Regularization Programs and the Fertility of Forced Migrants: Insights from a Panel Study of Venezuelan Migrants in Colombia[*]

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Abstract

How do regularization programs shape the fertility of forced migrants? Using data from a unique panel study of more than one thousand forced migrants from Venezuela residing in Colombia, we address this question through a quasi-experimental design. We compare changes in childbearing patterns of regularized migrant households, vis-a-vis those experienced by non-regularized but eligible to do so migrant families, before and after the launch of a regularization program that granted full access to social services, as well as work permits. We find that regularized migrant households reduced their childbearing, vis-a-vis their non-regularized counterparts. The impats appear to be driven by program beneficiaries' improved access to labor market opportunities and family planning services.

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I INTRODUCTION

Forced migration has more than doubled in the last decade and is expected to continue rising due to climate change, conflict, and other factors. To effectively address the needs of both forced migrants and host societies, it is imperative to improve our understanding of how forced migrants integrate into their new communities and the role of policy in facilitating this process. Host governments are grappling with the fiscal burdens associated with refugees, amidst concerns within native populations about perceived threats to national identity. In this context, higher fertility rates among forced migrants, when compared to natives, may only exacerbate natives' opposition toward them. This dynamic is especially pronounced in developing countries, where sudden and massive inflows of forced migrants can strain the host country's healthcare system if adjustments in service supply are not swiftly made. How can regularization programs that ease migrants' social and economic integration into the host economy shape their fertility?

We address this question in the context of a Colombian regularization program for forced migrants from Venezuela. Regularization programs allow undocumented migrants to regularize their status and confer specific rights and benefits over a finite period of time. A priori, the impact of such a program on migrant's fertility is an empirical question. On one hand, regularizations should lower childbearing and childrearing costs through the provision of full-blown access to health, education, and social services. We call this the *price effect* (e.g., Bleakley and Lange, 2009; Qian, 2009; Becker, Cinnirella and Woessmann, 2010). On the other hand, by enabling migrants access to formal labor market, regularization programs may increase women's opportunity cost of having children. We call this the *opportunity cost effect* (e.g., Mincer, 1963; DeFronzo, 1980; Falasco and Heer, 1985).

Our study centers on the impact of the Permiso Especial de Permanencia (PEP) —a regularization program offered by Colombia in July 2018 to approximately half a million of Venezuelan migrants.[1] Colombia is the primary hosting nation of Venezuelan forced migrants, which have been fleeing their country in massive numbers escaping the social, economic, and political crises in their country. By 2023, approximately, 7.7 million or 30 percent of the Venezuelan population

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[1]Venezuelan migrants who had previously registered in a census collected by the Colombian government between January and April of 2018 were eligible for the program. Registration was voluntary and aimed at counting the total number of undocumented Venezuelan migrants living in Colombia.

had left the country, of which over 2.5 million were hosted in Colombia, representing 5 percent of Colombia's population. PEP beneficiaries were granted work permits and full-blown access to social services for up to two years.[2] PEP was one of the most generous regularization programs offered to forcibly displaced migrants in a developing country, granting migrants full access to the labor market, health, education, and social protection system enjoyed by Colombians.

We examine how PEP impacted household fertility by leveraging information from two waves of the Venezuelan Refugee Panel Study (VenRePS). VenRePS is a unique representative survey of undocumented and regularized forced migrants from Venezuela residing in Colombia's main urban centers prior to the program's announcement.[3] Using longitudinal data on a total of 1,346 households, along with a difference-in-difference methodology, we compare the probability of having young children (conceived after the program launched) of households eligible for PEP and household ineligible for the program, before and after the program's implementation. The data enables us to observe households at three points in time: at baseline, two, and three years after the adoption of PEP. Our specifications include household, wave, and household-wave fixed effects, enabling us to account for unobserved fixed and time-varying factors shaping their fertility. In addition, we incorporate a rich set of municipality baseline covariates interacted with time trends to address non-parametric changes in city-wide characteristics affecting households' childbearing choices.

We find consistent and robust evidence of PEP effectively reducing fertility among regularized migrants. Specifically, migrant households eligible for PEP exhibited a 3.9 percentage point (pp) decrease in the likelihood of having children under one year old, a 7 pp decrease in the likelihood of having one-year-olds, and a 1.8 pp decrease in the likelihood of having two-year-olds. Falsification tests further validate our findings by confirming the absence of changes in the probability of having children conceived prior to the program's implementation. Additionally, event studies reveal a distinct break in eligible households' fertility trends subsequent to the program's introduction, with the effects gradually diminishing over time.

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[2]PEP was followed in 2021 by a program —-the *Estatuto Temporal de Protección para Migrantes Venezolanos*, which extended PEP's benefits for an additional 10 years.

[3]These localities include: Bogotá, Medellín, Barranquilla, and a fourth group of smaller cities. Approximately half of the households in the survey were eligible for the PEP program.

