IDB WORKING PAPER SERIES Nº IDB-WP-1412

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From Poverty Alleviation to Building Resilience in Middle-Income Households

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June 2023

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June 2023

Cataloging-in-Publication data provided by the
Inter-American Development Bank
Felipe Herrera Library

Re-thinking social protection: from poverty alleviation to building resilience in middle-income households / Diego A. Vera-Cossio, Bridget Hoffmann, Camilo Pecha, Jorge Gallego, Marco Stampini, David Vargas, Maria Paula Medina, Esteban Alvarez.

p. cm. — (IDB Working Paper Series ; 1412)

Includes bibliographic references.

1. Cash transactions-Colombia. 2. Transfer payments-Colombia. 3. Income maintenance programs-Colombia. I. Vera-Cossio, Diego A. II. Hoffmann, Bridget. III. Pecha, Camilo. IV. Gallego, Jorge. V. Stampini, Marco. VI. Vargas, David. VII. Medina, María Paula. VIII. Álvarez, Esteban. IX. Inter-American. Development Bank. Department of Research and Chief Economist. X. Series.

IDB-WP-1412

http://www.iadb.org

Abstract

We exploit an expansion in social protection to middle-income households to provide evidence on how middle-income households cope with economic shocks and how to build their resilience. We use a regression discontinuity design around the eligibility cutoff for a program that delivered monthly cash transfers mainly through bank accounts in Colombia. We find no impacts on food security, education, and health outcomes—the target outcomes of antipoverty programs. In contrast, program eligibility increases non-food consumption and reduces debt for routine expenses. Bank account ownership increases by 16%, and beneficiaries are more likely to borrow from formal lenders. Amid systemic and idiosyncratic shocks, the program prevents middle-income households from reducing non-food spending and acquiring debt for routine expenses. Moreover, when hit by severe shocks, beneficiary households substitute away from predatory loans. The results suggest that middle-income households are constrained by lack of insurance and that social protection can build middle-income households' resilience to shocks through both cash transfers and by integrating beneficiaries into formal credit markets.

JEL Classification: I18, I38, O15

Keywords: Basic income, Insurance, Cash transfers

Acknowledgements: The authors thank Olga Romero, Laura Pabon, Patricia Moreno, Darwin Cortes, Pablo Ibarrarán and Andres Bocanegra for their excellent comments and their support with the project. The authors also thank the staff at Departamento Nacional de Planeación and Departamento para la Prosperidad Social, for their collaboration throughout the project. The authors also thank Dayana Tellez, Juan Felipe García and IPA Colombia for their support during data collection. We thank Craig McIntosh, Mauricio Romero, Cynthia Kinnan, Rema Hanna, and numerous colleagues and seminar audiences for helpful suggestions. Santiago León and Juliana Dueñas provided excellent research assistance. Opinions, findings, conclusions, and recommendations expressed here are those of the authors and do not necessarily reflect the views of the Inter-American Development Bank.

I Introduction

Over 1 billion households escaped extreme poverty in the last 30 years giving rise to a tenuous middle class (Cuaresma et al., 2018). Compared to low-income households that receive a guaranteed income stream from social protection, and high-income households that can finance consumption through formal financial products, savings or wealth, middle-income households may be uniquely vulnerable to economic shocks because they primarily rely on labor earnings to finance consumption. Despite these distinct challenges and the large share of the global population represented by vulnerable middle-income households (e.g., 37% in Latin America, see Stampini et al. (2021)), little is known about how middle-income households cope with economic shocks and how to build their resilience.

We exploit quasi-random variation in income induced by an expansion in the coverage of social protection to middle-income households to provide evidence about how middle-income households smooth economic shocks. Specifically, we use a regression discontinuity design around the upper threshold of eligibility, determined by a proxy means test, for Ingreso Solidario in Colombia. Ingreso Solidario was implemented as a response to the COVID-19 pandemic and reached over 4 million households. We show that access to social protection builds middle-income households' resilience to economic shocks through both cash transfers and integration into the formal financial market.

There are three key features of the program. First, it expanded the coverage of Colombia's safety net to non-poor households. Specifically, the program had an eligibility cut-off of approximately the 40th percentile of the 2019 per capita income distribution (equivalent to roughly 1.3 times the poverty line or a per-capita daily income of USD $ 7 adjusting for purchasing power).[1] Second, the program delivered over 30 monthly payments of COP 160,000 (the equivalent of USD $121 adjusting for purchasing power) by December 2022, extending almost two years after COVID-19 related mobility restrictions were lifted.[2] Third, beneficiary households were encouraged to open digital bank accounts and the transfers were delivered mainly through direct deposit. These features allow us to analyze how an increase in the coverage of social protection affects the financial constraints of middle-income households and their adjustment strategy in the face of aggregate and idiosyncratic economic shocks. They also enable us to study the extent to which social programs provide a platform to integrate middle-income households into the formal financial system in the longer run.

Our analysis exploits a comprehensive set of administrative and survey data. We combine survey data on income, food spending and food security with administrative records on educational outcomes and health-care use to analyze if the expansion of the safety net relaxes financial constraints related to key necessities. We also exploit survey data on non-food spending and administrative records on the universe of debts with non-financial and financial (formal) firms in Colombia to study responses on other dimensions of financial well-being, and to test the dynamics leading to the integration of middle-income households into the formal financial system.

To begin, we document strong program compliance and increases in income. As of July 2021, marginally eligible households—those just below the program

[1] Throughout the paper, we follow Banerjee and Duflo (2008) and refer to middle-income households as those with daily per-capita income of USD 6 to 10, adjusted for purchasing power parity.

[2] While the program was implemented as response to the COVID-19 pandemic, it continued until December 2022. In contrast, the average duration of COVID-19 pandemic programs was 4.5 months (Gentilini, 2022).

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 program eligibility increased per capita income among marginal beneficiaries by 26%. Consistent with this increase in income, we do not find any negative effects on labor market outcomes.

Next, we estimate the effects of this increase in income averaged across all time periods encompassing various economic environments. Pooling data on all available post-program periods, we document null effects on food spending, an index of food security, educational outcomes such as attendance, grade retention, test scores, and use of medical services, which contrasts with the typically positive effects of cash transfers targeted to low-income households for these outcomes (Bastagli et al., 2019).

Considering that middle-income households concentrate spending in non-food categories, we explore non-food spending and short-term debt to non-financial firms.[3] In survey data, we find 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 (p-value=0.10). Using administrative records, we find a statistically significant one-percentage point decline in the probability of having past-due debt with utility and retail firms (i.e., non-financial firms).

Then, we exploit the diverse economic environments in which the program operated to provide evidence on how middle-income households cope with economic shocks in the absence of social protection both during an economic downturn and when exposed to idiosyncratic shocks during an economic re-

[3]Survey data reveals that 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.

bound. The null effects on food spending, school dropout, test scores, and health care are constant over time. The positive impacts of the program on non-food spending and debt with utilities and retail firms peak during the economic downturn, persist for one year, and dissipate by December 2022. Together, these results indicate that in the absence of cash transfers, middle-income households smooth their consumption of food, education, and health during an economic downturn by decreasing non-food spending and accumulating debt for routine expenses. Further, these results imply that once middle-income households acquire debt, they require substantial time to pay it off.

We focus on the period of economic recovery to analyze how middle-income households respond to a large idiosyncratic economic shock, the death of a household member. Similar to their response to aggregate economic shocks, in the absence of social protection, middle-income households smooth idiosyncratic shocks by reducing non-food spending. Specifically, per capita income and non-food spending decline for households that experience the shock by 22% and 15%, but these declines are fully offset for eligible households. This indicates that, in contexts with incomplete insurance, access to social protection has especially large impacts for middle-income households that experience an economic shock.

Finally, we investigate whether expanding the safety net to middle-income households integrated beneficiaries into the formal financial system. Using credit bureau data, we find that eligibility for the program increased bank account ownership by 10 percentage points, which represents a 16% increase relative to marginally ineligible beneficiaries. Despite this increase in bank account ownership, program eligibility had no effect on medium- to long-term savings (admin records) or on overall savings (survey data), which is consistent with previous studies (Dupas et al., 2018; de Mel et al., 2022). However, to the extent that debt with utility companies must be paid eventually, paying down debt is similar to increasing net financial assets.

The expansion of the safety net to middle-income households also increased the usage of digital accounts, 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. These effects are driven by households that did not own an active bank account before the program.

The positive effects on bank accounts are immediate and persist over 30 months after the first program payment. In contrast, the program's positive impact on credit takes almost a year to manifest, likely reflecting the time required to build a relationship with the lender and credit-worthy profile. This integration of middle-income households into the formal financial markets built resilience against idiosyncratic shocks. Program eligibility increased the likelihood of owning a formal loan for households that experienced an idiosyncratic shock, and this increase in formal credit mirrors a decline in the likelihood of holding high-interest predatory loans.

Our results contribute to our understanding of the financial constraints that middle-income households face. Banerjee and Duflo (2008) provide descriptive evidence on the spending, investment and occupational patterns of the middle-class. Using quasi-random variation in income across distinct economic environments, our results demonstrate that middle-income households are vulnerable to economic shocks and reveal how expanding the safety net relieves key financial constraints. Our results imply that, when employment and income declined, middle-income households without access to social protection reduced their non-food spending and accumulated debt for routine expenses. After employment and income recovered, middle-income households required more time to fully repay their debt and regain their financial footing because they have little savings or wealth to draw down. Further, middle-income households take advantage of their access to the formal financial market when they suffer an idiosyncratic shock. These adjustments are similar to those observed among individuals in wealthier countries like the US (Mello, 2021). They also indicate that middle-income households are constrained by lack of insurance.

Our results also contribute to the literature on how to deepen financial inclusion. Previous evidence shows that debit cards can relax savings constraints among low-income cash transfer recipients (Bachas et al., 2021). Beyond savings, our results show that social protection programs that induce the use of bank accounts can relax *credit* constraints among middle-income households. These results suggest that because middle-income households obtain income from less risky sources and because the extra income from the program enables them to make on-time recurrent payments, increasing access to credit for middle-income households may not entail the same costly, lengthy mechanisms traditionally used to bank lower-income households such as microfinance (Agarwal et al., 2021; Hoffmann et al., 2021). They also suggest that the long-term legacy of expanding the safety net to middle-income households may be a more-resilient middle-class that can rely on credit markets to smooth consumption during economic shocks.

Our results have policy implications for the design of social protection programs. Our evidence on the effects of providing a guaranteed income stream to middle-income households contributes to the discussion around Universal Basic Income programs (UBI). Because of the high fiscal costs of UBI programs, coverage of basic income programs may expand incrementally. Recent studies have analyzed the effects of 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.

In addition, our results documenting the vulnerability of middle-income households to economic shocks has implications for anti-poverty programs. While many studies have documented the success of programs that aim to "graduate" households out of poverty (Banerjee et al., 2015, 2021; Balboni et al., 2021; Blattman et al., 2014),[4] poverty is a two way street. Preventing households from sliding into poverty may be more cost-effective than helping them climb out.

II Context

II.A Economic Context

On March 24 2020, the Colombian Government officially declared a national health emergency and quickly implemented lockdown measures to contain the spread of COVID-19. The government simultaneously implemented policies to provide economic support to households whose livelihoods were disrupted by the lockdown measures. The lockdown was suspended on August 30, 2020, and children returned to in-person learning progressively starting in June 2021.

[4] The effects of cash transfer programs targeted to low-income households are well documented. For example, see Bastagli et al. (2019) and for a recent review and Londoño-Vélez and Querubín (2022) and Attanasio et al. (2021) for evidence of long-term impacts in Colombia and Ecuador. Likewise, Cañedo et al. (2023) and Bird et al. (2023) provide evidence on one-time cash transfers to low-income households in Mexico and Peru during the COVID-19 pandemic.

Figure A1 displays employment and income trends using data from ineligible middle-income households that were located just above the threshold of eligibility for Ingreso Solidario based on the proxy-means test. In panel a), we observe a sharp decline in employment. In February 2020, before the lockdown, roughly 62% of individuals of working age reported working. By June 2020, only 46% reported working. This decline in employment persisted through September 2020, just after the mobility restrictions were lifted. One year later, in October 2021, the employment rate bounced back to 55%, which was still below its initial levels. In Panel b), per capita income follows a similar pattern, with a sharp decline from February 2020 to June 2020 and a nearly complete recovery one year later.

