Basic income programs for middle-income households: Evidence from Colombia

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Abstract

We study the impact of monthly cash transfers to middle-income households in Colombia using a regression discontinuity design. The program increased household per-capita income by 25% but had no substantial impacts on food, health, or educational outcomes that are typically targeted by transfers to low-income households. In contrast, we find that the program increased non-food spending and reduced past-due debt with utility companies and retail firms. Likely because the transfers were largely delivered through digital bank accounts, the program increased access to formal credit. Further, the program enabled households to fully offset the negative impacts of economic shocks, suggesting that middle-income households may be constrained by lack of insurance. Moreover, the program enabled shocked households to substitute away from high-interest loans and toward formal credit. These results demonstrate that basic income programs can prevent vulnerable middle-income households from falling into poverty and could have long-last effects through increased engagement with the formal financial market.

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I Introduction

Traditionally, the fight against poverty has focused on social programs designed to provide assistance to the poorest households and lift them out of poverty. While many of these programs successfully graduated households out of poverty (Banerjee et al., 2015, 2021; Balboni et al., 2021; Blattman et al., 2014), poverty is not a one-way street. A large share of households are not structurally poor but are vulnerable to downward income mobility in lower and middle income countries (The World Bank, 2021b) and in high-income countries (Chetty et al., 2019). For example, on average, 37% of households in Latin American and the Caribbean were vulnerable to sliding into poverty by the end of 2019 (Stampini et al., 2021).

Evidence on the impacts of social programs for middle-income households is scarce despite their vulnerability to poverty. We leverage a unique setting to provide evidence on the effects of a cash transfer program for households that are not structurally poor and, therefore, are traditionally excluded from anti-poverty programs.

In April 2020, Colombia launched the Ingreso Solidario program as a response to the COVID-19 pandemic. The program reached over 4 million households and became Colombia's largest cash transfer program.[1] One key feature of the program is that it expanded de coverage of Colombia's safety net to non-poor households that were excluded from preexisting social programs. Specifically, the program has an eligibility cut-off of approximately the 40th percentile of the 2019 per-capita income distribution (equivalent to roughly 3 times the extreme poverty line).

[1]The average duration of COVID-19 pandemic programs was 4.5 months (Gentilini, 2022). In contrast, Ingreso Solidario delivered over 24 monthly payments by August 2022.

We exploit a comprehensive set of administrative records to estimate the program's impacts on marginal beneficiaries using a regression discontinuity design around the upper threshold of eligibility, based on a proxy-means test. In addition to social registry data, we use administrative data on program disbursement, the universe of debts with non-financial and financial (formal) firms in Colombia, formal employment, enrollment and grade completion for individuals of school age, test scores from Colombia's mandatory high school exit examination, and usage and motive of medical services. We then complement the administrative data with two waves of detailed household survey data collected 6 and 18 months after the implementation of the program for a sample of households within a narrow bandwidth around the eligibility cutoff. The comprehensive set of administrative records allows us to estimate the impacts of the program with precision and the survey data allows us to explore a wide range of specific outcomes.

To begin, we document strong program compliance and increases in income. As of July 2021, marginally eligible households—those just below the program eligibility cutoff—were 90.5 percentage points more likely to have received at least one program transfer. Pooling two rounds of survey data, we find that the program eligibility increased per-capita income among marginal beneficiaries by 26%. We also find that being eligible for the program increased the probability of reporting positive income by 4.6 percentage points, suggesting that the program was crucial for attenuating the most severe collapses in income. Consistent with this increase in income, we do not find any negative effects on labor market outcomes.

Next, we analyze the impacts of the program on food, health, and educational outcomes—the primary outcomes of in-kind and cash transfer programs targeted to low-income households. We find small and insignificant effects on food spending, which coincide with null effects on an index of food security. Similarly, we find no effects on attendance, grade retention, or test scores. Likewise, we do not find impacts on the use of medical services related to severe Covid-19 symptoms or mental health.

These null effects on food, education, and health contrast with the positive effects often found for cash transfers targeted to poor households (Bastagli et al., 2019). Two pieces of evidence suggest that this difference may reflect the fact that low-income and middle-income households have different priorities. First, while low-income households spend a substantial share of their budget on food, the middle-income households in our sample concentrated their spending in non-food categories. Second, in the first survey round, only 13% of marginally ineligible households reported food security as their main concern. In contrast, 25% of these households reported paying bills and debt as their top concern.

With middle-income households' budget allocations and concerns in mind, we next explore non-food spending and short-term debt to non-financial firms. In survey data, we find evidence of a 13% increase in non-food consumption that accounts for roughly 65% of the per-capita transfer amount, although this effect is not precisely estimated. Using administrative records, we find evidence of a statistically significant one-percent decline in the probability of having past-due debt with utility and retail firms (i.e. non-financial firms). Although the positive impacts on non-food spending and debt reduction to utility and retail firms are not transformative, they persist after COVID-19 lockdowns were lifted. This suggests that the results are informative beyond periods of severe economic crises.

Because the implementing agency attempted to maximize the number of beneficiaries receiving their transfers through direct deposit into digital savings accounts offered by commercial banks, we investigate the impact of the program on financial outcomes. Using credit bureau data, we find that eligibility for the program increased bank account ownership by 11 percentage points, which represents a 40% increase relative to marginally ineligible beneficiaries. While program eligibility had no effect on savings, it increased the usage of digital accounts, likely paving the way for access to formal credit. First, program eligibility increased credit inquiries in the credit bureau records, a first step toward obtaining a formal loan. Second, eligibility for the program increased the probability of having formal debt in good standing. Third, program eligibility decreased the probability of holding loans with past-due payments. Further, we find that the effects on financial outcomes persist over time indicating that the legacy of the program may be increased engagement with the formal financial system.

Finally, we investigate whether the program played a role in preventing middle-income households from sliding into poverty. Despite not being structurally poor, the middle-income households in our sample could be vulnerable to sliding into poverty. In survey data, income and consumption decline when households report having suffered the death of a household member. However, these declines are substantially attenuated in the case of marginal program beneficiaries. Further, using survey data, we find that among households that experienced an economic shock, program eligibility increased the probability of obtaining a formal loan while decreasing the probability of using predatory loans ("préstamos gota a gota"). Thus, the program helped prevent middle-income households from sliding into poverty through two mechanisms for coping with economic shocks, regular cash payments, which play a role similar to an insurance payout, and increased access to more-affordable formal loans.

This paper makes several contributions to the literature. First, it provides novel estimates of the impacts of monthly cash transfers on middle-income households that are often excluded from anti poverty programs. Recent studies have analyzed the effects of Universal Basic Income (UBI) programs on the average beneficiary in a subset of small villages in Kenya (Banerjee et al., 2020) and in Alaska (Jones and Marinescu, 2022). In contrast, our study focuses on the effects of a nation-wide expansion in the coverage of the safety net for the marginal beneficiary. This distinction is policy relevant. Given that UBI programs may carry a high fiscal cost, expansions to the coverage of social programs may occur incrementally.

Second, our results contribute to understanding the differences (and commonalities) in the effects of cash transfers to low- and middle-income households and whether cash transfers can prevent middle-income households from sliding into poverty. While there is a robust literature on the effects of cash-transfer programs targeted to households living in poverty,[2] there is little evidence on the effects for middle-income households. Our results indicate that while cash transfers to middle-income households may not affect the primary outcomes of programs targeted to the poor, they have positive impacts on outcomes aligned with middle-income households' priorities, such as non-food spending, debt reduction, and access to formal financial markets.[3] Further, our results indicate that the increased access to formal financial markets due to the program attenuates the impacts of economic shocks. This is important as there is recent evidence on the growing vulnerability of middle-income

[2]See Bastagli et al. (2019) and for a recent review and Londoño-Vélez and Querubín (2022) and Attanasio et al. (2021) for evidence in Colombia.

[3]Previous studies have analyzed differences between households living in poverty and middle-income households in spending and occupational choice (Banerjee and Duflo, 2008).

households in middle-income countries (The World Bank, 2021a).

Third, our results provide additional evidence that the implementation features of cash transfer programs matter, and in particular, can allow these programs to be a platform for financial inclusion. Among low-income households, there is evidence that rolling out debit cards to beneficiaries increases savings (Bachas et al., 2021) and that delivering transfers by mobile money reduces transaction costs for beneficiaries (Aker et al., 2016). We provide evidence that a cash transfer program with the preferred option to receive payments in digital bank accounts increased access to formal credit among middle-income households. Consistent with other studies, we find that financial technologies allow households to cope with economic shocks, but the channel varies across settings. Evidence from rural areas in Kenya and Tanzania shows that access to mobile money enables households to receive cross-households transfers amid shocks (Jack and Suri, 2014a; Riley, 2018). In the case of Colombia, a more urban setting, the program enabled households to cope with shocks by obtaining formal loans and avoiding debt with predatory lenders.

II Context: The Ingreso Solidario program

Program Features. Colombia launched Ingreso Solidario in March 2020 with the objective of mitigating the impacts of the COVID-19 emergency among households living in poverty and in conditions of economic vulnerability that do not receive monetary aid from other national programs.[4] Initially and for the period of this study, the transfer was COP 160,000 per month (the equivalent of USD $43 or USD $121 adjusting for purchasing power). The transfer represents 18% of the monthly minimum wage in force at that time

[4]See Prosperidad Social (2020) for specific program details.

and was equivalent to 115% of the per-capita extreme poverty line in 2019 (COP $137.350, according to Departamento Administrativo Nacional de Estadisticas (2020)). As of December 2021, the program had reached more than 4 million households across the country, becoming Colombia's largest cash transfer program.

Three unique features of the program are key for this study. First, the program broadened the coverage of Colombia's social safety net beyond households in poverty to include middle-income households that, according to a proxy-means test, were vulnerable to poverty. Second, the program targeted households that were not covered by pre-existing social programs and had a higher eligibility threshold than pre-existing social programs targeted to households in poverty.[5] Third, the program encouraged beneficiaries to interact with formal financial products by opening simplified savings accounts on the behalf of many beneficiaries and depositing their transfer digitally.

Eligibility. In collaboration with other public agencies and the private sector, the National Planning Department (DNP, its acronym in Spanish) used administrative records to identify the beneficiaries of the program. The starting point was the government's social registry called System for the Identification of Potential Beneficiaries (SISBEN by its acronym in Spanish). SISBEN IV, the most recent version of the social registry, utilizes a proxy of the ratio between a household's predicted per-capita income (based on a statistical model) and the extreme poverty line corresponding to a household's location

[5] Specifically, the program included households that, for various reasons, were not covered by other traditional social programs, such as Familias en Acción, Jóvenes en Acción or Colombia Mayor. These three programs are conditional cash transfers, so households must meet certain specific characteristics—such as having school-age children or older adults—in order to access them.

of residence.[6] This ratio is then used to classify households into 4 broad categories and several subcategories: extremely poor households (categories A1-A5, where A1 represents the most deprived households), poor households (categories B1-B7), non-poor but vulnerable households (category C1-C18), and households that are neither poor nor vulnerable to poverty (category D1-D21).

The implementing agency applied several criteria to identify potential program beneficiaries. First, as described above, households that were beneficiaries of other national social programs were excluded. Second, deceased persons were excluded. Third, households with at least one formal worker registering a Contribution Base Income (IBC) above 4 current legal monthly minimum wages were excluded (approximately USD 671 in 2019). Fourth, households whose members belonged to the Special Exception Regime, for example, public sector employees such as teachers, members of the military or police officers, were also excluded. Fifth, individuals with deposit accounts whose balance as of February 2020 was greater than COP 5 million (approximately USD $1,200 or USD $3,700 adjusting for purchasing power) were excluded.

Finally, the implementing agency defined an eligibility cut-off point, based on SISBEN categories. Only households below that threshold were eligible to participate in the program. In the case of SISBEN IV, the most recent version of the social registry, households in category C5 or below would be eligible, while those in category C6 and above would not. In terms of the underlying continuous variable used to generate the SISBEN IV categories, i.e. the ratio of predicted per-capita income to the extreme poverty line, the

[6] The social registry is based on surveys that capture different dimensions of family well-being and is used to identify the beneficiaries of traditional social programs. For details see Departamento Nacional de Planeación (2016).

program eligibility threshold is approximately 3. Thus, the households on the margin of eligibility had predicted per-capita incomes that were roughly 3 times the extreme poverty line.

Program delivery and implementation. The initial implementation was carried out in three stages (see Prosperidad Social (2020) for more details). In the first stage, using information from Banca de las Oportunidades (a national government program to promote financial inclusion), households with active bank accounts were identified and received their first transfers in their accounts. By April 2022, this included 1,162,965 households.

In the second stage, households that were not initially part of the banking system were assigned to a financial institution for receiving their transfers. In the case of the beneficiaries assigned to financial institutions that offered simplified saving accounts—which could be opened remotely, the financial institutions opened simplified bank accounts on their behalf and notified beneficiaries through SMS.[7] These bank accounts could be operated directly from a cellular phone without access to the internet and enabled beneficiaries to quickly access their transfers. In addition, users of these saving accounts could use their phones to send or receive money from other households; make utility payments remotely, pay at supermarkets and pharmacies, and conduct basic financial transactions through the network of local banking agents associated with the financial institution. In the case of beneficiaries assigned to financial institutions that did not offer simplified bank accounts, typically those in rural areas, the beneficiaries were notified with information on how to collect their transfers in person.[8] After including this group, as of June 2020, a total of

[7]For this, the government collaborated with cellphone companies to provide the beneficiaries contact information to the financial institutions.

[8]In the case of unbanked households located in areas with low penetration of private fi-

2,423,516 households were part of the program.

In the third stage, the remaining set of potential beneficiaries who could not be contacted by the implementing agency were located in the field. As of April 2021, a total of 3,084,987 beneficiary households were included in Ingreso Solidario.[9]

III Data

Study sample. We determined the study sample as follows. First, we considered all households who were registered in SISBEN IV as of February 2020, a month before the implementation of the program. Second, we excluded all households who were ineligible for the program because they were already beneficiaries of other social programs, because at least one household member received formal monthly earnings above 4 times the minimum monthly salary as of February 2020, and because at least one household member had a bank account balance exceeding COP 5 million as of February 2020. In addition, using SISBEN IV data, we drop households whose members were part of the special social security regime. This process mimics the process conducted by the implementing agency to select beneficiaries. Finally, to ensure that our estimates are not influenced by the eligibility cutoffs associated with other programs, we excluded all households whose SISBEN IV score located them below category C2, the closest eligibility threshold of the preexisting social

nancial services, the government made payments in cash through Banco Agrario (Colombia's state owned bank that operates mostly in rural areas).

