# Did Pandemic Emergency Cash Transfers Help or Hurt? Evidence from Poor Households in Peru[*]

Matthew D. Bird[†] Veronica Frisancho[‡] Pablo Lavado[§]

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**Abstract**

We study the enduring impact of emergency transfers targeting poor urban households in the informal sector in Peru during the pandemic. Using a fuzzy regression discontinuity design, we find that the transfer both helped and hurt beneficiaries. The support fostered labor market participation, improved financial health, reduced delinquency rates, and fostered substitution of informal for formal credit in the medium run. The program had null effects on food and housing security and increased mortality rates early on, especially among larger households. Our results suggest that the transfer was used as working capital instead of temporary income support.

**Keywords:** cash transfers, regression discontinuity, COVID-19.
**JEL Classification:** C14, C21, H55, H84, I15, I38

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[†]Universidad del Pacífico, Lima, Peru. Email: bird_md@up.edu.pe

[‡]CAF, Development Bank of Latin America, Buenos Aires, Argentina. Corresponding author: vfrisancho@caf.com.

[§]Universidad del Pacífico, Lima, Peru. Email: p.lavadopadilla@up.edu.pe

# 1 Introduction

The COVID-19 pandemic spurred a historic expansion of social protection programs to contain the spread of the virus and support the most vulnerable citizens (Gentilini et al. 2022). Emergency cash transfers became the most popular public policy response. By September 2020, 161 countries operated 340 cash transfer schemes, reaching more than a billion people (Gentilini et al. 2020). About two-thirds of these programs were new and nearly a third were one-off initiatives, lasting around three months. While there is emerging evidence of the longer-term effects of the COVID-19 pandemic on poverty (Decerf et al. 2023), learning loss (Moscoviz & Evans 2022), and physical and mental health (Zeng et al. 2023), we still lack rigorous evidence on the enduring effects of emergency cash transfers themselves. Understanding the longer-term and differential effects of these transfers is critical for the design of future social protection programs in either non-emergency or emergency contexts, be they natural disasters, conflict, or disease outbreaks.

Most of the COVID-19 unconditional cash transfer (UCT) schemes were designed on the go, with limited evidence on their effects in *emergency* contexts (Doocy et al. 2019). The transfers were initially intended to smooth consumption and thus minimize the households' daily mobility, fostering compliance with stay-at-home orders. The core policy intuition behind UCTs for the poor and vulnerable was that this exogenous change in income was expected to reduce labor supply, at least in the short run. However, even prior to the pandemic, the assumption of the income elasticity of labor underlying this prediction did not seem to hold for low-income households in developing countries (Banerjee et al. 2021, LaFave et al. 2021). If the income elasticity of labor is zero or positive, one could expect transfers to have a null effect or even increase labor supply.

This study examines the short- and medium-run effects of the emergency cash transfer program targeting poor urban households working mostly in the informal sector in Peru during the COVID-19 pandemic. Eligibility was determined based on a continuous variable and a cut-off poverty threshold, which allow us to implement a fuzzy regression discontinuity design (fRD). We rely on data from the National Registry of Identification and Civil Status linked to government records on household poverty scores to determine eligibility. We measure the impact of the transfer on a myriad of outcomes coming from two waves of phone-based household surveys, four waves of EQUIFAX credit bureau records covering up to 18 months after the onset of the pandemic, and national death registry records tracking up to 14 months after the pandemic began.

To support households dealing with the economic effects of the pandemic and the stay-at-home orders, the Peruvian government deployed multiple emergency cash transfer programs reaching nearly 70 percent of national households. This study focuses on the largest program - *bono "Yo me quedo en casa"* - which targeted poor urban households. Nearly all intended beneficiaries worked in the informal economy. The transfer was initially announced as a one-time installment of PEN 380 (USD 112 at the exchange rate in March 2020), but it was soon doubled. In October 2020, the government rolled out a second transfer of PEN 760 (USD 224). In the vast majority of cases, the transfer was delivered in cash and collection was made in person at bank branches. The total amount transferred (PEN 1520) was slightly above the monthly cost of the basic consumption basket for a household of four people living in poverty (INEI 2020).

We focus both on the short-term effects, corresponding to the first wave of telephone surveys (November 2020 - January 2021), and the medium-term effects, measured with the second phone survey wave (July - October 2021). The first survey wave takes place after the two rounds of transfers had been given out and while households were facing a second shelter-in-place order. The second survey wave was collected once stay-at-home orders were lifted and emergency cash transfer programs were discontinued. Our target sample for the phone surveys focused on households closest to the centered cutoff. The first and second rounds of the survey covered 5088 and 1766 households, respectively. The survey instrument was similar across rounds and included questions on consumption, employment, and coping strategies. Credit bureau records were requested for a sample of 1,000 households (i.e., 2,908 individuals) around the optimal bandwidth used to determine the survey sample. These data provide details on formal credit usage and delinquency between September 2020 and August 2021. Finally, the national death registry records all loss of life in the country between March 2020 and May 2021.

Our results show that the UCT program had mixed effects, both helping and hurting poor households. On one hand, the transfers increased economic activity in the short run. During the first six months of the pandemic, the transfer decreased the probability of closing a business by 38 percentage points, a 52 percent reduction relative to non-eligible households. Moreover, the household's probability of engaging in child labor in the medium run decreased. Even though we fail to detect changes in consumption or food and housing security, likely due to the timing of the survey, recipients of the UCT exhibited lower levels of financial stress in the short run (i.e., lower reliance on coping mechanisms such as savings and remittances). The treatment also led to sustained reductions of delinquency rates, which enabled households to substitute informal for formal credit sources in the medium run.

On the other hand, the transfer led to higher mortality rates in the first two months of the lockdown. We argue that the cash injection helped households to keep their businesses running, but this implied greater mobility, resulting in higher mortality rates. We show that greater mobility is mainly linked to working outside the household, as we did not find significant effects on the probability of leaving the household to shop at the market, visit banks, or exercise. While the treatment did not lead to significant changes in the probability of having a bank account, it did decrease usage of digital or phone payment platforms. The latter result suggests that treated households relied more on cash-based transactions related to home or work expenses. Since the transfer scheme was announced as a one-off transfer, beneficiary households may have treated the cash as a temporary capital injection, particularly given the extension of the stay-at-home order and the dilution of this positive income shock over time among larger households.

Indeed, we show that larger households, for whom the amount transferred represented a smaller per capita amount, faced the strongest incentives to bypass the mobility restrictions. Heterogeneous effects by household size show that the impact of the transfer on mortality is driven by households with more than four members. These households recorded larger short-run effects on business continuity, which made them more vulnerable to contagion. Higher density households may have also generated a compounding effect by increasing the risk of infection at home (Madewell et al. 2020). Since per capita transfers were relatively smaller for larger households, they did not record any impact on financial stress relative to the control and continued to rely on savings, remittances and informal loans as coping mechanisms. In contrast, smaller households increased their economic activity relative to non-eligible households of the same size and reduced their dependence on savings, remittances, and informal loans. In sum, smaller households still used the transfer as a capital injection, but the greater relative importance of the cash (as well as their lower density) appear to have contained virus transmission and thus mortality effects.

Our study broadly contributes to the literature on the impacts of cash transfer programs. Understanding the effects of emergency cash transfer programs is key to help governments better target and design future social protection schemes, particularly in the face of increased poverty rates after the pandemic (Decerf et al. 2023), slow economic recovery, and higher incidence and intensity of health and climate shocks. The COVID-19 pandemic constituted one of the biggest social protection events in history which revealed how traditional social protection schemes may not be sufficient to lift and keep people out of poverty.

More specifically, we contribute to the study of UCT programs during the COVID-19 emergency. The health emergency triggered a massive deployment of shock-responsive programs, but it remains to be seen if they helped households in a sustained manner. Although studies relying on high frequency measurements found immediate short-term effects on an array of food security outcomes (Aggarwal et al. 2022, Banerjee et al. 2020, Bird et al. 2023, Bottan et al. 2021, Brooks et al. 2022, Londoño-Vélez & Querubín 2022, Jaroszewicz et al. 2022, Stein et al. 2022), fewer studies have focused on the impact of emergency cash transfers on earnings and labor supply over time. UCT programs during the pandemic had positive short-term effects on earned income among poor households in Ghana (Karlan et al. 2022), on income per capita among vulnerable but non-poor households in Colombia (Alvarez et al. 2022), and on revenues, profits, and inventories of female microentrepreneurs in Kenya (Brooks et al. 2022). However, a separate study found that a supplemental UCT had no effect on the short-run labor supply of extremely poor households in Colombia (Londoño-Vélez & Querubín 2022), while another found that a one-off transfer to Venezuelan forced migrants in Peru had negative effects on short-term income and labor supply (Bird et al. 2023).

We build on the few rigorous studies that examine the effects of UCT programs on poor and vulnerable households during the COVID-19 pandemic. Our study is closely related to Londoño-Vélez & Querubín (2022) and Brooks et al. (2022), which rely on small randomized controlled trials to measure the impact of UCTs on poor households in Colombia and Kenya, respectively. We complement their findings in at least four ways. First, we present evidence on the impact of a sizable cash injection on poor urban households employed in the informal sector. While Londoño-Vélez & Querubín (2022) focused on the impact of a small recurrent transfer on extremely poor households, Brooks et al. (2022) analyzed the impact of a one-time transfer equivalent to a month of average profits on female microentrepreneurs. Second, as opposed to the Colombian or Kenyan cases, we study a setting in which mobile money or bank deposits were not used to deliver the transfer, which may lead to unintended mobility effects. Third, we extend the set of main outcomes and data sources by integrating administrative credit bureau and national mortality records in our analysis. On one hand, these outcomes measure important margins of adjustments during a pandemic as well as during other disasters and are novel relative to similar studies (Karlan et al. 2022, Londoño-Vélez & Querubín 2022, Brooks et al. 2022). On the other hand, the use of administrative data to measure some of the impacts allows us to deal with reporting biases, which may be more likely in distressing situations (Ross & Mirowsky 1984). Finally, our data allow us to examine the effects of emergency transfers over a longer period of time, up to 18 months after the launch of the program.

The Peruvian case is of particular interest. Although governments in Latin America provided generous emergency transfers, the region still accounted for roughly a quarter of global COVID-19 mortality, despite having only 8% of the global population (Mathieu et al. 2023). Peru was among the first countries in Latin America to introduce a shelter-in-place order (one of the longest and most extreme in the region) and implement an economic plan valued at nearly 22% of GDP (World Bank 2023), but the country still recorded the world's highest COVID mortality rate. Our results provide a mixed picture of the effects of emergency cash transfers and suggest that developing countries should focus more on adaptive social protection programs that help build resilience and reduce vulnerability during non-emergency settings.

The remainder of the paper is organized as follows. Section 2 provides background on the government's response to COVID-19 in Peru and describes its emergency cash transfer programs. Section 3 details the data accessed and collected for the analysis. Section 4 presents the regression discontinuity design, sample, and validity tests. Section 5 presents the results, while Section 6 examines heterogeneous effects to identify channels. Section 7 concludes.

# 2 Context

## 2.1 COVID-19 in Peru

Peru's first documented case of COVID-19 was detected on March 5. On March 11, the Ministry of Health (MINSA) declared a national health emergency for 90 days. A few days later, on March 16, a nationwide State of Emergency was enacted. Measures included border closures and a two-week mandatory shelter-in-place order. In conjunction with the lockdown notice, the government announced an initial one-off UCT program of 380 Peruvian

Soles (PEN) (approximately USD 112) for households qualifying as poor according to a government index. Up until then, no urban households were eligible to receive transfers as part of the government's pre-pandemic conditional cash transfer programs.

As elsewhere in the world, the lockdown was intended to restrict mobility and slow the spread of the virus, thus providing time to invest in personnel, equipment, and health infrastructure. Emergency cash transfers were intended to foster compliance with the mobility restrictions by offsetting the income loss among the most vulnerable households (Jaramillo & Ñopo 2020).

Despite the government's rapid response, the virus spread quickly and stressed the Peruvian health system, which already suffered from shortages of ICU beds, ventilators, medical supplies, and healthcare workers. At the start of the pandemic, the country, with a population of roughly 33 million, only had 217 ICU beds, rising to 1249 in April 2020, with just 576 ventilators.[1] Given the severity of the situation and the health system's lack of capacity, the lockdown was subsequently extended five times until June 30. Peru ultimately registered the globe's highest excess mortality rate (per 100,000 population) during the pandemic, with much of the differential coming in the first year of the public health emergency.[2]

Despite government efforts, toward the end of 2021 (at the time of our second survey) Peru had reversed many of the social gains made in the years prior to the pandemic (World Bank 2023). In 2020, 6.7 million jobs were lost during the first lockdown, with the economy contracting by 11% compared to 2019. Schools were closed for two full years, generating an estimated 1.7 learning-adjusted years of schooling (LAYS) loss. Despite an economic rebound in 2021, the national poverty rate returned to that last seen in 2012. Subsequent inflation, a rise in crime, and political instability further compounded the social and economic scars left by COVID-19 and mitigation measures and further hindered the country's post-pandemic

[1] See https://data.larepublica.pe/capacidad-sanitaria-en-peru-cuanto-cambio-nuestro-sistema-de-respuesta-a-dos-anos-de-la-pandemia/ and https://www.devex.com/news/inequality-and-corruption-why-peru-is-losing-its-covid-19-battle-97604.

[2] John Hopkins University, Coronavirus Research Center, Mortality Analyses. Available at https://coronavirus.jhu.edu/data/mortality

recovery.

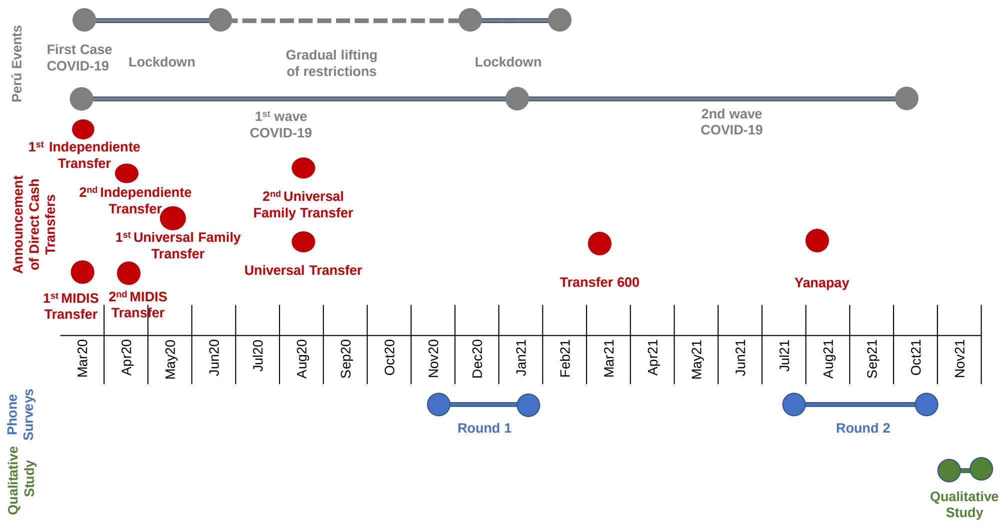
## 2.2 Emergency Cash Transfers in Peru

The government's initial support targeted rural and urban households living in poverty or extreme poverty. Rural households received in-kind transfers, while urban households received cash. The UCT scheme in urban areas was known as *Yo me quedo en casa* or "I'm staying home." The transfer's roll out began on March 23. Cash collection days were scheduled based on the final digit of the recipient's national document identification number. Given the scope of the emergency and its impact on vulnerable and middle-income households, the government extended the support in the first year of the pandemic through seven additional transfer schemes targeting non-poor households (see Appendix A for a description of these schemes).

