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Cash-based interventions improve multidimensional integration outcomes of Venezuelan immigrants☆

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ABSTRACT

Since 2015, over 7 million Venezuelans have been forced to leave their homes, seeking refuge predominantly in neighboring countries across Latin America and the Caribbean. The displacement is typically accompanied by vulnerability and marginalization, yet there is a scarcity of actionable evidence on how to alleviate poverty among immigrants and refugees and facilitate their economic, political, and social integration. This study evaluates the impact of a cash-based intervention (CBI) on multidimensional integration outcomes of highly vulnerable Venezuelan immigrants, predominantly women, residing in Peru. Utilizing an original panel survey of beneficiaries and the staggered rollout of the program, which provided a one-time payment of 760 soles (approximately 190 USD or 74% of the monthly minimum wage), we estimate that the CBI increased the IPL-24 index – an overall measure of immigrant integration capturing several dimensions – by at least 0.12 standard deviations. Moreover, the CBI boosted self-employment by 2 percentage points and raised the intention to emigrate from Peru by 1.2 percentage points. Additionally, our heterogeneity analysis reveals that the benefits of the fixed-amount cash payment diminish significantly with the size of the household. We discuss how these findings inform the design of future CBI programs aimed at supporting vulnerable immigrant and refugee families.

1. Introduction

Over the last years, the Latin America and the Caribbean (LAC) region has witnessed an unprecedented increase in the number of people on the move. As of September 2023, about 7.7 million Venezuelans were compelled to leave their home country due to the deteriorating political and socio-economic situation (UNHCR, 2023). The vast majority of these displaced Venezuelans have found refuge in other LAC countries, with an estimated 1.5 million residing in Peru alone, making it the second-largest host after Colombia, which hosts 2.5 million Venezuelans (R4V, 2023). In the aftermath of this massive influx, many LAC countries are struggling to meet the additional demands on infrastructure and public services, and to facilitate the integration of these immigrants.

The limited data available on the socio-economic conditions of Venezuelans who have emigrated presents a concerning scenario. In Peru, for instance, 29.3% of Venezuelan migrants live below the poverty line, and 12.1% suffer from extreme poverty. This rate of extreme poverty is approximately three times higher than that of the native Peruvian population.[1] Amidst these challenging conditions, the Peruvian government and international organizations have initiated various programs to assist Venezuelan exiles. A key strategy employed in Peru, and popular in other regions for aiding vulnerable populations, is the use of cash-based interventions (CBIs). In 2021, the International

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[1] The INEI (National Institute of Statistics and Informatics in Peru) defines that the poverty line is the monetary equivalent of the cost of a basic consumption basket of food and non-food items, which for the year 2021 amounted to 378 soles (approximately 95 USD) per month per inhabitant. Persons whose monthly expenditure is less than this amount are considered poor. Similarly, the extreme poverty line, which considers only the cost of a basic food basket, amounted to 201 soles per person per month in 2021 (?). The percentage of Venezuelans in Peru living under the poverty line was based on our own calculations using INEI's ENPOVE II data, which targeted Venezuelans living in Peru and was collected between February and March 2022 (INEI, 2022b).

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Organization for Migration (IOM) alone fielded cash and voucher assistance programs in over 100 countries for 1.9 million people (IOM, 2022). These interventions aim to cost-efficiently alleviate poverty while empowering individuals with agency and dignity (Arnold, Conway, & Greenslade, 2011; Grosh, Del Ninno, Tesliuc, & Ouerghi, 2008; Handa et al., 2012; Ranganathan & Lagarde, 2012).

Cash-based interventions have been used to target a variety of groups, including children (Cepaluni, Chewning, Driscoll, & Faganello, 2022; Manley, Gitter, & Slavchevska, 2013; Millán, Macours, Maluccio, & Tejerina, 2020), households living in protracted crisis (Kurdi, 2021), food insecure populations (Berhane, Gilligan, Hoddinott, Kumar, & Taffesse, 2014), women (Blattman, Fiala, & Martinez, 2014; Bonilla et al., 2017; Gazeaud, Khan, Mvukiyehe, & Sterck, 2023; Harris-Fry, Saville, Paudel, Manandhar, Cortina-Borja, & Skordis, 2022), or persons with disabilities (Banks et al., 2017; Gooding & Marriot, 2009; Mitra, 2010). Across these diverse populations, CBI has been shown to improve well-being and mental health (McGuire, Kaiser, & Bach-Mortensen, 2022), human capital (Millán, Barham, Macours, Maluccio, & Stampini, 2019), education (Adato & Bassett, 2009; Baird, McIntosh, & Özler, 2019; García & Saavedra, 2017), food consumption (Hidrobo, Hoddinott, Peterman, Margolies, & Moreira, 2014), and health outcomes (Lagarde, Haines, & Palmer, 2007), among others. More recently, this literature has devoted its attention to vulnerable immigrants and refugees. Using a regression discontinuity design, Moussa et al. (2022) find a positive impact of multi-purpose cash assistance on health and educational outcomes of Syrian refugee children. Altındağ and O'Connell (2023) study a two-year unconditional CBI for Syrian refugees in Lebanon and find that it improves consumption, child well-being, food security, and reduces livelihood coping. Salti et al. (2022) compare outcomes of beneficiary groups with varying duration of receipt of cash assistance and find that the group of long-run cash recipients shows the largest benefits in household expenditures, housing accessibility and children welfare. Gupta et al. (2024) show that a one-time cash transfer of 1,000 USD to refugees in Uganda had benefits on households' consumption, asset values, business ownership and revenue, psychological well-being, and self-reliance after 19 months. While this research provides important insights into the CBI effects on consumption, education and health, little attention has been paid to the question to what extent CBIs are able to facilitate the incorporation of vulnerable immigrants and refugees into the fabric of the host society along social, political and psychological integration dimensions.

In this pre-registered analysis, we evaluate the effect of a CBI program implemented by IOM and its partner organizations on a comprehensive set of Venezuelans' economic and non-economic integration outcomes three months after cash receipt.[2] To gather comprehensive data on integration outcomes and a wide range of covariates, we conducted an original panel survey of beneficiaries, collecting information both before and after they received the cash. While the share of men and women among the Venezuelan immigrant population in Peru is balanced (INEI, 2022b), the vulnerable beneficiaries in our study population are 82% women, which hints at the gendered nature of the Venezuelan humanitarian crisis. Following Harder et al. (2018), we define integration as immigrants' knowledge and capacity to lead a self-sufficient and successful life in the host society and employ an adapted version of IPL-24 integration index to measure it. This index is a pragmatic, survey-based measure of immigrant integration and is increasingly adopted in a diverse set of contexts (Aksoy, Poutvaara, & Schikora, 2023; Alrababah, Masterson, Casalis, Hangartner, & Weinstein, 2023; Emeriau, Hainmueller, Hangartner, & Laitin, 2022; Knefel et al., 2020; Schilling & Stillman, 2021). In line with the above definition, the IPL-24 integration index puts emphasis on knowledge and capacity. Knowledge covers aspects such as fluency in the national language and ability to navigate the labor market, political system, and social institutions of the host country. Capacity focuses on the immigrants' economic, social, and mental resources that they can invest in their futures. This definition is distinct from the concept of assimilation, which requires immigrants to disassociate from their home country's culture and embrace some of the cultural behaviors prevalent in the host country. The index forms a comprehensive scale covering six components of integration measuring psychological, economic, political, social, linguistic, and navigational (i.e., the ability to navigate the host society's formal and informal institutions) integration.

To identify the causal impact of the CBI on Venezuelans' integration, we leverage the staggered roll-out of the program, which assigned 760 soles (approximately USD 190 or 74% of the minimum monthly salary)[3] to weekly cohorts of vulnerable beneficiaries. The staggered roll-out coupled with the collection of panel data enables a range of empirical strategies that rely on comparisons between refugees who have already received the CBI and similar refugees who receive it later. A pre-treatment vulnerability assessment allows us to condition on a battery of controls and thus reduce the risk that the impact assessment is confounded by structural variations in beneficiary characteristics. This empirical framework provides a credible impact assessment in a context where, due to the urgent needs of this highly vulnerable population, randomization of treatment assignments, or even randomized treatment delays, were not considered feasible for ethical reasons. Along with other recent methodological developments such as fusing experimental with observational data (Asimovic, Ditlmann, & Samii, 2022), we hope that this empirical framework is helpful to researchers who want to maximize learning while working with international and civil society organization serving vulnerable populations with urgent needs.

Across several complementary fixed effects (FE) specifications, we find robust evidence for a small yet positive and statistically significant effect of the CBI on overall integration, observed three months after the cash receipt. The lower bound of our estimates indicates an increase by at least 0.12 standard deviation units in the overall IPL-24 integration index. This effect is primarily driven by economic, social and navigational integration, while effects on political integration are small and not robust. Furthermore, we find that the CBI raised the rate of self-employment by at least 1.9 percentage points. This result is consistent with the finding that prior to the intervention, more than 90% of beneficiaries stated that they wished to start their own business. We also show that the rise in household incomes is larger for beneficiaries in self-employment compared to those employed, indicating that self-employment seems to be a successful economic strategy in this context. Finally, the CBI increases the intention to emigrate from Peru, presumably because the extra cash puts the long-term goal of returning to Venezuela or going to a third country into closer reach.

