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ABSTRACT *

This paper uses a regression discontinuity design to study the impacts of a noncontributory pension program covering one-third of Bolivian households during the COVID-19 pandemic. Although the program was not designed to provide emergency assistance, it took on additional importance during the crisis, providing unintended positive impacts. Becoming eligible for the program during the crisis increased by 25 percent the probability that households had a week's worth of food stocked and decreased the probability of going hungry by 40 percent. Relative to the pre-pandemic years, the program's effect on hunger is magnified during the crisis. The program's effects were particularly large for households that lost their livelihoods during the crisis and for low-income households. The results suggest that, during a systemic crisis, a preexisting near-universal pension program can quickly deliver positive impacts in line with the primary goals of a social safety net composed of an income-targeted cash transfer and an unemployment insurance programs.

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Keywords: Cash transfers, Resilience, Social insurance, COVID-19, Noncontributory pensions

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I. Introduction

The COVID-19 pandemic and policies to contain it caused an unprecedented economic crisis with substantial income and labor market impacts for households. In developing countries, households are particularly vulnerable to economic crises because high levels of labor market informality limit the coverage of unemployment insurance schemes, access to formal credit is limited, and informal risk- sharing mechanisms break down during systemic shocks (Mace, 1991). Unless households have substantial savings to rely on, this leaves low- and middle-income households vulnerable to sliding into poverty. During the delay before new social programs can be implemented to confront a crisis, existing cash transfer programs with broad coverage may deliver unintended positive impacts and take on additional importance.

In developing countries, near-universal noncontributory pension programs are a fundamental component of social safety nets.[1] Although the introduction of noncontributory pensions has led to a wide array of welfare-increasing effects during non-crisis times,[2] their ability to provide relief during systemic crises has not been documented. These programs are not designed to provide emergency relief; they are instead designed to support elderly households' consumption as they withdraw from the labor market. But, in a context with many multi-generational households and little opportunity for intra-household substitution of labor, these programs may provide resources to attenuate the negative impacts of labor market shocks experienced by prime-age household members during crises.

In this paper, we provide evidence that an established, noncontributory pension program in Bolivia reduced financial insecurity, food insecurity, and stress during the pandemic, with particularly large impacts for low-income households and those that experienced a large labor market shock. We study the effects of becoming eligible for the Renta Dignidad program in Bolivia during the onset of the COVID-19 pandemic. Renta Dignidad, established in 2008, provides a basic monthly income of US$ 50 to the elderly, regardless of their income or contributions to social security. The program has broad coverage. In 2018, it reached one-third of Bolivian households, representing over 1% of Bolivian GDP. Because adults become eligible for the program upon turning 60 years old, we use a regression discontinuity (RD) design to compare outcomes of households with adults who just became eligible for the program during the onset of the pandemic (March 2020) to those

[1] In Latin America and the Caribbean, noncontributory pensions represent, on average, 0.38% of GDP which is slightly higher than the average spending on Conditional Cash Transfer programs as a share of GDP (0.34%) (Duryea and Robles, 2016)

[2] See for example, several impact evaluation of the introduction or expansion of noncontributory pension programs (Case and Deaton, 1998; Cruces and Bérgolo, 2013; Ardington, Case and Hosegood, 2009; de Carvalho Filho, 2008; Fan, 2010; Galiani, Gertler and Bando, 2016, 2018)

who would have been eligible had they turned 60 earlier. We implement our empirical strategy using near-real-time data collected online in April 2020, just days after the implementation of mobility-restriction policies.

The onset of the pandemic took a heavy toll on the livelihoods of Bolivian households. Among households close to becoming eligible for the program, 42% of households reported eating less healthily during the pandemic, 18% of households reported experiencing hunger, 48% of households did not have enough resources to cover a week of expenses, and 80% of survey respondents reported stress related to the pandemic. We find that eligibility for Renta Dignidad mitigated many of these negative impacts.

First, we provide evidence that eligibility for the program had substantial positive impacts at the onset of the pandemic. Specifically, we find that becoming eligible for the program increased the probability of having enough cash and food on hand to cover more than a week's worth of necessities by 12 and 8 percentage points, respectively. We also find a 9-percentage -point decline in the probability of experiencing hunger (a 40% reduction) and a similar decline in the probability of eating less healthily, which are consistent with evidence from the COVID-19 economic stimulus cash transfer in the United States (Baker et al., 2020). We also find suggestive, albeit noisier, declines in stress, which coincide with a decline in the probability of smoking. The results are robust to varying the estimation bandwidths, degree of the polynomials, and to falsification tests.

Second, we provide evidence that Renta Dignidad assumed greater importance during the pandemic. In the absence of binding liquidity constraints, we would not expect eligibility for an anticipated transfer to cause a contemporaneous jump in the most dire outcomes, such as household food consumption, which often follow a smooth trajectory.[3] We replicated our empirical strategy using household-survey data from 2016 to 2018, and as expected, we find no evidence that program eligibility decreased the probability of experiencing hunger in 2016-2018. The stark contrast between this finding and the results using data from the onset of the pandemic suggests that households faced with additional liquidity constraints during the pandemic relied on the program to avoid hunger and achieve basic levels of food consumption. This contrast is important because one of the negative consequences of the pandemic is expected to be a decrease in food security (Ray and Subramanian, 2020), which can decrease productivity (Schofield, 2019) and have long-lasting consequences (Maluccio et al., 2009).

Third, our results suggest that a near-universal pension program can quickly deliver positive impacts in line with the primary goals of a social safety net composed of an income-targeted cash

[3] Indeed, there is evidence on anticipation effects in these type of programs (Olivera and Zuluaga, 2014).

transfer program and an unemployment insurance program. We provide evidence that the positive impacts of the program are particularly large for households that experienced a large labor market shock—those that were likely facing tightened liquidity constraints due to the pandemic—and for low-income households for which the transfer represents a larger share of household income. We use data on closures of family-owned businesses (a proxy for self-employment) and job losses during the onset of the pandemic, which do not vary discontinuously at the eligibility cutoff, to show that the decline in the probability of experiencing hunger, the most dire outcome, was twice as large for households that experienced business closures. This consumption-smoothing effect is important, as over 65% of households in our sample reported business closures during the early stages of the pandemic and a large share of them are middle-income households, which tend to be excluded from income-targeted programs and are vulnerable to sliding into poverty (Busso et al., 2020). We also find that the effects of the program on resource availability and hunger are particularly large among low-income households.

This paper contributes to the literature that studies the effects of noncontributory pension programs in developing countries in two ways. First, previous studies analyzed the impact of these programs on recipient households' labor market outcomes and consumption (Case and Deaton, 1998; Cruces and Bérgolo, 2013; de Carvalho Filho, 2008; Fan, 2010), health (Duflo, 2000, 2003), and subjective well-being (Galiani, Gertler and Bando, 2016, 2018),[4] during regular and stable periods. Given that several similar programs in developing countries were implemented after the global crises of 2001-02 and 2007-08, we contribute with novel evidence on the salience of well-established noncontributory pension programs during the onset of a devastating crisis by documenting that eligibility for the program increased financial and food security. Second, there is evidence of within-household consequences of pension programs in health (Peluffo, 2019) and labor supply (Ardington, Case and Hosegood, 2009; Chong and Yáñez-Pagans, 2019) in the context of multi-generational households. We provide evidence of a novel mechanism operating in this context by showing that, during systemic crises when within-household substitution of labor is limited, the pension benefits obtained by the elderly can provide important assistance during labor market shocks that are likely affecting prime-age household members.

This paper also contributes to the literature studying consumption smoothing in developing countries by showing that public assistance programs can help households smooth consumption during a systemic crisis, when the success of risk-sharing networks is limited by the widespread

[4] In the case of Bolivia, Mena and Hernani-Limarino (2015) and Borrella Mas, Bosch and Sartarelli (2016) argue that the program induced a decline in labor force participation among recipients, leading to limited changes in income and consumption. However, Escobar, Martinez and Mendizabal (2013) find increases in per-capita consumption.

nature of the shocks.[5] These results have important implications for the design of emergency social programs in developing countries,[6] where automatic stabilizers such as unemployment insurance are rarely available, access to cash aid programs for the middle class is limited (Busso et al., 2020), and the welfare gains from insuring against economic shocks can be large (Chetty and Looney, 2006). In this context, our results provide novel evidence that, in a context with many multi-generational households, an established, near-universal pension program can quickly deliver positive impacts in line with the primary goals of a social safety net composed of an income-targeted cash transfer program and an unemployment insurance program. This is crucial, as timely implementation of new social programs at scale can be challenging, particularly during the onset of crises.[7]

II. Context

We study the context of the Renta Dignidad program in Bolivia. The program was initially established in 2008 with the aim of providing a basic income to the elderly. People become eligible for the program when they turn 60 years old, regardless of their income status.[8] As a result, Renta Dignidad is a large-scale program that represented 1.3% of Bolivia's GDP in 2018 and accounted for one-third of Bolivia's total spending in social protection.[9] The program has broad coverage. In 2018, 28% of households, approximately 1 million, received transfers from the program.[10]

The program provides monthly payments of US$ 50 to beneficiaries who do not have private retirement pensions (85% of beneficiaries in 2019), and of US$ 43 to beneficiaries who do have private retirement pensions.[11] The payments per beneficiary represent 30% of the median monthly per-capita household income and 12% of total income for eligible households.[12] To receive the funds, upon turning 60, adults need to register in the program's database by showing proof of identity. Once registered, beneficiaries access the transfers by the end of the month. For example, a person who turns 60 in March 2020 would start receiving benefits in April 2020. The transfers are cashed out at branches of Banco Union (the state-owned bank), although beneficiaries may request

[5] Townsend (1994), among others, analyze the ability of households to insure against idiosyncratic shocks, while Jack and Suri (2014) and Riley (2018) study the role of cross-locality transfers in smoothing consumption. Mace (1991) shows that, even with complete markets, households cannot ex ante insure against aggregate shocks.

[6] Banerjee, Niehaus and Suri (2019) and Hanna and Olken (2018) discuss targeting and coverage.

[7] Recent literature discusses issues related to targeting (Alatas et al., 2016; Niehaus et al., 2013), leakage (Banerjee et al., 2016; Muralidharan, Niehaus and Sukhtankar, 2016), mode of payment, and last-mile delivery (Muralidharan et al., 2018).

[8] There is one exception. Workers who are still on the public-sector payroll after turning 60 are ineligible to receive resources from Renta Dignidad. Less than 1% of adults 60 years old or older formally work in the public sector. The minimum retirement age in Bolivia is 55 for females and 58 for males.

[9] See `https://dds.cepal.org/bpsnc/programa?id=42`

[10] Statistic computed based on the 2018 wave of Encuesta de Hogares, which is conducted by the national bureau of statistics (Instituto Nacional de Estadisticas, INE)

[11] Only 15% of beneficiaries during 2019 also receive private retirement pensions according to administrative data from the pensions regulator Autoridad de Fiscalizacion y Control de Pensiones y Seguros (APS).

