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ABSTRACT

More Benefits, Fewer Children: How Regularization Affects Immigrant Fertility*

How do policies that ease the integration of immigrants shape their fertility decisions? We use a panel survey of undocumented Venezuelan migrants in Colombia to compare the fertility decisions of households before and after the launch of an amnesty program that granted such migrants a labor permit and access to social services. Our results suggest the amnesty reduced the likelihood that program beneficiaries would have a child due to better labor market opportunities for women and greater access to family planning resources through health care services.

JEL Classification: F22, O15, R23

Keywords: migration, refugees, amnesties, Latin America

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I INTRODUCTION

Refugee migration has more than doubled in the last decade and will likely continue to rise as a result of climate change and conflict, among other factors. To address the needs of both migrants and host societies, we must learn more about the integration of refugees into their new communities and the role of policy in facilitating that process. Host governments are concerned about the fiscal burden imposed by refugees amid native perceptions of threats to national identity. But other consequences could be positive: migrants of young, working, and childbearing age may vitalize host countries that are currently confronting imploding birth rates and unsustainable social security systems.

This paper examines how a Colombian regularization program for Venezuelan migrants shaped their fertility decisions. A priori, the impact of such a program on immigrant fertility is an empirical question. On one hand, this type of policy should lower the cost of having children by providing access to health care and social programs (including contraception and educational services), both of which should lower the price of raising children. We call this the *income effect* (e.g., Bleakley and Lange, 2009; Qian, 2009; Becker et al., 2010). On the other hand, regularization enables migrants to access the formal labor market, which raises women's opportunity cost of childbearing and child-rearing. We call this the *substitution effect* (e.g., Mincer, 1963; DeFronzo, 1980; Falasco and Heer, 1985).

Previous studies have examined how immigration policies to facilitate integration affect immigrants' fertility choices in very different settings.[1] While informative, these studies have focused on European countries with policies that may not qualify as regularization programs (a common element in Latin America) and on groups who may not be forced migrants. As such, their findings cannot be easily extrapolated to migrants in the Global South, where contraception rates and access to health care services are limited, and fertility and economic vulnerability rates are higher than in developed countries. Addi-

[1] See Avitabile et al. (2014) for Germany, Lanari et al. (2020) for Italy, and Amuedo-Dorantes et al. (2022) for Spain.

tionally, certain aspects of forcibly displaced populations—including a disproportionate share of women and children whose access to health care was already precarious before migration—may produce diverse effects from those in the Global North.

We focus on the Permiso Especial de Permanencia (PEP), a regularization program that Colombia offered in 2018 to approximately half a million undocumented Venezuelan migrants there. PEP beneficiaries received work authorization and full access to social services for up to two years.[2] We examine how PEP impacted household fertility by leveraging information from two waves of the Venezuelan Refugee Panel Study (VenRePS), a representative survey of undocumented Venezuelan migrants who were living in main urban centers in Colombia before PEP. [3] Approximately half of the households in the survey were eligible for the PEP program.

Using panel data on 1,346 households, we compare the probability of having young children (conceived after the program launched) among households that were eligible and ineligible for PEP before and after the program began. Specifically, we observe each household at three points in time: at baseline, two, and three years after PEP's rollout. Our models include household-survey and wave fixed effects to account for unobserved time-varying factors that potentially shaped household fertility. In addition, they incorporate a rich set of municipality baseline covariates interacted with time trends to address non-parametric changes in city-wide characteristics affecting childbearing choices.

We find consistent and robust evidence that the PEP program decreased childbearing likelihood among migrants. Based on our main estimates, migrant households eligible for PEP were 3.9 percentage points (pp) less likely to have children less than one year old, 7 pp less likely to have one-year-olds, and 1.8 pp less likely to have two-year-olds. Falsification tests confirm the lack of changes in the probability of having children conceived

[2]PEP was followed in 2021 by the opportunity, via a separate program, to enjoy the same benefits for an additional 10 years.

[3]Bogotá, Medellín, Barranquilla, and a fourth group of smaller cities.

prior to the program's implementation. In addition, there is clear evidence of a program impact right after implementation that dissipates over time.

We also explore mechanisms behind the program's fertility impacts, paying close attention to two potential explanations. One concerns improved access to public services that might have lowered childbearing costs, or the *income effect*. Notably, improved access to health care might have cut the cost of contraception, which could reduce fertility. The second explanation involves access to more and better employment opportunities; this could have increased the opportunity cost of childbearing, or the *substitution effect*. We show that households eligible for PEP enjoyed more access to public services (particularly health care) *and* better job opportunities, suggesting both mechanisms may have contributed to the results.

Our findings pertain to the design of policies to ease the integration of migrants in the Global South, especially in countries concerned about implications for immigrant fertility. We show that in the case of Colombia, regularization did not produce increases in immigrant fertility. Instead, it reduced it for the reasons noted above.

Our study contributes to three strands of literature. First, it extends work on the effects of amnesties, regularizations, and various humanitarian programs on immigrants. For example, Ginn (2022) examines the impacts of refugee camps, Miguel et al. (2022) investigate shelter programs, and Ozler et al. (2021) and Altındağ and O'Connell (2022) assess the role of cash transfers in welfare measured through food consumption, child well-being, food security, and livelihood coping. Hussam et al. (2022) evaluate the mental-health value of job permits and Amuedo-Dorantes and Antman (2017); Amuedo-Dorantes and Bansak (2011); Amuedo-Dorantes and De La Rica (2007); COB (1995); Chassambouli and Peri (2015); Devillanova (2017); Kaushal (2006); Monras (2018); Fallah et al. (2019); Bahar et al. (2021) assess the effect of amnesties on native labor outcomes in developed

countries.[4] The studies most relevant to our research are those by Ibanez et al. (2022) and Urbina Florez et al. (2023), which document PEP's positive impacts on Venezuelan migrants' consumption and labor income.[5]

Secondly, we add to a vast literature examining how policy shapes fertility (e.g., Lalive and Zweimüller, 2009; Milligan, 2005; Bailey, 2012). We focus on how immigration policy influences immigrant fertility. Low fertility rates and longer life spans in developed and developing countries have sparked government interest in understanding the potential role of immigration policy to bolster public pension systems. Immigration could alleviate the fiscal pressure caused by an increasing number of retirees and could support these programs through the growth of a workforce with higher fertility rates than those of natives (e.g., Storesletten, 2000). While this impact might be limited in nations with relatively low immigration and very low fertility rates (e.g., South Korea), it could be relevant for others such as Colombia.

Finally, our study contributes to a broader literature on immigrant integration(e.g.,Abramitzky et al., 2012, 2014; Pérez, 2021). Given declining global fertility trends and increased forced migration, it is vital to study how policy can shape immigrant integration into host societies. The higher fertility rates of immigrants compared to natives are controversial. This is particularly true given large migrant inflows over a short time span, as they can constrain the host country's health care system and elicit opposition from natives.