II.B The Ingreso Solidario Program

Program Features. In this context, Colombia launched Ingreso Solidario in April 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.[5] 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; roughly USD 1 per-capita per day, for a family of 4). The transfer represents 18% of the monthly minimum wage in force at that time 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.

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

Three unique features of the program are key for this study. First, because the program had a higher eligibility threshold than pre-existing social programs, it 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. For example, Familias en Acción, Colombia's conditional cash transfer program only included households that were classified as poor, with a much lower eligibility threshold.[6] Second, program eligibility did not depend on specific demographic characteristics, and the program targeted households that were not covered by pre-existing social programs.[7] 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 of residence.[8] This ratio is then used to classify households into four broad

[6] See Attanasio et al. (2021) for a long-term evaluation of the CCT program

[7] 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—and comply with program conditions in order to access them.

[8] 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).

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 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 (1.3 times the national poverty line and roughly equivalent to the 42nd percentile of the 2019 per capita income distribution).[9]

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 2020, 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.[10] 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.[11] After including this group, as of June 2020, a total of

[9] 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.

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

[11] 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.[12]

III Study Sample and Data

III.A 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 dropped 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

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

[12] In addition, starting April 2022, the coverage of the program was expanded, reaching 4,850,000 households, but without changing the eligibility threshold. Finally, the program was then discontinued in January 2023, because of a reform that consolidated all social programs in Colombia into one new program, Renta Ciudadana, which is still in design. Our analysis includes only households who received a payment during the initial three stages.

closest eligibility threshold of the preexisting social programs.[13]

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-income households with predicted per capita income above the thresholds of eligibility for social programs targeted to poor households.

III.B Characterizing The Middle-income Households In Our Sample

The households that we study are within a narrow bandwidth of program eligibility, and are classified as vulnerable non-poor by the social registry. They are all above the poverty line, based on a pre-program proxy-means test. Appendix Table A1 presents summary statistics corresponding to households in the social registry using administrative records (column 1), and households from different income terciles included in the 2019 wave of Colombia's nationally representative household survey (Gran Encuesta de Hogares, GEIH) (columns 3 to 5). It shows that, in fact, they exhibit similar pre-program incomes to the households located in the middle-third of the per-capita income distribution in Colombia. In this regard, they are middle-income households. 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 per day, but still within the range of high vulnerability to falling into poverty

[13]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.

[14]We use the PPP-adjusted exchange rate for 2017 (the year the admin data on income were collected). The data were obtained from: `https://data.worldbank.org/indicator/PA.NUS.PPP?locations=CO`

(The World Bank, 2021). This income level is consistent with Banerjee and Duflo (2008) classification of middle-class in developing countries—i.e., those with daily per-capita income of USD 6 to 10, adjusted for purchasing power parity.

The marginal beneficiaries of the expansion of the safety net, do not only differ from those living in poverty in terms of income. In terms of spending patterns, households located around the program's eligibility cutoff also differ substantially from the poorest households in the social registry. Appendix Figure A2 analyzes spending patterns among all the households in the social registry as a function of their pre-program PPP-adjusted per-capita daily income. Among the poorest households in the social registry, food spending represents roughly 60% of total expenses. Among the households located around the program eligibility cutoff, food spending only represents roughly 46% of total per capita expenses.

Finally, despite their higher incomes, middle-income households are vulnerable to losing their livelihoods and experiencing sharp reductions in income during periods of economic downturn (see Appendix Figure A1). Relative to high-income households, they are less able to rely on the formal financial system to cope with shocks. Before the program, 49% of households in the social registry that were not classified as poor or vulnerable had an outstanding formal loan by December 2019 compared to 43% of households (classified as vulnerable) on the margin of eligibility.

The unique economic position of middle-income households is also 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.[15]

III.C Data.

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 and to conduct balance checks.

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 households' financial situation. The credit bureau data contains information at the individual level on ownership of bank accounts and debt with financial institutions, utility companies and retailers. We access these records for six post-program half years (June 2020 - December 2022) 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.[16]

[15] We elicited household priorities by asking households to list their top concern from a set menu of options. For each household, we randomized the order in which these options were read to the interviewee by the enumerator.

[16] To maintain data privacy, the dataset that we were able to access only included 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 variable). In addition, the dataset was anonymized; 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. As in the case of the PILA records, we are not

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).

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 enrolled in 11th grade in 2019 to 2021, 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 or 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

able to merge RIPS with other administrative records.

products.

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 could not 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. [17]

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 sides of the cutoff). Next, they called the households who were the second closest to the cutoff on both sides, and so on.[18] 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).[19]

One key feature of our data collection process is that it defines the bandwidth that we will use to estimate the impacts of the program. This mini-

[17] Appendix Table A1 presents summary statistics corresponding to households in the social registry. Column 1 focuses on the households in a reduced bandwidth around the cutoff (the two SISBEN IV categories that are closest to the cutoff), and Column 2 focuses on the subset of these households that were interviewed for the follow-up surveys. Both groups are similar in terms of average baseline characteristics. This suggests that survey non-response may be uncorrelated with households' characteristics.

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

[19] 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%.

mizes 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.[20]

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:

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

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

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.[21] ψ_d denotes department-urban/rural 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.[22] 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). We also report sharpened False Discovery Rate (FDR) q-values following Anderson (2008).

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 program 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

[21]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.

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

the the coverage of the safety net to middle-income households that were not poor enough to be included in preexisting social programs but that can be highly vulnerable in times of economic crises (Busso et al., 2021; Bottan et al., 2020b)—i.e., the marginal beneficiaries of an expansion of the safety net.

Interpretation of the Program in the Economic Context. The treatment effects of the program capture the effects of providing a guaranteed income stream to middle-income households. The treatment effects pooled across the study period are policy relevant as they capture the impact of expanding the safety net to middle-income households averaged across various economic environments. However, the stark employment and income trends over the study period suggest that interpreting the effects of the program within the economic context provides additional insights. During the period of severe economic downturn, the treatment effects represent the differential response to the aggregate economic downturn for middle-income households that have a guaranteed income stream relative to those that do not. Therefore, the treatment effects indicate the adjustments that middle-income households would have made to manage the aggregate economic downturn in the absence of the program. During the economic rebound, the treatment effects indicate the impacts of additional, guaranteed income for middle-income households in a period of economic recovery. In our analysis, we first report average treatment effects across all available periods, followed by a discussion on how they vary over time.

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.

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 A3) 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 analyze 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 two of thirteen demographic characteristics from administrative data (Panel A). Specifically, we detect a difference in the age of the household head between eligible and ineligible households of 0.72 years, and a difference in the number of household members of 0.028 members. To make sure that these small differences do not systematically predict program eligibility, we used the variables in Panel A to compute predicted eligibility probabilities, and then tested for discontinuities in predicted eligibility around the cutoff. Reassuringly, we find neither substantial nor significant differences. Moreover, we 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, in Panel B, 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.[23]

[23]It is also worth nothing that only one of the 4 differences remains significant based on the sharpened FDR q-values.

V Effects on Program Take-Up and Household Income

First, based on administrative records on program disbursements, 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). Columns 1 and 2 show 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 and received 13.7 additional program payments up to July 2021 (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 (IHS) transformation to income to accommodate observations with zero income.[24] 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.[25] This increase in income is approximately equivalent to the average per capita transfer amount (COP $ 64,000). Column 4 in Table 1 shows that program eligibility increased the probability of reporting positive income by 4.6 percentage points. Over-

[24] 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.

[25] 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: $\exp(\hat{\beta}) - 1$.

all, 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 equivalent to the amount of the transfer suggests that there was relatively little crowding out of other income sources. 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.[26] 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.[27]

VI Effects on Downstream Outcomes

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, grade retention, and access

[26] We are able to rule out declines in employment as small as 3.8 percentage points at a 95% confidence level.

[27] 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)

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, consequently, avoiding costly penalties due to past-due payments in routine expenses.

As discussed in Section II, we report both average treatment effects, pooling all the available post-program periods, and dynamic treatment effects for key outcomes for which we have consistent measures at different points in time because what we learn from the treatment effects depends on the economic environment.

VI.A Effects on Food Security, School Attendance, and Health Care

We begin by analyzing the 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.[28] Figure 2a shows that there are no impacts on food security. This implies that middle-income households had enough resources to ensure reliable access to food. However, the program may have improved the quality of the food that households consume. To test this hypothesis, we analyze the effects on overall food spending. 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.[29] Columns 1 and 2 of table 2 corroborate the graphical evidence. The point

[28]Specifically, we collected information about the incidence and frequency of seven 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.

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

estimates are small and insignificant. These results are consistent with food security not being among the top stated priorities of middle-income households in our sample (see Section III.B).

We next analyze the impacts of the program on outcomes that are usually the key outcomes of programs targeted at the poor. Focusing on school age individuals, Figure2c uses administrative records for 2020 and 2021 to show that there is no discontinuity in school dropout around the cutoff of program eligibility. 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 students enrolled in 2021.[30] 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 and September 2021 (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 Figure 2d). The point estimates are small (0.001 of a standard deviation, see column 6 in Table 2).[31]

Next, we use administrative records to analyze the impacts of the program on the use of health care. One empirical challenge of working with health-

[30]We focus only on enrollment in 2021 as enrollment in 2020 is predetermined, with respect to the program. Likewise, we do not examine grade repetition for students enrolled in 2020, as it would capture pre-program behavior, and unfortunately, we do not have access to similar information for students enrolled in 2022. Therefore, our data only enable us to draw conclusions about grade repetition during a period of mostly virtual learning.

[31]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.

care use data is that usage of health care is a function of both a household's incidence of diseases and health-seeking behavior. We exploit the context of COVID-19 and detailed administrative data to measure program impacts along these two dimensions.

First, we analyze the effects of the program on the use of health care services related to severe episodes of COVID-19 (e.g., chest X-ray exams, visits to the emergency room or hospitalization) because they are likely to reflect the incidence of illness, not discretionary health care. Because these episodes occur with low frequency, we focus on cumulative outcomes between April 2020 and June 2021. Figure 2e shows no large discontinuity in use of health services related to severe COVID-19 cases. 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 no impacts on the probability of the death of a household member due to COVID-19 (column 9).

Second, we analyze the effects of the program on the use of health care services that are more likely to reflect changes in demand, as opposed to exposure. For this, we focus on the probability of receiving COVID-19 vaccines using survey data and on the probability of attending appointments or receiving procedures related to depression and anxiety using administrative data.[32] We find positive but insignificant effects on vaccination against COVID-19 using data from the second survey round (column 8). Turning to mental health care, figure 2f shows that there is no discontinuity in the probability of seeking

[32]To ensure we measure changes in demand, we exclude episodes related to visits to the emergency room or hospitalizations. Because visits to the doctor related to mental health issues are relatively infrequent, we focus on cumulative outcomes between April 2020 and June 2021.

mental health care around the eligibility threshold using administrative data for the 18 months following the implementation of the program. 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 mental health care.

Put together, the results suggest that the decisions of middle-income households related to food, education and health care use, on average, do not appear constrained by lack of income. In the absence of the program, they would have been able to maintain their food intake, send their children to school, and protect their family from infectious diseases.

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=0.102). 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-half of the per capita transfer amount.[33] Further, the 95% confidence interval rejects relatively small declines in non-food per capita spending (-3.4%) and

[33]The average household size in the household survey was 4, which yields a per-capita transfer amount of \$ CPO 40,000. The average per-capita nonfood spending amount for the control group was \$ CPO 217,000, so the treatment effect represents an increase of \$ CPO 26,000 in per-capita nonfood spending.

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 to December 2022), 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 percentage point (p-value=0.10) and past-due debt by 0.9 percentage points (a 3.4% decline relative to marginally ineligible households, p-value<0.05). Graphical evidence suggests that program eligibility slightly decreased past-due 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 using data pooled across time periods suggest that the households we study were able to cover their basic needs even in the absence of the program. In contrast, the program enabled these middle-income households to expand other spending and avoid the penalties and costs of past-due debt with utility companies. The fact that the average treatment effects reveal patterns that differ from those documented in the literature on social programs targeted to the poor also suggests that the marginal beneficiary of programs with broader coverage may respond differently to income support programs than the typical beneficiary of anti-poverty programs.[34]

[34]For example, increases in food consumption, school attendance, and health care use are often found in other cash transfer programs targeted at the poorest households (Bastagli et al., 2019). Likewise, 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

VI.C Coping with Aggregate and Idiosyncratic Shocks

In this section, we exploit the various economic environments during our study period to provide insights into how middle-income households cope with aggregate economic shocks and severe idiosyncratic shocks in the absence of social protection. By analyzing dynamic treatment effects and focusing on the effects of the program during an aggregate economic downturn, we can explore to what extent our pooled results reflect adjustments made by middle-income households to a large aggregate economic downturn in the absence of social protection. Similarly, we analyze how middle-income households cope with a large idiosyncratic shock in the absence of the program during a period of economic recovery, 18 months after the program implemented.

