LIFE OUT OF THE SHADOWS: THE IMPACTS OF REGULARIZATION PROGRAMS ON THE LIVES OF FORCED MIGRANTS

Ana María Ibáñez Inter-American Development Bank, USA

María Adelaida Ortega University of California Davis, USA Andrés Moya

Universidad de Los Andes, Colombia

Sandra V. Rozo World Bank, Development Research Group, USA

Maria José Urbina

World Bank, Development Research Group, UAE

Abstract

We examine the well-being effects of a regularization program offered to half a million Venezuelan forced migrants in Colombia. We collected data on more than 2000 such migrants and compared the well-being of those who arrived in Colombia before and after the date that defined program eligibility. Since this date was announced ex-post and was unknown to the public, we could credibly evaluate the program's impact. We find that program beneficiaries experienced large improvements in well-being, including consumption per capita (a gain of 48%), monthly labor income (an increase of 22%), and health status (an increment of 1.2 standard deviations). These positive outcomes largely stemmed from improved access to services, particularly the social protection system, subsidized healthcare system, and financial services. We also find that the fiscal costs incurred by the Colombian government for a regularized migrant household are *lower* than those for an irregular migrant household. (JEL: F22, O15, R23)

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Teaching Slides A set of Teaching Slides to accompany this article is available online as **Supplementary Data**.

"We would wake up at 5 am and by 5:30 am we were in the mountains. At 7 pm, when it was already dark, we were just finishing, covered in mud, wet because even if it rained we did not stop [...] that was the hardest part of this story. Living without a document is hard. You always lose."

Irregular (undocumented) Venezuelan migrant in Colombia, 2021.

1. Introduction

A dramatic rise in forced migration has made it one of the most pressing development challenges today. The number of forced migrants more than doubled in the last decade; by 2023, over 110 million people had been forcibly displaced worldwide, of which approximately one-third were international forced migrants (UNHCR 2023).¹ This trend, exacerbated by escalating conflicts and climate change, underscores the importance of addressing forced displacement through durable solutions to facilitate the recovery and socioeconomic integration of forced migrants and to support their hosts effectively (Moya and Rozo 2024).

We contribute to this area by examining the impact of a regularization program on the well-being of Venezuelan forced migrants in Colombia. Such programs are structured processes designed to confer specific rights and benefits to particular categories of international migrants, albeit for a finite period of time. These programs are typically enacted via administrative decrees or executive orders as a result of either economic or humanitarian motivations. They allow migrants who are residing without proper authorization in a country to regularize their status.

We focus on the short-term impacts of the *Permiso Especial de Permanencia* (PEP), a Colombian program to support the social and economic integration of Venezuelan forced migrants. In the last 7 years, 7 million Venezuelans have been forced to leave their country due to economic collapse, political turmoil, and a humanitarian emergency. They represent 19% of all forced migrants worldwide and constitute one

to thank IPA Colombia for their support in collecting the data for this project. Rozo is affiliated at CEGA, CESR and the Schaeffer Center from the University of Southern California, and IZA.

E-mail: anaib@iadb.org (Ibáñez); a.moya@uniandes.edu.co (Moya); adeortega@ucdavis.edu (Ortega); Corresponding author, sandrarozo@worldbank.org (Rozo); murbinaflorez@worldbank.org (Urbina)

^{1.} The overall number of forced migrants includes internally displaced persons (IDPs), refugees, asylum seekers, and other people in need of international protection. It does not include persons displaced by Russia's invasion of Ukraine, who are estimated at 5.9 million IDPs and 8 million refugees.

of the largest migration crises in the Western Hemisphere.² In fact, there are over 2.5 million such Venezuelans in Colombia alone. Through the PEP, the Colombian government regularized the status of 281,307 of these migrants, allowing them to hold formal employment and incorporating them into the preexisting social protection system, which provided access to social services and public safety nets.

PEP's features facilitate causal identification of its effects. First, the program was introduced unexpectedly, thereby isolating anticipatory decisions or ex-ante behavioral responses. Unknown to both government officials and forced migrants, expost eligibility was based solely on prior registration in a nationwide census of irregular Venezuelan forced migrants, the Registro Administrativo de Migrantes Venezolanos (Administrative Registry of Venezuelan Migrants, or RAMV for its Spanish acronym), that was administered between April and June of 2018. According to government officials who designed RAMV, they implemented the census to assess the number of irregular Venezuelan forced migrants in Colombia, not to precede or lead to a regularization program. Yet, unexpectedly in August 2018, Colombia's president announced that all Venezuelan forced migrants who had registered in RAMV could regularize their status by applying for PEP. Second, PEP did not have any eligibility requirements and was not paired with policies other than registration in RAMV, which was open to all Venezuelan forced migrants in Colombia. Third, unlike other contexts in which language and cultural differences explain many obstacles faced by forced migrants in host countries, Venezuelans and Colombians speak the same language and share similar cultures and traits. Thus, PEP provides a clean context to study the effects of regularization programs unmediated by a culture clash.³

To evaluate PEP's impact, we surveyed 2,232 Venezuelan forced migrant households that arrived in Colombia between January 2017 and December 2018, thus including migrants who arrived before and after RAMV. We designed the sample to be representative of cities with the largest share of Venezuelan forced migrants in Colombia: Barranquilla, Bogotá, Medellín (three of the largest cities), and a fourth "region" of smaller cities. Since forced migrants are a hard-to-reach population, we constructed the sampling frame for the survey using the RAMV census, referrals from other forced migrants, and databases of local migrant associations.⁴ The survey data enabled us to examine PEP's impact on three groups of outcomes: socioeconomic and health well-being, access to services, and labor market outcomes. We are most interested in the program's impact on the first dimension, socioeconomic, and health

^{2.} The Venezuelan population is counted in the forcibly displaced international figures of the United Nations Refugee Agency (UNHCR) but is categorized as a population of interest by UNHCR due to the political implications of calling Venezuelans refugees. The Venezuelan crisis is a large international crisis comparable to that of Syria (5.5 m refugees) and Ukraine (6.2 m refugees).

^{3.} Since 60% of all Latin and Central American countries speak Spanish, our results are relevant to the region. Moreover, since 74% of refugees are hosted in the Global South— most in neighboring countries that share a common language—our results are also relevant beyond the region.

^{4.} As shown in the analysis, forced migrants in these three data sources were otherwise similar across socioeconomic characteristics in Venezuela and in Colombia before the program began.

well-being; the latter two help us to discern possible mechanisms. Each dimension includes a series of individual outcomes and a summary index. The survey took place between October 2020 and February 2021. Our analysis thus provides a picture of PEP's short-term effects 2 years after its enactment.

Despite the advantages for causal identification that PEP's rollout offered, registration in RAMV and PEP was voluntary, so self-selection might have confounded the identification of effects. For this reason, our empirical strategy follows a fuzzy regression discontinuity design (RDD) that compares Venezuelan forced migrants who arrived before June 8, 2018 (and therefore could register in RAMV and later become eligible for PEP) with those who arrived shortly after that date (when RAMV registration had closed and who therefore would not become eligible for PEP). The validity of the empirical strategy rests on two facts: (i) that PEP was announced unexpectedly and its eligibility criteria were defined ex-post after RAMV had already closed and (ii) that forced migrants who arrived on either side of the RAMV cutoff date were otherwise similar. We demonstrate that there was no discontinuity in the number of Venezuelan forced migrants arriving in Colombia before or after June 8, 2018, meaning they did not move en masse to register in RAMV before the cutoff date. Likewise, the forced migrants arriving before and after the RAMV cutoff date were similar, and their baseline characteristics did not change discontinuously around the cutoff date. Estimates are largely robust to several sensitivity checks, including different polynomial orders, kernel choices, and bandwidth specifications.

Our main results suggest PEP had large and economically meaningful effects on the socioeconomic well-being of Venezuelan forced migrants in Colombia. The fuzzy RDD points to a sizeable and statistically significant positive effect of 1.65 standard deviations (sd) on the socioeconomic and health well-being summary index. When we unpack the effect on individual well-being outcomes, we find the program induced improvements of 48% and 22% in consumption per capita and labor income, respectively, and a positive effect of 1.2 sd on the health index.

To understand the mechanisms behind these results, we first analyze PEP's impact on Venezuelan forced migrants' access to services and labor market outcomes, two dimensions we had considered in our preanalysis plan. For the former, the results also point to positive and sizeable effects of 38 percentage points (pp) on the services summary index. This overall effect is explained by PEP's positive impacts on access to the Sisbén proxy means-testing system used to target social programs (57 pp), subsidized healthcare (27 pp), financial services (44 pp), and government transfers (22 pp). All these estimates are economically meaningful considering access was close to zero for the ineligible group. Nonetheless, to the extent that they are well below 100% access, the results also point to supply and demand constraints that prevent eligible migrants from enjoying full access to the array of services permitted by PEP.

For labor market outcomes, we estimate an effect of 25 pp on the summary index, an increase of 10.8 pp in labor formalization, and a reduction of 47 pp in selfemployment, although all results in this dimension are imprecisely estimated. This last result suggests the majority of regularized migrants remained in the informal sector. Moreover, the result aligns with other relevant work that documents negligible effects of the PEP program on Colombian workers (Bahar, Ibáñez, and Rozo 2021) and host nations' electoral outcomes Rozo, Quintana, and Urbina (2023).

Through a mediation analysis, we offer further evidence on the mechanisms behind the improvements in well-being. This analysis leverages the methodology introduced by Acharya, Blackwell, and Sen (2016) for estimating the Average Controlled Direct Effect (ACDE) of a treatment. In our context, the ACDE is the effect of providing forced migrants access to PEP after partialing out PEP's effect on access to services and labor market outcomes, the two domains we consider potential mechanisms. We find that the ACDE point estimate is not statistically significant at conventional levels and that the point estimate—relative to the Intent-to-Treat (ITT) point estimate falls by 50% when accounting for both mechanisms, 43% when considering only the access to services index, and 13% when factoring in the labor market outcomes index alone. These results demonstrate that access to services partially drives PEP's impact on well-being. When we assess the individual contribution of each variable in the indices, we find that improved access to the social protection system, healthcare system, and financial products are the most significant elements underlying PEP's impact on well-being. Furthermore, this analysis is consistent with perceptions from regularized migrants whom we interviewed. They reported that PEP's main benefit was access to healthcare followed by the likelihood of finding employment.

A relevant question remains regarding PEP's fiscal implications. We address this question by estimating a short term, fiscal cost-benefit analysis of the PEP program. The results suggest that the fiscal costs of hosting a regularized forced migrant household are *lower* than those of an irregular one. This is because improvements in consumption and income entail larger tax revenues. It is also due to the lower costs of providing full health services to regularized migrants, compared with providing emergency health services that are available to everyone.

All in all, this paper demonstrates that regularization programs are extremely powerful alternatives to improve undocumented migrants welfare in developing countries. Although most of the regularized migrants stayed in the informal sector in the short term, they induced lower fiscal costs, and helped improve the public budget. This happened mostly through increased consumption—which effectively raised VAT collection—and decreased healthcare costs. Likely, these effects will compound in the medium- to long-term as migrants integrate into the economy and society more deeply.