II INSTITUTIONAL CONTEXT: THE PEP REGULARIZATION PROGRAM

Colombia is the main recipient of Venezuelan migrants. According to data from the United Nations Refugee Agency, approximately 2.5 million Venezuelan migrants had arrived in Colombia by February 2022, with the vast majority arriving since 2016. This number does not include undocumented migrants who escaped detection by authorities.

[4] A related literature studies effects of migrant amnesties on crime in host communities. See Baker (2015) for the United States, Mastrobuoni and Pinotti (2015) for the European Union, and Pinotti (2017) for Italy.

[5] Other papers have also studied PEP's impacts on labor outcomes (Bahar et al., 2021), political outcomes (Rozo et al., 2023), firm outcomes (Bahar et al., 2022), and inequality (Lombardo et al., 2021).

This section describes the timeline of the PEP rollout with a detailed illustration of the exact dates and sequence of events in Figure 1.

II. A Registry of Irregular Migrants —- January–April 2018

In 2018, the Colombian government conducted a survey to estimate the number of irregular Venezuelan migrants living in Colombia. The survey, known as the *Registro Administrativo de Migrantes Venezolanos* or RAMV, was collected between January and April of 2018 in 441 municipalities with the largest populations of Venezuelan migrants.[6] The registry was voluntary and largely advertised through local migrant organizations and the media. Roughly half a million migrants had registered by the time it ended.

II. B The PEP program —- August–December 2018

In July 2018, just prior to leaving office, then-President Juan Manuel Santos unexpectedly announced that all migrants who had registered in the RAMV would be eligible for regularization through a program called the Permiso Especial de Permanencia (PEP). PEP offered a generous agenda of a two-year residency permit, a work permit, and access to SISBEN (a scoring program to award public resources) and financial services. By granting migrants access to SISBEN, PEP arguably enabled them to apply to all Colombian social programs for vulnerable populations, including full health care services through the subsidized regime. PEP boosted the consumption and labor income of treated migrants (Ibanez et al., 2022) and had negligible effects on the labor prospects of Colombian native workers in the short term (Bahar et al., 2021). We hypothesize that by giving Venezuelan migrants access to social programs and the formal labor market, PEP might have also impacted other household decisions, including fertility choices.

III THEORETICAL FRAMEWORK

In the standard Beckerian framework, where demand for children depends on a family's budget constraint (Becker, 1960), PEP should have effectively reduced the cost of having

[6]There are 1,122 Colombian municipalities.

children for eligible Venezuelan migrants. The lower per-unit cost of childbearing in these households would stem from better access to medical, educational, and childcare services after regularization, as well as from potentially higher wages. If we abstract from the opportunity cost of time (e.g., Hotz et al., 1997), the *income effect* would favor increases in fertility as long as children are considered normal goods (e.g., Becker, 1960; Black et al., 2013, Cohen et al., 2013).[7]

Nevertheless, PEP also provided work permits, which raised the opportunity cost of childbearing—the *substitution effect*. If we account for time-allocation decisions (e.g., Willis, 1973), PEP's impact on the fertility of eligible migrants becomes uncertain. Higher wages due to regularization could raise the opportunity cost of having children, inducing migrant mothers to increase their labor supply and curtail their fertility (Hotz and Miller, 1988; Heckman and Walker, 1990). Hence, PEP's effect on fertility ultimately depends on the relative size of the income and substitution effects.

The ambiguity surrounding PEP's implications for fertility is also present when using modified versions of the Becker and Lewis (1973) model, which underscores the trade-off between child quality and quantity. In that framework, parents maximize a utility function that depends on the consumption of goods and services, the number of children, and child quality subject to a budget constraint abstract from time considerations. Relying on that model, Avitabile et al. (2014) and Lanari et al. (2020), among others, demonstrate a trade-off between quantity and quality. Specifically, for two different immigration policies—one benefiting immigrants' offspring (the new German citizenship law) and one benefiting unauthorized immigrants (the Italian amnesty)—the authors document declines in immigrant fertility that they attribute to drops in the price of child quality. Yet, impacts remain heterogeneous. Lanari et al. (2020) show how the lower price of child quality incentivized childless women to have a baby given the lower per-unit

[7] As mentioned in the introduction, increased access to health care services could also reduce the cost of contraception and thus lower fertility rates.

cost of childbearing, even though it decreased the overall number of children that eligible women would have.

The next sections explore how PEP shaped fertility among Venezuelan migrants and suggest possible mechanisms for the observed responses.

IV DATA: VENREPS

Our main source of data is the Venezuelan Refugee Panel Study (VenRePS), a longitudinal study of irregular Venezuelan migrants in Colombia. The survey was conducted to examine PEP's impacts on migrant well-being and consisted of two waves of data collection, starting in October 2020 and one year later. The data represents four geographical areas: Bogotá, Medellín, Barranquilla, and a group of smaller cities that together comprise an area.[8] The first three cities are large urban centers in Colombia that host the most Venezuelan migrants in the country. In Figure 2, the location of each city in the VenRePS sample is compared with the location of Venezuelan migrants in Colombia based on the 2018 population census (the last one available).

Roughly half of the individuals interviewed in VenRePS were randomly selected from the RAMV survey. The other half originated from a "snowball" sample of referrals from local migrant organizations and respondents in the RAMV sample. Ibanez et al. (2022) show that migrants surveyed in VenRePS who were contacted through the RAMV survey or "snowball" referrals were comparable in terms of sociodemographic characteristics before the program's rollout. All migrants in the survey had no passport, were at least 18 years old, provided documents to prove they were born in Venezuela, and had arrived in Colombia between January 2017 and December 2018. In other words, they were irregular migrants living in Colombia at the time of PEP's implementation.

Table A.1 presents summary statistics distinguishing by gender. Panel A shows descrip-

[8]This includes migrants interviewed in ten municipalities including Cúcuta, Villa del Rosario, Cali, Cartagena, Riohacha, Maicao, Uribia, Valledupar, Santa Marta, and Arauca.

tive statistics for men and panel B for women. Three main patterns are worth noticing. First, migrants registered in the RAMV census (which made them eligible for PEP) were older, more educated, had been in Colombia longer, and enjoyed better access to public services before migrating, compared to their counterparts who were not registered in the RAMV census and therefore ineligible for PEP. Second, migrant women surveyed in VenRePS were generally younger, had more children, and were more educated than their male counterparts. Third, migrants in the survey had at least the same education as Colombian natives, and those registered in the RAMV census were more educated than Colombian natives. In addition, these migrants were generally younger than natives.