[12] We use the INE 2018 Household Survey to compute household income.

home delivery of the funds if they submit a certification of physical impairment.

A. The Program and the COVID-19 Pandemic

The first diagnosed case of COVID-19 in Bolivia was confirmed on March 10, 2020, and the first death was announced on March 29, 2020. The government imposed a strict mandatory quarantine on March 21, 2020 with strong enforcement in the main urban centers, limiting the operation of businesses to essential businesses (health centers, pharmacies, markets, and some government offices) and restricting the circulation of motorized vehicles to those with a government license.[13] Figure 1 shows that trips to workplaces fell sharply after March 21, 2020.

During the onset of the pandemic, the Renta Diginidad program was the main social-assistance program providing regular monthly payments to beneficiaries.[14] Starting April 1, 2020, the government doubled the transfer amount for beneficiaries who were not receiving other government pensions. The government also allowed the payments to be made to authorized family members on behalf of beneficiaries so that the elderly would not have to go to bank branches. The disbursements were still in person, but the government partnered with private banks to increase the number of locations authorized to disburse the transfers. The eligibility criterion was not modified.[15]

III. Data and Measurement

We use near-real-time data collected through online surveys in Bolivia implemented as part of the IDB/Cornell Coronavirus Survey.[16] Participants were recruited through the following process. First, links to the survey were posted by the Inter-American Development Bank (IDB) on social media using its institutional account. Second, the post was disseminated through paid social-media ads. The ads were targeted using keywords related to general-interest topics, such as “futbol” and the names of local celebrities.[17]

We received 26,181 complete responses in Bolivia from April 3, 2020 to April 30, 2020. Thus, the data provides information collected during the onset of the pandemic, in the period when mobility was restricted the most (see Figure 1). For a subset of 11,633 responses from households with at

[13] Some branches of Banco Union were kept open.

[14] There were two other cash-transfer programs: The Bono Juancito Pinto—a conditional cash transfer (CCT) program for children enrolled in public schools (Vera-Cossio, 2020), and the Bono Juana Azurduy—a CCT for pregnant women and mothers of children under two years old (Celhay et al., 2019). However, in the case of the former, the transfers are paid only twice per year and, in the case of the latter, the transfers are paid upon the completion of the conditions for disbursement.

[15] The government later announced other cash transfer programs targeting people with school-age children enrolled in public schools and self-employed workers. The funds were not disbursed until late April, the end of our sample time period, and eligibility for these programs does not change discontinuously at the age of 60.

[16] The IDB/Cornell Coronavirus Survey was implemented in 17 countries across Latin America and the Caribbean.

[17] A detailed description of the data collection approach can be found in Bottan, Hoffmann and Vera-Cossio (2020*b*)

least one household member age 55 or older, we collected information regarding month and year of birth of the oldest household member.

Our sample includes respondents from all income levels. However, respondents to our online survey are more educated than the average Bolivian. Appendix Table A1 shows that the online sample more closely resembles, in terms of demographic attributes, respondents to the 2018 nationally representative household survey in urban areas. When the data collection began on April 3, 2020, there were less than 100 cases in Bolivia, which were concentrated in the main urban areas. Thus, our online sample covers the subset of the population with highest exposure to the early impacts of the pandemic.

Figure 1 depicts the evolution of COVID-19 cases in Bolivia over time, the changes in trips to workplaces based on Google's Community Mobility Reports, and the beginning and end dates of data collection. It shows that our data was collected before the surge in COVID-19 cases, but just weeks after the national mandatory quarantine was put in place. Given that the recall period of the survey is at most the month preceding data collection, our responses capture information corresponding to the days following the implementation of the lockdown measures.

Table 1 illustrate the dramatic situation of households with at least one member between the age of 55 to 65 during the onset of the pandemic. It shows that across all income levels 68% of households experienced the closure of a family-owned businesses and 45% of households experienced a job loss. In addition, 52% of households report having enough cash in hand to cover a week's worth of expenses, and only 33% of households report having enough food reserves to cover meals for a week. In addition, 42% of households modified their diets and 18% experienced hunger. Further, over 85% of households report feeling stressed about the pandemic. These statistics may understate the dramatic situation during early stages of the pandemic because our data set captures information from wealthier and more educated households.

Finally, the vast majority of respondents belong to multi-generational households. Table 1 shows that the average age of the survey respondent was 34. In addition, around 95% of the responses of households with adults age 55 to 60 correspond to prime-age respondents. Thus, our dataset allows to study how a benefit provided to the elderly relates to household-level outcomes, and whether labor-market shocks to prime-age household members are attenuated by the benefits received by the elderly.

IV. Empirical Strategy

To identify the causal effects of the program, we exploit the discontinuity that arises from the fact that the sole eligibility criterion for receiving program benefits is age. As of April 1, 2020, adults who turned 60 in March 2020 became eligible to receive transfers from the program during April, while marginally younger adults were ineligible to receive the noncontributory pension. To identify eligible households in our survey, we collected information on the month and year of birth of the oldest adult in the respondent's household, conditional on the respondent reporting that at least one member of the household was 55 years old or older at the time of data collection (April 2020). Thus, our empirical design compares outcomes of households whose oldest member just became eligible for the program during the onset of the pandemic in Bolivia to those of households whose oldest member of the program was only months away from becoming eligible.

The effect of being eligible for the program on outcome y_i can be modeled in a regression discontinuity framework as:[18]

$$Y_i = \beta_0 + \beta_1 T_i + \theta_1 (Age_i - c) + \theta_2 T_i \times (Age_i - c) + \gamma x_i + \epsilon_i \tag{1}$$

where Age_i is the age of the oldest adult in the household of respondent i on March 30, 2020; c is the cutoff age of 60 in March 2020; x_i is a vector of demographic household and respondent characteristics that are unlikely to vary due to the program; and ϵ_i is an error term. $T_i = \mathbf{1}[Age_i \geq c]$ is an indicator of whether the age of the oldest member of household i is above the age cutoff. We use a linear specification of the running variable around the cutoff, and we allow for different slopes on either side of the cutoff. We also report estimates using a second-order polynomial in Section V.D.

We estimate equation (1) using triangular weights that assign a higher weight to observations of households closer to the eligibility cutoff, and conduct inference using robust standard errors. We report results using different bandwidths, including optimally selected bandwidths using the approach of Cattaneo, Jansson and Ma (2019).

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 and respondent outcomes. As eligibility is based on the age of the oldest person in the household in March 2020, our estimates

[18] We focus mostly on household outcomes as our sample is mostly composed of multi-generational households, and there is evidence of important within- household distributional effects in the context of non-contributory pension programs (Duflo, 2000, 2003).

capture the local average treatment effect of being included in the cash-assistance program at the onset of the pandemic and the beginning of mobility-restriction policies in Bolivia.

A. *Threats to Identification*

Manipulation. The validity of an RD design requires that individuals cannot perfectly manipulate the assignment variable, which in our setting is the oldest household member's age at the onset of the pandemic. There are two reasons why manipulation is unlikely in the Bolivian setting. First, we study the impact of program eligibility during the onset of the pandemic. Given the unanticipated nature of the pandemic, ex ante there was no incentive to manipulate eligibility in order to become eligible during the time period used in our analysis. Second, changes to official date of birth records are rare at any time, and extremely unlikely during this period, due to the closure of government offices in late March 2020.

As we rely on self-reported data, a similar threat to validity is that becoming eligible for the program during the onset of the pandemic may have caused differential response rates of households around the age 60 cutoff. Appendix Figure A1 reports the distribution of observations around the cutoffs, focusing on households with adults 55 to 65 years old, and shows no evidence of discontinuous changes at the cutoff according to the (McCrary, 2008) test for manipulation. In addition, Appendix Table A2 shows that we cannot reject the null hypothesis that there are no discontinuities in the distribution around the cutoff using the manipulation test following Cattaneo, Jansson and Ma (2019) (p-value=0.14 and p-value=0.55 with and without adjusting for bandwidth selection).

Balance. We also test for discontinuities in demographic characteristics around the cutoff using different bandwidths to estimate (1). Appendix Table A3 shows that, at a 5% confidence level, there are no significant differences around the cutoff. However, we did find some differences that are significant at 10%, but none of them persist across bandwidths. In addition, for each estimation bandwidth, we are unable to reject the null hypothesis that all the coefficients in each column are jointly zero. We show in Section V.D that, besides changes in statistical power, our RD estimates are very similar with and without demographic controls in the regression specification.

V. Effects of the Program on Resilience, Food Security, and Stress

A. Program Participation

Figure 2A graphs the relationship between self-reported program participation and month and year of birth. The running variable is the age of the oldest person in the household as of March 2020, normalized with respect to the eligibility cutoff of 60 years old as of March 30, 2020. Our age variable is recorded at the monthly level; each observation in the graph is the share of households that report receiving Renta Dignidad in three-month age bins. The solid line is a linear fit estimated on each side of the cutoff using triangular weights over the 24-month bandwidth. The figure shows a sharp increase in the share of households that report receiving transfers as part of the program around the cutoff. Appendix Figure A2 reports similar graphs using a 24-month bandwidth and nonparametric fits around each side of the cutoff.

Column 1 in Panel A of Table 2 shows the results of RD estimates using the specification in equation (1) over a 24-month bandwidth. There is a sharp, precise jump in the probability of receiving program resources at the cutoff. The increase in program participation is 20 percentage points. Panels B, C, and D show that this increase does not vary with respect to the choice of bandwidth. Note that the probability of being a program beneficiary does not jump from zero to one. The transfers could only be cashed out in person at branches of Banco Union (the state-owned bank), and the mobility restrictions and related public health concerns may have discouraged the elderly from visiting the bank branches to collect their transfers, or visiting government offices to enroll in the program.[19]

B. Effects on Financial Resilience, Food Security, and Stress

In this section, we report RD estimates of the ITT effect of becoming eligible for the transfer during the onset of the pandemic on households' ability to secure funds to pay for necessities, food security, and stress.

To measure financial resilience, we asked respondents about their households' ability to cover emergency expenses.[20] To measure the ability of households to cover basic expenses, we asked households to report whether they had enough resources to cover regular household expenses for 1 to 3 days, 4 to 7 days, 1 to 2 weeks, 3 to 4 weeks, or more than a month, in the event that they

[19]As mentioned in Section II, there is an option for home delivery of the funds, which is only available to adults with physical impairments. These adults tend to be older and less likely to be included in the analysis bandwidth.

[20]Specifically, we asked whether their households could come up with funds to cover emergency expenses equivalent to 0.5, 1 and 1.5 minimum monthly wages. We randomize the amount with equal probability in the survey.

lost their main source of income. Likewise, to measure the ability of households to secure their food supply, we asked the respondents to report whether they had enough food stocked to cover meals for 1 to 3 days, 4 to 7 days, 1 to 2 weeks, 3 to 4 weeks, or more than a month. We then constructed indicators of whether households had enough resources to cover more than a week's worth of necessities and more than a week's worth of meals.

