Effects of Non-Contributory Pensions on Older Adult Mortality in Rural Mexico*

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Abstract

We study an unconditional cash transfer program aimed at alleviating rural poverty among older adults. Using death records and a triple difference design, we find a 5.5% reduction in mortality for women, mostly due to a decrease in non-cardiovascular related mortality, and inconclusive evidence for men. We explore mechanisms using income and expenditure surveys and a triple difference-in-discontinuities design. We find little evidence of significant changes in key hypothesized mechanisms, except for declines in employment and hours worked.

Keywords: Mortality, Health Policy, Demography

*This version: December 2025. For their valuable comments and suggestions, we want to thank Victoria Barone, Pedro Bernal, Joel Ferguson, Matias Morales, Pablo Muñoz, Marianne Page, and Natalia Testa; and participants at the 2025 NBER Determinants of Mortality meeting, XXI RIDGE Forum - VIII Health Economics Network, and the IX American - European Health Economics Study Group. We are particularly indebted to Kevin Feeney whose prior work motivated our own work. We gratefully acknowledge financial support from NIA P30AG012839, NIA T32AG00037, NIA R01 AG058940, and Postdoctoral Scholarship ANID-Chile (#74230001). We want to thank the Mexican National Statistical Office (INEGI), specifically Dr. Natalia Volkow and Alejandra Robles, for access and data provided through the microdata laboratory and remote processing. The opinions expressed in this paper are those of the authors and do not necessarily reflect the views of the Inter-American Development Bank, its Board of Directors, or the countries they represent, nor of INEGI. All remaining errors are our own. All data used in this study are publicly available from INEGI, and can be obtained through formal request and process via INEGI's established channels.

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doi:10.3368/jhr.0225-14128R1

JEL Codes: I13, I14, I15, I18

1 Introduction

The economic consequences of rapid population aging have become one of the major challenges of this century.[1] According to the United Nations, 10% of the global population was aged 65 or above in 2021, and this is projected to rise to 16% by 2050 (Wilmoth et al., 2023). Among OECD countries, around 13% of individuals aged 66 and above are in poverty, with higher proportions in low- and middle-income countries (OECD, 2023). The most common policy to address older adult poverty has been to implement cash transfer programs, including non-contributory pensions.[2] Despite extensive literature estimating the effects of income on health, it remains unclear whether such cash transfers effectively improve the health of older adults. Empirical progress has also been limited due to a lack of credible evidence on behavioral mechanisms linking income shocks to health outcomes.

Empirical studies on the effects of cash grants on the health and mortality of older adults have produced mixed results, with recent research indicating both beneficial (Jensen and Richter, 2004; Salm, 2011; Barham and Rowberry, 2013; Miglino et al., 2023; Jeon et al., 2025) and adverse effects (Snyder and Evans, 2006; Dobkin and Puller, 2007; Evans and Moore, 2011; Feeney, 2018, 2017; Fitzpatrick and Moore, 2018). Despite the inconclusive findings, these studies consistently hypothesize the mediating roles of labor market activity, healthcare utilization, diet, and cardiovascular disease (CVD) such as hypertension and diabetes. These mechanisms are crucial to explore because they contribute to the leading causes of death, including heart disease and stroke, in many countries. This paper contributes to the literature by examining whether a non-contributory pension program in poor rural areas affects older adult mortality.

We study Mexico's most significant pension reform in the past 20 years: the nationwide non-contributory pension program for the elderly. The program guaranteed a basic pension every two months for individuals who were both age- and residence-eligible, regardless of gender, income, or previous contributions to social security.[3] The program is colloquially known as "70 y Más" and initially targeted individuals aged 70 or older living in rural localities. Due to the program's

[1] It is the inevitable result of the demographic transition—the trend toward longer lives and smaller families—that is taking place even in countries with relatively youthful populations.

[2] There is a large body of literature documenting the beneficial effects of these programs in targeting young children and mothers, summarized by Bastagli et al. (2016); Millán et al. (2019).

[3] Similar to the Mexican program are the non-contributory pensions established in 2006 in Chile and 2011 in Peru; both granted a permanent cash transfer to those aged 65 and above, conditional on income. Miglino et al. (2023) find beneficial effects on mortality due to improved food intake and access to healthcare in Chile, whereas Bernal et al. (2022) find an increase in obesity and chronic disease in Peru.

popularity, it gradually expanded to cover more localities and age groups and began to include means-tested eligibility criteria.

In this paper, we leverage rich administrative data and the timing of the expansions of the *70 y Más* program to evaluate its impact on mortality in the initially targeted rural population. We utilize Mexico's national vital statistics data, which include information on age at death, place of residence, locality size, and causes of death. We construct and classify age-locality group cells as treated or not based on locality size eligibility and age coverage of the program's rural expansion. We particularly focus on the first group covered (in 2007), because it is the most economically vulnerable population: adults aged 70 and over living in rural localities with fewer than 2,500 inhabitants. We use total death counts as the primary outcome, using a triple difference approach with a Poisson regression model that accounts for the total population exposed in every locality-age cell (Chen and Roth, 2024; Mullahy and Norton, 2024). We do not analyze the 2008 and 2009 expansions including localities between 2,501 and 20,000 and between 20,001 and 30,000, respectively, because there were seven other non-contributory programs at the state level that targeted these localities (Aguila et al., 2011). Moreover, the 2009 expansion only reached 48.1% of targeted localities (Juárez and Rodríguez Piña, 2021). Thus, our primary triple-difference empirical strategy analyzes pre-post mortality from 2002 to 2011 for adults ages 70-79 in localities with fewer than 2,500 inhabitants (2007 treated group), comparing them with ineligible adults aged 60-69, and with localities of 30,000-100,000 inhabitants that were not treated until after 2011.

We hypothesized potential gender differences in pension effects on mortality. We find evidence that the non-contributory pension program reduced overall mortality in women by 5.5% (s.e.=0.9%), with results appearing predominantly in non-cardiovascular causes. By contrast, point estimates for men are close to zero and statistically significantly different than those for women; however, pre-trends in our event study estimates for men are significant, thus we cannot confidently claim causal results for men.[4]

We explore possible mechanisms for these results, analyzing changes in individual income, household spending, and other behaviors plausibly related to the cash transfer. We pool repeated cross-sections of the national Mexican household income and expenditure surveys (ENIGH) between 2005 and 2010 to implement a triple difference regression discontinuity design around the eligibility cutoff age. Our regression discontinuity (RD) approach employs a triple difference design that compares outcomes for older adults just above and below the age-70 eligibility cutoff,

[4]Note Miglino et al. (2023) study a parallel universal pension in Chile for older low-income adults, and find significantly beneficial mortality effects for women but insignificant adverse effects for men)

across eligible and ineligible localities, and before and after the program's implementation. The RD estimates first confirm receipt of the *70 y Más* transfer, averaging approximately $25 per eligible person per month (in 2024 USD, inflation-adjusted). Prior to the transfer, the mean monthly income among eligible individuals was $172, meaning the cash transfer was equivalent to 15% of the average income of recipients.

We then examine the available measures of labor market activity, which yield more intriguing results. Prior research has found that pension programs can reduce labor market participation, although the mechanisms may vary. For example, Gruber and Wise (1998) found that many pension systems incentivize retirement, but by contrast *70 y Más* should represent a pure income effect. Galiani et al. (2016) found that one year after implementation of *70 y Más*, there was a significant shift of older adults out of the formal labor market (with some substitution to informal work in family businesses), accompanied by an improvement in the one health indicator that they report, geriatric depression. In contrast, Snyder and Evans (2006) hypothesized that income-induced reductions in labor force participation could potentially explain certain unexpected mortality increases after positive income shocks. Although we do not have measures of social isolation, our data analysis shows a substantial reduction in employment and a slightly significant decrease in hours worked in pre-post difference-in-discontinuities at age 70 between eligible and ineligible localities.