V EMPIRICAL STRATEGY

The fertility implications of regularization cannot be assessed by simply comparing households that were eligible for PEP to households that were not. As illustrated in Table A.1, the two sets of households differ in observable and unobservable characteristics potentially correlated to their fertility outcomes. For instance, migrants who were eligible for PEP were more educated than other migrants and natives. In addition, they might have differed with regard to unobservable traits. For example, migrants who were eligible for PEP could have been better-informed or more ambitious than their ineligible counterparts. Those differences could also explain gaps in fertility rates between the two groups.

To address this challenge, we leverage longitudinal data from VenRePS and estimate the fertility response to being eligible for PEP by comparing changes in fertility rates within the same household before and after the program was implemented. We observe household fertility rates at three points in time: at baseline on the day before the RAMV census (April 5, 2018) and post-treatment in two waves of VenRePS (2020 and 2021). Hence, we stack the data to evaluate the impacts of being eligible for PEP on the probability of having children of T years of age. Specifically, we estimate the following equation:

$$Child^{T}_{jdgt} = \beta_0 + \beta_1 I[PEP_{jgd} = 1] \times Post_t + \sum_{x \in X_{jdg}} \phi_x(x \times \gamma_t) + \phi_{d \times t} + \psi_{g \times t} + \alpha_t + \alpha_j + \epsilon_{jgdt} \quad (1)$$

where j stands for household, d for department, g for geographical sampling region, and t for the timing in which outcomes are observed (t=0,1,2 for baseline and the two waves of data collection). $Child^{T}_{jdgt}$ is the likelihood that household j has a child T years old (T = 0,1,2,3). $I[PEP_{jgd} = 1]$ is a dichotomous variable equal to one for households that applied for the PEP program, and $Post_t$ is a dummy equal to one after the program's rollout. $\sum_{x \in X_{jgd}} \phi_x(x \times \gamma_t)$ is a term that captures non-parametric temporal changes in a comprehensive list of pre-migration household traits, including: (i) household head traits (gender, age, and education); (ii) household head's labor history in Venezuela before migrating (probability of being employed, type of job, probability of having a written contract, and the time gap between the last job and the migration episode); (iii) household characteristics (number of children, household size, access to public services, owning dwelling, and having a smartphone); and (iv) networks prior to migration (had family and friends in Colombia, knew of job opportunities before migrating, and migrated for health-related reasons). Descriptive statistics for all control variables and outcomes used in the main specification are in Table 1. The analysis only includes individuals observed at the three points in time noted above. In the robustness section, we conduct a sensitivity analysis to gauge the extent of attrition in our sample and demonstrate that our main findings remain unchanged.

The model also includes fixed effects for each data wave (α_t) and each household (α_j) as well as department-wave trends ($\phi_{d \times t}$) for each of the five departments where the survey was collected and geographic-sampling wave trends ($\psi_{g \times t}$) for all regions in the survey. Finally, standard errors are clustered at the household level to account for intra-household serial correlation.

By including household fixed effects, we effectively purge from our estimates time-invariant differences between treated and non-treated groups that could confound PEP's fertility effects. In addition, by flexibly accounting for non-parametric temporal changes in a rich set of pre-migration household characteristics, we address dynamic differences between eligible and ineligible migrants. As such, β_1 measures fertility changes among treated migrant households relative to non-treated migrant households, from before to after PEP's rollout.[9] Specifically, we gauge the impact of regularization on the probability of having children less than one, one, two, or three years old in 2020 and 2021. Since the amnesty was announced in July 2018 and registration did not open until one month later, changes in fertility behaviors induced by the policy would only be observed during or after 2019. In 2020 and 2021, we should be able to observe changes in the likelihood of having children less than one, one, and two years old. However, we should not be able to observe changes in the likelihood of having children three years old. We will consider the likelihood of such an event to be a falsification test.

VI PEP'S FERTILITY IMPACTS

Table 2 illustrates the results of estimating equation (1) in three panels. Panel A shows results using the data from baseline and 2020 (the first wave of VenRePS). Panel B presents results using the data from baseline and 2021 (the second wave of VenRePS). Finally, panel C shows results stacking the three periods of data: (i) baseline data from before PEP, which relies on recall questions; (ii) the first survey wave (2020); and (iii) the second survey wave (2021). Each column corresponds to a different regression evaluating the effects of PEP eligibility on the probability of having children less than one year old (column 1), one year old (column 2), two years old (column 3), and three years old (column 4).

We find consistent evidence that PEP eligibility lowered the probability of having children in all panels. Our preferred results are those in panel C, as they include all data waves.

[9] Since PEP take-up rates were close to 94 percent, the derived Intent-to-Treat (ITT) estimates should not be very different from the Average Treatment Effects (ATE).

Based on those estimates, migrant households eligible for PEP were 3.9 pp less likely to have children less than one year old, 7 pp less likely to have one-year-olds, and 1.8 pp less likely to have two-year-olds. As expected, PEP eligibility had no significant impact on the likelihood of having three-year-olds given the program's implementation timing. In addition, the results are robust to the exclusion of control variables.[10]

When we restrict our sample to data collected at baseline and in 2020 (panel A), we only observe a policy impact on the probability of having children one year old or less, which aligns with the program's rollout. For that reason, in panel A, we observe policy impacts that are not statistically different from zero for the likelihood of having children two and three years old. As we add the 2021 data in panel B, we observe a policy impact on the probability of having children less than one year old, one year old, and two years old.

The results in panels A and B suggest that PEP's fertility impacts were not only immediate but also grew larger one year after the program's rollout, reflecting the usual delay in benefiting from regularization. For example, access to social services requires having PEP plus a SISBEN vulnerability score, which can take time to obtain from public authorities. Likewise, it can be time consuming to find a formal job, which explains the program's larger impact one year after implementation.

In sum, our main findings align with the timing of the program's rollout and robustly support our hypothesis that PEP reduced household fertility.

VI. A Robustness Tests

We conduct a series of sensitivity checks to gauge the extent of attrition in our sample and assess the robustness of our findings to various sample changes.

Attrition Concerns

Since we exploit the panel nature of the survey data for our analysis, a natural concern is the extent to which attrition may bias our findings. We conduct several robustness checks

[10] Results are available upon request.

to address this concern. First, we characterize the attrited sample by running a regression where the dependent variable equals one if the household did not respond to the second survey wave on all the covariates characterizing migrants before the program's rollout. As shown in Table B.1 in Appendix B, five of the 22 covariates appear to be correlated at a statistically signficant level, including having a partner in Venezuela, years of education before migration, gender, age, and length of residence in Colombia. Athough the estimated coefficients are small, they suggest that attrited individuals were more vulnerable and less rooted in Colombia.

Secondly, in Table B.2, we estimate PEP's effects on the fertility rates of individuals who were no longer in the sample by the second wave. Although we do not have data for these respondents in the second wave, we have their responses in the first wave. In line with our main results, we find that when they were interviewed in 2020, PEP reduced the probability of their having children zero years of age.