We also analyze whether the program protected households from going hungry due to lack of food during the onset of the pandemic and from having to reduce the quality of their diet, relative to pre-pandemic periods. Table 1 shows that 18% of respondents reported that someone in their households went hungry during the week preceding data collection, and that 42% of respondents reported that their household was eating less healthily.[21] Finally, to quantify the effects of the program on stress, we exploit self-reported information regarding the respondents' subjective perceptions of stress due to the pandemic and due to an increased health risk for the respondent's household members.[22] In addition, we asked respondents to report whether, due to the crises, they are smoking more than usual.[23]

We find that becoming eligible for the cash transfer significantly increases financial resilience. Figures 2B and 2C depict the discontinuous increases in the probability that households report having enough resources to cover emergency expenses and more than a week's worth of basic expenses. Column 2 in Table 2 shows that, depending on the bandwidth choice, beneficiary households are more likely to be able to come up with resources to cover unexpected expenses, although in some cases this effect is imprecisely estimated. Column 3 shows that, depending on the bandwidth, becoming eligible for Renta Dignidad during the onset of the pandemic increased the probability of having enough resources to cover more than a week of expenses by 0.10 to 0.13 percentage points. The effects represent over 20% of the mean for marginally ineligible households.

Becoming eligible for the program during the onset of the pandemic improved food security, relative to marginally ineligible households. Figure 2D shows a discontinuous increase in the probability that households report having enough food on hand to cover more than a week's worth of meals. Column 4 in Table 2 shows that, depending on the bandwidth, becoming eligible for Renta Dignidad during the onset of the pandemic increased the probability of having a large enough stock of food to cover more than a week of meals by 0.06 to 0.08 percentage points. The effects represent

[21]We asked respondents whether they agreed with the statement that their household is eating less healthily than normal using a 5-level Likert scale. We then coded an indicator of 1 if the respondent somewhat or strongly agreed with the statement, and 0 otherwise.

[22]Respondents were ask about their level of agreement with the following two statements: "I feel nervous about the current situation" and "I feel worried for the health of the members of my household." Answers were collected using a 5-level Likert scale. We then coded an indicator of 1 if the respondent somewhat or strongly agreed with the statement, and 0 otherwise.

[23]Non-smokers were coded as missing.

over 20% of the mean for marginally ineligible households.

Consistent with our results on food availability, Figure 2E shows that there is a discontinuous decline in the probability that someone in the household experienced hunger. Column 5 in Table 2 shows that, depending on the bandwidth, the probability of experiencing hunger during the pandemic is reduced by 0.08 to 0.12 percentage points due to the program. These effects account for 44 to 60% reductions with respect to the probability of experiencing hunger among marginally ineligible households. Note that all of our main findings are robust across bandwidths and remain significant after adjusting the p-values for multiple hypothesis testing using Benjamini, Krieger and Yekutieli (2006)'s approach.

Eligibility for the program also protected households from reductions in the quality of their diet. Figure 2F shows that there is a reduction in the probability that a household reported eating less healthily during the pandemic. Column 6 of Table 2 shows that although the decline is only significant at 10%, it is economically meaningful, accounting for 10–15% of the average among marginally ineligible households.

We also analyze the effect of the program on respondents' subjective perceptions of stress during the pandemic but find no strong evidence of declines in stress due to the program. For a 24-month bandwidth, Figures 2G and 2H show that the program did not significantly reduce stress related to the pandemic or related to the health of their family members. However, column 7 of Table 2 shows that, for some bandwidths, the program seems to reduce respondents' perception of stress due to the pandemic. Corroborating a reduction in stress, column 9 shows that the probability that respondents report increased smoking also declines in some specifications.

Overall, becoming eligible for cash benefits during the onset of the pandemic increased household resilience, helping households avoid changes in their food consumption and nutrition. Thus, having access to cash aid early in the pandemic may help prevent reductions in the stock of human capital because declines in nutrition can lower worker productivity (Schofield, 2019; Dasgupta and Ray, 1986) and affect long-term educational outcomes in the case of children (Maluccio et al., 2009).

C. Effects during the Pre-Crisis Period

In regular times, entrance into the program could be interpreted as an expected permanent increase in income. In the absence of binding liquidity constraints, households should adjust their food consumption very little at the time that they become beneficiaries. We find no evidence of discontinuous changes around the eligibility cutoff in the probability that a household member went hungry using pre-pandemic data. Figure 3 replicates our empirical strategy using data from the

2016-2018 Bolivian Household Surveys, comparing the effects of program eligibility on the probability that at least one household member went hungry during the three months before the data collection.[24] In contrast to the economically and statistically significant program effects on hunger using the data collecting during the onset of the pandemic, we find no evidence that becoming eligible for the program reduced hunger during pre-pandemic years.

Table 3 reports point estimates for different bandwidths. In all cases the point estimates have the opposite sign of the effects during the pandemic. Further, 95% confidence intervals rule out declines in hunger during pre-pandemic years of the size of the impact estimated during the onset of the pandemic. This contrast emphasizes the importance of quickly delivering resources during the onset of the pandemic and the program's ability to attenuate the effects of severe economic shocks.

D. Robustness

Exclusion of covariates. Table A4 reports estimates of our main results without including covariates in the RD regressions for different bandwidth choices. The point estimates barely change relative to our main specification including controls.

Higher-order polynomials. Appendix Table A5 shows that allowing for quadratic trends in the running variable does not change the point estimates, but it does increase the standard errors. The results are also robust to using flexible nonparametric estimates on each side of the cutoff (see Appendix Figure A2).

Placebo exercises. Finally, we conduct two placebo exercises by moving the cutoff 24 months before and after the cutoff of 60 years of age in March 2020. Panel A of Appendix Table A6 compares households whose oldest member became eligible for the program 2 years before the onset of the pandemic to those whose oldest member, at that point in time, would have been marginally ineligible for the program. This exercise should yield null or small differences, because those household members that were ineligible in March 2018 still became eligible long before our data collection period. Reassuringly, we find no evidence of statistically significant or economically substantial differences between these two groups. Panel B of Appendix Table A6 compares differences in outcomes between households whose oldest member will become eligible for the program 2 years in the future (March 2022) and those whose oldest member would be marginally ineligible at that time. There are no significant differences between these groups in 8 of the 9 outcomes that we study.

[24] The field work associated with household surveys is usually conducted during the last quarter of each year.

VI. Heterogeneity by Exposure to Labor-Market Shocks at the Onset of the Pandemic

In our data set, ninety-five percent of the households around the eligibility cutoff were multi-generational households—households in which prime-age and elderly members cohabit. In multi-generational households, although program beneficiaries are less likely to actively participate in the labor market, these households are still exposed to labor-market shocks to prime-age household members. For 65% of households in our sample, the pandemic led to large labor market shocks that triggered income reductions. We use data on closures of small family-owned businesses—a proxy for self-employment—and job losses during the onset of the pandemic, to analyze the extent to which the effects of the program varied with exposure to labor market shocks. Appendix Figure A3A shows that low- and middle-income households were substantially more likely to experience business closures and job loss during the weeks preceding the pandemic.

We combine this cross-household variation in the exposure to shocks induced by the pandemic with our RD approach to estimate the following specification:

$$
\begin{aligned}
Y_i = &\beta_0 + \beta_1 T_i + \beta_2 T_i \times Shock_i + \beta_3 Shock_i \\
&+ \theta_1 (Age_i - c) + \theta_2 T_i \times (Age_i - c) + X_i \Gamma + \epsilon_i
\end{aligned}
\tag{2}
$$

where $Shock_i$ is an indicator of whether any household member lost their livelihood during the month preceding data collection.[25] We focus on loss of livelihood related to closures of family-operated businesses and job losses in Panels A and B of Table 4, respectively. The ITT effect of the program, regardless of whether a household experienced a labor-market shock, is captured by β_1 (the direct effect). β_2 captures the differential effect in the case of households in which a household member lost their livelihood during the pandemic (the smoothing effect). For ease of exposition we report estimates using a bandwidth of 24 months before and after the program and report results using other bandwidths in the Appendix.

Estimates of equation 2 deliver valid comparisons to the extent that the exposure to shocks is exogenous with respect to changes in program eligibility. Column 1 of Table 4 reports RD estimates of equation 1 of the effect of the program on experiencing business closures (Panel A) and job losses (Panel B). Reassuringly, neither outcome varies discontinuously around the cutoff for program eligibility.

[25] We randomly varied the recall period of the questions related to loss of livelihoods across respondents. We considered three recall periods: the prior week, the prior two weeks, and the prior four weeks.

Column 3 of Panel A shows that program eligibility increased the likelihood that households had enough resources to cover their needs for one week and that this effect was smaller for households that closed their businesses, but this difference is not robust to alternative bandwidths (see Appendix Table A7). Likewise, we find strong evidence that the program improved the ability of households to stock up on food supplies, but we fail to detect heterogeneous effects by business closure (see Column 4).

We find strong evidence that the program enabled the hardest-hit households to maintain a basic level of food consumption. Column 5 shows that business closures are linked to an increase in the probability of reporting that a household member went hungry, and that this increase is attenuated by half in the case of households that became eligible for the program during the onset of the pandemic. This result is robust across different bandwidths (see Appendix Table A7). Column 6 suggests that program eligibility reduced the likelihood of reporting a deterioration in diet quality. In the case of households that closed their businesses, program eligibility had a smaller impact on diet quality, suggesting a substitution between quantity and quality of nutrition. In addition, Columns 8 and 9 suggest that, relative to households that did not close their businesses due to the pandemic, the program led to declines in stress related to the health status of family members and in the probability of smoking. Overall, it appears that the program's impacts for the most dire outcomes are concentrated among households that experienced a business closure—those that probably faced stronger liquidity constraints during the onset of the pandemic, while the impacts for less dire outcomes are experienced more broadly.

To quantify the importance of this attenuation effect, we use the estimates in Column 5 of Panel A of Table 4 to compute the contribution of the smoothing effect to the total effect of the program on the probability of going hungry. We multiply the differential effect of the program for households that closed a business ($\beta_2 = -0.09$) by the share of households that experienced a business closure ($S\bar{h}ock = 0.69$) and divide it by the average effect of the program ($\beta = \beta_1 + \beta_2 S\bar{h}ock = -0.03 - 0.09 \times 0.69$). The smoothing effect accounts for over 65% of the program's total effect on the probability of going hungry (Column 3 in Panel A in Table 4) and suggests that the program was crucial for households that experienced large labor-market shocks.

The heterogeneous effects are driven by shocks related to the closure of a family-owned business during the pandemic (Panel A). We do not find evidence of heterogeneous effects when we use job losses as a proxy for economic shocks (Panel B). This is unsurprising in the case of Bolivia, where 68% of working age adults are self-employed.[26] Indeed, the share of households reporting a business

[26]Data from the World Bank's World Development Indicators show that for 2019, the share of self-employed workers in Bolivia was 68%.

closure is substantially larger than the share reporting job losses (see Appendix Figure A3A).

Our results reveal a novel mechanism through which benefits to the elderly lead to household-level impacts. In regular times, one would expect within household substitution of labor supply to partially smooth out the effects of idiosyncratic labor market shocks experienced by prime-age household members. During the onset of COVID-19 pandemic, households experienced a systemic shock that disrupted labor markets and limited the scope of within-household substitution, expanding the importance of the program by assisting households that experienced a recent business closure in securing a basic level of food consumption.