We also explore mechanisms behind the program's fertility impacts, paying close attention to two potential explanations. One refers to improved access to labor market opportunities, which may have risen the opportunity cost of having children for women and, thereby, contributed to the reduction in fertility —PEP's opportunity cost effect. The second explanation refers to regularized migrants' access to family planning services. Alongside the increasing opportunity cost associated with childbearing and childrearing, improved access to these services may have countered any of the program's price effects lowering the cost of having children due to regularized migrants' comprehensive access to health, education, and social services. Our analysis indicates that households eligible for PEP indeed experienced better job prospects and enhanced access to public services, particularly healthcare. This suggests that both mechanisms likely contributed to the observed decline in fertility among regularized migrants.

Our findings have significant implications for the formulation of policies aimed at facilitating the integration of migrants in the Global South, particularly in countries concerned about migrants' fertility. In the case of Colombia, we demonstrate that regularization efforts curtailed migrant fertility. In a broader context, these insights can guide nations embarking on similar initiatives or hosting substantial numbers of forcibly displaced migrants, a reality prevalent across Latin America. Notably, 18 out of 26 nations in the region have implemented over 92 regularization initiatives since 2000, with many drawing inspiration from programs like the Personal Employment Plan (PEP) Ibáñez et al. (2024). These efforts are not confined to the Americas; developed nations in North America and Europe have also undertaken similar measures. For instance, the United States enacted the Immigration Reform and Control Act (IRCA) and established the Deferred Action for Childhood Arrivals (DACA) program. Canada has its temporary resident permit system, while countries like Ireland, Italy, Portugal, and Spain have adopted their own regularization programs.

Our study contributes to several strands of literature. First, it extends work on the effects of amnesties and regularizations, which has been mostly focused on developed nations in North America and Europe. The lion's share of these studies examines the effects of amnesties on native labor outcomes, generally documenting positive outcomes.[4] The studies in that group that are

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[4]See for example Cobb-Clark, Shiells and Lowell (1995); Kossoudji and Cobb-Clark (2002); Kaushal (2006); Amuedo-

most relevant to our research have examined how immigration policies that facilitate migrants' integration impact their fertility.[5] While informative, these studies predominantly focus on developed countries, policies that do not qualify as broad regularization programs, and migrants who may not be considered forced migrants. Consequently, their findings may not be readily extrapolated to migrants in the Global South, where access to healthcare and family planning services is limited, and rates of fertility and economic vulnerability are higher compared to developed nations. Moreover, certain characteristics of forcibly displaced populations, such as disproportionately higher shares of women and children with precarious access to healthcare prior to migration, may yield different effects compared to those observed in the Global North.

Second, we contribute to the migration literature by assembling and analyzing a unique longitudinal dataset representative of Venezuelan forced migrants, including both undocumented individuals and those who have regularized their status—a segment of the population that is notoriously difficult to reach. This type of data is extremely difficult to gather since forced migrants are highly mobile, which makes their tracking expensive. In addition, undocumented migrants logically shy away from answering surveys due to trust issues, explaining why most evaluations of regularization and amnesty programs use previously regularized migrants or the host country's population as a control group. Moreover, it is challeging to get adequate sampling frames for these populations. To address these issues, we devised a novel sampling strategy that integrates multiple survey methods to compile a list of eligible households for our VenRePs study and to track them over time. This approach allowed us to overcome some of the inherent difficulties associated with studying hard-to-reach populations like Venezuelan forced migrants.

Third, we contribute to our understanding of the impacts that the PEP program had in Colombia. For instance, Ibáñez et al. (2024) and Urbina Florez et al. (2023), document PEP's positive impacts on Venezuelan migrants' consumption, labor income, and health outcomes using the first wave

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Dorantes, Bansak and Raphael (2007); Amuedo-Dorantes and Bansak (2011); Pan (2012); Orrenius and Zavodny 2015; Pope 2016; Amuedo-Dorantes and Antman (2017); Ortega and Hsin (2022); Devillanova, Fasani and Frattini (2018); Porto, Martino and Naticchioni (2021); Deiana, Giua and Nisticó (2022). A related literature studies effects of migrant amnesties on crime in host communities (Baker 2015, Mastrobuoni and Pinotti 2015, and Pinotti 2017), poverty rates and consumption (Amuedo-Dorantes and Antman 2017 and Dustmann, Fasani and Speciale 2017), and education outcomes (Kuka, Shenhav and Shih 2020, Amuedo-Dorantes and Antman 2017, and Hsin and Ortega 2018)

[5]See Avitabile, Clots-Figueras and Masella (2014) for Germany, Kuka, Shenhav and Shih (2019) in the United States, Lanari, Pieroni and Salmasi (2020a) and Lanari, Pieroni and Salmasi (2020b) for Italy, and Amuedo-Dorantes, Borra and Rivera-Garrido (2023a) for Spain.

of the VenRePs survey. Other studies inform about PEP's labor market impacts (Bahar, Ibáñez and Rozo, 2021), political outcomes (Rozo, Urbina Florez and Quintana, 2023), firm outcomes (Amuedo-Dorantes, Borra and Rivera-Garrido, 2023b), and inequality ((Lombardo et al., 2021)). Our analysis adds to this literature through the analysis of medium-term effects of the PEP program on migrant fertility.