Figure 4 shows the effects of the program over time. Figure 4a 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 large during the height of economic downturn.[35] Eighteen months after the implementation of the program, the impacts of the program appear to have faded, likely due to a rebound in the incomes of non-eligible households (see Appendix Figure A1). Thus, one may think of the program operating as a income-protection program during the recession, and an income-supplement program during the rebound period.

Figure 4b to f shows that the lack of effects on food spending, school drop outs, test scores, and health care was consistent over time. The results

households Londoño-Vélez and Querubín (2022).

[35]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.

suggest that during the economic downturn, middle-income households were able to smooth out the systemic shock along these key dimensions even in the absence of the program. Reassuringly, we do not find substantial or significant differences between eligible and ineligible households before the program was implemented for any of these outcomes.

Next, we investigate whether ineligible households achieved a relatively smooth trajectory of food spending, school attendance, and health care through the economic downturn by making costly adjustments on other dimensions that eligible households were able to avoid. Figure 4g shows that the positive effects of program eligibility on non-food spending were statistically significant in the case of the first round of the survey (September 2020), albeit only at 10%. Reassuringly, we find neither substantial nor significant differences in pre-program non-food spending.[36] In Figure 4h, we observe that program eligibility led to significant declines in the probability of holding debt related to routine expenses, such as utilities, during 2020, which suggests that the program may have prevented beneficiary households from having to acquire debt with utility and retail companies to cover other routine expenses. These results suggest that, without the stream of income guaranteed by the program, middle-income households smooth their consumption of food and basic necessities over a large economic shock by reducing their non-food spending and acquiring debt for routine expenses—two key dimensions of financial wellbeing.

These adjustments could have longer-term negative impacts. Using the first round of survey data, we find that program eligibility increased per capita edu-

[36]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.

cation spending—a key component of non-food spending—and time dedicated to studying (see Appendix Table A6). These effects dissipate by the second survey round.[37] These results suggest that at least during the economic downturn, the program prevented middle-income households from cutting back on key expenses to ensure a minimum level of education quality for children.

The effects on non-food spending and debt with utilities and retail companies persist to the end of 2021—at least 18 months after the first program payment was disbursed and over a year after the mobility restrictions in Colombia were lifted– and die out by December 2022 (see figure 4h). These persistent effects during the economic rebound imply that middle-income households recover from economic shocks slowly; for example, once middle-income households acquire debt to non-financial firms, they require substantial time to fully repay it.

Next, we explore the effects of the program for households that experience idiosyncratic economic shocks that induce unexpected and unavoidable spending. Middle-income households may be vulnerable to poverty not only during recessions, but, more generally, when they experience severe shocks. Indeed, only 45% of households in upper-middle income countries and lower-income countries can come up with resources to cover a shock with little difficulty within 7 days (Demirgüç-Kunt et al., 2022).

To explore the extent to which the program assisted households in coping with large idiosyncratic 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

[37] 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 among test takers in 2020, while null impacts in 2021.

the 12 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 three 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 4 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. Third, we measure exposure to shocks during the rebound period, which enables us to study responses that are not specific to a period of economic crisis.

We estimate the heterogeneous treatment effects of the program on income and spending by exposure to a household member's death using the following specification:

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

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 4. Column 1 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 mirrors the significant decline in income for households that experienced the shock. In column 4, 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 2 and 3 suggest that the heterogeneous effects of total spending are primarily driven by non-food spending, the largest component of total household spending. Similar to the results for systemic economic shocks, these results suggests that middle-income households are able to protect food spending during idiosyncratic shocks and that non-food spending is the relevant margin of adjustment for middle-income households.

The results indicate that middle-income households are constrained by lack of insurance, which implies a role for formal financial products. In this context, the program may have helped households build resilience to idiosyncratic shocks through two channels, a guaranteed stream of income and access to formal credit as a consequence of the program being mostly delivered into bank accounts.

VII Integration into the Formal Financial Market

As with most modern social protection programs, digital payment of transfers was encouraged. However, the program is unique in its reliance on bank accounts with large Colombian private banks which also offer loans. Specifically, 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. Thus, one can interpret the program as a joint treatment: the provision of a monthly stream of income and the encouragement to use bank accounts. In this section, we trace the impacts of the program along the path toward becoming a user of formal financial products in good standing.

In contrast to their high-income peers, middle-income and low-income households may lack access to formal financial markets (Banerjee and Duflo, 2008). However, middle-income households may be better positioned than low-income households to leverage social programs as an entry to the formal financial markets since they are more likely to exhibit higher and verifiable sources of income from formal employment. At the same time, they may not seize this opportunity because they are less experienced with digital or financial products than their high-income peers. Further, if they do seize this opportunity, this inexperience may expose them to predatory lenders or products.

VII.A Effects on Bank Account Ownership, Savings, and Credit

To begin, we show that program eligibility increased bank account ownership. Figure 5a 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 10 percentage points (see column 1 in Table 5), which represents roughly a 14% increase relative to the control group.

Next, we investigate whether the increase in bank account ownership translates into savings. Column 3 in Table 5 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.[38]

Although we do not find effects of program eligibility on current savings, 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, which implies fewer penalties and lower fees. Further, to the extent that past-due debt must be paid off eventually, paying down debt is essentially as if middle-income households were increasing future savings or their net financial assets.

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. Thirty-seven percent of marginally eligible beneficiaries received at least one payment in one of the three simplified digital savings accounts offered by

[38]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.

partnering banks that impose caps on the balance that can be held in such accounts.[39] A more likely explanation is that paying down debt or increasing consumption are higher-return activities than saving for middle-income households.

Although households did not use these digital bank accounts for saving, these 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 usage for conducting transactions (see Figure 5b).[40] Column 2 of Table 5 suggests an increase in digital bank account usage of 0.19 standard deviations. This effect is likely explained by the increase in access to digital bank accounts induced by the delivery of the program combined with an increase in disposable income to conduct transactions. It is also consistent with evidence from other settings that lack of income prevents low-income individuals from using bank accounts (Dupas et al., 2018).

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 main-

[39] In addition, even though the point estimate is imprecisely estimated, it suggests a decline in the probability of holding savings. If the opportunity cost of holding precautionary savings is high, eligible households may choose not to hold savings because the monthly unconditional transfers allow greater consumption smoothing or they may have greater access to credit, reducing the need for precautionary savings.

[40] The index is computed using Anderson (2008)'s approach, based on four 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.

taining 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 5 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 half a percentage point.

Second, we investigate whether the program increased the likelihood that households hold formal loans only in good standing. Figure 5c shows a discontinuous jump in the probability of having outstanding formal loans only in good standing using data from the credit bureau. Similarly, column 6 of Table 5, 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 are maintaining their pre-existing loans in good standing, the increase in credit inquiries suggests that this effect at least partially represents new formal loans, some of which would mechanically be in good standing as they would not have had time to fall into past-due status. 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 5d shows a discontinuous fall in the likelihood of having at least one past-due loan in the credit bureau data.[41] Column 7 of Table 5 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.

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

Appendix Table A7 shows that the program was able to integrate previously unbanked households into the financial system. Ex-ante unbanked households experience effects on active bank account ownership that are four times as large as those for households that owned bank accounts before the program. The effect among already banked households is smaller but still significant, suggesting that the program may also have prevented bank accounts from going dormant. The effects on credit inquiries and ownership of loans in good standing are larger for ex-ante unbanked households. Past-due debt declines for both groups, though the effect is only significant among previously unbanked households. This suggest that the program reduced past-due debt by enabling credit-worthy unbanked households to access formal loans, as opposed to simply providing already banked households with extra liquidity to pay their debts.

VII.B Building Resilience Through Access to Formal Credit

Obtaining a formal loan is a complex process that benefits from the development of a sustained relationship between bank-account owners and the financial institution. Figure 6a shows that the effects of the program on the probability of holding an active bank account persist even after 30 months from the first program payment and over two years after the mobility restrictions were lifted.[42]

Figure 6b shows that the positive impacts of the program on the ownership

[42]The figure does suggest a mild reduction in the point estimates, but this is likely a consequence of ineligible households catching up with the adoption of bank accounts. Indeed, the share of ineligible households within a narrow bandwidth from the cutoff point (0.25 of the extreme poverty line) with registries of active bank accounts in the credit bureau increased from 57% in December 2019 to 76% three years later.

of loans in good standing took some time to manifest. They appear larger during 2021, almost a year after the program was implemented. Although the slower adjustment of loans may be partially due to a decline in the supply of credit amid the overall economic downturn in 2020, it may also reflect the time required to build a credit-worthy profile and a relationship with the lender. The delay in the positive impacts of the program on formal loans sheds light on the mechanisms behind the dynamic effects of the program discussed in Section VI.C. The effects of the program on spending in 2020 — a period of severe economic downturn—do not appear to reflect differences in credit across eligible and ineligible households.

Although the increase in loan ownership during the economic rebound of 2021 and onward is relatively small, it suggests that the program integrated beneficiaries into the formal financial system and increased their access to alternative financing options. Even if most households did not take advantage of these financing options, integration into the formal financial market may be an important source of resilience against future shocks. Therefore, the effects on loan ownership may be much larger when households face idiosyncratic shocks such as those studied in Section **??**.

To explore the extent to which the program assisted households in coping with large economic shocks through financial markets, we estimate the heterogeneous effects of program eligibility on loan ownership by exposure to an idiosyncratic shock. Specifically, we follow a similar strategy as in section **??** and use the death of a household member in the 12 months preceding the second survey round (November 2020 - October 2021) as an idiosyncratic shock.

Specifically, we estimate equation 2 and display the results in Table 6. Columns 1 to 4 report heterogeneous treatment effects of the program on different types of risk-coping strategies. 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 simple financial products like mobile money enable households to smooth consumption through risk-sharing networks in Kenya and Tanzania (Jack and Suri, 2014; 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 to a novel mechanism. Column 4 shows that program eligibility increased the probability that at least one household member obtained a formal loan during the year preceding the second survey wave for 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 3), 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. These effects are consistent with the idea that middle-income households are vulnerable to shocks and that they are constrained by lack of insurance, as we discussed in Section **??**.

VIII Robustness

Main Estimates. Our results are robust to alternative specifications. In the case of outcomes measured using administrative records, Appendix Figure A4 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 using MSE-optimal bandwidths for each outcomes and a quadratic polynomial. 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 A5 reports estimates for our main outcomes 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.

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 A8 to A11. Reassuringly, none of our results are driven by the inclusion of controls in the regressions.

Finally, starting May 2021 the City of Bogotá, Colombia's largest city, changed the eligibility criterion for their basic income program which also delivered monthly transfers, albeit of a lower amount. The new eligibility threshold coincides with that of Ingreso Solidario, though based on an updated version of the social registry. Thus, in the case of households in Bogota (roughly 28% of the households in our sample), the results of our analysis during the rebound period do not capture the effect of only Ingreso Solidario. Instead, they capture a reduced form estimate of the effect of access to a monthly stream of income from both programs. Appendix Table A12 reports estimates of our treatment effects using pooled data across periods excluding observations from Bogotá using administrative records. Reassuringly, the results are robust to excluding these observations.

Consumption Smoothing. Our results on consumption smoothing are robust to alternative specifications. First, Appendix Table A14 shows that the consumption smoothing results are robust to using linear and cubic polynomials instead of quadratic polynomials. Second, to overcome potential concerns that households that experienced the death of a household member in 2021 were structurally different than those that did not, we control for household unobserved time-invariant characteristics. In the spirit of Gertler and Gruber (2002), we use changes in income and consumption between the two survey rounds (i.e. in 2020 and 2021) to estimate a version of equation (2). Our sample size is smaller due to survey attrition, but the results in Appendix Table A13 are remarkably similar to those using our preferred specification.[43]

[43] Finally, we show that the results are robust to alternative definitions of the shock. Panel A of Table A15 shows that the results are robust to excluding deaths related to COVID-19, and Panel B reports results based on 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.