VII. Heterogeneity by Pre-Pandemic Income

During the onset of the pandemic, low-income households experienced business closures and job losses at high rates (see Appendix Figure A3A), and several middle-income households transitioned into lower income categories (see Appendix Figure A3B). We exploit the fact that eligibility for the Bolivian program is not based on income to analyze the impacts of the program across these key income groups.

Table 5 reports RD estimates corresponding to equation (1) for each pre-pandemic income group using a bandwidth of 24 months before and after the cutoff. In Column 2, we observe larger point estimates of the effect of the program on the availability of funds to cover a week's worth of expenses in the subsample of low-income households, for which the transfer represents a larger share of household income. Among low-income households, the program was also more effective at reducing the probability that somebody in the household went hungry. Overall, the larger impacts of the program were focused on low-income households.

The previous results suggest that restricting eligibility to households with low pre-pandemic income could have increased the program's overall impact. However, during periods with systemic shocks, many households experienced income reductions. Appendix Figure A3B shows the leftward shift of the income distribution during the onset of the pandemic. Narrow targeting based on proxies of the permanent component of income, which might be hard to timely update during crises, may exclude many middle- or high-income households that are vulnerable to sliding into poverty.

We estimate equation (2) by pre-pandemic income groups. We find that the program substantially attenuated the impacts of business closures on the probability of going hungry in middle income households (see Column 5 in Panel B of Appendix Table A8).

Our results suggest that there could be important unintended consequences of preexisting cash aid programs with broad coverage during crises when governments face the challenge of rapidly expanding social programs. In the case of the Bolivian noncontributory pension program, the program quickly provided support to vulnerable sub-populations: low-income households, and middle-income households that experienced a business closure induced by the pandemic.

VIII. Policy Implications and Conclusion

Amid the coronavirus pandemic, some countries have implemented near-universal programs, while others have applied narrow targeting methods. One key question related to the effectiveness of near-universal programs is whether the impacts across all income levels are sufficient to justify their broad coverage, or whether the impacts of these programs could be magnified through targeting (Banerjee, Niehaus and Suri, 2019).

We find that an ongoing near-universal noncontributory pension program in Bolivia had important positive impacts on resilience and food security, with particularly large impacts for low-income households and also for middle-income households that experienced a large labor market shock.

Our results suggest that narrowly targeting cash transfers to the poor would miss the positive consumption-smoothing impacts for middle-income households that are vulnerable to falling into poverty due to labor market shocks. The evidence from Bolivia suggests that, during an economic crisis, an established, near-universal noncontributory pension program can quickly achieve the same primary goals as a social safety net composed of targeted transfers to the poor and an UI program. Given the potential delays in the implementation of new social programs, strengthening preexisting programs may lead to a timely delivery of financial relief to households during the COVID-19 crisis.

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IX. Figures and Tables

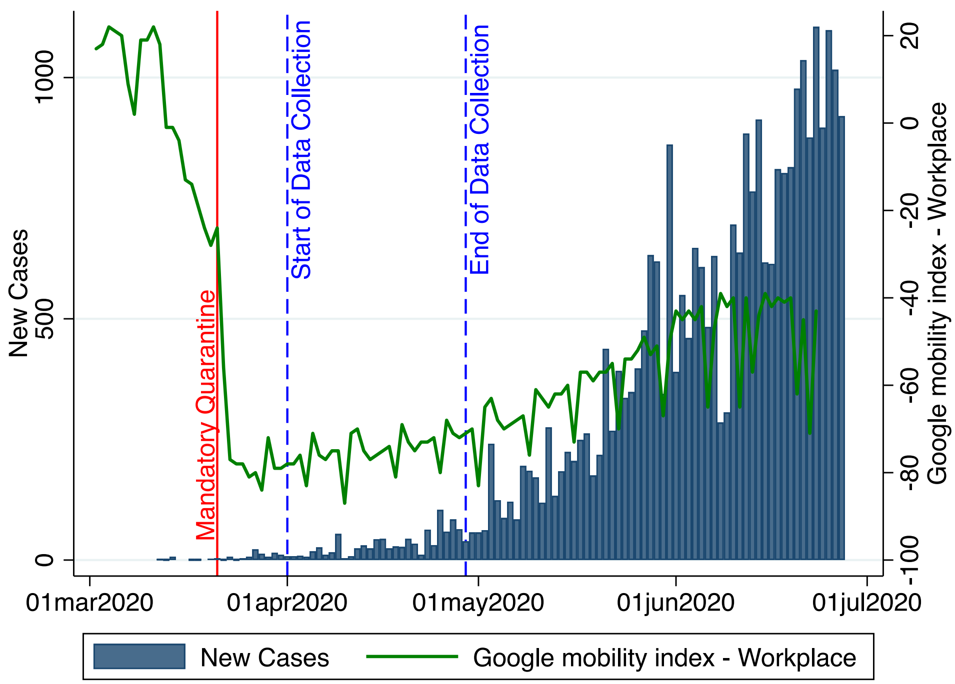

Figure 1. Data collection timeline and the spread of COVID-19 in Bolivia

Note: Own calculations based on data from Max Roser and Hasell (2020) on COVID-19 cases in Bolivia over time, and the Google Mobility Report for mobility trends in the workplace for Bolivia. The Google mobility index shows the percentage change in mobility to geographic locations classified as workplaces relative to a baseline level.

Table 1—Summary Statistics

	N	Mean	Std. Dev.	Min	Max
Respondent's Characteristics					
Gender (female)	5627	0.63	0.48	0	1
Age	5627	34.82	11.86	18	79
Marital/Civil status	5627	0.37	0.48	0	1
Education (Respondent)					
None	5627	0.00	0.03	0	1
Primary	5627	0.01	0.08	0	1
Secondary	5627	0.14	0.35	0	1
Technical/Vocational	5627	0.20	0.40	0	1
Univesity	5627	0.52	0.50	0	1
Graduate degree	5627	0.13	0.34	0	1
Household characteristics					
Number of household members	5360	5.45	2.87	1	16
Number of children	5627	0.93	1.22	0	5
Days since of data collection (wrt 4/02/2020)	5627	21	7	0	29
Household Resilience					
Reduced Income	5236	0.17	0.38	0	1
Can cover a shock	5619	0.30	0.46	0	1
Enough resources (>week)	5627	0.52	0.50	0	1
Enough food (>week)	5627	0.33	0.47	0	1
Health (household level)					
Went hungry	5627	0.18	0.39	0	1
Eats less healthy	5303	0.42	0.49	0	1
Stopped receiving med care	3022	0.16	0.36	0	1
Stress(respondent)					
Stressed about the pandemic (overall situation)	5555	0.87	0.34	0	1
Stressed about the health of family members	5549	0.90	0.30	0	1
Livelihood loss (household level)					
Lost job (past month)	4458	0.43	0.50	0	1
Closed business (past month)	3860	0.68	0.47	0	1

Note: The table presents summary statistics using the sample of households in which the age of the oldest member is between 55 and 65 years old by the time the data was collected.

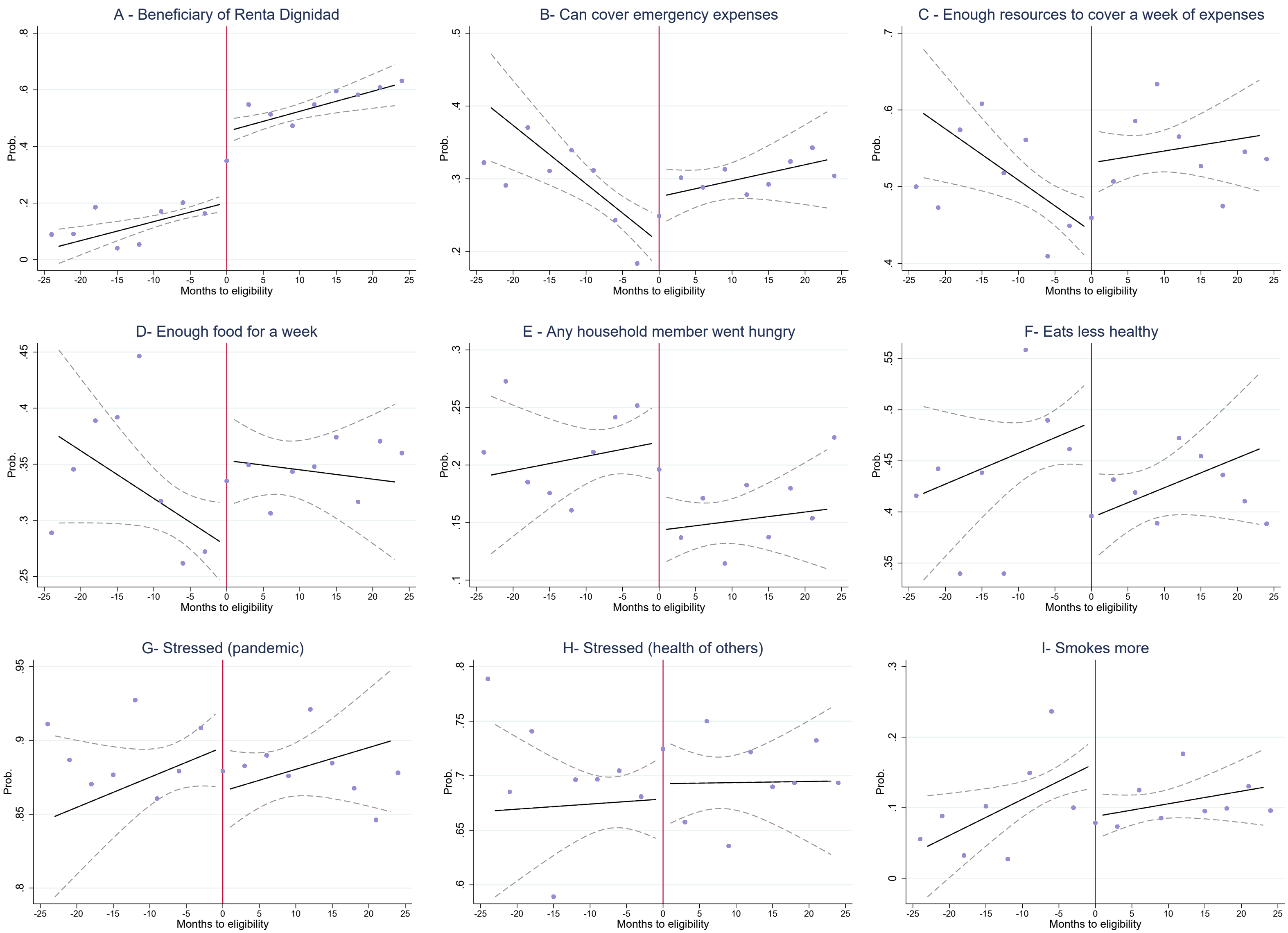

Figure 2. Discontinuities at the cutoff for main outcomes

Note: The figure reports means corresponding to three-month bins around the cutoff determining program eligibility, and linear fits on each side of the cutoff using triangular kernels and a 24-month bandwidth.