The original amount of the *Yo me quedo en casa* transfer was PEN 380 (USD 112 at the exchange rate in March 2020) and was intended to support household incomes for a two-week lockdown. The first round was implemented between late March and September 2020. Although the subsidies were initially announced as a fixed one-off transfer, the government lengthened the support in parallel with the stay-at-home order extensions. In the first week of May, the government issued a decree to deliver a second tranche of PEN 380 per household between October 2020 and January 2021. In August, a second installment of PEN 760 was disbursed to the same beneficiaries. The total amount transferred, PEN 1520, was roughly equivalent to the cost of the monthly basic needs of a family of four (INEI 2020). See Figure 1 for a timeline of the pandemic waves, transfer announcements, and data-collection activities.

Relative to other countries in Latin America and the Caribbean, the total transfer provided by the Peruvian program corresponded to the top half of the programs in the region in terms of generosity. Almost half of the programs in the region consisted of one-off transfers, while the rest had two or more installments as in the Peruvian case (see Appendix Table C.1 for more details).

*Figure 1: Timeline: Pandemic, Announcement of Transfers and Data Gathering*




Source: Own elaboration.
Note: The red circles represent the moment in which the government decrees the Supreme or Emergency Decrees that authorize the transfers. Although the announcement of the bonuses is made on the dates indicated, the delivery of the benefits begins approximately two weeks later and extends for several additional weeks.

The transfers were delivered to a household representative chosen from among all adult members in the following order of priority: (i) members with an account at a financial institution, (ii) adult female, preferably under 60 years, and (iii) adult male, preferably under 60 years of age. Given that account ownership is low in Peru, particularly among the most disadvantaged, most transfers were collected in person and in cash.[3] About 80% of the transfer recipients were women.

# 3 Data

## 3.1 Government Administrative Data

We obtained administrative data from three government sources. First, we accessed the National Registry of Identification and Civil Status (RENIEC). The database contains records

[3]Findex data from before the pandemic indicate that only 27% of the poorest 40% have a bank account (World Bank, 2017).

for 35 million people, including household size, the relationship between household members, geographic location, and whether they reside in urban areas. This database included dummy variables that identified eligible recipients of the cash transfers announced between March and July 2020. Second, the Ministry of Development and Social Inclusion (MIDIS) provided a registry of Peruvian households with the Household Targeting Index (Índice de Focalización de Hogares, IFH), calculated by the Household Targeting System (Sistema de Focalización de Hogares, SISFOH) and used to classify households as poor or extremely poor. Finally, we obtained data from the national death registry (SINADEF) at the individual level covering the period between March 2020 and May 2021. As we have access to individual identification numbers as well as household IDs from the RENIEC data, we are able to calculate the number of deaths at the household level.

## 3.2 Household Surveys

We conducted two rounds of household phone surveys. Our target sample for the phone surveys focused on households closest to the centered SISFOH IFH score cutoff, following Cattaneo et al. (2019). Since the government threshold determining poverty status is set at the geographic domain level, we analyze the density of the IFH score around each regional threshold. All our estimates focus on three geographic domains (Central Coast, South Coast, and Lima) in which the density of the variable resembles a normal distribution (see Subsection 4.2.1).

Relying on administrative records from MIDIS, we chose 626,084 observations which were located around the optimal bandwidth ($h = 0.150$) to estimate the first stage. Using a first order polynomial and controlling for age and sex of the household head and district fixed effects, we estimate a 56 percentage point increase in the probability of receiving the transfer (see Appendix Table C.2). Given this discontinuity, assuming a transfer adoption rate equal to 0.9, and power of 0.8, our power calculations yield a total sample size of 800 households which allows us to detect an MDE of 0.14 standard deviations.

To allow for a margin of error as well as to facilitate robustness checks, we conservatively expanded the sample size in the first survey round to 5,000 households to be proportionally located around the cutoff. Since our expected response rate was 30%,[4] we provided a list of 22,284 households to achieve the number of target surveys. To reach 5,000 surveys, priority was given to households closest to the threshold. Since low response rates were a threat to the study, we designed an incentive scheme under which 80% of the households on each side of the cutoff were randomly selected and offered an incentive of 10 soles (2.7 USD), delivered as a phone recharge upon completion of the survey. The final response rate in the first survey was 25%, yielding 5,088 household surveys. Women represented 81% of individuals surveyed. In this survey round, the incentive did not yield any significant impact on the response rate.

The second wave of telephone surveys followed the same contact protocol used in the first wave, moving outwards from the cut-off point in both directions along the standardized IFH score. Based on preliminary analysis of the first wave, we recognized that we could reduce the sample size and still maintain statistical power. In the second wave, the response rate was 33%, resulting in 1,706 household surveys. Women represented 61% of the individuals surveyed. Incentives in the second survey had a significant but modest effect of 3.5 percentage points on response rates.

To apply the instrument, the Ministry of Economics and Finance (MEF) coordinated with OSIPTEL, the country's telecommunications regulator, to obtain the telephone numbers of household members included in the study sample. The first survey was administered between November 2020 and January 2021. Application of the survey took, on average, 27.5 minutes. The fieldwork coincided with the disbursement of the second transfer of PEN 760, between the first and second wave of COVID-19 infections in Peru. The second round of surveys was administered between July and October 2021, which coincided with the end of the second wave of COVID-19 infections. Survey application took, on average, 47 minutes.

The survey instrument was similar in both rounds. These data provide information about

[4] Our estimates come from conversations with other researchers as well as local fieldwork firms conducting phone surveys in Peru during the pandemic.

individual and household demographic characteristics, consumption expenditures, income shocks and coping strategies, as well as self-reported health status and household mortality. The survey also collected information on labor market participation of the respondent and her/his spouse, either as an employed or self-employed worker. The second survey round also included retrospective questions on household mobility during the first lockdown between March and June 2020.

## 3.3 Credit Bureau Data

We obtained credit records from EQUIFAX Peru, a private credit bureau with data on nearly 90% of the Peruvian credit market. EQUIFAX records include the current debt balance according to delinquency status (i.e., current versus delinquent debt) and origin of the funds, distinguishing between loans from traditional banks and credit unions and loans from microfinance NGOs or local cooperatives. EQUIFAX's data contain records on all individuals in Peru who have reached legal age, irrespective of previous debt obligations. RENIEC provides the bureau with monthly updates of the roster of people who are over age 18 in Peru. EQUIFAX's records capture an individual's credit standing at the time in which she is searched.

Relying on the distribution of the SISFOH IFH score, we defined a sample of 1,000 households around the cutoff. These 1,000 households were randomly selected within the optimal bandwidth used to determine the sample for the first survey wave. Records were requested for all members of the selected households, yielding a total sample of 2,908 individuals. We obtained four rounds of snapshots for the following dates: September 2020, January 2021, April 2021, and August 2021. The first two dates coincide with the period between the first and the second COVID-19 waves in Peru. January 2021 also coincides with the first survey collection period. August 2021 coincides with the end of the second COVID-19 infection wave and the application of the second survey.

### 3.4 Outcome Variables

Table C.3 in the Appendix shares details on the construction of all outcome variables. To measure the impact of the program on consumption smoothing, we rely on survey data and calculate household monthly expenditures per capita. To examine food security, we construct measures such as household monthly food expenditures as well as a dichotomous variable that indicates if any household member experienced hunger due to lack of food. Housing security is measured via an indicator variable that is equal to one whenever the household faced difficulties paying rent over the last 30 days.

Labor market outcomes are measured both among respondents and their partners. Using survey data, we measure the probability of losing a job as well as the probability of closing a business since the start of the pandemic, in March 2020. We also construct an indicator variable to measure child labor which is equal to one if any household member aged 18 or less worked for pay during the week preceding the survey.

Financial stress is measured using survey data with an index that summarizes household reliance on savings, remittances, and loans from family and friends since the start of the pandemic, in March 2020. We construct the standardized weighted average of each of these three binary variables following Anderson (2008). Additionally, to complement our view on household coping strategies, we rely on a binary variable that captures if any household member moved to another district since the start of the pandemic, in March 2020.

Access to credit bureau records for all household members allows us to examine credit usage and delinquency. We look at four outcomes variables: i) the probability of having outstanding debt, which is captured via a binary variable that is equal to one if any household member has outstanding current and/or expired debt in the financial system (excluding judicial, restructured, refinanced and written-off debt), ii) total outstanding debt, which includes current and expired debt, transformed using the inverse hyperbolic sine transformation; iii) the probability of having debt in arrears, measured with a binary variable that is equal to one if any household member had a 30 to 180 days delay in the payment of debt (excluding written-off debt); and iv) debt in arrears, equivalent to the total amount of household debt that is 30-180 days past due, also transformed using the inverse hyperbolic sine transformation.

Relying on SINADEF records, we construct two measures of mortality at the household level: cumulative and monthly mortality. The former is captured as a dichotomous variable equal to one if at least one household member passed away between March 2020 and the month of observation. The latter is measured via a dichotomous variable that is equal to one if at least one household member passed away during the month of observation.

Finally, to analyze mobility during the first lockdown (March-June 2020), we construct four binary variables to measure if the survey respondent declared to have left their home to work, shop at the market, visit the bank or other financial institution, or exercise.

# 4 Empirical Strategy

## 4.1 Regression Discontinuity Design

To estimate the impact of the transfer, we apply a fuzzy regression discontinuity design. We defined treatment status at the household level, with treated households being those eligible to receive the emergency cash transfer. The running variable is the standardized IFH score, which was used to allocate the transfer.

We estimate the effect for the optimal bandwidth (`rdrobust`) and various parametric bandwidths. Appendix Table C.4 shows that there is no evidence of manipulation of the running variable around the threshold while Sub-section 4.2 indicates that there are no important differences in terms of the main characteristics of households surrounding the threshold in the survey sample.

Our main equation to estimate is:

$$Y_{id} = \alpha + \beta Z_{id} + f(B_{id}) + \gamma X_{id} + \delta_d + \varepsilon_{id} \tag{1}$$

where $Y_{id}$ is the outcome variable for household $i$ located in district $d$. The variable $Z_{id}$ is an indicator with a value of one if family $i$ is a beneficiary of the cash transfer and zero otherwise. *f*($B_{id}$) is a function of the *running variable*, which can take linear or quadratic form.[5] $\varepsilon_{id}$ represents the error term, which we cluster at the district level. We also include a set of pre-pandemic administrative variables as controls in $X_{id}$, including gender of the household head, age of household head, and number of household members. These controls come from RENIEC's administrative data.[6] Finally, we include fixed effects at the district level, $\delta_d$.

## 4.2 Validity Tests

### 4.2.1 First Stage

The household eligibility criteria for the *Yo me quedo en casa* transfer allows us to use a fuzzy regression discontinuity design to estimate local effects. Since the cash transfer targeted poor households, eligibility was determined based on the IFH score, after imposing a few restrictions. Inclusion criteria included residence in urban areas and being classified as poor or extreme poor, while the exclusion criterion was participation in other emergency programs. Relying on the administrative records and eligibility criteria, we reproduce the selection rule for beneficiary households.[7] Imposing the regional cutoffs in all domains, we accurately predict the eligibility of 89.48% of the households tagged as eligible for the *Yo me quedo en casa* transfer.

Next, we estimate the change in the probability of receiving the transfer as a function of the IFH score. As mentioned above, the official thresholds for determining poverty status are

[5]We use linear local polynomials to measure the impact, but results are consistent when using the quadratic form (results available upon request).

[6]Administrative sources were preferable to survey data to proxy the pre-treatment situation as the pandemic likely affected household demographics recorded in the survey.

[7]Eligibility for the transfer *Yo me quedo en casa* was established in the Emergency Decrees in the midst of the COVID-19 crisis: 027-2020 and 044-2020. Due to the exclusion criterion, we drop about 98,000 out of 2.5 million eligible households.

defined at the level of geographic domain.[8] We analyzed the density of the IFH score around each regional threshold and selected three geographic domains (Central Coast, Southern Coast, and Lima) in which the density of the variable resembles a normal distribution (see Figure B.1).

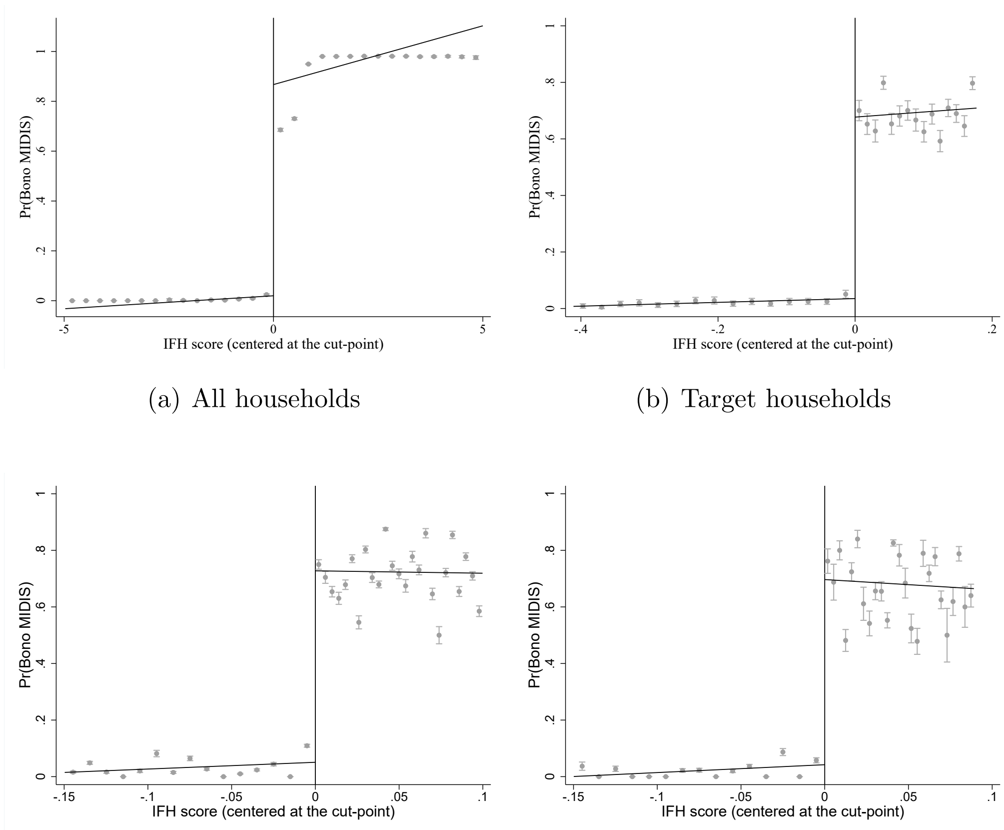
The first stage analysis also reveals that the official poverty thresholds by domain do not perfectly match the empirical cutoff determining transfer eligibility. Therefore, we chose an *empirical* threshold for each of the three domains. In practice, we focus on the households classified as *poor* in the transfer provision process, but whose IFH score is above the official poverty line of its corresponding geographic domain. We obtain the value of the IFH score at the fifth percentile of the score distribution in each of these samples by domain and use this value as the threshold around which we center the distribution of households. Figure B.1 in the Appendix presents the distribution and density of the running variable with all cut points set to zero.

