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IMPACTS OF FACILITATING INTEGRATION IN MIGRANT'S FERTILITY DECISIONS*

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Abstract

How does facilitating the economic integration of migrants change migrant's fertility decisions? We leverage a panel survey representative of Venezuelan irregular migrants in Colombia to compare the fertility decisions of eligible and ineligible households before and after a large migratory amnesty was launched in Colombia in 2018. The amnesty granted irregular migrants a labor permit and access to full social services. Our results suggest that the amnesty lowered the likelihood of having a child among program beneficiaries, possibly driven by better labor market opportunities for women and better access to family planning through health 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 the future picture is grim. Multiple factors, including climate change and conflict, are latent risk factors of continued forced migration. Gaining a better understanding of how refugees assimilate to their new communities is critical in shaping policy that addresses refugees' needs and those of their hosting economies. Some of the concerns raised by hosting governments refer to the fiscal burden imposed by refugees, as well as perceived national identity threats by natives. Yet, refugees can also vitalize hosting economies with imploding birth rates and unsustainable social security systems through inflows of younger, working- and childbearing-age migrants.

This paper examines how the broad amnesty offered by the Colombian government to Venezuelan refugees might have impacted their fertility. A priori, the impact of regularization programs 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 social programs, health care, and educational services -all of which should lower the price of child quality (e.g., [Bleakley and Lange, 2009](#); [Qian, 2009](#); [Becker et al., 2010](#)). On the other hand, regularization efforts allow migrants to access the formal labor market, raising women's opportunity cost of childbearing and child rearing (e.g., [Mincer, 2009](#); [DeFronzo, 2009](#); [Falasco and Heer, 1984](#)).

A handful of studies have examined the impact of immigration policy aimed at facilitating the assimilation of immigrants on their fertility choices, although in rather different settings. For instance, [Avitabile et al. \(2014\)](#) examine how the introduction of birthright citizenship in Germany impacted migrants' fertility choices. They document how the reform induced increased investments in child quality and reduced the number of children as predicted by [Becker and Lewis \(1973\)](#) in their model of quantity and quality trade-offs. In a similar vein, although distinguishing between fertility choices made by women at

their first pregnancy (extensive margin) and women who already have other children (intensive margin), [Lanari et al. \(2020\)](#) provide evidence of how a 2002 regularization law in Italy shaped immigrant fertility. The authors find that the reform raised the probability of having a first child by 6 percentage points, but had a negative (albeit not statistically different from zero) impact on these migrant women's subsequent childbearing. Finally, focusing in Spain, [Amuedo-Dorantes et al. \(2022\)](#) examine the fertility impact of a change in immigration policy granting temporary legal status to undocumented immigrants based on their offspring's nationality. The policy, enacted in a 2011 Royal Decree in Spain, recognized the ability for undocumented parents of eligible nationalities to become temporary legal residents if they had a Spanish child. As [Lanari et al. \(2020\)](#), the authors show that the decree increased the likelihood of having a first birth significantly likely to qualify for the granted amnesty, but had no significant impact on subsequent fertility. While informative, these studies have focused on the experience of European countries with immigrant groups that might not be classified as forced migrants, making it difficult to extrapolate their findings.

The focus of this study is Colombia—a country with extremely generous policies toward the Venezuelan migrant population fleeing the humanitarian crisis. Particularly, we examine the impact of the Permiso Especial de Permanencia (PEP) -a generous amnesty that the Colombian government offered in 2018 to approximately half a million Venezuelan irregular migrants in Colombia. PEP beneficiaries were offered work authorization and full access to social services for up to two years. The amnesty was followed by a 10 year potential extension for the same benefits in 2021 under a different program umbrella.

We examine how PEP impacted household fertility leveraging the information from two waves of the Venezuelan Refugee Panel Study (VenRePs). The VenRePS is a representative survey of unauthorized Venezuelan migrants residing in main urban centers in Colombia before the PEP was launched. ¹ Approximately, half of the households in the survey were

¹Bogotá, Medellín, Barranquilla and a fourth group of smaller cities.

eligible for the PEP program.