[9]Finally, starting April 2022, the coverage of the program was expanded, reaching 4,850,000 households. Our analysis includes only households who received a payment during the initial three stages.

programs.[10]

As a result, the study sample excludes the poorest households (based on their SISBEN IV category) in the Ingreso Solidario program and enables us to study the impacts of the program on a novel and key sub-population: ex-ante middle-incoem households with predicted per capita income above the thresholds of eligibility for social programs targeted to poor households.

Administrative records. We utilize administrative records from five sources. First, we utilize data from SISBEN IV (the social registry) as of February of 2020. This dataset includes the ratio of predicted per-capita income to the extreme poverty line, which we use as our running variable. It also includes a variety of household characteristics, which we use as controls in our estimations.

Second, we link the SISBEN IV data with data from one of Colombia's largest credit bureaus to measure the impacts of the program on a household's financial situation. The credit bureau data contains information at the individual level on ownership of bank accounts, debt with financial institutions, utility companies and retailers. We access these records for three post-program periods (June 2020, December 2020, and June 2021) and two pre-program periods (June and December 2019).

Third, to measure formal employment, we use formal workers' monthly contributions to social security (PILA, Planilla Integrada de Liquidación de Aportes) measured in December 2019, June 2020, December 2020, and June 2021. In maintaing data privacy, we were only able to merge these records with four variables from SISBEN IV, age, sex, an urban/rural indicator, and the ratio of predicted per-capita income to the extreme poverty line (the running

[10] Specifically, women age 54 and older and men age 59 and older in households with a score below the C2 category were eligible for Colombia's non-contributory pension program.

variable), and we were not able to merge PILA with other administrative records. In addition, data from firm-sex-and age-groups bins with less than five observations were dropped from the resulting matched sample.

Fourth, we use administrative records on the usage of medical services (RIPS, Registro Individual de Prestacion de Servicios de Salud) from January 2020 to June 2021 to measure the incidence of COVID-19 and the usage of medical facilities for mental health issues. These data include the diagnosis of each medical visit and the motive for the visit (e.g., regular consultation, procedures, emergency care, and hospitalizations). As in the case of the PILA records, we are not able to merge RIPS with other administrative records.

Fifth, we matched SISBEN IV data with administrative records on attendance and grade completion from SIMAT (Sistema Integrado de Matriculación). Specifically, we use the 2020 and 2021 records to measure attendance during two post-program periods and use the 2021 records, which include grade completion data corresponding to the 2020 school year, to proxy for academic achievement. For the subsample of households with children coursing 11th grade in 2020, we use scores on PRUEBA SABER 11, Colombia's mandatory standardized high-school exit exam to measure the impacts of the program on learning. Specifically, we focus on standardized global test scores corresponding to the following five examinations, Math, Reading, Social Sciences, Natural Sciences, and English.

Survey data. We complement the administrative records with two rounds of phone survey data collected during October-December 2020 (first round) and October-November 2021 (second round). We collected data on labor market outcomes for all household members, such as employment, type of employment (formal informal) and work hours. We also collected data on total household income. In the case of the first survey wave, we also collected retrospective information on these outcomes corresponding to February 2020 and April 2020. In both rounds we collected data on household consumption spending, time use, access to digital bank accounts, and usage of digital products.

Our data collection strategy follows the approach recommended by Cattaneo et al. (2019), which prioritizes collecting data for households that were closest to the cutoff of eligibility in the design of our survey sample. In the first round, enumerators called households with a ratio of predicted per-capita income to extreme poverty line closest to the cutoff (on both side of the cutoff). Next, they called the households who were the second closest to the cutoff on both sides, and so on.[11] Data collection stopped when the enumerators achieved the sample size agreed with the data-collection company based on our budget. As a result, we obtained 3,563 responses (1,797 eligible and 1,766 ineligible households).[12]

In the second round, we were able to collect data from 56% of the 3,563 households in the first round. To mitigate potential differences in household characteristics due to attrition, we replaced 682 of the households that we were unable to recontact in round 2 with similar households (in terms of distance to the cutoff) that we couldn't contact during the first round. We next interviewed 896 new households that were closest to the cutoff on either side of the cutoff and used them as replacements for the remaining households. As result, we collected 3,502 observations in the second round.

One key feature of our data collection process is that it defines the band-

[11] For each household, enumerators made five attempts to complete the survey.

[12] During the first round, the enumerators tried to contact the 14,200 households closest to the eligibility cutoff (7,100 on each side). Thus, the response rates for eligible and ineligible households are 25.09 and 24.87%.

width that we will use to estimate the impacts of the program. This minimizes our discretion in bandwidth choice while preserving the local nature of the identification strategy. The maximum distance between a household's predicted per-capita income to poverty line ratio in the survey and the eligibility cutoff is 0.0106 in the case of the first round and 0.1045 units in the case of the second round. The larger, second round bandwidth represents only 0.46% of the cutoff value and only includes observations corresponding to SISBEN IV categories C5 and C6, the two categories surrounding the cutoff.[13]

Summary statistics. Appendix Table A1 presents summary statistics using administrative records. Column 1 shows means for the study sample and column 2 shows means for households in a reduced bandwidth around the cutoff (the two SISBEN IV categories that are closest to the cutoff). Column 3 shows that surveyed households are very similar to all households in the study sample within a similar bandwidth in terms of average baseline characteristics. This suggests that survey non-response may be uncorrelated with households' characteristics.

For comparison, columns 4 to 6 show means for households in the 2019 wave of Colombia's nationally representative household survey (Gran Encuesta de Hogares, GEIH) by terciles of per-capita income. In terms of educational attainment of the household head and per-capita income, households on the margin of eligibility (column 2) are similar to households that belong to the middle-third of the per-capita income distribution (column 5). For reference, the average monthly per-capita income within a narrow bandwidth around the cutoff (COP 285,000) is equivalent to USD 7 per day, after adjusting for purchasing power;[14] well beyond the international poverty threshold of 2 USD

[13]For reference, the bandwidth corresponding to SISBEN IV categories C5 and C6 is 0.16.

[14]We use the PPP-adjusted exchange rate for 2017 (the year the admin data on income

per day, but still within the range of high vulnerability to falling into poverty (The World Bank, 2021a).

In terms of pre-program spending patterns, households located around the program's eligibility cutoff differ substantially from the poorest households in the social registry. Appendix Figure A1 shows that, for households in SISBEN IV, food spending as a share of total spending declines with per-capita household income. Among the poorest households (deciles 1 to 4), food spending represents roughly 60% of total expenses. Among the households located around the program eligibility cutoff, food spending only represents roughly 45% of total per-capita expenses.

IV Research Design

IV.A Econometric specification

We exploit the existence of a program eligibility cutoff to identify the causal effects of the program using a regression discontinuity design. Within our study sample, households with a ratio of predicted per-capita income to extreme poverty line below the program cutoff were eligible to receive Ingreso Solidario. Thus, our empirical design compares the outcomes of households that, based on their ratio, were marginally eligible for the program to the outcomes of those that were marginally ineligible.

We estimate the effect of being eligible for the program on outcome y_i using the following specification:

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was collected). The data was obtained from: `https://data.worldbank.org/indicator/PA.NUS.PPP?locations=CO`

$$
\begin{aligned}
Y_i =& \beta_0 + \beta_1 Eligible_i + \theta_1 f(c - ratio_i) + \theta_2 Eligible_i \times f(c - ratio_i) \\
& + \gamma x_i + \psi_d + \epsilon_i
\end{aligned} \tag{1}
$$

where $ratio_i$ denotes the predicted per-capita income to extreme poverty line ratio corresponding to household i recorded in SISBEN IV system as of February 2020; $Eligible_i = \mathbf{1}[ratio_i \leq c]$ is an indicator of whether household i's ratio is below the program eligibility cutoff (c); x_i is a vector of household demographic characteristics measured before the program. In most specifications, x includes the age, sex, and educational achievement of the household head, three indicators of whether the household head cohabits with their partner, contributed to social security, and was formally employed, and household-level characteristics, such as number of members, and an index of asset ownership and dwelling quality.[15] ψ_d denotes department-urban/rual fixed effects to account for the fact that the extreme poverty lines used to define *ratio* vary across departments and between urban and rural areas within a department.[16] Finally, $f()$ denotes polynomials based on the normalized running variable $(c - ratio_i)$, and ϵ_i is an error term.

We estimate equation (1) using triangular kernels so that a larger weight is given to observations closer to the cutoff. Because program eligibility varies at the household level, we conduct inference based on standard errors clustered at the household level (Abadie et al., 2022).

The parameter of interest, β_1, captures the reduced-form (RF) effect of being eligible for the program or the intention-to-treat (ITT) effect of the pro-

[15]To maintain privacy, we were able to merge data from PILA and RIPS only with administrative records on age, sex and an urban/rural indicator. In specifications using these data, we include only these three variables as controls.

[16]In Colombia, departments are the largest regional administrative unit.

gram on household outcomes. Our empirical approach enables us to estimate the effect of the program among households on the margin of eligibility. Thus, our estimates are informative for the policy decision of whether to expand the program's coverage to individuals on the margin of eligibility.

The program eligibility threshold was roughly equivalent to the 42th percentile of the 2019 (pre-program) per-capita income distribution.[17] Consequently, our results capture the impact of income-support programs among ex-ante lower middle-income households that were not poor enough to be included in preexisting social programs targeted at the poorest households but who can be highly vulnerable in times of economic crises (Busso et al., 2021; Bottan et al., 2020b).

Bandwidth and polynomial choice. We estimate (1) using a quadratic polynomial to flexibly control for the running variable in our main specification. We also report robustness to using 1st and 3rd degree polynomials in the Appendix. Our main specification using administrative records uses Cattaneo et al. (2019)'s data-driven selection process to define the estimation bandwidth for each outcome. In the case of survey data, the bandwidth is predetermined by the data collection process (see Section III), so we use all the available observations. We also report robustness checks to alternative bandwidths in the Appendix.

[17]We computed percentiles of the per-capita income distribution in Colombia, using the 2019 National Survey of Household Budgets (Encuesta Nacional de Presupuesto de los Hogares), which collects information from a nationally representative sample of households. We then divided these percentiles by the average extreme poverty rate to obtain comparable ratios.

IV.B Threats to identification

Manipulation. The running variable corresponds to administrative records from February 2020, two months before the announcement and implementation of the program. Thus, before the program, there was no incentive to manipulate the score in order to become eligible for the program or to register in the social registry to receive the program. A visual inspection of the administrative records (see Appendix Figure A2) suggests that there are no discontinuous changes in the density of observations around the cutoff. Following Cattaneo et al. (2019)'s density test, we are not able to reject the null that there are no mass points on either side of the cutoff using the SISBEN IV administrative records at conventional confidence levels (Panel A of Appendix Table A2). We observe similar results when we analyzed the data corresponding to the PILA-RIPS administrative records, and to the sample of students who participated in PRUEBA SABER 11 in 2020 and whose households were registered in the administrative records of SISBEN IV corresponding to February 2020.

A related threat to validity for results using the survey data is that becoming eligible for the program may have caused differential survey response rates for households on each side of the cutoff. Panel A of Table A2 finds no evidence of manipulation around the cutoff in the survey data. This is corroborated by Panel B, where we find that there are no significant differences in survey attrition based on program eligibility.

Balance. We also test for discontinuities in pre-determined demographic characteristics and outcomes in administrative records by estimating equation (1) and selecting the MSE-optimal bandwidth for a second-order polynomial following Calonico et al. (2019)'s approach for each variable. Appendix Table A3 reports the results. We detect small but significant differences for four of sixteen demographic characteristics from administrative data. Specifically, we detect a difference in the age of the household head between eligible and ineligible households of 0.72 years, a difference in the number of household members of 0.028 members, and a difference in the probability that the household head's highest educational level is high school of 1 percentage point, which mirror-images a similar negative difference in the probability that the household head's highest educational level is primary school. Reassuringly, we do not find neither substantial nor significant differences in terms of per-capita spending (a key outcome) and an index of asset ownership. Turning to predetermined outcomes, we find small, significant differences only in formal employment rates during 2019 (significant at 5%) and in grade repetition (significant at 10%) ; which are smaller than 1 percentage point and a tenth of a percentage point, respectively. We control for formal employment at baseline in our regressions to prevent these small differences from affecting the results.

V Effects on program take-up and household income

First, we use administrative records on program disbursement to show that program take-up varies discontinuously at the threshold of eligibility. Figure 1a shows that the probability of receiving at least one program payment by July 2021 varies discontinuously around the eligibility cutoff within a narrow bandwidth. Table 1 reports results for the reception of program transfers from equation (1). Column 1 shows that, relative to marginally ineligible households, marginally eligible households were 90.5 percentage points more likely to have received at least one program payment between April 2020 and

July 2021. Similarly, column 2 shows that eligible households received 13.7 additional program payments up to July 2021, relative to ineligible households (approximately USD 480 in total).

Second, we use survey data pooled across both survey rounds to show that program eligibility increased household income. We apply the inverse hyperbolic sine transformation to income to accommodate observations with zero income.[18] Figure 1b shows that marginally eligible households have greater household income approximately 6 and 18 months after the launch of the program than marginally ineligible households. Column 3 in Table 1 shows that the program increased the inverse hyperbolic sine of per-capita income by 0.25, which implies a 27% increase in per-capita income.[19] This increase in income is approximately equivalent to the average per-capita transfer amount (COP $ 64,000).

Because we study a period of economic recession coinciding with the COVID-19 pandemic in which many households lost their livelihoods (see Bottan et al. (2020b)) and a study population that did not previously have access to social programs, we investigate the impact of program eligibility on reporting positive income. Column 4 in Table 1 shows that program eligibility increased the probability of reporting positive income by 4.6 percentage points. Overall, the program increased household income and attenuated the most severe collapses in income.

The fact that we observe a positive effect on income that is roughly equiv-

[18]Because we study a setting in which many households lost their livelihoods (Bottan et al., 2020b), there is a substantial share of households who report no income.

