Migrant Exposure and Anti-Migrant Sentiment: The Case of the Venezuelan Exodus

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Abstract

The global increase in refugee flows and anti-migrant politics has made it increasingly urgent to understand when and how migration translates into anti-migrant sentiment. We study the mass exodus of Venezuelans across Latin America, which coincided with an unprecedented worsening in migrant sentiment in the countries that received the most Venezuelans. However, we find no evidence that this decrease occurred in the regions within-country that received the most migrants. We do this using multiple migrant sentiment outcomes including survey measures and social media posts, multiple levels of geographic variation across seven Latin American countries, and an instrumental variable strategy. We find little evidence for heterogeneity along a range of characteristics related to labor market competition, public good scarcity, or crime. The results are consistent with anti-migrant sentiment being a national-level phenomenon, divorced from local experiences with migrants.

Keywords: migration, discrimination, social capital

JEL Codes: F22, Z13, A13, J15

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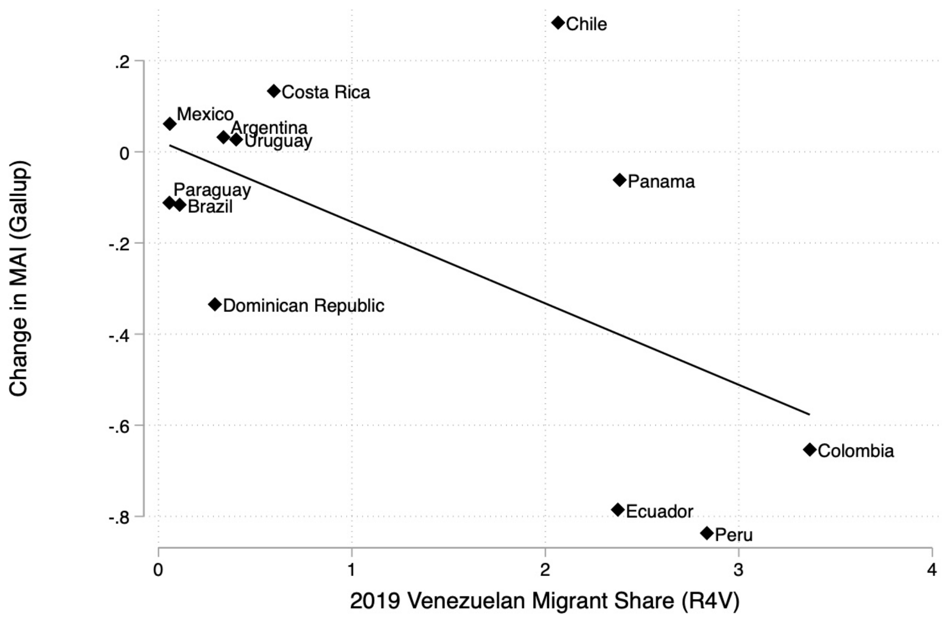
I Introduction

A series of severe political and economic crises have gripped Venezuela since 2013. Venezuela soon became one of the poorest and most violent countries in Latin America, and as a result, 4.5 million people — almost 1 in 5 Venezuelans — had fled the country by 2019 (Graham et al., 2020, World Bank, 2018). This mass displacement made Venezuela the second-largest external displacement crisis in the world (UNHCR, 2019). Venezuelan migrants resettled across several countries in Latin America, most notably Colombia — where they compromised roughly 4% of the population in 2019 — Ecuador, Peru, and Chile (R4V, 2019). Despite a shared language, religion, and broad cultural heritage, xenophobic responses to Venezuelans at the institutional and grassroots levels continue to pose an urgent policy challenge across Latin America (Migration Policy Institute, 2020, Chaves-González et al., 2021). These challenges are not unique to the Venezuelan case. The number of forcibly displaced people worldwide more than doubled from 41 million in 2010 to 110 million in 2023, with half of these seeking safety across international borders (UNHCR, 2022, Christophersen, 2023). Importantly — despite representing the bulk of scholarly work on refugee integration — Europe and North America receive just a fifth of the worlds' refugees (Christophersen, 2023). Instead, most refugees are displaced to neighboring countries that often share a language, religion, and culture, but where refugees are nevertheless perceived as outsiders who strain scarce public resources and job markets, among other pressures.

This unprecedented mass migration across Latin America has coincided with an equally unprecedented worsening of attitudes toward migrants at the national level. Figure I shows that migrant arrival at the country level is strongly correlated with a country-wide worsening of Gallup World Poll's "Migrant Acceptance Index" (MAI), which measures feelings towards having immigrants in the country, immigrants as neighbors, and immigrants marrying into the family. Colombia, Ecuador, and Peru — the three countries that received the most Venezuelans — experienced the largest drop of all 129 countries covered by this Gallup index over this period.

We investigate whether these countrywide trends appear at a sub-national level. Is anti-migrant backlash largest in regions that received the most migrants within-country? We might expect such a backlash given that natives living in host communities are more directly exposed to the potential effects of migration. These effects may occur through various mechanisms. The first channel is economic. Globally, the effect of migrant arrivals on hosts' labor market outcomes are null or sometimes even positive (Alesina & Tabellini, 2022). However, there may be short-term economic consequences for those who compete

Figure I: 2016-2019 Change in Migrant Acceptance across Countries



Gallup's Migrant Acceptance Index (MAI) is a standardized index based on 3 questions about migrants. A larger value indicates more favorable attitudes towards migrants. See Data section for details.

most directly with migrants, and in this context there is evidence that Venezuelan migration depressed wages for lower-income workers in the informal sector in various recipient countries (Lebow, 2022b, Delgado-Prieto, 2021, Olivieri et al. , 2022). Second, host communities may suffer strain over public services, including health and education services, especially if those areas were already under-served. There has been widespread concern about this migration wave putting pressure on public services provision in many regions (Oxfam, 2020, Migration Policy Institute, 2020, Namen et al. , 2021).[1] Third, host communities may have concerns about security, though there is little evidence that Venezuelan migration has increased crime (Ajzenman et al. , 2023, Bahar et al. , 2020, Knight & Tribin, 2020, Groeger et al. , 2022).

Another channel that can operate at a local level is the inter-group contact hypothesis. This hypothesis stipulates that interpersonal contact across social lines can reduce prejudice, build empathy, and improve inter-group relations, so long as contact is cooperative, socially acceptable to both sides, and places individuals on equal footing (Paluck et al. , 2019, Allport et al. , 1954). Despite concerns about conflict over resources, such contact seems plausible in the Venezuelan case. Venezuelans have settled freely in urban areas (as opposed to camps), and the linguistic, religious, and cultural overlap between migrants and natives has been

[1] A nationally representative survey found that 70% of respondents in Colombia, Peru, and Ecuador believe migrants increase crime and insecurity, lower salaries, and worsen working conditions (Oxfam, 2020).

shown to reduce perceived cultural threat and make social interaction more likely (Tabellini, 2010, Choi et al. , 2019, Hainmueller & Hopkins, 2014). Note, however, that exposure to migrants does not guarantee contact with migrants — in the framing of Chetty et al. (2022), 'friendship bias' can remain even after 'exposure bias' has been overcome. Indeed, previous studies have showed that relative isolation can persist across groups even when living in close proximity to each other (Bazzi et al. , 2019). Further, even when contact does occur, there is no guarantee that it will be positive, and negative contact experiences can have outsize effects on prejudice relative to positive ones (Paolini et al. , 2010).

Finally, a perceived out-group threat mechanism may be at work. Perceptions of out-group threat are sometimes — but need not be — driven by cultural concerns. Indeed, natives' fear that migrants will be unable or unwilling to assimilate into local culture is a major global driver of anti-migrant sentiment (Alesina & Tabellini, 2022, Hainmueller & Hopkins, 2014), particularly when migrants and natives are more culturally distant (Tabellini, 2020, Alesina et al. , 2021). Cultural distance is not needed for natives to feel threatened by an out-group threat, however — boundaries of the in-group and out-group are malleable and highly responsive to demographic and political changes (Fouka & Tabellini, 2022, Fouka et al. , 2022).[2] In the context we study, Venezuelans speak the same language as natives, share the same religion, and are not always immediately recognizable as foreigners, though they are typically recognizable by their distinctive accent. Regardless of their degree of visibility and cultural distinctiveness, Latin Americans have responded to the Venezuelan migration by "otherizing" them as an out-group using stereotypes and strongly-worded rhetoric, even in Colombia where the cultural overlap is greatest (Bellino & Ortiz-Guerrero, 2023, Blouin & Zamora Gómez, 2022, Rosales, 2020).

Each of these channels — economic competition, public services provision, security, inter-group contact, and cultural or group threat — most directly affect those who are living in host communities. However, sentiments may also change among those living in areas that have received little migration. There may be strong national perceptions about the effects of migration on economic competition, public services, or security in host communities that may be misinformed and divorced from local experiences with migrants (Alesina et al. , 2018, Ajzenman et al. , 2023). Cultural or group threat may also activate on a national scale, in particular through political rhetoric or migrant portrayal in media (Barrera et al.

[2] For instance, Mexican immigration to the U.S. improved White Americans' attitudes toward Black people by shifting the salient social cleavage from race to immigrant status (Fouka & Tabellini, 2022). Similarly, the Great Migration of Black Southerners to urban centers in the American North improved relations between European immigrants and native-born Whites in these areas, shifting the salient social cleavage from nationality to race (Fouka et al. , 2022).

, 2020, Müller & Schwarz, 2021, Djourelova, 2023, Campo et al. , 2021). In this paper, we do not directly test for the role of these national-level channels. Instead, we study the local within-country effects of migrant arrival on attitudes toward migrants. As we will see, we find precise null effects of local migration on xenophobic sentiment, which points to the possible importance of national-level channels in explaining the worsening attitudes toward migrants observed across Latin America in response to Venezuelan migration.

Various elements of this migration wave make it particularly suited for this analysis. First, this migration wave was large in scale, covering a vast region in Latin America and creating extensive geographic variation in migrant exposure across and within countries. Second, it was sudden. The majority of Venezuelan out-migration occurred over only two years, between 2017 and 2019. This left little time for individuals or institutions to adapt to the incoming migration, and there was minimal international aid targeted to the Venezuelan refugee crisis over the period of analysis (Bahar & Dooley, 2019). Third, this was a push-factor migration wave, driven by hyperinflation and drastic increases in crime and poverty in Venezuela, and unrelated to economic changes occurring in the rest of Latin America. Venezuela was the only country to produce significant numbers of migrants into our seven study countries during this period.

Our analysis can be divided into two components. First, we study the effect of migrant arrivals on changes in Gallup's MAI at the department or province level across seven Latin American countries. This index was introduced for the first time in 2016 and was included again in 2019, creating an ideal pre-post comparison for the large wave of Venezuelan migration that occurred between 2016 and 2019. To complement the Gallup outcome, we also study effects on a more extreme, negative behavioral outcomes: anti-migrant tweets. In the second component, we study the effects of migrant exposure at a smaller geographic level, across 118 municipalities in Colombia, which is the country hosting the largest share of Venezuelan migrants. Working in partnership with the Colombian National Administrative Department of Statistics (DANE), we added a series of questions to a large-scale nationwide survey in 2019 on trust toward foreigners, preferences for having a foreigner as a neighbor, and contact with foreigners. While this analysis has greater geographic precision, the caveat is that we do not have pre-period data, and we compensate for this by controlling for a long list of pre-period socioeconomic characteristics. Each analysis has strengths and limitations, and each finds null effects of local Venezuelan arrival on local attitudes toward migrants, with confidence intervals that rule out any non-trivial negative effects.

In our initial analysis, we make the key assumption that the arrival of migrants to a particular region is largely exogenous, and we show that places that received larger and smaller numbers of Venezuelan migrants had a similar trend in attitudes toward migrants before 2015. This strategy, however, cannot properly deal with endogenous location sorting — for example, Venezuelans migrating to regions with a more positive sentiments toward migrants (reverse causation) or with characteristics correlated with these sentiments (omitted variable bias). Thus, we also employ an instrumental variable (IV) based on the historic share of Venezuelans in each location in previous censuses, a common approach in the migration literature (Altonji & Card, 1991, Card, 2001). Because there was historically little migration out of Venezuela, these shares are small and therefore unlikely to directly affect long-term economic outcomes or preferences (Jaeger et al. , 2018). Supporting this assumption, we do not find pre-trends correlated with the instrument in variables related to migrant sentiment, economic outcomes, or other regional characteristics. At the same time, there were strong network effects driving where Venezuelans settled within-country, creating a strong first stage for our 2SLS analysis (Namen et al. , 2021). The results from the 2SLS analysis are similar to those from the OLS analysis in both the multi-country and Colombia-specific analysis.

In each analysis component, and for both OLS and 2SLS specifications, the null effects hold under a variety of robustness checks. In the multi-country analysis, this includes dropping each country one-by-one, using various measures of migration, dropping regions with very high migrant shares, and adding year trends interacted with pre-period controls. In the Colombia analysis, this includes dropping municipalities along the Colombia-Venezuela border, dropping Bogotá, including department fixed effects, and controlling for migrant shares in neighboring municipalities. In both analyses, the robustness tests indicate that the effect of local migration on migrant sentiment is not negative and in fact may be slightly positive.

We also explore whether these null results mask heterogeneity at the individual and community levels, with a focus on three mechanisms that we highlighted at the start of this section — economic competition, public goods scarcity, and crime. We find no evidence that poorer people or regions respond worse to migration, despite the fact that economic competition in this setting is highly concentrated among lower-income workers (Lebow, 2022b, Delgado-Prieto, 2021, Olivieri et al. , 2022). This result is consistent with evidence from various settings that economic forces tend to explain little variation in native attitudes towards immigration policies (Alesina & Tabellini, 2022, Hainmueller & Hopkins, 2014, Card et al. , 2012). Next, we fail to find evidence that the effects of migration differ according to pre-period crime rates, consistent with evidence that Venezuelans have had no effect on crime rates (Ajzenman et al. , 2023, Bahar et al. , 2020, Knight & Tribin, 2020, Groeger et al. , 2022). Likewise, we find that rural areas, and areas with worse public goods, do not respond more negatively to Venezuelan migration. In fact, they respond *more positively*.

This suggests that those better off are the most sensitive to *possible* strain, rather than those living with already-strained public goods. Finally, we find in the Colombia analysis that the positive effects of migrant exposure are dampened among those who consume news online, consistent with existing evidence that migrant portrayal in social media can causally affect attitudes toward migrants (Barrera et al. , 2020, Müller & Schwarz, 2021, Djourelova, 2023). However, we see these results as only suggestive, considering that these regional and individual characteristics may be correlated with other unobserved characteristics.

That the results are non-negative, and if anything are slightly positive, implies that the contact hypothesis may play a role in improving migrant sentiments at a local level. While we are unable to test this directly, we show that migrant inflows did in fact foster inter-group contact, which as discussed does not always occur after a migration wave (Chetty et al. , 2022). In the Colombia analysis, using data on self-reported social networks, we find that natives in regions with more Venezuelans are more likely to have Venezuelans in their close network, defined as someone whom they regularly visit (or receive a visit) at home, or someone whom they helped (or received help) in finding a job. These outcomes represent a high bar for inter-group contact, as they entail meaningful, repeated interactions rather than fleeting exposure.

I.A Literature and Contribution

This study makes two key contributions to the rich literature on how natives respond to mass migration outside of lab settings. First, we study a global South context, which has a small but growing literature on migrant sentiments despite hosting the majority of refugees and migrants worldwide (World Bank, 2023, UNHCR, 2022, Christophersen, 2023). These regions are often distinct because of heightened precarity of economic opportunities, security, and public goods. They are also more likely to have migration and displacement between neighboring countries with greater cultural overlap between migrants and natives, which increases the propensity for positive inter-group contact (Christophersen, 2023, Alrababa'h et al. , 2021). Both characteristics are true in the Venezuelan case.

Second, our nuanced trust and tolerance outcomes complement studies that measure political preferences and voting for far-right parties. Various studies have found negative effects of migrant exposure on preferences for redistribution, public goods expenditure, and immigration-friendly border policies (Alesina et al. , 2021, Tabellini, 2020, Enos, 2014a). While some studies find that migrant and refugee exposure increases vote shares for far-right anti-immigrant parties (e.g. Halla et al. (2017), Alsan et al. (2020), Dinas et al. (2019)), others find the opposite (e.g. Gamalerio et al. (2020), Vertier et al. (2022)) or high degrees of heterogeneity according to local characteristics. For example, some studies find that anti-immigrant political backlash is tampered in urban settings (Mayda et al., 2020, Dustmann et al., 2019). Others find evidence consistent with the contact hypothesis, in which pro-immigrant voting increases when there is sustained (not transitory) contact between migrants and natives — for example, in Austria (Steinmayr, 2020), Finland (Lonsky, 2021), Italy (Campo et al., 2023), and the Netherlands (Albrecht et al., 2020).[3] Across Latin America, Martinez-Correa et al. (2022) find that Venezuelan migration reduced preferences for redistribution, and in Colombia Rozo & Vargas (2021) find that Venezuelan migration increased political participation and voting for the right. However, this result highlights an important distinction between voting outcomes and directly measured preferences — Colombian political parties had not yet taken a strong stance on migration, and the authors explain that this result does not reflect anti-migrant sentiment but rather the Colombian right using the Venezuelan exodus to highlight the dangers of far-left socialist policies.[4] We confirm the results of Rozo & Vargas (2021) using our data and empirical strategy in Colombia — Venezuelan migration increases voter turnout and shifts political affiliations to the right, while having no effect on migrant sentiments.

We join a handful of studies of the effects of migration on directly measured attitudes toward migrants outside of a controlled lab setting. Notably, Bursztyn et al. (2021) show that exposure to Arab Muslims in the U.S. boosted long-term accepting behaviors. On the other hand, Hangartner et al. (2019) and Enos (2014b) find the opposite — that exposure to transit refugee flows in Greece, and to Spanish-speakers in commuter train stations in Boston, increased natives' hostility toward migrants, possibly driven by a lack of meaningful inter-group contact. In our setting, the potential for meaningful contact is larger because of the cultural and linguistic overlap between hosts and migrants, and this is an important difference from many upper-income country contexts. At the same time, however, the Latin American experience shows that out-group boundaries are highly flexible and can easily shift to demonize migrants that are otherwise culturally similar (Fouka et al., 2022). Our results most closely generalize to other cases in the global South where migrants and natives are more likely to have cultural overlap, which, as discussed, is relevant for the majority of the world's migration and refugee flows.

[3]Other studies also find evidence consistent with inter-group contact improving migrant sentiment, though it is difficult to establish a causal relationship. Betts et al. (2023) finds that East Africans who live near refugee camps are significantly more welcoming when they have contact with refugees. Ghosn et al. (2019) finds similar evidence in Lebanon. These results are consistent with extensive evidence from a laboratory setting that contact "typically reduces prejudice" (Paluck et al., 2019).

[4]Holland et al. (2021) also finds that Colombians overestimate the extent to which Venezuelan migrants in Colombia support socialist policies, and there was a fear among some Colombians that Venezuelans would support the left in local elections, despite the fact that most Venezuelan migrants do not vote.