We derive insights that are relevant for countries around the world that host significant numbers of forcibly displaced people. Their relevance is underscored in Latin America, where 18 out of 26 countries have launched more than 92 regularization programs since 2000. A substantial portion of these have begun in the last 7 years, primarily in response to the Venezuelan crisis, as illustrated in Online Appendix Figure A.1. The PEP was the pioneer program opening the door to the Venezuelan diaspora in the region. In fact, many countries in the region—including Perú (2017), Brazil (2018), Ecuador (2019–2020), Chile (2018, 2021), Trinidad y Tobago (2019), Costa Rica (2022), Curaçao (2022), Guyana (2022), Panamá (2022), and Dominican Republic (2023)—have modeled their regularization programs after PEP. These programs resemble the PEP in its temporary nature and benefits (see

Online Appendix Figure A.2 for details). Yet, the PEP has a longer time-span and was later modified to allow the possibility to obtain a permanent residency.⁵ Beyond Latin America, similar programs have been implemented in North America and Europe. Notable examples include the Immigration Reform and Control Act (IRCA) and Deferred Action for Childhood Arrivals (DACA) in the United States, as well as Canada's Temporary Resident Permit. Europe has also seen a wave of recent regularization measures: Spain, Italy, Portugal, and Ireland all introduced such initiatives in 2020. This global trend reinforces that the insights gained from PEP are germane to other countries that have embarked on similar efforts or that are experiencing increasing inflows of forcibly displaced individuals. Further, since regularization programs differ as to specific components and benefits, an analysis of the mechanisms behind PEP's effects can also highlight key ingredients for success.

Our research primarily contributes to the body of evidence on the effects of migrant amnesties. Existing studies largely focus on the United States and programs like DACA, IRCA, NACARA, and TPS, as well as on European countries with an emphasis on Italy. These studies commonly report positive outcomes of such amnesties on migrants' labor market performance.⁶ There is also evidence that these amnesties have led to reductions in migrants' poverty rates and increases in consumption, as highlighted by Amuedo-Dorantes and Antman (2017) and Dustmann, Fasani, and Speciale (2017). These programs appear to influence migrants' fertility decisions as well, as observed in Kuka, Shenhav, and Shih (2019) and Lanari, Pieroni, and Salmasi (2020). However, the impact on education outcomes is more mixed. Some research, like Kuka, Shenhav, and Shih (2020), points to improved high school attendance and graduation rates, while others, such as Amuedo-Dorantes and Antman (2017) and Hsin and Ortega (2018), indicate a decline in education enrollment and attainment.

Other studies have examined the impacts of amnesties on hosts' labor outcomes, crime patterns, and fiscal accounts. They document small detrimental effects on workers who compete with newly legalized migrant workers (Bahar, Ibáñez, and Rozo 2021), crime reductions for program beneficiaries (Baker 2015; Mastrobuoni and Pinotti 2015; Pinotti 2017), and an increase in payroll tax revenues (Monras, Vázquez-Grenno, and Elias 2020).⁷

^{5.} While these programs vary in terms of the target population and elements, PEP's influence is evident. The work of Acosta and Harris (2022) highlights this, noting similarities in the design and implementation of these programs across different countries. Moreover, 60% of all Latin and Central American countries speak Spanish. As such, our results are extremely pertinent to the region. Additionally, countries in the Global South host 74% of refugees, mostly nations that neighbor the crises. As such, common language is also present in many other contexts.

^{6.} See Cobb-Clark, Shiells, and Lowell (1995), Kossoudji and Cobb–Clark (2002), Kaushal (2006), Amuedo-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).

^{7.} Particularly, Monras, Vázquez-Grenno, and Elias (2020) investigate the consequences of the legalization of around 600,000 immigrants in Spain. They find that newly legalized immigrants increased local payroll tax revenues by only 55% of what was expected.

Our study also relates to the body of work regarding the effects of PEP and Venezuelan forced migration flows. The majority of this work has examined the effects of Venezuelan migration and the PEP program on labor markets (Bahar, Ibáñez, and Rozo 2021).⁸ The closest study to ours is Bahar, Cowgill, and Guzman (2023), who focus on PEP's effects on firm creation by Venezuelan workers and find positive effects.

Our research advances the literature on migrant regularization in various ways. First, it explores the impacts of a major temporary regularization program that specifically targeted forced migrants in a developing country. This is particularly important because developing countries, which have higher informality and limited fiscal resources, host around 76% of the world's forcibly displaced populations (UNHCR 2022). Our findings reveal that short-term regularization of forced migrants is both feasible and effective in these environments. Second, we shed light on the implications of integrating forced migrants into existing social protection schemes. This approach has been suggested as a sustainable, development-oriented alternative to basic humanitarian aid (Moya and Rozo 2024). Third, we investigate PEP's effects on a broader spectrum of well-being, encompassing not only labor outcomes, income, and consumption, but also health status. Additionally, our secondary analyses assess the program's impact on integration, migration intentions, and prosocial behaviors, which have never been studied.⁹

Fourth, our work is pioneering because it collects data from a hard-toreach population, contrasting newly regularized migrants with those who remain undocumented—a control group seldom available for the study. Prior research has typically compared newly regularized migrants with hosts or previously regularized individuals. Fifth, we identify the primary drivers of well-being improvements resulting from the PEP program; to the best of our knowledge, this has not been done before. Indeed, our mediation analysis enables us to understand how regularization programs work and it identifies areas for future improvements. We demonstrate that PEP's impacts are largely driven by improved service access, which suggests that future efforts should focus on better integrating migrants into formal markets. Sixth, we present a straightforward cost-benefit analysis of the PEP program that illustrates its short-term fiscal benefits. Last, a crucial contribution is our focus on forced (as opposed to voluntary) migrants. This difference offers a unique perspective and facilitates greater understanding of policy alternatives to address forced migration, which is becoming increasingly consequential for all countries.

^{8.} A few exceptions include studies examining PEP's impact on crime outcomes (Ibáñez, Rozo, and Bahar 2020), fertility (Amuedo-Dorantes et al. 2024), and political outcomes Rozo, Quintana, and Urbina (2023), as well as Rozo and Vargas (2021) about the effects of Venezuelan migration on electoral outcomes.

^{9.} Moreover, previous research on the same policy have implemented different empirical strategies, such as shift-share instruments, difference-in-differences and has largely used administrative data. Our study is also novel in this sense because it employs a fuzzy-RD taking advantage of the PEP's roll-out and eligibility criteria and because of the survey we collected and sampling frame we designed for this purpose.



FIGURE 1. Timeline rollout: RAMV Census registration, PEP application, and data collection. The figure delineates pertinent temporal markers spanning 2017–2021 for this study. Specifically, it portrays the beginning and conclusion of the RAMV census, the announcement and execution timeline of the PEP program, and the implementation of data collection tailored to this research.

2. The PEP Regularization Program

Rationale. The Colombian government created the PEP program to foster the integration of Venezuelan forced migrants into Colombia's society and economy by providing access to formal labor markets and entrepreneurship, and by removing barriers to education, healthcare, and other public and private services.

Colombia first introduced PEP in two waves that targeted more affluent Venezuelans who had migrated through official immigration checkpoints and had lawful migratory status, but they were not allowed to work because of legal regulations. During these two waves, nearly 182,500 permits were issued to legal migrants. This number excluded the lion's share of Venezuelans in Colombia because the majority had migrated through illegal border crossings, overextended stays, or with a temporary document (*Tarjeta de Movilidad Fronteriza*) that only allowed short stays in border areas.¹⁰ To address the remaining large share of forced migrants without regularized status, the Colombian government introduced a third PEP, known as the PEP-RAMV.

Rollout. The PEP-RAMV began in August 2018 for all Venezuelans who had registered in the RAMV. As noted above, the migrant registry took place between April 6 and June 8, 2018—2 months before PEP's enactment. Recall that RAMV was not designed to grant work permits and was not advertized in that way: It was only meant to count the number of Venezuelan forced migrants who had not yet regularized their status. However, in July 2018, just a few weeks before leaving office, Colombian President Juan Manuel Santos unexpectedly declared that forced migrants who had registered in RAMV were now eligible for a new wave of the regularization program: PEP-RAMV, the focus of this work. For simplicity, we refer to PEP-RAMV simply as PEP. Figure 1 illustrates the timeline of the RAMV and PEP rollouts.

^{10.} The *Tarjeta de Movilidad Fronteriza* facilitated the movement of Venezuelans who lived near the Venezuelan–Colombian border and crossed regularly to shop, visit family members, and attend school, among other reasons. It permitted free movement only inside the border areas and no longer exists.

According to official records, 442,462 Venezuelan forced migrants registered in RAMV and 64% of them (281,307) subsequently applied for PEP. The RAMV registry was implemented in 441 of the 1,122 municipalities in Colombia, especially those with the highest number of Venezuelan migrants, and was advertised on social media, in local newspapers, and through local organizations.

Eligibility Criteria. To be eligible to apply for PEP, Venezuelan forced migrants only needed to: (i) have previously registered in RAMV; (ii) reside in Colombia by August 2018, when the PEP decree was issued; (iii) have a valid Venezuelan ID or other proof of Venezuelan citizenship; and (iv) have no criminal record or deportation order. Migrants had to submit applications online but PEP processing was free.

Benefits. PEP gave beneficiaries regular migratory status, a work permit, access to private services, and access to the social protection system. The latter allowed regularized migrants to register in the Sisbén social registry, the proxy means-testing system used to target social programs and to access subsidized healthcare, early childhood services, and cash transfers. The PEP was valid for 2 years.¹¹

In contrast, Venezuelan migrants without regularized status have access only to education and emergency health services, and they cannot work in the formal sector. This restricts them to informal jobs that are often characterized by low wages, poor working conditions, skill downgrading, and exploitation. Likewise, these migrants are ineligible for the social protection system, meaning they cannot access full health services or receive government transfers, and they are excluded from private services. Online Appendix Table A.1 describes the services provided to all Venezuelan migrants and the additional services and benefits PEP offers.

3. Data

We use data from the first wave of the Venezuelan Refugees Panel Survey (VenRePS) that was administered to 2,232 households of forced migrants in Colombia. This section describes the sampling frame, data collection process, and outcomes. The methodological design was informed by a qualitative study conducted through 42 semi-structured phone interviews with forced migrants who had and had not registered in RAMV. The purpose was to identify potential challenges to building a sample of RAMV and non-RAMV forced migrants and to understand the factors that had influenced their decision to register (or not) in RAMV and PEP. Information regarding the design of the sampling frame and data collection protocol is briefly mentioned below, while the overall results of the qualitative study can be found in Romero and Uribe (2021).

^{11.} In May 2021, the Colombian government announced an even larger regularization program with the same benefits for 10 additional years. The program was open to all PEP beneficiaries and to any migrant who had arrived in Colombia before January 2021.

3.1. Sampling Frame

It is challenging to design sampling frames for forced migrants because they are a vulnerable and hard-to-reach population. This is particularly true in Colombia, where there are no refugee camps and Venezuelan migrants are highly mobile and dispersed throughout the country. For this reason, we drew the sample to be representative of four geographical regions that host the largest share of these migrants, according to the latest population census of 2018: Barranquilla, Bogotá, Medellín (and their metropolitan areas)—three of the largest cities in Colombia—and a fourth "region" of smaller cities.¹² Online Appendix Figure A.3 illustrates the geographic distribution of the sample and the number of Venezuelan migrants in the 2018 population census, which can be taken as a proxy of the overall distribution of Venezuelan migrants in the country. The figure illustrates that the sample was collected in cities with a large presence of forced migrants.

