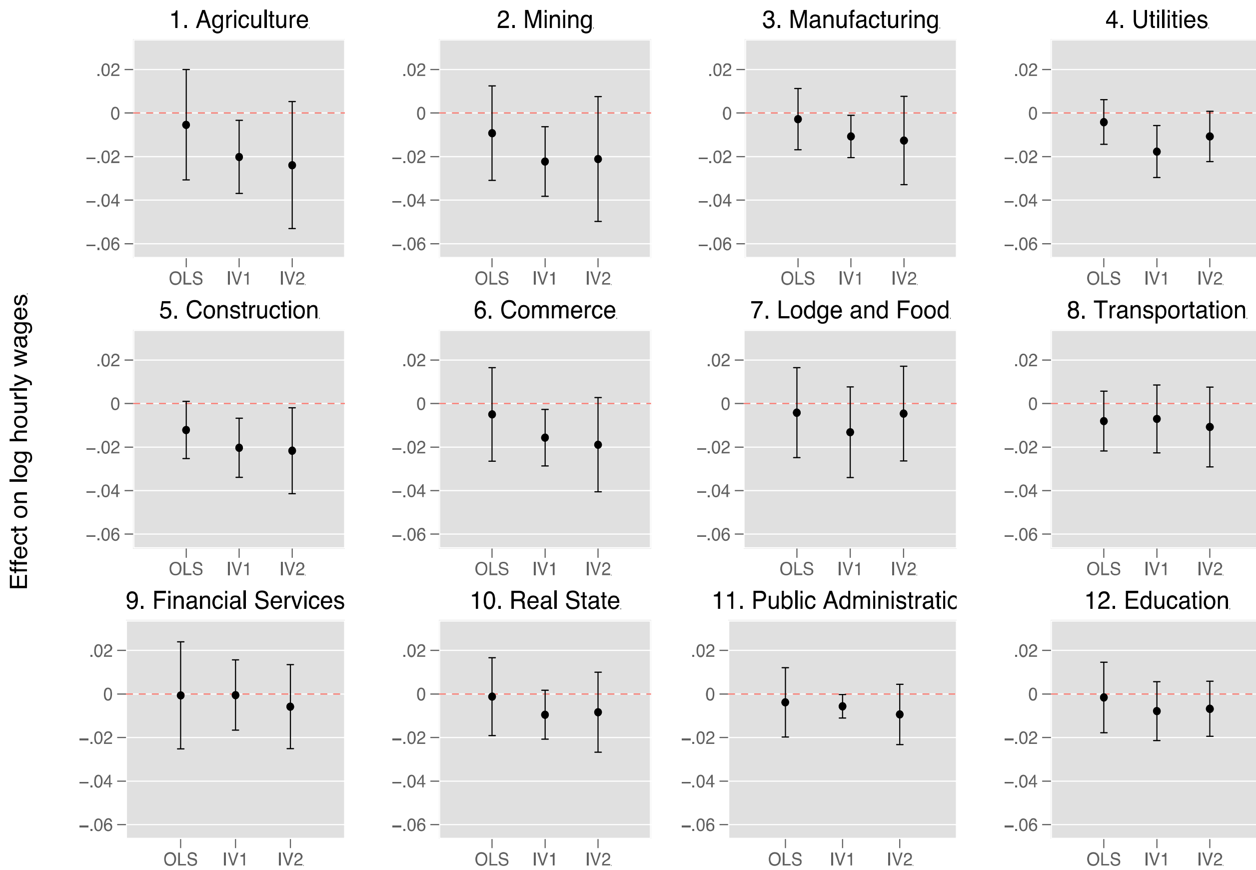
Effect of immigration on main labor market outcomes by skill