The paper proceeds as follows: Section II presents background on the Venezuelan exodus. Section III covers the analysis of the effects of Venezuelan migration on migrant sentiments across 116 regions of 7 major recipient countries in Latin America. Section IV covers the analysis of 118 municipalities in Colombia. Section V concludes.

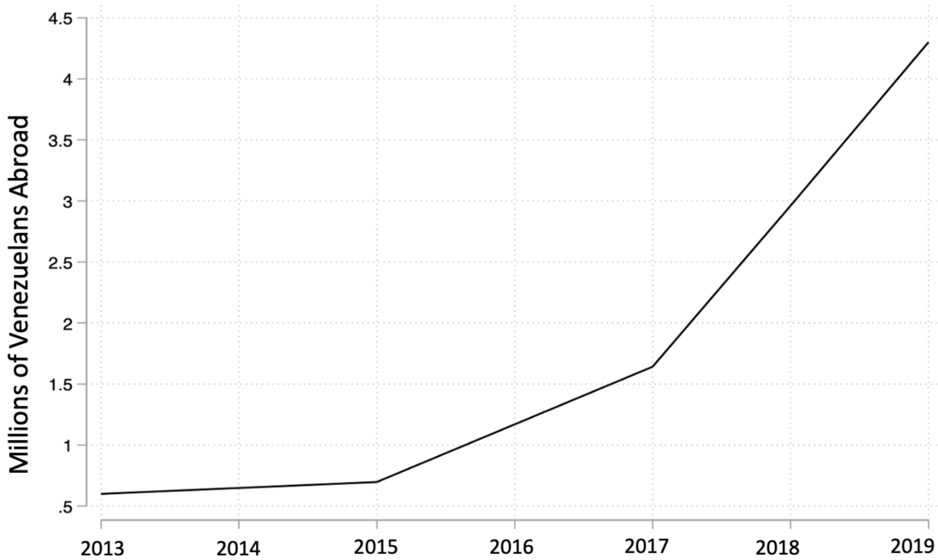
II Background on the Venezuelan Exodus

Since the death of Hugo Chávez and the rise to power of Nicolás Maduro in 2013, Venezuela has struggled with hyperinflation and negative economic growth, leading to large-scale increases in poverty and violence and decreases in access to food, health care, and education. During our study period of 2015 to 2019, 4.8 million Venezuelans fled the country, at the time making Venezuela the second-largest country of origin for internationally displaced refugees after Syria (Migration Policy Institute, 2020). The timing of this migration loosely follows the intensity of the crisis in Venezuela. As displayed in Figure II, migration picks up substantially in 2018, when Venezuela reaches a hyperinflation rate of 2 million percent and when the murder rate becomes the highest in Latin America (Wilson Center, 2019). When surveyed in 2018, displaced Venezuelans across Latin America cited food and medicine shortages, violence and insecurity, lack of access to social services, and the fear of political persecution as their primary reason for migration (UNHCR, 2018).

Until the late 1990's, favorable economic conditions meant that there was relatively little emigration from Venezuela. This began to change under the socialist policies of Hugo Chávez, as members of the wealthier class began to migrate, primarily to the U.S. and Spain, with concerns about private sector performance and economic instability (Freitez, 2011). These emigrants were highly educated and skilled. In contrast, the migration wave under study is much more representative of the Venezuelan population at large. Although men were slightly over-represented during the initial migration periods, gender distributions have balanced over time, and by 2019 between 40-60% of Venezuelan migrants were male across most countries in Latin America. The migrant profile is younger than the Venezuelan population on average — in Colombia, Peru, and Ecuador in 2019, about three out of four Venezuelan adult male migrants and refugees were between the ages of 18 and 35. Finally, levels of education among Venezuelan migrants tend to be high, typically similar to or higher than that of the native population (Chaves-González et al. , 2021).[5]

[5]High-quality labor market data on Venezuelan migrants in Colombia reveal the similarities between Venezuelans at home and abroad. According to UN Demographic and Social Statistics projections, in 2013 the mean age in Venezuela was 30 and women constituted 49.9% of the population. UN Human Development Reports also predict that the mean completed years of schooling in 2015 was 10.1. Compare this with the

Figure II: Venezuelan Emigration over Time



Sources: IOM, Interagency Coordination Platform for Refugees and Migrants from Venezuela (R4V)

Not only was there historically little migration out of Venezuela, but there was, in fact, extensive migration to Venezuela, as migrants from across Latin America sought to benefit from Venezuela's relatively prosperous economy and generous social programs. This is especially true for Colombia. Over the course of Colombia's decades-long civil war, millions of Colombians are believed to have fled to Venezuela. Colombia and Venezuela have historically had close relations, characterized by regular trade exchanges and migratory flows, and this is rooted in their history as a common nation "Gran Colombia" that dissolved in the mid-19th century. As a result, Colombia's immigration policy and rhetoric today have been widely perceived as relatively accepting.[6]

However, the initial positive reception soon began to change, driven in large part by concerns around strained health and education systems, crime, and labor market competition. Colombia's unemployment rate hovers around 10% and the informal sector is large, and there is evidence that competition with Venezuelan workers has reduced wages for low-skill and informal workers (Lebow, 2022b, Delgado-Prieto, 2021, Caruso et al. , 2019). Large protests in Bogotá in 2019 demanded, among many things, stricter border controls with Venezuela, and this coincided with increases in threats and violent attacks against Venezuelans (Colom-

average Venezuelan migrant according to the 2018 household labor survey in Colombia (GEIH), who was 24 years old, completed 10.5 years of schooling and had a 48.6% change of being female.

[6]For example, Colombian President Iván Duque repeatedly referred to Venezuelans as "our Venezuelan brothers" and announced before the UN in 2018 that the two countries were "united by fraternity"(Financial Times, 2019).

bia Reports, 2018, Reuters, 2019). This adds important context to our study because it implies that before 2015 there was little to no anti-Venezuelan sentiment in Colombia, a fact that has since changed. This story is broadly true across many countries in Latin America — in 2016, the 19 Latin American countries included in the Gallup MAI had more positive migrant sentiment than any other continent excluding Sub-Saharan Africa. Between 2016-2019, Latin America experienced the largest decrease in the MAI of any continent, led in particular by Colombia, Ecuador, and Peru.

An important characteristic of this migration wave is that there are relatively few restrictions on Venezuelans' mobility across borders, resulting from decades of regional and bilateral mobility agreements in Latin America. At the start of the migration, fewer than half of countries in Latin America had a passport requirement for Venezuelan entry. Since then, Ecuador, Peru, Chile, and other states have introduced passport or visa requirements. These increases in mobility restrictions have shifted Venezuelan migration from regular to irregular channels (Migration Policy Institute, 2020). In Colombia, Venezuelans typically bypass entry requirements by walking across "trochas", or footpaths around the official border crossings (World Bank, 2018). Most migrants then travel through Colombia through a combination of walking, taking the bus, and hitchhiking, either settling in Colombia or continuing through to Ecuador, Peru, or the Southern Cone (UNHCR, 2019).

Absent existing pathways to regularization for Venezuelans, various countries, including Colombia, Peru, and Ecuador, have launched regularization programs. However, coverage is not universal and migrants often choose not to register. It was estimated that, in late 2019, less than half of all Venezuelan migrants in the region had a form of legal status, and the vast majority continue to work in the informal sector (Migration Policy Institute, 2020). Finally, unlike with many other episodes of forced displacement, there are few refugee camps and relatively little international aid has been dedicated to the reception of Venezuelan migrants (Bahar & Dooley, 2019). To summarize, this was a migration wave of relatively uncontrolled mobility across borders, with migrants settling where they choose within-country, and typically working informally.

III Effects of Immigration on Migrant Acceptance across Latin America

We begin by considering the effect of exposure to migrants at the regional level on attitudes toward migrants. After this, in Section IV, we will consider the effects of migration at a much finer geographical level in Colombia, with a greater emphasis on testing mechanisms and heterogeneity.

III.A Data

Attitudes toward migrants: We use data from the seven Latin American countries: Argentina, Chile, Colombia, Ecuador, Panama, Peru, and Uruguay. These represent the seven countries in Latin America with the largest Venezuelan inflow as a share of the national population which were also included in the Gallup Migrant Acceptance Index (MAI), our primary measure of migrant sentiments.[7] The MAI was fielded by Gallup in 2016 and 2019 and it takes the principle component of responses to the following three questions: *"Do you, personally, think each of the following is a good thing or a bad thing: Immigrants living in (country); An immigrant becoming your neighbor; An immigrant marrying one of your close relatives"*, where "bad" is coded as 1, "depends/unsure/don't know" as 2, and "good" as 3. This is one of the few — and perhaps the only — population-representative surveys in Latin America with identical measurements of attitudes toward migrants before and after the bulk of the Venezuelan migration began in 2017.[8] We use this data to construct a panel at the sub-national level (department or province, from now on referred to as "region") and match it with the change in the Venezuelan population at the same level of aggregation.

A limitation of this data is that the MAI questions were not asked before 2016, restricting out ability to test for pre-trends. Therefore, we complement our analysis with data on migrant sentiments from Latinobarometro, which contains a consistently-phrased question in 2010, 2015, and 2020 on whether the respondent believes *"Rich countries have a responsibility to accept immigrants from poor countries"* on a scale from 1 (definitely disagree) to 5 (definitely agree). To our knowledge this is the only nationally representative survey with repeated questions on migrant sentiment in our study countries before 2016.[9]

[7] Table A1 lists the top Venezuelan-receiving countries in Latin America, the number and population share of Venezuelan arrivals, as well as whether or not they have Gallup's MAI index. We exclude Costa Rica from this study because the geographic units used by Gallup are not comparable from 2016-2019.

[8] The World Values Survey includes a question on trust in migrants in 2012 and 2018, but the latter wave was conducted in early 2018 before most of the Venezuelan migration occurred. Another candidate is the Latin American Public Opinion Project (LAPOP), which includes questions about attitudes towards Venezuelan migrants, but only in a handful of countries and it does not have any questions that were repeated before and after the migration.

[9] Another possible data source to study pretrends is LAPOP, which asks if the respondent agrees that government should provide services such as healthcare and education to migrants in Ecuador (2008, 2012, and 2017) and Panama (2008 and 2014). We don't see evidence of pre-trends in this question, though this should not be over interpreted given the limited coverage and sample size. For the curious reader, the mean agreement for regions under the median 2015-2019 migration rate in Ecuador is .45, .43, and .58 in 2008, 2012, and 2017 respectively, while for regions over the median it is .47, .50, and .61. In Panama, the mean

Table I: Gallup Summary Statistics

	N	Mean	SD	Min	Max	Cronbach's Alpha
Migrant Acceptance Index	13,366	0.00	1.00	-1.81	1.01	0.84
Migration in country is good	13,366	0.48	0.50	0.00	1.00	
Migrant becoming neighbor is good	13,366	0.56	0.50	0.00	1.00	
Migrant marrying relative is good	13,366	0.50	0.50	0.00	1.00	
Male	13,366	0.41	0.49	0.00	1.00	
Age	13,366	44.55	18.96	15.00	99.00	
Completed Secondary	13,366	0.60	0.49	0.00	1.00	
Completed Tertiary	13,366	0.11	0.32	0.00	1.00	
Urban	13,366	0.60	0.49	0.00	1.00	
Internet Access	13,366	0.64	0.48	0.00	1.00	
Ln Per-Capita HH Income	13,366	8.53	1.91	0.00	13.68	
Unemployed	13,366	0.08	0.27	0.00	1.00	

The sample includes Gallup respondents in 2016 and 2019 who are over age 18 and have been in the country for at least five years. See the Data Appendix for a description of each variable.

In Table A2 we present the correlates of MAI in 2016 and 2019 with a set of individual characteristics. At baseline, average country-level MAI is highest in Argentina and Uruguay, lower in Colombia, Ecuador, and Peru, and lowest in Chile and Panama. We also see that MAI is higher for men, more educated people, and to a lesser extent younger people and in urban areas. In 2019, there are large decreases in MAI in Colombia, Ecuador, and Peru. However, the relationship between demographic characteristics and MAI remains relatively unchanged, indicating that this decrease is not driven by a particular demographic group.

Anti-Migrant Tweets: We also test for effects on more extreme migrant response to migration — tweets classified as anti-migrant. In each of the 7 countries of analysis excluding Panama, Tweets were geolocated to regions and restricted to those classified as containing a keyword about Venezuelans (the full list of keywords is described in Section C.B). We only include regions for which in 2015 and 2019 there were at least 50 geolocated tweets.

Venezuelan migrant share of the population by region: We use data from the national household labor surveys in each country to measure the Venezuelan-born population

agreement for regions under the median migration rate is .48 in 2008 and .59 in 2014, and these are .60 and .69 for those over the median. Thus, while average agreement that the government should provide services to migrants has increased over time in these countries, these changes are relatively parallel among places that received more and less migration between 2015-2019.

in each region in 2015 and 2019.[10] For each country, we compute the share of respondents who are Venezuelan in each survey in each region for each year.[11]

These labor force surveys are generally designed to include migrant populations who have intention to stay in the country, thus excluding migrants transiting through countries. However, there may be concerns around measurement error given that these surveys, while representative at the regional level, are typically not designed to be representative of migrant populations. Existing analysis indicates that household surveys in Latin America often capture migrants similarly well as national censuses (Rico, 2022). However, this is based on a specific subset of countries and surveys, and many countries in our sample have not had a census conducted since the Venezuelan migration. Alternatively, migration can be measured using administrative data from migration authorities, but these have the large disadvantage of excluding irregular migrants, and as mentioned less than half of the Venezuelan diaspora in Latin America was estimated to have some form of legal status in 2019 (Migration Policy Institute, 2020). Administrative data also tends to better measure border crossings than the within-country locations of migrants, and region-specific estimates typically rely on voluntary migrant registration with local authorities. To test the correlation between these estimates, we access region-level administrative data in 2019 for two countries that have such data – Colombia and Peru – and we show in Figure A2 that there is a strong correlation between the migrant shares measured in the labor force surveys and the administrative data (the correlation coefficients are .9 for Colombia and .75 for Peru). Ecuador is a country in our sample where there has been particular concern about the quality of Venezuelan migration measurements in the labor force survey. Olivieri et al. (2022) estimates Venezuelan migrant shares across regions of Ecuador using a sample based on mobile phone records, and these have a correlation coefficients with migrants shares in the labor force survey of only .4. Therefore, as a robustness check, we replace our migration estimates in Ecuador with those estimated in Olivieri et al. (2022).

Individual and regional covariates: Gallup includes information on respondent characteristics – including sex, age, urban status of the area, having internet access, household in-

[10]These are: Encuesta Permanente de Hogares (EPHC) in Argentina ; Encuesta Nacional de Caracterización Socio-económica (CASEN) in Chile; Gran Encuesta Integrada de Hogares (GEIH) in Colombia; Encuesta Nacional de Empleo, Desempleo y Subempleo (ENEMDU) in Ecuador; Encuesta Continua de Hogares de Propósitos Múltiples (ECHPM) in Panama; Encuesta Nacional de Hogares (ENAHO) in Peru; and Encuesta Continua de Hogares (ECH) in Uruguay. This household data was harmonized and graciously provided to us by the IADB.

[11]In Peru, only the total foreign-born population is observed, but changes in this variable will closely correlate with changes in the Venezuelan population as the Venezuelan migration was by far the largest migration into Peru over this period.

come, and unemployment status – which we use as controls and in the heterogeneity analysis. Gallup also aggregates some questions into indexes indicating the respondent's perception of "law and order" (Law and Order Index) and public good provision (Community Basics Index). Details on each question and index construction are in Appendix C.A. We also use the measures of nightlight intensity from AidData geoquery as a proxy for regional income (Gibson et al. , 2020).

III.B Empirical Strategy

III.B.1 OLS

In our initial specification, we assume that, conditional on various control variables, the change in migrant population across regions in Latin America in 2019 is an exogenous shock. We focus on the sample of Gallup respondents over age 18 who have been in the country for at least 5 years, thus excluding recent migrants themselves from the analysis. We employ the following specification for individual i in region r and year t, for $t \in 2015, 2019$[12]:

$$Y_{irt} = \beta M_{rt} + \delta_r + \pi_{ct} + X_{irt}\gamma + \epsilon_{irt} \tag{1}$$

where M_{rt} is the number of Venezuelan-born divided by the 2015 region population, δ_r are region fixed effects, π_{ct} are country-year fixed effects, and X_{irt} are individual covariates.

We can take a causal interpretation of β if M_{rt} is independent of ϵ_{irt}. That is, we assume that absent the changes in migration brought about by the Venezuelan exodus, attitudes toward migrants would have followed the same trajectory across regions. The first threat to identification is possible anticipation effects, in which attitudes toward migrants were changing in 2015 in response to expected changes in migration. However, this is unlikely in our setting given the sudden onset and severity of this crisis - while many predicted that a collapse in oil prices would lead to an economic recession in Venezuela, few could have anticipated the severity of the crisis and the magnitude of the migration that followed.[13] A second threat is the possible geographic sorting of migrant into locations according to *changes* in unobserved characteristics. Given that our specification includes region fixed effects, we

[12] As mentioned, the MAI is measured in 2016 instead of 2015. We consider this to be a pre-period measure – the Gallup survey was implemented between March-August of 2016 when the Venezuelan migration was still relatively small and recent. We still consider our "migration period" to be 2015-2019 and use these years when measuring migration, though our main results are robust to changing this to 2016-2019.

[13] This is not only evident in the total number of emigrants from Venezuela, but also in the change in composition of the destination of Venezuelan migrants. For example, before 2015, Colombia and the rest of Latin America only received 2.9% and 4.8% of Venezuelan emigrants, respectively. After the exodus, the share increased to 59% for Colombia and 30.7% for the rest of Latin America (Pirovino & Papyrakis, 2023).

are not worried about migrants choosing to settle in regions that have more positive baseline sentiment toward migrants or economic opportunities (which may be correlated with migrant sentiment). However, we may worry that migrants choose to locate in regions according to time-varying characteristics. Although this can never be tested directly, we can test for possible pre-trends. As discussed, this is not possible with Gallup data because attitudes toward migrants are only measured starting in 2015. For this test we therefore rely on the Latinobarometro question on responsibility to accept immigrants in 2010 and 2015. Responses to this question hold a correlation of 0.3 with MAI at the regional level and we consider it to be a proxy for migrant sentiment.