We provide two validation exercises to demonstrate that our main findings are robust: First, we leverage respondents from the representative, national ENAHO survey as an additional control group to validate the impact of the CBI on employment, self-employment and discrimination, and find remarkably similar effect sizes. Second, we employ the sensitivity analysis of Cinelli and Hazlett (2020) to gauge how large a putative confounder would have to be in order to change our main results. We find that the main results are robust to even strong confounding.

Our findings contribute to several strands of literature. First, our program evaluation adds to the growing CBI literature. Focusing on LAC countries, Rawlings and Rubio (2003) show that conditional cash transfer programs in Brazil, Mexico, and Nicaragua were successful in the accumulation of human capital in low-income families, though questions about their broader impact and sustainability remain. Attanasio, Battistin, Fitzsimons, and Vera-Hernandez (2005) demonstrate that a government-run cash transfer program in Colombia increased child

[2] The pre-analysis plan is available at https://osf.io/7zqny.

[3] Based on the latest national monthly minimum salary increment, which was implemented on May 1st, 2022 (Peruvian State, 2022).

nutrition, school attendance, and health status. Focusing on Syrian refugees in Lebanon, recent research showed how CBI strengthens consumption, education and health (Altındağ & O'Connell, 2023; Moussa et al., 2022; Salti et al., 2022). We go beyond these studies by documenting that the benefits of cash assistance for refugees extend to broad impacts for a range of economic and non-economic outcomes, including navigational, political and social integration.

Second, our research adds to the few empirical studies that seek to understand the humanitarian crisis following the political and economic crisis in Venezuela. Previous studies have mostly focused on the political and economic consequences of Venezuelan arrivals on the host country, including Colombia (Caruso, Canon, & Mueller, 2019; Rozo & Vargas, 2020, 2021), Ecuador (Olivieri, Ortega, Rivadeneira, & Carranza, 2022), Brazil (Ryu & Paudel, 2022; Sant'Anna & Shrestha, 2023) and Peru (Ajzenman, Dominguez, & Undurraga, 2022; Morales & Pierola, 2020). However, despite the extent of the Venezuelan exodus, there is, to date, limited quantitative research on the living conditions of Venezuelans that have emigrated and even less on the effectiveness of support programs. One exception is Groeger, León-Ciliotta, and Stillman (2024) who study both the effect of Venezuelan immigration on Peruvians' labor market outcomes as well as Venezuelans' perceptions about discrimination. They show that over a third of Venezuelan feels discriminated against in Peru and, using a shift-share design, find that this feeling of discrimination decreases with the size of the informal sector. We add to this literature by providing evidence on the effectiveness of CBIs as scalable and actionable policy tools in mitigating the economic impact of forced displacement and strengthening social cohesion between refugees and host societies.

Third, several studies on refugee integration document that initial conditions and early interventions have a disproportionally large effect on refugees' long-term integration outcomes (Åslund & Rooth, 2007; Hainmueller, Hangartner, & Lawrence, 2016; Müller, Pannatier, & Viarengo, 2023). This phenomenon is sometimes referred to as the early "integration window" (Marbach, Hainmueller, & Hangartner, 2018). Against this background, finding evidence for benefits of CBI across multiple integration dimensions for immigrants who have been residing for more than three years in the host country suggests that in the context studied here, this "integration window" is still open even several years into immigrants' integration trajectory.

We proceed as follows. We contextualize our study by providing background on the conditions of Venezuelans living in Peru in Section 2. Section 3 describes the survey design and data collection. We introduce the methodology in Section 4 and present results in Section 5, followed by the aforementioned robustness checks in Section 6. Section 7 concludes.

2. Background

Since the early 2010s, Venezuela has suffered from a prolonged crisis characterized by a severe economic decline, a political shift towards an authoritarian regime and a collapse of public services. These intertwined factors contributed to deteriorating public health provision, food security and nutrition (Doocy, Ververs, Spiegel, & Beyrer, 2019; Grillet et al., 2019; Page et al., 2019), and led to an unprecedented Venezuelan exodus. When displaced Venezuelans started arriving in Peru in 2015, they were welcomed by liberal policies. Peru introduced a temporary stay permit (*Permiso Temporal de Permanencia*, PTP), allowing Venezuelans to enter and work in the country regularly. The program expired in 2019 and was replaced with a humanitarian visa program granting Venezuelans the right to work until their application was processed. The humanitarian visa also provided access to healthcare and social services. However, practical obstacles and the need to provide official identification, which for many Venezuelans is difficult to obtain, made it more difficult to receive an official permit and the right to work. Due to these obstacles, applying for asylum became for many the only viable pathway to obtaining a legal residence and work permit. This was also reflected in the number of asylum applications, which soared when the PTP program was phased out. However, the length of the application process – up to two years – and barriers to applying meant that many Venezuelans in Peru remain in Peru irregularly (Ble et al., 2020; R4V, 2022).

The Peruvian government made several attempts at regularizing Venezuelans, with mixed success. In 2020, Peru issued the temporary stay permit license (*Carné de Permiso temporal de Permanencia*, CPP) for Venezuelans already present irregularly. Between 2021 and 2022, additional regularization programs delivered 233,000 temporary permit cards to Venezuelans and almost 28,000 humanitarian residence permits. The need for applicants to provide official documentation limited the programs' effectiveness, which is reflected in the fact that over a third of Venezuelans find themselves irregular in Peru and cannot access the labor market legally (INEI, 2022b). As a consequence, 80.8% of employed Venezuelans work without a contract (INEI, 2022a). These informal work situations likely exacerbate the vulnerability of this population. Surveys show many of their basic needs are not met. For example, 73% of Venezuelan immigrants mention health insurance and 55.8% access to medical care as most urgent needs, followed by food (45.6%) and employment (40.3%) (INEI, 2023). It is helpful to keep this dire context in mind when considering the potential impact of a one-off cash payment.

3. Study design and data

3.1. Survey and implementation schedule

For this research, we collaborate with the IOM to assess the integration effects of its CBI program in Peru. To select potential beneficiaries, the IOM can draw from its database containing the contact information of Venezuelan immigrants who might be living under vulnerable conditions. The majority (88.3% of the sample) have been registered by IOM. A small share of additional contacts (11.5%) are provided by partner organizations located across Peru, and a few respondents had been referred by the Peruvian government (0.2%).

IOM implemented the CBI program as follows. IOM contacted potential beneficiaries listed in their database on a weekly first-come–first-served basis for a vulnerability assessment survey (VAS), which helps the organization determine whether an immigrant qualifies for assistance. IOM only contacted potential beneficiaries who had not received assistance through other programs from IOM or another organization. Based on a set of standardized questions, IOM calculated a VAS score, which constitutes a guideline that was used to decide if potential beneficiaries are eligible for CBI assistance. However, IOM caseworkers did not employ a fixed, pre-defined VAS score to decide on CBI eligibility. Instead, the caseworker made the final decision based on the VAS score and by taking the particular context of each household into account.[4] Shortly after the VAS, the pre-treatment interview took place with assigned beneficiaries.[5] After the pre-treatment interview and prior to the cash payment, IOM conducted a second review of each case to confirm that the respondents were indeed eligible for the CBI. If the review process revealed that the respondent or another household member had received assistance through another IOM project or through another organization in the meantime, they were excluded from the CBI.[6]

[4] The lack of a fixed threshold renders an impact evaluation using a (fuzzy) regression discontinuity design with the VAS score as forcing variable infeasible.

[5] Detailed information on the survey protocol can be found in Appendix L.

[6] Other reasons for exclusion from the program are missing identification documents or assignment to another project due to reallocation of funding. In our sample, 614 (8.5% of the original sample) individual cases were excluded from the program at this stage.