Finally, we add to a vast literature examining how policy shapes fertility (e.g., Lalive and Zweimüller, 2009; Milligan, 2005; Bailey, 2012). Our focus is on the impact of immigration policy on migrant 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, supporting the sustainability of social programs through a larger workforce with higher fertility rates than those of natives (e.g., Storesletten, 2000). At the same time, higher fertility rates may also be seen as a threat to national identity by natives. Awareness about the impact that various policies have on migrant fertility is well-warranted.

II   CONTEXT: THE PEP REGULARIZATION PROGRAM

Colombia is the main recipient of forced migrants from Venezuela. According to data from the United Nations Refugee Agency, approximately 2.9 million Venezuelan migrants had arrived in Colombia by 2023, with the vast majority arriving between 2016 and 2019.[6] 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 Undocumented Migrants: January–April 2018

In 2018, the Colombian government conducted a survey to estimate the number of undocumented 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.[7] 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.

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[6]This number does not include undocumented migrants who escaped detection by authorities.
[7]There are 1,122 Colombian municipalities.

II.B The PEP program: July–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 two-year residency permit, a work permit, 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 (Ibáñez et al., 2024), with negligible effects on the labor market prospects of Colombian native workers (Bahar, Ibáñez and Rozo, 2021). We hypothesize that, by giving Venezuelan migrants access to the formal labor market and family planning services, PEP may have also impacted other household decisions, such as their childbearing 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 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 enjoying higher household income. If we abstract from the opportunity cost of time (e.g., Hotz, McElroy and Sanders, 1997), the *price effect* would favor increases in fertility as long as children are considered normal goods (e.g., Becker, 1960; Black et al., 2013, Cohen, Dehejia and Romanov, 2013).

Nevertheless, PEP also provided work permits, which raised the *opportunity cost* of childbearing. 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). Alongside regularized migrants' access to family planning services, the opportunity cost of PEP on migrant fertility may have offset any price effects on the cost of childbearing and childrearing.

The ambiguity surrounding PEP's implications for migrant fertility is also present when using modified versions of the Becker and Lewis (1973) model underscoring 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, Clots-Figueras and Masella (2014) and Lanari, Pieroni and Salmasi (2020a), 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. Specifically, Lanari, Pieroni and Salmasi (2020a) show how the lower price of child quality incentivized childless women to have a baby given the lower per-unit cost of childbearing, even though it decreased the overall number of children that eligible women would have.

In what follows, we evaluate how PEP shaped migrant fertility, as well as the channels potentially responsible for the found impacts.

IV   THE VENEZUELAN REFUGEE PANEL STUDY

Representativeness.   Our main source of data are the two waves from the Venezuelan Refugee Panel Study (VenRePS). VenRePs is representative of undocumented and regularized migrants living in Colombia in 2018 in four geographical areas: Bogotá, Medellín, Barranquilla, and a group of smaller cities that together comprise an area. The geographic scope of the survey includes the municipalities of Cúcuta, Villa del Rosario, Cali, Cartagena, Riohacha, Maicao, Uribia, Valledupar, Santa Marta, and Arauca. The first three cities are large urban centers in Colombia hosting most Venezuelan migrants in the country. In Figure 2, we compare the location of migrants in the VenRePS sample relative to the location of Venezuelan migrants in Colombia based on the 2018 population census

Sampling frame. Roughly half of the undocumented migrants in VenRePS were selected from the *Registro Administrativo para Migrantes Venezolanos* (RAMV), a census with information on 442,462 Venezuelan forced migrants living in Colombia between January and April of 2021. The census was collected before the PEP was announced with the objective of counting the number of undocu-

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mented Venezuelan forced migrants in Colombia. Later, in July 2021, the Colombian government ruled that only individuals in the RAMV were eligible for the newly announced PEP program, which registered migrants starting in August of 2021. Every individual in the RAMV was eligible to apply to the PEP. From the RAMV census, we selected a geographically representative sample of 13,083 migrant households, from which we randomly chose half of the households in our study.

The other half of the undocumented migrant sample, composed of PEP-ineligible forced migrants, originated from a comprehensive listing exercise constructed using referrals from local migrant organizations and respondents in the RAMV sample. The listing exercise included 12,554 non-RAMV households. Using this sampling frame, we randomly selected half of the households in our study to ensure geographic representativeness in the main urban centers, as noted above. These individuals were not in the RAMV census; therefore, they were not eligible for the PEP program.