We provide evidence in support of the parallel trends assumption in two ways: by looking at raw means, and through a formal parametric test. We first present the mean of our proxy for attitude toward migrants in 2010, 2015, and 2019 across different discretized changes in the migrant population between 2015 and 2019 (our treatment). We discretize the changes in 2015-2019 change in Venezuelan shares as little or no increase, small increase, and large increase.[14] This visual test is a transparent way to evaluate possible differential pre-trends in our proxy outcome. Figure III shows that places that received differing 2015-2019 migration flows from Venezuela had very similar trends between 2010 and 2015.[15]

Next, to formally test for pre-trends, we regress our proxy for attitudes, $\tilde{Y}$, on the interaction between year indicators and the change in migration shares between 2015 and 2019 ($\Delta M_{r,2015-2019}$):

$$\tilde{Y}_{irt} = \delta_r + \tau_t + \sum_{t \in 2010,2020} \lambda_t 1(year = t) \times \Delta M_{r,2015-2019} + \tilde{\epsilon}_{irt} \tag{2}$$

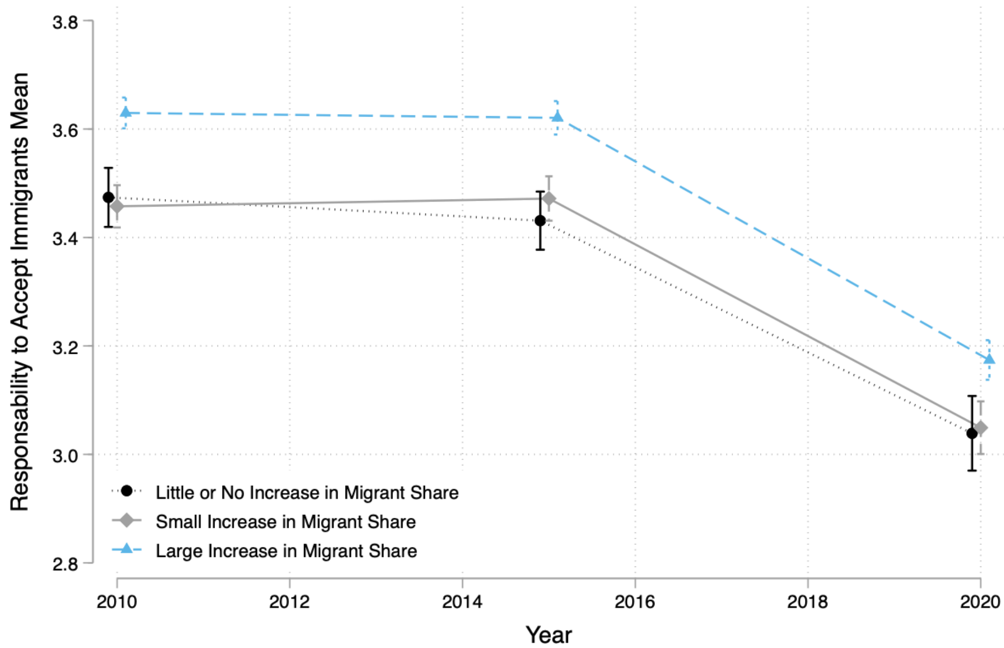
Here we omit the coefficient λ_{2015}, so the other coefficients are measured with respect to 2015. If there are no pre-trends in attitudes toward migrants across places that received more or less Venezuelan migrants after the 2016 exodus, then λ_{2010} should be indistinguishable from zero. This is what we find and present in Table II. The figure shows that places with a larger increase in the migrant share between 2015 and 2019 had little difference in attitudes towards migrants in 2010 relative to 2015.

Lastly, our model is equivalent to a differences-in-differences model where treatment is continuous instead of discrete. A recent literature discusses this class of models when there

[14] More precisely, we define the following groups: the change in migrant share is less than 0.05 pp.; the migrant share increases by more than 0.05 pp. but less than 0.69 pp. (the median across all regions); the migrant share increases by more than 0.69 pp.

[15] There is also little evidence of differential trends between 2015-2020, consistent with the result in the following section that migrant inflows had no causal effect on local-area migrant sentiment.

Figure III: Region-Level Mean of Anti-Migration Proxy by 2015-2019 Change in Venezuelan Migrant Share



This figure shows that, before 2015, the anti-immigrant proxy from Latinobarometro was evolving in similar ways across places that received more or fewer Venezuelan migrants between 2015 and 2019.

Table II: Latinobarometro Pre-Trends Correlated with 2015-2019 Migration

	Responsibility to Accept Immigrants		
	(Mean = 3.40, SD = 1.09)		
$\Delta M_{rc,2015-2019}$ X Year = 2010	-0.02	-0.04	-0.04
	(0.02)	(0.03)	(0.03)
$\Delta M_{rc,2015-2019}$ X Year = 2015	(omitted)	(omitted)	(omitted)
$\Delta M_{rc,2015-2019}$ X Year = 2020	-0.03	-0.01	-0.01
	(0.02)	(0.03)	(0.03)
N Obs	23,625	23,625	23,625
N Clusters	106	106	106
Region FE and Year FE	X	X	X
Country-Year FE		X	X
Individual Controls			X

Outcomes are measured in 2010, 2015, and 2020 in Latinobarometro across all 7 countries of analysis. Standard errors clustered at the region level. * p<0.10, ** p<0.05, *** p<0.01.

are heterogeneous treatment effects (de Chaisemartin & D'Haultfœuille, 2018, de Chaisemartin et al., 2019, 2022). In this setting, we further need to assume that treatment effects are uncorrelated with the size of treatment. That is, the causal effects of migration on migrant sentiment should not systematically differ in geographical areas with low migration if these areas are to serve as good counterfactuals for places with higher migration. We expand this discussion, and show alternative estimators for our effects, in Appendix D. We show that adopting a more flexible framework does not alter the fundamental conclusions of the empirical exercise presented below.

III.B.2 IV

Though we do not see evidence for pre-trends associated with $\Delta M_{rc,2015-2019}$, it remains possible that migrants sort into locations according to unobserved characteristics that are changing after 2015. Therefore, as an additional specification, we instrument M_{rt} with the Venezuelan-born share of the region in historical censuses.[16] Our identification strategy relies on the fact that migrants tend to move to places where migrant networks are larger at baseline (Card, 2001, Altonji & Card, 1991). This instrument has been used extensively in the immigration literature, including in the context of Venezuelan migration in Latin America (for example, see Groeger et al. (2022), Delgado-Prieto (2021), Rozo & Vargas (2021)).

$$Y_{irt} = \beta M_{rt} + \delta_r + \pi_{ct} + X_{irt}\gamma + \epsilon_{irt} \tag{3}$$

$$M_{rt} = \phi HistoricalShare_r \times 1[t = 2019] + \eta_r + \zeta_{ct} + X_{irt}\rho + e_{irt} \tag{4}$$

In this specification, the exogeneity assumption is that $HistoricalShare_r$, as opposed to M_{rt}, is uncorrelated with *changes* in potential outcomes. That is to say, we allow for historical migration to be correlated with the level of migrant sentiment in a region, but assume there is no correlation between historical migration and counterfactual *changes* in attitudes toward migrants between 2016 and 2019. The validity of the enclave instrument in this specific setting is strengthened by the fact that the historical number of Venezuelans in Latin America was relatively small compared to the current exodus, and there was relatively little migration out of Venezuela before 2015, mitigating the concern discussed in Jaeger et al. (2018) regarding serial correlation in economic outcomes.

[16]Specifically, we use the census 10% subsample from 2001 in Argentina, 2002 in Chile, 2005 in Colombia, 2010 in Ecuador, 2007 in Peru, 2000 in Panama, and 2011 in Uruguay. We use more recent censuses in Ecuador and Uruguay because previous versions do not measure the country of origin for migrants. All census data was accessed from IPUMS International (IPUMS, 2020).

Once again, we conduct the same tests for pre-trends presented above, in this case for $HistoricalShare_r$ rather than $\Delta M_{rc,2015-2019}$. Figure IV shows the unconditional means in attitudes across regions at the bottom, middle, and top third of the historical share of Venezuelans. We see that, although the instrument is correlated with the levels of attitudes toward migrants, it does not appear to be correlated with the change in attitudes between 2010 and 2015. The formal test for pre-trends is presented in Table III. This shows that, reassuringly, there are no differential trends in attitudes toward migrants between 2010 and 2015 according to the historical migration share.[17]

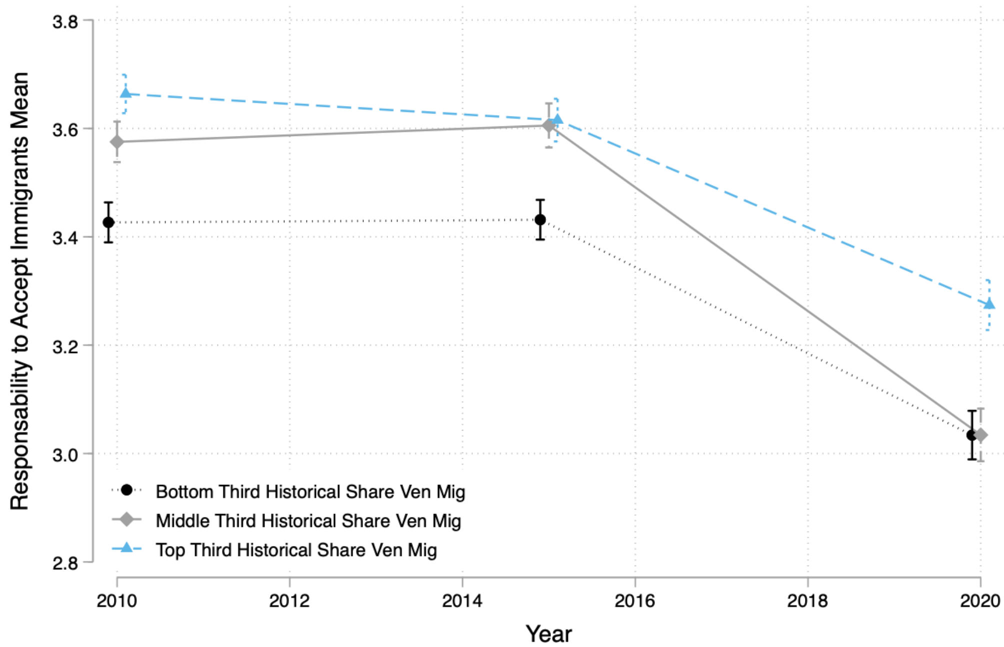
We further check if our instrument is correlated with other observable regional characteristics. We first test if historical Venezuelan shares are correlated with observable baseline characteristics at the regional level. These correlations are presented in Figure A3. Controlling for country fixed effects, our instrument is not correlated with the majority of observed individual characteristics, including secondary and tertiary education completion rates, internet access, mean per-capita household income, unemployment, public goods quality, security, and *baseline MAI*. The correlation on nightlight intensity is statistically significant at the 5% level: a 1 SD increase in the historical census Venezuelan share is correlated with a .12SD increase in baseline nightlight intensity, and similarly a .12SD increase in urbanization (significant only at the 10% level). To account for the possibility that these small correlations are affecting our instrument, we estimate a robustness specification in which these pre-period region-level controls are interacted with year fixed effects. Finally, to test for possible pre-trends that differ by value of our instrument, we construct an event study like in equation 2, for four different characteristics that we can observe throughout our sample period: urbanization, income, unemployment, and public goods provision.[18] We present these results in Figure A4. Reassuringly, we find no evidence that regions with a historically higher Venezuelan population have any different trends in these outcomes leading up to the start of the migration - thus, giving us more confidence in our identification assumption.

In terms of the relevance condition, we find that our instrument is highly predictive of migration to our regions of analysis during the Venezuelan exodus, consistent with qualitative evidence that migrant networks played an important role in where Venezuelans chose to locate (Namen et al. , 2021). The first-stage coefficient displayed at the bottom of Table V shows that a .1 pp. increase in the historical Venezuelan share in the region is associated with a 3-4 pp. increase in the 2015-2019 migrant growth rate. The large magnitude of the first-stage coefficient is driven by the fact that the historical migrant shares were very small,

[17]The number of clusters is slightly smaller than in our main analysis since regions not observed in 2010 are dropped.

[18]All these variables are measured by Gallup.

Figure IV: Instrument Pre-Trends



This figure shows that the anti-immigrant proxy from Latinobarometro was evolving in similar ways across places with different values of the historical share of Venezuelan migrants, which is used as an instrument in the analysis for the 2015-2019 change in migration.

Table III: Latinobarometro Pre-Trends Correlated with Instrument

	Responsibility to Accept Immigrants		
		(Mean = 3.40, SD = 1.09)	
Migrant Share in Census X Year = 2010	0.04	0.01	0.01
	(0.04)	(0.05)	(0.05)
Migrant Share in Census X Year = 2015	(omitted)	(omitted)	(omitted)
Migrant Share in Census X Year = 2020	0.01	0.01	0.01
	(0.04)	(0.04)	(0.04)
N Obs	23,625	23,625	23,625
N Clusters	106	106	106
Region FE and Year FE	X	X	X
Country-Year FE		X	X
Individual Controls			X

Outcomes are measured in 2010, 2015, and 2020 in Latinobarometro across all 7 countries of analysis. Venezuelan migration rates as measured in historical censuses (the instrument used in analysis) is standardized to a mean of 0 and standard deviation of 1. Standard errors clustered at the region level. * p<0.10, ** p<0.05, *** p<0.01.

never exceeding greater than 1% of the region's population, while the recent Venezuelan migration was very large. The instrument has a strong first stage with a Kleibergen-Paap first stage F-statistics between 33 and 47.

III.C Results

III.C.1 OLS Results

Migrant Acceptance Index: We start by presenting the results from the Migrant Acceptance Index (MAI) in Table IV. Column 1 shows the simple OLS specification controlling for only region and year-fixed effects. It shows that a one percentage point (pp.) increase in the migrant share is associated with an insignificant .01 SD improvement in the MAI between 2016-2019. In Column 2 we add country-year fixed effects to control for country-level changes in attitudes. The results remain unchanged, although the null effect is more precisely estimated. In Column 3 (our preferred specification) we include controls for individual-level characteristics and the results are again unchanged, with a point estimate of .02 and a 95% confidence interval ranging from -.02 to .05. The migration rate varies from 0% to 5.2% across the 10th-90th percentile of the sample, which is associated with a .1 SD worsening of the MAI at the lower bound of the confidence interval. This is less than one-twentieth of the variation in MAI across this range of the sample (-1.8 to 1), indicating that our confidence interval rules out any meaningful negative effect of migration on migrant sentiment, and leaves room for only a small positive effect.

It is useful to benchmark these estimates against existing estimates in the literature. The treatment most comparable to ours is most likely Hangartner et al. (2019), which finds that doubling the refugee flow to Greek islands decreases the migrant sentiment index among natives by .4 SDs. Given that the mean migration rate in their sample is 0.4%, this translates to a 1SD decrease from a 1 pp. increase in the migrant share – a large effect that is well outside our lower bound treatment effect of -.02 SDs. Thus, in the case of Venezuelans in Latin America, we can rule out negative treatment effects of this magnitude.

The equivalent specifications for each component of the index are presented in Appendix Table A3, which shows that the null effects of immigration on attitudes toward migrants are observed in each component question as well, and the effect is significantly positive for the second component on attitudes towards having migrant neighbors.[19] In Columns 6-

[19] In Table A5, we also test for non-linear effects — when the migrant share in regression 1 is split into deciles, there is evidence for a slight negative effect of migration on migrant sentiment around the 4th decile, but this returns to zero by the 6th decile. This small non-linear effect is eliminated once we move to our instrumental variable strategy.

8 of Table IV we present an equivalent specification using the Latinobarometro question about responsibility to accept migrants as the outcome, which we consider to be a less direct measure of migrant sentiment. Once more, we fail to find any evidence that the increment in the Venezuelan migrant population in the region negatively affects attitudes toward migrants.[20]

In Table A4, we conduct various robustness tests. To ensure that no individual country is driving our results, we drop each country one by one and show that there is no change in the point estimates. A related concern might be that the results are driven by regions with large migrant share increases, such as those along the Venezuelan border in Colombia. The second row of the first column in Table A4 shows that this is not the case — the point estimate is still zero after dropping the five regions with a Venezuelan migrant share larger than 6% (the importance of this check is discussed further in Section IV specific to Colombia). In the third row we use an alternative measure of the migrant share in Ecuador, where Olivieri et al. (2022) finds that the migration rate is not well measured by the labor force survey because it correlates poorly with estimates of regional Venezuelan shares according to a 2019 sample based on mobile phone records (see Figure A2). We replace our migration measure with these alternate estimates and the results are unaffected. In row four, we interact a list of pre-period regional characteristics with the year, and in row 5 we use the sampling weights provided by Gallup. In both cases the results remains unchanged.

Finally, in A7, we test whether the average null effect is masking positive and negative effects across different types of individuals and regions. We study heterogeneity along several individual and regional characteristics by interacting these characteristics with the migrant share. We find no evidence that effects vary according to economic characteristics at the individual or community levels — such as being unemployed, being in a lower-income household (indicative of facing more direct economic competition with Venezuelans who are mostly working in lower-skill occupations), or regions with lower nightlight intensity. We also find no heterogeneity for those living in urban areas, having access to the internet (which is a proxy for exposure to social media and online news sources), baseline MAI, or mean baseline values of Gallup indices that measure security and quality of public goods. However, when we move to the instrumental variable analysis, most of these tests are under-powered. In Section IV we present a heterogeneity analysis at a much finer level of geography in Colombia.

[20]The sample of regions included in the Latinobarometro analysis is slightly different from that of the MAI because of differences in which regions were observed in both 2015 and 2020.

Table IV: Regression of Attitudes Toward Migrants on Migrant Share - OLS

	Migrant Acceptance Index (MAI)			**Anti-Migrant Tweet Share**		**Latinobarometro**		
	(Mean = 0.00, SD = 1.00)			**(Mean = 34.57, SD = 12.45)**		**(Mean = 3.33, SD = 1.14)**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Migrant Share	0.01	0.01	0.02	-1.69**	-1.00	-0.03	-0.01	-0.01
	(0.05)	(0.02)	(0.02)	(0.83)	(0.66)	(0.02)	(0.03)	(0.03)
N Obs	13,366	13,366	13,366	86	86	16,109	16,109	16,109
N Clusters	116	116	116	43	43	121	121	121
Region FE and Year FE	X	X	X	X	X	X	X	X
Country-Year FE		X	X		X		X	X
Individual Controls			X					X

Estimates for Equation 1. **Migrant Acceptance Index (MAI):** outcomes are measured in Gallup 2016 and 2019. The sample is restricted to respondents who have been in the country for at least 5 years. Individual controls include gender, age FE (binned into 5-year intervals), education FE (grouped into not completed secondary, completed secondary, and completed post-secondary), and urban-rural status. Effects for each individual question in the MAI index are presented in Table A3; **Anti-Migrant Tweet Share:** outcome is the share of tweets about Venezuelans classified as "anti-migrant" that are geo-coded to each region. Regressions in Column 4 and 5 are at the region-level and weighted by the total number of tweets about Venezuelans. The sample includes only regions in which there are at least 50 tweets; **Latinobarometro:** outcome is proxy for attitudes toward migrants measured in 2015 and 2020. The question asked if individuals think that there is a responsability for rich countries to accept migrants from poor countries, measured on a scale of 1 (strongly disagree) to 5 (strongly agree). Standard errors clustered at the region level. * p<0.10, ** p<0.05, *** p<0.01.