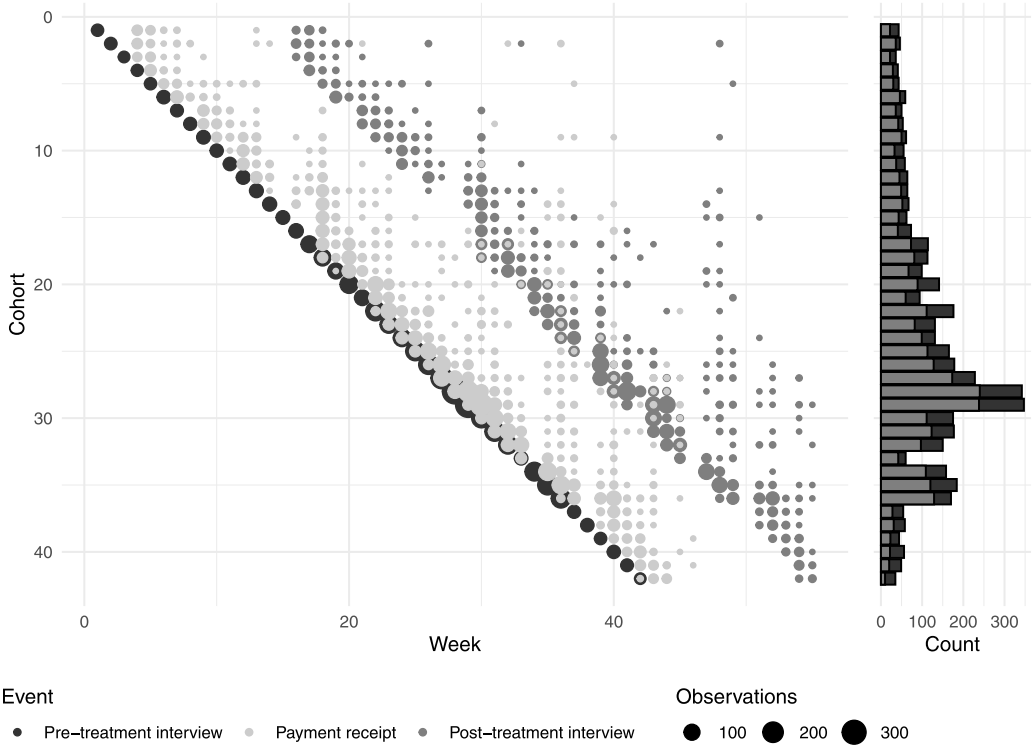

Fig. 1. Survey schedule.
Notes: The figure (left) visualizes the timing of pre-treatment interviews, payment receipts and post-treatment interview per cohort and week. A cohort represents the group of beneficiaries that conduct the pre-treatment interview in the same week. The size of dots is proportional to the group sizes. The figure (right) shows the count per cohort.

Only after the second review, the beneficiaries were informed that they were eligible for the CBI and would receive the one-time cash payment of 760 soles (approximately USD 190).[7] The payment was processed through external providers usually between one and five weeks after the pre-treatment interview. 95% of recipients received the payment within five weeks after the pre-treatment interview. The exact timing of the payment depended, among other factors, on the length of the review process, the payment providers' capacity, and the timing of cash withdrawal by the recipient. In the analysis, we account for the timing of the payment by including weeks-of-delay fixed effects. Finally, three months after the payment receipt, an endline interview was conducted, during which we collected post-treatment outcomes. Both baseline and endline surveys were conducted by the enumerators of a survey firm based in Peru. Trained enumerators interviewed respondents through phone calls via WhatsApp, phone line, or any other VoIP (Voice over IP) application depending on participants' access to the internet and preferences.

The field phase lasted from August 2021 to August 2022 and covered 42 cohorts of beneficiaries. We define a cohort as those beneficiaries who had their pre-treatment interview in the same week. Fig. 1 illustrates the realized survey schedule. Between weeks 1 to 16, the number of weekly pre-treatment interviews rose from below 43 to 73. During weeks 17–36, the average number of weekly pre-treatment interviews increased to 167, peaking at 347 in week 29. In the final weeks of pre-interviews, the number of interviews declined to between 35 and 58.[8] As shown in Fig. 1, some payments were received only weeks after the pre-treatment interview. The post-treatment interview was scheduled based on the timing of the cash receipt, but no later than the 55th week of the field phase (i.e., the end of the study period). In total, 4,504 beneficiaries were interviewed for the pre-treatment interview and received the CBI. Of these, 3,032 beneficiaries completed the post-treatment interview, which translates into a high retention rate for the final interview of 67.3%.[9] Appendix Tables K.16-K.18 provide an overview of missingness by question and variable. To account for attrition, we use Multiple Imputation by Chained Equations (MICE), but we find that our results are robust to different approaches to addressing attrition (as discussed in Section 6). Importantly, as we show in Appendix Figure K.6, attrition does not vary across cohorts.[10]

[7] One month after the payment receipt, a short follow-up interview was conducted for IOM's internal purposes, which only included a reduced set of questions and is not used for the main analyses in this study.

[8] Compared to the initial plan outlined in the pre-analysis plan, there are a few changes in the survey implementation that occurred due to irregularities in the interviews of one enumerator. In a regular data check, we detected that one enumerator conducted implausibly short interviews. The average length of interviews was around five minutes and the median was 4.21 min, while other enumerators took 11.6 min per interview on average. This affected 1,560 pre- and 561 post-treatment interviews. We excluded all cases interviewed by this enumerator due to data reliability concerns. While we were able to repeat surveys to ensure a roughly similar total sample size compared to the pre-analysis plan, the schedule was inevitably affected by the removal of these interviews: (i) the sample size in cohorts 1–15 was reduced from 140 to between 36 to 67, (ii) the sample size in cohorts 16–25 was reduced from 200 to between 73 and 165, (iii) to compensate for the number of lost cases, we increased cohorts 26–36 by 200 to 400 responses, (iv) cohorts 37–42 included between 35 and 54 beneficiaries. In contrast to the survey schedule outlined in the PAP, these latter cohorts conducted both pre-treatment and post-treatment interviews.

[9] We exclude respondents whose cash assistance was cancelled or not received (e.g., due to lack of a bank account or no identification documents), who received more or less than 760 soles, and respondents without a VAS score. We also drop five respondents whose gender is non-binary and 10 respondents who are not living in one of the following five main regions of analysis (i.e., Callao, La Libertad, Lima, Piura, or Tumbe) due to insufficient observations per group for the imputation algorithm.

[10] An exception are the final cohorts 37 to 42. This is because the survey firm was not able to reach some respondents belonging to these cohorts before the

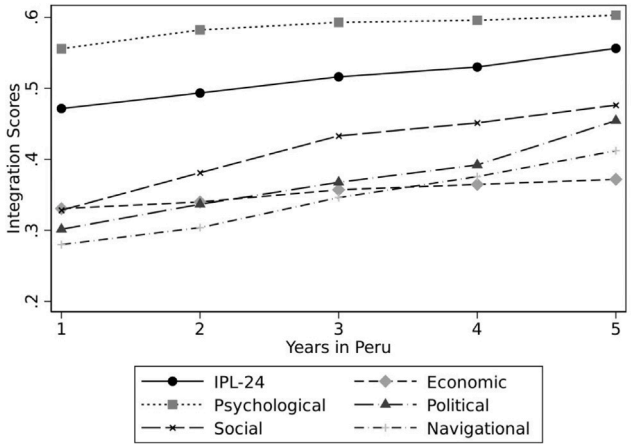

Fig. 2. Integration scores by year of arrival.

3.2. Outcome variables

We estimate the effect of the CBI on a series of pre-registered outcome variables. The first outcome measures the integration of Venezuelans in Peru using the IPL-24 index of Harder et al. (2018), adapted to the Peruvian context. The adapted index covers five dimensions of integration.[11] The psychological component captures respondents' feeling of connection with the host country, their wish to continue living there, and their sense of belonging. The economic component summarizes employment, household income, and financial liquidity. The political component relates to understanding the host country's political issues and informal (i.e., non-electoral) political engagement. The social component captures bridging social capital, i.e., social connections with native citizens. The navigational component captures the respondent's actionable knowledge and capacity to navigate the host society (such as searching for a job or asking for legal help).

Each integration dimension is measured with two to four questions.[12] Answers are measured on a scale from one to five, where higher values indicate a higher integration level. The answers are aggregated and rescaled for each respondent to have a mean zero and standard deviation of one in the pre-treatment sample, facilitating the interpretation of effect estimates. Fig. 2 shows our sample's integration path over the first five years after arrival in Peru, for the aggregate index and its five components, allowing us to validate the integration measures. As expected, we observe that integration increases with time since arrival. This trend is particularly pronounced for social and navigational integration.

In addition to the overall IPL-24 index and its five sub-indices, we analyze the following outcomes: the employment status, which is coded as 1 if the respondent answers that they performed *'any type of paid work during the last weeks'*, and 0 otherwise; a binary self-employment indicator; a binary indicator for the intention to emigrate from Peru in the next 12 months (which previous research has identified as a key predictor of actual migration behavior (Tjaden, Auer, & Laczko, 2019)); a binary indicator for whether a person feels discriminated against, and a binary indicator for the use of coping strategies. The last indicator captures whether the respondent used any of the following coping strategies: borrowing money, buying food from credit, withdrawing children from education, accepting a dangerous job, begging for money, or working informally.[13]

3.3. Data descriptives

Table 1 provides pre- and post-treatment descriptive statistics of the beneficiaries in the sample. Since the beneficiaries have been identified by IOM as highly vulnerable, they are likely to deviate from other Venezuelan immigrants in Peru. To contextualize our target population and to gain insights into differences between the beneficiaries and the general Venezuelan immigrant population, we compare our sample, where possible, with the ENPOVE II survey which was conducted from February to March 2022 and provides a representative picture of Venezuelans in Peru.[14] We report descriptive statistics from ENPOVE II in the two rightmost columns. A few differences are worth highlighting. First, 82% of beneficiaries in our sample are women, while the share of women in ENPOVE II is 51%, suggesting that IOM's pre-treatment vulnerability assessment found women to be particularly vulnerable. Second, pre-treatment employment levels in the IOM sample (71%) are slightly lower than in ENPOVE II (77%). Differences in terms of labor market integration are more striking when looking at monthly incomes: The average household income in our sample amounts to only 401 soles per month compared to 1,216 soles for Venezuelans in the ENPOVE II sample. Of those employed, most Venezuelans in our sample work in sales (58.1%), services (15.1%) and non-specialized tasks (21.3%) (see Appendix Figure E.3). Together, this suggests that many Venezuelans in our sample live in highly precarious conditions and those employed work predominantly in low-skilled, low-paid positions. Finally, Appendix Figure F.4 provides a map showing that most beneficiaries live in Lima and Callao (3,586 individuals or 79.6% of the sample). The remaining beneficiaries are clustered in the Northern regions of Tumbes and Piura (401 individuals or 8.9% of the sample). Finally, 517 beneficiaries or 11.5% live in the region of La Libertad.