After replicating the selection rule and imposing empirical cutoffs, we estimate the change in the probability to receive emergency cash transfers around the cutoff of the running variable. Panel (a) in Figure 2 depicts the jump using a first-order polynomial regression model, revealing a 56 percentage point increase in the probability of receiving the transfer at the cutoff (see Appendix Table C.2 for corresponding estimates). Panel (b) shows that a similarly sized discontinuity of about 55 percentage points remains when we restrict our estimates to the random sample of 22,284 households initially selected as targeted survey participants (see Appendix Table C.5).

Panels (c) and (d) in Figure 2 present the discontinuity for the households nearest to the threshold surveyed in the first and second waves of the phone surveys, respectively. The results confirm that the jump in the probability of receiving the transfer at the cut-off point

[8]These geographic domains are Northern Coast, Central Coast, Southern Coast, Northern Sierra, Central Sierra, Southern Sierra, the Amazon, and Lima. We used the latest version of these geographic domains identified by the National Institute of Statistics and Informatics (INEI). The values used as thresholds in each geographic domain can be found at http://www.sisfoh.gob.pe/el-sisfoh/que-es-el-sisfoh/normativa/send/7-normatividad/104- the-ministerial-resolution-n-151-2016-midis.

***Figure 2:*** *Yo me quedo en casa Cash Transfer - First Stage*




(a) All households
(b) Target households
(c) Surveyed households - Wave 1
(d) Surveyed households - Wave 2

NOTE: The vertical line denotes the threshold for the *running* variable. No control variables were included.

recorded for the universe of households carries over to both survey samples. Appendix Table C.6 provides the estimates of the first stage in the survey sample of the first wave. Relying on a linear fit, the estimated discontinuity in the sample of the first survey round indicates that being to the right of the cutoff increases the probability of receiving *Yo me quedo en casa* by approximately 51 percentage points. In the second survey sample, the jump is similar at 53 percentage points.

### 4.2.2 Balance

Even though we collected information on household characteristics, it is likely that survey responses reflect changes driven by the pandemic or the transfer itself (e.g. mortality rates or migration patterns may have affected household size or the identity of the household head). To avoid these biases, we use administrative data from before the transfer to test for balance in our survey samples. Available variables include sex and age of the household head. Household size was not considered because it is a component of the running variable. We also look at respondent's education as reported in the survey, since schooling levels among adults are not likely to be affected by the COVID-19 pandemic or the transfer. Finally, we examine household deaths in March 2020, which is the earliest month for which we have administrative mortality records. We argue that even though COVID-19 cases were first detected on March 5, mortality rates due to the pandemic occurred with a lag: there were less than 100 reported COVID-19 deaths in March 2020.

Table 1 presents balance tests for the first and second survey waves, labeled as short- and medium-term balance, respectively. Columns 2 and 6 present estimates for the optimal bandwidth, while additional columns with other bandwidths are included for robustness. In sum, both survey samples are balanced on either side of threshold.

***Table 1:*** *Balance Check*

| | Short-Term Balance | | | | Medium-Term Balance | | | |
|---|---|---|---|---|---|---|---|---|
| | Control Mean | MSE | 3/2 MSE | 1/2 MSE | Control Mean | MSE | 3/2 MSE | 1/2 MSE |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Household head (male) | 0.700 | 0.038 | -0.006 | -0.022 | 0.694 | -0.001 | -0.097 | 0.298* |
| | | (0.079) | (0.070) | (0.104) | | (0.118) | (0.095) | (0.167) |
| | | 1127 | 1587 | 517 | | 717 | 1035 | 321 |
| | | [-0.047 ; 0.047] | [-0.070 ; 0.070] | [-0.023 ; 0.023] | | [-0.059 ; 0.059] | [-0.089 ; 0.089] | [-0.030 ; 0.030] |
| Age of household head | 49.341 | 4.443 | 2.916 | 5.575 | 51.513 | 5.473 | 3.127 | 9.518** |
| | | (2.751) | (2.531) | (3.802) | | (3.815) | (2.977) | (4.682) |
| | | 1100 | 1552 | 503 | | 891 | 1123 | 430 |
| | | [-0.046 ; 0.046] | [-0.068 ; 0.068] | [-0.023 ; 0.023] | | [-0.076 ; 0.076] | [-0.114 ; 0.114] | [-0.038 ; 0.038] |
| Respondent with at least completed secondary education | 0.808 | -0.081 | -0.024 | -0.057 | 0.854 | 0.023 | 0.082 | -0.135 |
| | | (0.076) | (0.065) | (0.108) | | (0.084) | (0.065) | (0.125) |
| | | 861 | 1348 | 420 | | 642 | 940 | 287 |
| | | [-0.038 ; 0.038] | [-0.058 ; 0.058] | [-0.019 ; 0.019] | | [-0.053 ; 0.053] | [-0.079 ; 0.079] | [-0.026 ; 0.026] |
| Respondent has some university level studies or more | 0.224 | -0.029 | 0.010 | -0.068 | 0.290 | -0.023 | -0.002 | -0.241** |
| | | (0.063) | (0.052) | (0.080) | | (0.103) | (0.086) | (0.118) |
| | | 1326 | 1878 | 608 | | 727 | 1052 | 342 |
| | | [-0.057 ; 0.057] | [-0.086 ; 0.086] | [-0.029 ; 0.029] | | [-0.062 ; 0.060] | [-0.093 ; 0.091] | [-0.031 ; 0.030] |
| Deaths SINADEF in March 2020 | 0.004 | -0.013 | -0.009 | -0.036** | 0.003 | 0.010 | -0.001 | Insufficient variation |
| | | (0.011) | (0.006) | (0.017) | | (0.010) | (0.003) | |
| | | 916 | 1414 | 440 | | 458 | 721 | |
| | | [-0.041 ; 0.040] | [-0.061 ; 0.061] | [-0.020 ; 0.020] | | [-0.040 ; 0.040] | [-0.060 ; 0.060] | |

*Notes*: Balance checks are estimated at the household level. For each covariate, the estimated effect (row 1), clustered standard errors (row 2), the effective number of observations (row 3), and the optimal bandwidths (row 4) were computed using the `rdrobust` package, with a triangular weight matrix and MSE-optimally data-driven bandwidths selected for each side of the boundary. Linear local polynomials were used to construct the point estimator. Each specification includes district level fixed effects.
*** Significant at 1%, ** Significant at 5%, * Significant at 10%.

# 5 Results

## 5.1 Living Conditions and Consumption Smoothing

The first order intended effects of emergency transfers are related to a household's ability to smooth consumption, providing it with the means to cover basic needs in the face of a negative income shock. The intention of UCT programs deployed around the globe was to counteract the direct negative effects of the pandemic as well as the indirect effects of extended stay-at-home orders to foster compliance with the mobility restrictions.

Thus, we first examine the effects of the transfers on household monthly expenditures as well as on food and housing security. We focus both on the short-term effects, corresponding to the first wave of telephone surveys (November 2020 - January 2021), and the medium-term effects, measured with the second phone survey wave (July - October 2021). The first survey wave takes place after both round of transfers had been given out and while households were facing a second shelter-in-place order. The second survey wave was collected once stay-at-home orders were lifted and the emergency cash transfer programs were discontinued.

Results tables follow the same format throughout, with our preferred specifications in columns 2 and 6. Although results for the first stage are not reported given space constraints, they are statistically significant for all outcomes (results available upon request). Additional columns provide robustness checks by adjusting the bandwidth above and below the optimal.[9]

Table 2 suggests that the emergency transfer did not impact the household's ability to smooth consumption in the short run. At the optimal bandwidth (see column 2), monthly expenditures were unaffected relative to non-beneficiaries. When focusing on food expenditures, we are still unable to identify a significant effect of the transfer. These results are confirmed by a null impact on food and housing security, measured as the probability of experiencing hunger in the previous 7 days and the probability of having difficulties to pay rent in the previous 30 days, respectively. This pattern is sustained in the medium term. As

[9] Results for quadratic versions with optimal bandwidths were also examined. They were consistent with the main reported results and are available upon request.

reported in column 6, no significant effects were found in consumption or food and housing security once the transfers were discontinued.

***Table 2:*** *Effect of the* Yo me quedo en casa *Cash Transfer on Living Conditions and Consumption Smoothing*

| | Short-Term Effects | | | | Medium-Term Effects | | | |
|---|---|---|---|---|---|---|---|---|
| | Control Mean | MSE | 3/2 MSE | 1/2 MSE | Control Mean | MSE | 3/2 MSE | 1/2 MSE |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Household Monthly Expenditure (per capita) | 258.694 | -11.087 | 18.748 | -143.760** | 304.220 | -38.731 | -35.631 | 47.083 |
| | | (47.386) | (40.717) | (63.572) | | (47.979) | (48.868) | (44.903) |
| | | 594 | 1,009 | 315 | | 879 | 1,124 | 412 |
| | | [-0.028;0.028] | [-0.042;0.042] | [-0.014;0.014] | | [-0.074;0.073] | [-0.110;0.110] | [-0.037;0.037] |
| Household Monthly Food Expenditure (per capita) | 53.209 | 0.970 | 4.546 | -16.941 | 54.861 | -11.918 | -15.251 | 1.453 |
| | | (8.020) | (11.122) | (17.553) | | (11.703) | (10.771) | (17.313) |
| | | 847 | 1,317 | 411 | | 649 | 953 | 285 |
| | | [-0.038;0.038] | [-0.057;0.057] | [-0.019;0.019] | | [-0.056;0.051] | [-0.084;0.076] | [-0.028;0.025] |
| Hunger in the household over the last 7 days | 0.237 | 0.035 | -0.044 | 0.026 | 0.116 | -0.106 | -0.058 | -0.285** |
| | | (0.081) | (0.073) | (0.133) | | (0.083) | (0.064) | (0.112) |
| | | 671 | 1,118 | 346 | | 983 | 1,176 | 473 |
| | | [-0.031;0.031] | [-0.046;0.046] | [-0.015;0.015] | | [-0.081;0.081] | [-0.122;0.122] | [-0.041;0.041] |
| Difficulties to pay rent over the last 30 days | 0.864 | 0.037 | 0.062 | 0.069 | 0.854 | -0.108 | -0.121* | -0.111 |
| | | (0.066) | (0.060) | (0.090) | | (0.073) | (0.064) | (0.076) |
| | | 700 | 1,136 | 341 | | 877 | 1,095 | 421 |
| | | [-0.035;0.035] | [-0.052;0.052] | [-0.017;0.017] | | [-0.077;0.077] | [-0.116;0.116] | [-0.039;0.039] |

*Notes*: Effects are estimated at the household level. For each outcome, the estimated effect (row 1), clustered standard errors (row 2), the effective number of observations (row 3), and the optimal bandwidths (row 4) were computed using the `rdrobust` package, with a triangular weight matrix and MSE-optimally data-driven bandwidths selected for each side of the boundary. Linear local polynomials were used to construct the point estimator. Each specification includes the following covariates: sex and age of household head (from RENIEC administrative data), and district level fixed effects.
*** Significant at 1%, ** Significant at 5%, * Significant at 10%.

Previous studies of the effects of emergency transfers using high-frequency surveys conducted immediately after the transfer and at the beginning of the pandemic, when stricter stay-at-home orders were in place, indicate robust effects on food security (Aggarwal et al. 2022, Banerjee et al. 2020, Bird et al. 2023, Bottan et al. 2021, Brooks et al. 2022, Londoño-Vélez & Querubín 2022, Jaroszewicz et al. 2022, Stein et al. 2022). However, they also show that the effects on food security dissipate quickly, which may explain our results. It is likely that, by the time we collected our first survey round, too much time had passed since the last transfer collection. In fact, at the time of the first survey some mobility restrictions were still in place, but they were not as limiting as in the first three-and-a-half months of the pandemic in Peru (see Figure 1). These laxer restrictions may have fostered reentry into the labor market, dissipating the potential effects of the transfer. Nevertheless, notice that the transfer supported housing and food security in the medium run under certain bandwidths (i.e., column (8) for hunger in the past week and column (7) for difficulties to pay rent).

Although these effects are not robust, they could signal that the transfer may have played a role in securing household food and housing consumption through short-run investments.

## 5.2 Economic Activity

The *Yo me quedo en casa* transfer targeted poor households that relied mostly on earnings from the informal sector, as most target beneficiaries were self-employed or had a business. The transfer thus sought to support households who had limited chances of sustaining their labor market participation during the lockdown.

The effectiveness of the transfer to reduce mobility may hold whenever the income elasticity of labor is negative, i.e., households that benefit from a positive income shock reduce their labor supply (and stay at home). However, this assumption has been questioned in non-emergency contexts (Bastagli et al. 2018, Banerjee et al. 2021, Kaur et al. 2022, Vera-Cossio 2022) and remains an open issue given the limited evidence available.