Next, we study household spending patterns, including expenditure on obesogenic foods, which may be linked to a greater increase in CVD- and circulatory disease-related deaths. Such effects would be consistent with the increase in adult obesity and blood pressure estimated to have resulted from Mexico's *Progresa* cash transfers (Fernald et al., 2008). However, we find no significant effects on total household spending, healthcare, or food expenditures, nor on specific categories such as obesogenic food, alcohol, or tobacco (although the confidence intervals are not tight enough to rule out modest effects for some categories).

Our study builds on Feeney (2018, 2017)'s unpublished dissertation work, which report a significant increase in deaths attributed to greater spending on obesogenic foods due to the *70 y Más* program we study. While we adopt a similar empirical strategy, our findings diverge: we do not observe a statistically significant increase in deaths overall or by gender. However, both studies face important data limitations and methodological challenges. First, following recent literature that cautions against transforming non-negative outcomes in linear models, we employ a Poisson regression model, which better fits our data structure. This approach allows us to appropriately handle the large number of zero-death observations across age-locality-year cells. Second, to address limitations in population data without aggregation to higher geographic levels, we interpolate population counts at the locality-age-year level. This approach retains granularity and improves precision in small localities. It also allows us to avoid outcome transformation and to incorporate age-locality and time-varying exposure controls to account for potential migration flows between eligible and ineligible localities during the study period.

The remainder of the paper proceeds as follows: Section 2 describes the institutional background and the *70 y Más* program. Section 3 outlines the data sources and sample construction. Section 4 presents our empirical strategy and the main results. Section 5 discusses the potential mechanisms, and in section 6, we conclude.

2 Mexico's *70 y Más* Non-Contributory Pension Program

In the early 2000s, Mexico had multiple social security systems that provided pensions and health care insurance, but most excluded individuals who did not contribute to these systems (Aguila et al., 2015).[5] To address the economic vulnerability of those ineligible for social security programs, the Mexican government, through its Welfare Department, introduced two major policies in the early 2000s.[6] The first, *Seguro Popular*, launched in 2002, was a large-scale public health insurance program designed to address the lack of basic health coverage. The second, introduced in late 2007, was a non-contributory pension scheme, *Pensión para Adultos Mayores* (*70 y Más*), which is the focus of this paper. This pension program provided unconditional cash transfers to older adults. It is considered non-contributory because neither recipients nor their employers were required to contribute to the fund during their working years. Initially, eligibility was restricted to individuals residing in small cities or villages, known as *localidades*, which met a specific population size criteria.[7]

[5]Social security includes the Mexican Social Security Institute (IMSS), which provides benefits to private sector workers, covering nearly 30% of the country's population. To qualify for IMSS social security payments as of 2007, individuals must contribute to the fund for at least 1,250 weeks. The usual retirement age for these pensions is 65. Employees and employers contribute a percentage of wages, ensuring a minimum monthly pension of $178 USD (MX$1,754/month) in 2024. Other pension and health insurance systems cater to specific groups, such as the Social Security Institute for Government Workers (ISSSTE) and specialized funds for employees of the state oil enterprise, the military, and maritime services. For detailed information on social security benefits and non-contributory pension programs, see Gateway to Global Aging Data (2024b) and Gateway to Global Aging Data (2024a), respectively.

[6]Currently known as the "Secretaría del Bienestar," previously called the "Secretaría de Desarrollo Social" (SEDESOL). We use the Spanish acronym SEDESOL throughout the text, as it was the original government body that implemented the program.

[7]We will use *Localities*, the literal English translation of the original Spanish term, throughout the text.

Although *70 y Más* has been the largest program in terms of both scale and transfer amount for supporting pensioners outside the formal system, it is not the only one. Since 2001, several Mexican states and the Federal District have introduced their own state-specific non-contributory pension programs, each with varying eligibility criteria and benefit levels. According to Aguila et al. (2011), states that implemented such programs before 2007 include Mexico City (2001), Chihuahua (2004), Veracruz (2005), Nueva León (2004), and Quintana Roo (2006), targeting older population in large urban areas—generally the capital cities or localities with more than 2,501 inhabitants. Those states that implemented programs in 2007 and after are Chiapas (2007), Jalisco (2007), Yucatan (2007), Baja Norte (2008), Sonora (2009), Durango (2010), and Zacatecas (2011). Progresa/Oportunidades, a conditional cash transfer program for families with children that started in 1997, initially only targeted rural localities, thus state level non-contributory pension programs were not introduced in these localities. The Progresa/Oportunidades conditional cash transfer also included an old age component as a pension benefit for those older adults living in households already qualifying for benefits. However, the Progresa/Oportunidades pension benefit was much smaller—representing only about 51% of the *70 y Más* pension—and the government prohibited duplication of benefits. Hence, older recipients who qualified for both programs opted out of Progresa/Oportunidades and switched exclusively to *70 y Más*. As a result, the penetration of the Progresa/Oportunidades was low during our study period.

The *70 y Más* program paid a monthly cash transfer equivalent to MX$500, roughly US$ 55 (2024 USD), delivered every two months as a lump sum of MX$ 1,000.[8] This amount represents about 32% of the average income of eligible recipients before the program implemented. Individuals enrolled in the pension program must have a valid, government-issued identification card that displays their date of birth and address. In 2010, a one-time lump-sum transfer equivalent to two months of benefits was paid in the event of a participant's death, provided a designated survivor beneficiary had been appointed.[9]

At the beginning of the program in the second half of 2007, localities with 2,500 or fewer in-

[8]SEDESOL partnered with the same banking organizations used to distribute payments for the Progresa/Oportunidades program. Payments were distributed in person, but recipients could receive a direct deposit in their bank account if they had one. The exact benefit delivery schedule is not fixed and often changed: participating individuals in different localities received cash on different days of the month, all across the two-month payment period. The disbursement was well-publicized by local organizations and newspapers and was available through a telephone hotline.

[9]One could think there could be an incentive to misreport adults under age 70 as being 70 or older to claim benefits on behalf of deceased recipients. However, the lump sum payment was only to registered beneficiaries who had explicitly designated a recipient after death, and it was implemented only after 2010. Therefore, we do not think this could play a major role in our period of study.

habitants were eligible to participate.[10] In 2008, the eligibility expanded to include localities with up to 20,000 inhabitants, and again in 2009 to those with up to 30,000 inhabitants.[11] In the program's first phase, enrollment reached about 1.1 million beneficiaries, which SEDESOL equates with near-complete take-up. SEDESOL published a similarly high take-up rate for localities eligible in 2008 (90%) (Juárez-González and Pfutze, 2014). By that point, the number of beneficiaries in all eligible localities (inhabitants $\leq$ 20,000) had reached 1.7 million, covering over 40% of all Mexican adults aged 70 or older. However, when the program started to expand in 2009, the take-up rate dropped significantly.[12] While the program was well-publicized in localities eligible during the first two years, it was not well-promoted in those eligible in 2009. The localities eligible in 2008 and 2009 were excluded from our analysis because other state non-contributory pension programs also targeted these localities, the 2009 expansion covered less than 50% of eligible localities, and our study focuses on the effects of the transfer in rural areas.

Means testing was not introduced into the program until 2012, when it was extended to larger urban areas. However, this requirement applied only to new beneficiaries who were already receiving other social security payments, such as those from the IMSS or ISSSTE pension systems. In 2014, the program underwent another expansion, lowering the eligibility age from 70 to 65.