Anti-Migrant Tweets: Above, we found that the average response toward migrants did not worsen in response to local in-migration. Here we test for effects on more extreme migrant response to migration — tweets classified as anti-migrant. Like with the MAI, Figure A1 shows that countries with more Venezuelan migration experienced an increase in the share of tweets classified as anti-migrant. However, we again find no evidence that these national trends are observed at the region level. Columns 4 and 5 of IV show the results of an equivalent specification as in Equation 1, but aggregated at the region level and weighted by the number of tweets. Because analysis is restricted to regions with at least 50 geolocated Tweets about Venezuelans, we lose coverage in this analysis compared to the Gallup analysis (43 regions versus 116). However, consistent with those results, the estimates in Column 4 show a decrease of 1.69 pp. (0.14 SDs) in the share of anti-immigrant tweets following a 1 pp. increase in the migrant share. This indicates that local migration flows might induce a *reduction* in the share of anti-migrant tweets targeting Venezuelans. Column 5 further controls for country-year specific changes, which reduces the point estimate by almost half, and it is no longer statistically from zero.[21]

In Table A6 we conduct similar robustness tests from the previous section. When we drop the regions with a high migrant share of over 6%, the effects remain negative, but it is weakened by around 75% to -.26 and becomes more imprecise. A similar effect is achieved when we drop Colombia. Next, the main regression included only regions with at least 50 tweets – if we extend the sample to include regions with at least 10 tweets, the effect again becomes smaller. For the other checks, including using the alternative migration rates in Ecuador and dropping all other countries one by one, the results do not change. Finally, when we control for pre-period region characteristics interacted with year, the effect increases to -1.47 (.12 SDs) and becomes statistically significant, consistent with migration reducing the share of tweets about migration in the region that are negative.

III.C.2 IV Results

Migrant Acceptance Index: Columns 1 to 3 in Table V present the estimates of Equation 3. The results are similar to those from the OLS estimation presented in Table IV. In our base specification, we find an effect of increasing the migration share by 1% that is close to zero (-0.01) and insignificant. The coefficient is similar in Column 2 (0.01) once we include country-by-year fixed effects and in Column 3 (0.02) when we add individual controls. In conclusion, we again reject any meaningful negative effects of migration on migrant accep-

[21] In Table A5 we also show that larger migrant inflows increase the number of geocoded tweets about the Venezuelan population, though this estimate is imprecise. Thus, the more Venezuelans in the area, the more people reference Venezuelans in social media.

tance. In Columns 6 and 7 we show that we find similar estimates for our anti-migration sentiment proxy from Latinobarometro — the point estimate is 0.01 in both cases; less than 1% of a SD.

In Table A4, we present the same robustness tests and draw identical conclusions. When we interact each of the baseline regional characteristics with year fixed effects, the point estimate increases ever further to 0.06 and becomes statistically significant at the 5% level, indicating that, if anything, there is a positive effect of local migration on migrant sentiment. Finally, in Table A7 we repeat the same heterogeneity analysis as in the initial specification and find similar results. Some of these tests are underpowered — the first-stage F-statistic falls below 15 for various tests. In the following section, we discuss in more detail the more highly-powered heterogeneity analysis that we conduct across finer levels of geography in Colombia.

Anti-Migrant Tweets: Our OLS analysis estimated a decrease of 1.69 pp. (0.14 SDs) following a 1 pp. increase in the migrant share. Once we instrument for the migrant share, the coefficient increases in magnitude to -2.1 pp. (0.17 SDs) and becomes more precisely estimated, such that it is now statistically different from 0 at the 1% level. If we further control for country-year fixed effects, the estimated effect is reduced by about half, to -1.14 (0.09 SDs), but it remains statistically different from 0 at the 5% level. In Table A6 we conduct our robustness tests from the previous section and the conclusions are again identical. In no test is there evidence that increased migration increases the share of anti-migrant tweets in the region.

Table V: Regression of Attitudes Toward Migrants on Migrant Share - 2SLS

	Migrant Acceptance Index (MAI) (Mean = 0.00, SD = 1.00)			**Anti-Migrant Tweet Share (Mean = 34.57, SD = 12.45)**		**Latinobarometro (Mean = 3.33, SD = 1.14)**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Migrant Share	-0.01	0.01	0.02	-2.10***	-1.14**	0.01	0.01	0.01
	(0.05)	(0.02)	(0.02)	(0.38)	(0.44)	(0.03)	(0.03)	(0.03)
N Obs	13,366	13,366	13,366	86	86	16,109	16,109	16,109
N Clusters	116	116	116	43	43	121	121	121
Region FE and Year FE	X	X	X	X	X	X	X	X
Country-Year FE		X	X		X		X	X
Individual Controls			X					X
First-Stage Coef.	39.95***	33.63***	33.60***	46.83***	40.34***	39.72***	35.15***	35.15***
	(6.65)	(5.16)	(5.15)	(7.04)	(8.20)	(5.97)	(4.28)	(4.28)
Kleibergen-Paap F	36.11	42.49	42.64	44.28	24.18	44.21	67.54	67.50

Estimates for Equation 3. The instrument is the Venezuelan share of the population in the historical census. See notes to Table IV. Standard errors clustered at the region level. * p<0.10, ** p<0.05, *** p<0.01.

III.D Takeaways

We do not find any evidence that being exposed to a higher migrant share at the regional level worsens migrant acceptance. Both our OLS and 2SLS analysis fail to find evidence that more local migration worsens attitudes toward migrants. If anything, after controlling for regional characteristics interacted with time trends, we find a positive effect on Gallup's MAI. This result indicates that the worsening migrant sentiment at the national level in many countries is not driven by the locations within-country that are hosting the most migrants.

A possible limitation of this analysis is that the geographic regions are broad. If migrants and natives are segregated into different sub-divisions within the department or province, then region-level migration would not coincide with migrant-native contact or threat from economic competition, public goods scarcity, or crime. We now address this issue by zooming into a finer geographic area in Colombia — taking advantage of unique and large-scale data on both migration rates and migrant sentiments across municipalities.

IV Effects of Immigration on Trust and Migrant Sentiment in Colombia

We now focus on Venezuelan migration into Colombia, the country which received the most Venezuelan migration, and where we can observe migration shares and migrant sentiment at the municipality level — a much smaller geographical area than the region.[22]

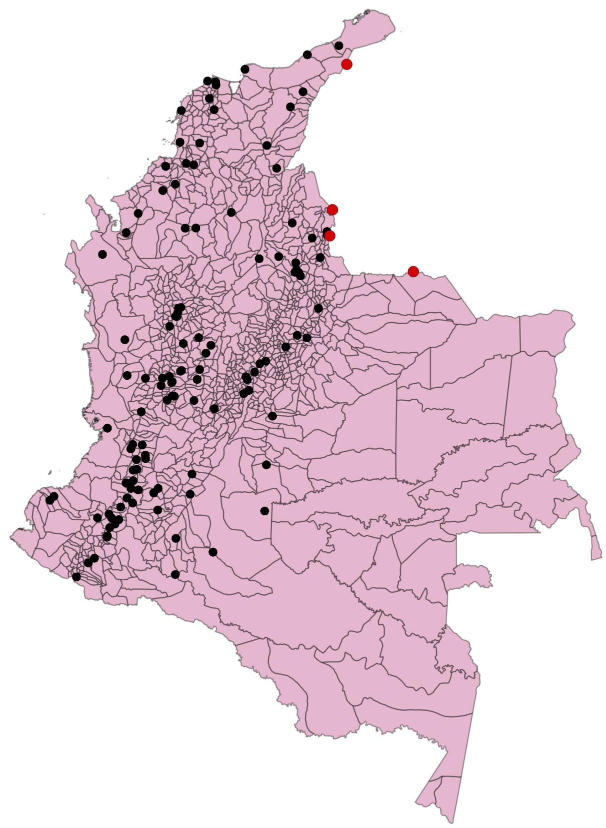
IV.A Data

Attitudes towards migrants: Data on attitudes towards migrants come from the Survey of Political Culture (ECP) collected by the Colombian National Administrative Department of Statistics (DANE). The ECP is a nationally-representative survey with the objective of characterizing key aspects of Colombian political culture, perceptions, and participation. We use data from the wave of July and August of 2019, generating a total of 41,850 people over age 18 in our sample, in 15,392 urban households and 3,974 rural households across 118 municipalities. These municipalities are presented in Figure V.

For the 2019 version of the ECP, working in partnership with DANE, we included a series of questions related to trust and attitudes toward foreigners. The survey asks the

[22] Colombia is divided into 32 regions (departments) and 1,122 municipalities.

Figure V: Municipalities in Colombia



Black dots represent the capitals of the 118 ECP municipalities, red dots represent border crossings.

respondent to rate their trust in foreigners on a scale of 1 to 5. This follows the wording of the World Values Survey (WVS), which was fielded in 2012 but is only available at a higher geographic level (department).[23] The survey also asks "who would you not like to have as a neighbor" and lists people of another nationality as one of the options. We refer to this distaste for foreign neighbors as "preferences for segregation". As we did in the previous analysis, we combine these two measures into a Migrant Acceptance Index (MAI-ECP) by taking the first principle component.[24] Table VI displays summary statistics for the MAI-

[23]Recent work using the Global Preference Survey (GPS) has suggested that other possible wordings of the trust question can predict behavior in the lab with precision (Falk et al. , 2018). In a second phone survey in the summer of 2020, we included the question suggested by (Falk et al. , 2018) alongside the initial question from the WVS to verify their correlation. We find that they are highly correlated — for individuals who respond that they do not "assume that foreigners have the best intentions", trust in foreigners on the 1 to 5 scale is heavily concentrated at 1 (mean value 1.27), whereas for those who responded 'yes' the values are distributed in an almost normal fashion (mean 2.42). We take this as evidence that the WVS trust question is also a good proxy for trusting behavior in this context.

[24]These questions refer to foreigners in general, and not specifically Venezuelans. Because almost 95% of foreigners in Colombia in 2019 were Venezuelan, we believe that respondents have Venezuelans in mind when responding. We can also confirm using the 2005 census that there are no large non-Venezuelan migrant

Table VI: ECP Summary Statistics

	N	Mean	SD	Min	Max
Migrant Acceptance Index (ECP)	41,850	0.0	1.0	-3.1	2.4
Trust in Foreigners	41,850	1.7	1.0	1.0	5.0
Okay with Immigrant Neighbor	41,850	93.4	24.7	0.0	100.0
Male	41,850	53.6	49.9	0.0	100.0
Age	41,850	44.6	17.6	18.0	104.0
Completed Secondary	41,850	55.7	49.7	0.0	100.0
Completed Tertiary	41,850	27.7	44.7	0.0	100.0
Urban	41,850	80.8	39.4	0.0	100.0
News from Internet/Social Media	41,850	32.5	46.8	0.0	100.0
Unemployed	31,103	11.2	31.5	0.0	100.0
Ln Income	31,103	12.3	4.5	0.0	17.7
Network Degree	41,850	4.0	4.1	0.0	20.0
Someone in Network is Foreign	28,531	3.5	18.5	0.0	100.0
Someone in Network Lost Econ Opp to Foreigner	28,531	25.8	43.7	0.0	100.0
Perceived Migrant Share	41,850	38.3	28.6	0.0	100.0

The sample includes 2019 ECP respondents over age 18 who have been in Colombia for at least 5 years. See the Data Appendix for a description of each variable.

ECP and its components. On average, trust in foreigners is 1.7 out of 5, and 93% of people are okay with having a foreigner as a neighbor. Table B1 shows that, as with the Gallup MAI, these sentiments are on average higher for more educated people and in urban areas.

Share of Venezuelan migrants per municipality: To measure migration at the municipality level, we calculate the population share of Venezuelan migrants using the 2018 full population census. We define a migrant as anyone who was born in a foreign country and was not living in Colombia five years prior, and we divide this by the 2013 municipality population to measure the migrant share of the municipality. One limitation of the census is that it does not distinguish the country of origin. However, according to the Colombian household labor force survey (GEIH), 95% of all migrants who arrived in the five years proceeding 2018 were Venezuelan, giving us confidence that our Venezuelan migrant shares are close to the shares of all foreigners.[25]

concentrations in any of the municipalities included in the ECP - the maximum non-Venezuelan migrant share is 0.6% of the municipality population. While country of origin is not available in the 2018 census, there was little change in the nationwide non-Venezuelan migrant share between 2005-2018.

[25]The census is the only data source that can accurately measure migration at the municipality level across all of Colombia. As discussed in the previous section, another potential option is the official estimate of the Colombian migration authorities, which is based on voluntary registrations with local officials and misses undocumented migrants. These two measures are highly correlated at the municipality level ($\rho = .8$).

Social networks and perceptions: We also included a series of questions in the 2019 ECP about strong social ties in the respondent's social network (Granovetter, 1973). Following the method proposed by (Breza et al., 2020), the total size of the network is constructed by asking respondents to think about the number of individuals whom they **frequently visit or have been visited by in their home**, or that **helped them or who they helped to get a job**. The respondent is then asked how many people in this network are foreigners, as well as how many people in this network the respondent feels have lost economic opportunities to migrants. These outcomes will be used to test for the effect of local migration on migrant contact and perceived economic competition. It is important to note that 32% of individuals report a degree size of zero, reflecting the fact that this is a measure of very close social ties. Table VI shows that, including zeros, the average degree size is 4. Among those with a positive network, 3.5% report having a foreigner in their network, and 26% believe that someone in their network lost economic opportunity because of foreigners. Finally, we include a question to test for effects on the perceived migrant share of the population. We asked respondents to indicate how many of 10 random individuals in the street they think are foreigners. The average response was 3.8 or 38% of the population, vastly larger than the true foreigner share of just over 4% in 2019, and consistent with evidence from other countries that individuals tend to overestimate migration (Alesina et al., 2018).

IV.B Empirical Strategy

For all ECP respondents over age 18 who have been in the country for at least 5 years, we run the following regression for individual i in municipality m:

$$Y_{im} = \beta M_m + X_i\gamma + Z_m\rho + \delta_r + \epsilon_{im} \tag{5}$$

where M_m is the share of the population that was living in Venezuela 5 years ago (in 2013). This varies extensively across the country, between 0%-2.5% for 90% of municipalities in our sample and up to 8% in municipalities geographically closer to the Venezuelan border. We include individual-level controls (X_i) for age (including separate indicators for age groups in 4-year intervals), gender, level of completed education, and urban-rural status. Results are presented with and without these controls, as well as a long list of municipality-level controls (Z_m) measured before the start of the migration. These include the log-2013 population, 2015 per-capita value added (DANE), the 2012 department-level average trust in foreigners measured by the WVS, urbanization rate, crime rate, and a public goods index based on secondary school test scores, infant mortality rate, electricity coverage, trash collection, and sewage coverage.[26] Because the outcome is measured in 2019 and we do not observe it in the pre-period, we are unable to include municipality fixed effects. To our awareness, there are no existing pre-period measures of attitudes towards migrants at such a fine geographic level. Thus, this rich set of pre-period municipality controls plays an important role in controlling for potentially endogenous municipality-level characteristics.[27]

Once more, we can take a causal interpretation of β if M_m is independent of ϵ_{im}. As emphasized in the introduction, this is a refugee wave driven by push factors rather than by pull factors, and there were no economic changes attracting migrants to Colombia over this period, reflected by the fact that there was no notable change in migration from any other country to Colombia.[28] To account for the potentially endogenous sorting of migrants across Colombian municipalities, we use two instruments for the migrant share M_m. The first is similar to the one we used in Section III.B, that is, the Venezuelan migrant share of the municipality as measured in the 2005 census, several years before the current migration. As before, this is appropriate because these historical Venezuelan populations were very small and are largely uncorrelated with pre-period characteristics (as we will show). The second instrument is the log of the minimum driving distance from the municipality capital to the closest of the four crossing points along the Colombian-Venezuelan border (depicted in Figure V). This leverages the fact that many Venezuelans have entered Colombia traveling on foot or via public transportation, and as a result many have ended up settling in areas closer to the Venezuelan border.[29] More than 99% of the Venezuelans who legally crossed the Colombian border in 2018 entered through one of these checkpoints, and illegal entries mostly occurred along paths that simply pass around legal entry points (Migración Colombia, 2019). Both instruments are highly predictive of recent Venezuelan migration. Another advantage of having two instruments is that it allows an overidentifying restrictions test of instrument validity, and in all specifications we fail to reject that the instruments are exogenous.

In order for these instruments to satisfy the exclusion restriction, they must only affect

[26] Whenever possible, we take the average across 2014-2016 to minimize measurement error. In a few cases, missing values are imputed using non-missing variables. See Data Appendix for a description of sources.

[27] Each of these specific controls was included because we believe they might be correlated with pre-period migrant sentiments. The most important is pre-period trust in migrants in the WVS at the department level. For the same reasons discussed throughout this paper, urbanization, crime, wealth, and public goods availability may directly affect historical attitudes toward migrants.

[28] Unfortunately, we do not see the region of origin for Venezuelan migrants within Venezuela, so we are unable to characterize migrants according to their origin.

[29] Driving distance is calculated using Open Street Maps algorithms in QGIS3.8. For 3 municipalities, the capital was isolated such that no driving distances could be calculated. In this case, the nearest city within driving distance was used, and the results are robust to dropping these municipalities. Results do not depend on whether we use the minimum driving distance to the closest entry point or the average driving distance to all four points. They also do not depend on whether we use driving distance or driving time.

trust through the channel of increased immigration from Venezuela post-2015. As mentioned, there is no available data to test for pre-trends in the outcome variable at the municipality level. We instead check for balance along a list of pre-period socioeconomic characteristics. Figure B1 shows that a 1 SD increase in the Venezuelan share in the 2005 census is not significantly related to baseline population, value added per capita, urbanization, crime, or public goods. Most importantly, the historical migrant share is not correlated with 2012 trust in foreigners measured at the department level, indicating that these small historical migrant populations did not directly affect average migrant sentiment. The largest coefficient in magnitude is urbanization – a 1 SD increase in the 2005 Venezuelan share is associated with a .35 SD increase in urbanization with a p-value of .19. Regarding the distance instrument, B2 shows that proximity to the border is associated with higher urbanization rates (significant at the 10% level) and lower crime rates (significant at the 5% level). It is also not correlated with 2012 trust in foreigners, which has a coefficient of less than .05 SD, indicating the regions closer to the border are not necessarily more trusting of migrants to begin with. We show all results with and without controlling for these baseline municipality characteristics.

As in the previous section, we implement various robustness tests that build confidence in our model and instruments. Most importantly, we exclude the 12/117 municipalities with migrant shares greater than 2.5%, which are also all the municipalities within 50km of the Venezuelan border. This is important because these municipalities may have been affected in other ways during the Venezuelan crisis, for example, through changes in cross-border commuting and trade or human trafficking along the border. They have also historically had more cross-border relations and contact with Venezuelans, potentially inducing a correlation between our instrument and pre-period attitudes towards migrants. Thus, it is encouraging that when we drop these municipalities the effect does not substantially change and in fact becomes more positive. We discuss our additional tests in the following section, which include dropping Bogotá, applying the ECP sampling weights, including Colombian-born return migrants in the migrant share, and including department fixed effects. We also control for migrant shares in neighboring municipalities to account for possible spillover effects.