***Table 3:*** *Effect of the* Yo me quedo en casa *Cash Transfer on Economic Activity*

| | **Short-Term Effects** | | | | **Medium-Term Effects** | | | |
|---|---|---|---|---|---|---|---|---|
| | Control Mean | MSE | 3/2 MSE | 1/2 MSE | Control Mean | MSE | 3/2 MSE | 1/2 MSE |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Respondent or partner lost job | 0.627 | -0.043 | -0.043 | -0.175 | 0.472 | 0.163 | -0.023 | 0.167 |
| | | (0.084) | (0.071) | (0.108) | | (0.147) | (0.149) | (0.223) |
| | | 1,049 | 1,480 | 479 | | 472 | 619 | 217 |
| | | [-0.044;0.044] | [-0.066;0.066] | [-0.022;0.022] | | [-0.073;0.069] | [-0.109;0.104] | [-0.036;0.035] |
| Respondent or partner closed business due to pandemic | 0.729 | -0.377*** | -0.328*** | -0.716*** | 0.256 | -0.041 | 0.014 | -0.147 |
| | | (0.125) | (0.110) | (0.181) | | (0.102) | (0.091) | (0.151) |
| | | 369 | 556 | 182 | | 339 | 485 | 155 |
| | | [-0.037;0.037] | [-0.055;0.055] | [-0.018;0.018] | | [-0.043;0.043] | [-0.065;0.065] | [-0.022;0.022] |
| Any child in the household had to work over the last 7 days | 0.112 | -0.050 | -0.017 | 0.038 | 0.093 | -0.160** | -0.117** | -0.259*** |
| | | (0.055) | (0.050) | (0.101) | | (0.064) | (0.052) | (0.080) |
| | | 528 | 848 | 247 | | 673 | 796 | 346 |
| | | [-0.039;0.039] | [-0.059;0.058] | [-0.020;0.019] | | [-0.085;0.082] | [-0.127;0.123] | [-0.042;0.041] |

*Notes*: Effects are estimated at the household level. The assessment of job loss and business closure spans from March to October 2020 in the first survey wave, and from January to June 2021 in the second survey wave. Child labor, on the other hand, is based on the week preceding the interview date. For each outcome, the estimated effect (row 1), clustered standard errors (row 2), the effective number of observations (row 3), and the optimal bandwidths (row 4) were computed using the `rdrobust` package, with a triangular weight matrix and MSE-optimally data-driven bandwidths selected for each side of the boundary. Linear local polynomials were used to construct the point estimator. Each specification includes the following covariates: sex and age of household head (from RENIEC administrative data), and district level fixed effects.
*** Significant at 1%, ** Significant at 5%, * Significant at 10%.

Indeed, the first row in Table 3 shows that the transfer did not affect the respondent's or partner's probability of losing a job in the short run. It also had a null effect on the probability of having an underage member of the household working. Yet, the transfer affected the household's probability of keeping a business running. By providing a cash influx, the transfers seem to have helped households to keep their businesses open, even during the second lockdown. The second row in Table 3 shows that the probability of closing a business in the short run was significantly reduced among transfer recipients: they were 52 percent less likely to shut down their businesses relative to the control group. This finding is robust across multiple bandwidths.

In the medium run, between January and June 2021, the impact on business closure dissipates. Comparison between the control means across both waves suggests that this was driven by a catch up effect among the control group, which exhibited a large drop in the probability of closing a business during 2021. However, we find a significant reduction of child labor among beneficiary households. This result indicates that the transfer may have had an impact on intra-household time allocation, protecting children in the family unit.

Jointly, the short- and medium-run effects on labor market participation suggest that the transfer may have been used as short-term working capital during 2020. The survival of these largely informal microenterprises may also explain the lower incidence of child labor in the medium term. This result implies that, in the Peruvian emergency setting, work and income were complements, at least in the short run.

Finally, survey data on mobility during the lockdown allows us to test the effect of the transfer on the probability of leaving home for one of several reasons, including working, going to the market, visiting a bank branch, or exercising. Table C.7 confirms the emerging story: household respondents in the treatment group were 72% more likely to leave home to work during the first lockdown (March-June 2020), while no differences were found for going to the market, visiting financial providers, or exercising. Furthermore, while no significant effects were found for the probability of having a bank account, the transfer did decrease usage of digital or phone payment platforms, suggesting that recipient households may have relied more on cash-based transactions related to home or work expenses.

## 5.3 Financial Stress

The negative effects of the transfers on business closures indicates that the program did not incentivize households to shelter-in-place, but instead recipients may have used the subsidy to maintain self-employment. Still, the total amount transferred was diluted in the several weeks that the lockdown lasted and households may have resorted to alternative and/or complementary coping strategies. Furthermore, the capital injection may have also altered the way in which beneficiaries adapted relative to control households.

Households can buffer consumption through the use of savings, remittances, informal loans, formal credit, or even temporary migration. Following Anderson (2008), we construct a financial stress index equivalent to the standardized weighted average of the binary variables that measures the likelihood of using savings, remittances, and informal loans. Table 4 presents the local effects of the transfer on the probability of relying on these strategies. In general, the transfer significantly reduced the level of financial stress that beneficiary households faced in the short term. This effect was driven by a lower reliance on savings and remittances. Meanwhile, the transfer had a positive effect on the probability of migrating to deal with the shock, likely because it reduced liquidity constraints as seen with transfers non-emergency contexts (Bryan et al. 2014, Angelluci 2015). In the medium run, most effects on coping mechanisms dissipated, which is consistent with the medium-term results on labor market participation (see column 6 in Table 3). However, a negative effect on informal loan use emerges.

***Table 4:*** *Effect of the* Yo me quedo en casa *Cash Transfer on Coping Strategies*

| | **Short-Term Effects** | | | | **Medium-Term Effects** | | | |
|---|---|---|---|---|---|---|---|---|
| | Control Mean | MSE | 3/2 MSE | 1/2 MSE | Control Mean | MSE | 3/2 MSE | 1/2 MSE |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Financial Stress Index [0,1] | 0.423 | -0.187*** | -0.170*** | -0.167*** | 0.408 | -0.163** | -0.136** | -0.180* |
| | | (0.048) | (0.045) | (0.065) | | (0.079) | (0.065) | (0.092) |
| | | 666 | 1,097 | 341 | | 596 | 868 | 261 |
| | | [-0.030;0.030] | [-0.045;0.045] | [-0.015;0.015] | | [-0.048;0.048] | [-0.072;0.072] | [-0.024;0.024] |
| Used savings since March 2020 (0,1) | 0.499 | -0.271*** | -0.178** | -0.149 | 0.570 | -0.147 | -0.084 | -0.298 |
| | | (0.101) | (0.089) | (0.113) | | (0.131) | (0.112) | (0.193) |
| | | 657 | 1,078 | 335 | | 572 | 823 | 246 |
| | | [-0.030;0.030] | [-0.045;0.045] | [-0.015;0.015] | | [-0.046;0.046] | [-0.069;0.069] | [-0.023;0.023] |
| Received remittances (0,1) | 0.146 | -0.171*** | -0.158*** | -0.182*** | 0.107 | -0.073 | -0.070 | -0.061 |
| | | (0.057) | (0.051) | (0.067) | | (0.066) | (0.054) | (0.080) |
| | | 1,233 | 1,705 | 545 | | 979 | 1,175 | 473 |
| | | [-0.053;0.052] | [-0.079;0.078] | [-0.026;0.026] | | [-0.081;0.081] | [-0.121;0.121] | [-0.040;0.040] |
| Borrowed from friends and/or family (0,1) | 0.602 | -0.114 | -0.093 | 0.006 | 0.501 | -0.293* | -0.243* | -0.194 |
| | | (0.088) | (0.074) | (0.117) | | (0.154) | (0.131) | (0.195) |
| | | 995 | 1,423 | 455 | | 608 | 879 | 265 |
| | | [-0.041;0.041] | [-0.062;0.062] | [-0.021;0.021] | | [-0.049;0.049] | [-0.074;0.073] | [-0.025;0.024] |
| Migrated (0,1) | 0.028 | 0.042** | 0.025 | 0.052** | 0.102 | 0.042 | 0.033 | 0.184** |
| | | (0.021) | (0.021) | (0.025) | | (0.066) | (0.057) | (0.092) |
| | | 974 | 1,395 | 446 | | 589 | 867 | 259 |
| | | [-0.041;0.041] | [-0.062;0.062] | [-0.021;0.021] | | [-0.052;0.048] | [-0.078;0.072] | [-0.026;0.024] |

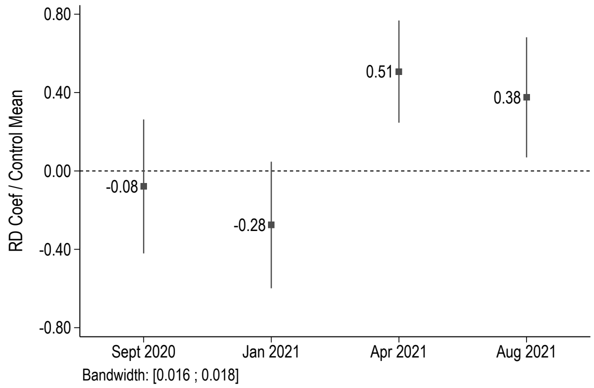
*Notes*: Effects are estimated at the household level. The assessment of remittances and informal loans spans from March to October 2020 in the first survey wave, and from January to June 2021 in the second survey wave. The financial stress index is constructed as a standardized weighted average of the binary variables that measure the likelihood of having used savings, remittances, and informal loans, as in Londoño-Vélez & Querubín (2022) and Anderson (2008). For each outcome, the estimated effect (row 1), clustered standard errors (row 2), the effective number of observations (row 3), and the optimal bandwidths (row 4) were computed using the `rdrobust` package, with a triangular weight matrix and MSE-optimally data-driven bandwidths selected for each side of the boundary. Linear local polynomials were used to construct the point estimator. Each specification includes the following covariates: sex and age of household head (from RENIEC administrative data), and district level fixed effects.
*** Significant at 1%, ** Significant at 5%, * Significant at 10%.

To have a more complete picture of borrowing strategies, we examine the effect of the transfer on formal credit usage. Panel (a) in Figure 3 presents the dynamic effects of the transfer on the probability of having credit from a bank or other formal financial institution between September 2020 and August 2021. Relative to the control group, the likelihood of having outstanding debt decreased among transfer recipients in the short run, narrowly missing traditional levels of significance in January 2021. Yet after January 2021 (i.e., during the second COVID-19 wave), the probability of having a formal loan increased significantly in both April and August 2021.

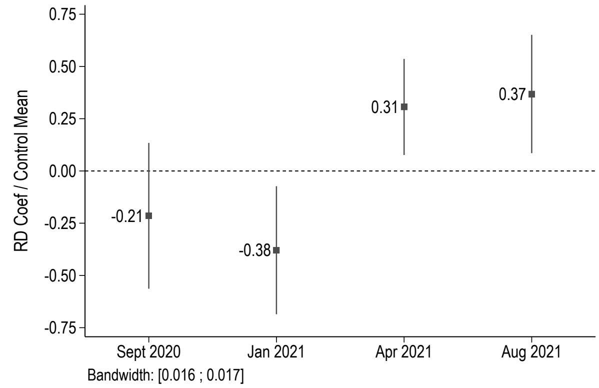
Panel (b) in Figure 3 reports the dynamic effects of the transfer on total outstanding debt, without conditioning on having a loan. Once more, transfer recipients exhibited a tendency to reduce their debt portfolios in the short run, but expanded them in the medium run.

In sum, the results on coping strategies suggest that transfer recipients relied less on

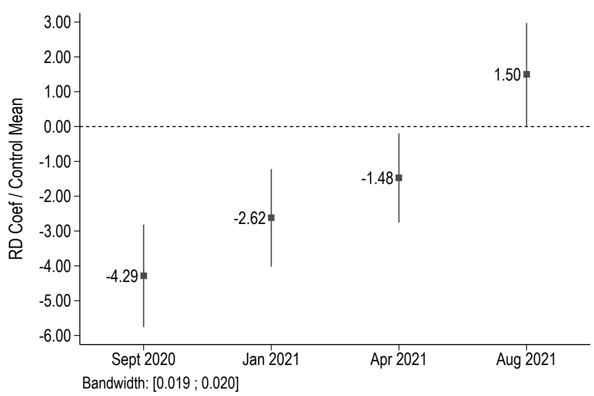
***Figure 3:*** *Effect of the* Yo me quedo en casa *Cash Transfer on Credit Usage and Delinquency*



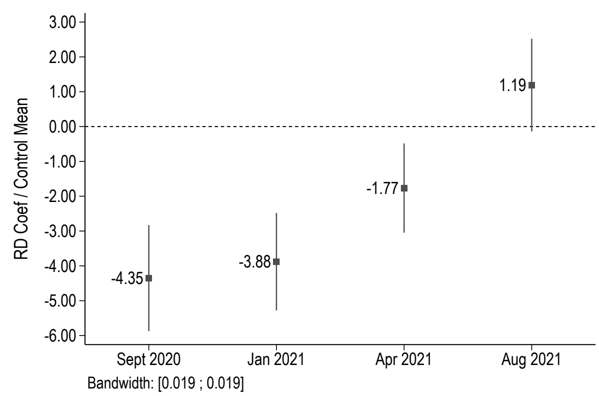

(a) Pr(Outstanding Debt)




(b) Total Outstanding Debt




(c) Pr(Arrears)




(d) Debt in Arrears

*Notes*: Total outstanding debt considers current and expired debt, excluding judicial, restructured, refinanced and written-off debts. Pr(Outstanding Debt) is an indicator variable equal to one of any household member has positive levels of outstanding debt. Pr(Arrears) is an indicator variable equal to one if a household member had a 30-180 days delay in the payment of a debt, excluding written-off debts. Total outstanding debt and debt in arrears are transformed using the inverse hyperbolic sine transformation. For debt in arrears, we impute a zero to households for which its debt has already been written-off. Effects are estimated at the household level using the `rdrobust` package, with a triangular weight matrix and MSE-optimally data-driven bandwidths selected for each side of the boundary. Linear local polynomials were used to construct the point estimator. For graphical purposes, the bandwidth used in estimation is the average of the 4 optimal bandwidths obtained individually for each month. The graph shows the ratio between the RD estimate and the control mean computed among the households who did not get the transfer and whose centered IFH score lies inside the average bandwidths, along with the ratio's 95% confidence interval. Each specification includes the following covariates: sex and age of household head (from RENIEC administrative data), and district level fixed effects. Detailed estimates are available in Appendix Tables C.8 and C.9.

alternative coping mechanisms such as savings, remittances, and formal loans in the short run (see detailed estimates in Appendix Tables C.8 and C.9). However, in the medium term, beneficiaries decreased their usage of informal loans from friends and family, who may have also been strained by the pandemic, and increased their reliance on formal credit. In particular, we find that the effect on formal borrowing was present not only in the extensive margin (i.e., the probability of having outstanding debt) but also in the intensive margin (i.e., total outstanding debt).