IX Conclusion

We study an expansion in the coverage of social protection to non-poor households in Colombia to provide novel evidence about how middle-income households cope with economic shocks, how social protection can be a platform to integrate middle-income households into the formal financial system, and how social protection builds middle-income households' resilience. In the absence of social protection, middle-income households that experience economic shocks smooth consumption of food, education, and health by decreasing non-food spending and accumulating debt for routine expenses. Among households that experienced severe idiosyncratic shocks, access to social protection fully offset the decline in non-food spending.

We provide evidence that social protection programs that encourage beneficiaries to open bank accounts and use them to receive their transfer can integrate middle-income households into the formal financial system. We find that access to social protection increased ownership of bank accounts, use of these accounts, credit inquiries (i.e., loan requests), and formal loans in good standing. Further, access to social protection helped households substitute from predatory loans to formal credit when they experienced an economic shock.

Our results indicate that middle-income households are constrained by lack of insurance and point to two mechanisms through which social protection can build middle-income households' resilience to economic shocks. First, programs that deliver a guaranteed income stream to middle-income households can function as insurance against shocks. Second, cash transfers delivered through financial technologies can integrate middle-income households into the formal financial system, which improves households' ability to cope with economic shocks.

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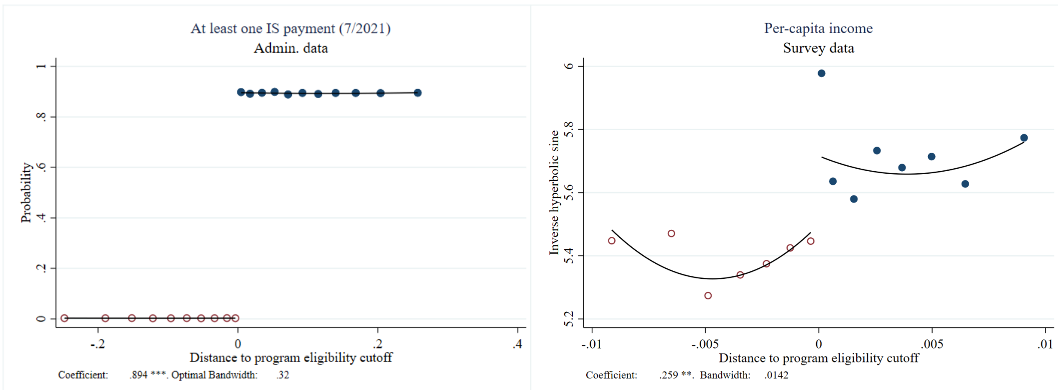
Figures and Tables



(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. Panel 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

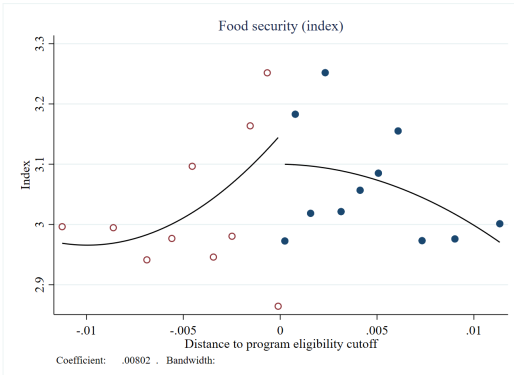
	(1) Transfer reception	(2) # of payments	(3) Per-cap income (IHS)	(4) Income>0
Eligible	0.905***	13.70***	0.246**	0.044***
	(0.002)	(0.036)	(0.114)	(0.017)
P-value	0.000	0.000	0.031	0.009
Q-value	0.001	0.001	0.013	0.007
Control mean (DV)	0.00272	0.0324	5.418	0.914
Bandwidth	0.297	0.273	0.014	0.014
Obs. (in bandwidth)	415762	382259	10144	10144
# of households (in bandwidth)	415762	382259	4900	4900
Adjusted R2	0.820	0.780	0.089	0.045
Data Source	IS records	IS records	Survey R1-R2	Survey R1-R2

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. Sharpened False Discovery Rate (FDR) q-values are computed following Anderson (2008).

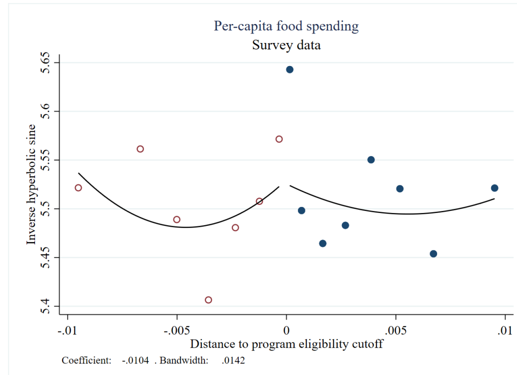
Table 2: Effects on Food Security, Education and Health

	(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-19	At least one Covid vaccine dose	Death Covid-19	Mental Health
Eligible	0.00802	-0.0104	-0.001	-0.007	-0.002	0.004	-0.001	0.0333	0.0145	0.001
	(0.120)	(0.0704)	(0.002)	(0.007)	(0.003)	(0.036)	(0.002)	(0.0415)	(0.0123)	(0.002)
P-value	0.947	0.883	0.565	0.329	0.543	0.908	0.651	0.512	0.237	0.71
Q-value	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Control mean (DV)	3.007	5.517	0.019	0.772	0.028	-0.030	0.036	0.823	0.021	0.040
Bandwidth	0.0137	0.0137	0.250	0.222	0.221	0.307	0.288	0.014	0.014	0.383
Obs. (in bandwidth)	3432	6611	313780	139197	138101	21299	353493	3502	3463	422741
# of households (in bandwidth)	3432	4816	119687	106227	105394	20564	353493	3502	3463	422741
Adjusted R2	0.0470	0.0358	0.005	0.027	0.005	0.107	0.005	0.058	0.011	0.006
Data Source	Survey R2	Survey R1-R2	SIMAT (2020-2021)	SIMAT 2021	SIMAT 2021	SABER 11	RIPS	Survey R2	Survey R2	RIPS

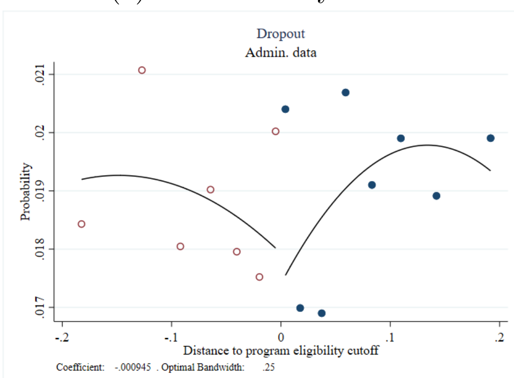
Notes: The table reports estimates of the reduced-form impact of the program using equation (1). Columns 1 and 8 and 9 report results using data only available in the second survey wave. Column 2 uses survey data pooled across two survey waves. We use all the available observations in the household surveys 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 to 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.Sharpened False Discovery Rate (FDR) q-values are computed following Anderson (2008).



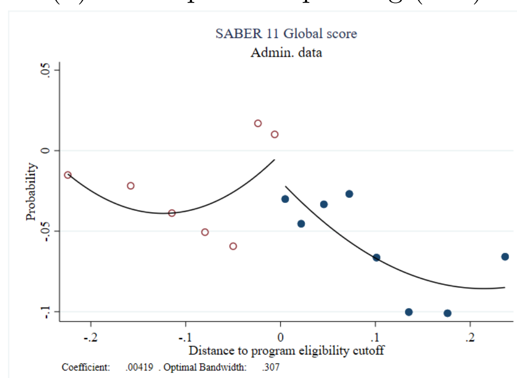
(a) Food Security Index



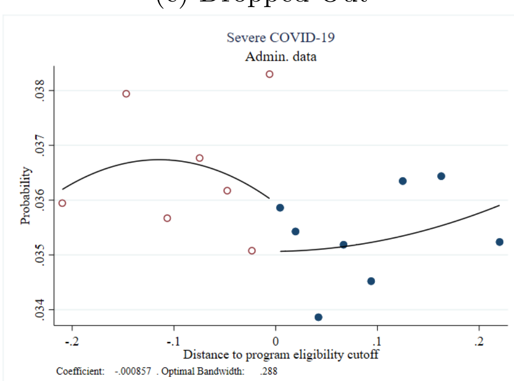
(b) Per Cap Food Spending (IHS)



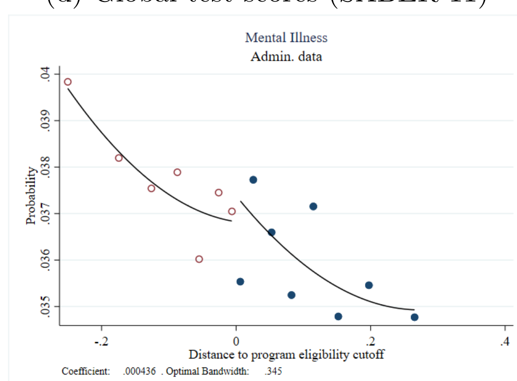
(c) Dropped Out



(d) Global test scores (SABER 11)



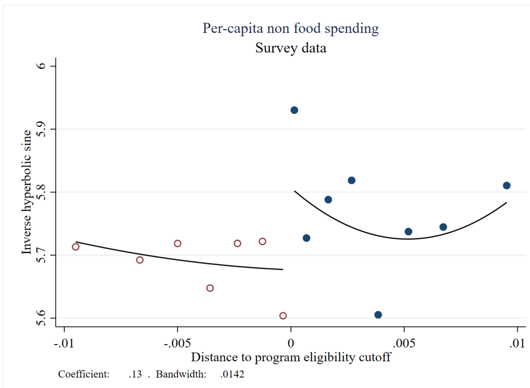
(e) Severe Covid



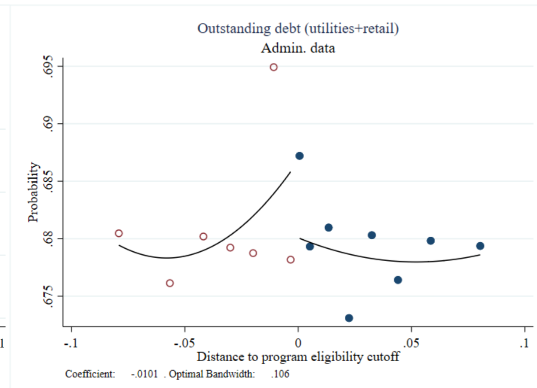
(f) Mental Illness

Figure 2: Effect on Food Security, Education and Health

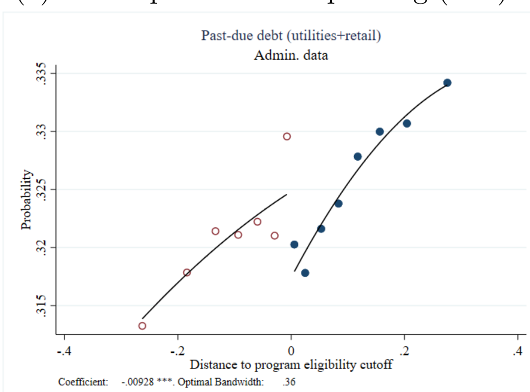
Note: The figure reports means by quantiles of the dependent variable around the cutoff determining program eligibility, and quadratic fits on each side of the cutoff using triangular kernels. Panels a) and b) uses survey data, and a bandwidth pre-defined by data collection (see notes to Table 2 for 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.



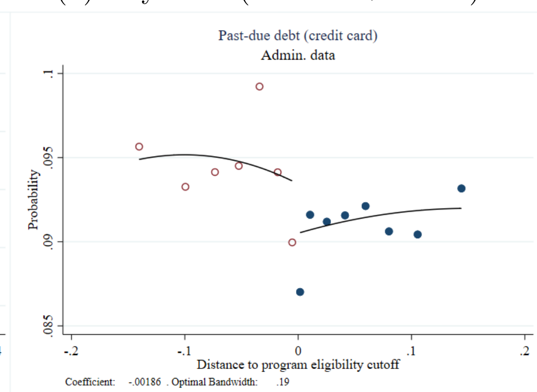
(a) Per Cap Non-Food Spending (IHS)



(b) Any Debt (Utilities + Retail)



(c) Past-Due Debt (Utilities+Retail)



(d) Past-Due Debt (Credit Card)

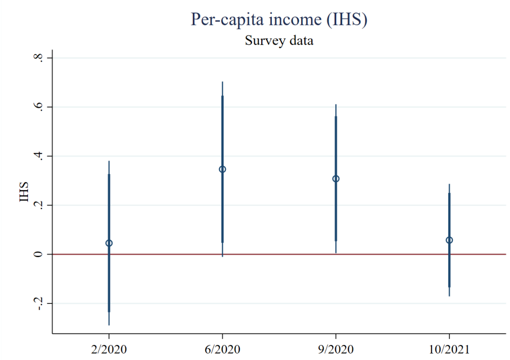
Figure 3: Effects on Non-Food Spending and Short-Term Consumption Debt

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.