Using panel data on 1,346 households, we compare the propensity to have young children (conceived after the program launched) of PEP-eligible and non-eligible households before and after the program was launched in 2018. Specifically, we observe each household at three points in time: at baseline, as well as two, and three years after PEP roll-out. Our models include household-survey wave fixed effects to account for unobserved time-varying factors potentially shaping household fertility, as well as 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 of the PEP program lowering the likelihood of having children. Falsification tests confirm the lack of changes in the probability of having children conceived prior to the program's implementation. In addition, there is evidence of a clear impact immediately after the program's implementation that dissipates over time.

We also explore the mechanisms behind the program's fertility impacts, paying close attention to two potential explanations, in particular. One refers to improved access to family planning through health services, which might have contributed to lowering fertility. The second one includes access to more employment opportunities, which raise the opportunity cost of childbearing and child rearing. We show that households eligible for PEP enjoyed better health care access, as well as improved job opportunities, suggesting both mechanisms may have been at play.

Our findings are relevant for the design of immigration policies aimed at facilitating the assimilation of both economic migrants and refugees in host countries concerned about the implications that such policies might have on posterior immigrant fertility. We show that, in the case of Colombia, the regularization of undocumented Venezuelan migrants did not lead to increases in fertility but, rather, in reductions based on both improved access to health care and family planning services, as well as to the availability of improved

job opportunities that raise the opportunity cost of childbearing and child rearing.

Our study contributes to three strands of literature. First, it adds to a growing number of studies examining the impact of amnesties, regularizations, and various humanitarian programs on immigrants. For example, [Ginn \(2022\)](#) examine the impacts of refugee camps, [\(Miguel et al., 2022\)](#) explores the impact of shelter programs, and [\(Ozler et al., 2021; Altındağ and O’Connell, 2022\)](#) assess the role of cash transfers in welfare measured through food consumption, child well-being, food security, and livelihood coping. [Hussam et al. \(2022\)](#) examine the mental health value of job permits and [\(Amuedo-Dorantes and Antman, 2017; Amuedo-Dorantes and Bansak, 2011; Amuedo-Dorantes and De La Rica, 2007; COB, 1995; Chassambouli and Peri, 2015; Devillanova, 2017; Kaushal, 2006; Monras, 2018; Fallah et al., 2019; Bahar et al., 2021b\)](#) the effects of amnesties on native labor outcomes and developed countries.² Perhaps most relevant to our research is the study by [Ibanez et al. \(2022\)](#), who document positive impacts of the Colombian PEP amnesty on Venezuelan migrants’ consumption and labor income. However, except for a few studies focused on European countries’ experiences (e.g., [Lanari et al., 2020; Amuedo-Dorantes et al., 2022](#)), limited attention has been paid to the impact of amnesties and regularizations on immigrant fertility.

Secondly, we add to a vast literature examining how fertility is shaped by policy (e.g., [Lalive and Zweimüller, 2009; Milligan, 2005; Bailey, 2012](#)), although not focused on immigrant fertility nor immigration policy. Our focus is on the role of immigration policy and its impact on immigrant fertility. Few studies fall into this category (e.g., [Avitabile et al., 2014; Lanari et al., 2020; Amuedo-Dorantes and Arenas-Arroyo, 2021; Amuedo-Dorantes et al., 2022](#)); yet, none has explored how recent regularization efforts are impacting fertility in Latin America.

²A related literature studies the effects of migrant amnesties on crime in hosting communities. See [Baker \(2015\)](#) for the United States, [Mastrobuoni and Pinotti \(2015\)](#) for the European Union, and [Pinotti \(2017\)](#) for Italy.

Finally, by focusing on how PEP has impacted Venezuelan migrants' fertility, our study contributes to a broader literature on migrant assimilation (see for example [Abramitzky et al., 2012, 2014](#); [Pérez, 2021](#)). Given declining global fertility trends, especially in the developed world, as well as the growing incidence of forced migration, gaining a better understanding of how policy can shape immigrant assimilation is critical and well-warranted.