Finally, we examine whether attrition rates in the second survey wave are correlated with our outcomes of interest during the first survey wave. As illustrated in Table B.3, they are not. This implies that those individuals not in the second wave were neither more nor less likely to have a child less than one year old, one year old, or two years old before they dropped out of the survey.

Excluding households along the Colombian-Venezuelan border

We also experiment with excluding from the sample individuals along the Colombian-Venezuelan border to avoid including Venezuelan residents who only visited Colombia for health care purposes. Thus, we exclude individuals residing in Colombian departments that border Venezuela and we re-estimate our models. Results from this exercise are in Table C.1. We continue to find evidence of fertility declines as captured by a similarly sized reduction in the likelihood of having a child less than one year old or one year old as in Table 2, thereby supporting our main conclusions.

Restricting the sample to household heads and their partners

Finally, we experiment with restricting our sample to household heads and their partners since they were the main survey respondents. It could be that the information gathered on other household members was subject to more measurement error. Table C.2 shows the results using this smaller sample. We continue to find evidence that PEP decreased fertility rates as captured by a significantly smaller reduction in the likelihood of having a child less than one year old and a similarly sized decline in the probability of having a one-year-old.

In sum, the robustness checks included in Tables B.1 through C.2 support our main findings and the theory that PEP lessened migrant fertility. The findings do not appear to be affected by attrition biases, the inclusion of regularly commuting migrants, or measurement biases related to information gathered from household members who were not the main survey participants. Next, we explore some likely mechanisms.

VII WHAT EXPLAINS THE DROP IN FERTILITY?

As noted in the conceptual framework, PEP might have curtailed migrant fertility through two main channels. Notably, the ability to work in the formal labor market might have increased the opportunity cost of childbearing and led to fertility reductions. In addition, through access to public health care services and other government assistance, PEP might have lowered fertility by giving migrant women access to contraception, but it mainly eased the price of child quality, inducing a quantity-quality trade-off that diminished migrant fertility.

To gauge the validity of these mechanisms, we re-estimate equation (1), changing the dependent variable. Instead of estimating the probability of having a child in a particular age range, we estimate the likelihood of access to governmental services, including health care services and financial assistance, as well as the probability of being employed and having a formal job. Specifically, the new outcome variables are: (i) having a SISBEN

score, (ii) being enrolled in the subsidized health care regime, (iii) being a beneficiary of public cash transfers, (iv) being employed, and (v) having a formal job. The first three outcomes are measured at the household level and labor market outcomes are measured at the individual level. Results are in Tables 3 and 4, respectively. All outcomes are observed before and after the program's rollout.

As shown in Table 3, PEP improved migrants' access to public assistance. In particular, eligible households were 49.2 pp more likely to have a SISBEN score, 11.4 pp more likely to have access to the subsidized health care regime, and 33 pp more likely to receive government transfers than ineligible households. In sum, PEP-eligible households enjoyed greater access to health and safety nets than their ineligible counterparts, lowering the price of child quality, which could induce a quantity-quality trade-off.

In addition, PEP-eligible migrants enjoyed better labor market opportunities than ineligible migrants, as shown in Table 4. They were approximately 7 pp more likely to have a formal job than ineligible migrants, even though only women appeared more likely to be employed. This suggests that most male migrants might have already worked in the informal market before PEP.

Results in Tables 3 and 4 support the notion that women who were eligible for PEP reduced their childbearing in response to improved access to public health care services and goverment aid, which lowered the price of child quality, likely inducing a quantity-quality trade-off (Becker and Lewis (1973); Avitabile et al. (2014); Lanari et al. (2020)). In addition, access to better labor market options may have raised the opportunity cost of childbearing (Willis (1973); Hotz and Miller (1988); Heckman and Walker (1990)), further constraining their fertility.

VIII CONCLUDING REMARKS

This paper examines the impacts of Colombia's massive 2018 regularization program on the fertility of Venezuelan migrants. Our results largely suggest that the amnesty caused

a significant drop in the likelihood of childbearing, an impact observed immediately after the program's implementation. The effects, which strengthened one year after the rollout, might have partially been driven by improved access to labor market opportunities and public services. The former raised the opportunity cost of childbearing and the latter lowered the price of child quality, inducing a quantity-quality trade-off.

These findings have profound implications for public policy due to increased forced migration worldwide and the reticence of host countries to facilitate these flows for several reasons, including the fear that natives view them as a threat to national identity. These concerns are particularly acute when incoming migrant groups have higher fertility rates than natives. Our analysis illustrates how regularization programs can appease such concerns. By facilitating access to labor market opportunities and public assistance—including educational services, health care, and financial aid—regularization programs may hasten the convergence of migrant fertility to that of natives while simultaneously promoting their integration and social contributions.

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Figure 1. PEP Program Rollout