Notes: The Figure reports OLS and 2SLS coefficients and the respective 95% confidence intervals obtained by regressing natives' log wages, unemployment, employment, and labor force participation status on the fraction of immigrants ($\widetilde{m}_{jt}$) separated by skill. IV1 instruments $\widetilde{m}_{jt}$ with the past settlement instrument as defined in Eq. (7). IV2 instruments $\widetilde{m}_{jt}$ with the distance instrument as defined in Eq. (9). Low skill: those with less than high-school degrees. Intermediate skill: those with high school degrees or technical degrees. High skill: those with a bachelor's or more. All regressions include year and area fixed effects, individual controls (sex, age, age squared), and dummies for education achievement (less than high school, high school, some college, college graduates, and graduate degrees). Wages are computed for wage and salary workers and include the labor income of self-employed workers. The sample is restricted to natives aged 15 to 64 living in metropolitan areas (MSAs) from 2013-2018. To alleviate the potential impact of outliers, wages were computed by trimming the wage distribution by year at 0.5% and 99.5%. Wages are expressed in 2018 equivalent COP. Estimates are weighted by sampling weights. Standard errors are clustered at the metropolitan area level.

Appendix A7

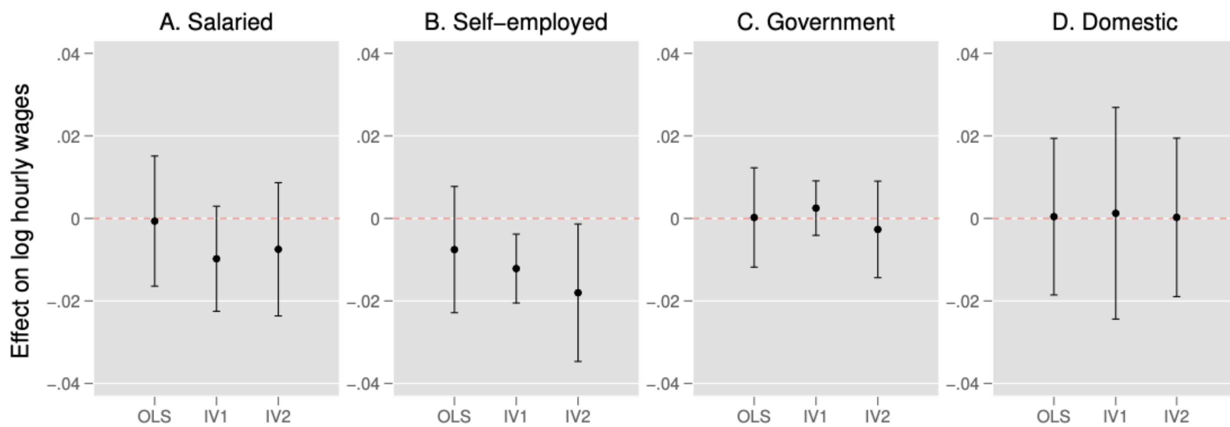
Effect of immigration on wages by sector



Notes: The Figure reports OLS and 2SLS coefficients and the respective 95% confidence intervals obtained by regressing natives' log wages, unemployment, employment, and labor force participation status on the fraction of immigrants ($\widetilde{m}_{jt}$) separated by economic activity. IV1 instruments $\widetilde{m}_{jt}$ with the past settlement instrument as defined in Eq. (7). IV2 instruments $\widetilde{m}_{jt}$ with the distance instrument as defined in Eq. (9). All regressions include year and area fixed effects, individual controls (sex, age, age squared), and dummies for education achievement (less than high school, high school, some college, college graduates, and graduate degrees). Wages are computed for wage and salary workers and include the labor income of self-employed workers. The sample is restricted to natives aged 15 to 64 living in metropolitan areas (MSAs) from 2013-2018. To alleviate the potential impact of outliers, wages were computed by trimming the wage distribution by year at 0.5% and 99.5%. Wages are expressed in 2018 equivalent COP. Estimates are weighted by sampling weights. Standard errors are clustered at the metropolitan area level.