4. Methodology

This section presents the empirical methodology for the impact analysis. A challenge for the analysis is that (almost) all eligible beneficiaries in our sample eventually receive the cash payment, complicating standard differences-in-differences approaches. We thus opt for two complementary identification strategies relying on "before–after" and "between" comparisons.

In our "between" estimation strategy, we compare already-treated beneficiaries from earlier cohorts with not-yet-treated beneficiaries in the same calendar week while flexibly controlling for a range of individual characteristics. The regression model can be written as

$$y_{ict} = \alpha D_{it} + \delta_t + \mathbf{x}_i'\beta + \varepsilon_{ict}, \quad (1)$$

where t is the week of interview with $t = 1, \ldots, 55$ and the treatment indicator $D_{it} = 1$ if the respondent i has received the CBI before

data collection terminated in August 2022. We thus show additional results excluding these cohorts in Appendix Table K.15. The results are virtually unchanged.

[11] The original IPL-24 index also includes a sixth component, capturing the respondent's self-assessment of host country language skills. Since we can assume that most Venezuelans are proficient in Spanish, we excluded the linguistic dimension. Furthermore, we removed five questions measuring the feeling of isolation, as well as a few selected questions relating to social and political aspects. After consultations with IOM Peru, these questions were deemed to be too sensitive or irrelevant for this study. Finally, we made several minor modifications to adapt questions to the Peruvian content, e.g., adjusting income brackets. We refer to the overall integration measure as the IPL-24 index for simplicity.

[12] A complete list of questions is provided in Table H.12 and H.13 in the Appendix.

[13] A detailed description of pre-registered outcome variables is in Table H.11.

[14] ENPOVE II provides information on the demographic, social, economic, vulnerability and protection needs of the Venezuelan refugee and migrant population in Peru, and to analyze the trends and characteristics of the Venezuelan population residing in Peru, with information disaggregated by demographic, social and economic characteristics.

Table 1
Pre- and post-treatment descriptive statistics.

	Pre-treatment		Post-treatment		Difference	ENPOVE II	
	Mean	SD	Mean	SD		Mean	SD
Panel A: Outcome variables							
IPL-24	0.52	0.09	0.53	0.09	0.01***		
Economic integration	0.36	0.13	0.37	0.13	0.01***		
Employment status	0.71	0.46	0.72	0.45	0.01	0.77	0.42
Self-employment	0.06	0.24	0.09	0.29	0.03***		
Discrimination	0.20	0.40	0.21	0.41	0.01		
Emigrate	0.03	0.16	0.05	0.21	0.02***		
Coping Strategies							
Any	0.78	0.41	0.82	0.38	0.04***		
Food on credit	0.43	0.50	0.48	0.50	0.04***		
Withdrew child from educ.	0.06	0.23	0.12	0.33	0.06***		
Dangerous job	0.13	0.34	0.14	0.35	0.01		
Begged for money	0.10	0.30	0.11	0.31	0.01		
Informal work	0.59	0.49	0.66	0.47	0.07***		
Panel B: Control variables							
Age	33.35	9.52	33.38	9.44	0.03	34.42	11.75
Female	0.82	0.38	0.83	0.38	0.01	0.51	0.50
Referred (NGO/Gov.)	0.12	0.32	0.12	0.32	-0.00		
Assistance time (weeks)	1.82	1.62	1.77	1.57	-0.05*		
Years in Peru	3.32	1.16	3.36	1.13	0.04***	3.43	1.32
Panel C: Other variables							
HH monthly income (Soles)	401.32	211.22	426.16	217.47	24.85***	1216.40	783.29
HH size	3.53	1.60	3.66	1.60	0.14***	3.24	1.73
Requested refugee status	0.05	0.22	0.05	0.22	0.00		
Single parent	0.36	0.48	0.35	0.48	-0.00		
Chronic illness	0.15	0.35	0.15	0.35	-0.00	0.17	0.37
Disabled	0.01	0.12	0.02	0.12	0.00	0.02	0.14

Notes: The table reports pre- and post-treatment descriptive statistics. The IOM sample includes all respondents who have received the cash payments, self-identified as male or female, and live in the regions Callao, La Libertad, Lima, Piura and Tumbes. The column labeled '*Difference*' reports the difference in pre- and post-treatment means, with clustered standard errors at the cohort level. The total number of individuals who completed the pre-treatment survey is 4,504. 3,032 individuals completed the post-treatment survey. The descriptive statistics are based on unimputed data. Where available, we show for comparison descriptive statistics from the ENPOVE II survey, which was conducted between February and March, 2022, and provide a representative picture of Venezuelans in Peru. The summary statistics are calculated using sample weights. The total number of observations in the ENPOVE II survey is 8,403.
Sign. Codes: * 0.1 ** 0.05 *** 0.01.

week t, and 0 otherwise. Note that D_{it} is equivalent to a dummy for the post-treatment interview. The week fixed effects δ_t account for unobserved time-specific shocks that might affect the immigrant population, e.g. due to seasonal effects and changes in labor market conditions. The sample includes all weeks for which we observe both treated and untreated immigrants. A concern might be that earlier cohorts (i.e., the already-treated) and later cohorts (the not-yet-treated) are not comparable. For example, if the employment potential of earlier cohorts is systematically higher relative to later cohorts, we might overestimate the effect of the CBI. While interviews are scheduled on a first-come–first-serve basis, we cannot rule out that cohorts differ in observed characteristics given that the timing of the first interview was not randomized. We thus include a rich set of potential covariates (collected in the vector $\boldsymbol{x}_i$) which comprises gender interacted with age and age squared, the VAS score in levels and squared, months in Peru in levels and squared, as well as fixed effects for the referral organization, weeks of payment delay, region and enumerator.[15] We employ post-double-selection lasso (PDS-Lasso; Belloni, Chernozhukov, & Hansen, 2014), a data-driven method, to select the relevant covariates by fitting outcome and treatment individually against all covariates using the Lasso estimator, and retaining those that are either predictive of the treatment or the outcome (or both). The underlying identification assumption is that conditional on this battery of covariates, assignment to cohorts is as good as random. We conduct comprehensive balance tests to probe this assumption. To this end, we regress outcomes and control variables, both measured at baseline, against cohort fixed effects. The results, provided in Appendix Section-C, show little evidence for over-time trends in outcomes after conditional on covariates.

In a complementary "before–after" identification strategy, we compare integration outcomes in the pre- and post-treatment interviews while accounting for time-invariant cohort characteristics:

$$y_{ict} = \alpha D_{it} + \mu_c + \boldsymbol{x}_i'\beta + \epsilon_{ict}. \quad (2)$$

Here, μ_c are the cohort fixed effects. This specification accounts for the varying composition of cohorts, which may be, for example, be reflected in variations in the levels of vulnerability. However, the specification does not adjust for the week of the interview and might therefore be affected by, for example, seasonal effects or labor market shocks. In Section 6.3, we conduct a sensitivity analysis and show that our results are robust to even large omitted variable biases. We also consider a more restrictive individual fixed effect specification, which yields similar results (see Appendix Table A.6). To further examine robustness, we combine the two approaches in Eqs. (1) and (2) in a two-way fixed effects (TWFE) specification. However, the power analysis in the pre-analysis plan has already shown that this approach suffers from low statistical power for our sample size and survey structure. While we will report TWFE results for comparison, it is helpful to keep these power concerns in mind when interpreting the estimates. In all three specifications, we employ cluster-robust standard errors where we cluster at the cohort level. Finally, we use Multiple Imputation

[15] In Appendix Table A.5, we present a non-pre-registered analysis where we, in addition, control for the regional employment rate, which we calculate from the ENAHO survey (see Section 6.2). The results are virtually identical.

by Chained Equations (MICE) with 10 imputations to impute missing values.[16]

5. Results

5.1. Main regression results

Table 2 shows the effect of the cash transfer on the main, pre-registered outcomes. Columns (1) to (3) report specifications with cohort, week or two-way fixed effects. In Columns (4) to (6), we also alternate between fixed effects specifications but employ the PDS-Lasso estimator to select control variables.[17]

Beginning with Panel A, we find that the CBI increases the overall IPL-24 integration index by 0.161 and 0.268 SDs in the week and cohort fixed effects specifications, respectively. When using lasso-selected controls, the coefficient from the week fixed effects model, 0.276, is virtually unchanged, whereas the point estimate from the cohort fixed effect model slightly decreases to 0.122 SD. All four coefficients are estimated with high precision and are statistically significant. As previewed in the pre-analysis plan, the two-way fixed effects estimator is underpowered and yields imprecise estimates, attenuated towards zero, although the coefficients are still positive. Generally, we find that our preferred specification with lasso-selected covariate adjustment yields slightly smaller estimates, which is why we interpret these estimates as lower bounds for the effects of cash transfers. Table A.2 in the Appendix shows a breakdown of the IPL-24 estimates in its components and reveals that the navigational, social and economic components mainly drive the increase in IPL-24.