[19]Specifically, following Bellemare and Wichman (2020) one can recover semi-elasticity coefficients by applying the following transformation to the estimated treatment effect ($\hat{\beta}$) on a transformed variable: $\exp(\hat{\beta}) - 1$.

alent to the amount of the transfer suggests that there was relatively little crowding out of other income sources. Specifically, we investigate the effect of program eligibility on two potential margins of adjustment, inter-household transfers and labor supply. We find no evidence of reductions in incoming transfers from friends or relatives (see column 1 in Appendix Table A4). Likewise, we rule out adjustments in labor supply using both survey data and administrative data. Using individual-level survey data pooled across both survey waves, column 2 shows no significant or substantial impacts on employment among adults. We are able to rule out declines in employment as small as 3.8 percentage points at a 95% confidence level. Further, we find no evidence of effects on hours worked (column 3) or on the probability of seeking a job or an opportunity to work more hours (column 4). Finally, using administrative records on formal employment and pooling individual-level data from June 2020, December 2020, and June 2021, we find no evidence of negative effects on the probability of formal employment (see column 5). These results indicate that the program did not decrease the labor market participation of adults or increase labor informality.[20]

VI Downstream effects.

The households that we study, by virtue of their position in the middle of the income distribution, may share some of the concerns of households located on either end of the income distribution. Households living in poverty may be primarily concerned with food security, sending their children to school,

[20]The program could have increase labor market informality, for example, if beneficiaries perceived that having formal, verifiable earnings, increased the risk of being excluded from the program (Bosch and Schady, 2019; de Brauw et al., 2015; Cruces and Bérgolo, 2013)

and keeping their family healthy. Middle-income households may share these concerns or they may be able to meet these basic needs without assistance. However, even if their economic situation frees them from the same concerns as households living in poverty, they may have concerns about the quality of food, education, and health services. At the same time, middle-income households may share some of the same financial concerns, such as reducing debt and accumulating savings, as high-income households. However, unlike higher-income households, they may lack access to formal financial markets (Banerjee and Duflo, 2008).

The unique economic position of middle-income households is reflected by the primary concern identified by households in our sample. Even during the onset of the COVID-19 pandemic (round 1), only 13% of marginally ineligible households stated that food security was their main concern. Twenty-eight percent stated that education and health were their top concerns, 27% identified job opportunities as their top concern, and 25% reported paying debts and rent as their top concern.

We organize the discussion of the program's downstream impacts into two families of outcomes. We start by discussing the effects on outcomes that are typically targeted by transfer programs to low-income households, such as food security, enrollment in school, drop out rates, and grade retention, and access to health care services (Bastagli et al., 2019). We next analyze the effects of the program on outcomes that may increase in priority as households move up the income distribution, such as non-food spending, ability to pay formal consumption debt, and to access the formal financial system.

VI.A Effects on food security, school attendance, and health care

Figure 2a shows that there are no impacts on food security. We measured food security using the Household Food Insecurity Access Scale (HFIAS, Coates et al. (2007)) collected in the second survey round.[21] Similarly, using survey data pooled across both rounds, Figure 2b shows that there is no discontinuous change in per-capita food spending around the program eligibility cutoff.[22] Columns 1 and 2 of table 2 corroborate the graphical evidence. The point estimates are small and insignificant. Thus, on average, program eligibility did not affect food security or food spending among marginal beneficiaries.[23]

Focusing on school age individuals, figures 2c and 2d use administrative records for 2020 and 2021 to show that there is no discontinuity in 2020 or 2021 school dropout or 2021 school enrollment around the cutoff of program eligibility.[24] Consistent with the graphical evidence, columns 3-5 of table 2 use administrative data to show that there are no impacts on dropout rates in 2020 and 2021, enrollment for the 2021 school year, or grade repetition for

[21]Specifically, we collected information about the incidence and frequency of 7 dimensions of food security. We used this data to create an index of food security that classifies households into four categories: severely food insecure HFIAS=1, mildly food insecure HFIAS=2, moderately food insecure HFIAS=3, and food secure=HFIAS=4.

[22]We use the inverse hyperbolic sine of food spending.

[23]This result is different to those of other studies analyzing the impacts of cash transfers among the poorest households. For example, increases in food consumption are often found in other cash transfer programs targeted at the poorest households (Bastagli et al., 2019), and there is recent evidence of increases in food access during the onset of the pandemic due to another cash transfer program in Colombia that targeted the poorest households Londoño-Vélez and Querubín (2022).

[24]We focus only on enrollment in 2021 as enrollment in 2020 is predetermined, with respect to the program.

students enrolled in 2021.[25] Our estimates are small and precise. For example, we are able to rule out declines in dropout as small as a third of a percentage point and increases in enrollment as small as two-thirds of a percentage point at a 95% confidence level. Finally, for the subsample of households with at least one family member taking the mandatory, standardized high school exit exam called SABER 11 in August 2020 (those with children enrolled in 11th grade), we find no impacts of the program on the global score that includes math, reading, social sciences, natural sciences, and english (see column 6).[26]

Overall, we find no evidence of effects of program eligibility on school enrollment or dropout despite the disruption to in-person instruction during the pandemic (The World Bank, 2021b). This may reflect the fact that access to education is near universal for the households in our sample. It may also mean that their concerns about education may reflect concerns about quality rather than access. Despite the null effects of the program on SABER 11 exam scores, we find evidence that suggests that program eligibility led to efforts to improve the quality of education. Using the first round survey data, we find that program eligibility increased per-capita education spending and time dedicated to studying (see Appendix Table A6). These effects dissipate by the second survey round. The transitory pattern appears consistent with a one-time investment to support schooling. Using information from a socioeconomic survey conducted as part of SABER 11, we find a positive but insignificant effect of program eligibility on owning a laptop or tablet (p-value=0.11).

[25]We do not examine grade repetition for students enrolled in 2020 as it will capture pre-program behavior, and unfortunately, we do not have access to similar information for students enrolled in 2022. Therefore, our data only enables us to draw conclusions about grade repetition during a period of mostly virtual learning.

[26]In Appendix Table A2, we show that there is no evidence of manipulation for the sample of students taking the SABER 11 examination in 2020.

Next, we use administrative records to analyze the impacts of the program on the ability of households to protect their members from highly infectious diseases and on their demand for mental health care. Figure 2e shows no large discontinuity in use of health services (e.g., chest X-ray exams, visits to the emergency room or hospitalization) related to severe Covid-19 cases between April 2020 and June 2021. We focus on health care services related to severe cases of Covid-19 because they are more likely to necessary medical services and, therefore, are more likely to reflect actual illness. In column 7 of Table 2, we find neither substantial nor significant impacts on health care related to severe Covid-19. At a 95% confidence level, we are able to rule out declines in the probability of receiving medical care due to severe COVID-19 symptoms as small as half a percentage point. Similarly, we find positive but insignificant effects on vaccination against COVID-19 using data from the second survey round (column 8) and no impacts on the probability of the death of a household member due to Covid-19 (column 9). This results suggest that middle-income households may had already been able to take the needed precautions to avoid infections even in the absence of the program.

A central concern during the lockdown-periods was that the combination of the public health pandemic, associated mitigation measures, and economic recession could increase the incidence of mental health issues. Thus, by relaxing a household's budget, the program could reduce the incidence of mental health issues. However, using administrative data on medical care for mental health issues during the 18 months following the implementation of the program, figure 2f shows that there is no discontinuity in the probability of seeking mental health care around the eligibility threshold. Consistent with the graphical evidence, column 10 of table 2 shows that there is no effect of eligibility for the program on the probability of seeking medical care for mental health issues. These null effects may reflect two opposing forces: cash transfers may have increased psychological well-being as in (Haushofer et al., 2020), offsetting the potential increases in the demand for mental health care due to the budget expansion induced by the program.

VI.B Effects on non-food spending and short-term debt for routine expenses

We next analyze the effects of the program on non-food spending—the largest spending category in our sample. For this, we pool data from both survey rounds to analyze the impacts of program eligibility on (the inverse hyperbolic sine of) per-capita non-food spending among marginally eligible households. Figure 3a suggests that program eligibility increased non-food spending. Column 1 of Table 3 shows that the effect is not estimated with precision and is not significant at conventional levels (p-value=0.106). However, the magnitude of the point estimate is non-negligible, suggesting a 13% increase in per-capita total non-food spending. This increase is equivalent to approximately one-third of the per-capita transfer amount. Further, the 95% confidence interval rejects relatively small declines in non-food per-capita spending (-3.4%) and includes increases as large as 27%.

The noisy increase in per-capita non-food spending is in line with declines in short-term debt to non-financial firms. This debt is to utility and retail companies and is typically associated with routine expenses. Using pooled data from the credit-bureau records across three points in time (June 2020, December 2020, and June 2021), figures 3b and 3c show a discontinuous decline in the probability that a household has any outstanding and past-due debts to utility or retail firms. Columns 2 and 3 of Table 3 shows that program eligibility reduces the probability of having outstanding debt by 1.3 percentage points and past-due debt for utilities by 1.1 percentage points (a 3.5% decline relative to marginally ineligible households). Graphical evidence suggests that program eligibility slightly decreased past-due debt credit card debt (see Figure 3d), but in columns 4 and 5, we find no impact on the probability of having outstanding or past-due credit card debt—two proxies for holding expensive debt. Together, the results suggest that the program helped households cover routine non-food expenses, but such effects were relatively modest.

Put together, the results suggest that the households that we study were able to cover their most basic needs even in the absence of the program. In contrast, the program appears to have enabled these middle-income households to cover other routine expenses, and avoid the penalties and costs of having past-due debt with utility companies.

VI.C Effects on access to formal financial products, savings, and credit

To ease the delivery of transfers, the implementing agency encouraged beneficiaries to open simplified savings accounts and receive their monthly transfers through direct deposit. Administrative records indicate that 74% of marginally eligible beneficiaries received at least one payment in a bank account as of July 2021. In this section, we trace the impacts of the program along the path toward becoming a user of formal financial products in good standing.

To begin, we show that program eligibility increased bank account ownership. Figure 4a shows a large discontinuous increase in the probability that at least one household member has an active bank account registered in the credit bureau at the threshold of eligibility. Specifically, program eligibility increased account ownership by 11 percentage points (see column 1 in Table 4), which represents roughly a 40% increase relative to the control group.

Next, we investigate whether the increase in bank account ownership translates into savings. Column 3 in Table 4 shows that the program did not increase ownership of fixed-term deposits (a proxy for formal savings). Similarly, in Column 4, we fail to detect an increase in the likelihood that a household has savings using survey data.[27]

Although we do not find effects of program eligibility on current savings, as discussed in Section VI.B, the program reduced the probability of holding past-due debt with utility and retail firms (see Table 3), and, as we discuss below, to financial firms. To the extent that holding past-due debt carries future penalties, reducing this type of debt is similar to increasing future net savings.

The lack of impacts on overall current savings may be a consequence of limitations on the bank accounts that households used to receive the transfers. 37% of marginally eligible beneficiaries received at least one payment in one of the three simplified digital savings accounts offered by partnering banks that impose caps on the balance that can be held in such accounts.

However, these digital bank accounts may have enabled households to conduct basic transactions, such as the payment for basic utility services or for purchases at grocery stores. Using survey data pooled across the two rounds, we find that the program substantially increased an index of digital account

[27]In Appendix Table A5, we are able to rule out sizeable effects on households' ability to cover at least a week worth of expenses using survey data. We also find no evidence of positive effects on the purchase of durable goods using survey data from round 2. Using survey data from both rounds, we are also able to rule out sizeable effects on investments in new businesses and the reception of transfers from other households.

usage for conducting transactions (see Figure 4b).[28] Column 2 of table 4 suggests an increase in digital bank account usage of 0.18 standard deviations. This effect is likely explained by the increased in access to digital bank accounts induced by the delivery of the program combined with an increase in disposable income to conduct transactions.

The increased use of these accounts may have paved the way for access to formal credit. Account use may have increased the beneficiary's familiarity with formal financial products or revealed important information about incoming and outgoing cash flows into the account, which can be used by lenders to improve borrower screening.

We investigate several distinct steps along the way to obtaining and maintaining formal credit. First, we investigate whether eligibility for the program increased credit inquires with the credit bureau, which is a prerequisite for obtaining a formal loan. Column 5 in table 4 shows that eligibility for the program increased the probability that at least one household member was the subject of a credit inquiry in the credit bureau records by almost 1 percentage point.

Second, we investigate whether the program increased the likelihood that households hold formal loans only in good standing. Figure 4c shows a discontinuous jump in the probability of having outstanding formal loans only in good standing using data from the credit bureau. Similar, Columns 6 of table 4, shows that eligibility for the program increased the likelihood of holding formal loans only in good standing by 1 percentage point. While this effect could imply that households obtained new loans, some of which would me-

[28] The index is computed using Anderson (2008)'s approach, based on 4 indicators on whether a respondent used the account to send or receive transfers to other people, to pay for basic utilities, to pay for the purchase of goods, and to save.

chanically be in good standing as they would not have had time to fall into past-due status or that households are maintaining their pre-existing loans in good standing, the increase in credit inquires suggests that this effect at least partially represents new formal loans. Either of these mechanisms implies that program eligibility increased households' history of loans in good standing and strengthened their relationships with the formal financial market.

Third, we investigate whether program eligibility led to over-borrowing and past-due formal loans. Figure 4d shows a discontinuous fall in the likelihood of having at least one past-due loan in the credit bureau data.[29] Columns 7 of table 4 shows that eligibility for the program decreased the probability of holding a formal loan with past-due payments or in bad standing, though more modestly. These results assuage concerns about over-borrowing.

VI.D Dynamics

Ingreso Solidario was created as a response to the economic crisis related to the COVID-19 pandemic. However, payments continued until December 2022—over a year since the mobility restrictions to contain the spread of the COVID-19 virus were lifted. By examining treatment effects over time, we can determine whether the effects that we document are specific to periods of economic crisis or persist during off-crises periods.