Appendix A8

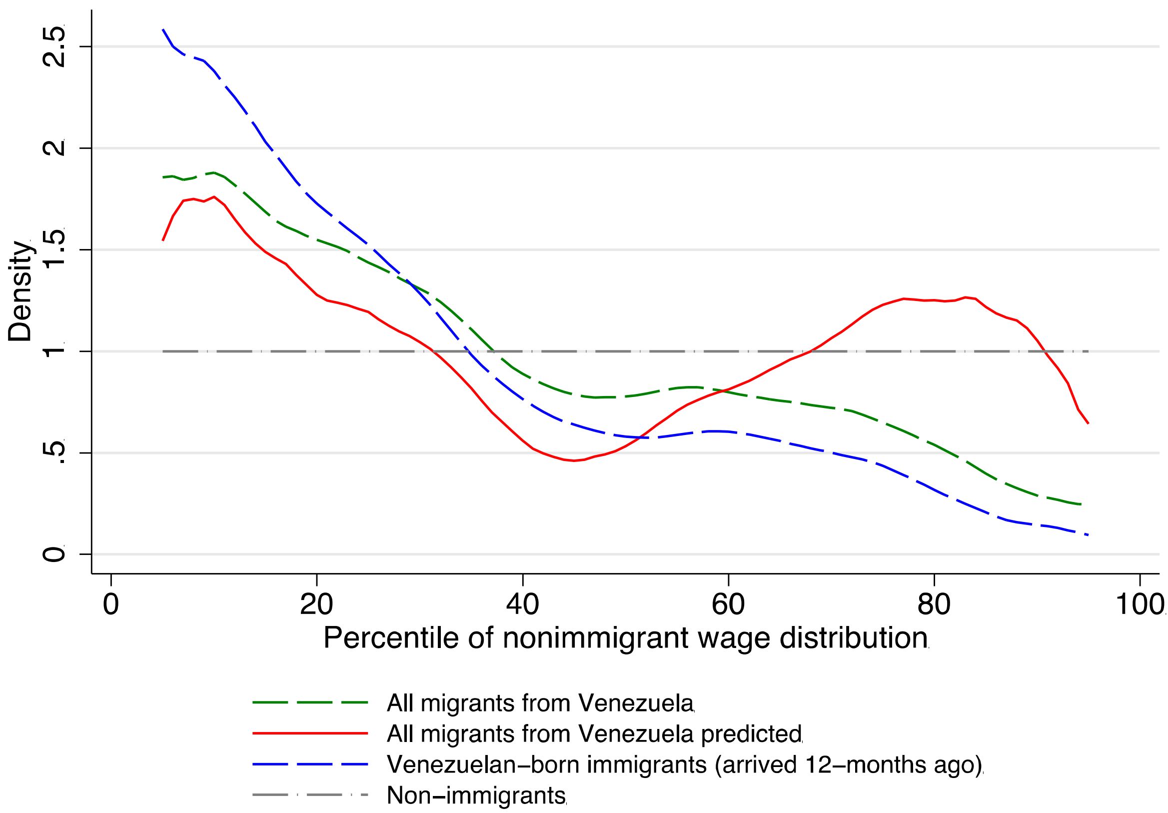
Effect of immigration on wages by type of job



Notes: The Figure reports OLS and 2SLS coefficients and the respective 95% confidence intervals obtained by regressing natives' log wages, unemployment, employment, and labor force participation status on the fraction of immigrants ($\widetilde{m}_{jt}$) separated by type of job. IV1 instruments $\widetilde{m}_{jt}$ with the past settlement instrument as defined in Eq. (7). IV2 instruments $\widetilde{m}_{jt}$ with the distance instrument as defined in Eq. (9). All regressions include year and area fixed effects, individual controls (sex, age, age squared), and dummies for education achievement (less than high school, high school, some college, college graduates, and graduate degrees). Wages are computed for wage and salary workers and include the labor income of self-employed workers. The sample is restricted to natives aged 15 to 64 living in metropolitan areas (MSAs) from 2013-2018. To alleviate the potential impact of outliers, wages were computed by trimming the wage distribution by year at 0.5% and 99.5%. Wages are expressed in 2018 equivalent COP. Estimates are weighted by sampling weights. Standard errors are clustered at the metropolitan area level.