Ibáñez et al. (2024) demonstrate that migrants surveyed in VenRePS, whether they had been contacted through the RAMV survey or through referrals, were comparable in terms of sociodemographic characteristics before the program's rollout. Specifically, there were no observable differences between both groups of migrants with regards to their personal characteristics.

Eligibility. Individuals in the study had no passport, were at least 18 years old, had documentation proving they were born in Venezuela, and had arrived in Colombia between January 2017 and December 2018. In other words, they were undocumented migrants living in Colombia at the time of PEP's implementation.

Response rates. The first wave of the survey was originally planned to be in-person, but was switched to a phone survey due to the COVID-19 pandemic. Phone interviews took place between October 2020 and March 2021. The second wave was collected one year later, between October 2021 and February 2022. We managed to trace 2,308 of the original 3,455 migrant households—a high follow-up rate considering this highly mobile population, which is also reluctant to provide information about themselves. In Section VII, we show our results are not driven by non-random attrition.

To maximize tracking rates, we employed multiple strategies. First, we used data collected in the

first wave comprehensive of multiple contacts and social media accounts. Second, we mantained interim contact with respondents, using lotteries of grocery vouchers and offering a document certifying that they were in Colombia prior to January 31, 2021. This documentation was a requirement to apply for the official *Estatuto Temporal de Permanencia* (ETPV) —a status enabling migrants to work and access social programs for a ten-year renewable period.[8] Third, we also worked with the same 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.[9] Fourth, we selected 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. Fifth, for individuals who refused ot answer, we offered the possibility of collecting a shorter survey focused on the following topics: labor market access, household consumption, and integration of migrants into Colombian society. Finally, in the case of hard-to-reach participants, we called individuals who had referred us to respondents to obtain updated contact information.

Sample size. Of the total number of households recontacted succesfully in the second wave (2,308), we excluded households with Colombian citizens over 10 years of age, who enjoyed full access to social services, split households, and households that only responded to the short survey. That left us with a sample of 1,346 households. 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.[10] 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.

Questionnaire. VenRePs has five modules collecting data on: i) sociodemographics, ii) the RAMV and PEP registration processes, iii) labor market activities as captured in the Colombian labor force

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[8]For the second wave, we prioritized the head of household and her/his partner as the primary individuals to follow within the nuclear household. We also 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.

[9]During the training, we offered enumerators resources to cope with stress during the data collection exercise, plus monetary incentives to achieve recontact objectives.

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

surveys, iv) comprenhensive health information, and v) household information on migration intentions, integration measures, prosocial preferences, housing, expenditures, and remittances. On average, the survey lasted 40 minutes and respondents received an incentive of 27,000 Colombian pesos (COP, about $USD 9) for participating.[11]

Table A.1 presents summary statistics distinguishing by gender. Panel A presents descriptive statistics for the male sample, and Panel B for the female sample. Several patterns are worth noticing. First, there are notable differences between migrants eligible and ineligible for PEP post-program implementation. 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 unregistered and PEP-ineligible counterparts. 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 alike educational attainment to Colombian natives, with those registered in the RAMV census being more educated and generally younger than Colombian natives.[12]

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 already differed with respect to characteristics that could be potentially correlated to their fertility outcomes, such as education. 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 of PEP eligible households, vis-a-vis changes in fertility of PEP-ineligible households, from before to after PEP was implemented. Because of the longitudinal nature of the survey, we are able to account for household fixed-effects, enabling us to account for unobserved household-level heterogeneity potentially correlated to changes in household fertility.

As noted earlier, we observe households at three points in time: at baseline on the day before the RAMV census (April 5, 2018), as well as twice post-PEP treatment, i.e., in 2020 and in 2021. Hence, we stack the data to evaluate the impacts of being eligible for PEP on the probability of having

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[11]We used different delivery options including cellphone credit, supermarket vouchers, and electronic transfers.
[12]Colombian individuals come from the same survey.

children of T years of age and estimate the following equation:[13]

$$Child_{hdrt}^{T} = \beta_0 + \beta_1 I[PEP_{hrd} = 1] \times Post_t + \sum_{x \in X_{hdr}} \phi_x(x_h \times \gamma_t) + \phi_{d \times t} + \psi_{r \times t} + \alpha_h + \alpha_t + \epsilon_{hrdt} \quad (1)$$

where h stands for household, d for department, r 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). $Child_{hdrt}^{T}$ is the likelihood that household h has a child T years old (T = 0,1,2,3). $I[PEP_{hrd} = 1]$ is a dichotomous variable equal to one for households eligible for PEP,[14] and $Post_t$ is a dummy equal to one after the program's rollout. $\sum_{x \in X_{hrd}} \phi_x(x_h \times \gamma_t)$ 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) the presence of 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. In addition, the model includes fixed effects for each household (α_h), for each survey wave (α_t), as well as department-specific wave trends ($\phi_{d \times t}$) for each of the five departments included in the survey, and geographic-sampling-region-specific wave trends ($\psi_{r \times t}$) 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 our estimates from household-specific, time-invariant differences between treated and non-treated groups potentially confounding PEP's fertility effects. In addition, by flexibly accounting for non-parametric temporal changes in a

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[13]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.