3 Data and Sample Construction

3.1 Data Sources

Mortality Data We use individual-level death registries derived from death certificates. These data provide each individual's cause of death, birth year, and sex. It comprises every death in the country between 2002 and 2011, with over three million records. Furthermore, the records include two key identifiers used to determine eligibility: the locality identifier and the population-size category of

[10]The program completed its expansion to all localities with less than 2,500 inhabitants in late 2008, thus there may be some lag in program effects. Subsequently, it started expanding to localities up to 20,000 inhabitants (Aguila et al., 2011; Garfias and Rubio, 2010).

[11]This limit refers to the total inhabitants of a locality as specified in the 2005 census, not the size of the pension-eligible population over 70.

[12]Sedesol. Dirección General de Seguimiento (General Directorate for Monitoring). With information presented for the Account of the Federal Public Treasury, of the Responsible Unit of the Program and Rules of Operation. Quarterly reports of executed budget (http://www.sedesol.gob.mx/en/SEDESOL/Informes_Trimestrales_del_Presupuesto_Ejercido).

the locality of residence.[13] Some registries do not report locality information when, for a given age group, locality, and year, there are fewer than three recorded deaths. In such cases, INEGI classifies the locality identifier as confidential in accordance with data protection legislation. These unidentifiable records represent 26% of deaths in eligible rural areas in 2007. Although we are still able to determine treatment eligibility for these localities, we exclude them from the analysis because the absence of locality identifiers prevents us from assigning population counts by age and sex at the locality level. The population size is classified into 17 groups according to the number of residents at the time of the death. The first three categories allow us to identify our group of interest, 1 to 999, 1000 to 1999, and 2,000 to 2,500. All data are available from Instituto Nacional de Estadística y Geografía (INEGI).[14]

Data quality is a crucial concern when using the vital statistics registry. Although Mexico generally maintains high-quality vital statistics (Mahapatra et al., 2007; Mikkelsen et al., 2015), records for older individuals in rural areas may be less reliable due to limited access to medical personnel, which can result in under-reporting or misclassification of causes of death in death certificates.[15] However, the expansion of Mexico's public health insurance program, *Seguro Popular*, which continued to grow throughout the implementation of *70 y Más*, may have improved the accuracy of death reporting by reducing financial barriers. This could have led to more accurate and timely documentation of deaths and their causes.[16]

To mitigate bias from changes in vital statistics reporting practices, we control for key indicators of reporting quality: the lag time between the date of death and its official registration, as well as whether a medical doctor certified the death. The selection of these variables is guided by previous studies, suggesting that welfare programs can impact the accuracy of reporting in low- and middle-income countries, as well as World Health Organization (W.H.O.) guidelines for identifying high-quality vital statistics reporting systems (Organization et al., 2013; Barham and Rowberry, 2013; Feeney, 2018, 2017; Bhalotra et al., 2019). Additionally, we control for the *Seguro Popular*

[13]For consistency across our period of study, we only focus on localities without boundary changes according to INEGI, and we defined its size based on the 2005 Census locality size identifier.

[14]The study horizon cannot extend to years before 2002 because locality identifiers were not available in publicly accessible vital statistics data until that year. These identifiers are crucial for capturing heterogeneity across localities eligible for *70 y Más*.

[15]When a person dies outside a healthcare facility, the administrative process relies primarily on the family. A doctor must be called to issue an official death certificate and determine the cause of death, typically for a fee. Once the certificate is completed, a relative must visit the civil registration office to forward it to the Ministry of Health and record the vital statistics in the database.

[16]*Seguro Popular* was introduced in 2002 in select municipalities and expanded in a quasi-random manner across Mexico, reaching full municipal coverage by 2006. However, significant portions of the eligible population remained uninsured, prompting continued enrollment efforts in subsequent years.

eligibility of a specific locality given the municipality's rollout, which addresses improvements in death registration during the period of our study.

Population Data We use INEGI population counts disaggregated by sex, locality, and age groups from the 2000, 2005, and 2010 Censuses.[17] These data are used to estimate population exposure and are combined with our deaths records at the locality level for each age group. To construct annual estimates, we linearly interpolate population counts for each sex and age group cell at the locality level between 2002 and 2011. Similar to the death records, INEGI suppresses population counts at the locality level when there are two or fewer individuals per age group due to confidentiality restrictions. To mitigate this issue, we aggregated into 10-year age groups, which increase data availability within each group and reduce the impact of age heaping around eligibility thresholds—an issue that can be more pronounced in narrower age ranges.[18]

Income and Expenditure Data We use the Mexican National Survey of Household Income and Expenditure (ENIGH) from 2005 to 2010. The survey provides a comprehensive overview of household income and expenditure by amount, source, and category; as well as occupational and sociodemographic characteristics, and housing infrastructure and household equipment. Specifically, we use detailed information on individual trimestral income sources (such as wages, independent business earnings, and transfers) and household trimestral spending on food and health, transportation, education, and clothing. Likewise, it has detailed information on household savings and individual non-monetary sources of income. All surveys are publicly available from INEGI.[19]

Additional Controls To account for potential confounding factors, we control for other programs and health services that were in place during the study period and could influence our results. First, we use administrative records on health infrastructure from 2002 to 2011, which include the number of clinics, hospitals, mobile clinics, health brigades, medical residents, and frontline healthcare personnel (doctors and nurses) at the locality level. These data are publicly available and provided by the Ministry of Health (Secretaría de Salud, 2022). Second, using administrative data on Progresa beneficiaries at the locality level, we follow Barham and Rowberry (2013); Aguila et al. (2024) to construct a continuous variable measuring program penetration. This helps account for potential bias before and after the introduction of the older adult component

[17] Data provided by INEGI Microdata Lab.

[18] INEGI also provided population data in 5-year and single-year age groups, which cover approximately 80% and 40% of the total population, respectively, in rural localities eligible in 2007 for individuals aged 60 to 79.

[19] INEGI Microdata Lab provided us with publicly unavailable locality identifiers to match treatment eligibility sizes for localities with fewer than 2,500 inhabitants, 2,500–19,999, and 20,000–29,999. Likewise, our preferred control group includes localities with 30,000–99,999 inhabitants.

in 2006, which later transitioned into the nationwide program we study. Third, as previously mentioned, we incorporate administrative data on *Seguro Popular* beneficiaries to include an indicator of whether a municipality was covered by this low-income health insurance program.[20] Finally, we include the locality-level marginality index from the Mexican National Population Council, which serves as a proxy for poverty and socioeconomic conditions.

3.2 Sample Construction and Descriptive Statistics

Our sample is restricted to deaths among individuals aged 60 to 79 for the mortality data to capture the program's effects on age ranges with comparable mortality profiles around the eligibility threshold of 70 years.[21] Then, we construct cells representing the number of deaths at the locality level for each age and year. We classify each cell as eligible and non-eligible using the locality and age eligibility for the 2007 group of eligible localities, defined as localities with fewer 2,500 inhabitants. We combine these data with the population counts and the aforementioned time-varying locality controls.

As previously mentioned, we exclude the 2008 and 2009 expansion localities because (i) the focus of our study is 2007 expansion localities in rural localities, the most economically vulnerable areas, and for which we hypothesize that any mortality effects would be largest; (ii) other state non-contributory pension programs also covered localities in the 2008 expansion; and (iii) the 2009 expansion, which covered localities with 20,000–30,000 inhabitants, had insufficient take-up to capture the full program effect and could bias our result downward.[22] We also end the analysis in 2011, as the 2012 expansion covered all localities, including large urban localities, preventing a valid control group after 2012. Following Feeney (2018, 2017), we use localities between 30,000 and 100,000 inhabitants as our preferred control group.[23] This restriction avoids comparisons to large urban centers where mortality patterns may differ substantially from smaller, less populated regions. Consequently, the analysis focuses on deaths in rural localities eligible in 2007.[24]

[20] *Seguro Popular* began a staggered rollout in 2002 at the municipal level.