II INSTITUTIONAL CONTEXT: THE PEP AMNESTY

Colombia is the main recipient of Venezuelan migrants. According to data from the United Nations Refugee Agency by February of 2022, approximately 2.5 million migrants had arrived to the country with the flows dramatically increasing since 2016. This number is not inclusive of all irregular migrants who may have crossed without the required paperwork and may escape the radar of migration authorities. 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 decided to collect a survey to estimate the number of irregular Venezuelan migrants living in Colombia. The survey received the name of RAMV, Registro Administrativo de Migrantes Venezolanos. It was collected between January and April of 2018 in municipalities with the largest numbers of Venezuelan migrants (it was collected in 441 of the 1,122 Colombian municipalities). The registry was voluntary and largely advertised through local migrant organizations and the media. By the time it closed, roughly half a million migrants had registered.

II. B The PEP program – August-December 2018

In July of 2018, the president leaving office (Mr. Juan Manuel Santos) decided to surprisingly announce that all the migrants registered in the RAMV would be granted the opportunity to become regular migrants. The program received the name of Permiso Especial

de Permanencia (PEP). It was a generous regularization program offering a two-year residency permit inclusive of a job permit, access to SISBEN (the score used in Colombia to target social programs), and access to financial services. By granting migrants access to the SISBEN score, PEP was arguably enabling migrants to apply to any of the social programs offered in Colombia to vulnerable populations, including full health services through the subsidized health regime. PEP boosted the consumption and labor income of treated migrants [Ibanez et al. \(2022\)](#), and had negligible effects on the job market prospects of Colombian native workers in the short-term ([Bahar et al., 2021a](#)). By granting Venezuelans access to social programs and the formal labor market, PEP might have also profoundly impacted other household decisions, including migrants' fertility choices.

III BACKGROUND ON FERTILITY RESPONSES TO IMMIGRATION POLICIES

As noted earlier, there is a long-standing literature on the role of policy in shaping fertility outcomes through parental leaves (e.g., [Lalive and Zweimüller, 2009](#)), cash transfers (e.g., [Milligan, 2005](#)), or family planning programs (e.g., [Bailey, 2012](#)), among many other policy examples. Yet, this literature has not particularly focused on immigrant fertility, nor on the role played by immigration policy.

More recently, however, low fertility rates and longer life spans in the developed and developing world have increasingly captured the interest of policy makers on the role that immigration policy might play in ensuring the sustainability of generous old-age pension systems in immigrant-receiving economies. Immigration may alleviate the pressure placed by a growing number of retirees on the tax system and the workforce sustaining government-funded retirement programs through a larger workforce with higher fertility rates than natives (e.g., [Storesletten 2000](#)). While this impact might be limited in countries with relatively low immigration and low fertility rates, e.g., South Korea (World Bank), it may prove relevant in countries experiencing large immigrant inflows, as has been recently the case for Colombia.

It is generally agreed upon that immigrant fertility converges to native fertility in the destination country through various channels,³. Through the impact on migrant assimilation, researchers have tried to gain a better understanding of how various immigration policies may impact migrant fertility. For instance, Amuedo-Dorantes and Arenas-Arroyo (2021) examine how the intensification of immigration enforcement in the United States lowered the childbearing likelihood of likely undocumented women. Similarly, [Avitabile et al. \(2014\)](#) explore how the introduction of birthright citizenship in Germany in 2000 reduced immigrant fertility. Yet, very few studies have explored the impact that regularization processes might play on migrant fertility.