Panels B-D focus on economic outcomes. Panel B shows a positive impact of the CBI on the economic integration sub-index, which takes employment, income, financial liquidity and type of work into account. The effect size is 0.093 and 0.146 SD in the PDS-Lasso specifications with cohort and week fixed effects. In line with the findings of LoPalo (2019), Panel C provides only limited evidence for a positive effect on employment. The effect on employment is only significant in column (1). Given that all point estimates are close to zero or slightly positive, this finding adds to the mounting evidence that cash transfers do not depress labor supply as a simple economic model of work-versus-leisure trade-off might suggest (Baird, McKenzie, & Özler, 2018; Banerjee, Hanna, Kreindler, & Olken, 2017). Similar to Blattman, Green, Jamison, Lehmann, and Annan (2016), who found an increase in micro-enterprises in response to a cash transfer to the ultra-poor in Uganda, Panel D shows a consistent increase in the probability of self-employment between 1.9 and 4.9 percentage points in our preferred specifications. This is consistent with the large share of respondents who expressed the intention to start a business in the pre-treatment survey (91.6%). Out of the 2,758 respondents who expressed the intention to launch a business in the pre-treatment survey, 197 reported owning a business in the post-treatment survey (see Appendix Table E.10.) In Table A.1 in the Appendix, we investigate whether self-employment serves merely as a second-best alternative to dependable, long-term employment, or whether it enables beneficiaries to realize their economic potential. To this end, we regress log standardized household income against the interaction of the cash treatment and self-employment indicator. The household income effect of the cash transfer amounts to around 4.8% ($s.e.$ = 1.3) for non-business-owners compared to around 14.8% ($s.e.$ = 2.9) for the self-employed. Self-employment, induced by the cash transfer, thus seems to be a successful strategy for increasing household income compared to dependable employment.

Turning to Panel E in Table 2, we find that the CBI increases the intention to leave Peru by between 1.2 and 1.5 percentage points. The point estimate from the TWFE specification is substantially larger at 4.1. In comparison, only 2.6% of respondents answered that they plan to leave Peru for another country in the pre-treatment survey. 67% of respondents with an emigration intention said that they prefer to return to Venezuela. Other named destinations are Chile (12%) and the United States (9%). At first glance, this result seems to contradict that the cash transfer facilitates integration in Peru. However, while beneficiaries might find it easier to live and work in Peru, the cash receipt could have revived the ultimate wish of returning to Venezuela in the long term or of emigrating to a third country by putting this plan in closer reach. In the pre-treatment survey, 83.5% of respondents answered that they are currently not able to pay for an unexpected expense of 50 soles (approximately 12 USD), suggesting that most beneficiaries face severe liquidity constraints and might not able to bear the costs of relocation. Our results are in line with previous studies on conditional cash transfers in Latin America and unconditional cash transfers in Africa have found that the effects on emigration can go in both directions and are context-dependent, with some showing an increase in emigration while others show a decrease (see review in Hagen-Zanker & Himmelstine, 2013).

Panel F suggests that there is no effect of the cash transfer on perceived discrimination. All estimates are close to zero and far away from conventional levels of statistical significance. Turning to coping strategies, Panel G finds mixed results. While the cohort fixed estimates suggest that the CBI increases the use of coping strategies, the week fixed effects specifications suggest a null effect. In Appendix B, we break the results down by individual coping strategies and find that the effect is mainly driven by working informally. However, given that the effect is not consistent across both specifications, we interpret this result with caution.

5.1.1. Heterogeneity analysis

We explore treatment effect heterogeneity by gender, age, year of arrival, education, and household size. With regard to age, we use a median split and compare the treatment effect above and below the median. For year of arrival, we distinguish between Venezuelans who arrived in 2017 or earlier,2018–2019, and 2020 or later. We form three education groups: primary school or less, technical or secondary school, and higher education. Finally, we categorize households into single-person households, two-person households, and households with three or more members. The estimation of heterogeneous treatment effects is implemented by interacting the treatment dummy D_{it} with these group indicators. We again focus on our preferred specifications in (1) and (2) employing the PDS-Lasso estimator to adjust for covariates.

Results of the heterogeneity analysis are presented in Fig. 3 (numerical estimates are provided by Appendix Table A.3). Focusing on overall and economic integration, we find no pronounced differences across gender, age group and arrival year. Female beneficiaries experience slightly larger effects on overall integration (by 0.02–0.04 SD) but slightly smaller effects on economic integration (by approximately the same amount). In contrast, the impact of the CBI on integration depends strongly on the education level: It is essentially zero for beneficiaries with primary education but surges as the education level increases, indicating that beneficiaries with higher formal education could make better use of the cash support. For beneficiaries with upper-level education, we find that the effect on overall and economic integration is around 0.4 SD and 0.2 SD, respectively.

[16] MICE relies on sequentially fitting univariate imputation models (van Buuren, 2007). The imputation model for one variable (either independent or dependent variable) leverages all other variables as predictors. The missing values are then replaced with imputed values that are drawn from the conditional density distribution. This process is repeated 10 times, yielding 10 imputed data sets and coefficient vectors (and associated standard errors), which are then aggregated into overall estimates. See Appendix G for more details on MICE.

[17] Appendix Table A.4 also provides p-values using the Wild Cluster Bootstrap (MacKinnon & Webb, 2019; Roodman, Nielsen, MacKinnon, & Webb, 2019) and without Multiple Imputation by Chained Equations.

Table 2
The effects of cash-based interventions on integration outcomes of Venezuelan immigrants in Peru.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. IPL-24 [Pre-treatment average = 0]						
CBI	0.162***	0.269***	0.110	0.123***	0.277***	0.073
	(0.020)	(0.027)	(0.112)	(0.028)	(0.034)	(0.108)
Panel B. Economic Integration [0]						
CBI	0.178***	0.190***	0.142	0.093**	0.146***	0.070
	(0.025)	(0.029)	(0.120)	(0.033)	(0.029)	(0.103)
Panel C. Employment [0.705]						
CBI	0.030*	0.015	-0.004	0.018	0.006	-0.023
	(0.012)	(0.011)	(0.042)	(0.012)	(0.013)	(0.038)
Panel D. Self-employment [0.0635]						
CBI	0.024**	0.048***	0.038	0.019**	0.049***	0.049
	(0.007)	(0.007)	(0.043)	(0.007)	(0.008)	(0.041)
Panel E. Emigration [0.0264]						
CBI	0.016***	0.014*	0.033+	0.015***	0.012*	0.041*
	(0.004)	(0.005)	(0.018)	(0.004)	(0.005)	(0.017)
Panel F. Discrimination [0.203]						
CBI	-0.001	-0.015	-0.064	0.007	-0.007	-0.072
	(0.009)	(0.012)	(0.057)	(0.009)	(0.010)	(0.057)
Panel G. Any coping strategies [0.248]						
CBI	0.042***	-0.002	-0.020	0.050***	-0.001	0.001
	(0.011)	(0.011)	(0.030)	(0.010)	(0.010)	(0.028)
Estimator	OLS	OLS	OLS	PDS-Lasso	PDS-Lasso	PDS-Lasso
Week-FE	No	Yes	Yes	No	Yes	Yes
Cohort-FE	Yes	No	Yes	Yes	No	Yes
Month-FE	No	No	No	No	No	No
Month-Cohort-FE	No	No	No	No	No	No
Observations	7536	7536	7536	7536	7536	7536
# of Individuals	4504	4504	4504	4504	4504	4504
# of Cohorts	42	42	42	42	42	42
# of Weeks	53	53	53	53	53	53

Notes: The table reports the impact estimates of the CBI on the main integration outcomes. Columns (1)–(3) use OLS with cohort, week and both fixed effects, respectively. Columns (4)–(6) rely on PDS-Lasso instead of OLS. Robust standard errors in parentheses, clustered at the cohort level. Pre-treatment averages of the outcome variables are shown in brackets. The set of controls comprises gender interacted with age and squared age, the VAS score in levels and squared, months in Peru in levels and squared, as well as fixed effects for the referral organization, enumerator, region and weeks of payment delay. We use Multiple Imputation by Chained Equations (MICE) with 10 imputations to impute missing values. *Sign. Codes:* † 0.1 * 0.05 ** 0.01 *** 0.001

The impact of the CBI on employment is primarily driven by beneficiaries aged 31 or older, with an effect size of around 4ppt, while there are no substantial differences between male and female beneficiaries. While one might expect that women would particularly benefit from self-employment, which provides greater flexibility to balance work and childcare, we find little evidence to suggest that the impact of CBI on self-employment differs by gender. Venezuelans who have arrived in 2017 or before, and thus already spent five years or more in Peru, experience, if anything, a larger increase in employment due to the CBI and also exhibit a greater boost in self-employment. This suggests that the effectiveness of CBI is not limited to refugees who just arrived in the host country but also carries over to later stages of the integration trajectories. The effect on self-employment increases in the education level, whereas the effect on employment does not systematically vary with education.