Table 2—Effects on household financial resilience, food security, and stress

	Received Transfer (1)	Can cover a shock (2)	Enough resources (>week) (3)	Enough food (>week) (4)	Went hungry (5)	Eats less healthy (6)	Stressed (pandemic) (7)	Stressed (health) (8)	Smokes more (9)
Panel A: Bandwidth -12 to 12									
Above cutoff	0.206***	0.0720	0.122**	0.101*	-0.123***	-0.0675	-0.0729**	-0.0400	-0.0201
	(0.0560)	(0.0478)	(0.0569)	(0.0559)	(0.0451)	(0.0609)	(0.0371)	(0.0340)	(0.0444)
Adjusted q-value		0.19	0.11	0.13	0.05	0.268	0.12	0.27	0.65
Mean (Below cutoff)	0.19	0.24	0.45	0.29	0.22	0.47	0.90	0.94	0.13
N	1183	1183	1183	1183	1183	1110	1167	1171	746
Panel B: Bandwidth -24 to 24									
Above cutoff	0.213***	0.0595*	0.127***	0.0781**	-0.0920***	-0.0831*	-0.0431	-0.0279	-0.0645**
	(0.0393)	(0.0353)	(0.0406)	(0.0397)	(0.0321)	(0.0438)	(0.0270)	(0.0239)	(0.0319)
Adjusted q-value		0.12	0.01	0.09	0.02	0.09	0.13	0.24	0.09
Mean (Below cutoff)	0.16	0.26	0.48	0.30	0.22	0.46	0.89	0.94	0.12
N	2085	2084	2085	2085	2085	1974	2060	2064	1317
Panel C: Bandwidth -36 to 36									
Above cutoff	0.245***	0.0354	0.106***	0.0577*	-0.0855***	-0.0614*	-0.0171	-0.0200	-0.0553**
	(0.0328)	(0.0300)	(0.0340)	(0.0333)	(0.0266)	(0.0366)	(0.0229)	(0.0194)	(0.0274)
Adjusted q-value		0.32	0.01	0.15	0.01	0.15	0.45	0.35	0.12
Mean (Below cutoff)	0.15	0.27	0.48	0.31	0.21	0.45	0.89	0.93	0.11
N	3056	3054	3056	3056	3056	2896	3019	3025	1921
Panel D: Optimal Bandwidth									
Above cutoff	0.205***	0.0679*	0.120**	0.0845*	-0.113***	-0.0836	-0.0744**	-0.0279	-0.0585
	(0.0511)	(0.0409)	(0.0561)	(0.0434)	(0.0411)	(0.0510)	(0.0345)	(0.0240)	(0.0361)
Adjusted q-value		0.1208476	0.09	0.10	0.05	0.12	0.09	0.24	0.12
Mean (Below cutoff)	0.18	0.25	0.45	0.30	0.22	0.47	0.89	0.94	0.13
Number of observations (total)	1399	1605	1263	1751	1399	1513	1381	2064	1057
Bandwidth (-/+)	14.2	17.1	12.2	20.0	14.5	17.5	14.0	24.0	18.5
Number of obs (left of the cutoff)	870	957	796	1024	870	895	856	1172	621
Number of obs (right of the cutoff)	638	768	565	857	638	727	630	1041	519

$***p < 0.01, **p < 0.05, ***p < 0.1$

Note: The table reports RD estimates corresponding to equation (1). Results for each outcome are reported in each column. All regressions include linear trends of the running variable on each side of the cutoff, as well as demographic controls, state fixed effects, and date-of-data-collection fixed effects. Demographic controls include the respondents' age, gender, civil status (single vs. married or cohabiting), and educational attainment (primary, secondary, college, graduate studies). We also control for household size and the number of school-age children in the household. Robust standard errors are reported in parentheses. All regressions use a triangular weights. Panel A, B and C report results using bandwidths of 12, 24 and 36 months before and after the eligibility threshold (60 years old in March 2020). Panel D reports RD estimates using the bandwidth selection procedure of Calonico, Cattaneo and Farrell (2019). Benjamini, Krieger and Yekutieli (2006)'s Adjusted q-values are computed using the 6 variables in columns 2 to 9 for each panel. The number of observations in column 9 is smaller as answers of respondents that selected the "non applicable" option were coded as missing.

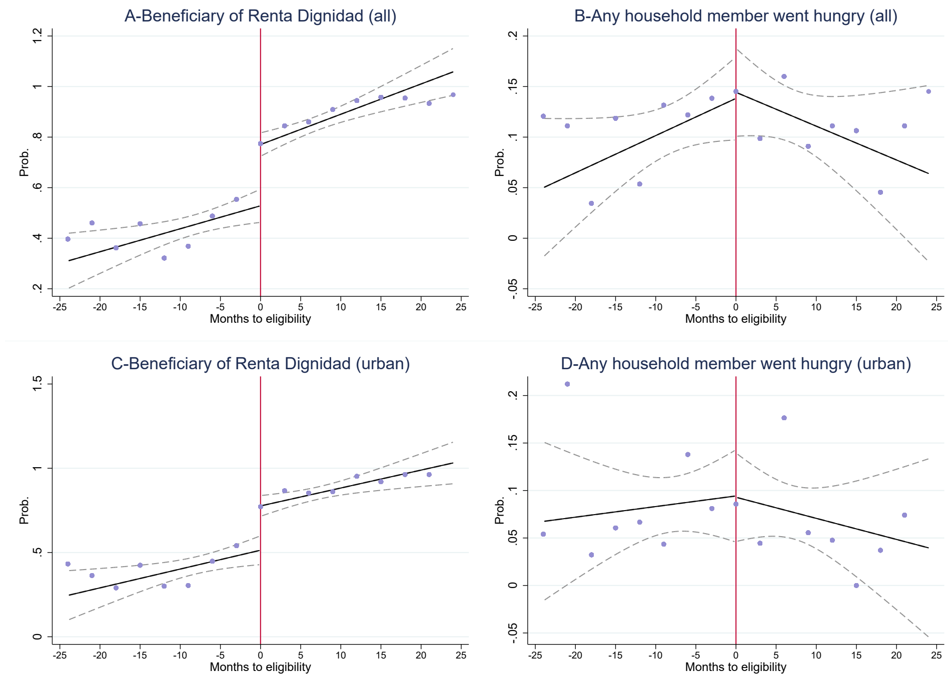

Figure 3. Effects of Renta Dignidad on hunger before the pandemic

Note: The figure reports means corresponding to three-month bins around the cutoff determining program eligibility, and linear fits on each side of the cutoff using triangular kernels and a 24-month bandwidth. The sample includes observations of the 2016 to 2018 waves of the Bolivian Household Surveys conducted by the National Institute of Statistics (INE). The figures in the top panels depict results using all observations, while the figures in the bottom two panels depict results restricting the sample to urban households.

Table 3—RD effects on hunger using pre-pandemic data

	-12 to 12		-24 to 24		-36 to 36	
	Received Transfer	Went hungry	Received Transfer	Went hungry	Received Transfer	Went hungry
	(1)	(2)	(3)	(4)	(5)	(6)
Above cutoff	0.208**	0.0291	0.318***	0.0446	0.364***	0.0384
	(0.0875)	(0.0890)	(0.0645)	(0.0622)	(0.0535)	(0.0512)
Mean (Below cutoff)	0.50	0.13	0.45	0.11	0.43	0.10
N	295	295	569	569	839	839

$* * *p < 0.01, * * p < 0.05, * * *p < 0.1$

Note: The table reports RD estimates corresponding to equation (1) using data from the 2016 to 2018 Household Survey ways conducted by INE. Results for each outcome are reported in each column. All Regressions include linear trends of the running variable on each side of the cutoff but do not include demographic controls. All regressions use a triangular weights. Robust standard errors are presented in parentheses. *Went hungry* is coded as 1 if any household member went hungry and could not eat during the three months preceding the interview.

Table 4—Heterogeneous effects by exposure to shocks induced by the pandemic

Panel A: Business closures during the pandemic									
	Business closure	Can cover a shock	Enough resources (>week)	Enough food (>week)	Went hungry	Eats less healthy	Stressed (pandemic)	Stressed (health)	Smokes more
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Business closure X Above cutoff		0.0497	-0.112*	-0.0388	-0.0977**	0.121*	-0.00782	-0.0577*	-0.104**
		(0.0575)	(0.0602)	(0.0614)	(0.0382)	(0.0640)	(0.0418)	(0.0310)	(0.0481)
Above cutoff	-0.0133	0.0261	0.206***	0.134**	-0.0335	-0.221***	-0.0528	0.0151	0.0242
	(0.0459)	(0.0610)	(0.0623)	(0.0640)	(0.0379)	(0.0671)	(0.0430)	(0.0330)	(0.0503)
Business closure		-0.157***	-0.0636	-0.0717*	0.192***	0.00255	0.0358	0.0311	0.0724**
		(0.0388)	(0.0417)	(0.0415)	(0.0266)	(0.0448)	(0.0277)	(0.0222)	(0.0312)
Adjusted q-value (interaction)		0.52	0.10	0.60	0.08	0.10	0.85	0.10	0.10
Mean (Below cutoff)	0.69	0.26	0.48	0.30	0.22	0.46	0.89	0.94	0.12
$\frac{\beta_2 * Share}{\beta}$		0.56	-0.58	-0.24	0.66	-0.59	0.09	1.63	1.52
N	1455	1454	1455	1455	1455	1382	1436	1441	966

Panel B: Job loss during the pandemic									
	Job loss	Can cover a shock	Enough resources (>week)	Enough food (>week)	Went hungry	Eats less healthy	Stressed (pandemic)	Stressed (health)	Smokes more
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Job Loss X Above cutoff		0.0673	-0.0324	-0.0263	-0.0500	-0.0213	0.00281	-0.0169	0.00191
		(0.0459)	(0.0520)	(0.0514)	(0.0419)	(0.0567)	(0.0343)	(0.0299)	(0.0416)
Above cutoff	-0.0525	0.0531	0.158***	0.0975*	-0.0560*	-0.0908*	-0.0355	-0.0460	-0.0585
	(0.0470)	(0.0489)	(0.0493)	(0.0515)	(0.0316)	(0.0548)	(0.0362)	(0.0314)	(0.0425)
Job Loss		-0.225***	-0.161***	-0.0767**	0.255***	0.119***	0.0435*	0.00191	-0.000588
		(0.0307)	(0.0353)	(0.0345)	(0.0299)	(0.0390)	(0.0233)	(0.0188)	(0.0298)
Adjusted q-value (interaction)		0.93	0.94	0.94	0.93	0.94	0.96	0.94	0.96
Mean (Below cutoff)	0.46	0.26	0.48	0.30	0.22	0.46	0.89	0.94	0.12
$\frac{\beta_2 * Share}{\beta}$		0.35	-0.10	-0.13	0.28	0.09	-0.04	0.14	-0.01
N	1670	1669	1670	1670	1670	1588	1653	1655	1067

$***p < 0.01, **p < 0.05, ***p < 0.1$

Note: The table reports estimates corresponding to equation (2). Results for each outcome are reported in each column. All regressions include linear trends of the running variable on each side of the cutoff, as well as demographic controls, state fixed effects, and date-of-data-collection fixed effects. Demographic controls include the respondents' age, gender, civil status (single vs. married or cohabiting), and educational attainment (primary, secondary, college, graduate studies). We also control for household size and the number of school-age children in the household. All regressions are estimated using a bandwidth of 24 months before and after the eligibility threshold (60 years old in March 2020) and triangular kernels. Robust standard errors are reported in parentheses. Panel A reports results using business closures during the pandemic as a measure of shocks. Observations of households without businesses before the pandemic are coded as missing. Panel B reports results using job losses as a measure of shocks. Observations of households that, before the pandemic, did not obtain income from paid work are coded as missing. Benjamini, Krieger and Yekutieli (2006)'s Adjusted q-values are computed using the 8 variables in columns 2 to 9 for each panel.