One exception is the study by [Lanari et al. \(2020\)](#), who examine how the regularization law approved in Italy in 2002 impacted immigrant fertility. The authors show that the Italian regularization increased the probability of having a first child, but had no significant impact on immigrant fertility beyond that. In a similar vein, [Amuedo-Dorantes et al., 2022](#) examine the fertility impact of a change in immigration policy granting temporary legal status to undocumented immigrants based on their offspring nationality. The policy, enacted in a 2011 Royal Decree in Spain, recognized the ability for undocumented parents to become temporary legal residents if they had a Spanish child under the age of 18. The authors find that the 2011 Royal Decree significantly increased fertility among individuals potentially affected by the reform, even though the impact was concentrated among first-time mothers.

In this study, we examine how the amnesty provided by PEP impacted fertility among Venezuelan migrants in Colombia. Understanding the fertility response to this program is key, as despite being potentially helpful, the traditionally higher fertility rates of immigrants, when compared to natives, is not without controversy. This is particularly the case

³Adsera and Ferrer (2014) summarize the three channels impacting immigrant fertility -namely, the selection channel, according to which migrants might have lower fertility rates than non-migrants to begin with; the disruption channel, which underscores fertility disruptions due to migration; and the adaptation channel, which emphasize the role played by assimilation.

in the presence of large migrant inflows over a relatively short period of time, as they can impose significant constraints on the host country's health care system and be viewed as a threat by natives. Despite its relevance, we still lack an understanding of the role that the amnesty offered by PEP played on fertility among Venezuelans seeking refuge in Colombia. This study is aimed at addressing this gap in the literature.

IV CONCEPTUAL FRAMEWORK

In the standard Beckerian framework, where the demand for children depends on a family's budget constraint (Becker, 1960), the enactment of the PEP program should have effectively lowered the cost of having a baby for eligible Venezuelan migrants. The lower per unit cost of having a baby would have resulted from legalized migrants' access to medical, educational, and childcare services, along with potentially higher wages earned once they regularize their immigration status. If we abstract from the opportunity cost of time (e.g. Hotz et al., 1997), the above mentioned 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).

Nevertheless, PEP also granted regularized migrants work permits, raising the opportunity cost of time spent childrearing. If we take into account time allocation decisions into the equation (e.g., Willis, 1973), the impact of the PEP program on eligible migrants' fertility becomes uncertain. Potentially higher wages brought about by legalization raise the opportunity cost of time spent in childrearing, inducing these migrant mothers to increase their labor supply and decrease their demand for children (Hotz and Miller, 1988; Heckman and Walker, 1990). Hence, the impact of legalization on fertility among eligible migrant women remains ambiguous, ultimately depending on the relative size of the substitution and income effects.

The ambiguity surrounding the impacts of the PEP program on eligible migrants' 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, as well as individual child quality, subject to a budget constraint that, generally, abstracts from labor market decisions. Relying on that model, [Avitabile et al. \(2014\)](#) and [Lanari et al. \(2020\)](#), among others, show that there is a trade off between quantity and quality. Specifically, for two different immigration policies -one benefiting immigrants' offspring (as in the case of the new German citizenship law) and one benefiting unauthorized immigrants (as in the case of the Italian amnesty), the authors document reductions in immigrant fertility that they attribute to reductions in the price of child quality. Yet, even then, impacts proved heterogenous. Specifically, [Lanari et al. \(2020\)](#) document how the lower price of child quality incentivized childless women to have a child given the lower per unit cost of childbearing, even though it lowered the overall number of children that eligible women would have.

In what follows, we explore how PEP, which provided access to the formal labor market as well as health care and social services, impacted fertility among Venezuelan migrants, as well as the channels potentially responsible for the observed fertility effects.