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

rich set of pre-migration household characteristics, we account for dynamic differences between treated vs. control households correlated to differences in their fertility changes. As such, β_1 captures how fertility changed in treated, relative to control households, from before to after PEP's implementation. The model is estimated for four different outcomes —namely, the probability of having children less than one, one, two, or three years old in 2020 and 2021. Since the regularization under PEP 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. Specifically, 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 —an outcome we look at as a falsification check.

VI   FERTILITY IMPACTS OF REGULARIZATION THROUGH PEP

Table 2 illustrates the results of estimating equation (1) in three panels. Panel A shows results using the data from the baseline and from 2020 (the first wave of VenRePS). Panel B presents results using the data from the baseline and from 2021 (the second wave of VenRePS). 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 regularizing through PEP 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 regularization through PEP lowered the probability of having children in all panels. Our preferred results are those in Panel C, as they include all data waves. Based on those estimates, migrant households who regularized through 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. Finally, to serve as a falsification check, we also look at PEP's impact on the likelihood of having three-year-olds, which is rightfully null given that those children were conceived prior to the program's announcement.

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, aligning with the pro-

gram's implementation timing. 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 policy impacts on the probabilities of having children less than one year old, one year old, and two years old.

The findings presented in Panels A and B indicate that the fertility effects of the PEP program were not only immediate but also intensified one year after its implementation, reflecting the typical delay in the manifestation of benefits from regularization initiatives. This delay can be attributed to various factors. For instance, gaining access to social services necessitates both enrollment in PEP and obtaining a SISBEN vulnerability score, a process that can be time-consuming and involve bureaucratic procedures with public authorities. Similarly, securing formal employment may also be a lengthy process, thereby explaining the program's heightened impact observed one year post-implementation.

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

VII ROBUSTNESS CHECKS

We conduct a series of sensitivity checks to gauge the extent to which sample attrition may be biasing our findings, as well as the robustness of our findings to various sample changes. All in all, the robustness checks described in ths section support our main findings and the hypothesis that PEP curtailed migrant fertility. The findings do not appear to be affected by attrition biases, the inclusion of regular migrant commuters in the sample, pre-trends, or measurement biases due to information gathered from household members who were not the main survey participants.

VII.A Assesing attrition concerns

Given the panel nature of the data, a natural concern is the extent to which attrition may be biasing our findings. We conduct several robustness checks 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 implementation. As shown in Table 3, only 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. Moreover, we examine the extent to which attrition is correlated with PEP take-up in Table 4. The results suggest that attrited individuals had a lower, albeit marginally significant, probability of taking up the PEP program. Although the estimated coefficients are small, both of these exercises suggest that attrited individuals were more vulnerable and less rooted in Colombia.

Next, in Table 5, we estimate the fertility impact of PEP on 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 take-up significantly curtailed their probability of having children zero years of age by 5.7 percentage points.

Finally, we examine if attrition rates in the second survey wave are correlated with our outcomes of interest during the first survey wave. As illustrated in Table 6, they are not. This implies that attrited respondents 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.

VII.B   Falsification test using children born before the PEP program

In addition to the falsification check using children who are three years old in column (4) of Table 2, we also conduct a placebo checks looking at the likelihood of having older children clearly conceived prior to the announcement of PEP. Figure B.1 displays the results from these estimations, along with our findings for younger children to serve as a comparison. As shown therein, while the program had a significant impact on the likelihood of having children less than 2 years old, it had no significant impact on having older children, confirming the results were not driven by differential fertility trends between households who benefited from the regularization offered through PEP and households that did not.

VII.C   Excluding households along the Colombian-Venezuelan border

We also experiment with excluding from the sample individuals along the Colombian-Venezuelan border, many of whom may not be residing in Colombia. Results from this exercise are shown in Table B.1. We continue to find evidence of fertility declines as captured by similarly sized reductions in the likelihoods of having a child less than one year old or one year old, as documented in

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Table 2.

VII.D Restricting the sample to household heads and their partners

We also experiment with restricting our sample to household heads and their partners to address concerns regarding mismeasurement of fertility of other household members who were not the main survey respondents. Table B.2 shows the results using this smaller sample. As shown therein, we continue to find evidence of PEP reducing fertility, as captured by a significantly lower likelihood of having a child less than one year old and a similarly sized decline in the probability of having a one-year-old as in Table 2.

VIII WHAT EXPLAINS THE REDUCTION IN FERTILITY UNDER PEP?

As discussed 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, leading to fertility reductions. In addition, access to public health care services and family planning services may have facilitated access to contraceptives, coupled with the now available social services and public assistance that lower the price of child quality, may have induced a quantity-quality trade-off and led to a lower childbearing likelihood.