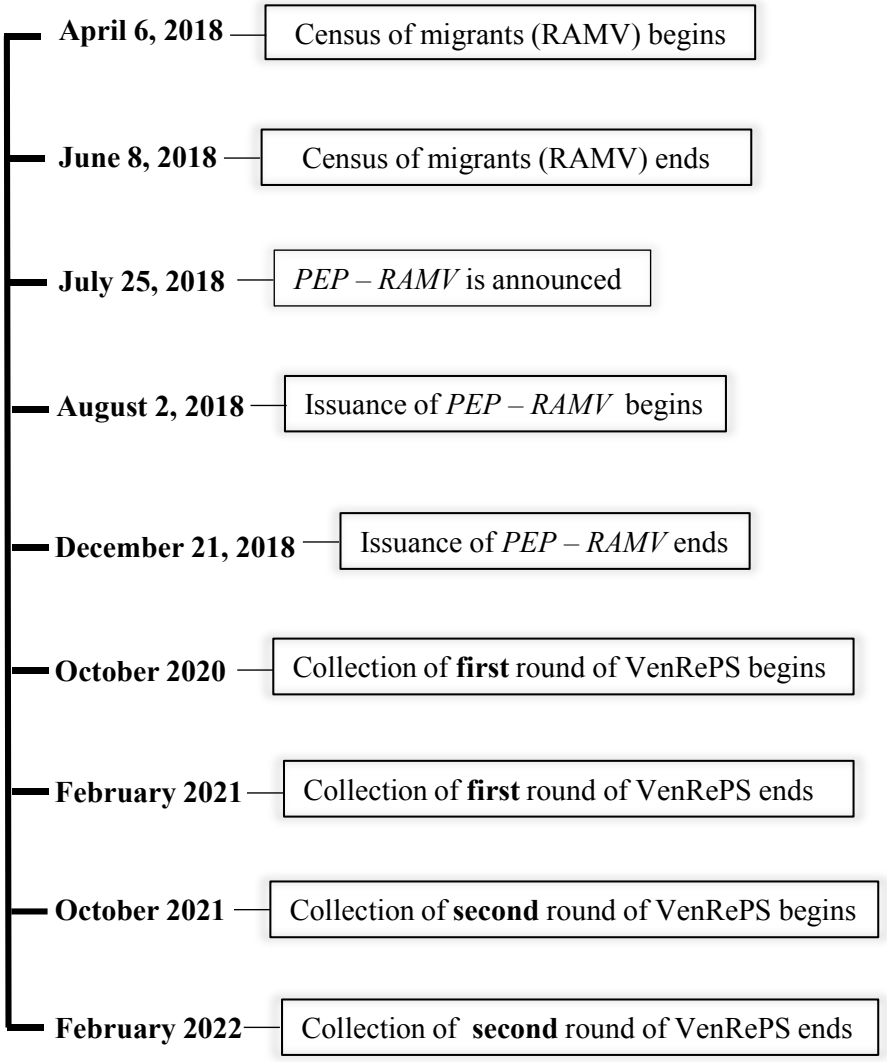
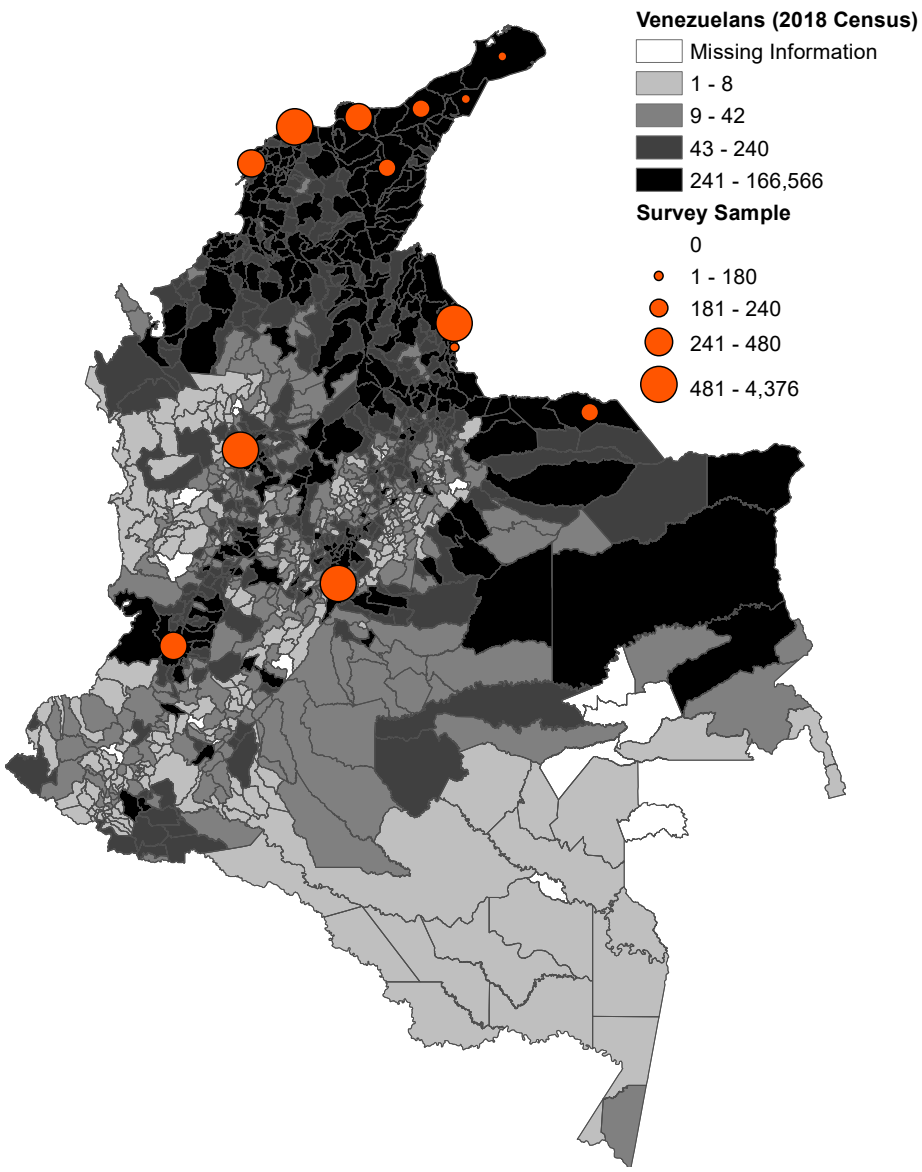


Figure 2. Share of Venezuelan Migrants and VenRePS Sample



Notes: The figure presents the share of Venezuelan migrants by 2018 and the sample of individuals surveyed in VenRePS 2020.

Table 1. Descriptive Statistics

	PEP Ineligible			PEP Eligible		
Panel A: Control Variables (baseline)	N	Mean	SD	N	Mean	SD
Age (years)	596	32.50	8.517	750	35.79	9.349
Number of children	596	1.661	1.426	750	1.479	1.508
Household Venezuela: parents or siblings [=1]	596	0.465	0.499	750	0.424	0.495
Household Venezuela: partner/spouse [=1]	596	0.539	0.499	750	0.564	0.496
Household Venezuela: others [=1]	596	0.129	0.336	750	0.0853	0.280
Knew of job opportunity before migrating [=1]	596	0.354	0.479	750	0.341	0.474
Ever worked [=1]	596	0.971	0.167	750	0.980	0.140
Employed at private firm [=1]	596	0.602	0.490	750	0.612	0.488
Employed with Government [=1]	596	0.148	0.355	750	0.153	0.361
Self-employed or employer [=1]	596	0.174	0.380	750	0.180	0.384
Written contract [=1]	596	0.451	0.498	750	0.563	0.496
Gap between last job and migration (months)	596	0.876	3.710	750	1.311	5.038
Years of education before migration	596	12.95	2.923	750	13.55	2.696
Migrated for health reasons	596	0.102	0.303	750	0.101	0.302
Friends/family in Colombia	596	0.773	0.419	750	0.700	0.459
Time in Colombia (months)	584	49.53	7.984	736	56.09	11.59
Had smartphone [=1]	596	0.492	0.500	750	0.648	0.478
Owner of dwelling in Venezuela [=1]	596	0.866	0.341	750	0.864	0.343
Electricity in Venezuela [=1]	596	0.995	0.0708	750	0.993	0.0814
Running water in Venezuela [=1]	596	0.837	0.369	750	0.875	0.331
Sewage in Venezuela [=1]	596	0.940	0.238	750	0.931	0.254
Panel B: Outcomes (All waves)						
Likelihood of having children of 0 years of age	2,538	0.0402	0.196	1,500	0.0447	0.207
Likelihood of having children of 1 years of age	2,538	0.0587	0.235	1,500	0.0447	0.207
Likelihood of having children of 2 years of age	2,538	0.0248	0.156	1,500	0.0200	0.140
Likelihood of having children of 3 years of age	2,538	0.00158	0.0397	1,500	0.000667	0.0258

Notes: The table presents descriptive statistics for the households in our sample (596 ineligibles and 750 eligibles = 1,346 households). Panels A and B show the head of household's characteristics measure before the migration episode and the main outcome measures for all waves, respectively.