V DATA: THE VENREPS STUDY

Our main source of data is the Venezuelan Refugee Panel Study (VenRePs). VenRePs is a longitudinal study representative of the irregular population in Colombia. The survey was originally collected to examine the impacts of the PEP program on migrant's well-being and includes two waves of data collected between October and December of 2020 and 2021. The data is representative of four geographical areas including Bogota, Medellin, Barranquilla and other smaller cities comprising a fourth geographical area.⁴ The first three cities are large urban centers in Colombia. They are also the cities with the highest number of Venezuelan migrants in the country. Roughly half of the individuals

⁴This includes migrants interviewed in 9 municipalities including Cúcuta, Villa del Rosario, Cali, Cartagena, Riohacha, Maicao, Uribia, Valledupar, Santa Marta and Arauca.

interviewed in the VenRePs study were randomly selected from the RAMV survey. The other half were collected from data provided through snowballing sampling and referrals from local migrant organizations. All migrants in the survey had no passport, were 18 years old, and able to provide documents that proved they were born in Venezuela. Lastly, they had arrived to Colombia between January of 2017 and December 2018. In other words, they were irregular migrants living in Colombia at the time of the PEP implementation.

VI EMPIRICAL STRATEGY

The impacts of the regularization program on fertility decisions cannot be estimated by simply comparing households that were and were not eligible the PEP program. What precludes this comparison is that there are other potentially relevant unobservable differences between these groups of households that may also explain the divergence observed in fertility rates. For instance, migrants eligible for PEP may be better informed or more educated than other migrants. Those differences may also explain divergences in fertility rates between the two migrant groups. To address this challenge, we leverage the longitudinal data from the VenRePs study and estimate the fertility impacts of PEP by comparing changes in fertility rates in the same households before and after the program was implemented. We observe household fertility rates at three points in time, i.e., at baseline, on the day before the start of RAMV (April 5, 2018), and post-treatment through the two waves of the VenRePs study (2020 and 2021). Hence, we stacked the data to evaluate the impacts of being eligible for the program on the probability of having children of T years of age using the following specification:

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

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). $Child_{jdg}^T$ is the likelihood that household j has a child T years old ($T = 0,1,2,3$) and $\sum_{x \in X_{jgd}} \phi_x(x \times \gamma_t)$ is a term that captures non-parametric time changes in a comprehensive list of pre-migration covariates observed at the household level. The list of pre-migration control variables includes i) household head traits (gender, age, and education), ii) the household head's labor history (probability of being employed, type of job, probability of having 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). The model also includes fixed effects for each data wave (α_t) and for each household (α_j), as well as controls for department-wave trends ($\phi_{d \times t}$) for each of the five department where the survey was collected, and geographic sampling-wave trends ($\psi_{g \times t}$) for each of the four geographical regions at which the survey is representative. The latter include the three largest urban centers (Bogotá, Medellín, and Barranquilla), as well as a fourth region grouping nine smaller urban centers with high rates of Venezuelan migration. Finally, standard errors are clustered at the household level to account for intra-household serial correlation.

By including household fixed effects, we are effectively purging our estimates from time-invariant differences between treated and non-treated groups confounding the fertility effects of PEP. In addition, by flexibly accounting for non-parametric time changes in a rich set of pre-migration characteristics, we address dynamic differences between eligible and non-eligible migrants. As such, β_1 captures the change in fertility rates among eligible migrant households, relative to non-eligible migrant households, following the program rollout. While we present intent-to-treat (ITT) estimates, PEP take-up rates were close to 94% in our sample. Given the high compliance rate, the derived ITT estimates are not

likely to differ much from the local average treatment effects (LATE).

We examine the effects of the amnesty on the probability of having children less than 1, 1, 2, or 3 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 in or after 2019. In 2020 and 2021, we should be able to observe changes in the likelihood of having children less than 1, 1, or 2 years old, but we should not observe any change in the likelihood of having children 3 years old. We will use the latter as a falsification test.

VII FERTILITY IMPACTS OF PEP

Table 2 illustrates the results of estimating equation (1) in three panels. Panel A shows the results using baseline data and data from 2020 (the first wave of the VenRePs study), Panel B presents the results using data from baseline and 2021 (the second wave of the VenRePs study), and Panel C shows the results stacking the three waves of data. Each column in the table corresponds to a different regression evaluating the effects of being eligible for the PEP amnesty on the probability of having children less than 1 year old (column 1), 1 year old (column 2), 2 years old (column 3), and 3 years old (column 4).