[21] As mentioned previously, we use 10-year age group to maximize the availability of population counts. While the number of deaths increases with age, the denominator decreases, posing challenges to the confidentiality restriction on the population counts for older age groups in narrower age ranges at the locality level.

[22] According to SEDESOL, the program covered approximately 1.03 millions of beneficiaries in its first year of expansion, it grew 80% in the following expansion in 2008, and 10% in the 2009 expansion.

[23] The results are not sensitive to including a higher inhabitants cutoff point.

[24] Furthermore, we decided not to pool the 2007 and 2008 group of expansion altogether because the recent literature on two-way fixed effects estimators has shown that estimates from linear models can differ from the group's average treatment on the treated (ATT) in the presence of treatment effect heterogeneity (De Chaisemartin and

We present summary statistics of deaths in Mexico for older adults aged 60 to 79 years old between 2002 and 2011, both overall and by eligibility group based on population locality size (Appendix Table A.1). There are on average 0.5 deaths per locality-age group in localities with fewer than 2,500 inhabitants (column 1). In localities eligible in the first expansion (year 2007), there were a total of 263,619 deaths across the study period (column 3), which accounted for over 15% of deaths nationally. One fourth of these deaths were attributed to CVD causes, as shown in column (2), which was the largest category among all causes.

Following the mortality results, we present analyses from the ENIGH household survey data to explore mechanisms that could help illuminate the mortality findings. Specifically, we use the 2005, 2006, 2008, and 2010 survey waves to examine income and expenditure behaviors, providing two waves before and after the start of the *70 y Más* program. From these, we create a harmonized repeated cross-sectional dataset at the individual level, selecting adults ages 60 to 80.[25] We define three main income outcomes: individual earnings from wages, total household income, and *70 y Más* transfer income. Expenditures in the ENIGH are only measured at the household level; thus, we define total household spending, food consumption, and health spending (drugs and medical services). We also define specific categories such as cereals (grain, corn, and starches), meat and dairy, and sugar-related food (pastries, sugars, soft drinks, etc.) to test whether the cash transfer affects consumption of obesogenic foods rich in processed carbohydrates, starches, sugars, and fats. Additionally, we define alcohol and tobacco consumption as these are major risk factors for CVD and CVD-related mortality. From the individual occupation and sociodemographic characteristics, we identify each household member's employment status, weekly hours worked, educational attainment, and whether the person is covered by *Seguro Popular*.

We then classify each individual by whether they reside in an eligible locality for the *70 y Más* program, based on the locality population size identifier. All income and expenditure monetary outcomes are converted to monthly values, updated to 2024 Mexican pesos, and then converted to U.S. dollars for comparability.[26]

We present summary statistics pre- and post-program for eligible and ineligible localities. First, we document the presence of the *70 y Más* cash transfer, which averages approximately $15 USD

d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). Therefore, in the absence of recent developments for heterogeneous treatment effects in non-linear models, we focus all the analyses separately for the 2007 expansion.

[25] We restrict our upper-end age to 80 given the small sample size above 80, and reduced comparability with the comparison ages under 70.

[26] All yearly consumer price index and exchange rates used are publicly available from the Bank of Mexico webpage.

in eligible localities during the post-expansion program period, and verify virtually no program transfer income in the pre-period and ineligible localities (Appendix Table A.2). Regarding labor market outcomes, we observe a potential decline in earnings and income in eligible localities following the program's introduction, consistent with reductions in employment and hours worked. As expected, enrollment in *Seguro Popular* increases in eligible localities, aligning with the program's municipal rollout during the same period. In terms of household-level indicators, we do not observe a relevant change in total spending among eligible localities (Appendix Table A.3); however, there is a suggestive increase in food expenditures——particularly in categories associated with obesogenic foods, including meat and dairy, sugar and fat, and cereals (Appendix Table A.3). Finally, health-related spending appears to decrease from $22.81 to $13.12 (Appendix Table A.4). Taken together, these patterns suggest plausible behavioral mechanisms through which the cash transfer may be influencing outcomes, and they merit further investigation.

4 Mortality Impact of the *70 y Más* Pension Program

We next describe our empirical strategy for the mortality analysis, present the main estimates of the program's effect on mortality, and discuss robustness checks.

4.1 Empirical Strategy

The phased rollout of the program, geographic coverage, and age eligibility allow us to implement a triple difference (difference-in-difference-in-differences) design, following Feeney (2017, 2018). In particular, we can use the timing of coverage among different localities and age eligibility to study changes in age-locality level outcomes before and after coverage. Since many small localities record large number of zero deaths for certain locality-age groups, the analysis relies on total death counts as the primary outcome, and models the population denominator controls for each age-locality cell as a population offset in the Poisson model. Recent literature recommends (Chen and Roth, 2024; Mullahy and Norton, 2024) the use of Poisson regression to model non-negative outcomes such as deaths, thereby avoiding outcome transformations to implement linear models with a large number of zero death rates. As a result, the Poisson regression naturally accommodates both zero and positive outcomes, assuming a consistent data-generating process across observations.[27] By including population as an offset, it models rates rather than raw counts, effectively adjusting for exposure. Importantly, it does so without requiring ad-hoc outcome transformations—such as $log(1+Y)$ or the inverse hyperbolic sine—that are unit-dependent and can distort results, and prevent interpretable effects as percentage changes.[28]

Therefore, following Menares and Muñoz (2025), we define the outcomes of interest as the annual number of deaths within a locality for a given age (e.g., deaths in locality A among people aged 60-69 in a given year). We then fit Poisson models for counts using a log link, including age-locality level population as an offset, with a general specification given by:

$$y_{alt} = \exp\left(\beta \cdot 70yMas_{alt} + \alpha_a + \gamma_l + \delta_t + \eta_{l_e a} + \xi_{l_e t} + \theta_{ta} + \Pi_{lt} + Log(Pop_{alt})\right)\epsilon_{alt}, \qquad (1)$$

where y_{alt} is the count of deaths for age-group a in locality l in year t.[29] $70ymas_{alt}$ is an indicator that equals one from the first time a locality with age above 70 is eligible and onward, i.e., the treatment is an absorbing state. α_a, γ_l, and δ_t are age group, locality, and year fixed effects, respectively. l_e is a group locality eligibility indicator; thus, $\eta_{l_e a}$ represents fixed effects that control for time-invariant unobservable variables specific to localities-ages eligible groups, $\xi_{l_e t}$ are time-variant fixed effects that account for year-specific eligible locality groups shocks common across age-groups, and θ_{ta} are time-variant fixed effects that account for age-eligible shocks common across localities. We also include a vector of controls, Π_{lt}, to account for time-variant locality characteristics, such as *Progresa* penetration, *Seguro Popular* coverage, health infrastructure, marginality index, and proxies for data quality, such as the average lag of death registration and the share of deaths certified by a medical doctor. Finally, ϵ_{alt} is the error term clustered (multi-way) by age and locality, the level of treatment. We weight all our regressions using the locality's 2005 population size age 60 and over. In this model, identification of the *causal* effect of the *70 y Más* program is

[27] This approach treats zero and positive counts within a unified framework, without assuming separate processes as in zero-inflated or hurdle models. Furthermore, we acknowledge that Poisson regression assumes equality between the mean and variance, and that violations of this assumption could motivate the use of a negative binomial model. Nonetheless, given our large sample size and the robustness of the Poisson pseudo-maximum likelihood estimator (PPMLE), we are confident that our estimates of the conditional mean remain consistent even under overdispersion. Moreover, we cluster standard errors at the treatment level to account for any residual variance misspecification.