Figure 5 shows the effects of the program over time. Figure 5a uses survey data to show that there were no substantial or significant differences in per-capita income before the program, validating our empirical design. It also shows that the impacts on per-capita income were particularly high during

[29]Unfortunately, the credit bureau data only enables us to observe the worst status among all the loans that appear in the database.

the height of the COVID-19 pandemic.[30]

Figure 5b shows that the lack of effects on food spending was relatively consistent over time. Figure 5c shows that the increases in nonfood spending were statistically significant in the case of the first round of the survey (September 2020), albeit only at 10%. The effect is not significant by the end of 2021, but in terms of magnitude, it is similar to that of round 1. Reassuringly, in both cases, we find neither substantial nor significant differences in pre-program outcomes.[31] Consistent with these patterns, in figure 5d, we observe that the declines in outstanding debt with utility and retail companies appear relatively constant over time, with no significant impacts before the program.

Turning to formal financial products, Figures 5e and 5f show that the increases in the ownership of active bank accounts and of loans in good-standing persist over time. This persistent positive impact on access to financial products suggests that increased access to formal financial products may be the program's unintended long-term legacy. It may also mean that, beyond having access to the steady stream of income offered by the program, the set of tools to cope with negative shocks available to these middle-income beneficiary households may have increased. We analyze the empirical implication of this in Section VII.

[30]In the case of income, during the first survey wave we collected retrospective data on total household income corresponding to the months of February 2020 (before the COVID-19 pandemic), June 2020, and September 2020. Due to limitations in the survey length, we were not able to obtain data on spending at the same frequency.

[31]Because we did not collect pre-program data on spending, we use administrative data from SISBEN IV for the set of households included in the survey to measure pre-program food and non-food spending.

VI.E Robustness

Our results are robust to alternative specifications. In the case of outcomes measured using administrative records, Appendix Figure A3 plots RD estimates using equation (1) for different estimation bandwidths using linear, quadratic, and cubic polynomials. In all cases, the results are quantitatively similar to those in our main specification. Reassuringly, this is also true in narrower bandwidths, specifically in the bandwidth defined by the two SISBEN IV categories closest to the cutoff (C5 and C6). Categories C5 and C6 define a bandwidth as narrow as 0.16, which is substantially narrower than the MSE-optimal bandwidths used in our baseline specifications for all outcomes.

In the case of outcomes measured in survey data the bandwidth in our baseline specification was defined by the data collection process. Appendix Figure A4 reports estimates using different polynomial specifications and narrower bandwidths, stopping at a bandwidth equivalent to 50% of the bandwidth used in our main specification, which roughly reduces the number of households by half. In all cases, the results are qualitatively and quantitatively similar to those in our main specification, although we lose power with narrower bandwidths, especially when estimating higher-order polynomials.

Finally, our main estimates use controls to increase precision and to account for some of the very small though statistically significant differences detected in our balance analysis around the cutoff (see Appendix Table A3). To demonstrate that the results are not driven by the inclusion of these controls, we also report results without including controls in Appendix Tables A7 to A10. Reassuringly, none of our results are driven by the inclusion of controls in the regressions.

VII Consumption smoothing

Although the program had no impact on food spending and a relatively small impact on non-food spending on average, the program may deliver substantial impacts on these outcomes when households experience economic shocks. The middle-income households in our sample may be able to satisfy their basic needs on average, but they may be vulnerable to economic shocks that induce unexpected and unavoidable spending. If households are constrained by lack of insurance against these economic shocks, then the program may enable households to smooth the effects of the shock and minimize adjustments to consumption.

To test the extent to which the program assisted households in coping with large economic shocks, we collected survey data on one of the starkest shocks that households can face, the death of a household member. Specifically, we ask households whether any household member passed away during the twelve months preceding the second survey round (November 2020 - October 2021). We use data on income and spending from the second round of the survey to analyze whether eligibility for the program yielded heterogeneous effects by exposure to an economic shock.

This type of economic shock has two important analytical characteristics. First, the timing of the death of a family member is unlikely to vary discontinuously on either side of the cutoff. Column 1 in Table 5 shows that there are no substantial or significant impacts of the program on the probability of experiencing the death of a household member. Second, the occurrence of such a shock is likely to squeeze a household's budget, by reducing income due to the lost earnings of the deceased household member or by requiring expenses for medical care or funerals.

To understand the extent to which the program helped smooth these severe negative shocks, we estimate the following specification:

$$\begin{aligned} Y_i = \beta_0 + \beta_1 Eligible_i + \beta_2 Eligible_i \times Shock_i + \beta_3 Shock_i \\ + \theta_1 f(c - ratio_i) + \theta_2 Eligible_i \times f(c - ratio_i) + \gamma x_i + \psi_d + \epsilon_i \end{aligned} \quad (2)$$

where $Shock_i$ identifies households that suffered the death of a household member during 2021. In this case, the parameters of interest are β_1 which captures the treatment effect of the program on households that did not experience a shock (the omitted category), β_2 which captures the differences in treatment effects between households that did and did not experience shocks, and β_3 which captures the correlation between the outcome of interest (income or spending) and exposure to the shock among households in the control group.

The results are reported in Table 5. Column 2 shows that per-capita income declines for households that experienced a shock. However, eligible households that experienced the shock are able to fully offset that decline. While the interaction term is not significant at conventional levels (p-value=0.15), the magnitude mirror-images the significant decline in income for households that experienced the shock. In column 5 we observe a similar pattern in total spending. Total spending significantly declines for households that experienced a shock but this decline is fully offset by the program. Relative to households that did not experience a shock, the impacts of the program on spending are substantially and significantly larger. Columns 3 and 4 suggest that the heterogeneous effects of total spending are primarily driven by non-food spending, the largest component of total household spending. The results indicate that middle-income households are constrained by lack of insurance and that the program may have prevented households from reducing non-food spending to smooth food consumption amid the shocks.

Robustness. One potential concern with the previous analysis is that households that experienced the death of a household member in 2021 were structurally different than those that did not. One way to circumvent this concern is to control for household unobserved time-invariant characteristics. We exploit the fact that we observe data on income and spending in 2020 (before the occurrences of the shocks) to compute changes in outcomes between 2021 and 2020. We then use these changes to estimate a version of equation (2) using changes in consumption between the two survey rounds (i.e., between 2021 and 2020) in the spirit of Gertler and Gruber (2002), who analyze how household consumption responds to changes in health status in Indonesia. Our sample size is smaller due to survey attrition, but the results in Appendix Table A11 are remarkably similar to those using our preferred specification.

Another potential concern is that the results might be particular to Covid-19 deaths, which may cast doubt on the external validity of the results. Panel A of Table A12 shows that the results are robust to excluding deaths related to COVID-19. To further allay concerns about the choice of economic shock, Panel B reports results using a different economic shock, whether any household member was hospitalized during the year preceding the second survey round (2021). The results are qualitatively similar, although the magnitudes are somewhat smaller, likely reflecting the differences in the severity of the shocks. Finally, in Appendix Table A13, we show that the results are robust to using linear and cubic polynomials instead of quadratic polynomials.

VII.A The role of increased access to finance.

The program may have enabled households to smooth consumption when experiencing an economic shock by providing a steady stream of guaranteed income. However, the program also may have enabled households to smooth consumption when faced with economic shocks by increasing access to formal financial products (see discussion in Section VI.C). For example, beneficiary households may use their digital bank accounts to receive transfers from other households as in Jack and Suri (2014a). Also, as discussed in Section VI.C, the digital footprint of their transactions using the digital bank accounts may have enabled them to obtain formal credit amid an emergency, likely at more reasonable rates than those offered by predatory lenders (called "préstamos gota a gota" in Colombia) or other informal lenders.

We explore these mechanisms using survey data in table 6. Columns 1 and 2 of table 6 show that there are no differential effects on the probability of receiving transfers or loans from other households by shock exposure. This is perhaps surprising given the evidence on how mobile money enables households to smooth consumption through risk-sharing networks in Kenya and Tanzania (Jack and Suri, 2014b; Riley, 2018). One explanation, is that cross-household transfers amid shocks are relatively less salient among middle-income households in Latin America and the Caribbean (Bottan et al., 2020a).

In contrast, the results point out to a novel mechanism for consumption smoothing. Column 3 shows that the probability that at least one household member obtained a formal loan during the year preceding the second survey wave increased due to the program for beneficiary households that experienced a shock relative to those that did not. This increase in formal loans coincides with a decline in high-interest predatory loans among households that experienced a shock (see column 4), indicating that households were able to substitute away from high-interest predatory loans toward formal loans. This result is policy relevant: the program enabled households to expand their access to formal financial products which are essential to cope with severe negative shocks in settings with incomplete insurance markets.

VIII Conclusion

This paper analyzes the effects of a Colombian cash transfer program on middle-income households that are typically excluded from the social safety net. Monthly transfers increased the per-capita income of beneficiaries by 26% but had no significant effects on the food, education, or health outcomes that we examine. Non-food spending increased and the probability of having past-due debts related to utilities and retail declined. The program had persistent positive effects on formal financial outcomes, such as ownership of bank accounts and formal loans, indicating that the legacy of the program may be increased engagement with the formal financial market. These results reflect the different economic situations and priorities of middle-income beneficiaries relative to the poorest ones.

While, on average, the welfare-increasing impacts are relatively small, the program prevented households from sliding into poverty when they experienced severe shocks. Our results point to two mechanisms. First, programs that deliver a steady stream of income to middle-income households guarantees a minimum income. For middle-income households, cash transfers can function as insurance against shocks. Second, basic income programs delivered through financial technologies can pave the way towards financial inclusion, which in turn increases households' ability to cope with shocks by smoothing consumption. These two mechanisms underscore the importance of policies to protect middle-income households as part of the fight against poverty. Most policy efforts have focused on providing support to the poorest households. Our results suggest that support to middle-income households also plays a role in reducing poverty.

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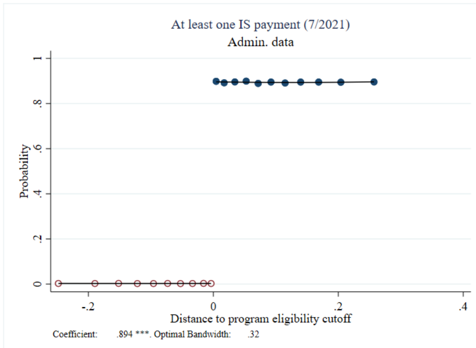
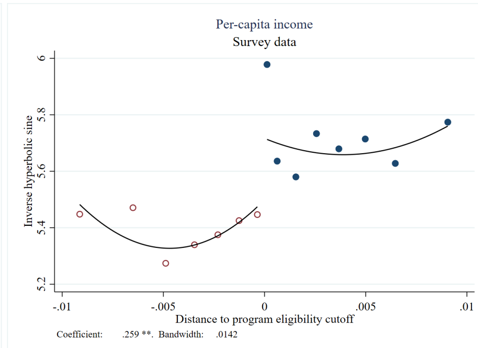
Figures and Tables



(a) At least one transfer payment



(b) Per-capita income (IHS)

Figure 1: Effects on transfer reception and income/

Note: The figure reports means by quantiles of the dependent variable around the cutoff determining program eligibility, and quadratic fits on each side of the cutoff using triangular kernels. Panels a) uses administrative records and a bandwidth that is selected based on Calonico et al. (2019)'s data-driven approach. Panel b) uses survey data and a bandwidth pre-defined by data collection.

Table 1: Effects on program reception and income

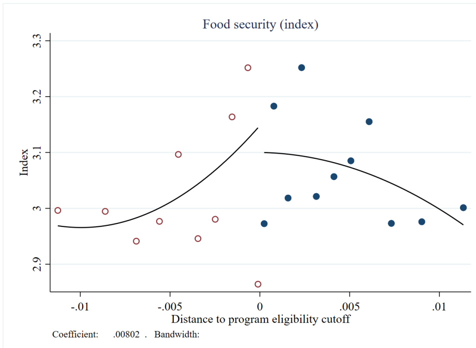
	(1) Transfer reception	(2) # of payments	(3) Per-cap income (IHS)	(4) Income>0
Eligible	0.905***	13.70***	0.259**	0.0458***
	(0.00204)	(0.0365)	(0.115)	(0.0169)
Control mean (DV)	0.00272	0.0324	5.435	0.914
Bandwidth	0.297	0.273	0.0137	0.0137
Obs. (in bandwidth)	415762	382259	10144	10144
# of households (in bandwidth)	415762	382259	4900	4900
Adjusted R2	0.820	0.780	0.0693	0.0253
Data Source	IS records	IS records	Survey	Survey

Note: The table reports estimates of the reduced-form impact of the program using equation (1). Columns 1 and 2 report results based on administrative records, using Calonico et al. (2019)'s data-driven approach to select the estimation bandwidth for each outcome. The rest of the coefficients are estimated based on survey data, using all the available observations. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.

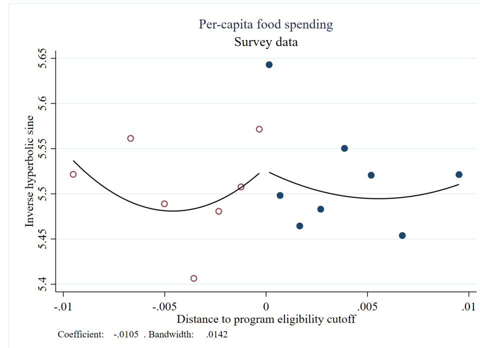
Table 2: Effects on food security, education and health

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Food security			**Education**				**Health**		
	Food Security Index	Per-cap. Food Spending (IHS)	Dropped out	Enrolled 2021	Repeating Grade (2021)	Standardized Global Score	Severe Covid	At least one COVID vaccine dose	Death Covid-19	Mental Health
Eligible	0.00802	-0.0105	-0.000946	-0.00701	-0.00181	-0.000977	-0.000909	0.0333	0.0145	0.00278
	(0.120)	(0.0704)	(0.00164)	(0.00719)	(0.00298)	(0.0557)	(0.00201)	(0.0415)	(0.0123)	(0.00258)
Control mean (DV)	3.007	5.517	0.0188	0.772	0.0278	-0.0103	0.0364	0.823	0.0206	0.0754
Bandwidth	0.0137	0.0137	0.250	0.222	0.221	0.264	0.288	0.0137	0.0137	0.344
Obs. (in bandwidth)	3432	6611	314010	139197	138101	9314	353493	3502	3463	422741
# of households (in bandwidth)	3432	4816	119774	106227	105394	9156	353493	3502	3463	422741
Adjusted R2	0.0470	0.0360	0.00498	0.0266	0.00543	0.125	0.00544	0.0584	0.0107	0.00978
Data Source	Survey R2	Survey R1-R2	SIMAT (2020-2021)	SIMAT 2021	SIMAT 2021	SABER 11	RIPS	Survey R2	Survey R2	RIPS

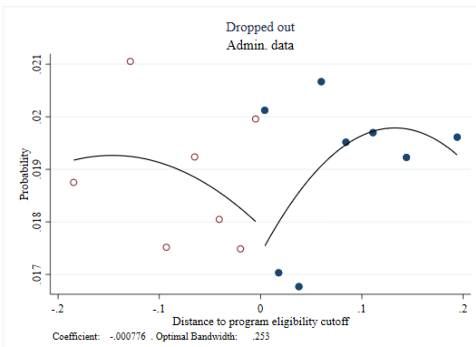
Notes: The table reports estimates of the reduced-form impact of the program using equation (1). Columns 1 and 8 and 9 report results using data only available in the second survey wave. Column 2 uses survey data pooled across two survey waves. We use all the available observations in the household surveys in the pre-defined bandwidth. The remaining results are obtained using administrative records, using a bandwidth selected based on Calonico et al. (2019)'s data-driven approach for each outcome. Column 3 use administrative records from SIMAT, at the individual level corresponding school-age household members in 2020 and 2021. Columns 4 and 5 use data from SIMAT corresponding to 2021, while column 5 uses administrative records from the 2020 round of Prueba SABER 11. Columns 7 and 10 use administrative records on the usage of medical services (RIPS) between April 2020 and July 2021. All regressions include quadratic polynomials on either side of the eligibility cutoff. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.