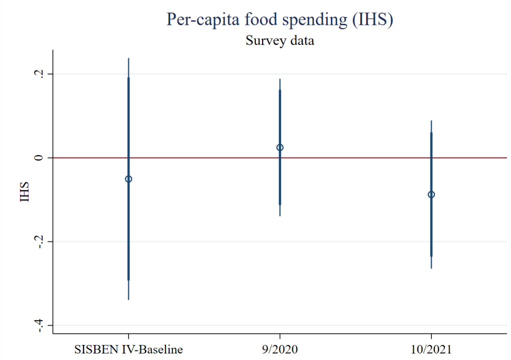
Table 3: Effects on Non-Food Spending and Short-Term Consumption Debt

	(1)	(2)	(3)	(4)	(5)
	Non-Food	Debt (utilities+retail)		Credit cards	
	Spending	Any debt	Past-due debt	Balance >0	Past-due debt
Eligible	0.130	-0.010	-0.009***	0.003	-0.002
	(0.0794)	(0.006)	(0.004)	(0.005)	(0.003)
P-value	0.101	0.100	0.009	0.500	0.517
Q-value	0.16	0.16	0.05	0.26	0.26
Control mean (DV)	5.710	0.682	0.318	0.276	0.095
Bandwidth	0.0137	0.106	0.360	0.159	0.190
Obs. (in bandwidth)	6918	893400	3031776	1335114	1595088
# of households (in bandwidth)	4978	148900	505296	222519	265848
Adjusted R2	0.136	0.156	0.077	0.152	0.048
Data Source	Survey R1-R2	Credit bureau	Credit bureau	Credit bureau	Credit bureau

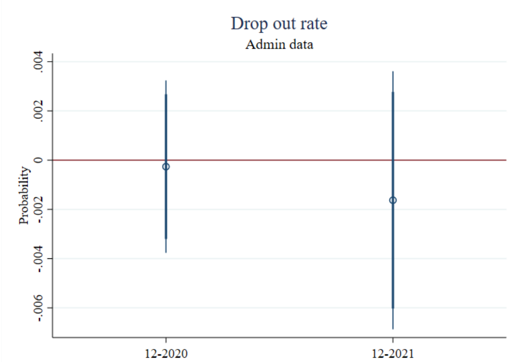
Notes: The table reports estimates of the reduced-form impact of the program using equation (1). Column 1 uses survey data pooled across two survey waves. We use all the available observations in the household surveys in the pre-defined bandwidth. The remaining results are obtained using administrative 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. Sharpened False Discovery Rate (FDR) q-values are computed following Anderson (2008).



(a) Per Capita Income (IHS)

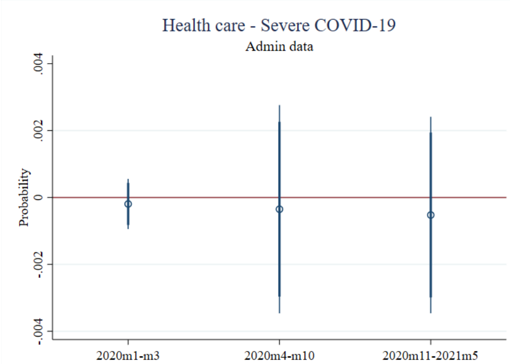


(b) Per Capita Food Spending (IHS)

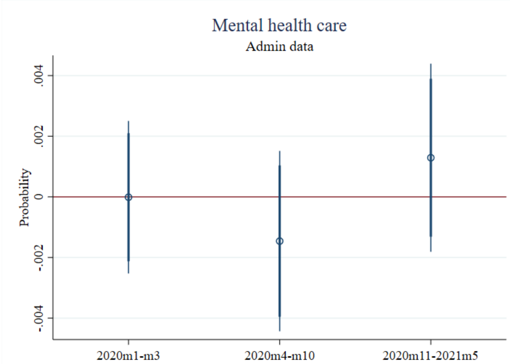


(c) Drop outs

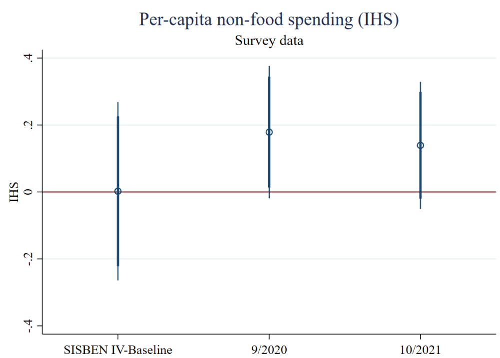
(d) Global test score (SABER11)



(e) Severe COVID-19



(f) Mental health care



(g) Per Capita Non-Food Spending (IHS)

(h) Any Debt (Utilities + Retail)

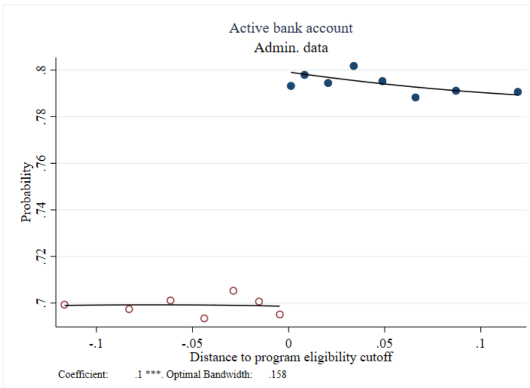
Figure 4: Effects of the Program Over Time

Note: The figure reports treatment effects estimated using the equation 1 at different points in time. Panels a), b) and g) use survey data. The rest of the panels use administrative records 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. 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 5 for more detail.

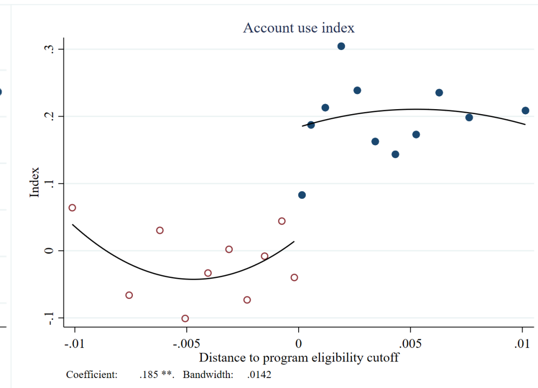
Table 4: Effects by Exposure to a Severe Shock

	(1)	(2)	(3)	(4)	(5)
	Death	Per-capita	Per-capita spending (IHS)		
		Income (IHS)	Food	Non food	Total
Death of a household member		-0.22*	-0.12	-0.15	-0.20*
		(0.12)	(0.12)	(0.10)	(0.11)
Eligible	0.01	0.04	-0.10	0.11	0.01
	(0.03)	(0.12)	(0.11)	(0.10)	(0.10)
Eligible X		0.21	0.14	0.29**	0.30**
Death of household member		(0.15)	(0.16)	(0.14)	(0.14)
Effect (Death=1)		0.25	0.04	0.40	0.31
P-value (Death=1)		0.15	0.81	0.01	0.05
P-value (interaction)		0.16	0.36	0.04	0.03
Q-value (interaction)		0.12	0.22	0.087	0.087
Control mean (DV)	0.07	5.92	5.56	5.70	6.43
Bandwidth	0.01	0.01	0.01	0.01	0.01
Obs. (in bandwidth)	3462	3393	3293	3462	3462.00
# of households (in bandwidth)	3462	3393	3293	3462	3462
Adjusted R2	0.02	0.11	0.04	0.17	0.10
Data Source	Survey R2	Survey R2	Survey R2	Survey R2	Survey R2

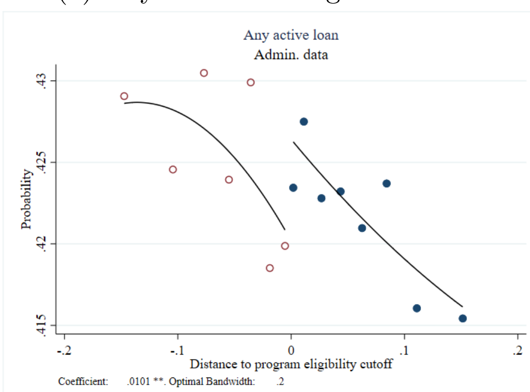
Notes: Column (1) reports estimates of the impact of the program on the death of a household member during 2021, using data from the second survey round. Columns (2) to (5) report results corresponding to the specification in equation (2) using quadratic polynomials. We applied the inverse hyperbolic sine transformation to income and spending. We use data corresponding to the second survey wave and use all the available observations in that round. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels. Sharpened False Discovery Rate (FDR) q-values corresponding to the coefficient of "Eligible X Death of household member" are computed following Anderson (2008).



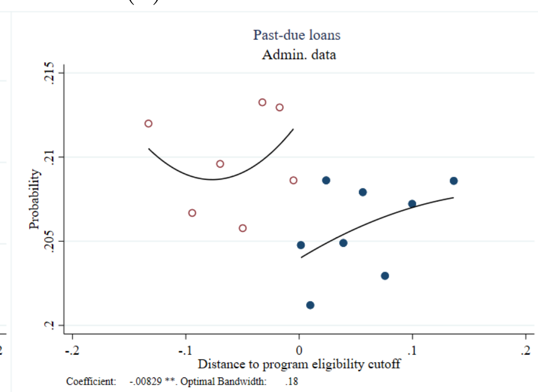
(a) Any Active Savings Account



(b) Account Use Index



(c) Formal Loans Only in Good Standing



(d) Past-Due Debt (Formal Loans)

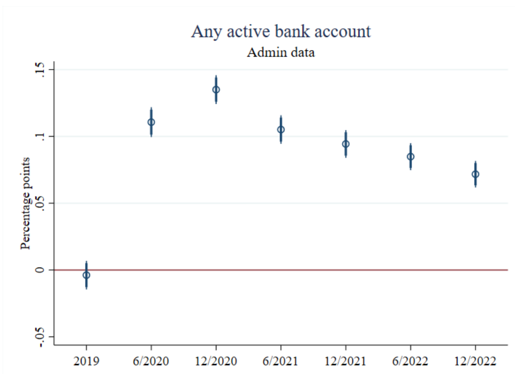
Figure 5: Effects on Saving Accounts and Formal Credit

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 5 for more details.

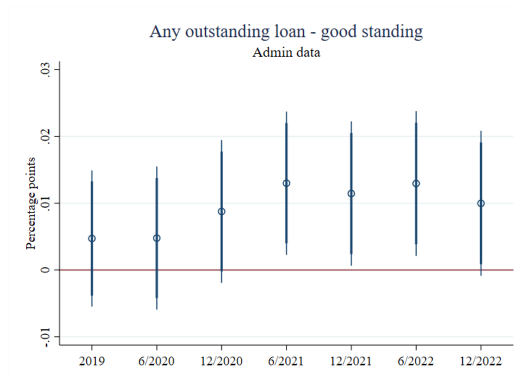
Table 5: Effects on Savings and Formal Credit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Savings					**Credit**	
	Has savings account	Mobile account Usage Index	Fixed term deposits	Has any savings	Credit Inquiry	Any outstanding loan	Any past-due loan
Eligible	0.100***	0.190**	-0.000	-0.0574	0.005***	0.010**	-0.008**
	(0.004)	(0.0766)	(0.002)	(0.0369)	(0.001)	(0.005)	(0.004)
P-value	0.000	0.0133	0.956	0.12	0.000	0.030	0.049
Q-value	0.00	0.02	0.16	0.06	0.00	0.03	0.04
Control mean (DV)	0.701	-0.000361	0.042	0.132	0.032	0.429	0.211
Bandwidth	0.158	0.0137	0.155	0.0137	0.222	0.200	0.180
Obs. (in bandwidth)	1323882	6918	1304946	3463	1860036	1675440	1509990
# of households (in bandwidth)	220647	4978	217491	3463	310006	279240	251665
Adjusted R2	0.172	0.187	0.021	0.0605	0.011	0.161	0.074
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. Sharpened False Discovery Rate (FDR) q-values are computed following Anderson (2008).