Figure A9

Actual vs. predicted position of Venezuelan immigrants in natives' wage distribution



Notes: The Figure depicts the observed kernel density (green dashed line) and the predicted kernel density estimates for all immigrants from Venezuela, including returnees and Venezuelan-born (red line), and for the Venezuelan-born immigrants that arrived in Colombia within the last 12 months. The predicted density is based on where Venezuelans would be located if they received the same return to education and experience as natives. The predicted line is obtained by estimating a flexible log regression separately for native males and females in each year between 2014 and 2018. The regression includes three age categories (15 to 28, 29 to 40, and 41 to 64), three education groups (less than high school diploma, high school diploma or technical degree, and bachelors degree or more), the interaction between age and education groups, dummies for Bogotá, Medellín, and Cali, and dummies for each quarter of the survey. The resulting coefficients are then used to predict out-of-sample wages for different groups of immigrants from Venezuela. This procedure was based on Dustmann *et al.* (2013). The horizontal dashed gray line shows the native wage distribution as a reference. Kernel estimates are above (below) the horizontal gray line where migrants from Venezuela are more (less) concentrated than natives. Nonimmigrants are all natives, excluding returnees. The sample is restricted to the urban working-age population (15-64 years old) in the labor force who report labor income and are not enrolled in school. To alleviate the potential impact of outliers, wages were computed by trimming the wage distribution by year at 0.5% and 99.5%. Wages are expressed in 2018 Colombian pesos. *Source:* Own estimates using information from the GEIH 2014-2018 for all 23 MSAs.

Table A1

Robustness estimates of the effect of immigration on natives' fiscal contributions – all budget

Estimates of immigrant inflows (m_{jt})	Coef.	SE	F-stat	Anderson–Rubin CI
(1) Net fiscal contributions				
(a) Using the shares from the 1993 census	0.008	0.033	23.707	[–0.046, 0.095]
(b) Normalizing ΔM_{jt} by the local pop. in 2013	0.007	0.035	22.672	[–0.050, 0.100]
(c) Using individual pooled data ($\widetilde{m}_{jt}$)	–0.012	0.014	25.567	[–0.040, 0.017]
(d) Using a distance-based instrument	–0.014	0.030	66.698	[–0.067, 0.049]
(e) Controlling for dynamic bias				
– Contemporaneous term: m_{jt}	0.191	0.101	30.433	[0.051, 0.612]
– Lagged term: m_{jt-1}	–0.278	0.121	74.634	[–0.699, –0.110]
(f) LIML	0.007	0.036	24.212	[–0.042, 0.097]
(2) Revenues				
(a) Using the shares from the 1993 census	0.076	0.037	23.707	[0.023, 0.187]
(b) Normalizing ΔM_{jt} by the local pop. in 2013	0.067	0.041	22.672	[0.001, 0.181]
(c) Using individual pooled data ($\widetilde{m}_{jt}$)	0.029	0.014	27.496	[0.003, 0.059]
(d) Using a distance-based instrument	0.036	0.038	66.698	[–0.032, 0.117]
(e) Controlling for dynamic bias				
– Contemporaneous term: m_{jt}	0.152	0.110	30.433	[–0.002, 0.535]
– Lagged term: m_{jt-1}	–0.125	0.139	74.634	[–0.510, 0.068]
(f) LIML	0.069	0.042	24.212	[0.014, 0.186]
(3) Expenditures				
(a) Using the shares from the 1993 census	0.051	0.024	23.707	[0.007, 0.106]
(b) Normalizing ΔM_{jt} by the local pop. in 2013	0.048	0.025	22.672	[–0.002, 0.105]
(c) Using individual pooled data ($\widetilde{m}_{jt}$)	0.034	0.007	25.935	[0.020, 0.047]
(d) Using a distance-based instrument	0.040	0.021	66.698	[0.003, 0.084]
(e) Controlling for dynamic bias				
– Contemporaneous term: m_{jt}	–0.070	0.074	30.433	[–0.275, 0.084]
– Lagged term: m_{jt-1}	0.179	0.073	74.634	[0.028, 0.381]
(f) LIML	0.049	0.026	24.212	[–0.002, 0.106]

Notes: The Table reports various estimates of the effect of changes in the fraction of immigrants on natives' net fiscal contributions, tax contributions, and expenditures. All regressions include year dummies and interactions of MSA-level controls with year dummies. Results are net of individual-level controls (sex, age, education) and computed by trimming the distribution of contributions each year at 1% and 99%. We report 5%-level identification-robust Anderson–Rubin confidence sets. Results are expressed as 2018 equivalent COP$ million.