Table 5—Heterogeneity by pre-pandemic income

	Can cover a shock	Enough resources (>week)	Enough food (>week)	Went hungry	Eats less healthy	Stressed (pandemic)	Stressed (health)	Smokes more
Panel A: Low income								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Above cutoff	0.0274	0.208***	0.0832	-0.128**	-0.107	-0.0234	-0.0426	-0.109**
	(0.0433)	(0.0602)	(0.0571)	(0.0611)	(0.0711)	(0.0402)	(0.0395)	(0.0506)
Adjusted q-value	0.56	0.00	0.23	0.10	0.23	0.56	0.37	0.10
Mean (Below cutoff)	0.10	0.27	0.20	0.36	0.54	0.92	0.94	0.14
N	827	828	828	828	761	812	813	523
Panel B: Middle income								
Above cutoff	0.0602	0.0650	0.0492	-0.0356	-0.0926	-0.0264	-0.0225	-0.0125
	(0.0560)	(0.0600)	(0.0589)	(0.0385)	(0.0650)	(0.0423)	(0.0337)	(0.0471)
Adjusted q-value	0.61	0.61	0.61	0.61	0.61	0.61	0.61	0.79
P-value (diff with low income)	0.63	0.08	0.67	0.19	0.88	0.96	0.69	0.14
Mean (Below cutoff)	0.30	0.57	0.34	0.13	0.41	0.87	0.95	0.09
N	964	964	964	964	926	958	961	625
Panel C: High income								
Above cutoff	0.125	-0.0509	0.147	-0.0355	0.118	0.0330	0.0578	0.0395
	(0.119)	(0.0936)	(0.117)	(0.0504)	(0.124)	(0.0871)	(0.0762)	(0.167)
Adjusted q-value	0.77	0.77	0.78	0.77	0.77	0.81	0.77	0.81
P-value (diff with low income)	0.38	0.01	0.58	0.21	0.08	0.51	0.19	0.30
Mean (Below cutoff)	0.63	0.84	0.51	0.05	0.41	0.83	0.89	0.16
N	283	283	283	283	278	281	281	163

$***p < 0.01, **p < 0.05, ***p < 0.1$

Note: The table reports RD estimates corresponding to equation (1) using a bandwidth of 24 months before and after the age eligibility cutoff (March 2020) and triangular kernels. All regressions include linear trends of the running variable on each side of the cutoff, as well as demographic controls, state fixed effects, and date-of-data-collection fixed effects. Demographic controls include the respondents' age, gender, civil status (single vs. married or cohabiting), and educational attainment (primary, secondary, college, graduate studies). We also control for household size and the number of school-age children in the household. Robust standard errors are reported in parentheses. Panels A, B, and C report results for the subsample of households with total January 2020 income below the national monthly minimum wage (<$ USD 300), between one and 4 times the national monthly minimum wage ($ USD 300 to $ USD 1,200), and over 4 times the national monthly minimum wage (> $ 1,200), respectively. Benjamini, Krieger and Yekutieli (2006)'s Adjusted q-values are computed using the 8 variables in columns 2 to 9 for each panel.

Supplementary Figures and Tables

Table A1—Comparative statistics for online and household survey data - Households with elderly members

	All households			24 month bandwidth		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	Online	**Field**	**Field Urban**	**Online**	**Field**	**Field Urban**
Panel A - Household characteristics						
Household size	4.38	3.47	3.69	4.65	3.70	3.87
Children under 5 years old in household (%)	0.33	0.11	0.11	0.26	0.15	0.14
# of children enrolled in school	1.84	0.51	0.52	1.69	0.59	0.61
Panel B - Household prepandemic income (relative to the national minimum wage)						
0-0.5 MW	0.07	0.17	0.03	0.06	0.14	0.02
0.5-1 MW	0.10	0.12	0.06	0.07	0.09	0.05
1-2 MW	0.30	0.20	0.20	0.29	0.21	0.21
2-3 MW	0.14	0.16	0.20	0.17	0.18	0.21
3-4 MW	0.15	0.12	0.16	0.15	0.13	0.16
4-6 MW	0.14	0.13	0.20	0.13	0.14	0.19
6-8 MW	0.05	0.06	0.09	0.07	0.07	0.10
8-11 MW	0.03	0.03	0.04	0.03	0.03	0.05
11+ MW	0.01	0.01	0.02	0.03	0.01	0.01
Panel C- Individual characteristics						
Female	0.52	0.52	0.53	0.56	0.51	0.52
No education	0.01	0.14	0.06	0.01	0.08	0.04
Completed primary	0.05	0.33	0.25	0.06	0.31	0.23
Completed secondary	0.45	0.28	0.32	0.49	0.33	0.35
College or vocational training	0.49	0.25	0.36	0.43	0.28	0.38
Age	34.23	53.87	51.24	34.19	47.03	45.20

Note: The table reports averages of households and individual characteristics corresponding to households of which the oldest household member is at least 55 years old (Columns 1 to 3), and to households whose oldest member age is withing 24 months on each side of the eligibility cutoff (Columns 4 to 6). Columns 1 and 4 report means using data collected online during the pandemic. Columns 2 and 5 report means using data from the 2018 wave of the Bolivian household survey collected through field visits by INE (National Institute of Statistics). Columns 3 and 6 report means focusing only on field-survey observations from households in urban areas. Variables in Panel C refer to characteristics of the respondent in the case of the online data, and to characteristics of all household members in case of the household-survey data. Survey weights were used to compute means. See Bottan, Hoffmann and Vera-Cossio (2020*a*) for a description of the construction of survey weights for the online sample.

Table A2—Manipulation test

Method	**T**	$P > \|T\|$
Conventional	-1.48	0.14
Robust	0.60	0.55

Note: The table reports results from the Manipulation test proposed by Cattaneo, Jansson and Ma (2019) estimated using local quadratic approximations using optimally selected bandwidths of 11 and 13 months to the left and right of the cutoff.

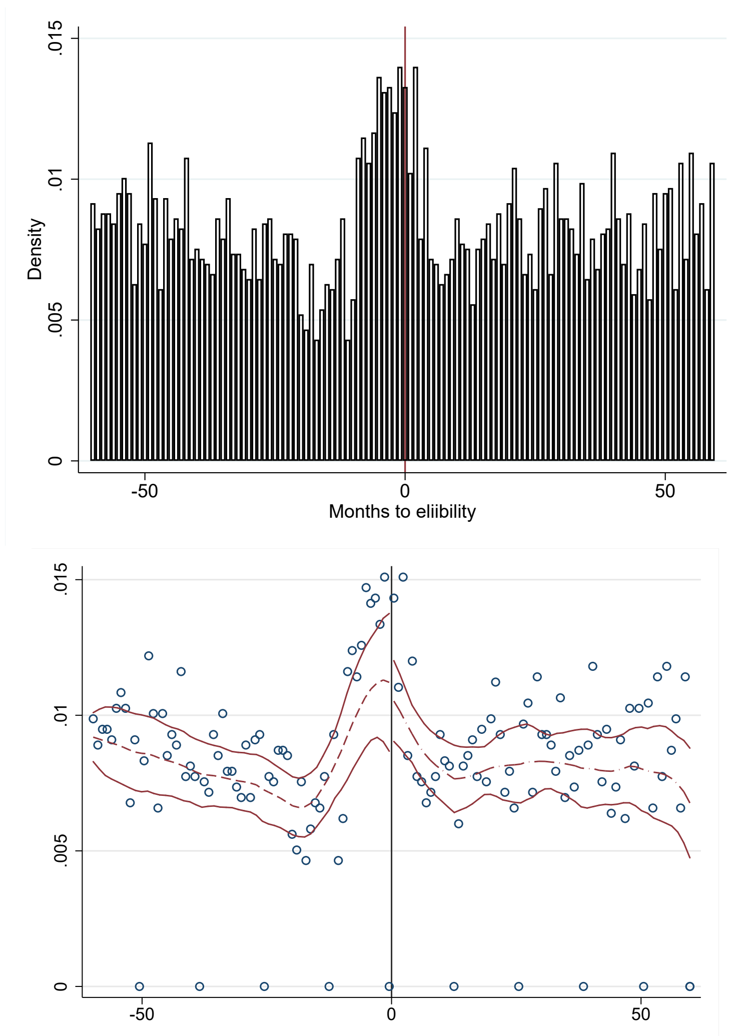

FIGURE A1. DISTRIBUTION OF AGE TO ELIGIBILITY (RUNNING VARIABLE)

Note: The figures reports the histogram corresponding to the month of birth of the oldest person in the respondents household normalized with respect to March 2020, the month preceding data collection, and the (McCrary, 2008) test for differences in densities around the cutoff. Negative numbers denote ineligible households while positive numbers denote eligible households.

Table A3—Balance of covariates around the cutoff

			Bandwidth					
			12		24		36	
	Mean Below cutoff	Mean Above cutoff	RD Difference	P-value	RD Difference	P-value	RD Difference	P-value
Respondent's Characteristics								
Gender (female)	0.62	0.62	-0.071	0.202	-0.029	0.472	-0.021	0.522
Age	33.56	36.95	0.869	0.511	1.533	0.109	0.726	0.364
Marital/Civil status	0.37	0.37	-0.079	0.160	-0.060	0.141	-0.061*	0.071
Education (Respondent)								
None	0.00	0.00	-0.001	0.599	-0.003	0.159	-0.003	0.144
Primary	0.01	0.00	0.023	0.123	0.012	0.229	0.008	0.309
Secondary	0.16	0.12	0.067*	0.091	0.022	0.441	0.019	0.425
Technical/Vocational	0.22	0.19	-0.050	0.271	-0.046	0.158	-0.048*	0.083
University	0.49	0.51	-0.035	0.542	-0.007	0.872	0.007	0.846
Graduate degree	0.12	0.17	-0.003	0.927	0.022	0.416	0.017	0.455
Household characteristics								
Number of household members	5.51	5.52	-0.026	0.939	0.173	0.479	0.145	0.478
Number of children	0.97	1.01	0.167	0.226	0.094	0.350	0.064	0.448
Days since last day of data collection (wrt 4/30/2020)	10.38	10.14	0.503	0.491	0.714	0.180	0.746*	0.097
P-val (All coefficients=0)				0.20		0.11		0.12

Note: The table reports means of demographic characteristics for households whose oldest member was between 55-60 years old at the time of data collection (Below cutoff), and for households whose oldest member was between 60-65 years old (Above cutoff). The table also report RD estimates corresponding to equation (1) using each covariate as a dependent variable for different bandwidths. RD regressions use triangular weights.