(a) Any Bank Account



(b) Any Active Loan

Figure 6: Effects of the Program Over Time

Note: The figure reports treatment effects estimated using the equation 1 at different points in time. Panels a) and b) 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 5 for more detail.

Table 6: Effects on Incoming Transfers and Borrowing by Exposure to a Severe Shock

	(1)	(2)	(3)	(4)
	Received		Took new loans	
	transfers	Informal	Formal	Gota a gota
Death of a household member	0.03	0.00	-0.04	-0.00
	(0.04)	(0.02)	(0.03)	(0.01)
Eligible	0.01	-0.02	0.03	-0.00
	(0.04)	(0.02)	(0.04)	(0.02)
Eligible X Death of household member	-0.05	0.00	0.09*	-0.02**
	(0.06)	(0.03)	(0.05)	(0.01)
Effect (Death=1)	-0.03	-0.01	0.12	-0.02
P-value (Death=1)	0.62	0.68	0.05	0.16
P-value (interaction)	0.43	0.96	0.07	0.03
Q-value (interaction)	0.40	0.92	0.14	0.14
Control mean (DV)	0.13	0.04	0.13	0.01
Bandwidth	0.01	0.01	0.01	0.01
Obs. (in bandwidth)	3457	3462	3462	3462
# of households (in bandwidth)	3457	3462	3462	3462
Adjusted R2	0.02	0.00	0.04	0.01
Data Source	Survey R2	Survey R2	Survey R2	Survey R2

Notes: Columns (1) to (4) 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 that round. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels. Sharpened False Discovery Rate (FDR) q-values corresponding to the coefficient of "Eligible X Death of household member" are computed following Anderson (2008).

Online Appendix

A Supporting Tables and Figures

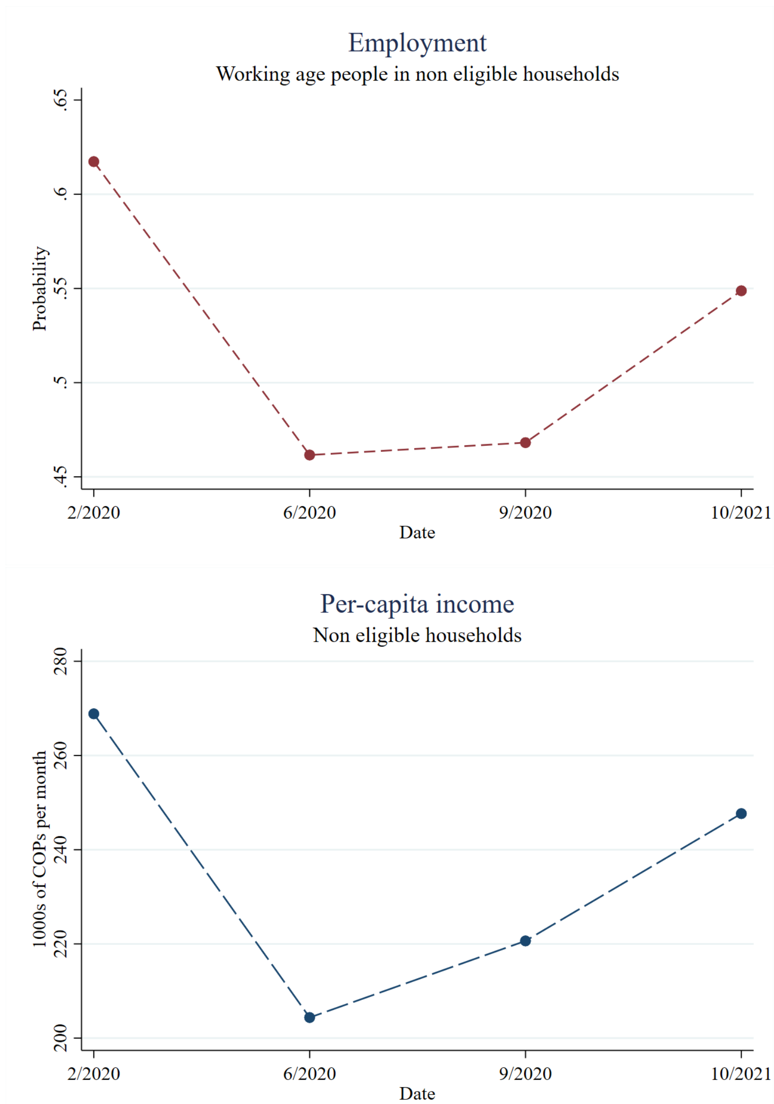


Figure A1: Employment and Income Time Trends (Non Eligible Households)

Note: The figure plots means of per-capita income across ineligible households and employment rates among individual in working age who are part of ineligible households. Data corresponding to 2020 was collected during the first round of the survey. Data for 2021 correspond to the second survey wave.

Table A1: Summary Statistics

	Social Registry (SISBEN IV)		GEIH 2019 (by per capita income)		
	Reduced bandwidth	Survey	Bottom	Middle	Top
	(1)	(2)	(3)	(4)	(5)
Age in February 2020	45.61	44.19	47.93	48.54	52.50
Head of household - Woman	0.49	0.41	0.37	0.35	0.39
No formal education	0.04	0.04	0.06	0.06	0.03
Primary education	0.48	0.45	0.51	0.47	0.32
Secondary education	0.34	0.35	0.25	0.32	0.24
Tertiary education	0.14	0.16	0.19	0.15	0.41
Number of household members	2.51	4.09	3.22	3.89	2.69
Urban area	0.81	0.80	0.80	0.81	0.94
Per-capita income (1000s of COP)	285.10	294.91	25.66	319.31	1405.37
Observations	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 within the two SISBEN IV categories that are closest to the program eligibility cutoff (C5 and C6), in column 1. Column 2 uses survey data corresponding to the first survey round. Columns (3) to (5) report summary statistics using survey data from the nationally representative 2019 GEIH household survey, by terciles of per capita household income.

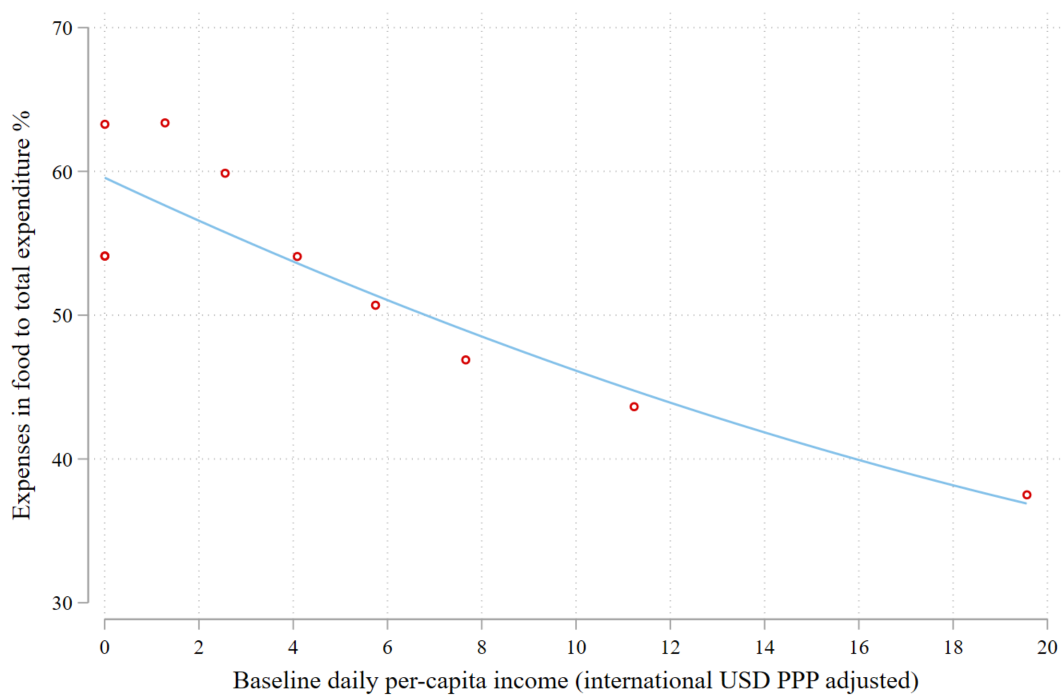
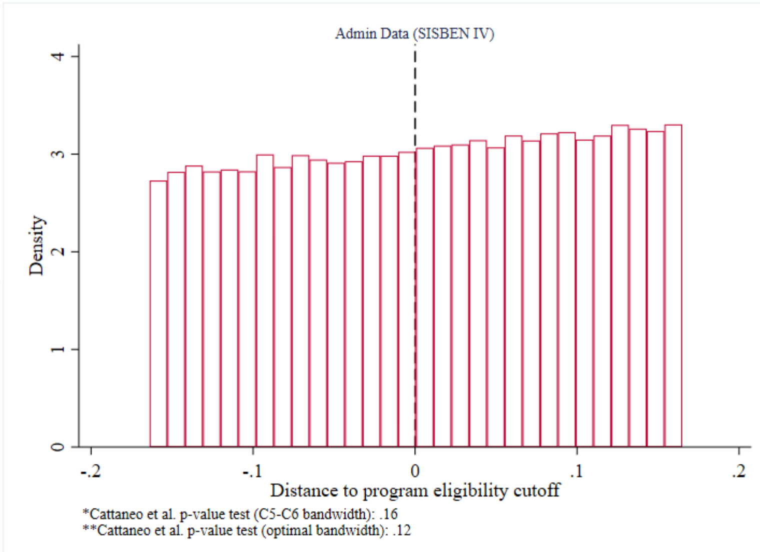
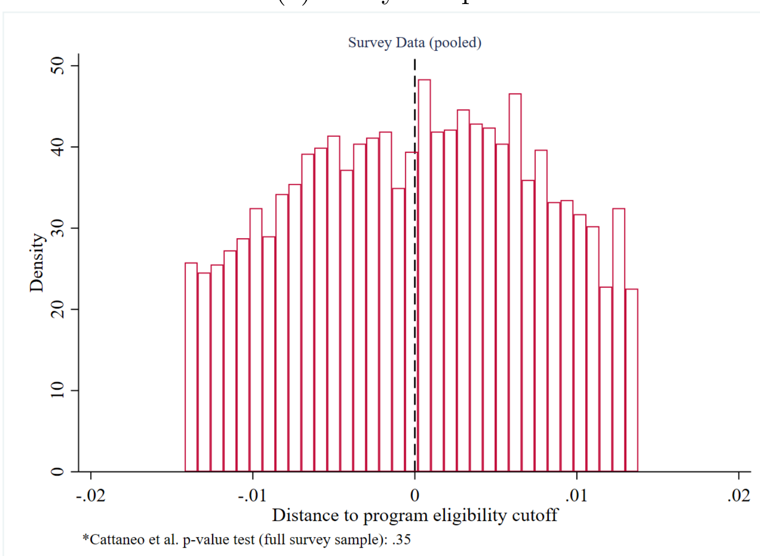


Figure A2: Food Spending/Total Spending Ratio by SISBEN IV Income Decile

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.



(a) Study Sample



(b) Survey

Figure A3: 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	q-value
	(1)	(2)	(3)	(4)	(5)
Age(head)	45.96	45.23	-0.72	0	0.00
Head of household - Man	0.51	0.51	-0.001	0.83	1.00
Number of household members	2.49	2.51	-0.028	0.04	0.34
Secondary or less	0.85	0.86	0	0.94	1.00
Technical education	0.09	0.09	0.002	0.58	1.00
University +	0.05	0.05	-0.001	0.45	1.00
Head cohabits with partner	0.54	0.54	-0.003	0.62	1.00
Divorced	0.09	0.09	0.001	0.69	1.00
Contributive SS regime	0.43	0.41	-0.005	0.29	1.00
Subsidized SS regime	0.47	0.49	0.003	0.59	1.00
Housing Quality Index	-4.28	-5.93	-0.312	0.25	1.00
Per-cap Spending (1000s COP)	307.3	293.51	-3.226	0.16	0.83
Covid cases per 100,000 people	0	0	0.000	0.87	1.00
Predicted eligibility	0.26	0.27	0.000	0.92	1.00

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

	Non-eligible mean	Eligible mean	Difference(RD)	p-value	q-value
	(1)	(2)	(3)	(4)	(5)
Any debt (utilities+retail)	0.7	0.69	-0.007	0.11	0.60
Any bank account	0.64	0.63	-0.004	0.45	1.00
Any active loan	0.45	0.44	0	0.92	1.00
Formal employment	0.36	0.35	0.011	0.01	0.11
Enrolled in school	0.83	0.83	-0.007	0.24	1.00
Repeating grade	0.03	0.03	-0.005	0.06	0.39
Global SABER 11 score (standardized)	-0.11	-0.12	0.012	0.75	1.00

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. Sharpened False Discovery Rate (FDR) q-values are computed following Anderson (2008).