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 having 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, whereas labor market outcomes are measured at the individual level. Results are in Tables 7 and 8, respectively. All outcomes are observed before and after PEP's implementation.

As shown in Table 7, PEP improved migrants' access to public assistance. In particular, households benefiting from the regularization offered through PEP were 43.1 pp more likely to have a SISBEN score, 10.6 pp more likely to have access to the subsidized health care regime, and 30 pp more likely to receive government transfers than ineligible households. Consequently, house-

holds benefiting from the PEP program enjoyed enhanced access to healthcare and social safety nets relative to those not benefiting from PEP. This improved access to essential benefits may have mitigated the cost associated with ensuring child quality, potentially leading to a trade-off between quantity and quality of children. Moreover, increased access to healthcare likely facilitated access to family planning services, thereby empowering households to more effectively manage their fertility decisions.

In addition, regularized migrants enjoyed better labor market opportunities than their non-regularized counterparts, as shown in Table 8. Both men and women benefited from the regularization in this regard. In the case of men, they were about 7.7 pp. more likely to be employed and 14.7 pp. more likely to hold a formal job compared to their non-regularized counterparts. The impacts were similar for women, for whom those probability increases averaged 6.5 pp. and 14 pp., respectively.

In sum, the results in Tables 7 and 8 support the notion that households who benefited from the regularization offered through PEP curtailed 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, Clots-Figueras and Masella 2014; Lanari, Pieroni and Salmasi 2020a). 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), providing further incentives to reduce their fertility.

IX CONCLUDING REMARKS

We examine the fertility impacts of Colombia's massive 2018 regularization program among Venezuelan migrants. Our results largely suggest that migrants' regularization led to a significant drop in their childbearing likelihood —an impact observed immediately after the program's implementation. The effects, which strengthened one year after the regularization was announced, might have partially been driven by improved access to labor market opportunities and social services —including health care and public transfers. The former likely raised the opportunity cost of childbearing, whereas the latter possibly lowered the cost 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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X   Tables and Figures