Table A4—Robustness to excluding covariates

	Received Transfer	Can cover a shock	Enough resources (>week)	Enough food (>week)	Went hungry	Eats less healthy	Stressed (pandemic)	Stressed (health)	Smokes more
Panel A: Bandwidth -12 to 12									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Above cutoff	0.240***	0.0619	0.0771	0.0983*	-0.111**	-0.0466	-0.0753**	-0.0527	-0.00599
	(0.0535)	(0.0493)	(0.0574)	(0.0543)	(0.0437)	(0.0583)	(0.0382)	(0.0333)	(0.0412)
Mean (Below cutoff)	0.19	0.24	0.45	0.29	0.22	0.47	0.90	0.94	0.13
N	1271	1269	1271	1271	1271	1189	1251	1256	805
Panel B: Bandwidth -24 to 24									
Above cutoff	0.245***	0.0618*	0.105**	0.0747*	-0.0890***	-0.0766*	-0.0390	-0.0383*	-0.0468
	(0.0383)	(0.0363)	(0.0415)	(0.0390)	(0.0320)	(0.0423)	(0.0271)	(0.0231)	(0.0312)
Mean (Below cutoff)	0.16	0.26	0.48	0.30	0.22	0.46	0.89	0.94	0.12
N	2238	2235	2238	2238	2238	2112	2208	2213	1419
Panel C: Bandwidth -36 to 36									
Above cutoff	0.268***	0.0418	0.0853**	0.0554*	-0.0831***	-0.0614*	-0.0174	-0.0287	-0.0388
	(0.0318)	(0.0306)	(0.0347)	(0.0326)	(0.0267)	(0.0355)	(0.0227)	(0.0189)	(0.0268)
Mean (Below cutoff)	0.15	0.27	0.48	0.31	0.21	0.45	0.89	0.93	0.11
N	3295	3291	3295	3295	3295	3109	3250	3258	2067

$***p < 0.01, **p < 0.05, ***p < 0.1$

Note: The table reports RD estimates corresponding to equation (1). Results for each outcome are reported in each column. All Regressions include linear trends of the running variable on each side of the cutoff but do not include demographic controls. Panel A uses a bandwidth of 12 months before and after the cutoff, while panels B and C use 24- and 36-month bandwidths. All regressions use a triangular weights. Robust standard errors are presented in parentheses.

Table A5—Robustness to higher order polynomials

Panel A: Bandwidth -12 to 12									
	Received Transfer	Can cover a shock	Enough resources (>week)	Enough food (>week)	Went hungry	Eats less healthy	Stressed (pandemic)	Stressed (health)	Smokes more
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Above cutoff	0.133	0.0180	0.182**	0.119	-0.163**	-0.0917	-0.0945	-0.0618	0.0355
	(0.0910)	(0.0730)	(0.0910)	(0.0880)	(0.0669)	(0.0955)	(0.0592)	(0.0545)	(0.0759)
Mean (Below cutoff)	0.19	0.24	0.45	0.29	0.22	0.47	0.90	0.94	0.13
N	1183	1183	1183	1183	1183	1110	1167	1171	746
Panel B: Bandwidth -24 to 24									
Above cutoff	0.187***	0.0686	0.111*	0.101*	-0.118**	-0.0828	-0.0821**	-0.0494	-0.0299
	(0.0576)	(0.0502)	(0.0590)	(0.0575)	(0.0463)	(0.0635)	(0.0389)	(0.0352)	(0.0460)
Mean (Below cutoff)	0.16	0.26	0.48	0.30	0.22	0.46	0.89	0.94	0.12
N	2085	2084	2085	2085	2085	1974	2060	2064	1317
Panel C: Bandwidth -36 to 36									
Above cutoff	0.185***	0.0684	0.135***	0.0877*	-0.105***	-0.103**	-0.0541*	-0.0363	-0.0607
	(0.0465)	(0.0417)	(0.0479)	(0.0467)	(0.0380)	(0.0517)	(0.0321)	(0.0286)	(0.0370)
Mean (Below cutoff)	0.15	0.27	0.48	0.31	0.21	0.45	0.89	0.93	0.11
N	3056	3054	3056	3056	3056	2896	3019	3025	1921

$***p < 0.01, **p < 0.05, ***p < 0.1$

Note: The table reports RD estimates corresponding to equation (1). Results for each outcome are reported in each column. All regression include linear and quadratic trends of the running variable on each side of the cutoff as well as demographic controls, state fixed effects, and date-of-data-collection fixed effects. Robust standard errors are reported in parentheses. Panel A uses a bandwidth of 12 months before and after the cutoff, while panels B and C use 24- and 36-month bandwidths. All regressions use a triangular weights.

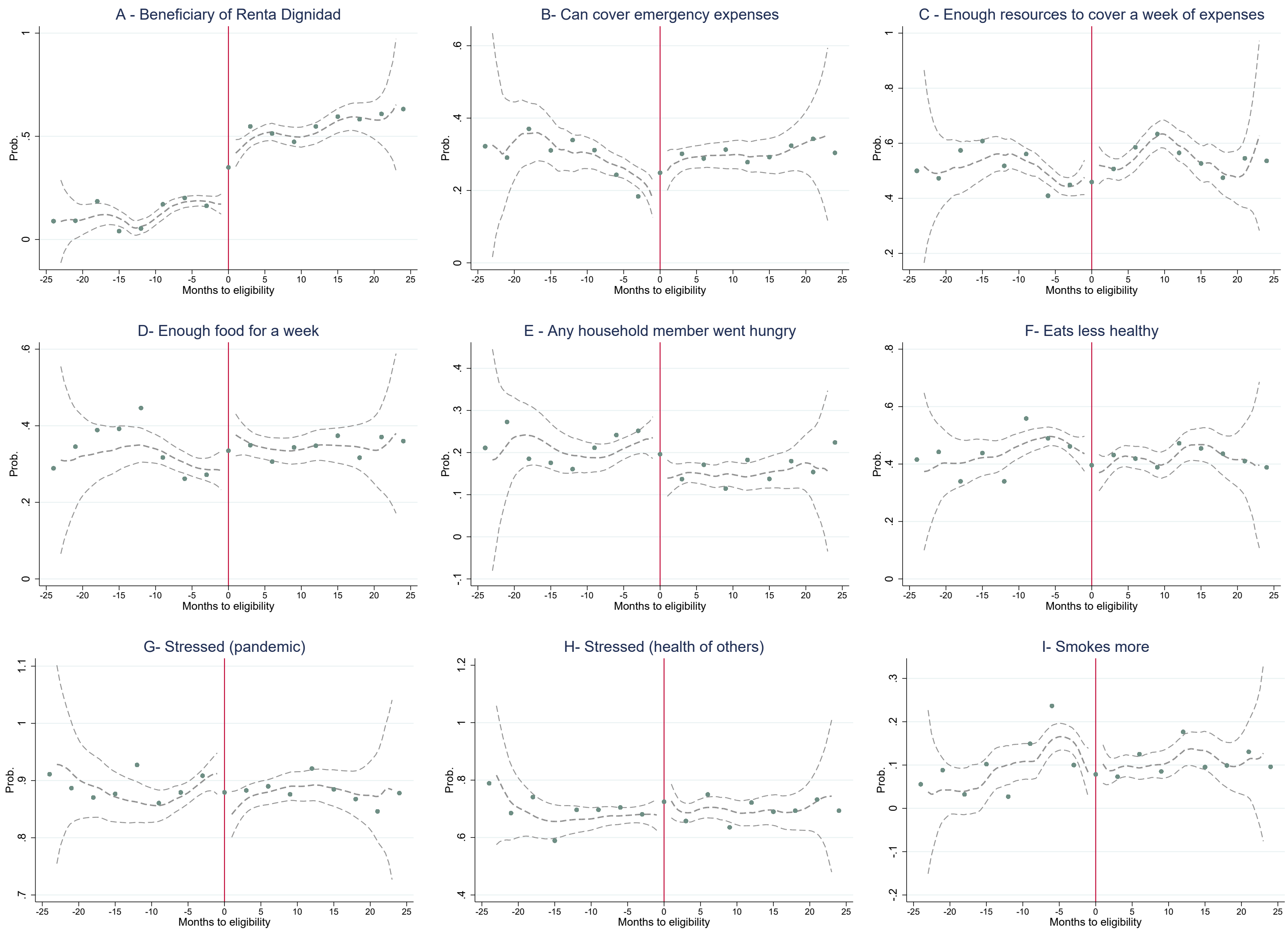

Figure A2. Discontinuities at the cutoff for main outcomes (non parametric estimates)

Note: The figure reports means corresponding to three-month bins around the cutoff determining program eligibility, and local linear regression estimates using triangular weights over a bandwidth of 24 months on each side of the cutoff. Dashed lines report 90% confidence intervals.

Table A6—Robustness to placebo cutoff dates

Panel A: Cutoff in -24	Received Transfer	Can cover a shock	Enough resources (>week)	Enough food (>week)	Went hungry	Eats less healthy	Stressed (pandemic)	Stressed (health)	Smokes more
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Above cutoff	-0.0336	0.0218	0.0174	-0.00431	-0.0350	-0.0552	0.00563	-0.0199	-0.000157
	(0.0471)	(0.0437)	(0.0451)	(0.0457)	(0.0360)	(0.0488)	(0.0334)	(0.0226)	(0.0369)
Mean (Below cutoff)	0.57	0.31	0.53	0.35	0.17	0.43	0.87	0.95	0.11
N	1988	1985	1988	1988	1988	1879	1963	1966	1274
Panel B: Cutoff in +24									
Above cutoff	-0.0131	0.0431	0.0215	0.00915	0.0523	-0.0523	0.0226	0.0116	-0.0866***
	(0.0283)	(0.0452)	(0.0486)	(0.0445)	(0.0383)	(0.0503)	(0.0309)	(0.0275)	(0.0320)
Mean (Below cutoff)	0.09	0.28	0.49	0.31	0.19	0.41	0.86	0.91	0.09
N	1974	1972	1974	1974	1974	1870	1952	1953	1211

$***p < 0.01, **p < 0.05, ***p < 0.1$

Note: The table reports RD estimates corresponding to equation (1) using two placebo cutoffs. Panel A reports results using March 2018 as a placebo cutoff (24 months before the actual age eligibility cutoff). Panel B reports results using March 2022 as a placebo cutoff (24 months after the actual age eligibility cutoff). Results for each outcome are reported in each column. All regressions include linear trends of the running variable on each side of the cutoff as well as demographic controls, state fixed effects, and date-of-data-collection fixed effects. Robust standard errors are reported in parentheses. Regressions are estimated over a bandwidth of 24 months before and after the placebo cutoffs. All regressions use a triangular weights.