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. Column 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) Can cover a week's worth of expenses	(2) Bought durables	(3) New business	(4) Incoming transfers
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

	(1)	(2)	(3)	(4)	(5)	(6)
	Per-capita Ed. Spending (IHS)		Time use studying (mins./day)		Owns a laptop - SABER 11	
	Round 1	Round 2	Round 1	Round 2	2020	2021
Eligible	0.248*	-0.0353	47.10**	19.89	0.0373	-0.00822
	(0.139)	(0.161)	(21.81)	(46.78)	(0.0273)	(0.0276)
Control mean (DV)	0.731	0.863	269.7	520.7	0.772	0.777
Bandwidth	0.0106	0.0106	0.0102	0.0102	0.288	0.288
Obs. (in bandwidth)	3421	3465	2449	1338	9683	9552
# of households (in bandwidth)	3421	3465	1678	930	9529	9390
Adjusted R2	0.0509	0.0822	0.114	0.0676	0.100	0.0787
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 Credit, by Pre-program Bank Account Ownership

	(1) Has Savings Account	(2) Fixed Term Deposits	(3) Credit Inquiry	(4) Any Outstanding Loan	(5) Past-due Loan
Eligible	0.181***	0.000	0.007***	0.015***	-0.010**
	(0.004)	(0.002)	(0.001)	(0.005)	(0.004)
Eligible X Account (baseline)	-0.137***	-0.000	-0.004***	-0.007**	0.003
	(0.003)	(0.002)	(0.001)	(0.003)	(0.003)
Effect (Account)	0.043	-0.000	0.003	0.008	-0.007
S.E. Effect (Account)	0.003	0.003	0.001	0.005	0.005
p-value	0.000	0.951	0.006	0.118	0.118
Control mean (DV)	0.702	0.042	0.032	0.429	0.210
Bandwidth	0.174	0.159	0.230	0.195	0.178
Obs. (in bandwidth)	1455216	1337976	1925292	1631796	1494624
# of households (in bandwidth)	242536	222996	320882	271966	249104
Adjusted R2	0.335	0.026	0.012	0.205	0.078
Data Source	Credit bureau	Credit bureau	Credit bureau	Credit bureau	Credit bureau

Notes: Columns (1) to (5) report results corresponding to the specification in equation (2) using quadratic polynomials. We use administrative records from the Credit Bureau covering from June 2020 to December 2022. 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 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 A9: 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	-0.00503	-0.000446	-0.00393	-0.00223	-0.0190	-0.000927	0.0353	0.0121	0.000660
	(0.121)	(0.0713)	(0.00156)	(0.00765)	(0.00297)	(0.0398)	(0.00201)	(0.0422)	(0.0123)	(0.00181)
Control mean (DV)	3.005	5.516	0.0188	0.772	0.0278	-0.0250	0.0364	0.821	0.0216	0.0400
Bandwidth	0.0137	0.0137	0.277	0.199	0.219	0.263	0.289	0.0137	0.0137	0.380
Obs. (in bandwidth)	3472	6689	351328	125617	138871	19136	355083	3503	3503	468207
# of households (in bandwidth)	3472	4875	134010	95867	106004	18478	355083	3503	3503	468207
Adjusted R2	0.0243	0.0178	0.00395	0.00200	0.00210	0.0270	0.00544	0.0188	0.00790	0.00638
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 A10: Effects on Non-Food Spending and Short-Term Consumption Debt - No Controls

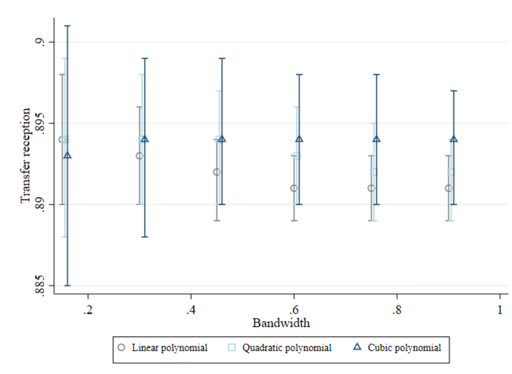
	(1)	(2)	(3)	(4)	(5)
	Non-Food	Debt (utilities + retail)		Credit cards	
	spending	Any debt	Past-due debt	Balance >0	Past-due debt
Eligible	0.141*	-0.00661	-0.00896**	0.00581	-0.00252
	(0.0824)	(0.00525)	(0.00455)	(0.00606)	(0.00293)
Control mean (DV)	5.710	0.680	0.321	0.275	0.0946
Bandwidth	0.0137	0.173	0.230	0.113	0.187
Obs. (in bandwidth)	6998	1472424	1958046	958704	1587792
# of households (in bandwidth)	5039	245404	326341	159784	264632
Adjusted R2	0.0697	0.0335	0.0228	0.0520	0.0198

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.

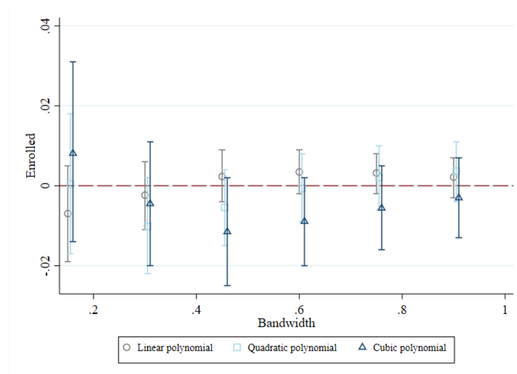
Table A11: Effects on Savings and Credit - No Controls

	(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	0.100***	0.176**	-0.00121	-0.0614	0.00432***	0.00567	-0.00752*
	(0.00485)	(0.0847)	(0.00243)	(0.0377)	(0.000976)	(0.00510)	(0.00419)
Control mean (DV)	0.700	-0.000272	0.0421	0.132	0.0322	0.427	0.209
Bandwidth	0.159	0.0137	0.158	0.0137	0.235	0.203	0.192
Obs. (in bandwidth)	1351968	6998	1346532	3503	1996632	1724130	1632414
# of households (in bandwidth)	225328	5039	224422	3503	332772	287355	272069
Adjusted R2	0.0520	0.0598	0.00517	0.0134	0.000890	0.0217	0.0247
Data Source	Credit bureau	Survey R1-R2	Credit bureau	Survey R2	Credit bureau	Credit bureau	Credit bureau

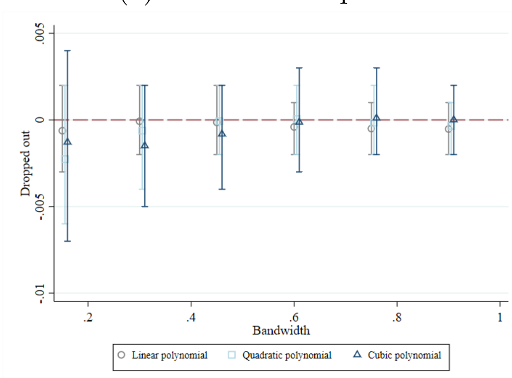
Notes: The table reports estimates of the reduced-form impact of the program using equation (1) excluding control variables. See notes on Table 5 for other details. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10%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



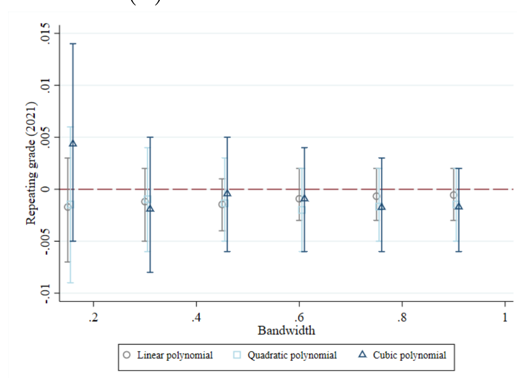
(a) Transfer Reception



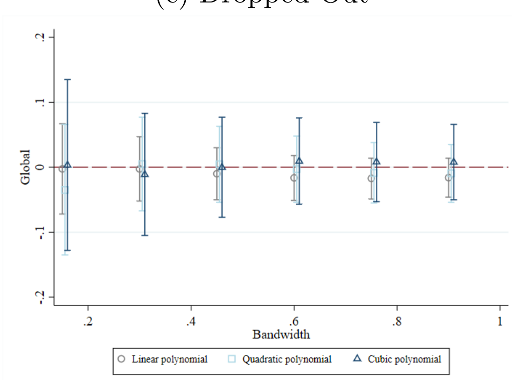
(b) Enrolled in School



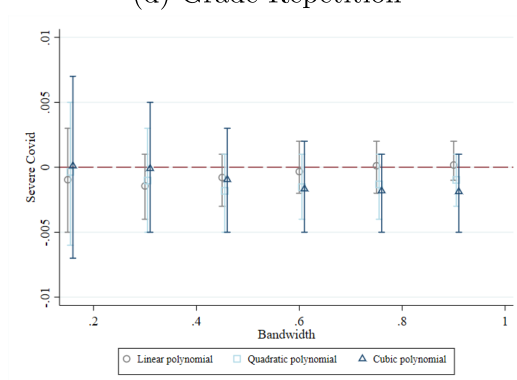
(c) Dropped Out



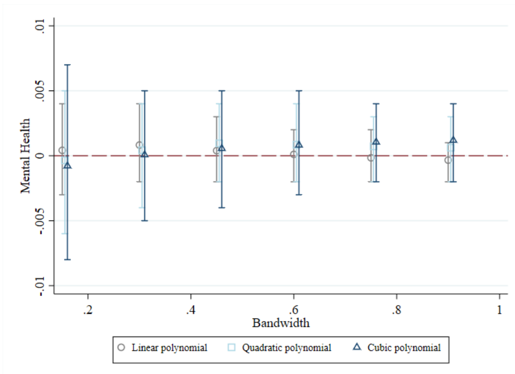
(d) Grade Repetition



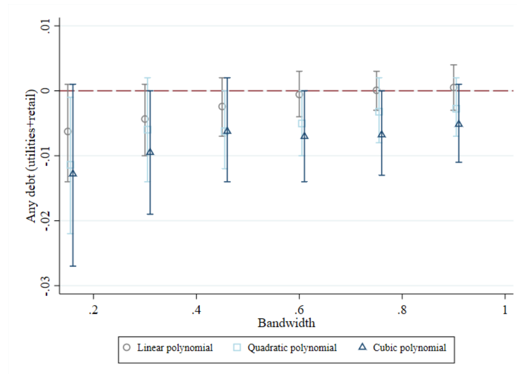
(e) Global Test Scores



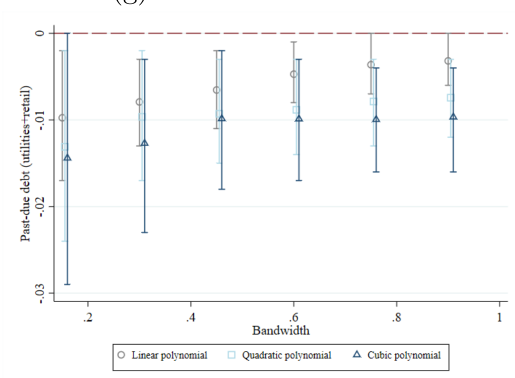
(f) Severe COVID-19



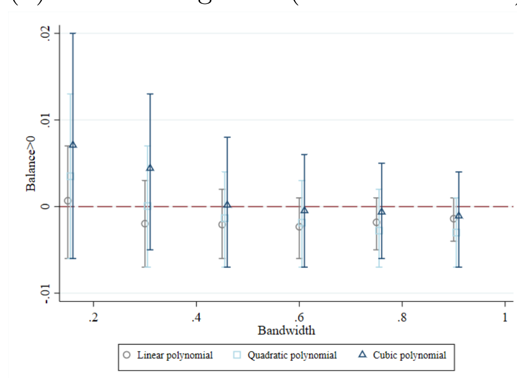
(g) Mental Health Care



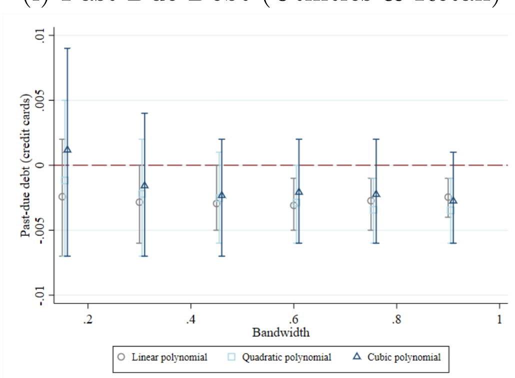
(h) Outstanding Debt (Utilities & Retail)