With respect to emigration, men's intention to leave Peru increase by 3.3–3.7 ppt (*s.e.* = 1.1), while the corresponding effect for women is only 1 ppt. Furthermore, the CBI leads to a greater increase in emigration intentions for the cohort of Venezuelans arriving in 2017 or earlier, indicating that even after more than 5 years since arrival many Venezuelans contemplate leaving Peru again. The impact on the feeling of being discriminated is slightly larger for males and older Venezuelans, and for beneficiaries with higher education.

We also find that the CBI effects exhibit considerable variation by household size. Specifically, the effect sizes sharply decrease in household size for overall and economic integration, as well as for emigration intention, highlighting the need to adjust cash support for household size to ensure a constant per-person benefit. Similarly, the impact on employment is around 10 ppt for single and two-person households, but disappears for households larger than two, possibly indicating that the presence of dependents (e.g., children or elderly) reduces any effects of the CBI on labor force participation for the household head.

In light of the high share of women among our beneficiary population, Appendix Table A.7 replicates all main results of Table 2 but focuses on women only. As expected given their high share and the relatively limited evidence for treatment effect heterogeneity across genders, the estimates are very similar to those obtained for the entire sample.

6. Robustness checks

In this section, we discuss three validation exercises with which we assess the robustness of our main results reported above. First, we provide balance tests to show that the pre-treatment characteristics are relatively stable across cohorts. Second, we leverage data from

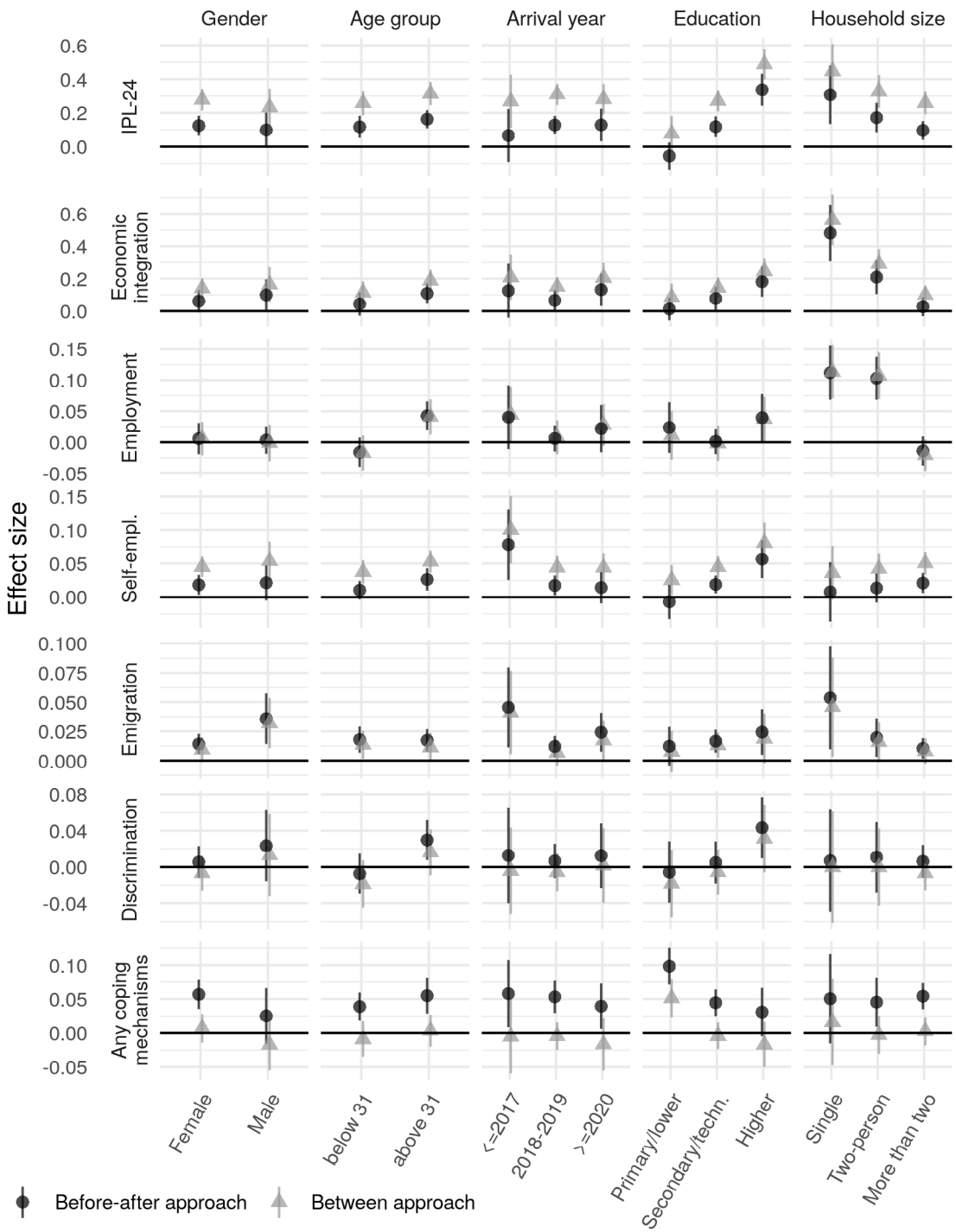

Fig. 3. Heterogeneous effects of cash-based interventions on integration outcomes by gender, age, arrival year, education and household size.
Notes: The figure shows the effect of the CBI on the outcomes (labeled on the left) by gender, age group, arrival year, education level and household size. Estimation is based on Eq. (1)–(2) and the PDS-Lasso estimator, but we interact the treatment variable with group indicators. 95% confidence intervals are shown. Numerical estimates are provided by Appendix Table A.3.

the *Encuesta Nacional de Hogares* (ENAHO) survey, which allows us to utilize Peruvians in the ENAHO survey as a control group and estimate the effect of the CBI on economic outcomes in a two-way fixed effects design. Third, we employ the sensitivity analysis of Cinelli and Hazlett (2020) to gauge how large the impact of confounding factors would need to be in order to flip the signs of our main coefficients and thus change the conclusions from our analysis. Beyond these three checks, we provide a number of additional analyses in the Appendix. We show that the main findings remain unaffected when we account for attrition using inverse probability weighting (see Appendix Table K.14), when leveraging the unimputed data (Appendix Table A.4), when controlling for the regional employment rate (Appendix Table A.5) and when we use a more restrictive individual fixed effects specification (Appendix Table A.6).

6.1. Balance tests

We conduct several balance tests for outcome variables and covariates to validate that cohorts are comparable, i.e., that observed

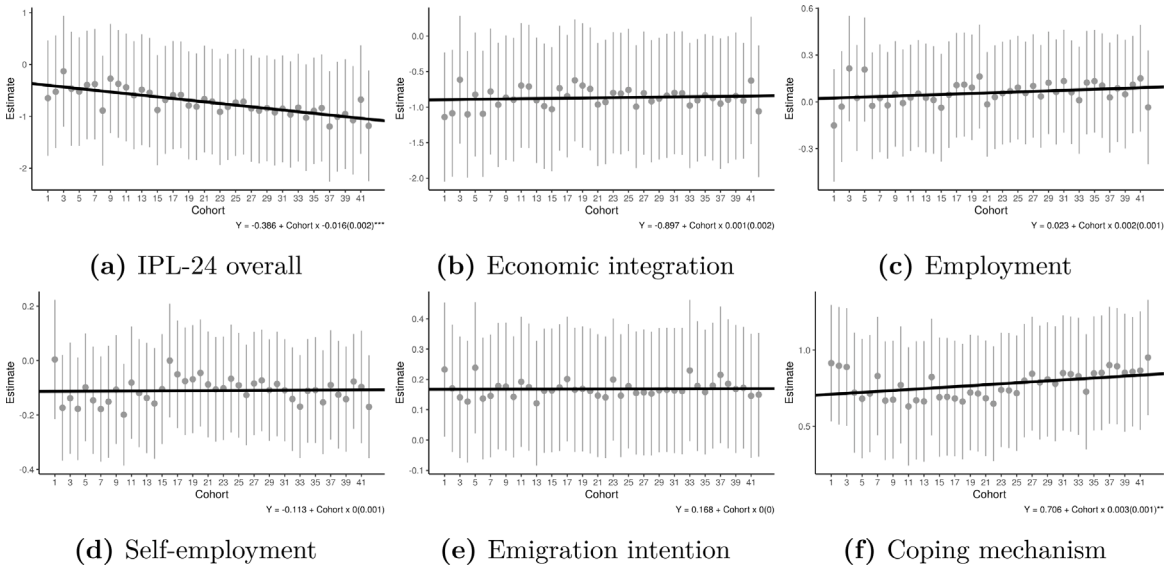

Fig. 4. Balance test across cohorts for outcome variables.
Notes: The figures show the coefficients and associated 95% confidence intervals from regressing the variable of interest against cohort fixed effects, while including the usual set of control variables. A linear trend is overlayed (the estimation equation is reported at the bottom). The set of controls comprises gender interacted with age and squared age, the VAS score in levels and squared, months in Peru in levels and squared, as well as fixed effects for the referral organization, enumerator, region, and weeks of payment delay. The sample only includes first-wave interviews. Standard errors are robust to heteroskedasticity.

characteristics are stable over time. Regarding the outcome variables, we regress outcomes against cohort fixed effects while conditioning on the usual set of control variables and using only data from the pre-treatment survey. The set of controls comprises gender interacted with age and squared age, the VAS score in levels and squared, months in Peru in levels and squared, as well as fixed effects for the referral organization, enumerator, region, and weeks of payment delay. We report the conditional means by cohort in Fig. 4. We find that the IPL-24 overall index exhibits a small downward trend across cohorts and coping mechanisms show a slight upward trend. While the slope coefficients of the linear trend (overlayed in blue) are statistically significant, they are, with slope coefficients of −0.016 and +0.003, respectively, very small in substantive terms. We also note that these trends are absorbed by the cohort fixed effects ("between") estimation strategy. We find no evidence for a trend in the conditional outcome means of the other outcome variables.