Forced migrants in the sample fulfilled the following criteria: (i) were aged 18 or older; (ii) were the household head or partner; (iii) were undocumented upon arrival in Colombia; and (iv) arrived in Colombia between January 1, 2017 and December 31, 2018.

We constructed the sampling frame separately for RAMV and non-RAMV migrants. For the former, we drew the sample directly from the RAMV census, which had information on 442,462 Venezuelan forced migrants in Colombia. From the census, we drew a representative sample of 13,083 migrant households in the four regions, from which we randomly chose 1,135 households to survey. Since we had no administrative data for non-RAMV migrants, we constructed the sampling frame by combining databases shared by associations of Venezuelan migrants in the four regions with referrals from migrants who were surveyed as part of the RAMV sampling frame. The non-RAMV sampling frame included data from 12,554 non-RAMV households, 81% of which were obtained from the associations. Using this sampling frame, we surveyed a random sample of 1,097 migrant households in the same four regions: 527 households referred by the associations and 570 referred by other irregular migrants. As discussed below, we elicited key outcomes in each household from the household head and partner or another randomly selected adult member. This produced an overall sample of 3,896 forced migrants surveyed in 2,232 households, including 1,947 RAMV and 1,708 non-RAMV individuals.

To assess whether the non-RAMV migrants in both subsamples were comparable, Online Appendix Table B.1 reports data for migrants in each group according to reasons for migration and pre-migration socioeconomic characteristics. The data suggest that both groups were comparable and that those referred by migrant associations were not more vulnerable before migration. Of 15 characteristics analyzed, only the time of settlement in Colombia was statistically different between

^{12.} The fourth region includes Cúcuta, Villa del Rosario, Cali, Cartagena, Riohacha, Maicao, Uribia, Valledupar, Santa Marta, and Arauca.

groups. While this difference is mechanical (because RAMV migrants arrived earlier in general and likely referred other forced migrants who came around the same time), it is also negligible (less than one month). Moreover, in Online Appendix Figure C.1, we show that the arrival date did not correlate with an index constructed with baseline socioeconomic characteristics of migrants in our sample during our period of analysis.

We employed multiple exercises to address concerns related to biases introduced by the characteristics of forced migrants sampled through different sources. First, we checked that RAMV and non-RAMV migrants who arrived around the cutoff date were comparable based on a rich set of baseline observable variables (see Table 1). Second, we checked for the comparability of RAMV and non-RAMV referrals from migrant associations and the comparability of RAMV and non-RAMV referrals from other forced migrants (see Online Appendix Tables B.2– B.3). All the exercises confirm the internal validity of the empirical design because the vast majority of tests point to no statistically significant differences between groups.

3.2. Survey and Data Collection

Our survey took place over the telephone between October 2020 and January 2021. Originally, we planned in-person data collection but shifted to a telephone mode because of the COVID-19 pandemic. To ensure quality responses during phone interviews, we shortened the overall survey and some specific modules, and only the household head and partner responded to key modules (including labor and health). Absent a partner, another adult member randomly selected from the household roster responded to them.

The questionnaire had five main modules. The first posed standard sociodemographic questions for all household members. The second module elicited information on the RAMV and PEP registration processes, including whether each member had PEP (in any version), its issue date, perceived benefits, and reasons for registering/not registering in RAMV and PEP. Next, the questionnaire included a labor module following the design of the Colombian Labor Force Survey (*Gran Encuesta Integrada de Hogares*) to make it comparable to existing administrative data on monthly and weekly income; this module also collected data on labor history in Venezuela and Colombia. Fourth, the survey included a module on health and access to healthcare that included the EQ-5D-3L, a standardized scale used to assess health across various dimensions, including physical and mental health, via a Likert scale.¹³ The final module offered information at the household level on (i) migration, (ii) integration into Colombian society and connections with migrant networks, (iii) prosocial preferences, (iv) housing, and (v) expenditure and remittances.

Qualitative findings from focus groups prior to survey collection informed the survey design and data collection protocols. First, during the focus groups, forced

^{13.} The questionnaire has been adapted to different settings including Colombia and Venezuela, and it has demonstrated appropriate psychometric properties and validity. The Spanish-language version adapted to the Venezuelan population was administered to elicit severe symptoms of anxiety and depression.

Variables in STD	Knew job opportunity before migrating [=1]	Ever worked [=1]	Employed at private firm [=1]	Employed by gov. [=1]	Self-employed or employer [=1]	Written contract [=1]	Gap between last job and migration (months)	Years of educ. before migration	HH. Ven. parents or siblings [=1]	HH. Ven. partner or spouse [=1]	HH. Ven. others [=1]
	(1)	(7)	(5)	(4)	(c)	(0)	(\cdot)	(8)	(6)	(01)	(11)
$\mathbb{1}[T_i < \overline{T}]$	-0.322	0.098	-0.051	0.285	-0.189	-0.025	-0.122	0.456	-0.555***	0.135	0.140
	(0.163)	(0.153)	(0.182)	(0.168)	(0.198)	(0.165)	(0.135)	(0.180)	(0.148)	(0.152)	(0.156)
FDR q values	[0.486]	[1.00]	[1.00]	[0.686]	[1.00]	[1.00]	[1.00]	[0.145]	[0.001]	[1.00]	[1.00]
Observations left	483	597	465	480	427	604	489	436	536	620	561
Observations right	912	1254	939	1017	835	1289	1090	852	1134	1308	1245
Observations	4,177	4,177	3,895	3,895	3,895	3,895	3,872	4,177	4,177	4,177	4,177
Variables in STD	Migrated for	Friends/	Had	Owner of	Electricity in	Running water	Sewerage in	Female [=1]	Age (years)	Number of	Time in
	health reasons	family in	smartphone	dwelling in	Venezuela	ii	Venezuela			children	Colombia
	[=]	Col. [=1]	[=1]	Venezuela	[=]]	Venezuela					
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
$\mathbb{1}[T_i < \overline{T}]$	-0.107	0.044	0.156	-0.191	-0.072	0.080	0.007	-0.004	-0.113	-0.182	-0.077
- 1 -	(0.256)	(0.164)	(0.165)	(0.215)	(0.045)	(0.177)	(0.243)	(0.184)	(0.180)	(0.139)	(0.094)
FDR q values	[1.00]	[1.00]	[1.00]	[1.00]	[0.686]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Observations left	352	423	533	436	277	413	409	519	422	620	407
Observations right	909	803	1123	852	506	781	763	1059	805	1308	<i>6LL</i>
Observations	4,177	4,177	4,177	4,177	4,177	4,177	4,177	4,177	4,173	4,177	4,050
Notes: The table	lepicts the estin	nates of a shar	p RDD specifi	cation of pre-	migration and	pre-RAMV co	ontrols used as	outcome variab	les. These var	iables corresp	ond to all the
controls described	i in equation (1)	and specified	in our preanaly	vsis plan.							

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migrants reported that although Venezuelans and Colombians both spoke Spanish, there were important differences in everyday words and terms that made it difficult for Venezuelans to understand information from local authorities and NGOs. For this reason, Venezuelans reviewed the survey to ensure appropriate language usage. Second, forced migrants also reported high levels of mistrust because they feared deportation and were often targeted by scams and misinformation via text and social media. To build trust and enhance participation, all surveys were administered by Venezuelan enumerators, many of them forced migrants themselves. Furthermore, we worked with multiple Venezuelan migrant associations that disseminated information on the objectives and scope of the survey.

On average, the survey was administered over an average of 1 h and 40 min, and respondents received an incentive of 27,000 Colombian pesos (COP, about \$USD 9) for participating. As most forced migrants are excluded from the financial system, it was hard to deliver the incentives during data collection. For this reason, we used different delivery options including cellphone credit, supermarket vouchers, and electronic transfers. Online Appendix D discusses the data collection procedures in more detail.

3.3. Outcomes

Our analysis of PEP's impact focuses on three groups of outcomes: the socioeconomic well-being of forced migrants, their access to services, and their labor market outcomes. The first dimension, well-being, is the core of this article, while the latter two delve into potential mechanisms. Each dimension includes the individual outcomes described below and a summary index to increase efficiency and precision. Online Appendix E describes in detail the outcome variables as well as the specific methodology used in each case to construct the summary indices.

The three dimensions of outcomes are:

- 1. Socioeconomic well-being. Encompasses consumption, income, and a health status index. The logarithm of total annual consumption per capita and the logarithm of total labor income (comprising wage, extra payments, and revenue from independent work) are expressed in logs of million COP. The health status index is derived using the EQ-5D-3L scale, a validated tool for assessing various health dimensions. It is based on self-assessment responses regarding (i) mobility, (ii) personal care, (iii) daily routine self-sufficiency, and (iv) physical discomfort, each rated on a scale from 1 to 5. The summary index is estimated following Kling, Liebman, and Katz (2007) to summarize each dimension by standardizing each variable, calculating the average, and then standardizing this average using the control group's mean as a reference.
- 2. Access to services. Captures effective access to PEP's direct benefits and services that are not available to migrants without it, including registration in Sisbén, the proxy means-testing system, and access to subsidized healthcare, financial products, and government transfers. Since all the variables composing the index are dichotomous, the summary index is calculated as the average of

the four variables. The index itself can formally be interpreted as the percentage of access to the four services.

3. *Labor market*. Includes employment, holding a formal job, quality of employment (measured as the inverse of the willingness to find a different job), and having salaried employment (in contrast to being self-employed). As all the variables composing the index are dichotomous, we constructed the summary index as the average of the components. The index can be interpreted as the percentage of labor conditions met.

We defined the outcomes of interest following a preanalysis plan registered before data collection. Additionally, the preanalysis plan specified a rich set of exploratory outcomes related to integration, social preferences, and resilience to the COVID-19 pandemic. The analysis of PEP's effects on integration, migration intentions, and trust are presented in Online Appendix F, and the impacts of the PEP program on COVID-19 resilience are analyzed separately in Urbina et al. (2023).

3.4. Descriptive Statistics

Table 2 reports descriptive statistics on the summary indices and individual outcomes. The data are stratified between RAMV and non-RAMV migrants to describe the differences in well-being, access to services, and labor market outcomes between these two groups, the latter being ineligible for PEP.

The data in the table indicate that RAMV migrants were better off at the time of data collection across several dimensions of interest, with statistically significant and economically meaningful differences in most outcomes. First, RAMV migrants had higher levels of socioeconomic well-being, including higher consumption, income, and better health status. Second, RAMV migrants also had more access to services, with large differences across all outcomes. While this points to PEP's effectiveness, access to services was far from complete. For instance, at the time of the survey, 50% of RAMV migrants did not have access to Sisbén, 67% did not have access to subsidized healthcare, and 76% had been unable to access the financial system. Moreover, only 14% of these migrants were receiving transfers from the government. The data thus imply other barriers to migrants' service access, including weak institutional capacities; lack of information among migrants, civil servants, and service providers; and discriminatory practices, all of which accord with our qualitative findings. The data additionally suggest that RAMV migrants had more favorable labor market outcomes than non-RAMV migrants. This is evident from higher employment rates, higher likelihood of formal employment, higher likelihood of having salaried employment, and better job quality.

To summarize, the data in Table 2 highlight meaningful and statistically significant differences between forced migrants who were registered and not registered in RAMV of 0.45 sd in the socioeconomic well-being index, 27% in the access to services index, and 10% in the labor market outcomes index.