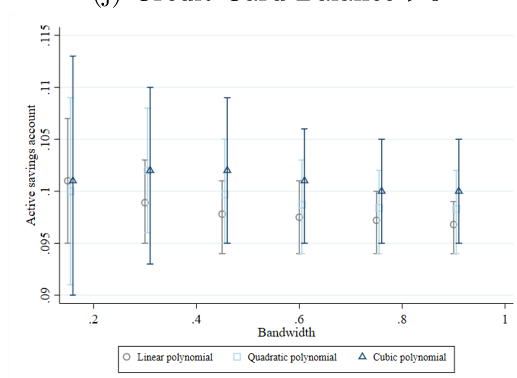
(i) Past-Due Debt (Utilities & Retail)



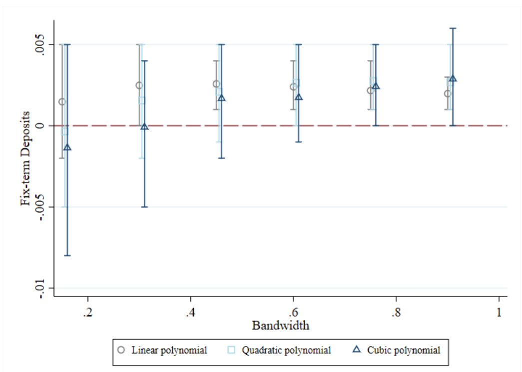
(j) Credit Card Balance >0



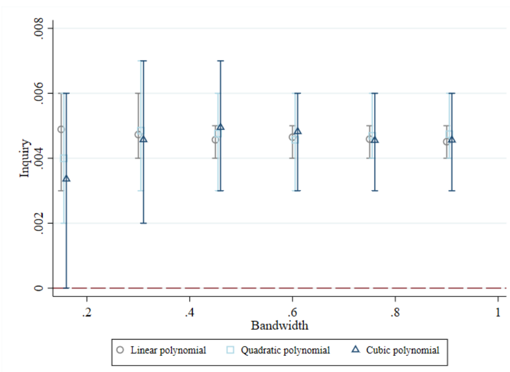
(k) Past-Due Credit Card Debt



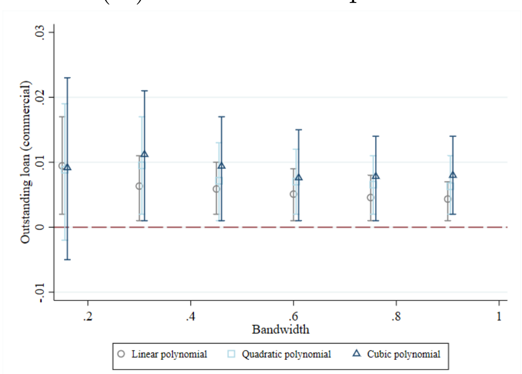
(l) Any Active Bank Account



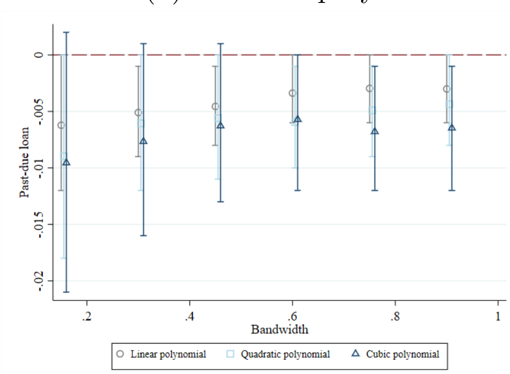
(m) Fixed Term Deposits



(n) Credit Inquiry



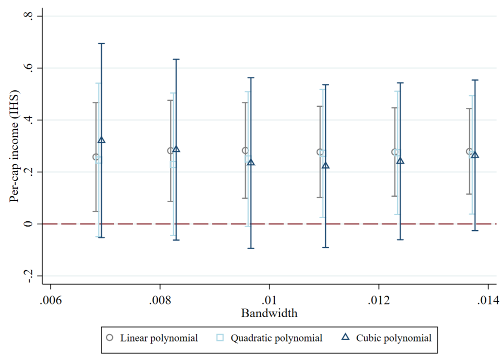
(o) Any Outstanding Loan



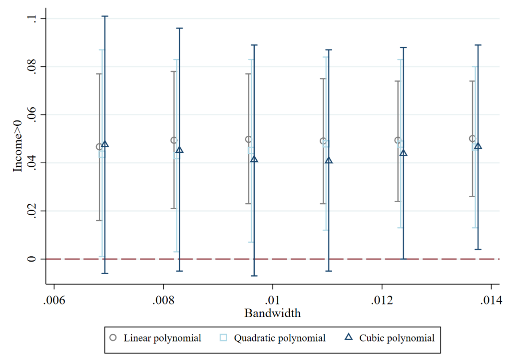
(p) Past-Due Loans

Figure A4: Robustness to Alternative Bandwidth Choices and Polynomial Degree (administrative data)

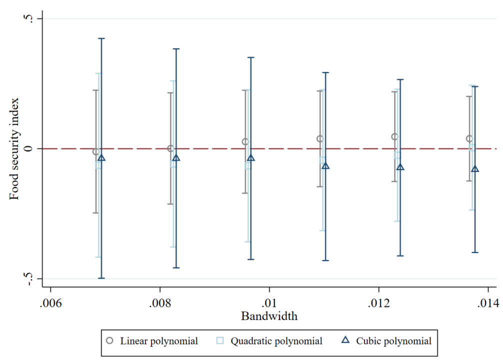
Note: The figure reports reduced-form estimates of the impacts of the program based on equation (1) estimated over different bandwidths and controlling for different polynomial degrees. The smallest bandwidth is equivalent to the maximum distance to the cutoff for the observations at the limits of SISBEN 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.



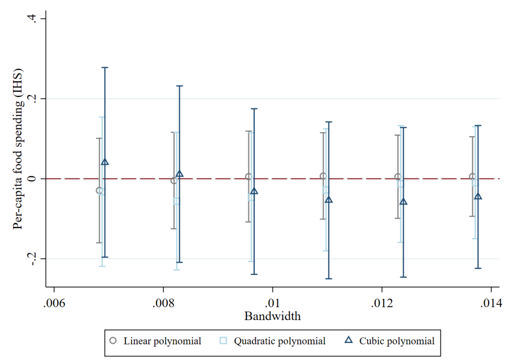
(a) Per Capita Income (IHS)



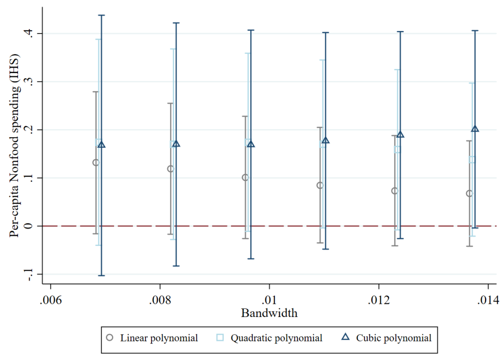
(b) Income>0



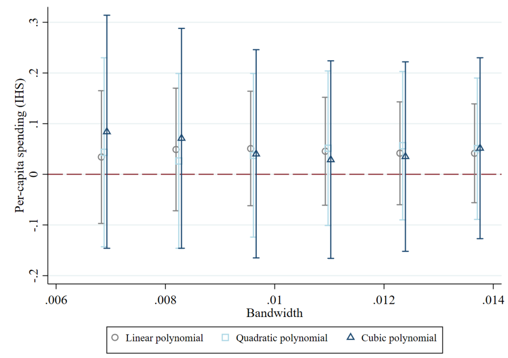
(c) Food Security Index



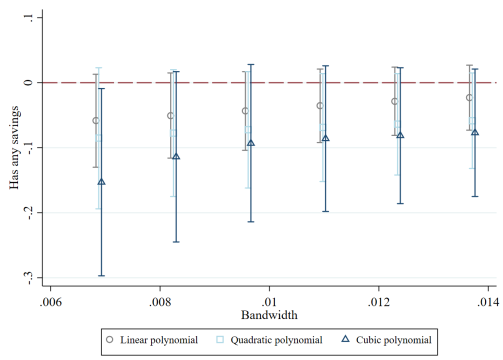
(d) Per Capita Food Spending (IHS)



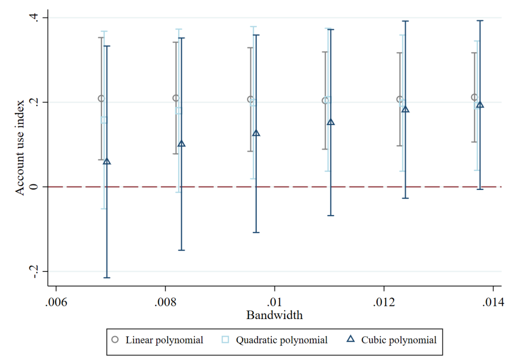
(e) Per Capita Non-Food Spending (IHS)



(f) Per Capita Spending (IHS)



(g) Savings



(h) Bank Account Use Index

Figure A5: 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 A12: Robustness to excluding Bogota - Admin data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
		Education			**Health**		**Debt (routine expenses)**				**Savings**		**Credit**		
							Debt (utilities + retail)		Credit cards						
	Dropped out	Enrolled 2021	Repeating Grade (2021)	Standardized Global Score	Severe Covid	Mental Health	Any debt	Past-due debt	Balance >0	Past-due debt	Has savings account	Fixed term deposits	Credit Inquiry	Any outstanding loan	Any past-due loan
Eligible	-0.001	-0.013*	-0.003	0.030	-0.003	0.000	-0.015**	-0.011**	0.004	0.000	0.110***	0.001	0.006***	0.008*	-0.005
	(0.002)	(0.008)	(0.003)	(0.048)	(0.002)	(0.002)	(0.006)	(0.005)	(0.005)	(0.003)	(0.005)	(0.002)	(0.001)	(0.005)	(0.004)
Control mean (DV)	0.02	0.694	0.026	-0.054	0.031	0.036	0.657	0.302	0.231	0.080	0.677	0.041	0.031	0.413	0.186
Bandwidth	0.282	0.258	0.231	0.274	0.285	0.342	0.133	0.200	0.152	0.180	0.182	0.204	0.228	0.213	0.258
Obs. (in bandwidth)	263369	121737	108866	12379	289783	347503	891420	1330362	1014804	1203270	1214994	1357650	1521762	1422594	1723578
# of households (in bandwidth)	108467	97638	87369	12006	289783	347503	148570	221727	169134	200545	202499	226275	253627	237099	287263
Adjusted R2	0.005	0.191	0.008	0.124	0.006	0.009	0.144	0.071	0.116	0.039	0.170	0.023	0.011	0.163	0.058
Data Source	SIMAT (2020-2021)	SIMAT 2021		SABER 11	RIPS						Credit bureau				

Notes: The table reports estimates of the reduced-form impact of the program using equation (1) excluding observations from Bogota using a quadratic polynomial. We use Calonico et al. (2019)'s data-driven approach to select the estimation bandwidth for each outcome. Standard errors are clustered at the household level and presented in parentheses. ***, **, and * denote significance at the 1, 5 and 10% levels.

Table A13: Effects by Exposure to a Severe Shock - Differences

	(1)	(2)	(3)	(4)
	First differences			
	Per capita Income (IHS)	Per capita spending (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 A14: 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 reports 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.

Table A15: 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 reports 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.