IV.C Results

Table VII displays the main results for MAI - ECP.[30] In an OLS regression that controls only for the log population, there is a significant and positive correlation between the migrant population and trust towards foreigners (column 1). It indicates that a 1 pp. increase in the migrant share of the population is associated with a statistically significant .06 SD increase

[30] Table B2 in the Appendix presents the results for each individual question in the Index.

Table VII: Regression of ECP Migrant Sentiment Index on Municipality Migrant Share in Colombia

	Migrant Acceptance Index - ECP (Mean = 0.00, SD = 1.00)			
Migrant Share	0.06*** (0.02)	0.05** (0.02)	0.04** (0.02)	0.04* (0.02)
N Obs	41,850	41,850	41,850	41,850
Kleibergen-Paap F		243.58	248.40	135.66
Sargen-Hansen Chi-Sq P		0.245	0.196	0.718
2SLS		X	X	X
Individual Controls			X	X
Municipality Controls				X

Migrant Acceptance Index from ECP is a standardized index of trust in foreigners and being OK with having a foreigner neighbor. Results for each individual outcome is presented in Table B2. MAI is regressed on the migrant share of the population in the municipality (117 units). Individual controls include gender, age FE (binned into 5-year intervals), education FE (grouped into not completed secondary, completed secondary, and completed post-secondary), and urban-rural status. Municipality controls include (all measured in the pre-period) log per capita value added, urbanization rate, crime rate, a public goods index, and department-level trust in foreigners measured in the 2012 WVS. All columns control for pre-period log municipality population. See Data Appendix for description and source of each variable. Standard errors are clustered at the municipality level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

in the MAI-ECP. This indicates that, in line with our results across regions in Latin America, we do not find evidence of negative effects of migration flows on attitudes toward migrants.

The 2SLS model is applied in columns 2 through 4 of Table VII. The coefficient reduces slightly but remains statistically significant. Column 4 shows that, once we include individual and municipality controls, the estimated effect falls to 0.04 SD with a p-value close to 5%. An increase in the Venezuelan share across the 10th-90th percentile of municipalities, which ranges from 0.04% to 2.72%, is thus associated with a .1 SD increase in the MAI-ECP, which for context is about 70% of the raw difference in MAI-ECP reported in urban versus rural areas. Thus, we fail to find evidence of negative effects of migration on attitudes toward migrants. In our most demanding specification (Column 4) the 95% confidence interval ranges from -0.01 to 0.09 SD. That is, at the lower bound of this confidence interval, an increase in the Venezuelan share across the 10th-90th percentile of municipalities causes a decrease of 0.02 SD in the MAI-ECP. This is more than 10 times smaller than the national change in trust in foreigners between the 2012 WVS and the 2019 ECP, which decreased by around .3 SDs.[31]

In Table B3, we implement the robustness tests discussed in Section IV.B. When we drop the 12 municipalities with migrant shares greater than 2.5%, which includes all municipalities within 50km of the Venezuelan border, the coefficient increases to 0.13 and becomes less precisely estimated. This ensures that results are not driven by other changes occurring along the Colombian-Venezuelan border during this period and that historical exposure to Venezuelans along the border is not inducing a positive correlation between the distance instrument and pre-period migrant sentiment. Next, it is important to note that almost 20% of the Venezuelans who entered Colombia between 2015 and 2019 were born in Colombia. We choose to exclude them from our primary analysis because we are specifically interested in the social reaction to incoming foreigners rather than returning natives. However, excluding them could create omitted variable bias, since their settlement within Colombia is closely correlated with that of Venezuelan-born migrants (Lebow, 2022b). Row 3 shows that results are similar when we include Colombian-born return migrants in the migration measure. We then show that results are robust to dropping the capital city, Bogotá, which makes up around 12% of the sample. We also show that the choice of whether or not to use ECP sampling weights has little effect on the results. Next, one might worry that the instruments are correlated with unobservable characteristics affecting attitudes toward migrants across broad regions of Colombia. These could include, for example, department-level differences in economic growth, unemployment, or trade with Venezuela.[32] In Row 6, we include fixed effects for the departments in Colombia. While this specification is more demanding, our instrument remains sufficiently powered, and the coefficient increases to 0.11 SDs with a 95% confidence interval between 0.06 and 0.17. Thus, once again, our robustness tests indicate that if anything there is a positive effect of local migration on migrant acceptance.

Finally, given this analysis is conducted at a relatively small geographic level, there is a concern that spillover effects of migration in neighboring municipalities may create a downward bias — this was found to be the case, for example, with spillovers across Italian municipalities in a study of the effects of refugee presence on voting outcomes (Bratti et al. , 2020). In Table B4, we control for the migrant share across the union of all neighboring municipalities. The standard errors increase substantially reflecting the high correlation between the two migrant share measures. However, the main point estimate remains stable, indicating that cross-municipality spillovers are not adding large bias to the results.

[31] We present evidence that the first and second stage are roughly linear in Figures B4 and B3, respectively.

[32] The latter is less concerning considering that, while Venezuela used to be a top trading partner of Colombia, its trade shares steadily declined during the 2000s such that it represented less than 5% of exports and less than 1% of imports in 2010 (COMTRADE).

IV.D Evidence of contact and perceived demographic change

Contact: Two factors jointly determine the social integration of any two groups – (1) exposure, in this case, an increase in the migrant share of the population, and (2) friending bias, or the tendency to befriend migrants conditional on exposure (Chetty et al. , 2022). We thus cannot assume that inter-group exposure automatically translates into inter-group contact. It may be that, as migrant exposure increases, actual migrant-native interactions do not occur because of anti-migrant friending bias among Colombians. The first two columns of Table VIII test whether the share of foreigners in the social network increases when the migrant share increases using the specification as equation (5). Column 1 uses only the variation in individuals who respond that their network size is at least size one, while in Column 2 we replace the missing shares (not defined when the network size is zero) with 0 in order to bound the possible effects. We find that a 1 pp. increase in the migrant share increases the foreigner share of the network by 0.7pp relative to a mean of 3.6%, or 0.5pp relative to a mean of 2.4%.[33] These effects are precisely estimated. To conclude, when using a "high bar" for social interaction, we see that exposure does indeed translate into contact, implying that contact may be driving the positive coefficients we observe for the effect of local migration on migrant sentiments.

Perceived demographic change: Lastly, could the null effects of migration on attitudes toward migrants be driven by a lack of knowledge about the number of foreigners arriving to the municipality? We explore this possibility by examining if respondents perceive a larger migrant population as the migrant share in the area increases. The third column of Table VIII shows that a 1 pp. increment in the migrant share increases the perceived migrant share by 7.4 pp., a much larger increment than the true one. We thus find strong evidence that not only do people know that the share of migrants increased — they overestimate these effects by almost an order of magnitude.

IV.E Heterogeneity and mechanisms

Above, we presented evidence that larger migrant shares did not worsen attitudes toward migrants, and if anything, they improved them. We also showed that local migration increased native perceptions of the size of the migrant population and increased the number of migrants in natives' close social network, indicating that Venezuelan migration in

[33]A concern may be that migrants locate in places with higher connectivity in general, measured by network degree, or number of links. However, because we are instrumenting for migration, the predicted migrant share should not be correlated with average degree. We also show in Figure B5 that the raw correlation between the average degree and the migrant share is close to zero.

Table VIII: Foreign Network Share and Perceived Migration in Colombia

	Share Foreigners in Network		Perceived Migrant Share
Migrant Share	0.68***	0.50**	7.40***
	(0.25)	(0.19)	(1.26)
N Obs	28,531	41,850	41,850
Kleibergen-Paap F	97.02	135.66	135.66
Outcome Mean	3.55	2.42	38.35
Include Missing as 0		X	

All models control for full set of individual and municipality controls. The share of foreigners in the respondents' close network and the share of the population they think is a foreigner are measured in the ECP and lie between 0 and 100. In the second column, the share is set to 0 for respondents with a network size of 0. Standard errors are clustered at the municipality level. * p<0.10, ** p<0.05, *** p<0.01.

Colombia was salient to locals and induced meaningful native-migrant interactions. In this section, we explore key dimensions of heterogeneity in the response to migration — economic competition, urbanization and public good provision, and crime — as well as an additional variable related to source of media consumption. To do this, we interact the migrant share with the individual- or municipality-level characteristic being studied and instrument it with both of the instruments and their interactions with the same characteristic.[34]

It should be emphasized that the causal impact of having, for example, a higher urbanization rate cannot be identified because we do not attempt to instrument for urbanization. Instead, what we identify here may be related to other characteristics that are correlated with each heterogeneity variable. To account for multiple hypothesis testing, we compute sharpened false discovery rate q-values for the interaction terms, which can be interpreted as the expected portion of rejections that are type-I errors (Anderson, 2008). We test 8 heterogeneity variables and thus adjust for 8 multiple tests. A few of our interaction variables – income, unemployment, and news source – are measured at the individual level and thus could change in response to migration. We show in Table B6 that when these variables are used as outcomes none of them are statistically significant.[35] The remainder of

[34]Another interesting dimension of heterogeneity is the baseline level of contact and integration between migrants and natives. The contact hypothesis predicts that places with greater baseline integration are more likely to respond more positively to additional migration, and this has been documented in some settings (Campo et al. , 2023). However, in this context, there were very few Venezuelan migrants in Latin America before 2016, and thus little pre-period variation in Venezuelan integration across regions.

[35]The results in Table B6 also coincide with existing evidence in the literature. The coefficient on income is negative, as expected by the literature showing small negative wage effects in this setting, but it is small and not significantly different from zero. In the same table, we also present evidence that we can replicate results from Rozo & Vargas (2021) – Venezuelan migration increased voter turnout in the 2018 presidential elections and pushed voters to the right. In Colombia up until 2019, political parties had not taken a strong

the interaction variables are measured at the municipality level in the pre-period and thus do not change in response to the migration. Finally, we include all of the individual and municipality controls in these regressions, but we confirm that results are not affected if we remove them, and we conduct all of the same robustness tests from the previous section.[36]

IV.E.1 Labor market competition and economic characteristics:

Table IX presents the heterogeneity analysis by economic characteristics. Here we consider 5 hypotheses. The first two relate to direct competition in the labor market.

Unemployment: The literature in Colombia has mostly found that Venezuelan migration affected native wages and not employment, though there may have been increased unemployment among some subgroups such as youth under age 25 or less-educated women(Lebow, 2022b, Otero-Cortés et al. , 2022, Pedrazzi & Peñaloza-Pacheco, 2022). We test if unemployed individuals respond differently as they may be the most directly exposed to real or perceived employment effects. In this case, we do not find evidence that unemployed individuals respond differently to migration.

Labor market competition: In the second exercise we test for heterogeneity according to an individual's propensity to work in sectors facing more migrant competition. A variety of studies find that negative wage effects of Venezuelan migration in Colombia are mostly concentrated among lower-income native workers in sales, services, construction, and manufacturing jobs (Lebow, 2022b, Delgado-Prieto, 2021, Caruso et al. , 2019). These workers may therefore respond more negatively to local migration. On the other hand, contact in the workplace could cause an improvement in migrant sentiment.

We do not observe industry in the ECP. Therefore, we use the GEIH to construct a measure of "industry exposure" based on demographic characteristics at the department level, which is the finest geographic unit for which industry information is available. We calculate:

$$IndExp_{dg} = \sum_{k} NativeShare_{kdg,2014} * VenShare_{k,2015-2019} \tag{6}$$

stance against migration and the majority of politicians were supportive of migrant regularization initiatives. However, the Colombian right was pointing to socialist policies in Venezuela as the cause of the Venezuelan exodus and using this as a campaign slogan against left-wing candidates and ideologies. Our results highlight that these political effects did not coincide with local worsenings in migrant sentiment.

[36]These robustness tests are presented in Table B5 and only discussed when they meaningfully affect the results. In the few cases where the Kleibergen-Paap first stage F-statistic falls below 15, we suppress the output.

Within each department d and each of the 2-by-3-by-4 gender-age-education groups g, we multiply the share of working natives with the share of working Venezuelans in industry k, summed over 8 core industries. The industry shares for natives are calculated in 2014 to prevent the statistics from being influenced by industry movements in response to the migration wave. The Venezuelan shares are calculated at the national level using data from 2015-2019 to maximize the sample size.[37] In column 2 of Table IX, we interact the model with $IndExp_{dg}$, restricting the sample to those who are working or unemployed. There are no differences in MAI-ECP by predicted industry exposure.

Income of respondent: Given the strong evidence that any effects of Venezuelan migration on Colombian native wages was strongly concentrated among lower-income workers, in Column 3 we interact the migrant share with the respondent's income decile within their municipality. Here we find that the interaction is negative and statistically different from zero at the 10% level after accounting for multiple hypothesis testing. This implies that the effects of being exposed to larger migration flows are most positive for those individuals in the lowest income decile, for whom there is an increment in the MAI-ECP of 0.06 SD. This effect decreases with income, becoming .02 SD for those in the top 10% of income in the municipality. That is, these results show that people *more likely to face wage competition* with Venezuelan migrants is precisely the group that responds more positively to the migration.

Average income in municipality: We also test if the effect is predicted to be larger in wealthier municipalities, as measured by value-added per capita in 2015. We fail to find evidence for this being the case.

Perception of economic competition within social circle: One way in which fear of economic competition might present itself is not necessarily through fears of the respondents' own lost opportunities, but instead from those within their close social circle. We test if migration increases individuals reporting someone in their social circle who has lost economic opportunity to migrants.In the last two columns of Table IX, we present the results when this variable is used as the outcome (this test does not involve an interaction). In Column 5 we present the results only for those who report a social network with at least one person in it, while in Column 6 we impute the number of people who lost economic opportunities to zero if the network size is zero. The estimate is positive (a 1pp increase in migrant share

[37] We do not calculate Venezuelan industry shares within department or demographic groups. This is to maintain a sufficient sample size within subgroups. Demographic groups are also less relevant for Venezuelans in terms of labor market competition since many young and more educated Venezuelans are working jobs traditionally filled by older and less educated natives (Lebow, 2022a).

Table IX: Colombia ECP Analysis - Heterogeneity by Economic Characteristics - 2SLS

	Interaction Variable: Unemployed	Ind. Exp. Index	Income Decile	Ln Municip VA Per-Cap		
	Migrant Acceptance Index (ECP)				Someone in Network Lost Econ Opp.	
Mig Share	0.04* (0.02)	0.04* (0.03)	0.062*** (0.020)	0.04 (0.02)	2.28 (1.55)	1.70 (1.09)
Mig Share X Interaction Var	-0.02 (0.02)	-0.02 (0.01)	-0.004* (0.002)	-0.01 (0.03)		
Test: Mig Share X Int. Var = 0 (FDR Sharpened q-values)	[q = 0.43]	[q = 0.25]	[q = 0.10]	[q = 0.66]		
Kleibergen-Paap F	70.09	70.47	92.97	19.90	97.02	135.66
N Obs	31,103	31,103	31,103	41,850	28,531	41,850
Outcome Mean					25.79	17.58
Include Missing as 0						X

All models control for full set of individual and municipality controls. Log 2015 municipality value added per-capita is provided by DANE and standardized. All other interaction variables are measured at the individual level in the ECP. Unemployed is binary, income decile lies from 1-10, and the Industry Exposure Index is standardized. The Industry Exposure Index measures the share of migrants working in the respondent's predicted occupation using the Colombian labor force survey (GEIH) from 2014-2019. Someone in Network Lost Econ Opp. is an indicator (multiplied by 100) for having anyone in the reported network that lost economic opportunity to foreigners. Standard errors are clustered at the municipality level. * p<0.10, ** p<0.05, *** p<0.01.

causes a 1.7pp-2.3pp increase in the likelihood of knowing someone with this characteristic) but it is not statistically different from zero.

Given the evidence for economic competition among low-income workers in heavily impacted industries, why do we not find that these workers are more likely to respond negatively to Venezuelan migration? First, it may not be salient when and where migrants are increasing economic competition. Second, when this competition is salient, this may not translate into lower trust or preferences for segregation among the native workers in impacted occupations. Third, positive interactions between natives and migrants in the workplace could counteract any concerns over labor force competition. The lack of results on economic competition is consistent with existing evidence from various settings that anti-migrant sentiment is often not driven by fears over economic competition (Card et al. , 2012, Alesina & Tabellini, 2022).

IV.E.2 Public Goods, Urbanization, Crime, and Media Source:

Urbanization: Previous work has found that individuals in rural and urban areas can respond differently to migration inflows (Dustmann et al. , 2019, Mayda et al. , 2020). We explore this possibility in our setup. Column 1 of Table X shows that, although the effect of migration is positive throughout, it is substantially larger in non-urban areas and smaller in urban ones. In the former, the effect of a 1 pp. increase in the migrant share is 0.13 SD of MAI-ECP, whereas in the latter it is 0.03 SD. This difference is statistically significant even after adjusting for multiple hypothesis testing.

Table X: Colombia ECP Analysis - Heterogeneity by Other Characteristics - 2SLS

	Interaction Variable:			
	Urbanization Rate	News from Internet or Social Media	Crime Rate	Public Goods Index
	Migrant Acceptance Index (ECP)			
Mig Share	0.13***	0.05*	0.03	0.07***
	(0.05)	(0.03)	(0.02)	(0.02)
Mig Share X Interaction Var	-0.10**	-0.03*	0.00	-0.06**
	(0.04)	(0.02)	(0.02)	(0.03)
Test: Mig Share X Int. Var = 0	[q = 0.10]	[q = 0.10]	[q = 0.68]	[q = 0.10]
(FDR Sharpened q-values)				
Kleibergen-Paap F	21.42	91.49	90.49	51.64
N Obs	41,850	41,850	41,850	41,850

All models control for full set of individual and municipality controls. Urbanization rate, crime rate, and public goods index are standardized. Whether they access news from internet or social media is binary and measured at the individual level. Public Goods Index is a PCA-derived combination of the CEDE score for secondary test scores, (negative) infant mortality rate, piped water, sewage, and electricity coverage. Standard errors are clustered at the municipality level. * p<0.10, ** p<0.05, *** p<0.01.

Why would the effect of immigration on migrant sentiment be more positive in more urbanized municipalities? One possibility is that urbanized municipalities are wealthier and have higher quality public goods. Above, we did not find evidence that the average income of the municipality matters for the response. Here, we test if this difference is tied to the quality of public good provision.

Public Goods Quality: In Column 4 of Table X we interact the migration share with an index of public goods quality, which is based on the first principal component of pre-period average secondary test scores, (negative) infant mortality rates, electricity coverage, trash collection, and sewage coverage. On the one hand, over-crowding of education and health resources has been at the forefront of the debate surrounding Venezuelans in Colombia and could generate worse responses in places where public goods provision is strained. On the other hand, where there is better public goods provision, individuals might reject sharing those resources or worry that they have more to lose in terms of public goods quality (Alesina et al., 1999). Our results show that the effect of migration becomes less positive as the public goods index increases. The interaction term is significant at the 5% level and at the 10% level after accounting for multiple hypothesis testing. However, Table B5 shows that this interaction effect disappears after department fixed effects are included, indicating that this heterogeneity result may only hold across and not within departments.