The substitution effect of informal for formal credit sources in the medium run suggests that, relative to the control group, treated households ran businesses that were in better shape to capture external formal funding after the peak of the pandemic and relaxation of mobility restrictions. We look deeper into this channel by analyzing the effect of the transfer on the households' ability to repay their debts on time. At the national level, the 90–day delinquency rate grew by a third between March 2020 and December 2020, moving from 2.8% to 3.7%.[10] In the short run, the transfer may have made beneficiaries more likely to repay past loans in a timely manner. Indeed, Panels (c) and (d) in Figure 3 confirm that both the probability of having a delinquent loan and the amount of delinquent debt initially fall among treated households relative to the control. Between September 2020 and April 2021 transfer recipients were less likely to be in arrears and had lower total delinquent loan amounts. The transfer may have improved recipients' credit scores relative to the control group by helping households stay on track with payments at the start of the pandemic. This effect may have motivated lenders to extend more credit to program beneficiaries. Yet the effect on delinquency dissipates, coinciding with the end of the program and stay-at-home orders.

## 5.4 Mortality

The previous sections provide suggestive evidence on a positive link between the transfer and labor market participation. Beneficiary households were less likely to close a family business, but are thus suspected to have complied less with the mobility restrictions.

Given that the transfers were mostly collected in person and their use (for either consumption or business investment) required greater mobility, beneficiary households may have been exposed to greater risks of contagion. The possibility of increased exposure was particularly

[10] See periodic SBS reports on the evolution of the financial system in Peru here.

worrisome during the initial stages of the pandemic when government information campaigns were incipient, the use of masks was still not normalized, there was less understanding of COVID-19 treatment options, and vaccines were not yet available.

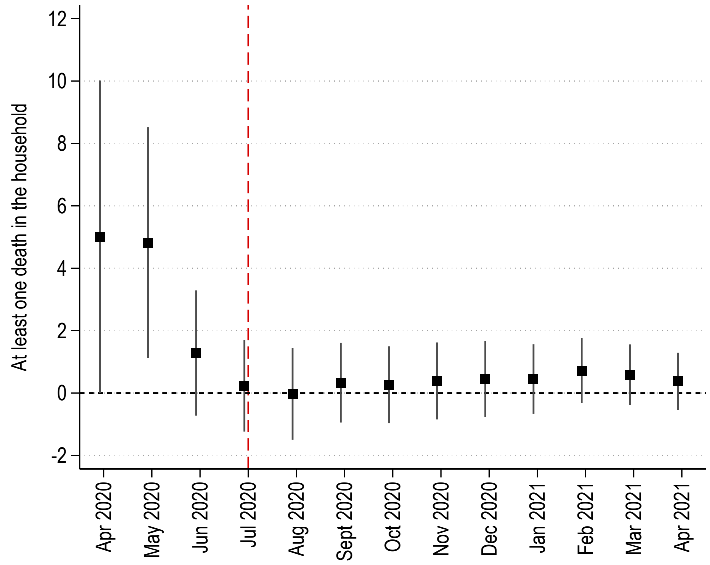
The compound effect of greater labor market participation and cash collection and usage likely increased mobility of beneficiary household members. In fact, Table C.7 confirms that treated households reported greater levels of mobility, but this was exclusively driven by leaving the house to work. The transfer did not lead to significant differences in mobility patterns related to shopping, visiting a bank branch, or exercising in the short run. However, transfer beneficiaries relied less on digital or phone payment platforms. These greater levels of mobility may have led to higher morbidity and mortality rates. We thus analyze the effect of the transfers on cumulative mortality using daily records at the individual level from SINADEF. These data are aggregated at the monthly and household level to measure the dynamic effects of the transfer on the probability of losing at least one household member since the beginning of the pandemic in March 2020.

Figure 4 reveals that the *Yo me quedo en casa* transfer had a positive causal effect on cumulative mortality rates at the beginning of the pandemic. This effect was concentrated in the first two months of the public health emergency, while the population was still facing a strict lockdown and overall mobility was at its nadir (Bird et al. 2023). Following the end of the first lockdown, in July 2021, cumulative mortality differences disappeared.[11]

These results are consistent with Londoño-Vélez & Querubín (2022) who argue that the Colombian UCT induced households to leave their homes due to limited adoption of mobile money. It is also consistent with households' usage of the transfer as working capital for the family business. This pattern is supported by Brooks et al. (2022), who find that female entrepreneurs in Kenya who benefited from a one-off emergency transfer increased their inventory expenditures.

[11]Detailed estimates on the impact of the transfer on cumulative mortality and monthly mortality are presented in Appendix Tables C.10 and C.11.

***Figure 4:*** *Effects of the* Yo me quedo en casa *Cash Transfer on Cumulative Mortality*




*Notes*: The dependent variable is the cumulative mortality rate, constructed as a binary indicator equal to one if at least one household member passed away between March 2020 and each corresponding month. Effects are estimated at the household level using the `rdrobust` package, with a triangular weight matrix and MSE-optimally data-driven bandwidths selected for each side of the boundary. Linear local polynomials were used to construct the point estimator. For graphical purposes, the bandwidth used in estimation is the average of the 13 optimal bandwidths obtained individually for each month. The graph shows the ratio between the RD estimate and the control mean computed among the households who did not get the transfer and whose centered IFH score lies inside the average bandwidths, along with the ratio's 95% confidence interval. Each specification includes the following covariates: sex and age of household head (from RENIEC administrative data), and district level fixed effects. The dotted line represents the end of the lockdown in the country.

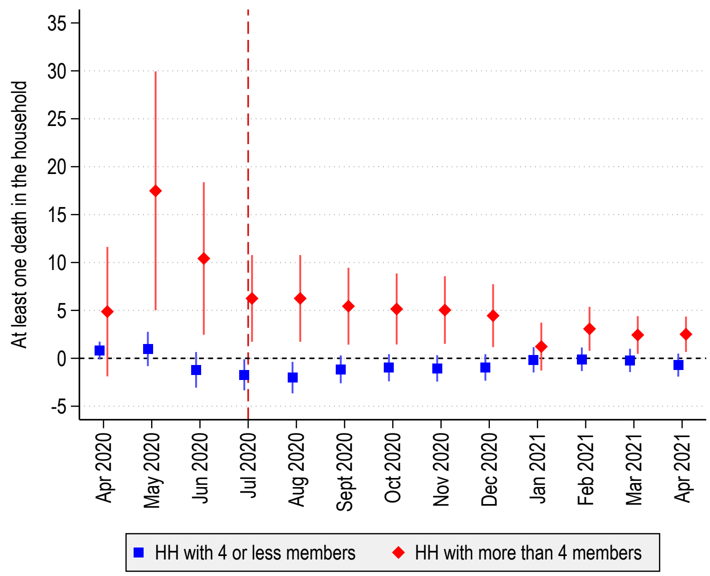
# 6 Heterogeneous Effects

The main results indicate that the transfer represented a cash injection that helped households keep their businesses running, but led to greater mobility and, consequently, higher mortality rates at the beginning of the pandemic. Since the transfer scheme was initially announced as a one-off transfer, which was later repeated and discontinued, beneficiary households may have treated the cash as a temporary capital injection.

Given the (unexpected) extension of the stay-at-home orders and dilution of the transfer over time and across household members, we examine if the effects on mortality and labor market participation are driven mostly driven by larger households, for whom the amount transferred represented a smaller per capita amount. These households were more likely to find the transfer insufficient and thus faced the strongest incentives to bypass the mobility restrictions to secure additional income.

Indeed, heterogeneous effects by household size show that the impact of the transfer on mortality is driven by households with more than four members. Figure 5 presents the effect of the transfer on cumulative mortality by household size. Clearly, the mortality effects detected in Figure 4 were exclusively driven by larger households. Furthermore, these effects seem to persist over time until December 2020. Interestingly, the transfer's effects among smaller households are null or negative.

***Figure 5:*** *Heterogeneous Effects on Cumulative Mortality by Household Size*




*Notes*: The dependent variable is the cumulative mortality rate, constructed as a binary indicator equal to one if at least one household member passed away up until each corresponding month. Effects are estimated at the household level using the `rdrobust` package, with a triangular weight matrix and MSE-optimally data-driven bandwidths selected for each side of the boundary. Linear local polynomials were used to construct the point estimator. For graphical purposes, the bandwidth used in estimation is the average of the 13 optimal bandwidths obtained individually for each month. The graph shows the ratio between the RD estimate and the control mean computed among the households who did not get the transfer and whose centered IFH score lies inside the average bandwidths, along with the ratio's 95% confidence interval. Each specification includes the following covariates: sex and age of household head (from RENIEC administrative data), and district level fixed effects. The dotted line represents the end of the lockdown in the country.

We further examined other household outcomes by household size to determine whether the mortality effects can be traced through them. Table 5 presents heterogeneous effects by household size on labor market participation and coping strategies in the short run. First, notice that there were less business closures in both small and large households that benefited from the transfer. The magnitude of the effect is greater among larger than smaller households. While the former reduce their probability of facing business closures by 42%, the latter had a corresponding 32% effect. This result is aligned with the heterogeneous effects on mobility by household size presented in Table C.12. The impact of the transfer on increased mobility due to work or the need to exercise was greater among respondents in larger households. At the same time, respondents in larger households were less likely to leave the house to visit a bank branch.

Second, several differences arise in terms of household coping strategies. On one hand, smaller households used less savings, borrowed less from family and friends, and relied less on remittances compared to non-eligible small households. On the other hand, larger households who were eligible for the transfer exhibit no significant differences in terms of their use of alternative coping strategies.

In sum, both small and large households seem to have relied on the transfer as a capital injection for their businesses. However, larger households that benefited from the transfer were relatively more likely to continue operating their businesses. This implied greater mobility and a higher risk of contagion and mortality both due to more active participation in the labor market and higher household density. Moreover, smaller households had less of a need to rely on additional coping strategies such as savings, remittances, or informal credit, while larger households were not able to reduce their reliance on such coping mechanisms. This effect was likely linked to the dilution of the cash injection among more household members in larger households.

**Table 5:** *Short-term Impacts of the* Yo me quedo en casa *Cash Transfer by Household Size*

| | HH has 4 or less members | | | | HH has more than 4 members | | | |
|---|---|---|---|---|---|---|---|---|
| | Control Mean | MSE | 3/2 MSE | 1/2 MSE | Control Mean | MSE | 3/2 MSE | 1/2 MSE |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| ***Panel A: Economic Activity*** | | | | | | | | |
| Respondent or partner lost job | 0.602 | 0.029 | -0.006 | 0.010 | 0.665 | -0.186 | -0.074 | -0.306 |
| | | (0.083) | (0.076) | (0.101) | | (0.156) | (0.124) | (0.249) |
| | | 789 | 1,104 | 371 | | 343 | 508 | 144 |
| | | [-0.052;0.052] | [-0.078;0.078] | [-0.026;0.026] | | [-0.044;0.044] | [-0.065;0.065] | [-0.022;0.022] |
| Respondent or partner closed business due to pandemic | 0.676 | -0.217* | -0.015 | -0.353*** | 0.833 | -0.352* | -0.260* | 0.183 |
| | | (0.115) | (0.089) | (0.126) | | (0.211) | (0.147) | (0.392) |
| | | 354 | 494 | 180 | | 191 | 259 | 77 |
| | | [-0.056;0.056] | [-0.084;0.083] | [-0.028;0.028] | | [-0.050;0.050] | [-0.075;0.074] | [-0.025;0.025] |
| Any child in the household had to work over the last 7 days | 0.091 | -0.120** | -0.078* | -0.174*** | 0.160 | 0.236** | 0.236*** | 0.073 |
| | | (0.053) | (0.041) | (0.065) | | (0.115) | (0.079) | (0.156) |
| | | 715 | 1,030 | 406 | | 185 | 307 | 80 |
| | | [-0.092;0.087] | [-0.138;0.130] | [-0.046;0.043] | | [-0.036;0.035] | [-0.053;0.052] | [-0.018;0.017] |
| ***Panel B: Coping Strategies*** | | | | | | | | |
| Household Monthly Expenditure (per capita) | 294.073 | -59.959 | -48.995 | -276.790*** | 207.874 | 75.961 | 123.377* | -54.922 |
| | | (47.601) | (42.545) | (74.287) | | (80.650) | (71.671) | (106.414) |
| | | 459 | 751 | 243 | | 301 | 472 | 132 |
| | | [-0.032;0.032] | [-0.048;0.048] | [-0.016;0.016] | | [-0.039;0.039] | [-0.059;0.059] | [-0.020;0.020] |
| Credit (0,1) - Sept20 | 0.552 | 0.180** | 0.101 | 2.823*** | 0.727 | -0.755*** | -1.521*** | 4.363*** |
| | | (0.074) | (0.093) | (0.207) | | (0.084) | (0.310) | (0.000) |
| | | 114 | 158 | 59 | | 42 | 73 | 21 |
| | | [-0.016;0.018] | [-0.025;0.027] | [-0.008;0.009] | | [-0.016;0.018] | [-0.025;0.027] | [-0.008;0.009] |
| Credit (0,1) - Jan21 | 0.598 | 0.162*** | -0.020 | 2.350*** | 0.758 | -0.755*** | -1.491*** | 4.363*** |
| | | (0.062) | (0.083) | (0.176) | | (0.084) | (0.310) | (0.000) |
| | | 114 | 158 | 59 | | 42 | 73 | 21 |
| | | [-0.016;0.018] | [-0.025;0.027] | [-0.008;0.009] | | [-0.016;0.018] | [-0.025;0.027] | [-0.008;0.009] |
| Financial Stress Index [0,1] | 0.420 | -0.182*** | -0.149*** | -0.292*** | 0.413 | -0.143 | -0.152 | 1.421 |
| | | (0.048) | (0.043) | (0.063) | | (0.106) | (0.097) | (1.391) |
| | | 721 | 1,007 | 345 | | 197 | 323 | 97 |
| | | [-0.045;0.045] | [-0.068;0.068] | [-0.023;0.023] | | [-0.028;0.028] | [-0.042;0.042] | [-0.014;0.014] |
| Used savings since March 2020 (0,1) | 0.512 | -0.186** | -0.097 | -0.504*** | 0.504 | 0.043 | -0.121 | -1.742 |
| | | (0.085) | (0.080) | (0.129) | | (0.191) | (0.197) | (1.836) |
| | | 693 | 957 | 329 | | 203 | 342 | 101 |
| | | [-0.043;0.043] | [-0.064;0.064] | [-0.021;0.021] | | [-0.029;0.029] | [-0.043;0.043] | [-0.014;0.014] |
| Receives remittances over last 6 months (0,1) | 0.142 | -0.194*** | -0.184*** | -0.220*** | 0.126 | -0.150 | -0.164 | 0.132 |
| | | (0.062) | (0.054) | (0.071) | | (0.125) | (0.108) | (0.233) |
| | | 813 | 1,158 | 377 | | 291 | 465 | 130 |
| | | [-0.054;0.054] | [-0.081;0.081] | [-0.027;0.027] | | [-0.039;0.039] | [-0.058;0.058] | [-0.019;0.019] |
| Borrowed from friends and/or family (0,1) | 0.567 | -0.174* | -0.172** | -0.111 | 0.620 | -0.006 | -0.066 | -0.043 |
| | | (0.091) | (0.084) | (0.111) | | (0.150) | (0.121) | (0.217) |
| | | 682 | 946 | 324 | | 453 | 633 | 194 |
| | | [-0.042;0.042] | [-0.063;0.063] | [-0.021;0.021] | | [-0.055;0.055] | [-0.083;0.082] | [-0.028;0.027] |
| Migrated (0,1) | 0.021 | 0.036* | 0.016 | 0.044 | 0.018 | 0.014 | 0.034 | -0.039 |
| | | (0.020) | (0.021) | (0.032) | | (0.028) | (0.034) | (0.024) |
| | | 519 | 819 | 271 | | 287 | 455 | 126 |
| | | [-0.036;0.036] | [-0.054;0.054] | [-0.018;0.018] | | [-0.040;0.040] | [-0.060;0.060] | [-0.020;0.020] |

*Notes*: Effects are estimated at the household level. The assessment of job loss, business closure, remittances, and informal loans spans from March to October 2020. The financial stress index is constructed as a standardized weighted average of the binary variables that measure the likelihood of having used savings, remittances, and informal loans, as in Londoño-Vélez & Querubín (2022). For each outcome, the estimated effect (row 1), clustered standard errors (row 2), the effective number of observations (row 3), and the optimal bandwidths (row 4) were computed using the `rdrobust` package, with a triangular weight matrix and MSE-optimally data-driven bandwidths selected for each side of the boundary. Linear local polynomials were used to construct the point estimator. For credit, the bandwidth used in estimation is the average of the 4 optimal bandwidths obtained individually for each available month. Each specification includes the following covariates: sex and age of household head (from RENIEC administrative data), and district level fixed effects.
*** Significant at 1%, ** Significant at 5%, * Significant at 10%.