			М	ean difference	
	RAMV (1)	Non-RAMV (2)	P-value (3)	Bonferroni <i>P</i> -value (4)	Observations (5)
A. Socioeconomic Well-being					
Well-being (Index)	0.462	0.005	0.000	0.000	3,649
	(1.076)	(1.011)			
Consumption per capita (log)	1.615	1.362	0.000	0.000	3,437
	(0.558)	(0.469)			
Labor income (log)	0.483	0.347	0.000	0.000	1,813
	(0.257)	(0.210)			
Health status (Index)	0.060	-0.004	0.040	0.560	3,648
	(0.887)	(0.996)			
B. Service access					
Service access (Index)	0.302	0.033	0.000	0.000	3,650
	(0.279)	(0.093)			
Sisbén	0.505	0.021	0.000	0.000	3,632
	(0.500)	(0.144)			
Subsidized healthcare	0.329	0.014	0.000	0.000	3,602
	(0.470)	(0.117)			
Financial products	0.240	0.030	0.000	0.000	3,643
-	(0.427)	(0.170)			
Transfers from government	0.141	0.066	0.000	0.000	3,648
C C	(0.348)	(0.248)			
C. Labor outcomes					
Labor outcomes (Index)	0.346	0.240	0.000	0.000	3,649
	(0.321)	(0.272)			
Employed	0.636	0.557	0.000	0.000	3,437
	(0.481)	(0.497)			
Formal employment	0.107	0.001	0.000	0.000	2,257
	(0.309)	(0.029)			
Salaried worker	0.685	0.568	0.000	0.000	2,030
	(0.465)	(0.496)			
Quality of employment	0.444	0.321	0.000	0.000	2,044
	(0.497)	(0.467)			
	. /	. ,			

TABLE 2. Descriptive statistics: Outcome variables.

Notes: The table reports the descriptive statistics of the three groups of outcomes on which we examine the impacts of the PEP program.

4. Empirical Strategy

4.1. Threats to Validity

Despite the meaningful differences between RAMV and non-RAMV migrants, the descriptive analysis of the previous section cannot be taken to portray PEP's causal effects on well-being because of two main threats to identification. First, as Table 3 highlights, RAMV migrants arrived in Colombia earlier (by 7 months, on average), meaning that some differences could be due to longer assimilation time. Second, although PEP was introduced unexpectedly and RAMV was introduced previously without any announcement or expectation that it would be used to provide benefits,

			Mean	difference
	RAMV (1)	Non- RAMV (2)	<i>P</i> -value (3)	Bonferroni P-value (4)
A. Demographics				
Female [=1]	0.505 (0.500)	0.589 (0.492)	0.000	0.000
Age (years)	34.028 (10.193)	29.963 (11.612)	0.000	0.000
Number of Venezuelan children	1.461 (1.496)	1.657 (1.490)	0.000	0.000
Years of education before migration	13.516 (2.732)	12.9 (2.940)	0.000	0.000
Migrated for health reasons	0.108 (0.311)	0.097 (0.296)	0.265	1.000
Time in Colombia (months)	27.52 (10.952)	20.119 (8.828)	0.000	0.000
Friends or family in Colombia before migration	0.703 (0.457)	0.752 (0.432)	0.001	0.021
B. Employment in Venezuela				
Ever worked in Venezuela [=1]	0.984 (0.127)	0.971 (0.167)	0.013	0.273
Employed at private firm in Venezuela [=1]	0.61 (0.488)	0.583 (0.493)	0.111	1.000
Employed with Government in Venezuela [=1]	0.15 (0.357)	0.154 (0.361)	0.734	1.000
Self-employed or employee in Venezuela [=1]	0.188 (0.391)	0.171 (0.377)	0.210	1.000
Had a written contract in Venezuela [=1]	0.519 (0.500)	0.414 (0.493)	0.000	0.000
Knew of job opportunity before migrating [=1]	0.343 (0.475)	0.346 (0.476)	0.890	1.000
Gap between last job and migration (months)	1.054 (4.399)	0.739 (3.321)	0.018	0.378
C. Housing characteristics in Venezuela				
Had smartphone in Venezuela [=1]	0.647 (0.478)	0.503 (0.500)	0.000	0.000
Had a dwelling in Venezuela [=1]	0.866 (0.341)	0.864 (0.343)	0.890	1.000
Had electricity in Venezuela [=1]	0.994 (0.080)	0.994 (0.075)	0.759	1.000
Had running water in Venezuela [=1]	0.875 (0.331)	0.855 (0.352)	0.075	1.000
Had sewage in Venezuela [=1]	0.937 (0.244)	0.93 (0.256)	0.388	1.000
Lived in Venezuela with parents or siblings [=1]	0.421 (0.494)	0.465 (0.499)	0.008	0.168
Lived in Venezuela with partner or spouse [=1]	0.586 (0.493)	0.526 (0.499)	0.000	0.000
Observations	1,706	1,944	3,650	

TABLE 3. Descriptive statistics: Control variables.

Notes: The table reports the descriptive statistics of the control variables included in our main estimates.

registration in both was still voluntary. Hence, it is possible that RAMV and non-RAMV migrants were already different or that the decision to register was driven by unobservable characteristics correlated with migrant well-being.

The data in Table 3, Panels B and C, ease the first concern by demonstrating that RAMV and non-RAMV migrants were largely comparable across a range of pre-RAMV sociodemographic characteristics, including retrospective socioeconomic characteristics in Venezuela prior to migration and factors that might correlate with migration to Colombia. However, the data on reasons for not registering in RAMV and PEP suggest potential confounding factors (Online Appendix Tables G.1–G.2). For instance, the decision to not register in RAMV stemmed from a lack of information, a lack of proof of Venezuelan nationality, and an inability to take time off from income-generating activities. The decision to not register in PEP also included lack of information and loss of proof of RAMV registration, which was sent to registered email accounts. To the extent that these characteristics might correlate with the outcomes of interest and with unobserved factors such as connections to relevant networks and entrepreneurship, the simple comparisons between RAMV and non-RAMV migrants would produce biased estimates of PEP's effects.

4.2. Identification Strategy: Fuzzy RDD

To address these challenges, we identify PEP's causal effects through a fuzzy RDD.¹⁴ The fuzzy RDD exploits the discontinuity in the likelihood of receiving PEP based on the RAMV cutoff date. As described earlier, only forced migrants who had registered in RAMV were eligible to apply for PEP. The RAMV registry was open between April 6 and June 8 of 2018, meaning that forced migrants who arrived in Colombia after June 8 could not register in RAMV and thus were ineligible for PEP. Furthermore, the RDD takes advantage of the fact that PEP was unexpected, available to all migrants registered in RAMV, and not paired with other eligibility requirements or policies. This enables us to rule out behavioral and anticipatory effects as well as simultaneous treatments that have precluded the analysis of similar programs.

Specifically, the fuzzy RDD compares eligible and ineligible migrants on each side of the RAMV cutoff date under the following two-stage specification:

$$\mathbb{1}[PEP_i = 1] = \beta_1 + \beta_2 \mathbb{1}[T_i < T] + \beta_3 f(d_i) + \theta' X_{ij} + \gamma' Z_j + \varphi + \varepsilon_{ij}, \quad (1)$$

$$Y_{ij} = \alpha_0 + \alpha_1 \mathbb{1}[PEP_i = 1] + \alpha_3 f(d_i) + \omega' X_{ij} + \Psi' Z_j + \varphi + \mu_{ij}.$$
 (2)

Equation (1) models the likelihood of receiving PEP based on whether a Venezuelan migrant arrived in Colombia before the RAMV registry closed, while equation (2) models the effects on the outcomes of interest Y_{ij} as a function of the predicted likelihood of having PEP. $\mathbb{1}[PEP_i = 1]$ is an indicator variable that takes the value of one for migrants with PEP; T_i and \overline{T} are the date of arrival in Colombia and the

^{14.} The preanalysis plan proposed both the discontinuity design and reduced-form ITT and IV estimates. This article focuses on the RDD since it offers the strongest causal evidence.

date when the RAMV registry closed, respectively; and $\mathbb{1}[T_i < \overline{T}]$ is an indicator variable for whether the migrant arrived in Colombia when the registry was still open. Therefore, the treatment is equal to 1 for forced migrants who arrived in Colombia before RAMV closed, and who could register in RAMV and subsequently in PEP. As many PEP benefits are defined for the individual holder (e.g., the right to work), the PEP treatment variable $\mathbb{1}[PEP_i = 1]$ is defined at the individual level for the majority of outcomes. For other outcomes, however, the treatment is defined at the household level because access is legally defined at this level (e.g., access to Sisbén and government transfers), or because outcomes (e.g., consumption) were measured at the household level in the survey.¹⁵

The running variable in the fuzzy RDD is d_i , the distance measured in days between the migrant's arrival date and June 8, 2018 ($d_i = T_i - \overline{T}$). In turn, $f(d_i)$ is a local polynomial of the running variable, which is defined as a local-linear polynomial that allows the linear relationship to differ on both sides of the cutoff (before and after the RAMV closing date). Following Cattaneo, Idrobo, and Titiunik (2020), the optimal bandwidth choice for robust bias-corrected inference is estimated using the mean squared error optimal bandwidth (MSERD) and is estimated separately for each outcome. That is, each outcome has its own optimal bandwidth and thus a different number of observations. For robustness, we estimate all results using alternative functional forms of the polynomial and a range of different bandwidths.

The RDD includes a set of vectors X_{ij} and Z_i of baseline individual and household controls, respectively. Vector X_{ij} captures pre-RAMV individual controls including age, gender, and years of education before migration; labor history in Venezuela; time of settlement in Colombia; and the time gap between the last job in Venezuela and migration to Colombia. Vector Z_i includes pre-migration household characteristics including demographic composition (household size, composition, and number of children); access to public services; house ownership; whether the household had a smartphone; and variables related to the migration decision such as whether they had family or friends in Colombia, knew about job opportunities there before migrating, and whether migration was motivated by health reasons. φ is a vector of fixed effects for the sampling city and state of residence. Finally, ε_{ij} and μ_{ij} are the two error terms. In all specifications, we report the False Discovery Rate (FDR) q values to adjust for multiple hypothesis testing.

The migrant's arrival date in Colombia is a crucial variable for our identification strategy. The RAMV census collected this information and our survey confirmed it. Both surveys are extremely accurate and discrepancies are rare. Furthermore, our qualitative evidence suggests that migrants recall this date because it was a very salient event.

4.3. Validity of the Discontinuity

The top panel of Figure 2 illustrates the discontinuity in the probability of treatment for forced migrants who arrived in Colombia after June 8, 2018. The figure illustrates

^{15.} In any case, the treatment variable equals 1 when the main respondent has PEP and 0 otherwise.