Crime rate: Next, we test if the crime rate modifies the response to migration. In Column 3 of Table X we interact the model with the pre-migration crime rate in the municipality. This is an important hypothesis given that, while there is limited evidence that Venezuelans actually increased crime, many natives continue to associate Venezuelans with crime (Knight & Tribin, 2020, Bahar et al. , 2020, Mora, 2020). The effect of migration on attitudes towards migrants may be larger in municipalities with higher baseline crime rates, where crime is more of a concern. On the other hand, it could be higher in places with lower baseline crime, as these places may be less accustomed to crime or feel as if they have more to lose in terms of community safety. We do not find any meaningful differences in the treatment effect across this dimension.

Online news: Sources of information, particularly those of non-traditional news outlets such as social media, might affect how natives respond to larger migration inflows (Vosoughi et al. , 2018). In Column 2 of Table X we interact the migrant share with an indicator for the respondent's main news source being either the internet or social media. We find some suggestive evidence that those who take these outlets as their source of information do not improve their attitudes toward migrants as much as those who do not. The coefficient implies that the response of those who consume online news is 60% less positive than those who do not, and it is statistically significant at the 10% level after accounting for multiple hypothesis testing.

IV.F Takeaways

To summarize, we find a small positive effect of local migration on migrant sentiment. The effect is not negative for any subgroup that we study, and it is more positive for lower-income natives, in rural areas, and in areas with worse public goods provision, though these results are only significant at the 10% level after adjusting for multiple-hypothesis testing. This is the opposite of what one might expect based on much of the dialogue around Venezuelan migration in Colombia, which has focused on concerns about public resource scarcity and economic competition in the most vulnerable regions of the country. Instead, our evidence indicates that feelings of migrant threat may be more easily fomented in places where these characteristics are *less strained*, rather than more strained. Other characteristics, such as predicted industry competition or baseline crime rates, are unrelated to the effect of migration on migrant sentiment. Finally, there is evidence that individuals who rely on internet and social media for their news are less likely to respond favorably to migrant inflows. Though this evidence is only indicative given that this variable may be correlated with other important characteristics, it points to the potentially important role of migrant portrayal in online media in driving migrant sentiment, and this is an important area for future research. Once again, the takeaway from this section is not that Venezuelan migration had no effect on migrant sentiment in Colombia — indeed, there was a large worsening of migrant sentiment across Colombia over this period. Instead, the takeaway is that there is no evidence that this effect was driven by the places in Colombia that received the most migrants.

V Conclusion

In this paper, we study the effect of the Venezuelan exodus on migrant acceptance across Latin America. We study multiple outcomes including self-reported attitudes toward migrants, hateful tweets about Venezuelans, trust in foreigners, and preferences for segregation. We implement both OLS and 2SLS analyses using two units of geographic variation - regions across seven countries and municipalities across Colombia. In each case, we evaluate the exclusion restriction by testing for pre-trends in variables related to migrant sentiment. Despite the observable decline in migrant acceptance in the countries that received the most migration, we find no evidence that there is a causal effect of migration on local xenophobic sentiment within the host communities. This suggests that the nationwide deterioration in attitudes towards migrants is not primarily attributed to the mechanisms commonly proposed in the existing literature -— economic competition, strain on public resources, increased crime, or group threat -— among those residing in areas with more migration. In fact, our Colombia-specific analysis finds that the effect of migrant arrival is *more positive* for people who are poorer and in locations that are more rural and have lower-quality public goods. Instead, the national level decline in migrant sentiments must be driven by factors acting on a national level, which may include feelings of national group threat or misperceptions of the true effects of migration in migrant-hosting regions.

We also find evidence that close, repeated, and meaningful contact increases in locations where migrants arrive, supporting the possibility that migrant-native contact mitigates any negative sentiments induced by local migrant arrival. These findings are in a context in which migrants and natives speak the same language and have close cultural overlap, but where Venezuelan migrants have nonetheless been discriminated against and placed into an out-group boundary. This setting is crucial for understanding the majority of global migration and forced displacement, which predominantly occur between neighboring countries in the Global South.

From a policy perspective, these findings imply that policies to reduce xenophobia towards migrants should not focus only on the places that received the most migration. Future researchers on this topic should strive to better understand the nationwide drivers of migrant sentiment. This includes media portrayal and a national dialogue that may or may not reflect the real effect of migration on hosting communities.

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A Additional Tables and Figures - Gallup and Twitter Analysis

Table A1: Venezuelan Migration Across Latin America

Host Country	Venezuelan Share (2019)	Venezuelan Pop. (2019)	Has Gallup MAI	Included in Study
Colombia	3.37	1,600,000	X	X
Peru	2.83	864,000	X	X
Panama	2.38	946,000	X	X
Ecuador	2.37	385,000	X	X
Guyana	2.13	17,000		
Chile	2.07	371,000	X	X
Trinidad & Tobago	1.38	21,000		
Costa Rica	0.60	289,000	X	
Uruguay	0.40	137,000	X	X
Argentina	0.34	145,000	X	X
Dominican Republic	0.29	30,000	X	
Brazil	0.11	224,000	X	
Mexico	0.06	715,000	X	
Paraguay	0.06	380,000	X	

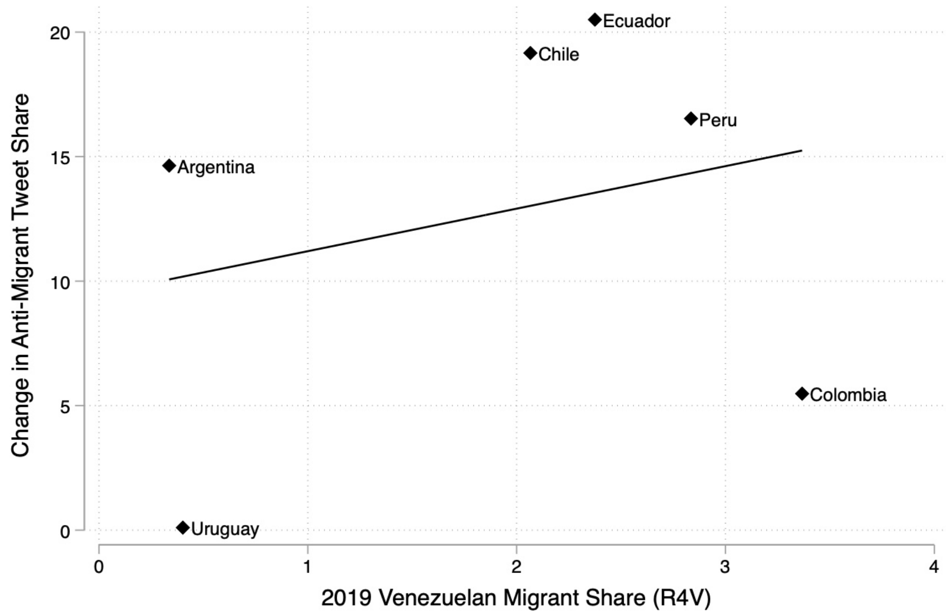
This table presents the Venezuelan migrant population in the primary Venezuelan-hosting countries in Latin America and the Caribbean as a raw number and as a share of the national population. Costa Rica was excluded from the study because of a change in Gallup geographic units between 2016 and 2019. Source for migration data: R4V (2019).

Table A2: Correlates of Gallup Migrant Sentiment

	Migrant Acceptance Index	
	2016	**2019**
Argentina	(omitted)	(omitted)
Colombia	-0.30***	-0.93***
	(0.07)	(0.09)
Ecuador	-0.28***	-1.12***
	(0.06)	(0.08)
Peru	-0.20***	-1.07***
	(0.07)	(0.08)
Panama	-0.79***	-0.71***
	(0.10)	(0.10)
Chile	-0.53***	-0.19***
	(0.07)	(0.06)
Uruguay	-0.02	0.11*
	(0.05)	(0.06)
Male	0.07***	0.10***
	(0.02)	(0.02)
Age	-0.00***	-0.00***
	(0.00)	(0.00)
Completed Secondary	0.17***	0.19***
	(0.03)	(0.03)
Completed Tertiary	0.35***	0.36***
	(0.04)	(0.05)
Urban	0.07*	0.11***
	(0.04)	(0.04)
N	6,558	6,808
R^2	0.10	0.22

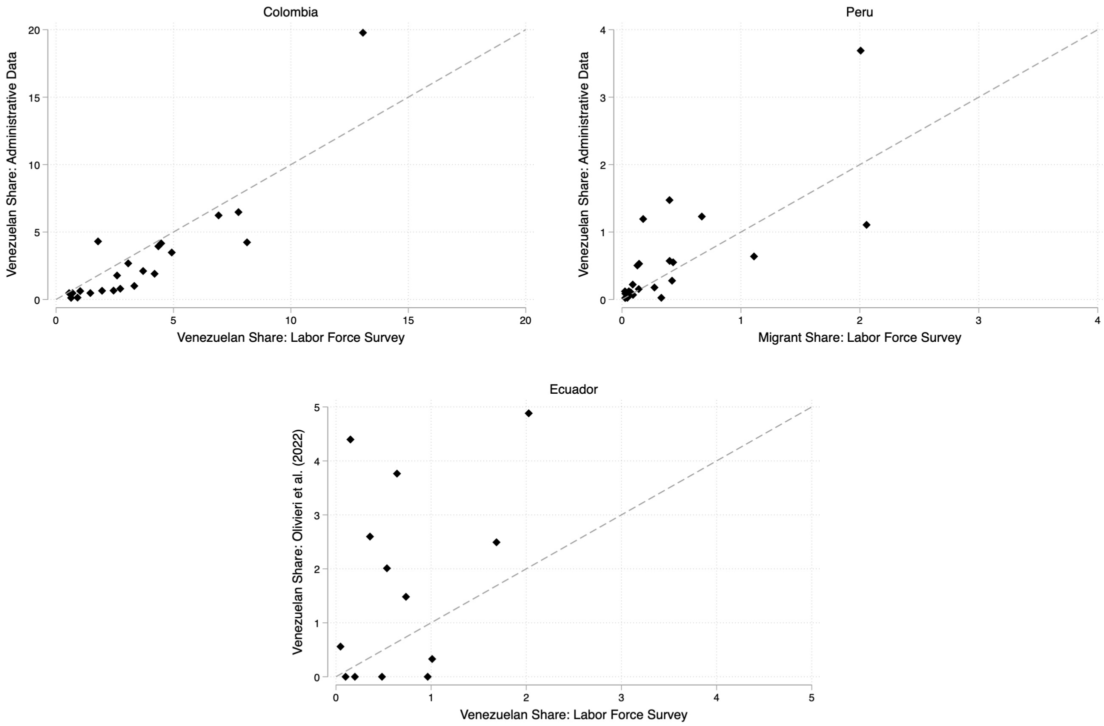
See Data Appendix for description of each variable. Standard errors clustered at the region level. * p<0.10, ** p<0.05, *** p<0.01.

Figure A1: Change in Anti-Migrant Tweet Share across Countries



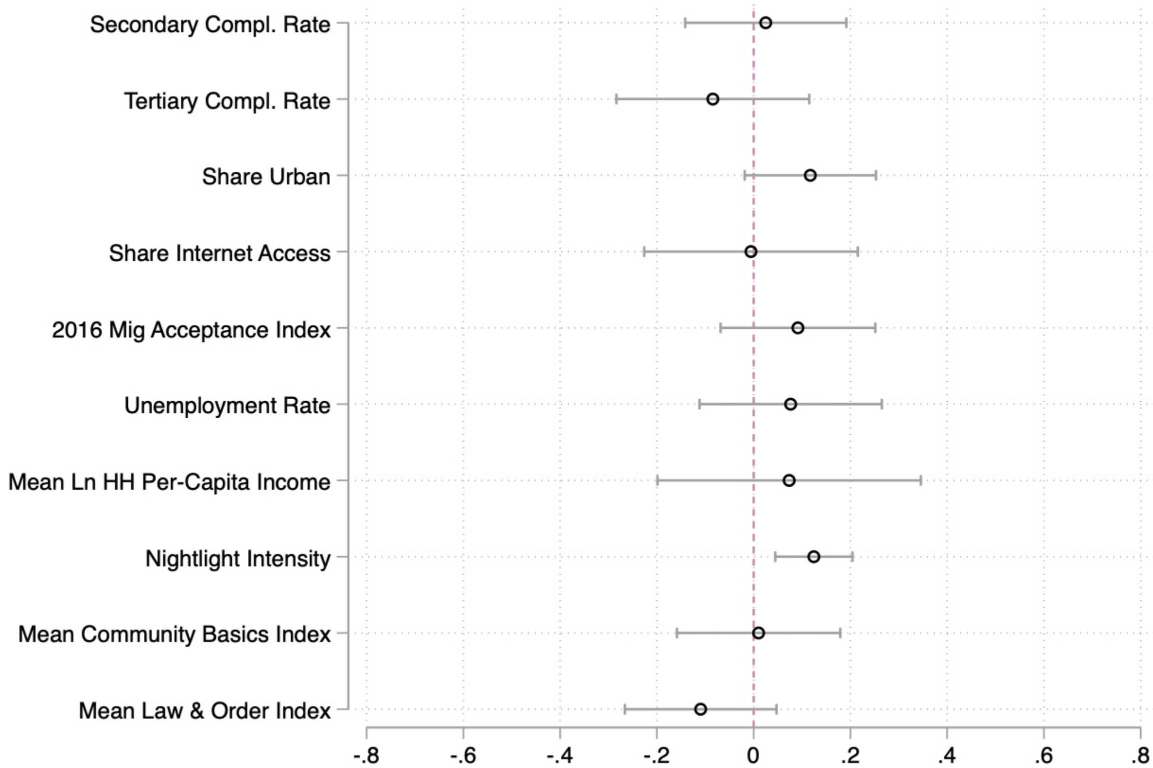
The 2015-2019 change in the share of tweets about Venezuelans that are classified as "anti-migrant" are plotted against the Venezuelan migrant share.

Figure A2: Migrant Shares using Alternate Data Source



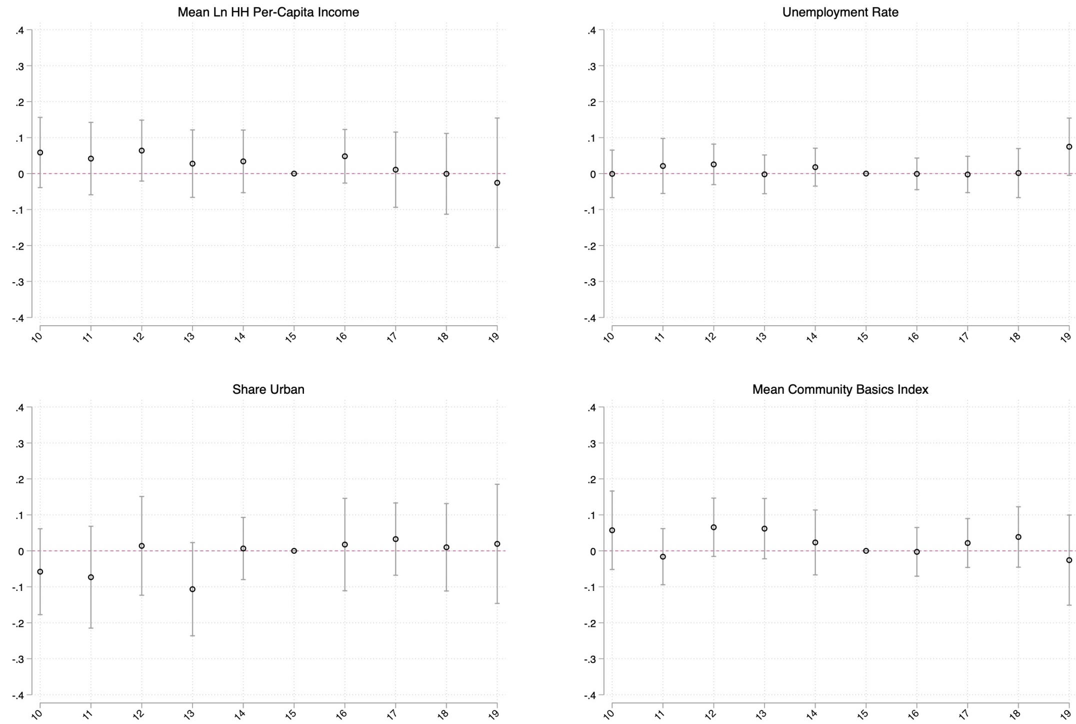
These figures compare the 2019 Venezuelan population (or total foreign-born population in Peru) as a share of the 2015 population at the region level as measured in labor force surveys (x-axis) and in alternate data sources (y-axis). The alternate estimates for Colombia and Peru are official estimates from migration authorities based on migrant regularizations by region, while the alternate estimates for Ecuador are from a mobile-phone based survey collected by (Olivieri et al. , 2022). The correlation coefficient is .9 in Colombia, .75 in Peru, and .4 in Ecuador.

Figure A3: Baseline Correlates of Instrument (Historical Migrant Share)



This figure shows that our instrument – the migrant share in the 2005 census (IPUMS) – is not correlated with baseline characteristics of the region. The figure presents the point estimate of regressing the migrant share of the population in the past census on region-level covariates measured at baseline (2016) and country FE for the 116 regions in our analysis. Nightlight intensity is measured by AidData and all other covariates are measured in Gallup (see Data Appendix for details). The outcome and all non-binary covariates are converted to a Z-score. Robust 95% confidence intervals presented.

Figure A4: Pre-trends Interacted with Instrument (Historical Migrant Share)



This plot presents a series of event studies for each outcome variable on year FE, region FE, and the historical migrant share (Z-score) interacted with year excluding 2015. It shows that there are no differential trends in the outcomes by values of our instrument by region. The outcomes are: Mean Log Household Per-Capita Income (upper left); Unemployment Rate (upper right); Share Urban in Region (lower left); and Mean Community Basics Index - an index for quality of provision of public goods (lower right). All outcomes are measured by Gallup. Standard errors clustered by region. 95% confidence intervals presented using uniform confidence intervals by Montiel Olea & Plagborg-Møller (2019)

Table A3: Gallup Analysis - Individual Questions

	Mig in Country is Good (Mean = 47.94, SD = 49.96)		
Migrant Share	-0.31	-1.01	-0.67
	(2.80)	(0.80)	(0.81)
	Mig Becoming Neighbor is Good (Mean = 56.16, SD = 49.62)		
Migrant Share	1.84	1.19	1.57**
	(2.49)	(0.77)	(0.76)
	Mig Marrying Relative is Good (Mean = 50.40, SD = 50.00)		
Migrant Share	0.87	0.44	0.88
	(1.99)	(0.75)	(0.77)
N Obs	13,366	13,366	13,366
N Clusters	116	116	116
Region FE and Year FE	X	X	X
Country-Year FE		X	X
Individual Controls			X

Estimates of Equation 1. Outcomes are each of the individual questions that make the Migrant Acceptance Index, as measured in Gallup 2016 and 2019 (see Table IV). The sample is restricted to respondents who have been in the country for at least 5 years. Individual controls include gender, age FE (binned into 5-year intervals), education FE (grouped into not completed secondary, completed secondary, and completed post-secondary), urban-rural status. Standard errors clustered at the region level. * p<0.10, ** p<0.05, *** p<0.01.