We find consistent evidence of negative impacts of being eligible for the program on 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 eligible for PEP were 3.9 p.p. less likely to have children less than 1 year old, 7 p.p. less likely to have 1 year-olds, and 1.8 p.p. less likely to have 2 year-olds. As expected, being eligible for PEP had no significant impact on the likelihood of having 3 year-olds given the program's implementation timing. In addition, the results prove robust to the exclusion of control variables.⁵

Remarkably, when we restrict our sample to the data collected at baseline and in 2020 in

⁵Results are available from the authors upon request.

Panel A, we only observe a policy impact on the probability of having children of 1 year of age or less, which aligns with the amnesty rollout. In October of 2020, when wave I was collected, we would only be able to observe a policy impact on the probability of having 1 year-olds or less than 1-year olds, since the amnesty was enacted in 2018. 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 of age. Likewise, as we add the 2021 data in Panel B, we observe a policy impact on the probability of having children less than 1, 1 or 2 years old.

Finally, the results in Panels A and B also suggest that PEP's fertility impacts not only occur in the short-run, but also get larger one year after the program's rollout. The timing of these impacts conform with the usual delay in receiving the benefits from the program. For example, access to social services requires having PEP, as well as having a SISBEN vulnerability score. Getting such a score from public authorities may take time, as well as finding a formal job.

In sum, our main findings are consistent with the program rollout and robustly support the hypothesis that the PEP amnesty reduced household fertility. In the next section, we explore the different mechanisms that may be driving these effects.

VIII WHAT EXPLAINS THE REDUCTIONS IN FERTILITY?

As noted in the conceptual framework, there are two main channels through which the amnesty might have curtailed migrant fertility. Notably, the ability to work in the formal labor market might have increased the opportunity cost of childbearing and childrearing for both men and women *substitution effect*. In addition, by granting access to public health services and other government aid, PEP effectively lowered the cost of having children *income effect*.

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 enjoying improved access to governmental services and labor market opportunities. Specifically, the new outcome variables include: i) having a SISBEN score (used to target social programs in Colombia), ii) being enrolled in the subsidized health regime, iii) being a beneficiary of public cash transfers, iv) being employed, and v) having a job in the formal labor market. The first three outcomes are observed at the household level, whereas the labor market outcomes are defined at the individual level. Results are presented in Tables 3 and 4. All these outcomes are observed before, as well as after the program's rollout.

As shown in Table 3, PEP improved migrants' access to public assistance. In particular, eligible households were 49.2 p.p. more likely to have a SISBEN score, 11.4 p.p. more likely to have access to the subsidized health regime, and 33 p.p. more likely to receive government transfers when compared to non-eligible households. In sum, PEP-eligible households enjoyed greater access to health and safety nets than their non-eligible counterparts.

In addition, PEP-eligible migrants enjoyed better labor market opportunities than non-eligible migrants, as shown in Table 4. They were all more likely to have a formal job, even though only women appeared more likely to be at work, suggesting most male migrants were probably working in the informal market before PEP.

In sum, the results in Tables 3 and 4 support the notion that women eligible for the PEP reduced their fertility rates in response to improved access to public health services and government aid that lowered the price of child quality, likely inducing a quantity-quality trade-off [Becker and Lewis \(1973\)](#); [Avitabile et al. \(2014\)](#); [Lanari et al. \(2020\)](#), and as a byproduct of access to better labor market opportunities that raised the opportunity cost of childbearing [Willis, 1973](#); [Hotz and Miller, 1988](#); [Heckman and Walker, 1990](#).

IX CONCLUDING REMARKS

This paper examines the impacts of a large and generous amnesty enacted by the Colombian government in 2018 on the fertility of irregular Venezuelan immigrants. Our results largely suggest that the amnesty induced a profound reduction in the likelihood of childbearing -an impact observed immediately after the program's implementation. The effects, which do not immediately disappear, appear to be likely driven by improved access to labor market opportunities and public services by eligible migrants. Overall, the findings have profound implications for public policy, illustrating how facilitating the economic integration of migrants might also speed up the convergence of their traditionally higher fertility rates to those of natives.