# 7 Conclusion

This study examines the short- and medium-run effects of the emergency cash transfer program targeting poor urban households in Peru during the COVID-19 pandemic. Using a fuzzy regression discontinuity design and survey and administrative data, we provide evidence on the impact of the transfer on a myriad of outcomes such as household expenditures, food and housing security, labor market participation, financial health, and mortality. This multidimensionality of outcomes provides a comprehensive view of the effect of the transfer on the well-being of beneficiary households in a developing country.

Taken together, the results confirm that the UCT program had mixed effects on poor households. On one hand, the transfers increased economic activity in the short run by reducing the probability of closing a family business. Even though we fail to detect changes in consumption or food and housing security, recipients of the UCT exhibited better financial health in the short run (i.e., lower reliance on coping mechanisms such as savings and remittances). The treatment also led to sustained reductions of delinquency rates, which likely enabled households to substitute informal for formal credit sources in the medium run.

On the other hand, the transfer seems to have fostered greater mobility among beneficiaries, which led to higher initial mortality rates among treated households. Our results show that this is mainly explained by the need to leave the home to work, i.e., use of the transfer as working capital in their businesses. We argue that the cash was treated as a temporary capital injection, particularly given the extension of the stay-at-home order and the dilution of this positive income shock over time among larger households. Indeed, heterogeneous effects by household size show that the impact of the transfer on mortality is driven by households with more than four members, with persistent effects until December 2020. In turn, the impacts of the transfer among smaller households are either null or negative between March 2020 and April 2021.

In other words, the transfer served as a labor incentive for several months in the middle of a global public health emergency. In a context in which about 75% of the economy is informal and over 40% is self-employed and has the need to generate daily income, it was expected that compliance with social distancing and quarantine measures would not be total. The transfer led to unintended effects on labor market participation, mobility, morbidity, and mortality that counteracted the success of the program in terms of financial health. Our results are aligned with Brooks et al. (2022), who analyze the impact of mobile money transfers to micro-entrepreneurs in Kenya and find that the UCT was used as working capital to increase inventory expenses and had a positive impact on the probability of reopening the business and the number of hours worked. Unlike in Kenya, in which the transfer was designed for a small-scale study, our results come from the evaluation of a national transfer scheme reaching millions of households.

Our results contribute to the literature on the impacts of cash transfer programs, particularly in emergency contexts. They are also linked to the literature on poverty traps, which indicates that the transfer of assets to poor households only allows them to escape poverty by exceeding a certain minimum threshold (Balboni et al. 2020). The UCT in the Peruvian context was a limited injection delivered via two rounds and, despite some positive short-run effects, did not lead to sustained effects on living conditions, labor market activity, or financial health. This suggests that the transfer was not sufficient to reach a sustainable balance of income and consumption, and beneficiary households were thus likely to fall back into the situation of the control group. Even differences in mortality rates dissipated as societal infection rates progressed. Our results also shed light on the potential unintended effects of emergency UCTs when targeting poor households working in the informal sector. Our findings can be incorporated in the design of adaptive social protection or emergency programs, providing more resilience or support in the face of inevitable future natural disasters, conflicts, or disease outbreaks.

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# A COVID-19 Transfers in Peru

1. Direct Transfers

    - Transfer *Yo me quedo en casa*: cash transfer for households in poverty or extreme poverty: 760 LCU (local monetary unit, approximately 324 USD) delivered to vulnerable households. This transfer was granted twice for a total value of 1520 LCU.
    - Transfer *Independent*: 380 LCU in total (102 USD approx.) to urban workers whose households are classified as non-poor by the Household Targeting System (SISFOH) and whose income is below 1,200 LCU per month (322 USD approx.)[12]. This transfer was granted twice.
    - Transfer *Rural*: 760 LCU (204 USD approx.) for rural households in a situation of poverty or extreme poverty.
    - Transfer *Familia Universal*: 760 LCU (204 USD approx.) provided to urban workers whose households are classified as non-poor by the Household Targeting System (SISFOH), and for households in which the income of none of its members exceeds 3,000 LCU per month (804 USD approx.) [13].
    - Transfer *Universal*: 760 LCU (204 USD approx.) provided to urban workers whose households are classified as non-poor by the Household Targeting System (SISFOH) and whose income is below 3,000 LCU per month (804 USD approx.). Unlike the *Universal Family* transfer, this transfer does not exempt beneficiaries from the previous vouchers or transfers (*Yo me quedo en casa, Rural, Subsidy to "employment-generating companies"*).
    - Transfer *600*: 600 LCU (approx. 162 USD) for households in the regions and provinces of the country at extreme risk levels during the second wave of COVID-19. This transfer was granted in two stages, each one directed to new households that entered the extreme alert level in the different provinces of the country.
    - Bono *Yanapay*: 350 LCU (approx. 95 USD) for adults classified as poor or extremely poor, according to the SISFOH, and for users of targeted assistance social programs (e.g., Juntos) whose income was below 3,000 LCU per month (804 USD approx.) [14].

2. Indirect transfers

[12] In addition, it is verified that they are not beneficiaries of social programs (Juntos, Pensión 65 or Contigo), not eligible for the transfer *Yo me quedo en casa*, if any member of the household is registered as a dependent worker in the public or private sector or if they are mayors, regional governors or congressmen

[13] The same additional verifications made for the eligibility of the transfer *Independent* apply, adding that you have not received the transfer *Independent*, transfers related to the "Subsidy to companies that generate employment", or the *Rural* transfer

[14] Households made up of only one adult, with at least one dependent minor, received an additional 350 LCU, subject to certain conditions.

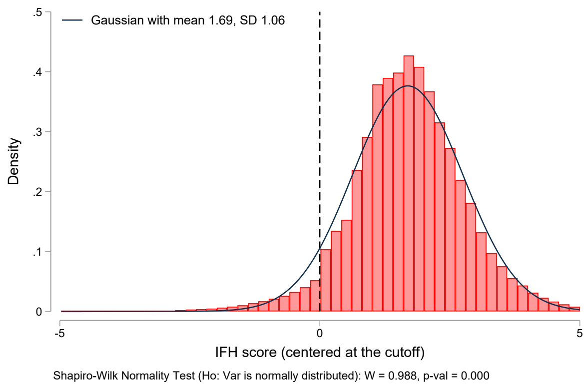
- Subsidies to job-creating companies: aimed at employers in the private sector for payroll payments. The employer receives, exceptionally, a 35% subsidy for each worker with fifth category income of up to 1,500 LCU [15].
- Monetary transfers of economic support: the Ministry of Culture makes available lines of support aimed at natural or legal persons who carry out cultural activities that have been affected, as well as the bearers of intangible heritage. Applicants for support apply for a contest, and if selected, their projects are financed [16].

[15] The fifth category income is that obtained from work in a dependency relationship.

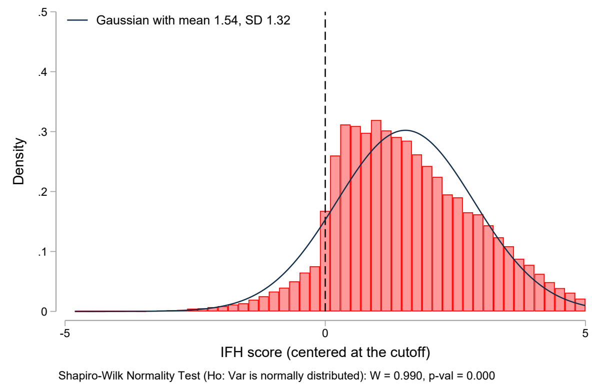
[16] In the application, evidence must be presented to prove that they have been unable to carry out cultural activities due to the health emergency, and a contingency plan.

# B Figures

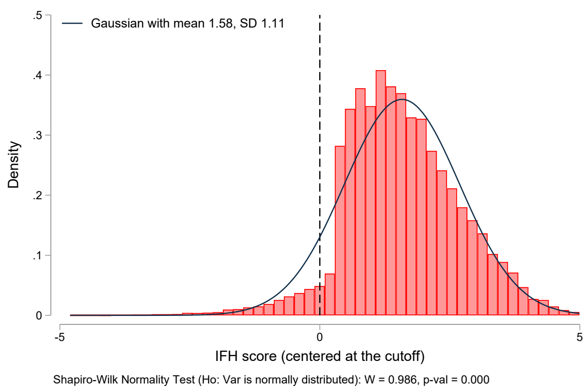
***Figure B.1:*** *Transfer Yo me quedo en casa - Density of the IFH score*



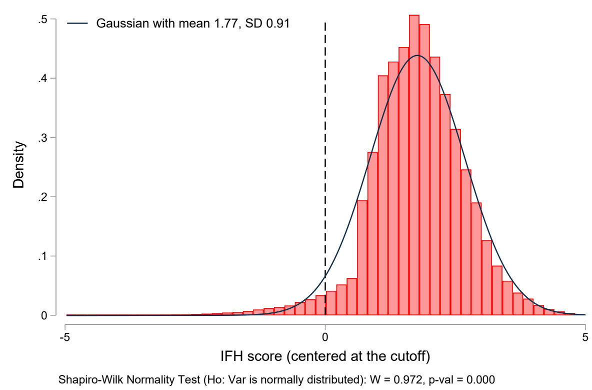

(a) All households




(b) Central Coast




(c) South Coast




(d) Lima

NOTE: The vertical line denotes the threshold for the *running* variable. The mean and standard deviation for the Gaussian density (blue line) are those of the running variable estimated in each corresponding sample.

# C Tables

***Table C.1:*** *Overview: Emergency Transfers in LAC during the COVID-19 Pandemic*

| Country | Name of the transfer | Amount (USD) per installment | Installments | Approx. Delivery frequency |
|---|---|---|---|---|
| Argentina | *Ingreso Familiar de Emergencia* | 155 | 3 | Bi-monthly |
| | *Bono Extraordinario para beneficiarios de Prestación Universal por Hijo y de Asignación Familiar por Embarazo* | 47 | 1 | - |
| Bolivia | *Bono Familia* | 72 | 1 | - |
| | *Bono Canasta Familiar* | 58 | 1 | - |
| | *Bono Universal* | 72 | 1 | - |
| | *Bono Contra el Hambre* | 144 | 1 | - |
| Brazil | *Auxilio Emergencial* - tramo 1 | 115 | 5 | Monthly |
| | *Auxilio Emergencial* - tramo 2 | 58 | 4 | Monthly |
| Chile | *Ingreso Familiar de Emergencia* (IFE) | 126 | 6 | Monthly |
| | *Bono Covid Navidad* | 32 - 70 USD | 1 | - |
| | *COVID-19 Subvención de Emergencia* | 63 | 1 | - |
| Colombia | *Ingreso Solidario* Program | 108 | 9 | Bi-monthly |
| | *Compensación financiera - régimen subsidiado* | 138 | 1 | - |
| Costa Rica | IMAS Subsidy | 205 | 3 | Monthly |
| Ecuador | *Bono de Contingencia - Bono de Protección Familiar* | 120 | 2 | Monthly |
| | *Bono de Apoyo Nutricional* | 240 | 1 | - |
| El Salvador | *Bono de Compensación 300* | 300 | 1 | - |
| Panamá | *Bono Solidario del Plan Panamá Solidario* | 80, then 100 | 3+6=9 | Monthly |
| Paraguay | Pytyvõ | 86 | 5 | Monthly |
| | *Seguridad Alimentaria Ñangareko* Program | 36 | 1 | - |
| Perú | *Bono Universal* | 204 | 3 | Bi-monthly |
| | *Bono Yo Me Quedo en Casa* | 224 | 2 | Bi-monthly |
| | *Bono Rural* | 204 | 1 | - |
| | *Bono Independiente* | 102 | 2 | Bi-monthly |
| Dominican Republic | *Quédate en Casa* Program | 92 | 9 | Monthly |
| Uruguay | *Canasta de Emergencia Alimentaria* Program | 30 | 2 | Monthly |

Source: Prepared by the authors based on information collected by the World Bank (Social Protection and Jobs Responses to COVID-19: A Real-Time Review of Country Measures, 2022).

*Table C.2: First Stage of the* Yo me quedo en casa *Cash Transfer - All Households*

| Observations | Polynomial | Estimated RD | Inference Conventional | | Robust | Controls |
|---|---|---|---|---|---|---|
| | | | p-value | 95% C.I. | 95% C.I. | |
| 626,084 | 1 | 0.647 | 0.000 | [0.632,0.662] | [0.635,0.666] | No |
| 626,084 | 2 | 0.654 | 0.000 | [0.636,0.673] | [0.631,0.669] | No |
| 626,081 | 1 | 0.564 | 0.000 | [0.500,0.627] | [0.502,0.630] | Yes |
| 626,081 | 2 | 0.557 | 0.000 | [0.491,0.624] | [0.494,0.627] | Yes |

*Notes*: The regressions include the following regions: Central Coast, South Coast and Lima. The specification with controls includes age and sex of the head of the household, and district fixed effects, along with standard errors clustered at the district level.