Table 2. Effects of the PEP Program on Fertility Decisions

	Dependent Variable: Likelihood of having children of			
	0 years of age	1 year of age	2 years of age	3 years of age
	(1)	(2)	(3)	(4)
Panel A: Estimates with baseline and wave I				
PEP [=1]	-0.072***	-0.057***	0.007	-0.000
	(0.017)	(0.016)	(0.005)	(0.003)
Observations	2,640	2,640	2,640	2,640
Panel B: Estimates with baseline and wave II				
PEP [=1]	-0.006	-0.084***	-0.043***	0.001
	(0.013)	(0.018)	(0.016)	(0.003)
Observations	2,640	2,640	2,640	2,640
Panel C: Estimates with baseline, wave I and II				
PEP [=1]	-0.039***	-0.070***	-0.018*	0.001
	(0.010)	(0.012)	(0.009)	(0.003)
Observations	3,960	3,960	3,960	3,960
Controls in all panels				
Wave FE	Yes	Yes	Yes	Yes
HH FE	Yes	Yes	Yes	Yes
Department ×wave	Yes	Yes	Yes	Yes
Geographic sampling ×wave	Yes	Yes	Yes	Yes
Pre-migration controls ×wave	Yes	Yes	Yes	Yes

Notes: The table presents the estimates of the specification described in equation (1). Panel A presents results using data from the baseline and wave I, panel B shows results using data from the baseline and wave II, and panel C presents results stacking all the data together (baseline, wave I, and wave II). Department corresponds to the five departments in which the sample was collected and geographic sampling corresponds to the four geographic levels at which the sample is representative, including three main cities and a fourth group that accounts for nine smaller urban centers with prevalent migration from Venezuela. Pre-migration control variables include: (i) individual controls for the head of household (gender, age, and education); (ii) labor history for the head of household (probability of being employed, type of job, probability of having a written contract, and the time gap between the last job and the migration episode); (iii) household characteristics (number of children, household size, access to public services, owning dwelling, and having a smartphone); and (iv) networks prior to migration episode (had family and friends in Colombia, knew of job opportunities before migrating, and migrated for health-related reasons). Standard errors clustered at the household level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3. Effects of the PEP Program on Access to Government Programs

Dep Variable:	SISBEN [=1]	Subsidized health care [=1]	Transfers [=1]
	(1)	(2)	(3)
PEP [=1]	0.492***	0.114***	0.330***
	(0.021)	(0.016)	(0.020)
Observations	3,873	3,959	3,903
Wave FE	Yes	Yes	Yes
HH FE	Yes	Yes	Yes
Department ×wave	Yes	Yes	Yes
Geographic Sampling ×wave	Yes	Yes	Yes
Pre-migration controls ×wave	Yes	Yes	Yes

Notes: The table presents the estimates of the specification described in equation (1) using variables on access to government programs as main outcomes. Department corresponds to the five departments in which the sample was collected and geographic sampling corresponds to the four geographic levels at which the sample is representative, including three main cities and a fourth group that accounts for nine smaller urban centers with prevalent migration from Venezuela. Pre-migration control variables include: (i) individual controls for the head of household (gender, age, and education); (ii) labor history for the head of household (probability of being employed, type of job, probability of having a written contract, and the time gap between the last job and the migration episode); (iii) household characteristics (number of children, household size, access to public services, owning dwelling, and having a smartphone); and (iv) networks prior to migration episode (had family and friends in Colombia, knew of job opportunities before migrating, and migrated for health-related reasons). Standard errors clustered at the household level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4. Effects of the PEP Program on Labor Market Access

Dep Variable:	Employed [=1]	Formal Job [=1]
	(1)	(2)
Panel A: All sample		
PEP [=1]	0.032	0.075***
	(0.011)	(0.037)
Observations	6,339	4,104
Panel B: Women		
PEP [=1]	0.061*	0.066***
	(0.026)	(0.017)
Observations	3,591	1,437
Wave FE	Yes	Yes
HH FE	Yes	Yes
Department ×wave	Yes	Yes
Geographic Sampling ×wave	Yes	Yes
Pre-migration controls ×wave	Yes	Yes

Notes: The table presents the estimates of the specification described in equation (1) using variables on labor market access as main outcomes. Panel A presents results for the whole sample and panel B for women only. Department corresponds to the five departments in which the sample was collected and geographic sampling corresponds to the four geographic levels at which the sample is representative, including three main cities and a fourth group that accounts for nine smaller urban centers with prevalent migration from Venezuela. Pre-migration control variables include: (i) individual controls for the head of household (gender, age, and education); (ii) labor history for the head of household (probability of being employed, type of job, probability of having a written contract, and the time gap between the last job and the migration episode); (iii) household characteristics (number of children, household size, access to public services, owning dwelling, and having a smartphone); and (iv) networks prior to migration episode (had family and friends in Colombia, knew of job opportunities before migrating, and migrated for health-related reasons). Standard errors clustered at the household level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendix A: Descriptive Statistics by Gender