FIGURE 2. Discontinuity in the probability of PEP treatment on June 8, 2018 (when RAMV closed). The top panel illustrates the weekly probability of treatment for all of the sample on a weekly basis (black line) and the number of observations in each week in the survey (gray bars). The bottom panel depicts the discontinuity in treatment probability spanning 200 days centered around June 8, 2018, when the RAMV census closed. The bars represent 95% confidence intervals of the mean value within each bin. The number of bins is determined using the IMSE-optimal quantile-spaced method employing a polynomial regression procedure. Additionally, a triangular kernel is applied in constructing the local polynomial estimator.

the average probability of PEP application for all migrants in the sample on a weekly basis. This figure confirms the existence of a sharp discontinuity in the probability of applying for PEP after June 8, 2018, when RAMV registration closed.¹⁶

Surprisingly, the figure also highlights that the likelihood of having PEP was not zero for migrants who arrived after the RAMV closed, even though the official PEP decree declared otherwise. This pattern is likely due to administrative and bureaucratic loopholes that might have let non-RAMV migrants apply for PEP. For instance, we ruled out that these discrepancies were driven by recall error of the arrival date: We compared the reported arrival dates in our survey with those reported on PEP applications and found they were the same in 98.2% of cases. Likewise, as noted above, results from the qualitative survey suggest that the arrival date was extremely salient for migrants, marking as it did the end of one life and the start of another. Finally, these discrepancies were also not due to misinformation or misreporting by respondents without PEP since we requested proof of PEP registration for anyone who reported applying for PEP.

For completeness, our main results include the full sample depicted in Figure 2. Robustness tests show the results are remarkably robust (both in magnitude and statistical significance) when the observations of these "defiers" are dropped (Online Appendix Figure H.1 and Online Appendix Tables H.1–H.3).

Figure 2 also plots gray bars that illustrate the density of forced migrants who arrived in Colombia each week. Visual inspection of the figure indicates no discontinuity in the number of individuals who arrived in Colombia before or after June 8, 2018. In addition, the McCrary test rejects the existence of any discontinuity in the density of the sample or manipulation by individuals (P = 0.96). This is expected because RAMV was not designed to regularize migrants and there were no public discussions, announcements, or expectations in this regard. Moreover, the survey data indicate that only 0.5% of respondents reported migrating in order to register in RAMV.

We also test for a discontinuity in the inflows and outflows of migrants between Venezuela and Colombia using data from the Colombian Migration Agency. While this data only include regular migration, it is a good proxy of total migration flows. Using this data, we do not observe evidence of a discontinuity of flows around the time when RAMV closed (Online Appendix Figures I.1 and I.2).

4.4. Validity of the Local Continuity Assumption

Table 1 examines whether migrants who arrived just before and after the RAMV cutoff date were similar across a range of individual and household characteristics. For

^{16.} The lower panel in Figure 2 illustrates the discontinuity in the probability of treatment, estimated as the average treatment take-up in each bin. This figure illustrates the discontinuity using a linear polynomial and confirms the existence of a large, robust discontinuity in the probability of treatment around June 8, 2018. At each point, the figure illustrates the mean probability of treatment in each bin and its 95% confidence intervals. Online Appendix Figure I.4 illustrates the discontinuity fitting a quadratic polynomial and also illustrates the existence of a large discontinuity around the RAMV cutoff date.

this purpose, a sharp RDD model was estimated with a set of pre-migration and pre-RAMV controls used as outcome variables. Only 1 out of 22 estimated coefficients is statistically significant for the robust RDD estimator. The conventional, bias-corrected, and robust estimators, illustrated in Online Appendix Figure I.5, further confirm the validity of the local continuity assumption. Moreover, Online Appendix Tables B.2– B.3 report the same exercise but restrict the sample of non-RAMV migrants obtained through referrals or migrant associations. The data in both tables confirm that the local continuity assumption holds regardless of the sample of non-RAMV migrants.

Finally, we present robust evidence that the socioeconomic characteristics of forced migrants are uncorrelated with their arrival date during our period of analysis. For this purpose, we first regress the arrival date on a rich set of baseline socioeconomic characteristics before the program onset (and RAMV registration). The results show that the covariates are not jointly statistically significant (Online Appendix Table C.1). Second, we create an index of baseline socioeconomic characteristics and plot them relative to the arrival date in Online Appendix Figure C.1. The figure illustrates that there is no clear correlation between both variables.

5. Results

Figure 3 previews the results by illustrating the standard graphical representation of the fuzzy RDD for the three indices that summarize the families of outcomes. For brevity, the RD plots of the individual outcomes are displayed in Online Appendix Figures J.1–J.3. The observed discontinuity at the cutoff represents the difference in each outcome around the RAMV closing date. The line illustrates the prediction that comes from estimating equations (1) and (2) through a two-step procedure and the respective 95% confidence intervals, while the dots represent the averages of each index in each bin.

A visual inspection of the four figures highlights sizeable differences in the indices of socioeconomic well-being and access to services between forced migrants who arrived before June 8, 2018 and could register in RAMV and be eligible for PEP, and those who arrived later and could not. It also illustrates PEP's positive effects on labor market outcomes, although results are less precise for this index than for the other two. The sections below detail the main results from the point estimates and multiple robustness tests.

5.1. Socioeconomic Well-Being

Table 4 reports estimates of PEP's impact on migrants' socioeconomic well-being. Column (1) reports the estimated coefficient for the summary index, while columns (2)–(4) report coefficients for the individual outcomes: labor income, consumption per capita, and health status.¹⁷ For each estimated coefficient, the table includes the

^{17.} The results for the health status index components as described in the "Outcome" section are shown in Online Appendix Table K.1.



FIGURE 3. Fuzzy RD plots with fitted local-linear polynomial. Each graph depicts the bias-corrected estimator 200 days around June 8, 2018, when the RAMV census closed, and the mean squared error (MSERD) optimal bandwidths. Confidence intervals are at the 95% significance level of the mean value on each bin. The number of bins is determined using the IMSE-optimal quantile-spaced method employing a polynomial regression procedure. Additionally, a triangular kernel is applied in constructing the local polynomial estimator.

	Well-being Index (1)	Labor Income (log) (2)	Consumption per Capita (log) (3)	Health Status (Index) (4)
A. Second Stage				
$\mathbb{1}[PEP_i = 1]$	1.655***	0.221**	0.481**	1.201**
	(0.462)	(0.107)	(0.181)	(0.460)
FDR q-values	[0.001]	[0.013]	[0.010]	[0.010]
B. First Stage				
$\mathbb{1}[T_i < \overline{T}]$	0.369***	0.401***	0.363***	0.362***
	(0.062)	(0.061)	(0.083)	(0.064)
Observations left	411	255	476	377
Observations right	731	569	1036	701
Observations	3,423	1,819	3,801	3,422
Mean values (non-RAMV refugees)	0.000	0.351	1.373	0.000
Outcome Level	Individual	Individual	Household	Individual

TABLE 4. Impacts of PEP on socioeconomic well-being.

Notes: Dependent variables: (i) well-being (index) is constructed using the outcome variables of columns (2)-(4) using the methodology of Kling, Liebman, and Katz (2007). This methodology involves standardizing each variable within the index, calculating the average, and then standardizing this average using the mean of the control group as a reference. (ii) Labor income (log) is the logarithm of the monthly labor income that includes wage, extra pay, and revenue from independent work in million COP; (iii) Annual consumption (log) is the logarithm of annual consumption per capita in million COP; and (iv) health status (index) is constructed using the methodology of Kling, Liebman, and Katz (2007) with the following variables. The health status index is derived using the EQ-5D-3L scale, a validated tool for assessing various health dimensions including: (a) mobility, (b) personal care, (c) daily routine, and (d) pain and discomfort on a scale of 1–5. All columns include department (Antioquia, Atlántico, Bogotá, and Norte de Santander) and sampling-city fixed effects. Individual controls include: age, gender, and years of education before migration. Labor history in Venezuela controls include: ever worked [=1], type of job, had a written contract [=1], and gap between last job and migration. Household controls in Venezuela include: number of children; household size; if had energy, water, and sewerage [=1]; owner of dwelling [=1]; and had smartphone [=1]. Migration decisions controls include: had family/friends in Colombia before migrating [=1], knew of job opportunities before migrating [=1], if the head migrated for health reasons [=1], and time of settlement in Colombia. Standard errors are reported in parentheses and FDR q-values are reported in brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

estimated standard error and the FDR q-value that adjusts for multiple hypothesis testing.

The results in Table 4 indicate PEP had positive and substantial effects on socioeconomic well-being, represented by a positive impact of 1.65 sd on the summary index. Further, the results point to statistically significant and economically meaningful effects across the three individual outcomes in this dimension. PEP led to a positive effect of 48% on per capita consumption, 22% on labor income, and 1.2 sd on the health status summary index for migrants with PEP, compared with non-PEP migrants. The RD plots for each outcome in this dimension are depicted in Online Appendix Figure J.1, which illustrates large differences in outcomes around the RAMV cutoff date.



FIGURE 4. RD estimates for the well-being outcomes using different bandwidths. The figure shows the robust bias-corrected point estimators and confidence intervals for different bandwidths measured in days around June 8, 2018. In black, manually inputed ad hoc bandwidths with a frequency of 20 days. In gray, optimal bandwidths according to different methodologies proposed by Cattaneo, Idrobo, and Titiunik (2020): (i) mean squared error (MSE), (ii) MSE for the sum of regression estimates (MSESUM), (iii) coverage error rate (CER), and (iv) CER for the sum of regression estimates (CERSUM). We run the same specification used in the estimates of Table 4. Confidence intervals are at the 90% significance level of the mean value on each bin.

To understand the size of PEP's effect on consumption, a useful benchmark comes from the impacts of conditional and unconditional cash transfers in different countries. Research in Colombia, Mexico, and Indonesia has found that conditional cash transfers had impacts of at most 15% on total consumption and 23.1% on food consumption (Attanasio and Mesnard 2006; Angelucci and Attanasio 2009; Cahyadi et al. 2020). Perhaps more relevant given our population of interest, cash transfers or vouchers to refugees in Turkey and Lebanon had effects ranging from 5% to 23% on aggregate consumption (Chaaban et al. 2020; Özler et al. 2021; Altindağ and O'Connell 2023). Although the context of each program and country is different, these comparisons highlight that PEP's impact on per capita consumption was two or even three times larger than those of the conditional and unconditional cash transfers cited here.

The above results are robust to different specifications. First, Figure 4 illustrates the estimated coefficients of the fuzzy RDD across a range of bandwidths, encompassing the different optimal bandwidths suggested by Cattaneo, Idrobo, and Titiunik (2020). For consumption and labor income, the figures illustrate that the effects are large but imprecisely estimated under tighter bandwidths. Yet, they become statistically

significant and remarkably robust across wider bandwidths. For forced migrants' health status, the results are robust to the four different types of optimal bandwidth choice employed, but the size effects are reduced under larger bandwidths.

Likewise, Online Appendix Tables L.4–L.7 report the estimated coefficients for the aggregate index and individual outcomes under different specifications of the RDD. These include the different optimal bandwidths proposed by Cattaneo, Idrobo, and Titiunik (2020) and different kernels under the local-linear polynomial, a polynomial of degree 0, and a quadratic polynomial. By and large, PEP's estimated effects on forced migrants' socioeconomic well-being are qualitatively robust under the different specifications. The only exceptions are the results for PEP's effects on health status, which were robust for 26 out of 36 different specifications. Finally, Online Appendix Figure J.4 illustrates the RD plots under the quadratic polynomial and the sharp discontinuity in outcomes, albeit less so for health status.

5.2. Service Access

In this subsection and the next, we focus on PEP's effects on access to services and labor market outcomes to provide a first approximation of the mechanisms behind the large impacts on migrants' well-being.