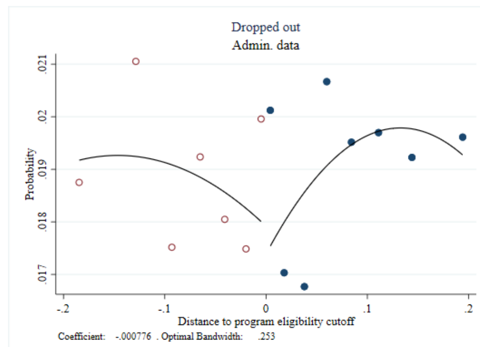
(a) Food security index



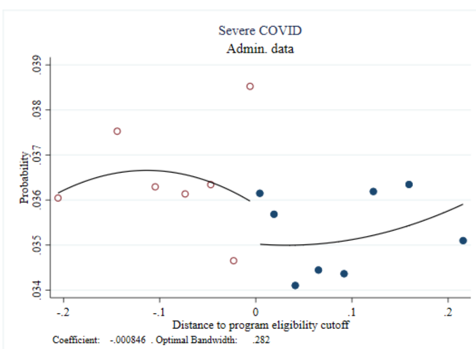
(b) Per-cap food spending (IHS)



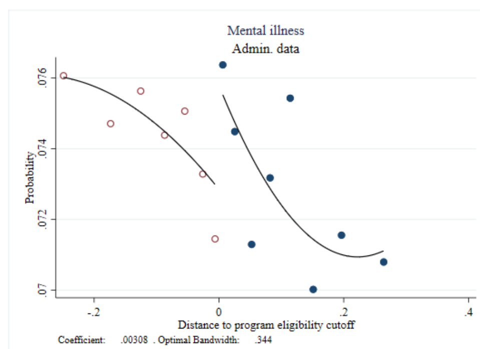
(c) Dropped out



(d) Enrolled (2021)



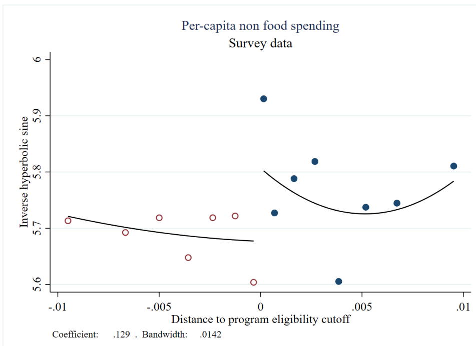
(e) Severe Covid



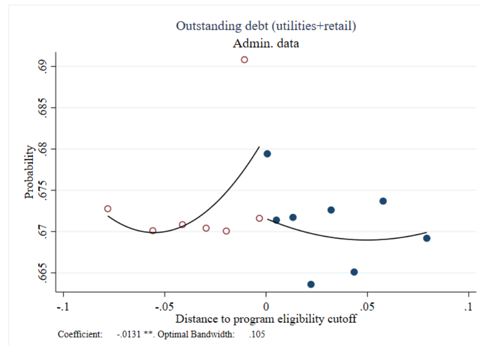
(f) Mental Illness

Figure 2: Effect on food security education and health

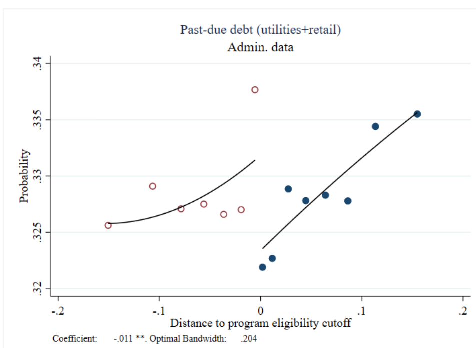
Note: The figure reports means by quantiles of the dependent variable around the cutoff determining program eligibility, and quadratic fits on each side of the cutoff using triangular kernels. Panels a) and b) uses survey data, and a bandwidth pre-defined by data collection (see notes to Table 2 for more details). Panels c) to f) use administrative records using a bandwidth selected based on Calonico et al. (2019)'s data-driven approach, for each outcome (see notes to Table 2 for more details). The bottom of each figure reports point estimates following equation (1). ***, **, and * denote significance at the 1, 5 and 10% levels.



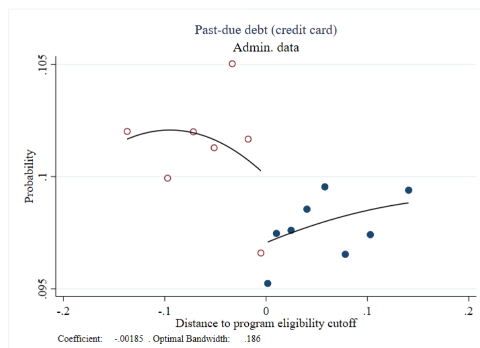
(a) Per-cap nonfood spending (IHS)



(b) Any debt (utilities + retail)



(c) Past-due debt (utilities+retail)



(d) Past-due debt (credit card)

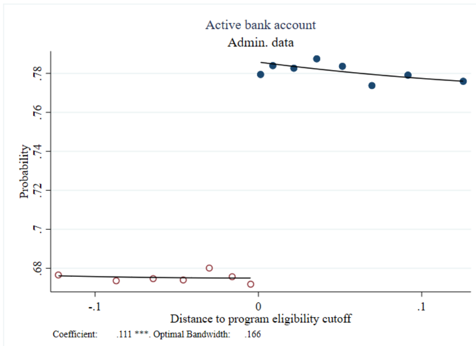
Figure 3: Effects on nonfood spending and short-term consumption debt.

Note: The figure reports means by quantiles of the dependent variable around the cutoff determining program eligibility, and quadratic fits on each side of the cutoff using triangular kernels. Panel a) uses survey data and a bandwidth pre-defined by data collection. Panels b)-d) use administrative records using a bandwidth selected based on Calonico et al. (2019)'s data-driven approach. The bottom of each figure reports point estimates following equation (1). ***, **, and * denote significance at the 1, 5 and 10% levels. See notes in Table 3 for more detail.

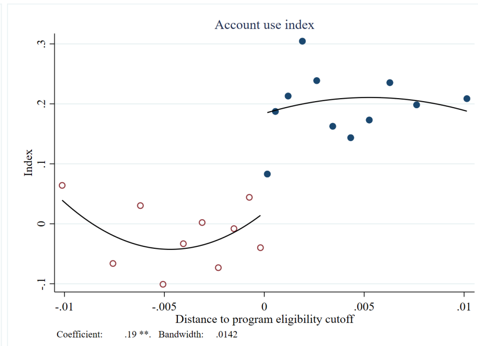
Table 3: Effects on non-food spending and short-term consumption debt

	(1)	(2)	(3)	(4)	(5)
	Nonfood	Debt (utilities+retail)		Credit cards	
	Spending	Any debt	Past-due debt	Balance >0	Past-due debt
Eligible	0.129	-0.0133**	-0.0110**	0.00226	-0.00523
	(0.0795)	(0.00656)	(0.00481)	(0.00507)	(0.00363)
Control mean (DV)	5.710	0.673	0.327	0.278	0.228
Bandwidth	0.0137	0.104	0.212	0.168	0.313
Obs. (in bandwidth)	6918	582008	1188988	704502	1314723
# of households (in bandwidth)	4978	145502	297247	234834	438241
Adjusted R2	0.135	0.153	0.0755	0.157	0.0788
Data Source	Survey R1-R2	Credit bureau	Credit bureau	Credit bureau	Credit bureau

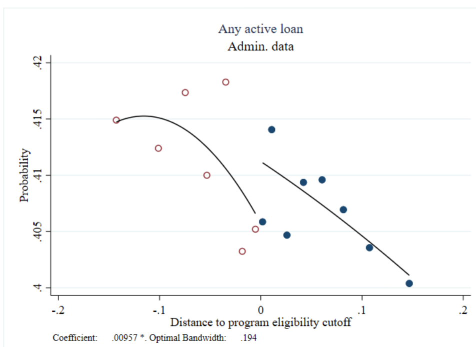
Notes: The table reports estimates of the reduced-form impact of the program using equation (1). Column 1 uses survey data pooled across two survey waves. We use all the available observations in the household surveys in the pre-defined bandwidth. The remaining results are obtained using administrative records from the Credit Bureau, using a bandwidth selected based on Calonico et al. (2019)'s data-driven approach for each outcome, and pooling observations corresponding to the four half-years following the implementation of the program. All regressions include quadratic polynomials on either side of the eligibility cutoff. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.



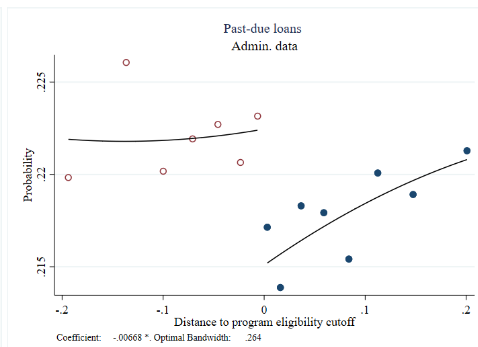
(a) Any active savings account



(b) Account use index



(c) Formal loans only in good standing



(d) Past-due debt (formal loans)

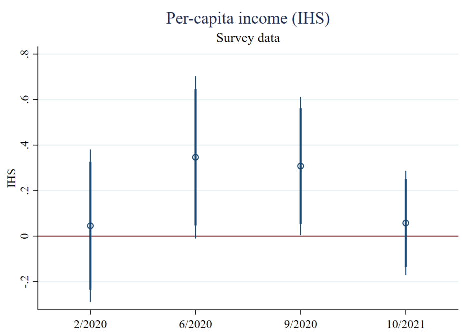
Figure 4: Effects on saving accounts and formal credit

Note: The figure reports means by quantiles of the dependent variable around the cutoff determining program eligibility, and quadratic fits on each side of the cutoff using triangular kernels. Panels a),c) and d) use administrative records corresponding to the credit bureau. The bandwidth is selected based on Calonico et al. (2019)'s data-driven approach pooling all periods so that it is constant across all point estimates within each panel. Panel b) uses survey data, pooled across two survey waves. The bottom of each figure reports point estimates following equation (1). ***, **, and * denote significance at the 1, 5 and 10% levels. See notes in Table 4 for more details.

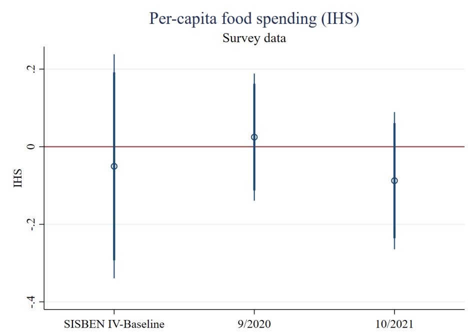
Table 4: Effects savings, and formal credit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Savings				**Credit**		
	Has savings account	Mobile account Usage Index	Fixed term deposits	Has any savings	Credit Inquiry	Only good standing loans	Any past-due loan
Eligible	0.111***	0.177**	0.000205	-0.0575	0.00739***	0.00957*	-0.00668*
	(0.00454)	(0.0768)	(0.00265)	(0.0370)	(0.00117)	(0.00497)	(0.00380)
Control mean (DV)	0.678	-0.00627	0.0435	0.135	0.0315	0.416	0.222
Bandwidth	0.166	0.0137	0.152	0.0137	0.221	0.194	0.264
Obs. (in bandwidth)	926536	6989	852176	3502	1235104	1082228	1477064
# of households (in bandwidth)	231634	5028	213044	3502	308776	270557	369266
Adjusted R2	0.172	0.187	0.0222	0.0647	0.0104	0.160	0.0770
Data Source	Credit bureau	Survey R1-R2	Credit bureau	Survey R1 - R2	Credit bureau	Credit bureau	Credit bureau

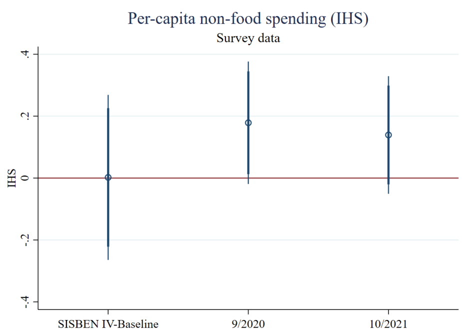
Note: The table reports estimates of the reduced-form impact of the program on several outcomes. All results are based on the specification in equation (1) using quadratic polynomials. Columns (1),(3) and (5) to (7) use administrative records from the credit bureau corresponding to the four post-program half years. Column (2) uses survey data, pooled across two survey waves. Column (4) uses survey data from the second survey wave. We use Calonico et al. (2019)'s data-driven approach to select the estimation bandwidth in the case of administrative records, and use all the available observations in the household surveys. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.