[28] Feeney (2018, 2017), implemented outcome transformations following the common practice at the time, that are currently considered to bias results, and hard to understand effects, which are largely analyzed in Chen and Roth (2024); Mullahy and Norton (2024)

[29] It is important to notice that we have two age groups under the subscript a, those between 60 to 69 and 70 to 79, which represent the ineligible age groups, respectively.

predicated upon the assumption that—conditional on locality, age group, aggregate year shocks, time-invariant locality-age-eligible group, and time-variant locality-eligible and age-eligible cell indicators—there are no unobserved factors that correlated with both the timing and eligibility of coverage and other determinants of health outcomes.

Following Chen and Roth (2024), our parameter of interest is the average proportional treatment effect on the treated.

$$\theta_{ATT\%} = \frac{E[Y_{alt}(1) - Y_{alt}(0) \mid 70yMas_{alt} = 1, Post_t = 1]}{E[Y_{alt}(0) \mid 70yMas_{alt} = 1, Post_t = 1]} \quad (2)$$

This is the percentage change in the mortality rate for the age- and locality- eligible group in the post-treatment period. As shown in equation (2), what is being estimated is an average level effect—an increase or reduction—scaled by an average mortality level, and not a weighted average of the percentage decline caused by the intervention. This distinction matters because localities with higher baseline mortality could experience relatively smaller or larger program effects on mortality.

Conveniently, $\hat{\theta}_{ATT\%}$ can be obtained from $\exp(\hat{\beta}) - 1$, where $\hat{\beta}$ is the estimated coefficient from the Poisson model given by equation (1).[30] Thus, we expect the $\hat{\theta}_{ATT\%}$ to be negative if the program led to a decrease in deaths among the treated locality-age group and positive otherwise. The parameter of interest in (1) is the result of the three-way interaction between (i) an indicator for eligible localities in 2007, (ii) an indicator for those deaths with ages above 70, and (iii) an indicator for the post period for a given group of localities.

In this case, identification of the causal effect of the program $\hat{\theta}_{ATT\%}$ is predicated upon the assumption that in the absence of treatment, the *percentage changes* in the mortality rate would have been the same between the eligible and non-eligible locality-age groups.[31] As in Wooldridge (2023), this can be formalized using a "ratio" version of the parallel trends assumption, sometimes referred to as the "parallel relative trends" assumption.

$$\frac{E[Y_{alt}(0) \mid 70yMas_{alt} = 1, Post_t = 1]}{E[Y_{alt}(0) \mid 70yMas_{alt} = 1, Post_t = 0]} = \frac{E[Y_{alt}(0) \mid 70yMas_{alt} = 0, Post_t = 1]}{E[Y_{alt}(0) \mid 70yMas_{alt} = 0, Post_t = 0]} \quad (3)$$

[30] The Poisson quasi-maximum likelihood (QMLE) consistently estimates the population coefficient β, which satisfies $\exp(\beta) - 1 = \frac{E[Y(1)]}{E[Y(0)]} - 1 = \theta_{ATT\%}$. The parameter identified through the Poisson model including population as an offset is a mortality rate.

[31] Since the treated and control groups might have different pre-treatment means, assuming parallel trends *in levels* could be a strong assumption, i.e., it may be unreasonable to expect that time-varying factors have equal level effects on mortality between the eligible and non-eligible locality age groups.

Intuitively, it states that if the *70 y Más* program had not occurred, the average percentage change in mortality for the eligible groups would have been the same as the average percentage change in the mean mortality for the ineligible groups. Under the assumption of parallel trends, we can estimate the counterfactual percentage change in the mean mortality for the eligible group using the observed percentage change for the ineligible group.

To assess the plausibility of this parallel relative trends assumption, we examine the dynamic effects of *70 y Más* using event studies around the time when the locality-age group becomes eligible. Given our data availability and focus on the initial 2007 rural expansion, we use five years before and four years after treatment (i.e., $t = -5$ in 2002, and $t = 4$ in 2011).[32] To estimate this dynamic version of our difference-in-difference-in-differences specification, we use a leads-and-lags model in event time, with the 2007 expansion year set to zero. Specifically, we estimate the following equation:

$$y_{alt} = \exp\left(\sum_{k=-5}^{-2}\beta_k \cdot D_{alt}^k + \sum_{k=0}^{3}\beta_k \cdot D_{alt}^k + \alpha_a + \gamma_l + \delta_t + \eta_{la} + \xi_{lt} + \theta_{ta} + \Pi_{lt} + Log(Pop_{alt})\right) \cdot \epsilon_{alt}, \tag{4}$$

where $D_{alt}^k = 1[t = 70yMas_{al} + k]$, and $70yMas_{al}$ is the timing of coverage of a locality eligible-age group al. In other words, D_{alt}^k is a dummy variable indicating that a locality with deaths above the age 70, al, was covered by the program k years ago (or included k years ahead, for negative values of k). We normalize the coefficients such that $\beta_{k=-1} = 0$, i.e, treatment is re-coded in event time relative to the year before each locality age group was eligible for *70 y Más*. Therefore, the β_k coefficients can be interpreted as the effect of *70 y Más* coverage on the mortality rate y_{alt} for each k period relative to the year before the coverage of a locality-age group al in the *70 y Más* program.

4.2 Results: *70 y Más* Effect on Older Adult Rural Mortality

We begin the results section by discussing the effect of the program on mortality using raw data. In Appendix Figure A.1, panel (a) presents age-adjusted mortality rates, disaggregated by locality and age eligibility, with the left y-axis representing those age-eligible and the right y-axis repre-

[32] We cannot analyze data before 2002 because of the absence of locality identifiers in the vital statistics data available to us. We end the analysis in 2011 because in 2012 the program further expanded to cover all localities in the country.

senting individuals below age 70. We compare trends with our preferred control group of deaths in localities with 30,000–100,000 inhabitants. Red lines indicate deaths in eligible localities, while black lines represent those in ineligible localities. Likewise, solid lines indicate age-eligible deaths, while dashed lines represent those in age-ineligible. The trends in the 2007 eligibility group (ages 70-79 in localities smaller than 2,500 population) show an increase in the mortality rate after the program started, with a peak in 2010 of about 29 per 1,000 deaths. The younger (ages 60-69) comparison group in these localities shows a similar pre-trend, and visually similar post-trends (as would be expected, since plausibly-sized mortality effects should be hard to visually see in a graph such as this). We also show trends in these two age groups in the comparison localities with populations of 30,000-100,000. Although the levels and trends in these comparison localities differ somewhat from those of the eligible localities, it is reassuring that the pre- and post-trends are visually similar when comparing the older and younger groups within these comparison localities.

We next present the overall event study graph in Appendix Figure A.1, panel (b), to continue the visual assessment of trends. The event study analyzes mortality in our Poisson model using the count of deaths as the dependent variable with a population offset to adjust for exposure at the locality level. The figure plots the point estimates obtained from the triple difference model (4) to show the dynamic effect of *70 y Más* between age-locality eligible and ineligible groups during our period of study. The horizontal axis shows the years relative to 2007, with event time zero denoting the first year of the program. We omit event time -1 so that all estimates are measured relative to the year before eligibility. Point estimates of leads and lags are plotted along with their 95% confidence intervals. The figure shows that pre-program estimates are mostly positive and statistically different from zero, a result that suggests violation of the assumption of parallel relative trends in this pooled model. Because of this violation, we do not emphasize results from this pooled model. Instead, we turn next to examine results in sub-groups that have more plausible pre-program parallel trends, with an emphasis on sex-specific analyses given the differential predictions and findings by sex that we discuss above, e.g. in the similar program in Chile (Miglino et al., 2023).