Table 5 reports estimates of PEP's impact on access to services. Column (1) reports the estimated coefficient for the summary index, while columns (2)–(5) report coefficients for the individual outcomes in this dimension: Sisbén enrollment, access to subsidized healthcare and financial products, and government transfers. As in the previous analysis, the table includes the estimated standard errors and the FDR q-value that adjusts for multiple hypothesis testing for each coefficient.

The results in Table 5 indicate PEP positively and substantially improved access to the different services defined by law. For instance, column (1) shows PEP had a large and statistically significant effect of 38 pp on the summary index. When we break down the overall effects by individual outcomes, the results further indicate sizeable and statistically significant effects on each dimension, including a 57 pp effect on the likelihood of enrollment in the Sisbén, a 27 pp effect on the likelihood of having access to the subsidized healthcare system, and a 44 pp effect on the likelihood of having a bank account or another financial product. Moreover, column (5) indicates that the likelihood of receiving government transfers was 22 pp higher for migrants who arrived before the RAMV closed and were therefore eligible for PEP. For a "visual" confirmation of the results, Online Appendix Figure J.2 includes the RD plots for individual outcomes in service access. The figures highlight sizeable discontinuities for all outcomes except for government transfers, which follow a downward-sloping linear trend according to the arrival date in Colombia.

All the above effects are substantial considering that access across all outcomes is close to 0 for ineligible migrants (as reported in the second-to-last row of Table 5) and considering that these are short-run effects that emerged less than 2 years after PEP's introduction. This means the Colombian government could expand social protection services in a short period to serve Venezuelan forced migrants, although this occurred

	Service access (Index) (1)	Sisbén (2)	Subsidized healthcare (3)	Financial products (4)	Transfers from government (5)
A. Second stage					
$\mathbb{1}[PEP_i = 1]$	0.382***	0.567***	0.267***	0.444***	0.221***
- 6 -	(0.079)	(0.127)	(0.118)	(0.110)	(0.097)
FDR q-values	[0.001]	[0.001]	[0.010]	[0.001]	[0.010]
B. First stage					
$\mathbb{1}[T_i < \overline{T}]$	0.338***	0.395***	0.353***	0.398***	0.414***
	(0.067)	(0.061)	(0.063)	(0.061)	(0.059)
Observations left	363	467	429	466	539
Observations right	655	990	764	992	1169
Observations	3,424	3,781	3,375	3,795	3,799
Mean values (non-RAMV refugees)	0.033	0.023	0.015	0.030	0.065
Outcome level	Individual	Household	Individual	Household	Household

TABLE 5. Impacts of PEP on service access.

Notes: Dependent variables: (i) service access (index) is the average of the following variables: (ii) Sisbén is an indicator equal to 1 if the respondent is enrolled in the vulnerability score system; (iii) subsidized healthcare is an indicator equal to 1 if the respondent benefits from subsidized healthcare; (iv) financial products is an indicator equal to 1 if the respondent benefits from subsidized healthcare; (iv) financial products; (v) transfers from government is an indicator equal to 1 if the respondent receives transfers from any official social assistance program. All columns include department (Antioquia, Atlántico, Bogotá, and Norte de Santander) and sampling-city fixed effects. Individual controls include: age, gender, and years of education before migration. Labor history in Venezuela controls include: ever worked [=1], type of job, had a written contract [=1], and gap between last job and migration. Household controls in Venezuela include: number of children; household size; if had energy, water, and sewerage [=1]; owner of dwelling [=1]; and had smartphone [=1]. Migration decisions controls include: had family/friends in Colombia before migrating [=1], knew of job opportunities before migrating [=1], if the head migrated for health reasons [=1], and time of settlement in Colombia. Standard errors are reported in parentheses and FDR q-values are reported in brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

with some limitations from both the supply and demand sides as discussed during the descriptive analysis.

PEP's estimated effects on access to services are also robust under different specifications, albeit less so for government transfers. Figure 5 illustrates that the estimated coefficients of PEP's impact are stable and remain statistically significant under a range of different bandwidths for the summary index and access to Sisbén, subsidized healthcare, and financial products. Moreover, estimates across all dimensions become more statistically precise as the bandwidth and number of observations increase (as expected). By contrast, the estimated coefficients for the effect on the likelihood of receiving government transfers dwindle as the bandwidth increases (from an estimated effect of 0.22 pp under the optimal bandwidths to an effect of 0.14 pp for bandwidths larger than 250 days). Furthermore, the results in Online Appendix Tables L.8–L.12 show that the estimated coefficients for the summary index and



FIGURE 5. RD estimates for the service access outcomes using different bandwidths. The figure shows the robust bias-corrected point estimators and confidence intervals for different bandwidths measured in days around June 8, 2018. In black, manually inputed ad hoc bandwidths with a frequency of 20 days. In gray, optimal bandwidths according to different methodologies proposed by Cattaneo, Idrobo, and Titiunik (2020): (i) mean squared error (MSE), (ii) MSE for the sum of regression estimates (MSESUM), (iii) coverage error rate (CER), and (iv) CER for the sum of regression estimates (CERSUM). We run the same specification used in the estimates of Table 5. Confidence intervals are at the 90% significance level of the mean value on each bin.

individual outcomes are remarkably stable and robust under different functional forms, optimal bandwidths, and kernels. Finally, Online Appendix Figure J.5 illustrates the RD plot under the quadratic local polynomial and highlights sharp discontinuities in outcomes—consistent with PEP's positive effects—for the summary index and individual outcomes, except for government transfers.

	Labor outcomes (Index) (1)	Employment (2)	Formal employment (3)	Salaried worker (4)	Quality of employment (5)
A. Second stage					
$\mathbb{1}[PEP_i = 1]$	0.253	0.261	0.108	0.471	0.119
	(0.148)	(0.248)	(0.107)	(0.263)	(0.248)
FDR q-values	[0.283]	[0.302]	[0.302]	[0.283]	[0.611]
B. First stage					
$\mathbb{1}[T_i < \overline{T}]$	0.364***	0.366***	0.405***	0.396***	0.395***
	(0.063)	(0.063)	(0.074)	(0.079)	(0.078)
Observations left	379	401	267	243	255
Observations right	706	710	635	526	574
Observations	3,424	3,424	2,048	2,034	2,048
Mean values (non-RAMV refugees)	0.266	0.561	0.001	0.570	0.324
Outcome level	Individual	Individual	Individual	Individual	Individual

TABLE 6. Impacts of PEP on labor market outcomes.

Notes: Dependent variables: (i) labor market outcomes (index) is the average of the following variables: (ii) employed is an indicator variable with a value of one if the respondent reports being employed and receiving a wage. This category includes both independent workers and family workers. (iii) Formal employment is an indicator variable with a value of one if the respondent is employed, reports having a pension fund, and has a written contract; (iv) salaried worker is an indicator variable with a value of one if the respondent is employed, reports having a pension fund, and has a written contract; (iv) salaried worker is an indicator variable with a value of 1 if the respondent's main occupation is a salaried job and 0 if it is classified as independent or self-employed; (v) quality of employment is an indicator with a value of 1 if the respondent does not wish to change their current job. All columns include department (Antioquia, Atlántico, Bogotá, and Norte de Santander) and sampling-city fixed effects. Individual controls include: age, gender, and years of education before migration. Labor history in Venezuela controls include: ever worked [=1], type of job, had a written contract [=1], and gap between last job and migration. Household controls in Venezuela include: number of children; household size; if had energy, water, and sewerage [=1]; owner of dwelling [=1], and had smartphone [=1]. Migration decisions controls include: had family/friends in Colombia before migrating [=1], if the head migrated for health reasons [=1], and time of settlement in Colombia. Standard errors are reported in parentheses and FDR q-values are reported in brackets. *** significant at the 1%, ** significant at the 5%, * significant at the 10%.

5.3. Labor Market Outcomes

In addition to PEP's positive effect on labor income documented in Table 4, this subsection analyzes the effects on more detailed labor market outcomes in order to understand how PEP supported the improvements in income and socioeconomic wellbeing. Table 6 reports the results of the fuzzy RDD for PEP's effect on an index that summarizes this dimension (column (1)) and on the individual outcomes, including the likelihood of employment, having formal employment, the likelihood of being a salaried worker, and employment quality (measured as the inverse of the willingness to find a different job).

By and large, after adjusting for multiple hypothesis testing, all estimated coefficients are imprecisely estimated and statistically insignificant. Nevertheless, they still point to PEP's economically meaningful effects across this dimension. In fact, although some of the effects are statistically significant at 10%, they are not robust

after adjusting for multiple hypothesis testing. For example, the results in columns (1) and (3) point to positive effects of 25 pp on the summary index and 10.8 pp on the likelihood of having a formal job. The latter effect is sizeable considering (i) these are short-term impacts, (ii) they emerged during the COVID-19 pandemic, and (iii) accounting for the high informality of Colombian labor markets. In 2020, 55% of the labor force in the main cities was employed in formal activities, and this rate was much lower (24%-31%) for the first three deciles of the income distribution. This means the short-term effect of regularization would correspond to 1/5 of the formalization rate for the average Colombian and 1/3 for those in the first three income deciles. Other results in Table 6 also point to positive and big but imprecisely estimated effects on employment quality (11.9 pp) and a gain of 47% in salaried employment. These effects are substantial, corresponding to 17% and 110% effects versus average rates for non-RAMV migrants.

Online Appendix Figure J.3 illustrates the RD plots where discontinuities in outcomes are visible for the summary index, employment, job formalization, and salaried employment—albeit indicating imprecise estimates. Furthermore, the robustness analysis confirms the main insights above: PEP's effects on different labor outcomes are not statistically significant under alternative bandwidths (Figure 6) or different specifications (Online Appendix Tables L.13–L.16), and they are not visually striking under the quadratic polynomial RD plot (Online Appendix Figure J.6). Yet, when looking more closely at the robustness tests for formal employment, the estimated coefficients are large and remarkably robust in magnitude (Figure 6).

5.4. Secondary Outcomes

In accordance with the preanalysis plan, we explored additional outcomes, focusing on PEP's effects on migration intentions, integration, and trust. The summarized findings presented in Online Appendix Tables F.1–F.3 indicate that, overall, many estimated coefficients lack precision and fail to achieve statistical significance after adjusting for multiple hypothesis testing. For instance, the results in Online Appendix Table F.1, which examine PEP's impact on migration intentions, do not reveal any statistically significant effects.

However, there are notable exceptions. Online Appendix Table F.2 unexpectedly demonstrates a decrease in the number of Colombian friends (7.4 pp) and a significant 62% reduction in reported instances of discrimination against Venezuelan migrants. Finally, Online Appendix Table F.3 suggests a positive effect on migrants' trust in other Venezuelans in Colombia (an increase of 88 pp).

6. Unpacking the Mechanisms of Impact

In this section, we present a mediation analysis to pinpoint the elements of the PEP program that were pivotal in producing positive effects on the well-being of Venezuelan forced migrants. This analysis leverages the methodology introduced by Acharya,



FIGURE 6. RD estimates for the labor outcomes using different bandwidths. The figure shows the robust bias-corrected point estimators and confidence intervals for different bandwidths measured in days around June 8, 2018. In black, manually inputed ad hoc bandwidths with a frequency of 20 days. In gray, optimal bandwidths according to different methodologies proposed by Cattaneo, Idrobo, and Titiunik (2020): (i) mean squared error (MSE), (ii) MSE for the sum of regression estimates (MSESUM), (iii) coverage error rate (CER), and (iv) CER for the sum of regression estimates (CERSUM). We run the same specification used in the estimates of Table 6. Confidence intervals are at the 90% significance level of the mean value on each bin.