Table A.1. Descriptive Statistics by Gender

	PEP Ineligible			PEP Eligible			Colombians		
	N	Mean	SD	N	Mean	SD	N	Mean	SD
Panel A: Men									
Age (years)	320	32.88	8.167	472	35.90	9.155	275	37.53	10.53
Number of children	320	1.512	1.351	472	1.386	1.484	275	1.462	1.330
Household Venezuela: parents or siblings [=1]	320	0.409	0.492	472	0.400	0.491	275	0.455	0.499
Household Venezuela: partner/spouse [=1]	320	0.691	0.463	472	0.689	0.464	275	0.578	0.495
Household Venezuela: others [=1]	320	0.125	0.331	472	0.0784	0.269	275	0.102	0.303
Knew of job opportunity before migrating [=1]	320	0.381	0.486	472	0.367	0.482	275	0.407	0.492
Ever worked [=1]	320	0.994	0.0789	472	0.992	0.0918	275	0.949	0.220
Employed at private firm [=1]	320	0.634	0.482	472	0.638	0.481	275	0.596	0.492
Employed with Government [=1]	320	0.144	0.351	472	0.163	0.370	275	0.102	0.303
Self-employed or employer [=1]	320	0.194	0.396	472	0.178	0.383	275	0.215	0.411
Written contract [=1]	320	0.500	0.501	472	0.585	0.493	275	0.338	0.474
Gap between last job and migration (months)	320	0.895	3.822	472	1.373	5.080	275	0.615	2.672
Years of education before migration	320	13.01	2.945	472	13.57	2.661	271	13.01	3.060
Migrated for health reasons	320	0.113	0.316	472	0.0826	0.276	275	0.142	0.349
Friends/family in Colombia	320	0.781	0.414	472	0.706	0.456	275	0.724	0.448
Time in Colombia (months)	310	49.96	8.856	462	56.51	12.33	173	62.11	17.36
Had smartphone [=1]	320	0.472	0.500	472	0.644	0.479	275	0.596	0.492
Owner of dwelling in Venezuela [=1]	320	0.869	0.338	472	0.881	0.324	275	0.822	0.383
Electricity in Venezuela [=1]	320	1	0	472	0.989	0.102	275	0.996	0.0603
Running water in Venezuela [=1]	320	0.813	0.391	472	0.892	0.311	275	0.847	0.360
Sewage in Venezuela [=1]	320	0.928	0.259	472	0.934	0.248	275	0.931	0.254
Panel B: Women									
Age (years)	296	29.88	7.712	360	33.08	8.574	136	35.98	10.35
Number of children	296	1.581	1.343	360	1.542	1.470	136	1.324	1.376
Household Venezuela: parents or siblings [=1]	296	0.399	0.490	360	0.347	0.477	136	0.338	0.475
Household Venezuela: partner/spouse [=1]	296	0.726	0.447	360	0.794	0.405	136	0.647	0.480
Household Venezuela: others [=1]	296	0.135	0.342	360	0.0750	0.264	136	0.118	0.323
Knew of job opportunity before migrating [=1]	296	0.385	0.487	360	0.392	0.489	136	0.463	0.500
Ever worked [=1]	296	0.993	0.0821	360	0.994	0.0744	136	0.993	0.0857
Employed at private firm [=1]	296	0.568	0.496	360	0.653	0.477	136	0.574	0.496
Employed with Government [=1]	296	0.172	0.378	360	0.156	0.363	136	0.140	0.348
Self-employed or employer [=1]	296	0.189	0.392	360	0.147	0.355	136	0.199	0.400
Written contract [=1]	296	0.361	0.481	360	0.439	0.497	136	0.287	0.454
Gap between last job and migration (months)	296	0.448	2.054	360	1.014	4.673	135	1.659	5.800
Years of education before migration	296	13.04	2.921	360	13.72	2.540	136	12.37	3.557
Migrated for health reasons	296	0.105	0.307	360	0.0778	0.268	136	0.154	0.363
Friends/family in Colombia	296	0.791	0.408	360	0.692	0.462	136	0.713	0.454
Time in Colombia (months)	291	46.89	7.640	357	51.11	12.10	92	56.39	13.41
Had smartphone [=1]	296	0.449	0.498	360	0.608	0.489	136	0.610	0.489
Owner of dwelling in Venezuela [=1]	296	0.878	0.327	360	0.881	0.325	136	0.801	0.400
Electricity in Venezuela [=1]	296	1	0	360	0.989	0.105	136	0.993	0.0857
Running water in Venezuela [=1]	296	0.804	0.398	360	0.883	0.321	136	0.897	0.305
Sewage in Venezuela [=1]	296	0.922	0.268	360	0.936	0.245	136	0.941	0.236

Notes: The table presents descriptive statistics for PEP-eligible individuals, ineligible individuals, and Colombian citizens. All variables for migrants correspond to the retrospective measure before the migration episode. Panel A shows statistics for male heads of household and panel B for female partners.

Appendix B: Characterizing Attrition

Table B.1. Determinants of Attrition

	(1) Attrited HH [=1]
Household Venezuela: parents or siblings [=1]	-0.033
	(0.024)
Household Venezuela: partner/spouse [=1]	-0.068***
	(0.025)
Household Venezuela: others [=1]	0.003
	(0.034)
Knew of job opportunity before migrating [=1]	-0.022
	(0.022)
Ever worked [=1]	-0.004
	(0.082)
Employed at private firm [=1]	-0.042
	(0.054)
Employed with Government [=1]	-0.050
	(0.060)
Self-employed or employer [=1]	-0.048
	(0.057)
Written contract [=1]	0.005
	(0.025)
Gap between last job and migration (months)	-0.002
	(0.002)
Years of education before migration	-0.010***
	(0.004)
Migrated for health reasons	0.038
	(0.034)
Friends/family in Colombia	-0.037
	(0.023)
Had smartphone [=1]	0.007
	(0.021)
Owner of dwelling in Venezuela [=1]	0.003
	(0.031)
Electricity in Venezuela [=1]	-0.077
	(0.129)
Running water in Venezuela [=1]	0.046
	(0.032)
Sewage in Venezuela [=1]	-0.022
	(0.045)
Female [=1]	-0.051**
	(0.023)
Age (years)	-0.004***
	(0.001)
Number of children	0.000
	(0.008)
Time in Colombia (months)	-0.002**
	(0.001)
Observations	2,200

Notes: The table presents the correlation between pre-migration control variables and the likelihood of attrition at the head-of-household level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.2. Results using Sample of Attrited Individuals

	Likelihood of having children of			
	(1)	(2)	(3)	(4)
	0 years of age	1 year of age	2 years of age	3 years of age
PEP [=1]	-0.057***	-0.017	-0.007	-0.001
	(0.022)	(0.020)	(0.016)	(0.005)
Observations	880	880	880	880
Wave FE	No	No	No	No
HH FE	No	No	No	No
Geographic Sampling	Yes	Yes	Yes	Yes
Pre-migration controls	Yes	Yes	Yes	Yes

Notes: The table presents the estimates of the specification described in equation (1) but restricted to individuals who were not contacted in VenRePS round 2. Department corresponds to the five departments in which the sample was collected and geographic sampling corresponds to the four geographic levels at which the sample is representative, including three main cities and a fourth group that accounts for nine smaller urban centers with prevalent migration from Venezuela. Pre-migration control variables include: (i) individual controls for the head of household (gender, age, and education); (ii) labor history for the head of household (probability of being employed, type of job, probability of having a written contract, and the time gap between the last job and the migration episode); (iii) household characteristics (number of children, household size, access to public services, owning dwelling, and having a smartphone); and (iv) networks prior to migration episode (had family and friends in Colombia, knew of job opportunities before migrating, and migrated for health-related reasons). Standard errors clustered at the household level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.3. Attrition using Main Outcomes as Predictors

	(1)
Likelihood of having children of	Attrited [=1]
0 years of age	0.038
	(0.036)
1 year of age	-0.006
	(0.035)
2 years of age	0.020
	(0.050)
Observations	2,232