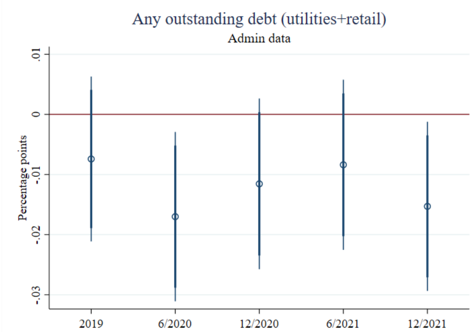
(a) Per-capita income (IHS)



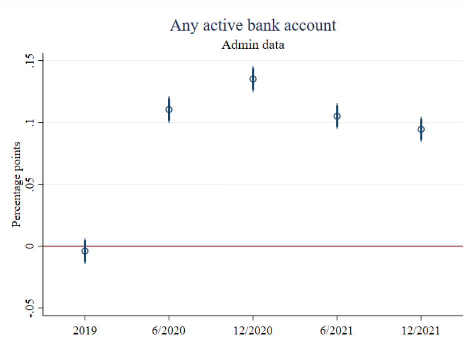
(b) Per-capita food spending (IHS)



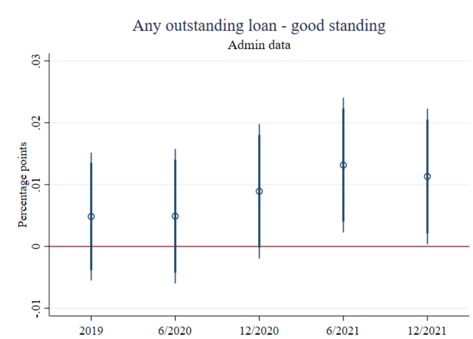
(c) Per-capita non-food spending (IHS)



(d) Any debt (utilities + retail)



(e) Any bank account



(f) Any active loan

Figure 5: Effects of the program over time

Note: The figure reports treatment effects estimated using the equation 1 at different points in time. Panels a),c) and d) use administrative records corresponding to the credit bureau. The bandwidth is selected based on Calonico et al. (2019)'s data-driven approach pooling all periods so that it is constant across all point estimates within each panel. Panel b) uses survey data, pooled across two survey waves. The bottom of each figure reports point estimates following equation (1). ***, **, and * denote significance at the 1, 5 and 10% levels. See notes in Table 4 for more detail.

Table 5: Effects by exposure to a severe shock

	(1)	(2)	(3)	(4)	(5)
	Death of a	Per-capita	Per-capita spending (IHS)		
	household member	Income (IHS)	Food	Non food	Total
Death of a household member		-0.217*	-0.124	-0.147	-0.200*
		(0.124)	(0.115)	(0.0995)	(0.106)
Eligible	0.0150	0.0418	-0.0989	0.110	0.00823
	(0.0272)	(0.118)	(0.105)	(0.101)	(0.0957)
Eligible X Death of household member		0.206	0.142	0.290**	0.301**
		(0.145)	(0.156)	(0.139)	(0.138)
Control mean (DV)	0.0680	5.916	5.556	5.704	6.430
Bandwidth	0.0137	0.0137	0.0137	0.0137	0.0137
Obs. (in bandwidth)	3462	3393	3293	3462	3462
# of households (in bandwidth)	3462	3393	3293	3462	3462
Adjusted R2	0.0167	0.115	0.0407	0.171	0.0988
Data Source	Survey R2	Survey R2	Survey R2	Survey R2	Survey R2

Notes: Column (1) reports estimates of the impact of the program on the death of a household member during 2021, using data from the second survey round. Columns (2) to (5) report results corresponding to the specification in equation (2) using quadratic polynomials, using income and spending in levels as dependent variables, using the inverse hyperbolic sine transformation. We use data corresponding to the second survey wave and use all the available observations in such round. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.

Table 6: Effects on incoming transfers and borrowing by exposure to a severe shock

	(1)	(2)	(3)	(4)
	Received		Took new loans	
	transfers	From individuals	Formal	Gota a gota
Death of a household member	0.0291	0.00165	-0.0367	-0.00194
	(0.0413)	(0.0206)	(0.0270)	(0.00661)
Eligible	0.0132	-0.0153	0.0291	-0.00480
	(0.0420)	(0.0205)	(0.0397)	(0.0167)
Eligible X Death of household member	-0.0453	0.00154	0.0902*	-0.0190**
	(0.0578)	(0.0284)	(0.0501)	(0.00899)
Control mean (DV)	0.126	0.0382	0.127	0.0133
Bandwidth	0.0137	0.0137	0.0137	0.0137
Obs. (in bandwidth)	3457	3462	3462	3462
# of households (in bandwidth)	3457	3462	3462	3462
Adjusted R2	0.0162	0.00388	0.0385	0.0112
Data Source	Survey R2	Survey R2	Survey R2	Survey R2

Notes: The table report results corresponding to the specification in equation (2) using quadratic polynomials. We use data corresponding to the second survey wave and use all the available observations in such round. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.

Online Appendix

A Supporting tables and figures

Table A1: Summary Statistics

	Administrative records (SISBEN IV)		Survey	GEIH 2019 (by terciles of per capita income)		
	Study Sample	Reduced bandwidth		Bottom	Middle	Top
	(1)	(2)	(3)	(4)	(5)	(6)
Age in February 2020	46.97	45.61	44.19	47.93	48.54	52.50
Head of household - Woman	0.44	0.49	0.41	0.37	0.35	0.39
No formal education	0.03	0.04	0.04	0.06	0.06	0.03
Primary education	0.44	0.48	0.45	0.51	0.47	0.32
Secondary education	0.32	0.34	0.35	0.25	0.32	0.24
Tertiary education	0.21	0.14	0.16	0.19	0.15	0.41
Number of household members	2.06	2.51	4.09	3.22	3.89	2.69
Urban area	0.83	0.81	0.80	0.80	0.81	0.94
Per-capita income (1000s of COP)	539.49	285.10	294.91	25.66	319.31	1405.37
Observations	2971013	235476	5042	9824	9510	9667

Note: The table presents means of pre-program characteristics using administrative records from the social registry (SISBEN IV) for the households in study sample (column 1) and for the households within the two SISBEN IV categories that are closest to the program eligibility cutoff (C5 and C6). Column 3 uses survey data corresponding to the first survey round. Columns (4) to (6) report summary statistics using survey data from the nationally representative 2019 GEIH household survey, by terciles of per-capita household income.

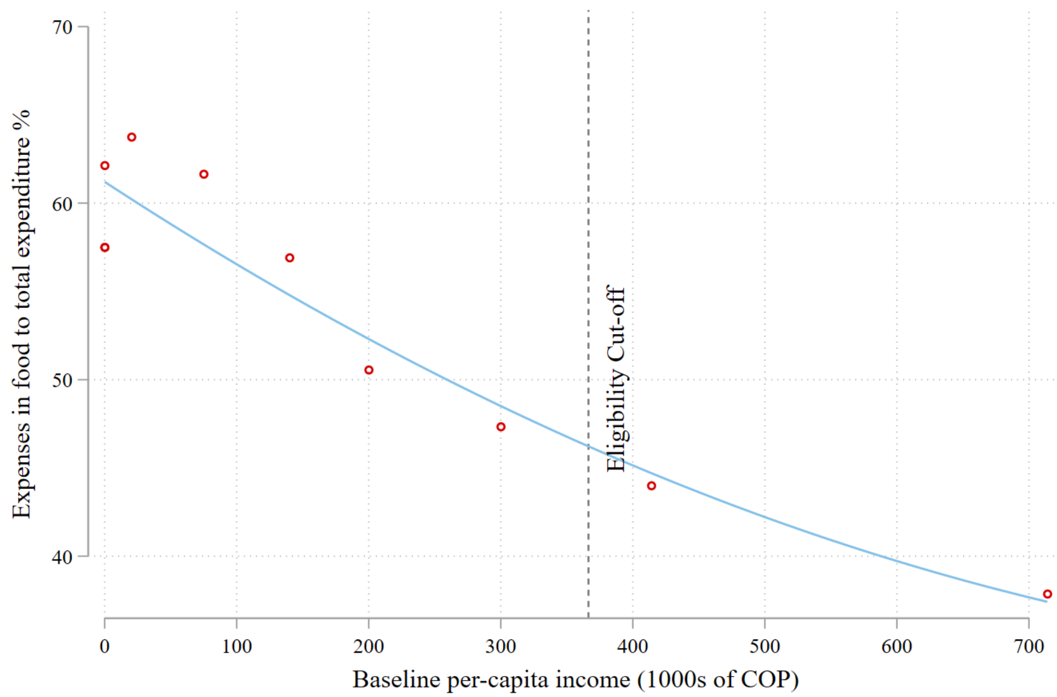
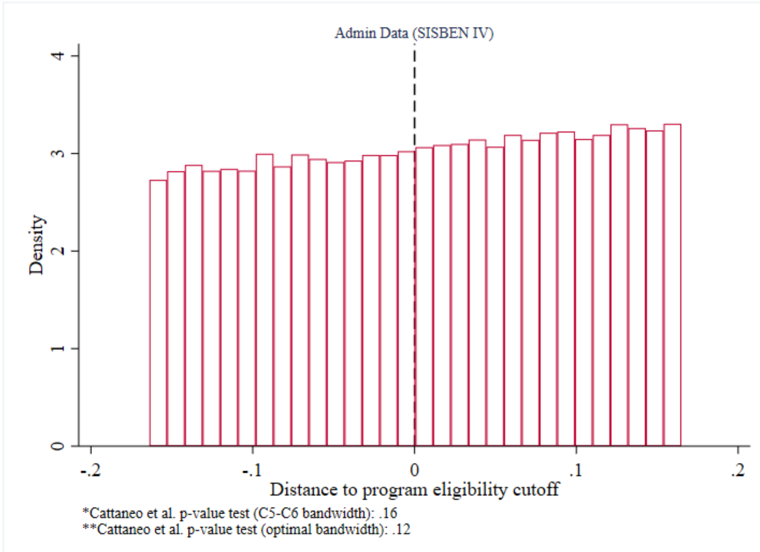
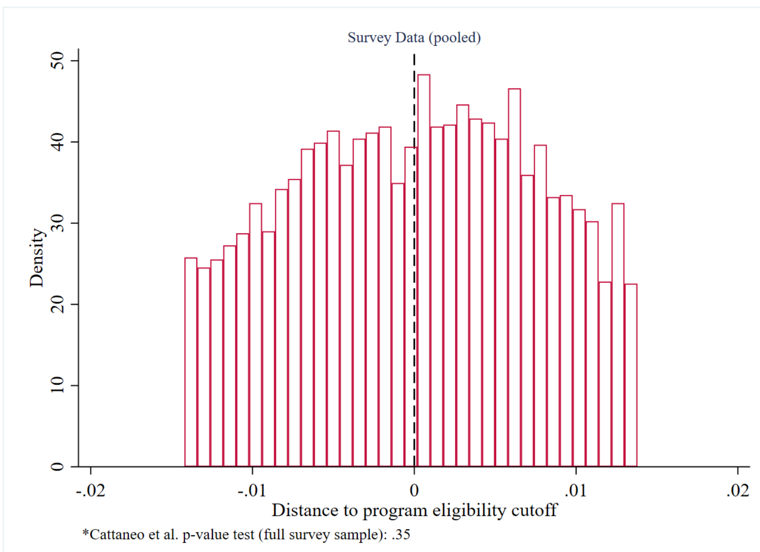


Figure A1: Food spending/total spending ratio by SISBEN IV income decile

Note: The figure plots means of food spending as a share of total household spending by deciles of per-capita income using the universe of households registered in Sisben IV. The vertical dashed line represents the equivalent of the program eligibility cutoff.



(a) Study sample



(b) Survey

Figure A2: Distribution of households around the program eligibility cutoff

Note: The figure depicts the distribution of the predicted per-capita income to extreme poverty line ratio, normalized with respect to the program eligibility cutoff. Panel a) uses administrative records from SISBEN IV, and panel b) uses data on all the households that participated in either survey round. P-values corresponding to Cattaneo et al. (2019)'s manipulation tests are reported at the bottom of each figure.

Table A2: Tests for Manipulation and survey attrition

Panel A: Manipulation tests (p-value)	
SISBEN IV (Study sample)	0.12
Survey sample	0.36
PILA-RIPS(Feb2020)	0.26
SABER11 (2020)	0.40
Panel B: Attrition	
R2 Survey response rate (Eligible households)	0.54
R2 Survey response rate (Ineligible households)	0.58
Difference(RD)	0.05
p-value (difference)	0.19

Note: Panel A reports results of (Cattaneo et al., 2019)'s density test using administrative records and using survey data. In the case of results based on administrative records we use Calonico et al. (2019)'s data-driven approach to select each bandwidth. In the case of survey data, we use all the available observations. Panel B reports the probability that households in the first survey round respond to the second survey round by program eligibility as well as the differences in these probabilities based on equation (1) using a quadratic polynomial. Inference is based on heteroskedasticity-robust standard errors.

Table A3: Balance around the program eligibility cutoff.

Panel A. Demographic Characteristics - Administrative records Sisben IV

	Non-eligible mean	Elegible mean	Difference(RD)	p-value
	(1)	(2)	(3)	(4)
Age(head)	45.96	45.23	-0.72	0
Head of household - Man	0.51	0.51	-0.001	0.83
Number of household members	2.49	2.51	-0.028	0.04
Secondary school or less	0.85	0.86	0.00	0.94
Technical education	0.09	0.09	0.002	0.58
University or Graduate studies	0.05	0.05	-0.001	0.45
Household head cohabits with partner	0.54	0.54	-0.003	0.62
Divorced	0.09	0.09	0.001	0.69
Contributive to social security regime	0.43	0.41	-0.005	0.29
Subsidized social security regime	0.47	0.49	0.003	0.59
Housing Quality Index	-4.28	-5.93	-0.312	0.25
Per-cap Spending (1000s COP)	307.3	293.51	-3.226	0.16
Covid cases per 100,000 people	0	0	0	0.87

Panel B. Selected baseline outcomes (2019) - Administrative records

	Non-eligible mean	Eligible mean	Difference(RD)	p-value
	(1)	(2)	(3)	(4)
Any debt (utilities+retail)	0.7	0.69	-0.007	0.11
Any bank account	0.81	0.8	0.004	0.28
Any active loan	0.45	0.44	0	0.92
Formal employment	0.36	0.35	0.011	0.01
Enrolled in school	0.83	0.83	-0.007	0.24
Repeating grade	0.03	0.03	-0.005	0.06
Global SABER 11 score (standardized)	-0.11	-0.12	0.09	0.16

Note: The table reports estimates of differences on pre-program household characteristics between eligible and ineligible households around the program eligibility cutoff using equation (1) using a quadratic polynomial. Panel A reports results based on administrative records of SISBEN IV. Panel B reports results on pre-program outcomes using administrative records. For each variable, we use Calonico et al. (2019)'s data-driven approach to select the estimation MSE-optimal bandwidth.