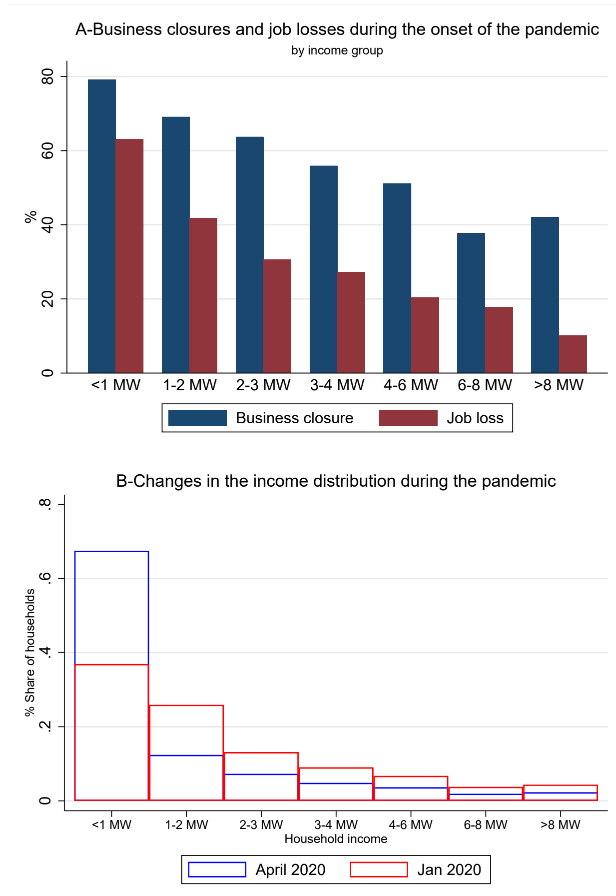

Figure A3. Business closures, job losses, and changes in the income distribution during the pandemic

Note: Panel A depicts the share of households reporting business closures and job losses during the onset of the pandemic by total household income in January 2020. Panel B shows the share of households by income category corresponding to pre-pandemic income (January 2020) and income during the pandemic (April 2020). In both panels, the shares are computed over the sample of households of which the age of the oldest household member was between 55 and 65 years old during the data collection period (April 2020).

Table A7—Robustness of effects by business closures to alternative bandwidths

Panel A: -12 to 12									
	Business closure (1)	Can cover a shock (2)	Enough resources (>week) (3)	Enough food (>week) (4)	Went hungry (5)	Eats less healthy (6)	Stressed (pandemic) (7)	Stressed (health) (8)	Smokes more (9)
Business closure X Above cutoff		0.0186	-0.114	-0.0749	-0.0867*	0.133	0.0159	-0.0494	-0.143**
		(0.0748)	(0.0791)	(0.0804)	(0.0498)	(0.0845)	(0.0545)	(0.0421)	(0.0631)
Above cutoff	0.0224	0.0514	0.205**	0.196**	-0.0749	-0.239***	-0.0954	0.00371	0.110
	(0.0635)	(0.0825)	(0.0862)	(0.0880)	(0.0551)	(0.0921)	(0.0623)	(0.0470)	(0.0719)
Business closure		-0.102**	-0.0259	-0.0218	0.195***	0.0000599	0.00964	0.0233	0.0884**
		(0.0484)	(0.0521)	(0.0518)	(0.0326)	(0.0566)	(0.0324)	(0.0264)	(0.0387)
Mean (Below cutoff)	0.71	0.24	0.45	0.29	0.22	0.47	0.90	0.94	0.13
$\frac{\beta_2 * Share}{\beta}$		0.20	-0.60	-0.35	0.44	-0.60	-0.13	1.13	-7.05
N	839	839	839	839	839	787	826	830	557
Panel B: -36 to 36									
	Business closure (1)		Enough resources (>week) (2)	Enough food (>week) (3)	Went hungry (4)	Eats less healthy (5)	Stressed (pandemic) (6)	Stressed (health)	Smokes more (7)
Job Loss X Above cutoff		0.0574	-0.0823	-0.0180	-0.0900***	0.0885*	-0.0287	-0.0549**	-0.0745*
		(0.0482)	(0.0504)	(0.0512)	(0.0328)	(0.0535)	(0.0352)	(0.0258)	(0.0398)
Above cutoff	-0.00658	0.00683	0.161***	0.0940*	-0.0298	-0.156***	-0.00422	0.0256	0.00277
	(0.0386)	(0.0511)	(0.0522)	(0.0535)	(0.0313)	(0.0559)	(0.0365)	(0.0272)	(0.0423)
Job Loss		-0.179***	-0.0864**	-0.0985***	0.186***	0.0293	0.0443*	0.0358*	0.0560**
		(0.0331)	(0.0357)	(0.0354)	(0.0233)	(0.0382)	(0.0245)	(0.0193)	(0.0263)
Mean (Below cutoff)	0.68	0.27	0.48	0.31	0.21	0.45	0.89	0.93	0.11
$\frac{\beta_2 * Share}{\beta}$		0.85	-0.53	-0.15	0.67	-0.62	0.82	3.23	1.06
N	2125	2123	2125	2125	2125	2018	2098	2104	1400

$***p < 0.01, **p < 0.05, ***p < 0.1$

Note: The table reports estimates corresponding to equation (2). Results for each outcome are reported in each column. All regressions include linear trends of the running variable on each side of the cutoff, as well as demographic controls, state fixed effects, and date-of-data-collection fixed effects. Regressions in Panel A are estimated using a bandwidth of 12 months before and after the age eligibility threshold (60 years old in March 2020). Regressions in Panel B are estimated using a bandwidth of 24 months before and after the age eligibility threshold (60 years old in March 2020). Robust standard errors are reported in parentheses. All regressions use a triangular weights. Observations of households without businesses before the pandemic are coded as missing.

Table A8—Effects by business closures and pre-pandemic income groups

Panel A: Lower-income households (Jan 2020 monthly total household income < $USD 300)									
	Business closure	Can cover a shock	Enough resources (>week)	Enough food (>week)	Went hungry	Eats less healthy	Stressed (pandemic)	Stressed (health)	Smokes more
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Business closure X Above cutoff		-0.0129	-0.322***	-0.0736	-0.136	0.270**	0.0410	-0.0443	-0.222**
		(0.0751)	(0.106)	(0.106)	(0.0982)	(0.115)	(0.0752)	(0.0607)	(0.0910)
Above cutoff	-0.00567	0.0196	0.482***	0.181*	-0.0272	-0.463***	-0.0832	-0.0366	0.0369
	(0.0642)	(0.0729)	(0.101)	(0.101)	(0.101)	(0.108)	(0.0755)	(0.0556)	(0.0951)
Business closure		0.0105	0.0312	0.00851	0.249***	0.0327	-0.0197	0.0106	0.130***
		(0.0477)	(0.0631)	(0.0653)	(0.0587)	(0.0825)	(0.0411)	(0.0368)	(0.0424)
Adjusted q-value (interaction)		0.86	0.02	0.65	0.34	0.05	0.67	0.65	0.05
Mean (below cut-off)	0.780	0.0995	0.266	0.202	0.365	0.539	0.918	0.938	0.137
$\frac{\beta_2 * Share}{\beta}$		-1.090	-1.124	-0.474	0.798	-0.857	-0.641	0.490	1.266
N	563	562	563	563	563	521	550	553	376
Panel B: Middle-income households (Jan 2020 monthly total household income $USD 300- 1200)									
Business closure X Above cutoff		0.175**	-0.0653	0.00262	-0.0983**	0.120	0.0264	-0.0453	-0.0163
		(0.0869)	(0.0849)	(0.0887)	(0.0433)	(0.0920)	(0.0615)	(0.0437)	(0.0656)
Above cutoff	0.0119	-0.0388	0.0832	0.0572	0.00632	-0.191**	-0.0285	0.0282	-0.00479
	(0.0685)	(0.0908)	(0.0897)	(0.0932)	(0.0395)	(0.0971)	(0.0607)	(0.0465)	(0.0668)
Business closure		-0.180***	-0.0196	-0.0700	0.166***	-0.00928	0.0150	0.0292	0.0118
		(0.0601)	(0.0620)	(0.0618)	(0.0304)	(0.0641)	(0.0418)	(0.0313)	(0.0440)
Adjusted q-value (interaction)		0.18	0.71	0.98	0.18	0.52	0.89	0.60	0.52
Mean (below cut-off)	0.488	0.304	0.567	0.343	0.127	0.406	0.873	0.948	0.0928
$\frac{\beta_2 * Share}{\beta}$		1.517	-1.048	0.0289	1.109	-0.687	-1.513	22.92	0.689
N	688	688	688	688	688	660	684	686	461
Panel C: Higher-income households (Jan 2020 monthly total household income > $USD 1200)									
Business closure X Above cutoff		-0.0872	-0.243	-0.259*	0.00612	0.287	-0.0666	-0.0206	-0.202
		(0.177)	(0.159)	(0.156)	(0.0838)	(0.181)	(0.128)	(0.0829)	(0.206)
Above cutoff	-0.172	0.0114	0.0120	0.148	-0.0501	-0.0158	-0.00855	0.0402	0.314
	(0.157)	(0.171)	(0.127)	(0.144)	(0.0750)	(0.196)	(0.115)	(0.0972)	(0.229)
Business closure		-0.0885	0.0500	0.0298	0.0789	-0.0637	0.0958	0.0373	0.134
		(0.112)	(0.0903)	(0.118)	(0.0604)	(0.129)	(0.0877)	(0.0723)	(0.108)
Adjusted q-value (interaction)		0.83	0.34	0.34	0.34	0.34	0.83	0.83	0.65
Mean (below cut-off)	0.488	0.634	0.845	0.509	0.0459	0.409	0.828	0.888	0.164
$\frac{\beta_2 * Share}{\beta}$		1.410	1.123	-3.704	-0.0583	1.139	0.778	-0.301	-0.409
N	198	198	198	198	198	196	197	197	125

$* * *p < 0.01, * * p < 0.05, * * *p < 0.1$

Note: The table reports estimates corresponding to equation (2). Results for each outcome are reported in each column. All regressions include linear trends of the running variable on each side of the cutoff, as well as demographic controls, state fixed effects, and date-of-data-collection fixed effects. All regressions are estimated over a bandwidth of 24 months before and after the age eligibility threshold (60 years old in March 2020). Robust standard errors are reported in parentheses. All regressions use a triangular weights. Panel A reports results for the subsample of households with total January 2020 income below the national monthly minimum wage (<$ USD 300). Panel B reports results for the subsample of households with total January 2020 income between one and 4 times the national monthly minimum wage ($ USD 300 to $ USD 1,200). Panel C reports results for the subsample of households with total January 2020 income over 4 times the national monthly minimum wage (> $ 1,200). Observations of households without businesses before the pandemic are coded as missing.