Figure 1. PEP Program Rollout

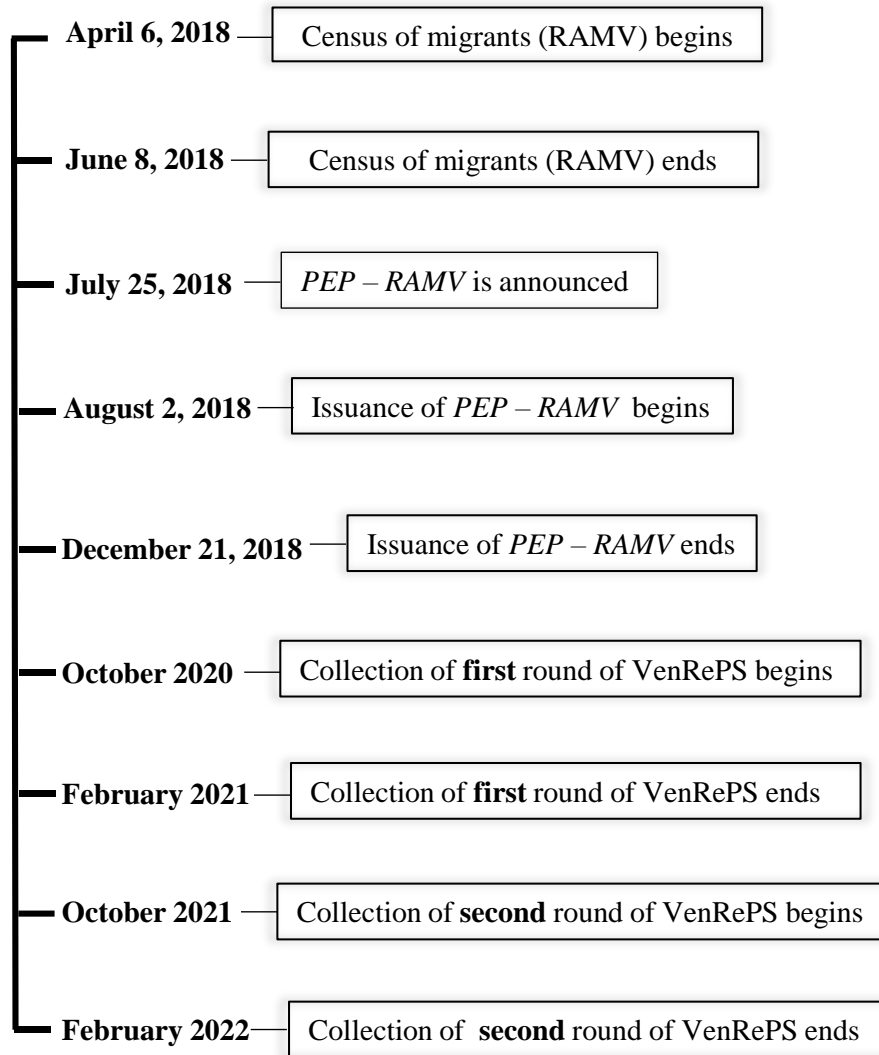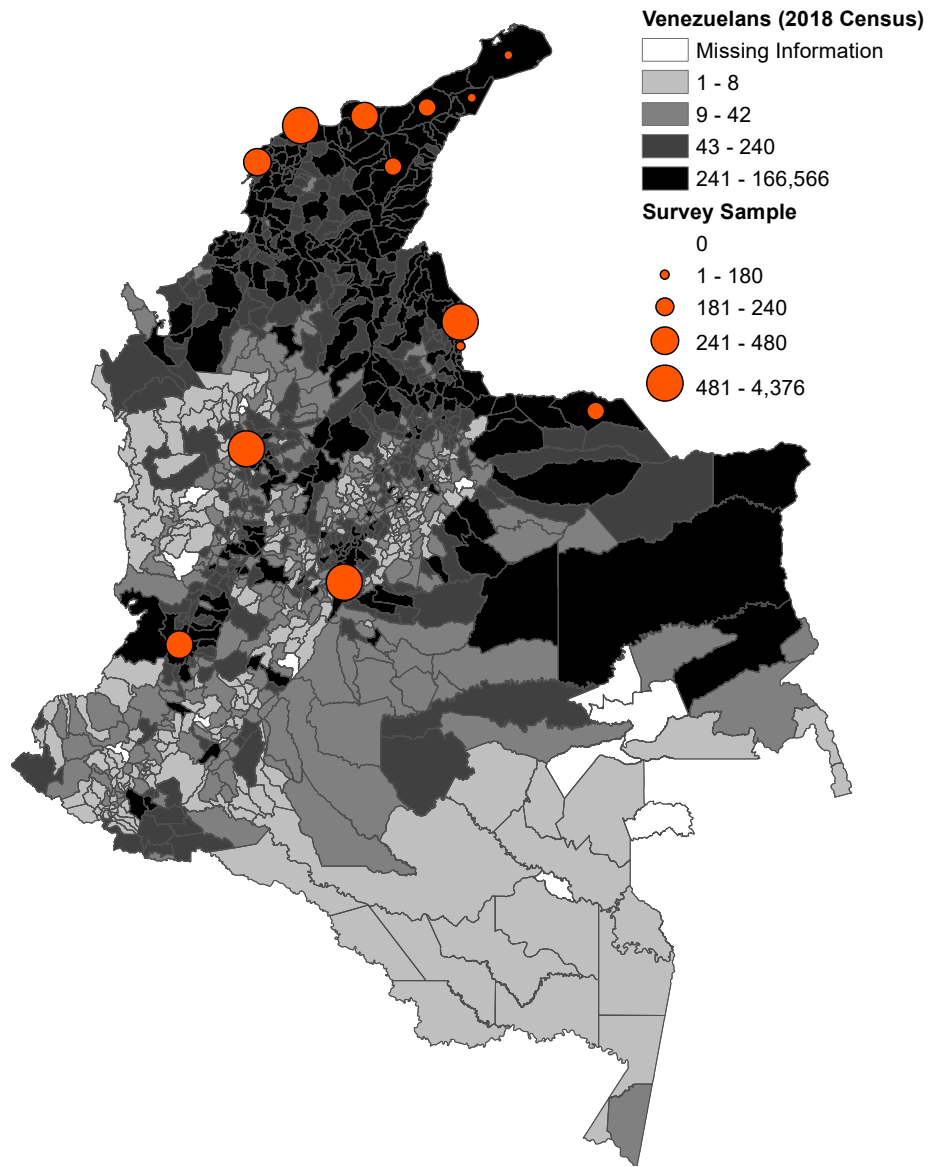|		
April 6, 2018	—	Census of migrants (RAMV) begins
June 8, 2018	—	Census of migrants (RAMV) ends
July 25, 2018	—	*PEP – RAMV* is announced
August 2, 2018	—	Issuance of *PEP – RAMV* begins
December 21, 2018	—	Issuance of *PEP – RAMV* ends
October 2020	—	Collection of **first** round of VenRePS begins
February 2021	—	Collection of **first** round of VenRePS ends
October 2021	—	Collection of **second** round of VenRePS begins
February 2022	—	Collection of **second** round of VenRePS ends

Figure 2. Share of Venezuelan Migrants and VenRePS Sample

Notes: The figure presents the share of Venezuelan migrants in the Colombian population Census of 2018 and the sample of Venezuelans surveyed in VenRePS 2020.

Table 1. Descriptive Statistics

Control Variables (baseline)	PEP Ineligible			PEP Eligible		
	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

Notes: The table presents descriptive statistics for the households in our sample (596 ineligibles and 750 eligibles = 1,346 households).