Table A4: Effects on incoming transfers and employment

	(1) Outgoing transfer	(2) Works	(3) Hours/week	(4) Search (job/more hours)	(5) Formal job
Eligible	0.0281	0.0169	-1.723	0.0353	0.00317
	(0.0278)	(0.0284)	(1.519)	(0.0299)	(0.00257)
Control mean (DV)	0.143	0.484	19.45	0.321	0.312
Bandwidth	0.0137	0.0102	0.0102	0.0102	0.206
Obs. (in bandwidth)	6972	22335	13123	13633	1502865
# of households (in bandwidth)	5023	3272	3249	3272	237256
Adjusted R2	0.0330	0.0239	0.0253	0.0215	0.466
Data Source	Survey R1- R2	Survey R1-R2	Survey R1-R2	Survey R1-R2	PILA

Note: The table reports estimates of the reduced-form impact of the program on several outcomes. Column 1 uses household-level survey data, pooling across two survey waves. Columns 2 uses survey data at the individual level, pooled across both survey waves. In the case of employment, we collected data corresponding to June and September 2020 and October 2021. Columns 3 and 4 use survey data pooled across survey waves. Column 5 uses data from PILA, pooled across the three half years after the program implementation. All results are based on the specification in equation (1) using quadratic polynomials. We use Calonico et al. (2019)'s data-driven approach to select the estimation bandwidth in the case of administrative records, and use all the available observations in the household surveys. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.

Table A5: Effects on financial resilience, investment in assets, and cross-household transfers.

	(1)	(2)	(3)	(4)
	Can cover a week's worth of expenses	Bought durables	New Biz.	Incoming transfer
Eligible	0.000410	-0.0352	-0.0241	0.00300
	(0.0546)	(0.0443)	(0.0190)	(0.0288)
Control mean (DV)	0.358	0.204	0.0669	0.156
Bandwidth	0.0137	0.0137	0.0137	0.0137
Obs. (in bandwidth)	3370	3463	6982	6972
# of households (in bandwidth)	3370	3463	5023	5022
Adjusted R2	0.0277	0.0275	0.0153	0.0168
Data Source	Survey R2	Survey R2	Survey R2	Survey R2

Notes: The table reports estimates of the reduced-form impact of the program using equation (1), using survey data. In columns (1) to (3), we use data from the second survey wave. For column (4), we pooled both survey rounds. We use all the available observations in the household surveys in the pre-defined bandwidth. All regressions include quadratic polynomials on either side of the eligibility cutoff. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.

Table A6: Effects on education spending and time use (survey data)

	(1)	(2)	(3)	(4)	(5)	(6)
	Per-capita Ed. Spending (HIS)		Time use studying (mins./day)		Admin records SABER 11	
	Round 1	Round 2	Round 1	Round 2	Owns laptop/tablet	Books
Eligible	0.248*	-0.0353	47.10**	19.89	0.0472	-0.0135
	(0.139)	(0.161)	(21.81)	(46.78)	(0.0296)	(0.0671)
Control mean (DV)	0.731	0.863	269.7	520.7	0.769	2.055
Bandwidth	0.0106	0.0106	0.0102	0.0102	0.246	0.234
Obs. (in bandwidth)	3421	3465	2449	1338	8203	7834
# of households (in bandwidth)	3421	3465	1678	930	8069	7703
Adjusted R2	0.0509	0.0822	0.114	0.0676	0.102	0.0485
Data Source	Survey R1	Survey R2	Survey R1	Survey R2	SABER 11	SABER 11

Notes: The table reports estimates of the reduced-form impact of the program on several outcomes using equation (1). All regressions include quadratic polynomials on either side of the eligibility cutoff. Columns (1) to (4) report results using survey data, by survey round. We use all the available observations in the household surveys in the pre-defined bandwidth. Columns (5) and (6) use administrative records corresponding to test-takers of the SABER 11 examination. In this case, we use Calonico et al. (2019)'s data-driven approach to select the estimation bandwidth. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.

Table A7: Effects on program reception and income - no controls

	(1) Transfer reception	(2) # of payments	(3) Per-cap income (IHS)	(4) Income>0
Eligible	0.893***	13.51***	0.236**	0.0427**
	(0.00212)	(0.0343)	(0.118)	(0.0168)
Control mean (DV)	0.00270	0.0327	5.438	0.915
Bandwidth	0.304	0.334	0.0137	0.0137
Obs. (in bandwidth)	431974	474072	10262	10262
# of households (in bandwidth)	431974	474072	4960	4960
Adjusted R2	0.796	0.756	0.0295	0.0131
Data Source	IS records	IS records	Survey	Survey

Note: The table reports estimates of the reduced-form impact of the program using equation (1) excluding control variables. See notes on Table 1 for other details. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.

Table A8: Effects on food security, education and health - no controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Food security			**Education**				**Health**		
	Food Security Index	Per-cap. Food Spending (IHS)	Dropped out	Enrolled 2021	Repeating Grade (2021)	Standardized Global Score	Severe Covid	At least one COVID vaccine dose	Death Covid-19	Mental Health
Eligible	-0.0315	-0.00540	-0.000446	-0.00393	-0.00223	-0.00670	-0.000927	0.0353	0.0121	0.00277
	(0.121)	(0.0713)	(0.00156)	(0.00765)	(0.00297)	(0.0507)	(0.00201)	(0.0422)	(0.0123)	(0.00257)
Control mean (DV)	3.005	5.516	0.0188	0.772	0.0278	-0.00282	0.0364	0.821	0.0216	0.0755
Bandwidth	0.0137	0.0137	0.277	0.199	0.219	0.338	0.289	0.0137	0.0137	0.346
Obs. (in bandwidth)	3472	6689	351328	125617	138871	12141	355083	3503	3503	425806
# of households (in bandwidth)	3472	4875	134010	95867	106004	11939	355083	3503	3503	425806
Adjusted R2	0.0243	0.0179	0.00395	0.00200	0.00210	0.0329	0.00544	0.0188	0.00790	0.00978
Data Source	Survey R2	Survey R1-R2	SIMAT (2020-2021)	SIMAT 2021	SIMAT 2021	SABER 11	RIPS	Survey R2	Survey R2	RIPS

Note: The table reports estimates of the reduced-form impact of the program using equation (1) excluding control variables. See notes on Table 2 for other details. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.

Table A9: Effects on non-food spending and short-term consumption debt - no controls

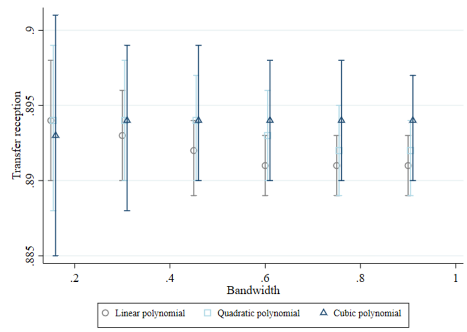
	(1)	(2)	(3)	(4)	(5)
	Nonfood	Debt (utilities + retail)		Credit cards	
	spending	Any debt	Past-due debt	Balance >0	Past-due debt
Eligible	0.139*	-0.00902	-0.00748*	0.00694	-0.00247
	(0.0825)	(0.00590)	(0.00418)	(0.00655)	(0.00328)
Control mean (DV)	5.710	0.672	0.325	0.273	0.102
Bandwidth	0.0137	0.149	0.297	0.106	0.184
Obs. (in bandwidth)	6998	844044	1684008	599500	1038924
# of households (in bandwidth)	5039	211011	421002	149875	259731
Adjusted R2	0.0689	0.0323	0.0224	0.0543	0.0206
Data Source	Survey R1-R2	Credit bureau	Credit bureau	Credit bureau	Credit bureau

Note: The table reports estimates of the reduced-form impact of the program using equation (1) excluding control variables. See notes on Table 3 for other details. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.

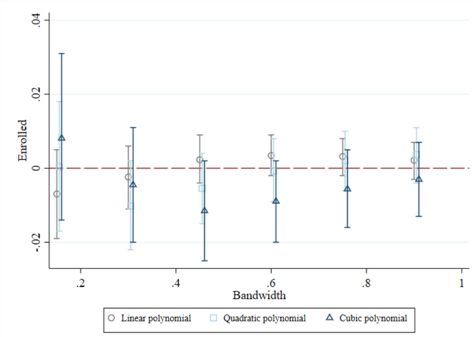
Table A10: Effects on savings and credit - no controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Savings				**Credit**		
	Has savings account	Mobile account Usage Index	Fixed term deposits	Has any savings	Credit Inquiry	Any outstanding loan	Any past-due loan
Eligible	0.111***	0.181**	-0.000876	-0.0614	0.00682***	0.00435	-0.00703*
	(0.00470)	(0.0848)	(0.00263)	(0.0377)	(0.00114)	(0.00487)	(0.00413)
Control mean (DV)	0.677	-0.000272	0.0433	0.132	0.0314	0.415	0.222
Bandwidth	0.187	0.0137	0.155	0.0137	0.235	0.241	0.234
Obs. (in bandwidth)	1061136	6998	877388	3503	1334348	1365800	1325212
# of households (in bandwidth)	265284	5039	219347	3503	333587	341450	331303
Adjusted R2	0.0480	0.0520	0.00493	0.0134	0.000936	0.0187	0.0255
Data Source	Credit bureau	Survey R1-R2	Credit bureau	Survey R2	Credit bureau	Credit bureau	Credit bureau

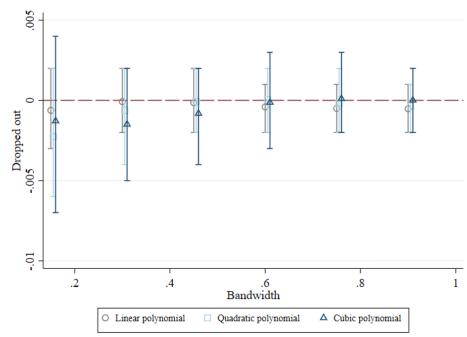
Notes: The table reports estimates of the reduced-form impact of the program using equation (1) excluding control variables. See notes on Table 4 for other details. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.



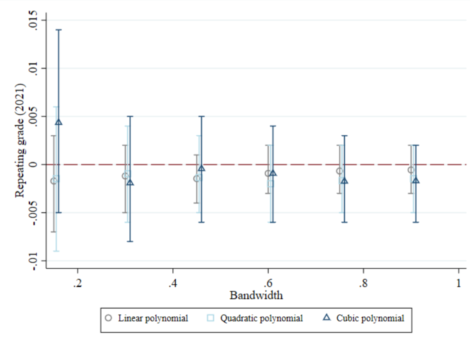
(a) Transfer reception



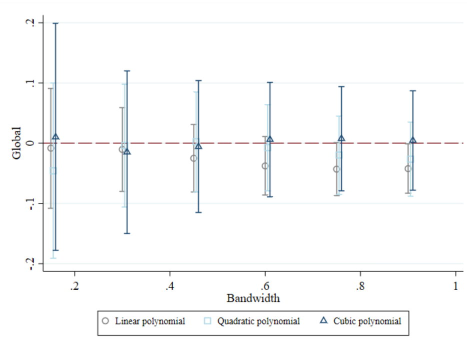
(b) Enrolled in school



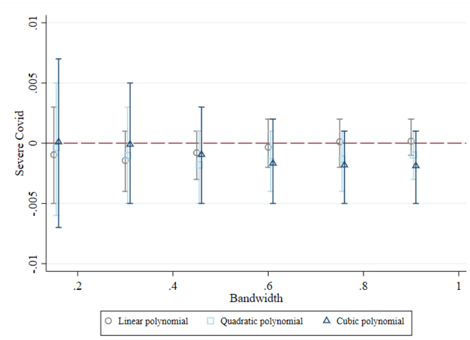
(c) Dropped out



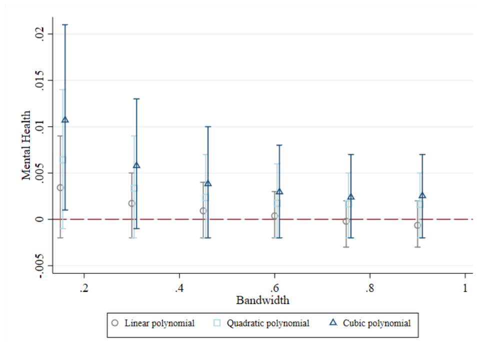
(d) Grade repetition



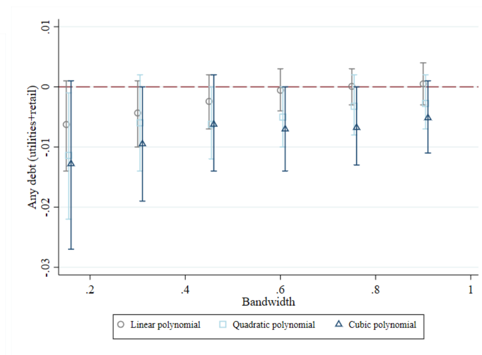
(e) Global test scores



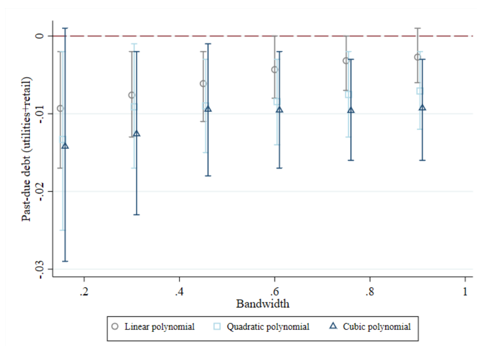
(f) Severe COVID-19



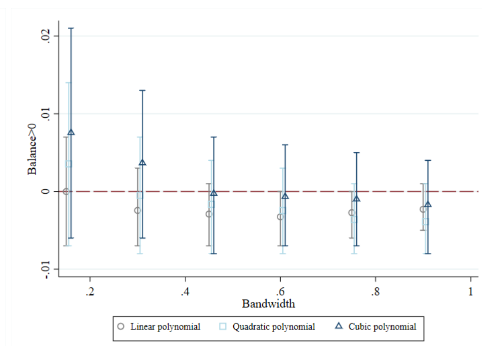
(g) Mental health care



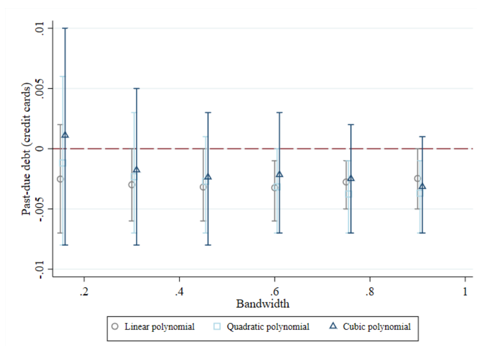
(h) Outstanding debt(utilities & retail)