Blackwell, and Sen (2016) for estimating the ACDE of a treatment. The ACDE is the effect of providing these migrants access to PEP after partialing out PEP's effect on access to services and labor market outcomes, the two domains previously evaluated as potential mechanisms.¹⁸ This approach provides a formal test of mechanisms

^{18.} This approach provides a formal alternative to the common approach of simultaneously controlling for the treatment and mechanisms, which often leads to post-treatment bias. See Acharya, Blackwell, and Sen (2016) for a detailed discussion.

at play, assessing whether service access and/or labor market outcomes were the primary drivers of PEP's impact on forced migrants' well-being, or if other unknown mechanisms contributed to this effect.

Intuitively, the mediation analysis is estimated by partialing out the effect of the two mediating indices on the well-being index, and then estimating the ACDE by regressing the de-mediated well-being index on PEP access. Formally, this is estimated through a two-stage model as follows:

$$Y_{ij} = \delta_0 + \delta_1 \mathbb{1}[T_i < \overline{T}]_{ij} + \delta_2$$
 Access to Services Index_{ij}

+
$$\delta_3$$
 Labor Market Index_{ij} + $\lambda X'_i$ + βZ_j + ε_{ij} , (3)

$$\hat{Y}_{ij} = \gamma_0 + \gamma_1 \mathbb{1}[T_i < \overline{T}]_{ij} + \theta X'_i j + \upsilon_{ij},$$

$$\tag{4}$$

which follows the same notation as the one used in our main analysis. In the first stage, Y_{ij} is the well-being index and $\mathbb{1}[T_i < \overline{T}]_{ij}$ is an indicator variable that is equal to 1 for Venezuelan forced migrants who were eligible for PEP based on their arrival date in Colombia. Access to Services Index_{ij} and Labor Market Index_{ij} are the potential mediator factors, and X'_i and Z'_j are the same matrices of individual and household covariates used in the main analysis, respectively. In the second stage, \hat{Y}_{ij} is the de-mediated well-being index $[\hat{Y}_{ij} = Y_{ij} - (\hat{\delta}_2 \operatorname{Access} to \operatorname{Services Index}_{ij} + \hat{\delta}_3 \operatorname{Labor Market Index}_{ij})]$ and v_{ij} is the error term estimated through bootstrapping.

Before turning to the results of the mediation analysis, we need to clarify two points. First, the mediation analysis, as formulated, is not designed for a fuzzy RDD. Consequently, our focus is on the ITT estimate of eligibility for the PEP program. As a result, the estimate of the ACDE (denoted as γ_1) cannot be directly compared with the estimates derived from the fuzzy RDD. For better comparability and to discern the inherent differences between forced migrants who arrived significantly before or after the RAMV closing date, we restrict the mediation analysis to observations within the optimal bandwidth of the fuzzy RDD. Second, γ_1 will only be a consistent estimator of the ACDE under the assumption of sequential unconfoundedness, which is difficult to fulfill in observational studies and even in experiments.¹⁹ For these two reasons, the results of the mediation analysis below should be considered suggestive.

The findings of the mediation analysis are summarized in Figure 7. This figure displays the ITT point estimate for eligibility for the PEP program and the point estimates for the ACDE, accounting for the access to services index, the

^{19.} The assumption of sequential unconfoundedness requires two separate conditions: (1) no omitted variable bias for the effect of the treatment on the outcome, conditional on pre-treatment confounders and (2) no omitted variable bias for the effect of the mediator on the outcome, conditional on the treatment and on pre-treatment and intermediate confounders. The first condition could be defended based on PEP's unexpected introduction and the discussion above on the validity of the Fuzzy RDD. However, the second assumption requires that the likelihood of accessing services or labor market performance—conditional on being eligible for PEP and on the set of baseline covariates—is not correlated with these migrants' unobserved characteristics, something that cannot be tested for.



FIGURE 7. Mediation analysis: Drivers of PEP causal improvement in migrant well-being. *ITT* presents the results of an ordinary least squares regression depicting the relationship between the well-being index and the dichotomous variable $\mathbb{1}[T_i < \overline{T}]$. This variable takes the value of 1 for forced migrants eligible for the PEP program based on their arrival date in Colombia. The model incorporates the covariates outlined in Table 4. *ACDE (Services)* reports the estimated coefficient representing the ACDE of the PEP program on the well-being index. This estimation is conducted while controlling for the access to services index in the second stage of the mediation analysis. Similarly, *ACDE (Labor)* presents the estimated coefficient of the ACDE while controlling for the labor market outcomes index in the second stage. Additionally, *ACDE (Services and Labor)* displays the estimated coefficient of the ACDE, accounting for simultaneous control of the access to services index and the labor market outcomes index in the second stage of the mediation analysis. The sample is restricted to the optimal bandwidth proposed by Cattaneo, Idrobo, and Titiunik (2020) in Tables 4–6. The lines represent 90% confidence intervals.

labor market outcomes index, and both indices together. The ITT point estimate is statistically significant and indicates that eligibility for PEP correlates with an increase in well-being by 0.13 sd. Importantly, the figure reveals that when the two proposed mechanisms are considered (either individually or concurrently), the ACDE is not statistically different from zero. This suggests that these two mechanisms primarily facilitate the improvement in well-being. Moreover, the results in Figure 7 imply a more pronounced role of improved access to services, relative to the gains in labor market outcomes. Specifically, the ACDE point estimate is 50% smaller than the ITT when accounting for both mechanisms, 43% smaller when considering only the access to services index, and 13% smaller when factoring in the labor market outcomes index alone. This differential in magnitude highlights the importance of service access in PEP's impact on well-being.²⁰

^{20.} In fact, the ACDE is marginally different from zero when only controlling for the labor market outcomes index.



FIGURE 8. Mediation analysis: Service access drivers of PEP causal improvement in migrant wellbeing. *ITT* presents the results of an ordinary least squares regression illustrating the relationship between the well-being index and the dichotomous variable $\mathbb{1}[T_i < \overline{T}]$. This variable takes the value of 1 for forced migrants eligible for the PEP program based on their arrival date in Colombia. The model incorporates the covariates outlined in Table 4. *ACDE* reports the estimated coefficient representing the ACDE of the PEP program on the well-being index. This estimation is conducted separately while controlling for each component included in the access to services index in the second stage of the mediation analysis. The ACDE results for Sisbén access are reported in the second line, subsidized healthcare in the third line, transfers from the government in the fourth line, and financial products in the fifth line. The sample is restricted to the optimal bandwidth proposed by Cattaneo, Idrobo, and Titiunik (2020) in Tables 4 and 5. The lines represent 90% confidence intervals.

To further assess the factors that explain PEP's positive effects on well-being, we estimate the ACDE after partialing out the individual components of the access to services index, namely, Sisbén registration, subsidized healthcare regime access, and financial product accessibility. Our findings, depicted in Figure 8, highlight that Sisbén registration, subsidized healthcare access, and financial product access are the primary factors driving PEP's impact on well-being. When these factors are considered, PEP's overall effect on well-being becomes statistically insignificant. This analysis is consistent with perceptions from regularized migrants interviewed in our survey, who reported that the main benefit of having PEP was access to healthcare, followed by the likelihood of finding employment (Online Appendix Table M.1).

The qualitative data reported in more detail in Romero and Uribe (2021) shed light on the different ways in which improved access to services explains PEP's positive effect on well-being in addition to the direct effects on income. First, forced migrants who participated in the focus groups and interviews reported that having access to these services brought "peace of mind" and enabled them to think beyond immediate and primary needs. This could have spurred changes in behavior and indirectly contributed to their socioeconomic well-being. Second, access to Sisbén and subsidized healthcare likely had a direct effect on their health and well-being, and thus enhanced their economic productivity and capacities. Third, these migrants also reported that access to these services reduced out-of-pocket health expenses, which can be thought of as an income effect on their consumption patterns. Fourth, they also reported that access to financial services was instrumentally valuable to their socioeconomic integration because it permitted them to pursue jobs in different sectors—including the gig economy—where workers need bank accounts to accept customer or employer payments. Finally, Sisbén enrollment was essential to receive monthly transfers from the "Ingreso Solidario" program established during the COVID-19 pandemic. These transfers were sizeable, corresponding to approximately 20% of the minimum monthly wage in Colombia, and likely bolstered forced migrants' well-being and resilience during the crisis.²¹

Detailed point estimates from our mediation analysis are presented in Online Appendix Table M.2. Additionally, we provide a breakdown of the access to services index into individual outcomes in Online Appendix M.3.

7. Cost-Benefit Analysis and Service Overcrowding

Beyond the large and positive effects on well-being documented above, a large-scale regularization program like PEP can also entail large fiscal costs and overcrowd public services and the labor market. To explore these issues, this section reports the results of a short-run cost-benefit analysis of PEP and discusses the extent to which it prompted negative effects on hosts and even on migrants without PEP due to overcrowding of public services and changes in the labor market.

7.1. Fiscal Net Cost

We first compare fiscal net cost for the Colombian government of hosting Venezuelan forced migrants (both with and without PEP) per year. We base this analysis on a simple accounting exercise that only considers the short-term costs and benefits and does not incorporate benefits that migrants can bring to host countries, including firm capital tax contributions (Clemens 2021) and firm creation (Bahar, Cowgill, and Guzman 2023), which are beyond the scope of our paper. Therefore, our analysis only considers a lower-bound estimate of PEP's potential revenue benefits and should be viewed as a first step toward understanding the costs and benefits in the medium- to long run of a large-scale regularization program. This analysis differs from those of other studies like Monras, Vázquez-Grenno, and Elias (2020) or Clemens (2021), since we do not try to estimate the fiscal effect of migration policy reforms but instead provide a picture of the costs and revenue that migrants with different statuses represent for Colombia.

^{21.} Urbina et al. (2023) analyze PEP's effects on migrants' resilience to the COVID-19 pandemic and Londoño-Vélez and Querubin (2022) study overall impacts of the "Ingreso Solidario" program.

We calculate the cost of providing forced migrants with access to public Costs. services and social assistance programs by groups according to: migratory status (PEP or non-PEP), age profiles, and rates of job formality. Because migrants without PEP also have access to some services, we calculate the costs of providing these different services for each group of migrants. Further, we estimate the costs for a reference migrant household, taking the average socioeconomic and demographic profiles of the households in the sample. For PEP migrants, we also consider the differences in costs according to whether adults in the household have a formal job since formal workers pay payroll taxes. Finally, we estimate these costs under two scenarios: (i) universal access to services as established by Colombian law and (ii) a more conservative scenario where service access rates are below 100%. For the latter, we use the service access rates reported in our survey—which is a better approximation of the reality on the ground. Importantly, according to Colombian law, any person, regardless of nationality and migratory status, who visits an emergency room must receive healthcare. Thus, emergency health services are universal in Colombia. Our qualitative interviews with migrants suggest that this is true in practice.