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X TABLES AND FIGURES

Figure 1. PEP Program Roll-out

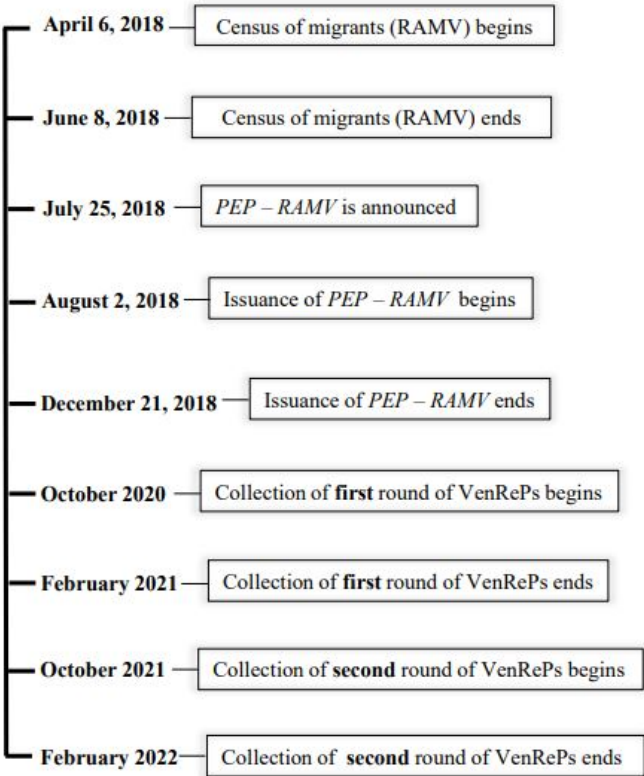


Table 1. Descriptive Statistics

<i>Panel A: Control Variables (baseline)</i>	Non - PEP			PEP		
	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
<i>Panel B: Outcomes (All waves)</i>						
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