Sex-Specific Event Studies Figure 1, Panel (a), presents event studies stratified by sex. For females, in the pre-program period there are no significant differences in mortality when comparing those age-localities that would become eligible in 2007 versus those that remained ineligible from 2007-2011. Given these satisfactory pre-trends, we note that in the post-expansion years one through four the graph indicates statistically significant reductions in mortality in the group eligible for *70 y Más*, relative to the comparison groups. Among males in Panel (b), the results are less clear: there are large estimated mortality increases in the post period years one to three, but there is also evidence of significant pre-trends (though of a smaller magnitude), and a small estimated mortality reduction in year four. Thus while we find strong beneficial mortality effects for women, our event study estimates do not allow us to convincingly claim causal results for men.[33]

Cause-Specific Event Studies Next, in light of research suggesting that some diseases may be more responsive to income shocks than others (Snyder and Evans, 2006; Adda et al., 2009; Ahammer et al., 2017; Aguila et al., 2024), we study the differential impact of the program on deaths (1) from causes related to CVD and circulatory diseases, and (2) mortality from non-CVD/non-circulatory disease-related causes. CVD and circulatory disease-related mortality includes causes of death attributable to ischemic heart disease, stroke, and diabetes, among many others. Major risk factors for these diseases are obesity, weight gain, and high blood pressure; there are theoretically ambiguous signs of income effects on these risk factors, making CVD mortality effects particularly valuable to test empirically. Non-circulatory/non-CVD-related mortality includes all other causes of death, grouped into the following categories: infections, accidents (traffic and other types), cancers, respiratory illnesses, kidney disease and digestive illnesses, congenital disorders, violence, and a small minority of "other" causes unrelated to the cardiovascular/circulatory system. Figure 1, panels (c) and (d) show the event study results by CVD and non-CVD causes for females and males, respectively. Among women, CVD mortality effects are negative, but with magnitudes similar to several pre-trend years, thus providing no strong evidence of effects; non-CVD mortality, though, appears to significantly improve. Among males, the event studies suggest potentially larger adverse effects among CVD than non-CVD deaths, but these must be interpreted cautiously when comparing to similarly sized pre-trends in some years.

Triple Difference Regression Estimates To test our hypotheses more formally—and quantify the effects—we next show results from our triple difference Poisson models. Table 1 presents the estimates obtained from the model in equation (1). Column (1) presents the overall results pooled across sex and cause groups, indicating a statistically significant and beneficial effect of the *70 y Más* program on mortality. Specifically, for the group that began receiving benefits in 2007, mortality rates among individuals over 70 in newly eligible localities decreased by 2.3 percent. This effect reflects the average proportional treatment effect on the treated across eligible localities and age groups following the program's implementation. However, the pre-trends shown in the corresponding event study graph discussed above caution against a strong causal interpretation of this effect.

[33] Appendix Figure A.2 additional shows event studies for all group of diseases in panel (a), and by CVD and non-CVD in panel (b).

Based on the event studies, we have more confidence in the regression results by sex, particularly the significant decrease of 5.5% for female mortality (Table 1, column 2). While as previously mentioned, the inconclusive results for male, exhibits an insignificant 1.1% increase in mortality (Table 1, column 3). Columns (4) to (7) further disaggregate these results, presenting regression estimates by sex-specific cause of death groups. In column (6), we find a significant 6.4% decrease in female non-CVD mortality. The stable pre-trends shown in Figure 1 panel (c) support interpreting this estimate as the causal effect of the old age non contributory pension. However, the 5.5% increase in male CVD mortality should be interpreted cautiously given the pre-trend estimates shown in Figure 1 panel (d). There are no significant effects on female CVD mortality (-2.3%) and male non-CVD mortality (0.5%), shown in columns (4) and (7), respectively.

Appendix Table A.5 also shows results on mortality rates separately by marital status, although again these must be interpreted cautiously since we do not have the marital-specific population denominators that would be required for proper mortality rate analysis. Nevertheless, it shows significantly negative effects on unmarried females and significantly positive effects on unmarried males, indicating that our main mortality results are unlikely to have been substantially biased by any potential spillovers.

Robustness Tests A challenge faced in our design is the possibility of differential trends in our chosen control group, that is, localities with 30,000–100,000 inhabitants. To address this concern, we implement a series of sensitivity analyses and robustness checks, using different samples. We re-estimate models including only localities above 500 inhabitants, removing states with benefits before the nationwide program *70 y Mas* started, and including all ineligible localities as control group regardless of their size.[34] Across all model and sample specifications, the results exhibit similar overall effects (Appendix Table A.6), and similar pre-program trends (Appendix Figure A.3).

Additionally, we implement a propensity score weighting estimator using the marginality index as well as disaggregated components of the index to predict eligible localities (Appendix Table A.7, columns 1 and 2).[35] We then use the predicted propensity scores as weights in our main model (columns 6 and 7), finding little change in the magnitude or significance of results.[36] We

[34] As mentioned in Section 2, there were other programs in placed at the state level, but with different scope and coverage.

[35] Disaggregating marginality index also works as a way of showing balance in pre-treatment characteristics at baseline between eligible and ineligible localities, including type of insurance, highest educational attainment, gender, marital status, and geographical location.

[36] The Column (5) point estimate does not match the main results because of the exclusion of the marginality index as a covariate.

repeat the procedure only for localities classified as very low to medium marginalized (columns 3-4 and 9-10), again finding little sensitivity of our results.

The validity of our results would also be threatened if (1) recipients manipulated their age to qualify for the benefits earlier, or (2) ages at death were misreported in the vital statistics. We believe the first concern is minimal because birth certificates were required for program enrollment. For the second concern, as mentioned in section 3.1, Mexico has high-quality vital statistics, and we further include data quality covariates to address potential misreporting.[37]

5 Mechanisms

For the first time in Mexico, the *70 y Más* program provided a fixed amount of income to older adults living in rural areas nationwide.[38] Following its implementation, we document beneficial effects of the cash transfer on mortality among older adult females living in eligible localities. We do not find conclusive evidence of an effect among males. Following previous literature, we initially hypothesized that analyzing deaths by cause could help illuminate underlying mechanisms. This section therefore investigates potential mechanisms using household income and expenditure survey data to examine whether targeted individuals received the benefits and to identify the main spending channels through which behavior changes may have occurred.

5.1 Empirical Strategy

To estimate the causal effect of the cash transfer, we leverage the program's eligibility rule, which required adults to be at least 70 years old and to live in eligible localities to receive the benefits. Therefore, individuals who are just above the age threshold should be, on average, comparable to those just below age 70. This structure enables a regression-discontinuity (RD) design to estimate

[37]Furthermore, we estimate an event study on population size following the same specification as equation (4), but instead modeling population counts. We find that the program appeared to induce a substantial increase in the eligible population. This could indicate possible program effects on migration; thus, further work would be valuable to test if the program led age-eligible adults over 70 to move to these small rural localities. We also estimate the naive event study for death counts when the population offset is not modeled, showing a large increase in deaths, indicating that failing to account for the population increase would inappropriately produce a large spurious estimated increase in deaths.

[38]As previously discussed in section 2, there were other programs implemented at the state level before 2007, but these did not necessarily target rural areas or the whole nation. We find no significant differences in the trend once we remove states with other non-contributory pension programs before *70 y Más* started (Appendix Figure A.3).

the intent-to-treat (ITT) effect of *70 y Más* on behavioral outcomes among older adults. This RD specification is not feasible for the mortality analyses due to the likely lagged effects of the program on mortality, as well as the binned 10-year age groups used above and below the cut-off to maximize count completeness on the denominator, given the confidentiality restrictions. However, because behavioral responses can plausibly occur more quickly, the RD approach is preferred for this analysis of behavioral mechanisms.