***Table C.3:*** *Definitions of Outcome Variables*

| Variable | Definition |
|---|---|
| Household Monthly Expenditure (per capita) | Total household spending in the month preceding the interview date, divided by the number of members in the household. |
| Household Monthly Food Expenditure (per capita) | Total household spending in food and groceries the month preceding the interview date, divided by the number of members in the household. |
| Hunger in the household over the last 7 days | Binary variable equal to one if any household member experienced hunger due to a lack of food, and zero otherwise. |
| Difficulties to pay rent over the last 30 days | Binary variable equal to one if the household considered it either difficult or very difficult to pay rent and utilities during the month preceding the interview date, and zero otherwise. |
| Respondent or partner lost job | Binary variable equal to one if either the respondent or his/her partner lost their job since the start of the pandemic in March 2020, and zero otherwise. |
| Respondent or partner closed their business due to the pandemic | Binary variable equal to one if either the respondent or his/her partner were forced to close their business since the start of the pandemic in March 2020, and zero otherwise. |
| Any child in the household had to work over the last 7 days | Binary variable equal to one if any household member aged 18 or less worked for pay over the week preceding the interview date, and zero otherwise. |
| Financial Stress Index | Standardized weighted average of the binary variables that measure the likelihood of having used savings, received remittances, and using informal loans, following Londoño-Vélez & Querubín (2022) and Anderson (2008). |
| Used savings since March 2020 | Binary variable equal to one if the respondent used any amount of their savings to cover household expenses since the start of the pandemic in March 2020, and zero otherwise. |
| Received remittances | Binary variable equal to one if any household member received remittances to cover household expenses between March and October 2020 for the first survey wave, and between January and June 2021 for the second survey wave, and zero otherwise. |
| Borrowed from friends and/or family | Binary variable equal to one if any household member borrowed from friends and/or family to cover household expenses between March and October 2020 for the first survey wave, and between January and June 2021 for the second survey wave, and zero otherwise. |
| Migrated | Binary variable equal to one if any household member moved to another district since the start of the pandemic in March 2020, and zero otherwise. |
| Pr(outstanding debt) | Binary variable equal to one if any household member has outstanding debt in the financial system, and zero otherwise. Outstanding debt considered includes current and expired debt and excludes judicial, restructured, refinanced, and written-off debts. |
| Total Outstanding Debt | Total amount of household outstanding debt including current and expired debt and excluding judicial, restructured, refinanced, and written-off debts. This outcome is transformed using the inverse hyperbolic sine transformation due to the large number of zeros. |
| Pr(Arrears) | Binary variable equal to one if any household member had a 30-180 days delay in the payment of a debt, excluding written-off debts, and zero otherwise. |
| Debt in Arrears | Total amount of household expired debt, excluding written-off debts. This outcome is transformed using the inverse hyperbolic sine transformation due to the large number of zeros. A zero is imputed to households for which its debt has already been written-off. |
| Cumulative Mortality | Binary variable equal to one if at least one household member passed away between March 2020 and each month of observation, and zero otherwise. |
| Monthly Mortality | Binary variable equal to one if at least one household member passed away during the month of observation, and zero otherwise. |
| Left home to work | Binary variable equal to one if the respondent left their home to work during the lockdown (March-June 2020), and zero otherwise. |
| Went shopping at the market | Binary variable equal to one if the respondent left their home to go shopping at the market during the lockdown (March-June 2020), and zero otherwise. |
| Visited the bank or other financial institution | Binary variable equal to one if the respondent left their home to visit the bank or any other financial institution during the lockdown (March-June 2020), and zero otherwise. |
| Exercised or went for a walk | Binary variable equal to one if the respondent left their home to exercise or go for a walk during the lockdown (March-June 2020), and zero otherwise. |

*Table C.4: Manipulation Test*

| | Robust p-value | Left Coef. | Right Coef. | Diff. |
|---|---|---|---|---|
| SISFOH score | 0.1639 | 0.0643 | 0.0714 | 0.0070 |
| Survey wave 1 | 0.2232 | 2.2874 | 1.7329 | -0.5546 |
| Survey wave 2 | 0.1347 | 2.9038 | 1.5212 | -1.3826 |
| Equifax | 0.4111 | 1.3886 | 3.0322 | 1.6435 |

*Notes:* Table shows the p-values of the manipulation test in the running variable proposed in Cattaneo, Jansson and Ma (2020). Null hypothesis: the density of the score or running variable changes discontinuously at the cut-off point or threshold. The Stata *rddensity* package was used to implement the test. For the tests, data is restricted to the optimal bandwidth.

*Table C.5: First Stage of the* Yo me quedo en casa *Cash Transfer - Target Households*

| | | | Inference Conventional | | Robust | |
|---|---|---|---|---|---|---|
| Observations | Polynomial | Estimated RD | p-value | 95% C.I. | 95% C.I. | Controls |
| 22,284 | 1 | 0.616 | 0.000 | [0.573,0.659] | [0.566,0.670] | No |
| 22,284 | 2 | 0.502 | 0.000 | [0.401,0.602] | [0.383,0.593] | No |
| 20,963 | 1 | 0.549 | 0.000 | [0.507,0.592] | [0.498,0.595] | Yes |
| 20,963 | 2 | 0.473 | 0.000 | [0.409,0.536] | [0.397,0.533] | Yes |

*Notes*: The regressions include the following regions: Central Coast, South Coast and Lima. The specification with controls includes age and sex of the head of the household, and district fixed effects, along with standard errors clustered at the district level.

*Table C.6: First Stage of the* Yo me quedo en casa *Cash Transfer - Surveyed Households*

| | | | Inference Conventional | | Robust | |
|---|---|---|---|---|---|---|
| Observations | Polynomial | Estimated RD | p-value | 95% C.I. | 95% C.I. | Controls |
| *IPA Sample* | | | | | | |
| 5088 | 1 | 0.550 | 0.000 | [0.452,0.648] | [0.416,0.647] | No |
| 5088 | 2 | 0.524 | 0.000 | [0.403,0.646] | [0.372,0.657] | No |
| 4854 | 1 | 0.550 | 0.000 | [0.472,0.627] | [0.449,0.624] | Yes |
| 4854 | 2 | 0.538 | 0.000 | [0.428,0.648] | [0.399,0.643] | Yes |
| *PRISMA Sample* | | | | | | |
| 1766 | 1 | 0.653 | 0.000 | [0.543,0.763] | [0.527,0.790] | No |
| 1766 | 2 | 0.678 | 0.000 | [0.526,0.830] | [0.525,0.830] | No |
| 1695 | 1 | 0.517 | 0.000 | [0.434,0.600] | [0.418,0.608] | Yes |
| 1695 | 2 | 0.517 | 0.000 | [0.409,0.625] | [0.402,0.639] | Yes |

*Notes*: The regressions include the following regions: Central Coast, South Coast and Lima. The specification with controls includes age and sex of the head of the household, and district fixed effects, along with standard errors clustered at the district level.

***Table C.7:*** *Effect of the* Yo me quedo en casa *Cash Transfer on Activities during Lockdown (March-June 2020)*

| | | **Short-Term Effects** | | |
|---|---|---|---|---|
| | Control Mean | MSE | 3/2 MSE | 1/2 MSE |
| | (1) | (2) | (3) | (4) |
| Left home to work | 0.280 | 0.203** | 0.253*** | 0.358*** |
| | | (0.101) | (0.088) | (0.130) |
| | | 961 | 1,175 | 464 |
| | | [-0.080;0.079] | [-0.121;0.119] | [-0.040;0.040] |
| Went shopping at the market | 0.711 | -0.101 | -0.033 | -0.201 |
| | | (0.113) | (0.093) | (0.135) |
| | | 925 | 1,152 | 446 |
| | | [-0.077;0.077] | [-0.116;0.116] | [-0.039;0.039] |
| Visited the bank or other financial institution | 0.265 | -0.109 | -0.011 | -0.208 |
| | | (0.106) | (0.088) | (0.140) |
| | | 858 | 1,101 | 398 |
| | | [-0.070;0.070] | [-0.105;0.105] | [-0.035;0.035] |
| Exercised or went for a walk | 0.244 | 0.140 | 0.135 | 0.391** |
| | | (0.124) | (0.106) | (0.166) |
| | | 625 | 902 | 275 |
| | | [-0.050;0.050] | [-0.076;0.076] | [-0.025;0.025] |
| Any member has a bank account | 0.475 | 0.028 | -0.091 | 0.111 |
| | | (0.083) | (0.078) | (0.106) |
| | | 758 | 1,218 | 374 |
| | | [-0.035;0.035] | [-0.053;0.053] | [-0.018;0.018] |
| Any member used digital means of payment | 0.354 | -0.308*** | -0.265*** | -0.433*** |
| | | (0.083) | (0.074) | (0.120) |
| | | 837 | 1,306 | 408 |
| | | [-0.038;0.038] | [-0.057;0.057] | [-0.019;0.019] |

*Notes*: Financial inclusion data was reported by the survey respondent during the first survey wave whereas mobility patterns were reported during the second survey wave. For the latter, retrospective data taking March-June 2020 as the period of reference, when the strict lockdown measures were at their peak. Effects are estimated at the household level. For each outcome, the estimated effect (row 1), clustered standard errors (row 2), the effective number of observations (row 3), and the optimal bandwidths (row 4) were computed using the `rdrobust` package, with a triangular weight matrix and MSE-optimally data-driven bandwidths selected for each side of the boundary. Linear local polynomials were used to construct the point estimator. Each specification includes the following covariates: sex and age of household head (from RENIEC administrative data), and district level fixed effects.
*** Significant at 1%, ** Significant at 5%, * Significant at 10%.

***Table C.8:*** *Effect of the* Yo me quedo en casa *Cash Transfer on Credit Usage and Delinquency*

| | **September 2020** | | | | **January 2021** | | | |
|---|---|---|---|---|---|---|---|---|
| | Control Mean | MSE | 3/2 MSE | 1/2 MSE | Control Mean | MSE | 3/2 MSE | 1/2 MSE |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Credit (0,1) | 0.617 | -0.049 | -0.126 | 0.746*** | 0.625 | -0.172* | -0.247* | 0.748*** |
| | | (0.108) | (0.138) | (0.073) | | (0.103) | (0.136) | (0.064) |
| | | 156 | 231 | 80 | | 156 | 231 | 80 |
| | | [-0.016;0.018] | [-0.025;0.027] | [-0.008;0.009] | | [-0.016;0.018] | [-0.025;0.027] | [-0.008;0.009] |
| Debt | 4.943 | -1.061 | -1.756 | 6.187*** | 5.092 | -1.931** | -2.965*** | 5.987*** |
| | | (0.880) | (1.110) | (0.497) | | (0.796) | (1.036) | (0.374) |
| | | 152 | 225 | 78 | | 152 | 225 | 78 |
| | | [-0.016;0.017] | [-0.024;0.026] | [-0.008;0.009] | | [-0.016;0.017] | [-0.024;0.026] | [-0.008;0.009] |
| Arrears (0,1) | 0.146 | -0.626*** | -0.428*** | -0.668*** | 0.124 | -0.325*** | -0.221*** | -0.049 |
| | | (0.110) | (0.091) | (0.071) | | (0.089) | (0.077) | (0.058) |
| | | 181 | 263 | 96 | | 181 | 263 | 96 |
| | | [-0.019;0.020] | [-0.029;0.030] | [-0.010;0.010] | | [-0.019;0.020] | [-0.029;0.030] | [-0.010;0.010] |
| Arrears debt | 0.997 | -4.338*** | -3.041*** | -4.175*** | 0.863 | -3.346*** | -2.334*** | -1.293*** |
| | | (0.775) | (0.655) | (0.466) | | (0.615) | (0.529) | (0.262) |
| | | 179 | 259 | 96 | | 179 | 259 | 96 |
| | | [-0.019;0.019] | [-0.029;0.029] | [-0.010;0.010] | | [-0.019;0.019] | [-0.029;0.029] | [-0.010;0.010] |

*Notes*: Debt measures outstanding loans considering current and expired debt, excluding judicial, restructured, refinanced and written-off debts. Arrears is an indicator variable equal to one if a household member had between 30 and 180 days delay in the payment of a debt (in the regulated or unregulated financial system) in each corresponding month, excluding written-off debts. Total and Arrears debt is transformed using the inverse hyperbolic sine transformation. For arrears debt, we impute a zero to households for which its debt has already been written-off. Effects are estimated at the household level using the `rdrobust` package, with a triangular weight matrix and MSE-optimally data-driven bandwidths selected for each side of the boundary. Linear local polynomials were used to construct the point estimator. The bandwidth used in estimation is the average of the 4 optimal bandwidths obtained individually for each month. The control mean is computed among the households who did not get the transfer and whose centered IFH score lies inside the average bandwidth. Each specification includes the following covariates: sex and age of household head (from RENIEC administrative data), and district level fixed effects.
*** Significant at 1%, ** Significant at 5%, * Significant at 10%.

***Table C.9:*** *Effect of the* Yo me quedo en casa *Cash Transfer on Credit Usage and Delinquency*

| | **April 2021** | | | | **August 2021** | | | |
|---|---|---|---|---|---|---|---|---|
| | Control Mean | MSE | 3/2 MSE | 1/2 MSE | Control Mean | MSE | 3/2 MSE | 1/2 MSE |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Credit (0,1) | 0.600 | 0.304*** | 0.113 | 1.067*** | 0.642 | 0.241** | -0.019 | 1.422*** |
| | | (0.080) | (0.112) | (0.061) | | (0.100) | (0.115) | (0.080) |
| | | 156 | 231 | 80 | | 156 | 231 | 80 |
| | | [-0.016;0.018] | [-0.025;0.027] | [-0.008;0.009] | | [-0.016;0.018] | [-0.025;0.027] | [-0.008;0.009] |
| Debt | 4.913 | 1.505*** | -0.110 | 7.733*** | 4.943 | 1.820** | -0.576 | 11.982*** |
| | | (0.576) | (0.847) | (0.376) | | (0.714) | (0.865) | (0.611) |
| | | 152 | 225 | 78 | | 152 | 225 | 78 |
| | | [-0.016;0.017] | [-0.024;0.026] | [-0.008;0.009] | | [-0.016;0.017] | [-0.024;0.026] | [-0.008;0.009] |
| Arrears (0,1) | 0.102 | -0.151** | -0.135* | 0.078*** | 0.095 | 0.142** | 0.076 | 0.111*** |
| | | (0.067) | (0.069) | (0.023) | | (0.072) | (0.094) | (0.041) |
| | | 181 | 263 | 96 | | 181 | 263 | 96 |
| | | [-0.019;0.020] | [-0.029;0.030] | [-0.010;0.010] | | [-0.019;0.020] | [-0.029;0.030] | [-0.010;0.010] |
| Arrears debt | 0.664 | -1.173*** | -1.269*** | 0.564*** | 0.642 | 0.764* | -0.016 | 0.689** |
| | | (0.434) | (0.448) | (0.169) | | (0.437) | (0.495) | (0.285) |
| | | 179 | 259 | 96 | | 179 | 259 | 96 |
| | | [-0.019;0.019] | [-0.029;0.029] | [-0.010;0.010] | | [-0.019;0.019] | [-0.029;0.029] | [-0.010;0.010] |

*Notes*: Debt measures outstanding loans considering current and expired debt, excluding judicial, restructured, refinanced and written-off debts. Arrears is an indicator variable equal to one if a household member had between 30 and 180 days delay in the payment of a debt (in the regulated or unregulated financial system) in each corresponding month, excluding written-off debts. Total and Arrears debt is transformed using the inverse hyperbolic sine transformation. For arrears debt, we impute a zero to households for which its debt has already been written-off. Effects are estimated at the household level using the `rdrobust` package, with a triangular weight matrix and MSE-optimally data-driven bandwidths selected for each side of the boundary. Linear local polynomials were used to construct the point estimator. The bandwidth used in estimation is the average of the 4 optimal bandwidths obtained individually for each month. The control mean is computed among the households who did not get the transfer and whose centered IFH score lies inside the average bandwidth. Each specification includes the following covariates: sex and age of household head (from RENIEC administrative data), and district level fixed effects.
*** Significant at 1%, ** Significant at 5%, * Significant at 10%.