Table A4: Gallup Analysis - Robustness

	Migrant Acceptance Index (MAI)				
	Dif-in-dif	IV Dif-in-dif	N	C	K-P F
Original Model	0.02 (0.02)	0.02 (0.02)	13,366	116	42.64
Drop migrant share outliers	0.01 (0.02)	0.01 (0.03)	12,009	111	20.26
Use Olivieri et al. Ecuador mig rate	0.01 (0.02)	0.02 (0.02)	13,328	116	44.36
Control pre-period region char. X year	0.03* (0.02)	0.06** (0.03)	13,366	116	27.09
Gallup sampling weights applied	0.00 (0.02)	0.01 (0.02)	13,366	116	44.18
Drop Colombia	0.02 (0.03)	0.01 (0.02)	11,563	94	27.66
Drop Ecuador	0.02 (0.02)	0.03* (0.02)	11,663	102	38.01
Drop Peru	0.02 (0.02)	0.02 (0.02)	11,390	93	39.82
Drop Panama	0.01 (0.02)	0.03 (0.02)	11,375	107	35.70
Drop Chile	0.03* (0.02)	0.02 (0.02)	11,348	101	44.92
Drop Argentina	0.02 (0.02)	0.02 (0.02)	11,527	101	40.05
Drop Uruguay	0.02 (0.02)	0.03 (0.02)	11,330	98	39.75

All models include region FE, country-year FE, and individual controls. Standard errors clustered at the region level. Pre-period region-level controls include the 2016 mean MAI, completed secondary and tertiary education, urbanization rate, internet access, mean log household per-capita income, unemployment rate, nightlights index, and the mean law and order index and community basics index. * p<0.10, ** p<0.05, *** p<0.01.

Table A5: Twitter Analysis - Extensive Margin

	Number of Tweets about Venezuelans (Mean = 419.35, SD = 1403.72)		
Migrant Share	113.33 (203.62)	81.90 (109.14)	346.69* (203.10)
N Obs	86	86	86
N Clusters	43	43	43
Kleibergen-Paap F		10.01	5.01
Region FE and Year FE	X	X	X
2SLS		X	X
Country-Year FE			X

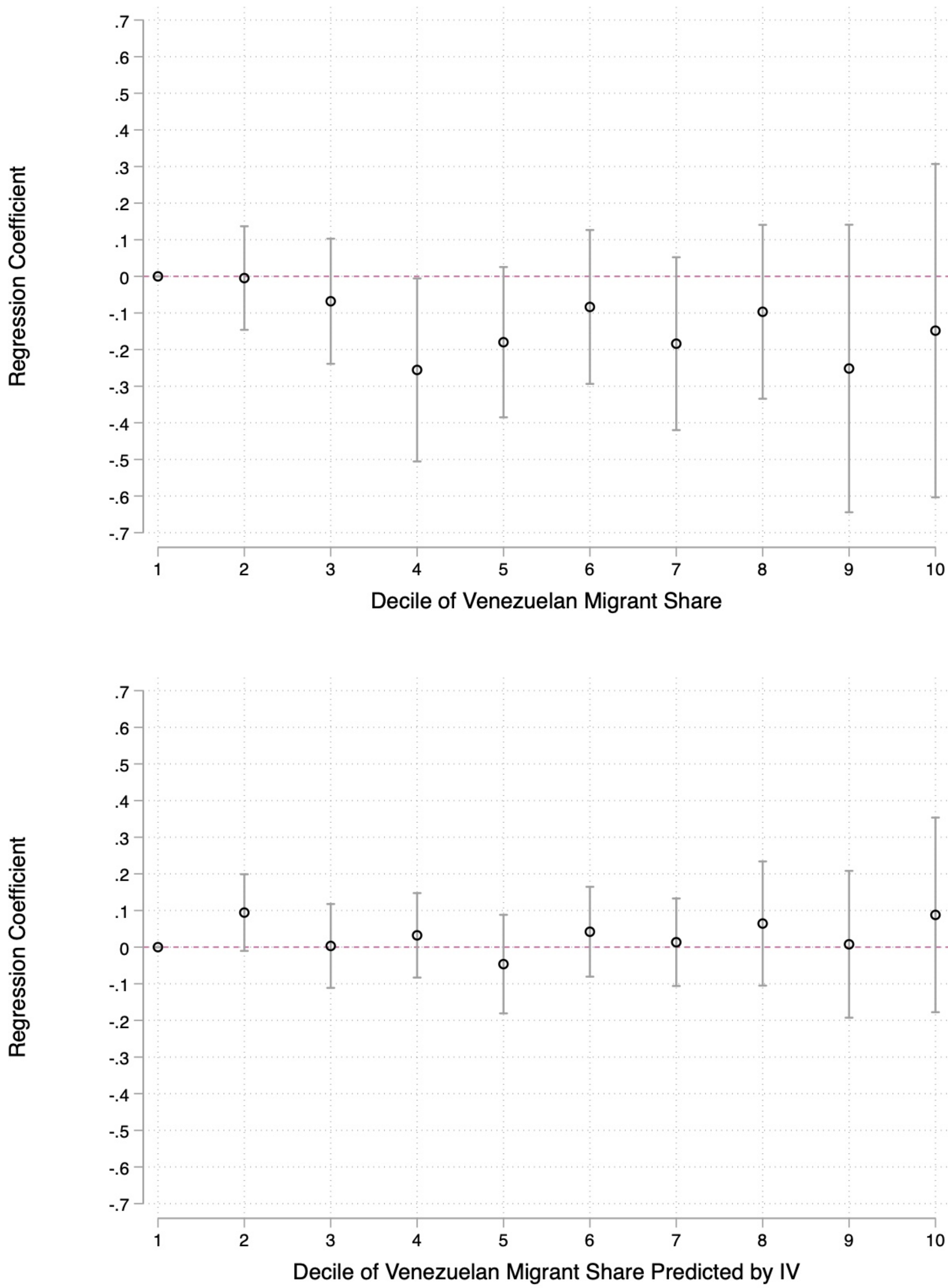
The outcome is the number of tweets per region that are geo-coded and that include reference to the Venezuelan population. The sample includes only regions in which there are at least 50 tweets. Standard errors clustered at the region level. * p<0.10, ** p<0.05, *** p<0.01.

Table A6: Twitter Analysis - Robustness

	Anti-Migrant Tweet Share				
	Dif-in-dif	IV Dif-in-dif	N	C	K-P F
Original Model	-1.00 (0.66)	-1.14** (0.44)	86	43	24.18
Drop migrant share outliers	-0.26 (1.17)	-0.67 (0.86)	78	39	10.59
Use Olivieri et al. Ecuador mig rate	-1.00 (0.66)	-1.14** (0.44)	84	42	24.07
Control pre-period region char. X year	-1.47*** (0.51)	-1.60*** (0.41)	78	39	13.92
Include regions with >10 tweets	-0.76 (0.64)	-0.75* (0.45)	154	77	30.74
Drop Colombia	-0.44 (1.52)	-0.63 (0.93)	48	24	41.85
Drop Ecuador	-0.99 (0.66)	-1.12** (0.44)	80	40	24.09
Drop Peru	-1.03 (0.65)	-1.17*** (0.45)	82	41	24.18
Drop Panama	-1.00 (0.66)	-1.14** (0.44)	86	43	24.18
Drop Chile	-1.05* (0.62)	-1.13** (0.45)	74	37	21.49
Drop Argentina	-1.16* (0.67)	-1.36*** (0.47)	64	32	18.84
Drop Uruguay	-0.99 (0.65)	-1.10** (0.45)	82	41	23.73

All models include Region and Country-Year FE. Standard errors clustered at the region level. Weighted by the total number of tweets about Venezuelan migrants. Pre-period region-level controls include the 2016 mean MAI, completed secondary and tertiary education, urbanization rate, internet access, mean log household per-capita income, unemployment rate, nightlights index, and the mean law and order index and community basics index. * p<0.10, ** p<0.05, *** p<0.01.

Figure A5: Non-Linear Effects of Migration on Migrant Sentiment - Gallup



Gallup MAI is regressed on deciles of the migrant share, or the migrant share predicted from the instrument, alongside region fixed effects, country-year fixed effects, and individual controls. Standard errors clustered at the region level. 95% confidence intervals presented.

Table A7: Gallup Heterogeneity Analysis - Outcome: Migrant Acceptance Index

	Interaction Variable:							
	Non-Economic			**Economic**			**Public Goods**	
	Urban (Binary)	Internet Access (Binary)	Mean Baseline Migrant Acceptance Index (Z-Score)	Unemployed (Binary)	Income Decile in Country (1-10)	Baseline Nighttime Light Index (Z-Score)	Mean Baseline Law & Order Index (Z-Score)	Mean Baseline Public Goods Index (Z-Score)
OLS								
Mig Share	0.01	0.02	0.02	0.02	0.01	0.02	0.02	0.03*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Mig Share X Interaction Var	0.01	0.00	-0.04	-0.01	0.00*	0.00	0.01	0.02
	(0.01)	(0.01)	(0.03)	(0.01)	(0.00)	(0.01)	(0.06)	(0.02)
Test: Mig Share X Int. Var = 0 (FDR Sharpened q-values)	[q = 1.00]	[q = 1.00]	[q = 0.89]	[q = 0.92]	[q = 0.89]	[q = 1.00]	[q = 1.00]	[q = 0.89]
2SLS								
Mig Share	0.03	0.04**	0.03*	0.03	0.02	0.02	0.04*	0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Mig Share X Interaction Var	-0.01	-0.01	-0.07	-0.02*	0.00	0.00	0.10	0.02
	(0.01)	(0.01)	(0.04)	(0.01)	(0.00)	(0.01)	(0.08)	(0.02)
Test: Mig Share X Int. Var = 0 (FDR Sharpened q-values)	[q = 0.88]	[q = 0.88]	[q = 0.88]	[q = 0.88]	[q = 1.00]	[q = 1.00]	[q = 0.88]	[q = 0.88]
Kleibergen-Paap F	13.78	22.23	17.94	23.34	19.32	18.97	13.29	25.38
N Obs	13,366	13,366	13,366	13,366	13,366	13,366	13,366	13,366
N Clusters	116	116	116	116	116	116	116	116

All models include region FE, country-year FE, and individual controls. Standard errors clustered at the region level. Urban, internet access, unemployed, and income decile are measured at the individual level. Baseline regional averages for MAI, nightlights index, law & order index, and community basics index are standardized such that "Migrant Share" measures the treatment effect at the mean region and "Migrant Share X Interaction" measures the change in the treatment effect from a 1SD increase in the interaction variable. See Data Appendix for description of each variable and sources. * p<0.10, ** p<0.05, *** p<0.01.

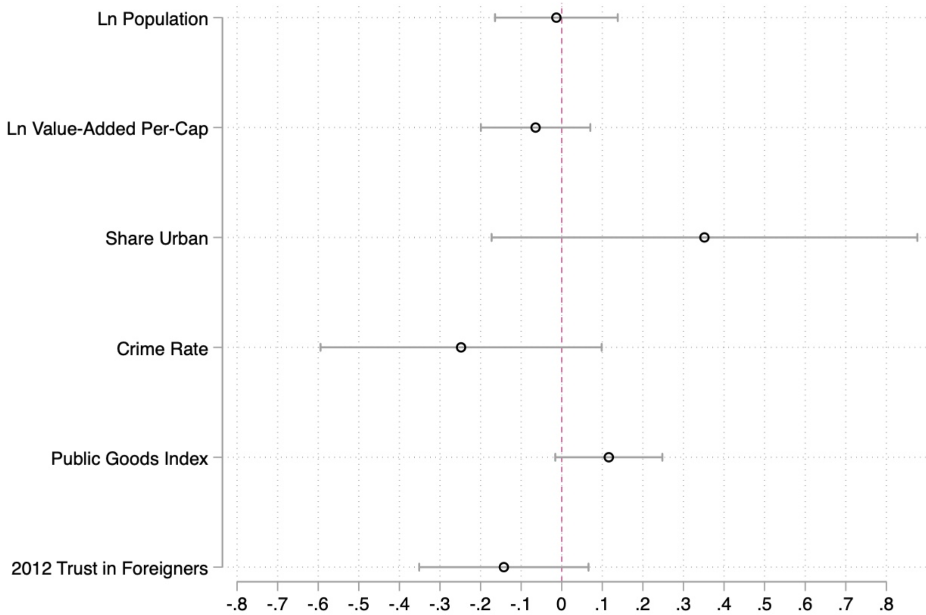
B Additional Tables and Figures - Colombia Analysis

Table B1: Correlates of MAI-ECP

	Migrant Acceptance Index (ECP 2019)
Male	-0.05***
	(0.01)
Age	0.00
	(0.00)
Completed Secondary	0.10***
	(0.02)
Completed Tertiary	0.15***
	(0.04)
Urban	0.08**
	(0.03)
N	41,850
R^2	0.01

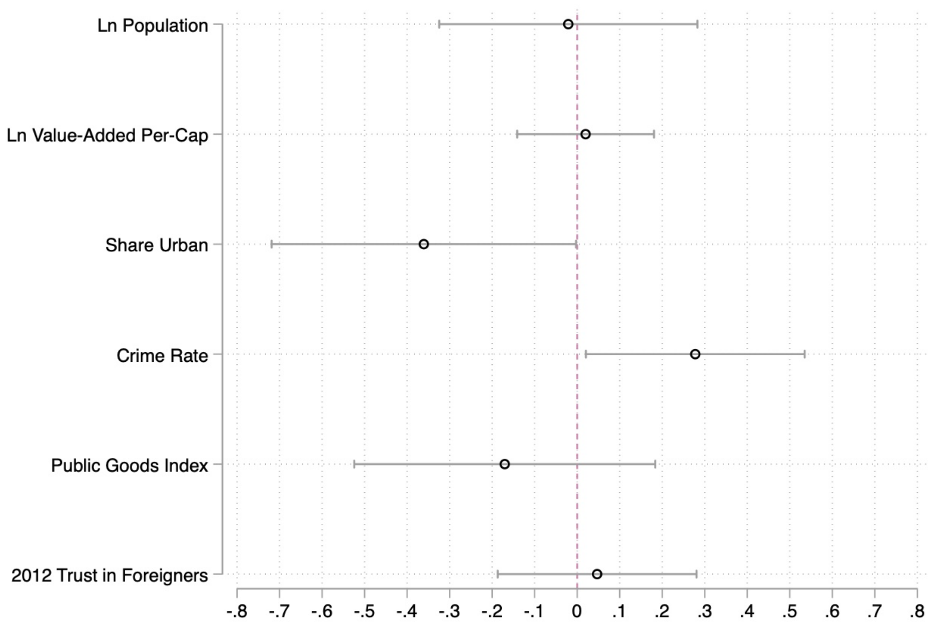
Standard errors clustered at the municipality level. * p<0.10, ** p<0.05, *** p<0.01.

Figure B1: Baseline Correlates of 2005 Venezuelan Share



The 2005 census Venezuelan share is regressed on the above of municipality-level pre-period covariates for the 118 municipality in our analysis. The outcome and all covariates are converted to a Z-score. Robust 95% confidence intervals presented.

Figure B2: Baseline Correlates of Driving Distance to Border



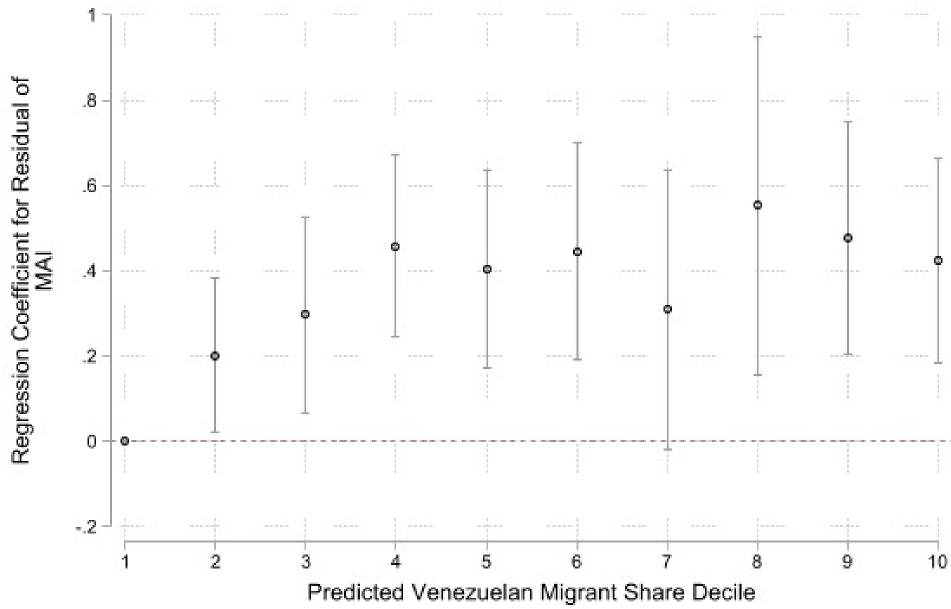
Driving distance to the Venezuelan border is regressed on the above of municipality-level pre-period covariates for the 118 municipality in our analysis. The outcome and all covariates are converted to a Z-score. Robust 95% confidence intervals presented.

Table B2: Colombia ECP Analysis - Individual Questions

	Trust in Foreigners (Mean = 1.70, SD = 1.01)			
Migrant Share	0.06**	0.04	0.03	0.03
	(0.02)	(0.02)	(0.02)	(0.02)
	Okay with Foreigner Neighbor (Mean = 93.45, SD = 24.74)			
Migrant Share	0.73**	0.79*	0.74	0.77
	(0.31)	(0.47)	(0.47)	(0.48)
N Obs	41,850	41,850	41,850	41,850
Kleibergen-Paap F		243.58	248.40	135.66
Sargen-Hansen Chi-Sq P		0.245	0.196	0.718
2SLS		X	X	X
Individual Controls			X	X
Municipality Controls				X

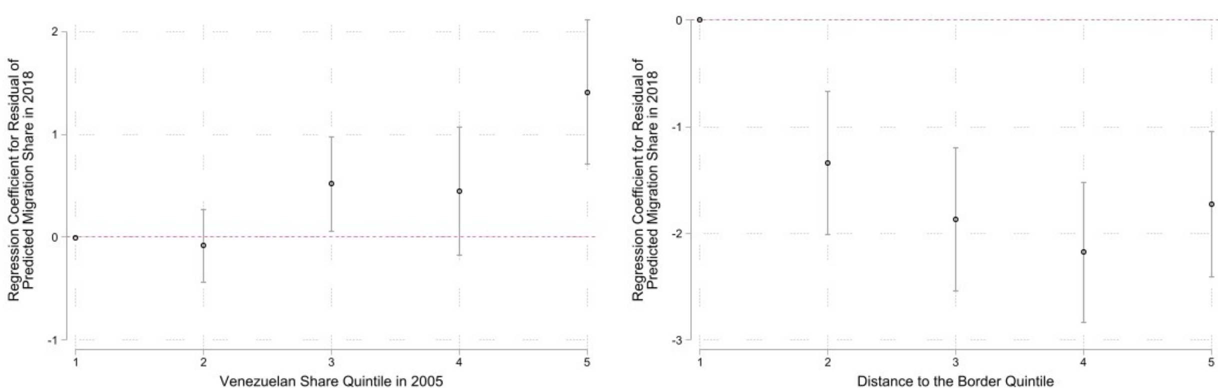
Outcomes are each of the individual questions that make the MAI-ECP (see Table VII). Standard errors are clustered at the municipality level. * p<0.10, ** p<0.05, *** p<0.01.