We turn to the main covariates. We again regress the variable of interest against cohort fixed effects, but without including control variables. The means by cohort are reported in Appendix Figure C.1. While we find significant linear trends for all covariates except the VAS score, the trends are small in absolute size. For example, we find that the share of women decreases by less than 0.002 ($s.e. = 0.001$) per week. We again note that these structural differences are absorbed by the cohort fixed effects in the "between" model.

6.2. ENAHO control group

The ENAHO survey is a nationally representative survey that monitors indicators of living conditions in Peru. As a robustness check, we make use of ENAHO to compare the outcomes of Venezuelan beneficiaries in our sample with respondents from ENAHO. Since ENAHO has run every month since 1995, the field phase of our study is completely covered by ENAHO.[18]

To implement our analysis, we consider the regression model

$$y_{ist} = \alpha D_{ist} + \mu_i + \psi_t + \boldsymbol{x}'_{ist}\gamma + \varepsilon_{ict}, \tag{3}$$

where μ_i and ψ_t denote individual and month fixed effects. That is, in this robustness check, we compare the outcomes of untreated ENAHO respondents with treated and untreated Venezuelan beneficiaries observed in our data, while only exploiting within-person variation. We control for gender, age (in levels and squared), household income and other vulnerability-relevant variables (indicators for health insurance, single parent, disability, chronic illness, pregnant, and dependent family members). Since the ENAHO survey includes information on whether respondents receive any form of cash, food, or non-food assistance, we are able to exclude these respondents from the control group.

There are three constraints limiting our robustness check: First, since ENAHO only includes the outcomes employment, self-employment and perceived discrimination, we cannot perform a similar robustness check for our other integration measures. Second, while the three questions are sufficiently similar, the wording is not exactly identical (see Appendix I for details). One example of such a difference is that we code respondents as employed in the IOM survey if they have performed paid work over the last four weeks, while we code respondents in ENAHO as employed if they state they either are in permanent employment or have worked during the last week. Since we only leverage within-individual variation, level differences in responses arising from the slightly different wording should be neutralized by the individual fixed effects. Third, as a nationally representative survey, the ENAHO respondents differ from the sample of Venezuelans included in the IOM survey across various dimensions. In particular, IOM respondents are more likely to be vulnerable, female and have a lower income than the average resident in Peru. However, this complementary analysis relies on a parallel trends assumption stating that, if CBI beneficiaries had never been treated, their outcomes would have evolved in parallel to those of untreated ENAHO respondents. While the parallel trends assumption does not require characteristics of treated and untreated to match in levels, we consider subsetting the ENAHO control group to female, low-income and highly vulnerable respondents to further increase the credibility of the analysis. As we show below, the results are robust across these different definitions of

[18] Another potential strategy would utilize respondents in the ENPOVE survey as the control group. Unfortunately, there is an insufficient temporal overlap between the ENPOVE and IOM survey, which is why we abstain from conducting this analysis.

Table 3
The effects of cash-based interventions on employment, self-employment and discrimination of Venezuelan immigrants in Peru using the ENAHO control group.

	Employment		Self-employment		Discrimination	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. All observations						
CBI	0.015+	0.001	0.029***	0.029*	0.002	-0.008
	(0.009)	(0.017)	(0.008)	(0.013)	(0.009)	(0.016)
N	14379	6634	12612	6398	14300	6494
Panel B. Only female						
CBI	0.011	0.010	0.036***	0.032+	-0.005	-0.015
	(0.012)	(0.022)	(0.009)	(0.017)	(0.010)	(0.021)
N	8752	5182	7894	5088	8694	5078
Panel C. Only ENAHO with low income						
CBI	0.008	-0.008	0.033***	0.029+	0.003	-0.018
	(0.010)	(0.020)	(0.008)	(0.016)	(0.009)	(0.019)
N	10831	6206	9539	6082	10747	6062
Month-FE	Yes	Yes	Yes	Yes	Yes	Yes
Survey-FE	Yes	No	Yes	No	Yes	No
Individual-FE	No	Yes	No	Yes	No	Yes

Notes: Regressions shown in the table leverage the ENAHPO survey as the control group. The outcome variables are employment (columns 1 and 2), self-employment (columns 3 and 4) and discrimination (columns 5 and 6). All models include months fixed effects. Odd-numbered columns use survey fixed effects. Even-numbered columns use individual fixed effects.
Sign. Codes: † 0.1 * 0.05 ** 0.01 * 0.001.

the control group. Finally, we also show in Appendix Figure J.5 that there is considerable overlap between ENAHO respondents and IOM respondents with respect to age, income and the propensity to receive cash assistance.

Table 3 shows the results for the outcomes employment (columns 1 and 2), self-employment (columns 3 and 4) and discrimination (columns 5 and 6). Even-numbered columns rely on the specification in (3) with individual fixed effects, while odd-numbered columns use a simplified specification only using months and survey fixed effects. Panel A includes all IOM respondents and ENAHO respondents who have not received any form of assistance. Panel B also restricted the sample to female respondents. Panel C restricts the sample to low-income individuals who have a household income of less than 1,350 soles, which corresponds to the 95% percentile in the IOM sample. Throughout these regressions, the regression coefficients are remarkably stable, and support our main results. The estimated coefficients for employment and self-employment from the TWFE regressions fall within the range of the ones that only use the IOM survey. These estimates, which leverage a separate control group, increase our confidence in a causal interpretation of our main results based on the IOM data and support the conclusion that CBI boosts self-employment.

6.3. Sensitivity analysis

Estimates from the "between" approach in Eq. (1), which compares treated and not-yet-treated cohorts, might be biased if unobserved factors affect the integration potential of beneficiaries. For example, earlier and later cohorts might differ in their level of vulnerability, which in turn might influence employment potentials. While we control for the VAS score, there is a risk that this approach is insufficient if the VAS score does not capture all dimensions of vulnerability or if we are not able to fully capture the non-linear effects of vulnerability on integration outcomes. Similarly, the "before–after" approach in Eq. (2) could be biased in the presence of seasonal effects or labor market shocks that take place at particular points in time.

To assess how sensitive our main results are to unobserved confounding, we employ the sensitivity analysis of Cinelli, Ferwerda, and Hazlett (2020), Cinelli and Hazlett (2020). Their approach asks how large (measured in terms of partial R^2) the association of an unobserved confounder with both outcome and treatment would have to be in order to eliminate the estimated effect and thus change the conclusion of the analysis. Table 4 reports three sensitivity metrics that assess coefficient stability. The test statistic RV_q indicates how much of the remaining outcome and remaining treatment variation would have to be explained by an unobserved confounder for the coefficient estimate to go zero. Similarly, $RV_{q,\alpha}$ indicates how much the unobserved confounder would have to explain for the coefficient estimate to turn insignificant (at the $\alpha = 5\%$ level). Finally, in the extreme scenario where the unobserved confounder explains all the remaining outcome variation, it would need to explain $R^2_{Y\sim D|X}$% of the residual treatment variance for the treatment effect to vanish. The results indicate that the hypothesized confounder would have to be fairly potent for the estimates to turn zero. For example, it would have to explain, after adjusting for observed covariates, at least 6.25% of the remaining variation of both outcome and treatment to change the CBI effect on the IPL-24 index to zero. The effect on economic integration exhibits a similar level of robustness, while the emigration and self-employment effects are somewhat more sensitive to confounding.

To put these sensitivity metrics into perspective, we relate the strength of the association of the putative confounder to the highly predictive covariates that we observe in our data and statistically adjust for (in particular, the VAS score) Specifically, we ask how large the estimated effect of the CBI would be if the unobserved confounder would exhibit an association with both outcome and treatment that is 1–15 times as large as the combined association of, first, the covariates age and age-squared interacted with gender, second, residency months in Peru in levels and squared or, third, the vulnerability score in levels and squared. Fig. 5 shows the results of the sensitivity analysis. Here, we focus on those outcomes for which we have found consistent CBI benefits in the main analysis: the IPL-24 index, economic integration, emigration and self-employment.[19] For each of these outcomes (shown in rows), the figure reports the adjusted effect under the presence of a confounding factor of varying size where size is measured relative to the three groups of observed covariates mentioned above (shown

[19] We show the sensitivity analysis for the remaining outcomes in Appendix Figure D.2.