Figure 2. Share of Venezuelan migrants and VenRePs sample

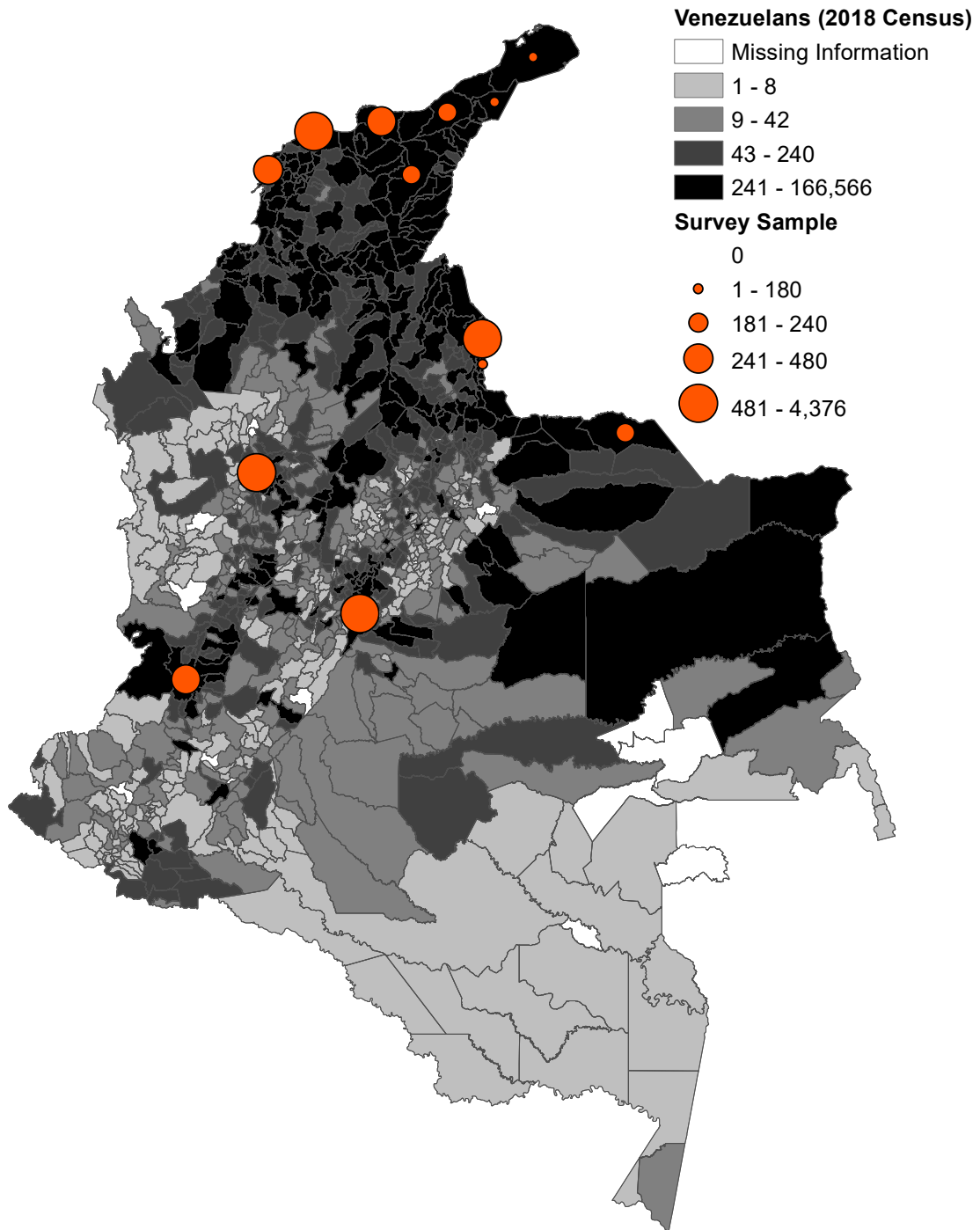


Table 2. Effects of the PEP Amnesty on Fertility Behaviors

	Dependent Variable: Likelihood of having children of			
	0 years of age (1)	1 years of age (2)	2 years of age (3)	3 years of age (4)
<i>Panel A: Estimates with baseline and wave I</i>				
PEP [=1]	-0.072*** (0.017)	-0.057*** (0.016)	0.007 (0.005)	-0.000 (0.003)
Observations	2,640	2,640	2,640	2,640
<i>Panel B: Estimates with baseline and wave II</i>				
PEP [=1]	-0.006 (0.013)	-0.084*** (0.018)	-0.043*** (0.016)	0.001 (0.003)
Observations	2,640	2,640	2,640	2,640
<i>Panel C: Estimates with baseline, wave I and II</i>				
PEP [=1]	-0.039*** (0.010)	-0.070*** (0.012)	-0.018* (0.009)	0.001 (0.003)
Observations	3,960	3,960	3,960	3,960
<i>Controls in all panels</i>				
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 specification described in equation (1). Panel A presents the results using data from the baseline and wave I, panel B shows the results using data from the baseline and wave II, and panel C presents the results stacking all the data together (baseline, wave I and 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 9 smaller urban centers in Colombia where migration from Venezuela is also prevalent). The 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 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 on access to Government Programs

<i>Dep Variable:</i>	SISBEN [=1] (1)	Subsidized healthcare [=1] (2)	Transfers [=1] (3)
PEP [=1]	0.492*** (0.021)	0.114*** (0.016)	0.330*** (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 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 9 smaller urban centers in Colombia where migration from Venezuela is also prevalent). The 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 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 on Labor Market Access

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

Notes: The table presents the estimates of specification described in equation (1) using variables on labor market as main outcomes. Panel A presents the results for all the 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 9 smaller urban centers in Colombia where migration from Venezuela is also prevalent). The 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 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.