Table 2. Effects of the PEP 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)
Mean inelegible	0.124	0.107	0.001	0.000
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)
Mean inelegible	0.046	0.139	0.093	0.000
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)
Mean inelegible	0.068	0.095	0.044	0.000
Observations	3,960	3,960	3,960	3,960
Controls in all panels				
Wave FE	Yes	Yes	Yes	Yes
Household 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. Assessing the Determinants of Attrition in Wave II

	(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 in the second wave of VenReps at the head-of-household level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Correlation between Attrition Probability and PEP Eligibility

	(1) Attrited HH [=1]
PEP [=1]	-0.039*
	(0.023)
Observations	2,200
Pre-migration controls	Yes

Notes: The table presents the correlation between the PEP eligibility and the attrition probability in the second wave of VenRePs at the head-of-household level. Standard errors clustered at the household level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Effects of PEP Eligbility for the 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 6. Attrition Probability using Main Outcomes as Predictors

Likelihood of having children of	(1) Attrited [=1]
0 years of age	0.003
	(0.037)
1 year of age	-0.018
	(0.036)
2 years of age	0.008
	(0.051)
Observations	2,200
Pre-migration controls	Yes

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

Table 7. Effects of the PEP on Access to Government Programs

Dep Variable:	SISBEN [=1]	Subsidized health care [=1]	Transfers [=1]
	(1)	(2)	(3)
PEP [=1]	0.431***	0.106***	0.304***
	(0.020)	(0.015)	(0.019)
Observations	3,952	3,959	3,940
Wave FE	Yes	Yes	Yes
Individual 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 individual level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 8. Effects of the PEP on Labor Market Access

Dep Variable:	Employed [=1]	Formal Job [=1]
	(1)	(2)
Panel A: All sample		
PEP [=1]	0.077***	0.147***
	(0.021)	(0.038)
Observations	6,045	2,588
Panel B: Women		
PEP [=1]	0.065**	0.140**
	(0.031)	(0.059)
Observations	3,432	1,098
Wave FE	Yes	Yes
Individual 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 individual 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: Robustness Tests

Table B.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***	-0.016	-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 B.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]	-0.041***	-0.086***	-0.017	-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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure B.1. Effects of the PEP Program on the likelihood of having children from 0 - 10 years of age
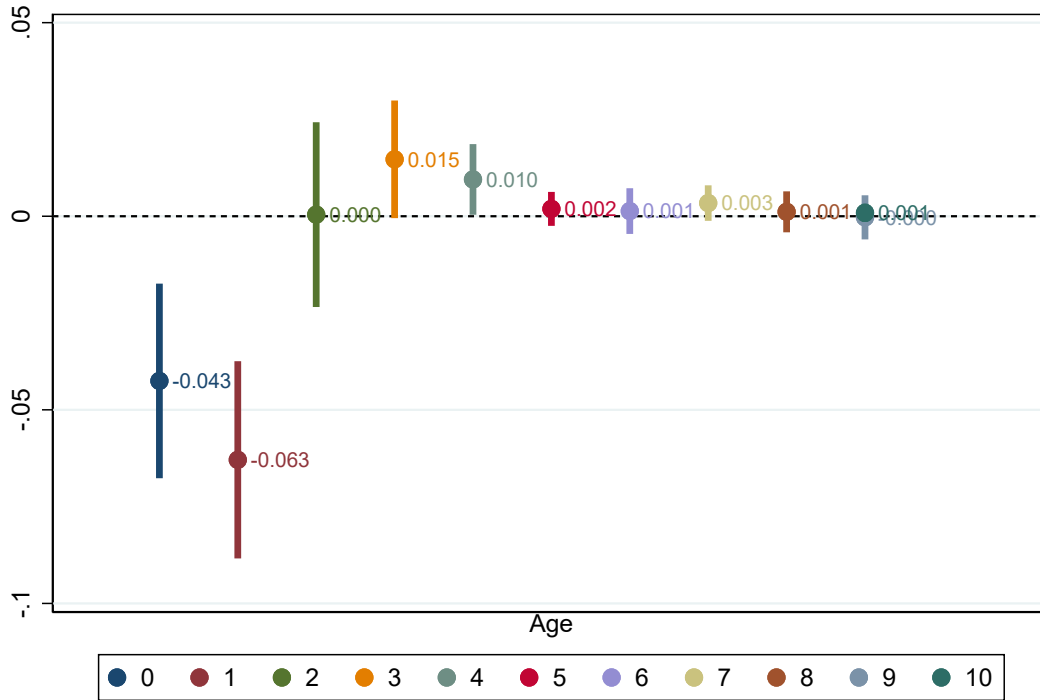
Notes: The figure presents the estimates of the specification described in equation (1) for children from 0 years to 10 years of age. Estimates include department and sample fixed effects. Department corresponds to the five departments in which the sample was collected and geographic sampling corresponds 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). 95 percent confidence intervals that consider clustered standard errors at the household level are provided in solid lines.