Our RD specification employs a local linear model following Gelman and Imbens (2019), using age at eligibility (the running variable) centered around 70.[39]

$$y_{it} = \alpha + \beta \cdot \mathbb{1}(age_i \geq 70) + \gamma \cdot (age_i - 70) + \delta \cdot \mathbb{1}(age_i \geq 70) \cdot (age_i - 70) + \theta_t + \epsilon_{it}, \qquad (5)$$

where y_{it} is one of several outcomes of interest for the individual or household i in year t. Since we observe each individual's age reported in the survey, the term $\mathbb{1}(age_i \geq 70)$ is an indicator for an individual i being older than 70. However, given that we have income at the individual level, and expenditure at the household level, we use the maximum age of a member within the household for all expenditure outcomes, thus our running variable depends on the outcome. The parameter of interest is β, which measures the jump in the regression function at the discontinuity. θ_t is the survey year fixed effect, and ϵ_{it} is the error term, clustered at the household level. We use survey weights and drop each outcome outlier upper 1%. The fourth term of equation (5) allows for different slopes of the regression function on either side of the cut off.

To better understand the dynamics of the income shock before and after the cash transfer, and also between the eligible and ineligible localities samples, we summarize these separate RD models in equation 5 using a triple difference in discontinuities model via the following specification:

[39] For simplicity, we still write our preferred specification using age 70 as the discontinuity cutoff, although we drop them in our analyses.

$$
\begin{aligned}
y_{it} = &\beta_0 + \beta_1 \cdot \mathbb{1}(\text{age} \geq 70) \cdot Eligible_i \cdot Post_t + \beta_2 \cdot \mathbb{1}(\text{age} \geq 70) \cdot Eligible_i \\
&+ \beta_3 \cdot \mathbb{1}(\text{age} \geq 70) \cdot Post_t + \beta_4 \cdot \mathbb{1}(\text{age} \geq 70) \\
&+ \beta_5 \cdot (\text{age} - 70) \cdot Eligible_i \cdot Post_t + \beta_6 \cdot (\text{age} - 70) \cdot Eligible_i \\
&+ \beta_7 \cdot (\text{age} - 70) \cdot Post_t + \beta_8 \cdot (\text{age} - 70) \\
&+ \beta_9 \cdot \mathbb{1}(\text{age} \geq 70) \cdot (\text{age} - 70) \cdot Eligible_i \cdot Post_t + \beta_{10} \cdot \mathbb{1}(\text{age} \geq 70) \cdot (\text{age} - 70) \cdot Eligible_i \\
&+ \beta_{11} \cdot \mathbb{1}(\text{age} \geq 70) \cdot (\text{age} - 70) \cdot Post_t + \beta_{12} \cdot \mathbb{1}(\text{age} \geq 70) \cdot (\text{age} - 70) \\
&+ \mathbb{1}Eligible_i + \mathbb{1}Post_t + \epsilon_{it}, \qquad (6)
\end{aligned}
$$

where *Eligibile* is an indicator for an individual or household i living in the eligible localities. By setting both *Eligibile* and *Post* to zero, the equation collapses to equation (5) for the ineligible sample. The key parameter of interest in the specification is β_1, which measures the difference in the outcome discontinuities between eligible and ineligible samples before and after the program's implementation. The terms starting in the third line of equation (6) allow for different slopes of the regression function on either side of the cut off and for these slopes to vary between the two samples.[40][41]

The key identifying assumption of the RD design is that the potential outcomes are smooth at the age-70 cutoff in the absence of the treatment. Accordingly, the RD estimate compares the jump between the projected estimate immediately to the left of the discontinuity to the regression function immediately to the right. Therefore, any estimated discontinuity can be attributed to the cash transfer received by individuals aged 70 or older residing in eligible localities after the program started. We perform several checks to validate the research design. First, we test for balance in baseline covariates around the discontinuity by replacing y_{it} in equations (5) with household size, and labor market characteristics such as employment and earnings at baseline. Second, we verify that the density of the age distribution is smooth at the discontinuity. However, since the age eligibility comes from one respondent per household, there could be misreporting of household members' ages. As we observe age heaping at ages 60, 65, 70, and 75 (Appendix Figure A.4), we

[40] See Grembi et al. (2016) for a formal analysis of difference in discontinuities models and Deshpande (2016), Malamud et al. (2023), Masuda and Shigeoka (2023), and Hyman et al. (2024) for recent examples.

[41] For a more concise version of the previous equation, it could be considered as a comparison of eligible and ineligible localities separately for the pre- and post-periods: $y_{it} = \beta_0 + \beta_1 \cdot \mathbb{1}(age_i \geq 70) \cdot \mathbb{1}(Eligible_i) + \beta_2 \cdot \mathbb{1}(age_i \geq 70) + \beta_3 \cdot (age_i - 70) \cdot \mathbb{1}(Eligible_i) + \beta_4 \cdot (age_i - 70) + \beta_5 \cdot \mathbb{1}(age_i \geq 70) \cdot (age_i - 70) \cdot \mathbb{1}(Eligible_i) + \beta_6 \cdot \mathbb{1}(age_i \geq 70) \cdot (age_i - 70) + \theta_t + \mathbb{1}(Eligible_i) + \epsilon_{it}$

exclude older adults who reported age 70 as previously mentioned. Even though such age heaping is due to misreporting, older adults are required to use birth certificates to claim the benefits, meaning individuals cannot manipulate their date of birth for cash transfer eligibility.[42]

5.2 The *70 y Más* Program Impact on Work, Income and Household Expenditure

To illustrate the variation identifying our estimates, we present scatterplots with linear fits for various outcomes. These are shown in two panels, distinguishing the effects between eligible and ineligible localities, with each panel containing linear fits for periods before and after the program started. Panel (a) of Figure 2 clearly shows the first stage effect for *70 y Más* cash transfers, exhibiting a marked increase at age 70—from 0 to about 40 USD—after the program was introduced in eligible localities. This finding supports the validity of our use of ENIGH data to study household effects.[43] We next present suggestive evidence of program impacts on labor market activity. Visually, Figure 3 suggests a decrease in employment among individuals over age 70 in the post-period compared to the pre-period, and there is a possible reduction in hours worked as well (Appendix Figure A.5). However, there are no visible discontinuities in individual earnings from work nor in total household earnings (Appendix Figures A.6 and A.7).

To test whether these visual differences described above are statistically significant, we estimate the triple difference in discontinuities model in equation (6) both for individual level (Table 2) and household level outcomes (Table 3).[44] Each table shows the results of our RD-DD model, which captures the different effects in discontinuities before and after the program started, and between eligible and ineligible localities.

Table 2, reports individual-level outcome RD-DD results consistent with our visual inspection above. Column (4) shows a large and statistically significant decrease in employment, with similar magnitudes for males and females. This is reflected also in a substantial decrease in hours worked

[42] These can also be thought of as a donut RD, where the assumption is that potential outcomes would have evolved smoothly through the excluded donut-age 70 in the absence of treatment. As a robustness check, we estimate the models including individuals aged 70 and and find nearly identical results.

[43] Appendix Figures A.8 and A.9 suggests outcomes are nearly balanced between eligible and ineligible samples at baseline (before the program started). The household size is important, as we can use it to transform our outcomes to per capita household spending, given the absence of individual-level spending measurement. Likewise, it is important to notice potential differences in the post period for the *Seguro Popular* coverage in eligible localities, align with ongoing expansion of the program in rural localities.

[44] Appendix Table A.8 presents additional household-level outcomes.

as shown in column (5), though the result is only significant at the 10% level; again the magnitudes are similar for males and females. The point estimates for wage earnings in column (2) and overall income in column (3) suggest that the reduction in work offsets roughly the amount of the new non-contributory pension, resulting in little net change in overall income; however, these results are imprecisely estimated. The other notable finding in this table is the significant reported increase in health insurance coverage through *Seguro Popular* (which was controlled for in the above mortality analyses).