Notes: The table presents the correlation between the main outcome variables and the likelihood of attrition at the head-of-household level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix C: Robustness Tests

Table C.1. Excluding Border Departments

	Likelihood of having children of			
	(1)	(2)	(3)	(4)
	0 years of age	1 year of age	2 years of age	3 years of age
PEP [=1]	-0.034***	-0.072***	-0.015	-0.002
	(0.011)	(0.013)	(0.010)	(0.002)
Observations	3,588	3,588	3,588	3,588
Observations by wave	1,196	1,196	1,196	1,196
Wave FE	Yes	Yes	Yes	Yes
HH FE	Yes	Yes	Yes	Yes
Department ×wave	Yes	Yes	Yes	Yes
Geographic Sampling	Yes	Yes	Yes	Yes
Pre-migration controls ×wave	Yes	Yes	Yes	Yes

Notes: The table presents the estimates of the specification described in equation (1). The analysis excludes migrants in the departments bordering Venezuela. Department corresponds to the five departments in which the sample was collected and geographic sampling corresponds to the four geographic levels at which the sample is representative, including three main cities and a fourth group that accounts for nine smaller urban centers with prevalent migration from Venezuela. Pre-migration control variables include: (i) individual controls for the head of household (gender, age, and education); (ii) labor history for the head of household (probability of being employed, type of job, probability of having a written contract, and the time gap between the last job and the migration episode); (iii) household characteristics (number of children, household size, access to public services, owning dwelling, and having a smartphone); and (iv) networks prior to migration episode (had family and friends in Colombia, knew of job opportunities before migrating, and migrated for health-related reasons). Standard errors clustered at the household level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table C.2. Head of the HH and Partner in RAMV only

	Likelihood of having children of			
	(1)	(2)	(3)	(4)
	0 years of age	1 year of age	2 years of age	3 years of age
PEP [=1]	-0.040***	-0.086***	-0.015	-0.002
	(0.014)	(0.016)	(0.012)	(0.003)
Observations	2,430	2,430	2,430	2,430
Observations by wave	810	810	810	810
Wave FE	Yes	Yes	Yes	Yes
HH FE	Yes	Yes	Yes	Yes
Department ×wave	Yes	Yes	Yes	Yes
Geographic Sampling ×wave	Yes	Yes	Yes	Yes
Pre-migration controls ×wave	Yes	Yes	Yes	Yes

Notes: The table presents the estimates of the specification described in equation (1). In the analysis, the treated units are households in which only the head of household or the partner has PEP. Department corresponds to the five departments in which the sample was collected and geographic sampling corresponds to the four geographic levels at which the sample is representative, including three main cities and a fourth group that accounts for nine smaller urban centers with prevalent migration from Venezuela. Pre-migration control variables include: (i) individual controls for the head of household (gender, age, and education); (ii) labor history for the head of household (probability of being employed, type of job, probability of having a written contract, and the time gap between the last job and the migration episode); (iii) household characteristics (number of children, household size, access to public services, owning dwelling, and having a smartphone); and (iv) networks prior to migration episode (had family and friends in Colombia, knew of job opportunities before migrating, and migrated for health-related reasons). Standard errors clustered at the household level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendix D: VenRePS Follow-up and Final Database Cleaning

We hired Innovations for Poverty Action (IPA) to administer the survey over the telephone between October 2021 and March 2022. This operation represented the follow-up of individuals surveyed one year earlier. We managed to recontact 2,308 out of 3,455 migrant households—a high figure considering the challenges of following very mobile individuals who are often reluctant to give information for fear of deportation.

The tasks carried out in the design and data collection of the survey's first round were crucial to implementing the follow-up phase. From the baseline, we needed detailed information that would allow us to track individuals in future rounds. Therefore, we asked for more than one telephone number and current residence. An essential part of the design was the pursuit of a "snowball" sampling strategy, which consists of identifying individuals who refer other potential participants. This allowed us to broaden the sample of potential respondents and helped us recontact respondents as needed.

The first round of survey collection ended in March 2021. We next conducted a WhatsApp survey, which enabled us to update participants' telephone information prior to the start of the second round. We implemented two additional strategies. First, we telephoned individuals we could not reach on WhatsApp. Second, we incentivized participants to respond by conducting raffles and offering a document certifying that they were in Colombia prior to January 31, 2021. The last was a requirement to apply for the official Estatuto Temporal de Permanencia (ETPV), a status that allows migrants to work and access social programs for a ten-year renewable period. Between June 2021 and September 2021, we designed the questionnaire for the second round using three criteria. First, we prioritized the head of household and partner as the primary individuals to follow within the nuclear household. Second, we included questions to identify individuals who joined the household and those who were no longer part of it. Finally, we devised a strategy to characterize split households.

Before collecting the second round, we trained a team of Venezuelan enumerators who had already been part of the first round. This was important because of their familiarity with the questionnaire and their commitment to the study. The enumerators were also crucial at earlier stages of the survey design and provided valuable feedback. During the training, we offered them resources to cope with stress during data collection plus monetary incentives to achieve recontact objectives.

We began the collection using a calling protocol as a first recontact strategy. This consisted of contacting participants using the phone numbers they gave us in the first round of the survey and the updated numbers we obtained in the intermediate WhatsApp recontact mentioned above. We sent an SMS message to each individual and offered them the chance to participate in a raffle and a monetary incentive to answer the survey. After that, we called the numbers we had for each participant up to four times at different hours over three days. Once contacted, we scheduled an appointment to complete the survey if the individual was not available to do so at the time of the call. We also provided flexibility to reschedule the completion of independent modules of the survey.

We followed three alternative strategies to reach individuals we could not recontact using the calling protocol. First, we assembled a small team of highly productive enumerators who worked in later time slots and focused on contacting individuals at the busiest hours of the day. Second, we shortened the number of questions by focusing on three content modules: labor market access, household consumption, and integration of migrants into Colombian society. We conducted this shorter version only for the heads of household who refused the original survey. Finally, we called the original and referred individuals to pursue updated numbers for hard-to-reach participants.

Of the total number of households recontacted (2,308), we used only 1,346 for two reasons. First, we excluded households with Colombian citizens over 10 years of age. Second, in the second round of the survey, we did not consider households that were split or to

which we could only apply the short survey.

We stacked both rounds of VenRePS and constructed a baseline using the date of birth of household members prior to the opening of RAMV (April 5, 2018). By that point in time, no households were beneficiaries of the PEP-RAMV program. For each of the three waves (baseline, VenRePS, and VenRePS follow-up), we observe the age of the head of household's children who were born in Colombia.[11] We excluded from the analysis children who were conceived before the PEP-RAMV announcement (August 2, 2018) since the program could not have affected the decision to have these children.

[11] As a consequence of a decree issued in 2019, all children born in Colombia to Venezuelan parents are Colombian.