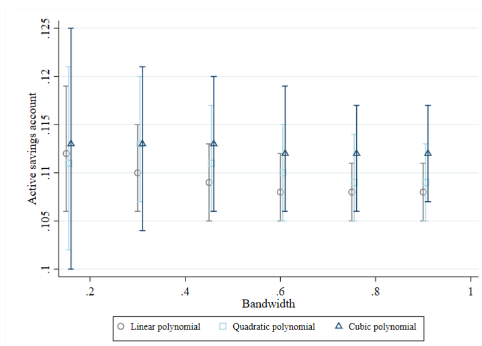
(i) Past-due debt(utilities & retail)



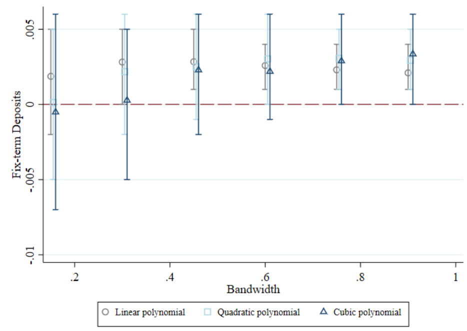
(j) Credit card balance >0



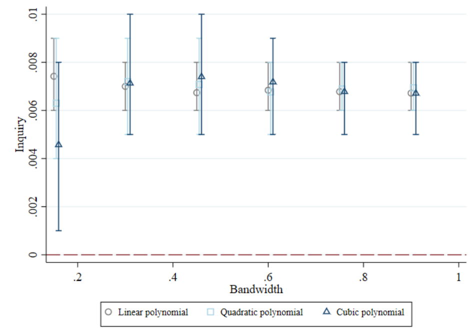
(k) Past-due credit card debt



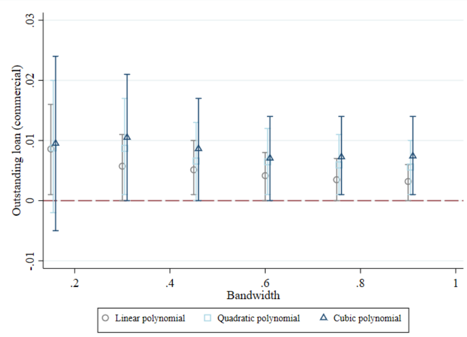
(l) Any active bank account



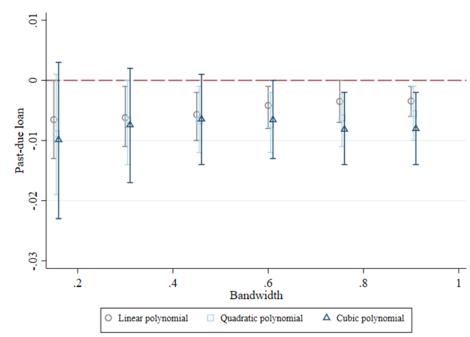
(m) Fixed term deposits



(n) Credit Inquiry



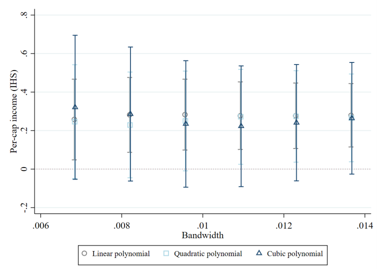
(o) Any Outstanding loan



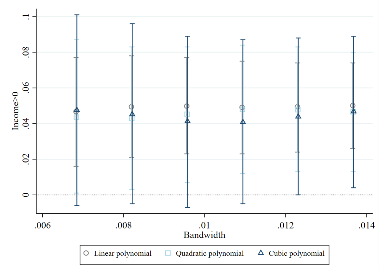
(p) Past-due loans

Figure A3: Robustness to alternative bandwidth choices and polynomial degree (Administrative data).

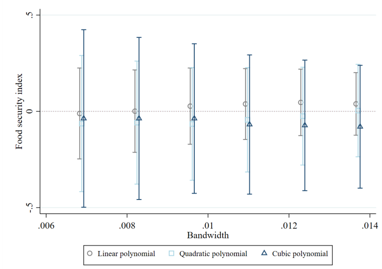
Note: The figure reports reduced-form estimates of the impacts of the program based on equation (1), estimated over different bandwidths and controlling for different polynomial degrees. The smallest bandwidth is equivalent to the maximum distance to the cutoff for the observations at the limits of SISBEN categories C5 and C6, which are the closest categories on each side of the cutoff. The 95% confidence intervals are based on standard errors clustered at the household level.



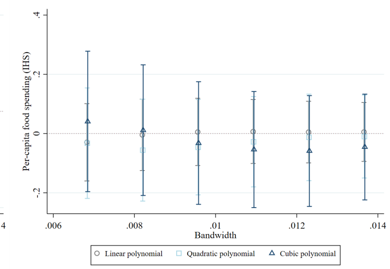
(a) Per-capita income (IHS)



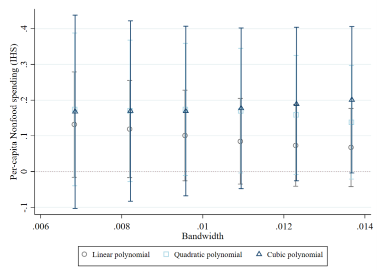
(b) Income>0



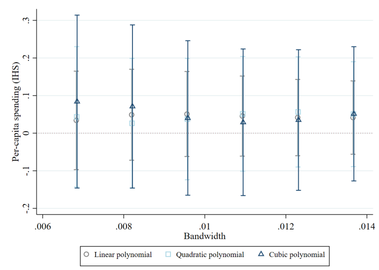
(c) Food security Index



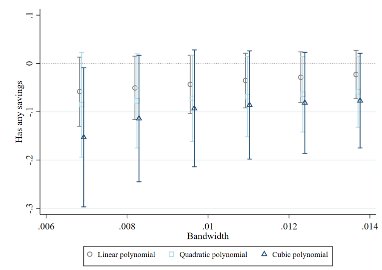
(d) Per-capita food spending (IHS)



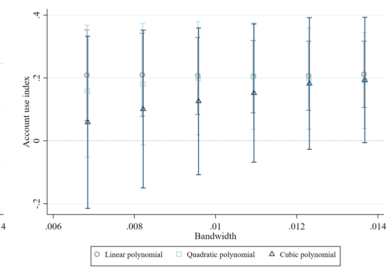
(e) Per-capita non food spending (IHS)



(f) Per-capita spending (IHS)



(g) Savings



(h) Bank account use index

Figure A4: Robustness to alternative bandwidth choices and polynomial degree (Survey data).

Note: The figure reports reduced-form estimates of the impacts of the program based on equation (1), using different polynomial degrees and bandwidths. All estimates are computed using survey data. The largest bandwidth is predefined by the survey data collection process. The smallest bandwidth represents half the narrow bandwidth available for the survey data. 95% confidence intervals are based on standard errors clustered at the household level.

Table A11: Effects by exposure to a severe shock - Differences

	(1)	(2)	(3)	(4)
		First differences		
	Per-capita	Per-capita spending (IHS)		
	Income (IHS)	Food	Non food	Total
Death of household member	-0.193	-0.0193	-0.387**	-0.396***
	(0.199)	(0.148)	(0.173)	(0.125)
Eligible	-0.0286	-0.162	-0.296**	-0.275**
	(0.186)	(0.134)	(0.134)	(0.120)
Eligible X Death of household member	0.280	0.0196	0.437*	0.424**
	(0.227)	(0.213)	(0.227)	(0.177)
Control mean (DV)	0.188	-0.0623	-0.123	-0.117
Bandwidth	0.0137	0.0137	0.0137	0.0137
Obs. (in bandwidth)	1881	1792	1936	1936
# of households (in bandwidth)	1881	1792	1936	1936
Adjusted R2	0.0113	-0.00523	0.0424	0.0239
Data Source	Survey R2	Survey R2	Survey R2	Survey R2

Notes: Columns (1) to (2) report results corresponding to the specification in equation (2) using quadratic polynomials, using changes in the the inverse hyperbolic sine income and spending as dependent variables. We use data corresponding to the second survey wave and use all the available observations in such round. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.

Table A12: Robustness to alternative definitions of shocks.

Panel A: Excluding COVID-19 deaths

	(1)	(2)	(3)	(4)	(5)
	Death	Per-capita	Per-capita spending (IHS)		
	(excluding COVID-19)	Income (IHS)	Food	Non food	Total
Death of household member		-0.217*	-0.124	-0.147	-0.200*
		(0.124)	(0.115)	(0.0995)	(0.106)
Eligible	0.0150	0.0418	-0.0989	0.110	0.00823
	(0.0272)	(0.118)	(0.105)	(0.101)	(0.0957)
Eligible X Death of household member		0.206	0.142	0.290**	0.301**
		(0.145)	(0.156)	(0.139)	(0.138)
Control mean (DV)	0.0680	5.916	5.556	5.704	6.430
Bandwidth	0.0137	0.0137	0.0137	0.0137	0.0137
Obs. (in bandwidth)	3462	3393	3293	3462	3462
# of households (in bandwidth)	3462	3393	3293	3462	3462
Adjusted R2	0.0167	0.115	0.0407	0.171	0.0988
Data Source	Survey R2	Survey R2	Survey R2	Survey R2	Survey R2

Panel B: Using hospitalizations as shocks

	Hospitalization	Per-capita	Per-capita spending (IHS)		
	of a household member	Income (IHS)	Food	Non food	Total
Hospitalization of a household member		-0.112*	-0.0422	-0.0484	-0.0429
		(0.0677)	(0.0658)	(0.0561)	(0.0536)
Eligible	0.0783	-0.00757	-0.0773	0.0732	0.00507
	(0.0493)	(0.118)	(0.108)	(0.104)	(0.0974)
Eligible X Death of household member		0.233***	-0.0294	0.201**	0.0895
		(0.0871)	(0.0872)	(0.0801)	(0.0732)
Control mean (DV)	0.268	5.916	5.556	5.704	6.430
Bandwidth	0.0137	0.0137	0.0137	0.0137	0.0137
Obs. (in bandwidth)	3463	3394	3294	3463	3463
# of households (in bandwidth)	3463	3394	3294	3463	3463
Adjusted R2	0.0137	0.116	0.0410	0.172	0.0974
Data Source	Survey R2	Survey R2	Survey R2	Survey R2	Survey R2

Notes: The table report results corresponding to the specification in equation (2) using quadratic polynomials. We use data corresponding to the second survey wave and use all the available observations in such round. Panel A excludes deaths related to COVID-19 from the definition of shock. Panel B uses whether any family member was hospitalized during 2021 as a shock. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.

Table A13: Robustness to alternative polynomial degrees.

	(1)	(2)	(3)	(4)	(5)
Panel A: Linear polynomial					
	Death of a household member	Per-capita Income (IHS)	Per-capita spending (IHS) Food	Non food	Total
Death of household member		-0.217*	-0.125	-0.147	-0.201*
		(0.124)	(0.116)	(0.0995)	(0.107)
Eligible	0.00293	0.0695	0.00419	0.108	0.0840
	(0.0184)	(0.0773)	(0.0733)	(0.0690)	(0.0650)
Eligible X Death of household member		0.206	0.140	0.291**	0.299**
		(0.145)	(0.157)	(0.139)	(0.138)
Control mean (DV)	0.0680	5.916	5.556	5.704	6.430
Bandwidth	0.0137	0.0137	0.0137	0.0137	0.0137
Obs. (in bandwidth)	3462	3393	3293	3462	3462
# of households (in bandwidth)	3462	3393	3293	3462	3462
Adjusted R2	0.0169	0.115	0.0404	0.171	0.0987
Data Source	Survey R2	Survey R2	Survey R2	Survey R2	Survey R2
Panel B: Cubic polynomial					
	Hospitalization of a household member	Per-capita Income (IHS)	Per-capita spending (IHS) Food	Non food	Total
Death of household member		-0.217*	-0.122	-0.149	-0.200*
		(0.124)	(0.115)	(0.0999)	(0.106)
Eligible	0.0291	0.0239	-0.178	0.193	-0.00719
	(0.0360)	(0.162)	(0.136)	(0.132)	(0.123)
Eligible X Death of household member		0.207	0.140	0.292**	0.300**
		(0.145)	(0.156)	(0.139)	(0.138)
Control mean (DV)	0.0680	5.916	5.556	5.704	6.430
Bandwidth	0.0137	0.0137	0.0137	0.0137	0.0137
Obs. (in bandwidth)	3462	3393	3293	3462	3462
# of households (in bandwidth)	3462	3393	3293	3462	3462
Adjusted R2	0.0163	0.114	0.0405	0.171	0.0982
Data Source	Survey R2	Survey R2	Survey R2	Survey R2	Survey R2

Notes: The table report results corresponding to the specification in equation (2) using different polynomials. Panel A uses a linear polynomial while Panel B uses a cubic polynomial. We use data corresponding to the second survey wave and use all the available observations in such round.Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.