Table 4
Results for three sensitivity metrics based on Cinelli and Hazlett (2020).

Outcome	Fixed effect Week	Fixed effect Cohort	RV_q	$RV_{q,\alpha}$	$R^2_{Y\sim D\|X}$
IPL-24	✓		8.96	6.66	0.87
		✓	6.25	3.89	0.42
Economic integration	✓		6.27	4.10	0.42
		✓	7.46	5.32	0.60
Emigration	✓		2.53	0.16	0.07
		✓	4.03	1.70	0.17
Self-empl.	✓		5.84	3.67	0.36
		✓	3.66	1.44	0.14

Notes: The table reports additional results for the sensitivity analysis of Cinelli and Hazlett (2020). For each outcome, and for both the week and cohort fixed effect specification, we report the coefficient estimates and standard errors for reference. We then report three sensitivity statistics (where we use the default parameters $q = 1$ and $\alpha = 0.05$).

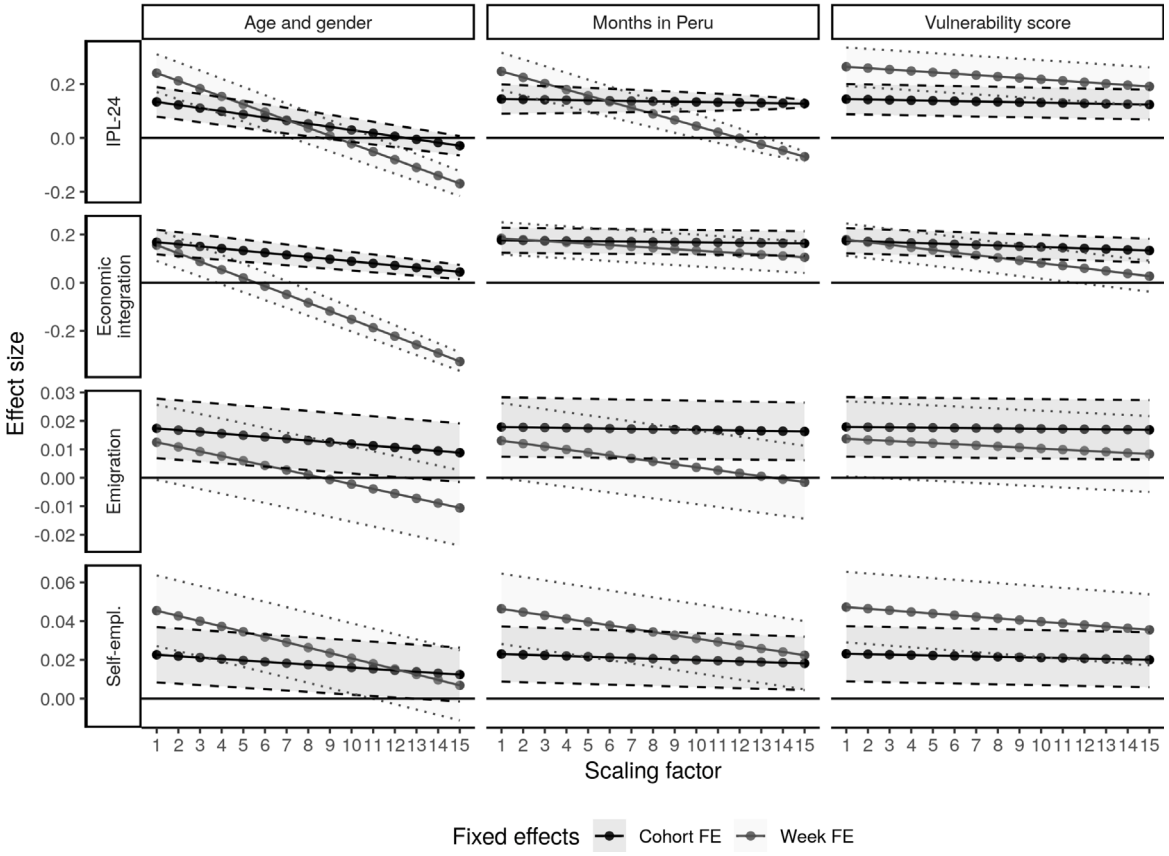

Fig. 5. Sensitivity analysis for the main outcomes.
Notes: Results of the sensitivity analysis due to Cinelli et al. (2020), Cinelli and Hazlett (2020). The sensitivity analysis relies on the OLS specifications with either week or cohort fixed effects. The outcome variables are the IPL-24 main index, economic integration, self-employment and emigration. The graphs show how much the baseline effects would be attenuated in the presence of a putative confounder. The association between putative confounder and outcome/treatment is scaled by the joint association of age and age-squared interacted with gender (column 1), residency months in Peru in levels and squared (column 2) or the vulnerability score in levels and squared (column 3).

in columns). The sensitivity analysis considers estimates from both the "between" and "before–after" approach. Beginning with the IPL-24 integration index, we find that, for the CBI effect to approach zero, the association of the unobserved confounder with both outcome and treatment would have to be 9–12 times as large as the combined association of age and gender, about 11–12 times the association of residency months in Peru (in levels and squared) and more than 15 times association of the vulnerability score. The CBI effects on economic integration are similarly insensitive and only break down if the confounder association is more than five times as large as the age and gender association (for the more sensitive "between" specification) or by a factor greater than 15 relative to the vulnerability score. The findings for self-employment are robust across all dimensions in the considered range. Lastly, the effect on emigration intentions disappears in "between specification" for confounding at least 8 and 13 times larger than the association with age/gender and months in Peru, respectively, but is again robust to a confounding factor of more than 15 when benchmarked against the vulnerability score. Overall, we conclude that our findings with respect to these four outcomes are robust to even large distortions arising from unobserved confounders.

7. Conclusion

Focusing on highly vulnerable Venezuelan immigrants in Peru, the second-largest destination country in the region, we evaluate the impact of a one-off cash payment, equivalent to 74% of the minimum monthly salary, across a range of economic and non-economic integration outcomes. Leveraging a tailored panel research design that carefully balances inferential leverage with ethical concerns about rapidly serving people in need, our "between" and "before–after" analysis finds that the CBI has a positive effect on our overall integration measure, the adapted IPL-24 index. The lower-bound impact estimate amounts to an increase of 0.12 pre-treatment standard deviations. This effect is mainly driven by changes in the navigational, social and economic components of the index. Furthermore, the CBI program increases the probability of self-employment and raises the incomes for self-employed more than for dependable employed, indicating that self-employment is a primary and viable route to economic self-sufficiency, rather than a secondary option. Furthermore, we find that the CBI increased Venezuelans' intentions to emigrate by 1.2 to 1.5 percentage points. One explanation for this finding could be that the cash assistance revived the long-term aspiration to continue the journey for those beneficiaries who do not see Peru as their final destination. We affirm the robustness of our main findings by utilizing a nationally representative survey of Peruvian non-recipients as an additional control group, alongside sensitivity analyses which indicate that even substantial unobserved confounders would not alter the main estimates.

These results expand our understanding of the integration benefits of CBIs for vulnerable immigrant populations, and especially for vulnerable women. Going beyond the standard education, employment and health outcomes covered in existing CBI studies, our findings suggest that even a one-off cash payment can generate significant benefits for immigrants' social and navigational integration. While these results paint a rather positive picture of the CBI, there are also some limitations and open questions. In particular, we find some mixed and inconclusive results that the CBI may lead to an increase in the usage of coping strategies, suggesting that further research is needed to study the impact of the CBI on living and working conditions. Our results also have implications for policy. First, our results provide strong arguments for scaling the existing CBI program in Peru. The results also provide some support for considering to expand the usage of CBI programs targeted to vulnerable immigrants and refugees in other contexts, but more research is needed to assess whether the benefits documented here also expand to immigrant and refugee populations that are more gender-balanced than the predominantly female beneficiaries studied here. Second, our heterogeneity analysis reveals that the benefits of the cash transfer, which is a fixed amount regardless of the number of beneficiaries in a support unit, diminish significantly as the number of household members increases. This indicates that more tailored cash-assistance programs that take into account the number of household members and potentially other household characteristics could improve the efficacy and fairness of cash transfers for the most vulnerable immigrant families.

CRediT authorship contribution statement

Achim Ahrens: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Methodology, Formal analysis, Data curation, Conceptualization. **Marine Casalis:** Writing – review & editing, Writing – original draft, Supervision, Project administration, Investigation, Funding acquisition, Conceptualization. **Dominik Hangartner:** Writing – review & editing, Supervision, Funding acquisition, Conceptualization. **Rodrigo Sánchez:** Writing – original draft, Visualization, Validation, Formal analysis, Data curation.

Declaration of competing interest

The authors have no competing interests to declare.

Data availability

Data will be made available on request.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.worlddev.2024.106658.

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