Fiscal Revenue. To estimate PEP's short-term fiscal benefits, we calculate the tax revenue paid by each group of forced migrants at the household level. We consider two sources of revenue: value-added taxes (VATs) and payroll taxes.²² For the VAT, we calculate average per-capita consumption for each group and impute this average consumption to every member of the representative household regardless of their age. This avoids making intra-household distributional assumptions on consumption while still being able to calculate individual net fiscal costs. Payroll taxes are paid by PEP migrants who have formal employment according to average income levels.

Fiscal Net Cost. Table 7 reports the net fiscal cost of a representative migrant household. Net cost is the difference between fiscal revenue and public expenditure.²³ Under the assumption of universal access to services, we find that a non-PEP migrant household has a net annual cost of \$USD 2,552 (column 1), while a PEP formal household (column 3) has an annual cost of \$USD -572, a decrease of 122%.

For our preferred results, we compare the net fiscal costs considering the observed service access rates and formality since these are arguably better representations of reality. Non-PEP households (column 5) have a net annual cost of \$USD 1,056, while the average PEP household (column 8) has a net cost of \$USD 610. This difference means that regularization represents a reduction of 42% in the net annual fiscal costs of hosting a Venezuelan forced migrant. The main driver of this change is an increase

^{22.} We do not consider income taxes since individuals in our sample have annual incomes below the income tax threshold. In the long run, this could be an additional source of revenue if the effects we document persist and forced migrants are better able to enter the formal labor market.

^{23.} Online Appendix Section N.1 describes in more detail the assumptions and sources of information for this analysis and reports the result, disaggregating the costs and benefits for each age group according to each service (Online Appendix Table N.1).

		Universal access 1	rates (De Jure)		In S.	ample Access I	Rates (De Facte) (0
	Without PEP (1)	PEP informal (2)	PEP formal (3)	PEP 10% formal (4)	Without PEP (5)	PEP informal (6)	PEP formal (7)	PEP 10% formal (8)
Net Fiscal Cost by HH me	smber:							
Head Partner	-282.64 -282.64	-51.20	769.77	30.89 30.89	-95.27 95.27	41.65 41.65	869.68 869.68	124.45 124.45
Child 0–5 years of age	-656.55	-466.35	-444.02	-464.12	-152.89	-88.63	-59.24	-85.70
Child 6-18 years of age	-1,330.69	-1,206.31	-522.88	-1,137.96	-712.50	-842.19	-812.80	-773.14
Total	-2,552.52	-1,775.06	572.64	-1,540.29	-1,055.93	-847.53	867.31	-609.94
Net Fiscal Cost by accoun	ting item:							
Fiscal revenue	498.00	693.59	3,109.59	935.19	498.00	693.59	2,448.50	935.19
Expenditure	3,050.52	2,468.65	2,536.95	2,475.48	1,553.93	1,541.12	1,581.18	1,545.13
Health care	1,651.63	921.48	989.78	928.31	898.11	545.75	585.82	549.76
Education	1,048.05	1,048.05	1,048.05	1,048.05	617.23	883.56	883.56	883.56
Social assistance	350.83	499.12	499.12	499.12	38.59	111.81	111.81	111.81
Notes: Quantities are presented are obtained from the VenRePs including insurance premium (Health (2020a)), and vaccinatic secondary, and tertiary) based o (2020); (v) early childhood pro Inflation and exchange rate dat	1 in 2020 \$USD. No s Survey; (ii) incom- UPC) from Ministr- on costs from Ministr- on UNESCO data vii grams data from Tri a are sourced from t	et Costs = Fiscal Rev e tax rates and payrol y of Health (2020b), try of Health (2019b) a World Development libín-Uribe et al. (202	<i>renue</i> —Expenditure II data from Melo-I the number of visit ; (iv) education cos Indicators Ministr; D); and (vi) conditi olombia.	2. The data sources Becerra et al. (2023 is from Ministry of sts, which include g y of Health (2018), i onal cash transfer p	for the variables an), and value-added Health (2019a), El overnment expendi and school meals (P rogram (Familias er	tax rates from E tax rates from E R premium from ture per student AE) and transpoi n Acción) data fr	income and con JIAN (2016); (iii) Manual Tarifari (weighted averag tr costs from Trib om Prosperidad	sumption data) health costs, o (Ministry of ce for primary, fn-Uribe et al. Social (2022).

TARTE 7 Fiscal net costs from regularization for a representative household.

in fiscal revenue. When we compare non-PEP households (column 5) with partially formal PEP households (column 8), fiscal revenue rises by \$USD 437 or 88%. This is because PEP households, even if they are fully informal, consume more and thus pay more VAT, while formal workers pay payroll taxes.

On the expenditure side, we find that the spending on healthcare falls from \$USD 898 to \$USD 550 for PEP households. This is because irregular migrants can get healthcare through the emergency room even for non-urgent conditions and these services are more expensive. With the PEP migrants get full health access inclusive of preventive and clinic consults (Online Appendix N.1). In contrast, spending on education rises for PEP households, from \$USD 617 to \$USD 883, because enrollment rates are higher for this group. These two effects cancel out and the total public expenditure for non-PEP and PEP households ends up being similar at around \$USD 1,500.

7.2. Discussion on Potential Overcrowding

A different potential cost of PEP is the possibility that it led to negative spillovers on hosts and non-PEP households because of overcrowding of health services and greater supply in the labor market.²⁴

For labor market dynamics, Bahar, Ibáñez, and Rozo (2021) have already found negligible effects of PEP on the employment and salaries of Colombian workers in the short term. This result may be explained by the small and insignificant effect we documented on the probability of transitioning to the formal sector. Future research should analyze if the same results hold for the medium- or long term and should explore the distributional implications of improved labor market access for regularized forced migrants.

For health services, negative spillovers would emerge if improved access to these services for PEP migrants overcrowded the health system and led to less access in practice or lower quality services, especially in the short run when funding and capacities are fixed. However, it is also possible that the negative spillovers on non-PEP migrants and hosts may be compensated if PEP migrants have improved health, make a better use of health services, and take-up more preventive public health programs (Ibáñez et al. 2021). This is in practice the case as illustrated in Online Appendix Table N.1, which shows that the health costs of regularized migrants is lower than the one of an undocumented migrants. This emerges because of the better use of the health system through a substitution of emergency to preventive services. Moreover, these fiscal savings will compound if migrants transition to better jobs and contribute by paying taxes that fund the health system. The measurement of these effects in the medium- to long term presents an exciting opportunity for future research.

^{24.} Since any child can enroll in public education in Colombia independent of their migratory status or nationality, the PEP program did not change access to education services for forced migrants. As such, PEP should not create crowding-out effects on the education outcomes of natives.

Taken together, this section suggests that PEP is a good investment as the net short-term fiscal cost is lower under the regularization program than under the alternative scenario of excluding forced migrants from the labor market and the social protection system while still providing access to basic services. Furthermore, our results also highlight how progress toward regularization and greater well-being for forced migrants makes sense. They can "pay it back" through payroll and value-added taxes as well as eventually (as Monras, Vázquez-Grenno, and Elias (2020) demonstrate) through income taxes and better use of the healthcare system. Yet, more work is needed to understand the extent of service and labor market overcrowding and their distributional effects.

8. Discussion

We analyze the short-term effects of a regularization program on the life outcomes of Venezuelan forced migrants in Colombia. The PEP program granted them a work permit and access to all government social programs, effectively integrating these migrants into Colombia's social protection scheme. We document three main effects. First, forced migrants who participated in the PEP program saw significant improvements in socioeconomic well-being. This improvement is evident in increased consumption, higher labor income, and enhanced health status compared to similar migrants who did not enroll in the program. Second, the gains in well-being primarily stemmed from improved access to essential services. These include registration in the Sisbén social stratification system, subsidized health services, and financial products. Third, we conducted a cost-benefit analysis from a fiscal perspective, comparing costs associated with households regularized via the PEP program to those of non-regularized households in the short term. Our findings show that regularized households are fiscally less burdensome due to PEP's positive impact on consumption and income levels as well as to the reduced costs of providing comprehensive health services compared to emergency services (which are accessible to all migrants regardless of immigration status).

Importantly, we could not identify the statistically significant effects of the program on formalization rates for forced migrants. Although our point estimates are close to 10 pp, they are also imprecise. This could be due to several factors. First, the pandemic and consequent economic crisis made additional job creation difficult. Second, forced migrants reported other hurdles that prevented them from securing formal employment, including the struggle to get a bank account. Third, some formal firms might not have recognized the validity of the PEP. Fourth, migrants might have been reluctant to move to formal employment as they would then have to pay taxes. (Previous work by Bahar, Ibáñez, and Rozo (2021) suggests a large premium for working in Colombia's formal sector, so the last hypothesis is unlikely). Fifth, there might not have been demand for workers in the formal sector. According to the Colombian Statistics Agency, informal employment accounted for roughly half of total employment in 2019. As such, formal jobs are probably available to individuals who have high education, are well-connected, and have been working in Colombia for many years. Forced migrants have fewer networks and—even if educated—face barriers to education certification and validation.

These findings suggest that in most developing countries like Colombia, where the informal sector is large,²⁵ deportations are not common, and firms face no penalties for hiring irregular migrants, informality is a viable option for many migrants independent of their status. Our analysis highlights the challenges involved in helping forced migrants to attain long-term self-reliance. Specifically, PEP granted complete rights to migrants, yet its short-term benefits largely came from improved access to services. This finding highlights the necessity of focusing on medium- to long-term strategies that empower migrants to secure formal and quality employment. To make policies like PEP sustainable, it appears that more migrants must enter the formal labor market.

While our analysis offers numerous contributions to the existing literature, it also features several notable limitations. First, to the extent that PEP households may have networks and support non-PEP households, our estimates of the program's impacts could represent a lower bound. As such, PEP's effects could be even larger than the estimated effects that already demonstrate the incredible success of this program. Second, since we collected data for forced migrants living in Colombia in 2018, our results are conditional to that group and exclude those who might have left the country. For the individuals in our sample, we find no evidence that having PEP changed their intentions to return to Venezuela, stay in Colombia, or migrate elsewhere (as illustrated in Online Appendix Table F.1), but it is worth noting that we do not observe the ones who left. It is difficult to predict the characteristics of these individuals because there is little information on irregular migrant flows in the region. Furthermore, both the most and least vulnerable could have reasons to leave, which would not allow us to assess the direction of bias from those who left. As such, it would be valuable for future research to study the effects of PEP (or a similar regularization scheme) on international migration flows and on who leaves and stays because of these programs.

Finally, PEP could have also induced other effects we did not appraise that pose extremely interesting questions for future research. For instance, PEP's announcement could have become a pull factor for other Venezuelan forced migrants in the medium-to long term, thereby increasing inflows to Colombia. Aggregate monthly figures of Venezuelan forced migrants arriving in Colombia (illustrated in Online Appendix Figure I.3) point to a spike in migration inflows in the short- and medium term after the PEP and ETPV (a bigger regularization program enacted in 2021) were announced. At this time, however, this is only suggestive evidence on the potential effects of these announcements; future research should address this question with more rigor. Moreover, although our assessment of PEP's crowding effects implies that they are not a first-order concern because irregular migrants already had full access to education and emergency health services before the program was implemented, a richer, more detailed analysis beyond the scope of this paper will be fertile ground for future efforts.

^{25.} In fact, in Colombia as in Latin America, informality accounts for at least 50% of all economic activity (Acevedo et al. 2021).

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Supplementary Material

Supplementary data are available at *JEEA* online.

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