***Table C.10:*** *Impacts of the Yo me quedo en casa Cash Transfer on Mortality by Household Size*

| | **HH has 4 or less members** | | | | **HH has more than 4 members** | | | |
|---|---|---|---|---|---|---|---|---|
| | Control Mean | MSE | 3/2 MSE | 1/2 MSE | Control Mean | MSE | 3/2 MSE | 1/2 MSE |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Apr 2020 | 0.000 | 0.003* | 0.002 | 0.001 | 0.004 | 0.018 | 0.019* | 0.001 |
| | | (0.002) | (0.002) | (0.002) | | (0.013) | (0.011) | (0.004) |
| May 2020 | 0.004 | 0.004 | 0.006* | 0.001 | 0.008 | 0.132*** | 0.097*** | 0.283*** |
| | | (0.004) | (0.003) | (0.002) | | (0.048) | (0.032) | (0.101) |
| Jun 2020 | 0.008 | -0.010 | -0.005 | -0.022* | 0.011 | 0.118** | 0.079*** | 0.267*** |
| | | (0.008) | (0.007) | (0.013) | | (0.046) | (0.030) | (0.099) |
| Jul 2020 | 0.012 | -0.020** | -0.014 | -0.036*** | 0.019 | 0.118*** | 0.073** | 0.280*** |
| | | (0.010) | (0.009) | (0.014) | | (0.044) | (0.030) | (0.105) |
| Aug 2020 | 0.013 | -0.026** | -0.020** | -0.043*** | 0.019 | 0.118*** | 0.073** | 0.280*** |
| | | (0.011) | (0.010) | (0.015) | | (0.044) | (0.030) | (0.105) |
| Sept 2020 | 0.015 | -0.017 | -0.015 | -0.034** | 0.021 | 0.113*** | 0.072** | 0.280*** |
| | | (0.011) | (0.011) | (0.016) | | (0.043) | (0.029) | (0.105) |
| Oct 2020 | 0.018 | -0.018 | -0.020* | -0.041** | 0.021 | 0.107*** | 0.066** | 0.280*** |
| | | (0.013) | (0.012) | (0.017) | | (0.039) | (0.028) | (0.105) |
| Nov 2020 | 0.021 | -0.022 | -0.022* | -0.041** | 0.023 | 0.114*** | 0.080*** | 0.311*** |
| | | (0.015) | (0.012) | (0.017) | | (0.041) | (0.029) | (0.109) |
| Dec 2020 | 0.021 | -0.020 | -0.018 | -0.041** | 0.025 | 0.110*** | 0.072** | 0.311*** |
| | | (0.015) | (0.012) | (0.017) | | (0.041) | (0.029) | (0.109) |
| Jan 2021 | 0.024 | -0.004 | -0.011 | -0.015 | 0.034 | 0.042 | 0.031 | 0.150* |
| | | (0.016) | (0.014) | (0.021) | | (0.043) | (0.032) | (0.088) |
| Feb 2021 | 0.026 | -0.003 | -0.009 | -0.009 | 0.042 | 0.128*** | 0.091** | 0.227** |
| | | (0.016) | (0.015) | (0.022) | | (0.049) | (0.038) | (0.101) |
| Mar 2021 | 0.029 | -0.006 | -0.011 | -0.028 | 0.049 | 0.120** | 0.117*** | 0.221** |
| | | (0.018) | (0.016) | (0.029) | | (0.049) | (0.040) | (0.101) |
| Apr 2021 | 0.033 | -0.023 | -0.022 | -0.075** | 0.059 | 0.148*** | 0.129*** | 0.295*** |
| | | (0.020) | (0.017) | (0.034) | | (0.055) | (0.045) | (0.112) |
| Observations | | 2124 | 3597 | 1119 | | 850 | 1325 | 418 |
| Bandwidth | | [0.033, 0.033] | [0.049, 0.049] | [0.016, 0.016] | | [0.033, 0.033] | [0.049, 0.049] | [0.016, 0.016] |

*Notes*: The dependent variable is the cumulative mortality rate, constructed as a binary indicator equal to 1 if at least one household member passed away up until each corresponding month. Effects are estimated at the household level using the `rdrobust` package, with a triangular weight matrix and MSE-optimally data-driven bandwidths selected for each side of the boundary. Linear local polynomials were used to construct the point estimator. The bandwidth used in estimation is the average of the 13 optimal bandwidths obtained individually for each month, and this bandwidth is used across both subsamples. The control mean is computed among the households who did not get the transfer and whose centered IFH score lies inside the average bandwidth. Each specification includes the following covariates: sex and age of household head (from RENIEC administrative data), and district level fixed effects.
*** Significant at 1%, ** Significant at 5%, * Significant at 10%.

***Table C.11:*** *Impacts of the Yo me quedo en casa Cash Transfer on Mortality by Household Size*

| | **HH has 4 or less members** | | | | **HH has more than 4 members** | | | |
|---|---|---|---|---|---|---|---|---|
| | Control Mean | MSE | 3/2 MSE | 1/2 MSE | Control Mean | MSE | 3/2 MSE | 1/2 MSE |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Apr 2020 | 0.000 | 0.003 | 0.001 | 0.003* | 0.003 | 0.019 | 0.016* | 0.017 |
| | | (0.002) | (0.002) | (0.002) | | (0.012) | (0.009) | (0.010) |
| May 2020 | 0.005 | 0.003 | 0.006** | 0.001 | 0.004 | 0.080*** | 0.054*** | 0.172*** |
| | | (0.002) | (0.003) | (0.003) | | (0.031) | (0.021) | (0.064) |
| Jun 2020 | 0.003 | -0.011 | -0.011* | -0.022** | 0.004 | -0.019 | -0.012 | -0.019* |
| | | (0.007) | (0.006) | (0.011) | | (0.012) | (0.011) | (0.011) |
| Jul 2020 | 0.004 | -0.010 | -0.011* | -0.013* | 0.005 | -0.005 | -0.009 | -0.004 |
| | | (0.006) | (0.006) | (0.007) | | (0.012) | (0.011) | (0.010) |
| Aug 2020 | 0.002 | -0.005 | -0.003 | -0.007 | 0.000 | 0.000 | -0.005 | 0.000 |
| | | (0.004) | (0.004) | (0.005) | | (0.000) | (0.004) | (0.000) |
| Sept 2020 | 0.003 | 0.005 | 0.001 | 0.008* | 0.001 | -0.002 | 0.000 | 0.000 |
| | | (0.004) | (0.004) | (0.004) | | (0.003) | (0.003) | (0.000) |
| Oct 2020 | 0.003 | -0.003 | -0.008** | -0.007* | 0.000 | -0.006 | 0.002 | -0.004 |
| | | (0.005) | (0.004) | (0.004) | | (0.004) | (0.004) | (0.003) |
| Nov 2020 | 0.002 | -0.002 | -0.001 | -0.003 | 0.001 | 0.013 | 0.013* | 0.006 |
| | | (0.005) | (0.003) | (0.003) | | (0.010) | (0.007) | (0.017) |
| Dec 2020 | 0.001 | 0.003 | 0.002 | 0.001 | 0.001 | -0.008* | -0.005 | 0.006 |
| | | (0.002) | (0.002) | (0.002) | | (0.005) | (0.006) | (0.011) |
| Jan 2021 | 0.003 | 0.008 | 0.003 | 0.024*** | 0.006 | -0.044** | -0.032* | -0.102* |
| | | (0.006) | (0.006) | (0.009) | | (0.022) | (0.018) | (0.059) |
| Feb 2021 | 0.003 | 0.003 | -0.000 | 0.002 | 0.008 | 0.065*** | 0.038** | 0.098*** |
| | | (0.006) | (0.006) | (0.008) | | (0.024) | (0.018) | (0.038) |
| Mar 2021 | 0.003 | -0.007 | -0.005 | -0.017 | 0.008 | 0.029*** | 0.040*** | -0.040** |
| | | (0.007) | (0.006) | (0.011) | | (0.010) | (0.011) | (0.017) |
| Apr 2021 | 0.003 | -0.012 | -0.007 | -0.028* | 0.008 | 0.018 | -0.001 | 0.037 |
| | | (0.009) | (0.006) | (0.016) | | (0.018) | (0.014) | (0.034) |
| Observations | | 3474 | 4843 | 1574 | | 1266 | 1930 | 571 |
| Bandwidth | | [0.045, 0.048] | [0.068, 0.072] | [0.023, 0.024] | | [0.045, 0.048] | [0.068, 0.072] | [0.023, 0.024] |

*Notes*: The dependent variable is the mortality rate for each month, constructed as a binary indicator equal to 1 if at least one household member passed away in each corresponding month. Effects are estimated at the household level using the `rdrobust` package, with a triangular weight matrix and MSE-optimally data-driven bandwidths selected for each side of the boundary. Linear local polynomials were used to construct the point estimator. The bandwidth used in estimation is the average of the 13 optimal bandwidths obtained individually for each month, and this bandwidth is used across both subsamples. The control mean is computed among the households who did not get the transfer and whose centered IFH score lies inside the average bandwidth. Each specification includes the following covariates: sex and age of household head (from RENIEC administrative data), and district level fixed effects.
*** Significant at 1%, ** Significant at 5%, * Significant at 10%.

***Table C.12:*** *Effect of the* Yo me quedo en casa *Cash Transfer on Activities during Lockdown (March-June 2020) by Household Size*

| | **HH has 4 or less members** | | | | **HH has more than 4 members** | | | |
|---|---|---|---|---|---|---|---|---|
| | Control Mean | MSE | 3/2 MSE | 1/2 MSE | Control Mean | MSE | 3/2 MSE | 1/2 MSE |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Left home to work | 0.294 | 0.187* | 0.173* | 0.066 | 0.285 | 0.367*** | 0.423*** | 0.289* |
| | | (0.100) | (0.092) | (0.114) | | (0.128) | (0.119) | (0.154) |
| | | 725 | 937 | 435 | | 429 | 502 | 230 |
| | | [-0.132;0.117] | [-0.197;0.175] | [-0.066;0.058] | | [-0.092;0.091] | [-0.138;0.137] | [-0.046;0.046] |
| Went shopping at the market | 0.708 | -0.087 | -0.083 | 0.028 | 0.711 | 0.007 | 0.136 | -0.322 |
| | | (0.093) | (0.082) | (0.101) | | (0.138) | (0.115) | (0.205) |
| | | 698 | 801 | 343 | | 436 | 514 | 241 |
| | | [-0.156;0.067] | [-0.233;0.100] | [-0.078;0.033] | | [-0.096;0.095] | [-0.145;0.143] | [-0.048;0.048] |
| Visited the bank or other financial institution | 0.251 | 0.133 | 0.211** | -0.150 | 0.279 | -0.403*** | -0.225** | -1.035*** |
| | | (0.101) | (0.090) | (0.120) | | (0.139) | (0.114) | (0.213) |
| | | 424 | 618 | 182 | | 431 | 504 | 229 |
| | | [-0.059;0.059] | [-0.088;0.088] | [-0.029;0.029] | | [-0.093;0.090] | [-0.139;0.135] | [-0.046;0.045] |
| Exercised or went for a walk | 0.240 | 0.005 | 0.029 | 0.069 | 0.236 | 0.205 | 0.178* | 0.509*** |
| | | (0.076) | (0.066) | (0.103) | | (0.130) | (0.107) | (0.192) |
| | | 809 | 941 | 601 | | 427 | 496 | 228 |
| | | [-0.167;0.166] | [-0.250;0.250] | [-0.083;0.083] | | [-0.090;0.089] | [-0.135;0.134] | [-0.045;0.045] |
| Any member has a bank account | 0.507 | -0.101 | -0.170** | -0.085 | 0.417 | 0.197 | 0.147 | 0.200 |
| | | (0.081) | (0.081) | (0.107) | | (0.161) | (0.133) | (0.207) |
| | | 569 | 887 | 288 | | 381 | 540 | 154 |
| | | [-0.040;0.040] | [-0.060;0.060] | [-0.020;0.020] | | [-0.048;0.048] | [-0.072;0.072] | [-0.024;0.024] |
| Any member used digital means of payment | 0.379 | -0.327*** | -0.285*** | -0.508*** | 0.322 | -0.191 | 0.004 | -0.336 |
| | | (0.076) | (0.061) | (0.089) | | (0.182) | (0.154) | (0.294) |
| | | 855 | 1,208 | 403 | | 260 | 425 | 114 |
| | | [-0.058;0.057] | [-0.087;0.086] | [-0.029;0.029] | | [-0.035;0.035] | [-0.053;0.053] | [-0.018;0.018] |

*Notes*: Financial inclusion data was reported by the survey respondent during the first survey wave whereas mobility patterns were reported during the second survey wave. For the latter, retrospective data taking March-June 2020 as the period of reference, when the strict lockdown measures were at their peak. For each outcome, the estimated effect (row 1), clustered standard errors (row 2), the effective number of observations (row 3), and the optimal bandwidths (row 4) were computed using the `rdrobust` package, with a triangular weight matrix and MSE-optimally data-driven bandwidths selected for each side of the boundary. Linear local polynomials were used to construct the point estimator. For credit, the bandwidth used in estimation is the average of the 4 optimal bandwidths obtained individually for each available month. Each specification includes the following covariates: sex and age of household head (from RENIEC administrative data), and district level fixed effects.
*** Significant at 1%, ** Significant at 5%, * Significant at 10%.