Figure B3: Non-Linear Effects of Migration on MIA-ECP



This graph shows that the effect of migration on migrant sentiment is roughly linear. It presents the coefficients from regressing the MAI-ECP on each decile of the predicted migrant share from both instruments, alongside all individual and municipalities controls. Standard errors clustered at the municipality level. 95% confidence intervals presented.

Figure B4: Non-Linear Effects of each Instrument on the Migrant Share



This graph shows that the first stage of the effect of each instrument (migrant share in 2005 [left] and log distance to the Venezuelan border [right]) is roughly linear. It presents the coefficients from regressing the migrant share (residualized from all individual and municipalities controls) on each quintile of each instrument. Standard errors clustered at the municipality level. 95% confidence intervals presented.

Table B3: Colombia ECP Analysis - Robustness

	MAI-ECP	N	K-P F
(1) Original Model	0.04* (0.02)	41,850	135.66
(2) Drop migrant share outliers	0.13 (0.08)	36,355	28.09
(3) Include return migrants in migrant share	0.03* (0.02)	41,850	139.52
(4) Drop Bogotá	0.04* (0.02)	36,582	125.91
(5) ECP sampling weights applied	0.01 (0.03)	41,850	103.17
(6) Include Departament FE	0.11*** (0.03)	41,850	33.19

Each row represents a different specification. Migrant Acceptance Index from ECP (MAI-ECP) is a standardized index constucted based on the ECP question on trust toward foreigners and being OK with having a foreigner as neighbor. All models control pre-period log population and full list of individual and municipality controls. Row 2 drops the 12 out of 117 municipalities with a migrant share greater than 2.5%. Row 3 counts any returned colombian migrant as part of the migrant share Row 6 includes fixed effects for the 24 departments in Colombia. Standard errors clustered at the municipality level. * p<0.10, ** p<0.05, *** p<0.01.

Table B4: Colombia ECP Analysis - Testing for Spillovers

	Migrant Acceptance Index - ECP (Mean = 0.00, SD = 1.00)	
Migrant Share in Municipality	0.05**	0.04
	(0.02)	(0.04)
Migrant Share in Neighboring Municipality		0.02
		(0.04)
N Obs	41,850	41,850
Individual Controls	X	X
Municipality Controls	X	X

See notes to Table VII. Migrant share in neighboring municipality is the average migrant share across all municipalities bordering the municipality of the observation. * p<0.10, ** p<0.05, *** p<0.01.

Figure B5: Average Network Size and Venezuelan Share by Municipality in Colombia

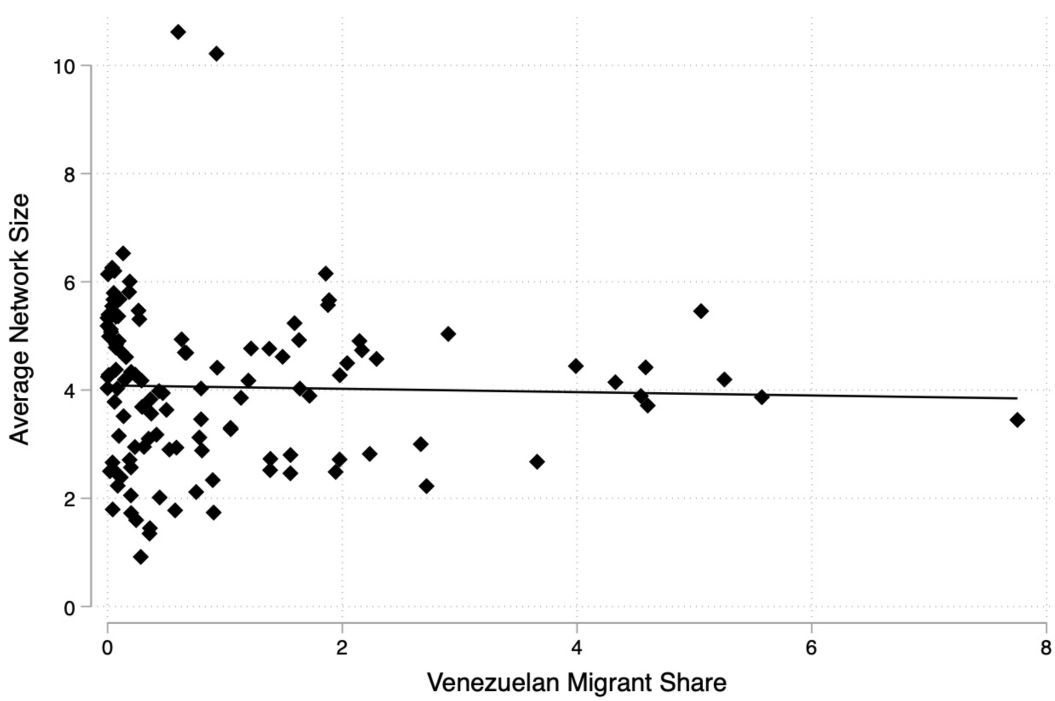


Table B5: Colombia ECP Heterogeneity Analysis - Robustness

	Interaction Variable:							
	Unemployed	Ind. Exp. Index	Income Decile	Ln Municip VA Per-Cap	Urbanization Rate	News Int Soc Media	Crime Rate	Public Goods Index
(1) Original Model	-0.02	-0.02	-0.004*	-0.01	-0.10**	-0.03*	0.00	-0.06**
	(0.02)	(0.01)	(0.002)	(0.03)	(0.04)	(0.02)	(0.02)	(0.03)
(2) Drop migrant share outliers	.	.	.	.	.	.	-0.01	-0.07*
	()	()	()	()	()	()	(0.04)	(0.04)
(3) Include return migrants in migrant share	-0.01	-0.01	-0.003*	-0.01	.	-0.02*	0.00	-0.05**
	(0.02)	(0.01)	(0.002)	(0.02)	()	(0.01)	(0.02)	(0.02)
(4) Drop Bogotá	-0.02	-0.01	-0.004**	-0.02	-0.07*	-0.03*	-0.00	-0.04*
	(0.02)	(0.01)	(0.002)	(0.03)	(0.04)	(0.01)	(0.02)	(0.03)
(5) ECP sampling weights applied	0.02	-0.01	-0.008**	-0.02	-0.10*	-0.02	0.00	-0.06
	(0.02)	(0.01)	(0.003)	(0.05)	(0.06)	(0.02)	(0.03)	(0.04)
(6) Include Departament FE	-0.03	0.01	-0.004*	-0.02	.	-0.03*	-0.02	-0.00
	(0.02)	(0.01)	(0.002)	(0.02)	()	(0.01)	(0.01)	(0.02)
(7) No municipality or individual controls	-0.01	-0.03**	-0.004*	-0.02	-0.08**	-0.03**	0.00	-0.05*
	(0.02)	(0.01)	(0.002)	(0.03)	(0.04)	(0.01)	(0.02)	(0.03)

The coefficients on the interaction term between migration rate and the column header are presented under various robustness checks. Output is supressed if the Kleibergen-Paap first-stage F-statistic falls below 15. All models control pre-period log population and full list of individual and municipality controls unless otherwise specified. Row 2 drops the 12 out of 117 municipalities with a migrant share greater than 2.5%. Row 3 counts any returned colombian migrant as part of the migrant share. Row 6 includes fixed effects for the 24 departments in Colombia. Standard errors clustered at the municipality level. * p<0.10, ** p<0.05, *** p<0.01.

Table B6: Colombia ECP Analysis - Other Individual-Level Outcomes

	Income Decile	Unemployed	News from Internet or Social Media	Voted in 2018 Pres. Election	Position on L-R Political Spectrum
			OLS		
Mig Share	-0.04*	-0.30	-0.01	0.64	0.01
	(0.02)	(0.39)	(0.49)	(0.67)	(0.02)
			2SLS		
Mig Share	-0.02	-0.70	-0.26	1.67**	0.04*
	(0.03)	(0.53)	(0.65)	(0.78)	(0.02)
Kleibergen-Paap F	139.55	139.55	135.66	135.66	158.08
N Obs	31,103	31,103	41,850	41,850	33,251
N Clusters	118	118	118	118	118
Variable Mean	5.26	11.18	32.48	76.29	0.00

All models control for full set of individual and municipality controls. Standard errors are clustered at the municipality level. Income decile within the municipality and political position range from 1-10, where 1 is left and 10 is right. Political position is standardized and missing when not reported or unknown. * p<0.10, ** p<0.05, *** p<0.01.

C Data Appendix

C.A Gallup Variables

Migrant Acceptance Index Questions: Now, I would like to ask you some questions about foreign immigrants - people who have come to live and work in this country from another country. Please tell me whether you, personally, think each of the following is a good thing or a bad thing? How about:

- Immigrants living in (country)
- An immigrant becoming your neighbor
- An immigrant marrying one of your close relatives

Migrant Acceptance Index: First principle component of these three questions, where "bad"==1, "depends/unsure/don't know"==2, and "good"==3.

Has Internet Access: Do you have access to the internet in any way, whether on a mobile phone, a computer, or some other device?

Ln Per-Capita Household Income: What is your total monthly household income in (country), before taxes? Please include income from wages and salaries, remittances from family members living elsewhere, farming, and all other sources. Brackets are used when the respondent doesn't report a value. This is converted into per capita annual income in international dollars.

Unemployed: Not employed, has been actively looking for work within the last four weeks, and say they would have been able to begin work in the last four weeks

Law & Order Index: First principle component of the following questions ("yes"==1, "no"==0):

- Within the last 12 months, have you had money or property stolen from you or another household member?
- Within the last 12 months, have you been assaulted or mugged?

Community Basics Index: First principle component of the following questions ("satisfied"==1, "dissatisfied"==0): In your city or area where you live, are you satisfied or dissatisfied with:

- ...the public transportation systems?
- ...the roads and highways?
- ...the quality of air?
- ...the quality of water?
- ...the availability of good affordable housing?
- ...the educational system or the schools?

- ...the availability of quality healthcare?

C.B Twitter Data - 2015, 2019

We collect all tweets from Twitter API that have been geo-coded, and that include the following list of keywords: 'venezolano', 'veneco', 'venezolanos', 'venecos', 'inmigrante', 'inmigrantes', 'extranjero','extranjeros', 'chamo', 'chamos'. 'Veneco' is a slang word to refer to the population from Venezuela, initially used in Colombia, but now used in several other Latin American countries. 'Chamo' means kid in Venezuelan slang, but has also been known to be used to refer to the Venezuelan population. We exclude tweets that refer to 'el extranjero', as it means 'outside the country' instead of the inteded meaning of 'foreigner'. We also drop tweets that are only talking about the Venezuelan government or regime, instead of the Venezuelan migrants. 'Tweets are collected across the following countries: Colombia, Argentina, Ecuador, Chile, Uruguay, Peru, and Costa Rica. We collect the data for 2015 and 2019. Once we identify tweets that can be go-located, we analyse the sentiment of said sample of tweets through the package PySentimiento Pérez et al. (2021), publicly available on Python. The library uses a neural network to classify text as positive or negative, and has been trained with a large sample of texts in Spanish. Once we collect the sentiment for each tweet, for the final analysis, we only consider regions in which we can geo-located at least 50 tweets across both years.

C.C Latinobarometro Variables - 2010, 2015, 2020

Responsibility to accept immigrants: What do you think is the impact of foreign nationals coming to live in (country)? Of the following phrases that I mention, please tell me whether you strongly disagree, disagree, neither agree nor disagree, agree, or strongly agree... "Rich countries have responsibility of accepting immigrants from poor countries"
Note - In 2020, this statement was worded differently, which is why we do not rely on this variable heavily in our analysis: "Our country should help immigrants that suffer political persecution in their countries."

C.D Colombia ECP - 2019

Trust: On a scale of 1-5, where 1 is none and 5 is a lot, how much do you trust [People with a different nationality]?

Segregation Preferences: For each of the following, who would you *not* like to have as a neighbor? [Foreigner]

News from Internet or Social Media: Do you follow national current events? If answer==yes: What sources do you follow? [Social media] or [Internet]

Network shares: How many people do you frequently visit in their home, or frequently visit you in your home? How many people have helped or tried to help you, or you helped or tried to help them, to get a job (separate from the ones listed above)? Of all of these people, how many are:

- Foreigners

- In your opinion have lost economic opportunity because of migrant workers

Migrant perceptions: Of 10 people that you find in the street, how many do you think will be foreigners?

Municipality-level variables:

- Log population (CEDE 2015)
- Log per-capita value added (DANE 2015)
- Urbanization rate (CEDE 2014-2016 average)
- Crimes per 100,000 residents (CEDE 2014-2016 average)
- Public goods index is the mean of the following standardized variables:
  - Saber11 test scores (CEDE 2014)
  - (Negative) Infant mortality rate (CEDE 2014-2016 average)
  - Trash collection coverage (CEDE 2014-2016 average)
  - Sewage system coverage (CEDE 2014-2016 average)
  - Electricity coverage (CEDE 2014-2016 average)

C.E WVS Variables - 2012 Colombia

Trust: Can you tell me, for each of the following groups, whether you trust the people of each group completely, somewhat, a little, or not at all: [People with a different nationality] (Made comparable to 2019 ECP using the following mapping: 1==not at all, 2==a little, 4==somewhat, 5==completely)

D Continuous Treatment and Difference-in-Difference

Model in 1 is equivalent to a difference-in-difference model where the treatment is continuous instead of discrete. This model can be estimated by Two-Way-Fixed-Effects (TWFE), which identifies the treatment effect of migration (β) in cases of homogeneous treatment effect across units. In this case, our estimation in Table IV identifies the effect of migration if there is no unobservables which are correlated with migration arrival, which we discuss in Section III.B.

There is a new literature discussing further identification requirements in cases of continuous treatment in which there are heterogeneous treatment effects – in particular, in cases in which treatment effects are correlated with the treatment observed (de Chaisemartin & D'Haultfœuille, 2018, de Chaisemartin et al. , 2019, 2022). We present 3 estimators from this literature below and in Table B7. All models average the response to Gallup across respondents by region and year. One approach is to consider a model with Fuzzy Difference-in-difference (FDID). In this setting, treatment assignment does not go from 0 to 1 as in standard DID settings but is higher in a treatment group than in a control, but both groups are treated. This setting allows us to consider differential trends for units at different levels of treatment – relaxing the strong parallel trend assumption. Furthermore, given that the treatment is continuous, but one needs to construct a counterfactual outcome based on units where treatment did not change, one can define a threshold under which treatment is considered to remain stable. Under these assumptions, one can estimate a type of local treatment effect for treatment group switchers. That is, one can identify the treatment effect of migration for those units that increase the level of migrants after the Venezuelan exodus. This is referred to as the 'Fuzzy DID estimator' (FDID) – presented in Columns 1 (with no country-level trends) and 2 (with country-level trends). The results are qualitatively similar to those in Table IV, finding that increments in the share of migrants did not worsen attitudes toward migrants. Columns 3 and 4 defined a different version of the treatment group. FDID estimates a type of Wald estimator which normalizes the change in outcome by the change in treatment intensity. Thus, in our case, we can also define the treatment group by our instrument. This is what Columns 3 and 4 do, where the treatment group is defined by having a positive share of Venezuelan migrants in the historic census. In this setting, once more, the estimator is close to our TWFE estimator, although somewhat noisier.

Panel B in B7 presents the Time-Corrected FDID, which adjusts the FDID estimator by allowing differential trends in the outcome by treatment level. This is estimated under the assumption that geographical areas with similar changes in migration would have followed similar trends. Thus, we assume that changes of up to 0.5% in the share of migrants are equivalent to not having changed treatment status. We consider three groups that allow for the common support to exist between treatment and control groups: no change migration, up to 1% share, up to 2%, or more than 2% increase. Once again, either if we consider treatment status as having increased the migration share (Columns 1 and 2) or having some historic migration (Columns 3 and 4), the estimated effect of migration is comparable to our main specification.

Lastly, de Chaisemartin et al. (2022) propose an estimand for settings in which treatment is continuous, but there is a group of 'stayers': some units that do not change treatment status. In this case, we can implement this by considering treatment to be the increment in migration between 2015 and 2019. Under a similar parallel trend assumption, the authors show that one can identify a Weighted Average of Switchers Slopes (WAOSS), which is the marginal treatment effect defined by the response to increases in treatment for switchers: units that change treatment assignment.[38] That is, here the treatment group increases the share of migrants, whereas the control has the same or lower level of migrants. We present the results from this exercise in Panel C of B7, when country-level trends are not considered (Column 1), and when they are (Column 2). Here again, we find that increases in the migration share does not negatively impact attitudes toward migrants.

Table B7: Effect of Migrant Share on Attitudes Toward Migrants - Fuzzy Difference-in-difference

	Migrant Acceptance Index (MAI)			
	(1)	(2)	(3)	(4)
Panel A: Fuzzy DID				
Migrant Share	-0.06	0.01	0.02	-0.02
	(0.08)	(0.08)	(0.09)	(0.10)
Panel B: TC-Fuzzy DID				
Migrant Share	0.13	0.11	-0.02	-0.01
	(0.10)	(0.09)	(0.08)	(0.08)
Panel C: DID with cont. dist. treatment				
Δ Migrant Share	-0.05	0.03		
	(0.06)	(0.09)		
N Obs	216	216	216	216
Country Time Trends		X		X

Migrant Acceptance Index (MAI): outcomes are measured at the regional level as the average response in Gallup 2016 and 2019. The sample is restricted to respondents who have been in the country for at least 5 years. Panels A and B present estimation using a Fuzzy DID method, following de Chaisemartin & D'Haultfœuille (2018), de Chaisemartin et al. (2019). Panel A presents what the authors call the *Fuzzy DID* estimator; Panel B presents the *Time-Corrected DID* estimator. The latter allows for differential trends for different groups defined by a discretized level of treatment (we use the following categories: 0 migration, up to 0.1 share, up to 0.2 share, and higher than 0.2). Panel C presents the DID with continuous distribution of treatment estimator by de Chaisemartin et al. (2022) where the treatment is defined as the difference in migration between 2015 and 2019 per region. Standard errors clustered at the region level are presented. Columns 1 and 2 define the treatment group as regions where the share of migrants increased, while Columns 3 and 4 define the treatment group as regions where the historical share of migrants was greater than 0.

[38] See details in de Chaisemartin et al. (2022) for how the WAOSS estimand relates to the FDID ones.