We also explore results disaggregated by marital status (Appendix Tables A.9 and A.10), as unmarried (primarily widowed) older adults may face more constraints and hence be more responsive to a new non-contributory pension earnings. While most findings remain consistent, we observe notable heterogeneity in hours worked. Specifically, the overall decrease in hours worked appears to be driven by unmarried individuals, who on average worked 12 fewer hours per week—a statistically significant reduction.[45] Note that the Appendix Table A.5 also shows results on mortality rates separately by marital status, since analyzing unmarried individuals is informative not only because of their greater constraints, but because they are less likely to have effects biased toward zero due to within household spillovers if pension transfers to an older spouse also benefit the comparison group under age 70.[46]

Finally, we examine effects on overall household aggregates in Table 3. These results must be interpreted cautiously, as we observe a substantial increase of over one member in households size among females, with no significant change for males (though the point estimate is negative). Again, we find little change in total income, but with wide confidence intervals. Similarly, household aggregate spending, food spending, and health expenditures show insignificant changes. When examining household food consumption by category, we find small and statistically insignificant effects. One possible exception is that male-headed households may have increased consumption of obesogenic foods in the sugar and fat categories, while female-headed households may have decreased them. This pattern could represent a potential mechanism for differential health effects by sex, though the estimates are imprecise (Appendix Table A.8, columns 2-4). It is important to note that statistical power is limited in many of our comparisons; for example, 95% confidence intervals cannot rule out modest changes in food expenditure categories.[47] Interpretations are

[45]We do not find significant results for other household outcomes examined by marital status (Appendix Table A.11).

[46]We previously report significantly negative effects for unmarried females and significantly positive effects on unmarried males, indicating that our main mortality results are not substantially biased by potential spillovers.

[47]For all results in Tables 2 and 3, we include the multiple hypothesis testing p-value based on the Romano-Wolf approach, which shows that the significance of our notable findings are generally robust to correcting for multiple

further limited due to the fact that this household survey measures expenditure at the household level only, thus we cannot examine changes in the spending of just the newly eligible older adults.

6 Conclusion

We analyze the effects of a large cash transfer program on adult health and mortality outcomes via a non-contributory pension targeting low-income older adults in Mexico. We tested for differential effects by gender, but while the program suggests significant beneficial effects on mortality among women, we find inconclusive evidence for men. Our estimates show a 5.5% reduction in female mortality, which is in the range of what other recent studies have found in similar settings and contexts. To compare this magnitude with recent literature, we compute the elasticity of mortality with respect to the transfer income for females, which yields an elasticity of -0.23.[48] By comparison, we see for example that Miglino et al. (2023) found a 2.7% reduction in female mortality following the non-contributory pension transfer in Chile, with an elasticity of -0.39.[49] Likewise, Jeon et al. (2025) finds a 1.5% short-term reduction in female mortality as a result of a pension income transfer, yielding an elasticity of -0.043. A more detailed comparison of results can be found in Miglino et al. (2023), Figure 3, which provides an exhaustive comparison with previous literature on the mortality elasticity with respect to income; our elasticity of -0.23 is close to the average elasticity of -0.21, whether for females, males, or both combined.

Our analysis of potential mechanisms suggests reductions in both employment and hours worked for both men and women. We do not find strong evidence of differences in mechanisms that could explain different mortality effects by gender. However, similar changes in behaviors—such as reductions in work hours—may have opposite health implications for men and women. For example, women may have stronger social networks that encourage healthier behaviors during non-work time, while men in this context may have lower social support outside work and/or belong to social networks that support less healthy behaviors. Further research, including qualitative studies, is needed to better understand why pension programs appear to have greater benefits for women. It is

testing.

[48]For the numerator, we use the estimate on the reduction in mortality from Table 1, column (2). For the denominator, we compute the variation of income as a result of the transfer from Table 2, Column (1) Panel C, and the female pre-intervention eligible average income similar to the one on Table A.2, column (2), which results in -0.23 = -0.05 / (26.98/125.36).

[49]Even though they compute the elasticity of mortality with respect to the change in net income (after accounting for any labor supply change), the transfer is essentially equal to the net income change in their setting, as per Table 1 in their paper.

also important to note that our analysis of health effects focuses exclusively on mortality outcomes and does not capture broader dimensions of well-being, such as quality of life, mental health, or life satisfaction. These aspects may also have improved as a result of the cash transfer. Future research should incorporate such measurements to provide a more comprehensive assessment of the program's overall welfare effects among older adults.

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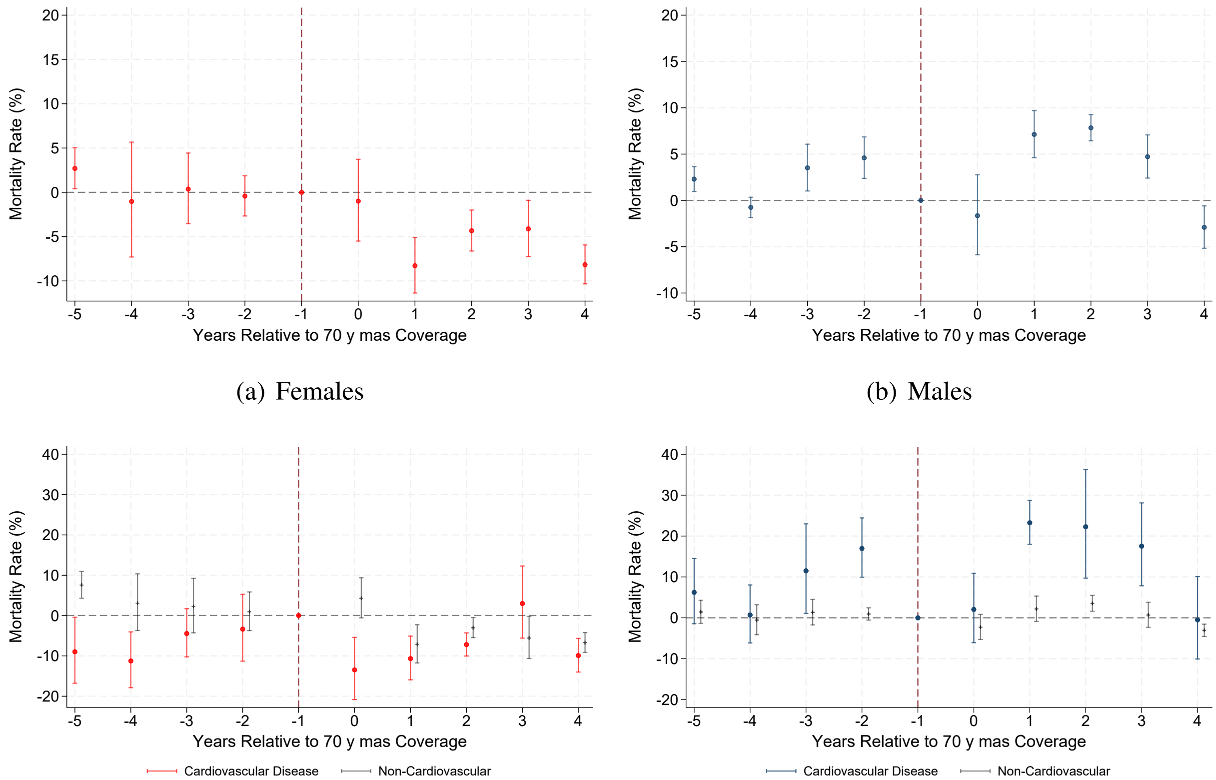
Figure 1: *70 y Más* Effects on Mortality Rates



(a) Females

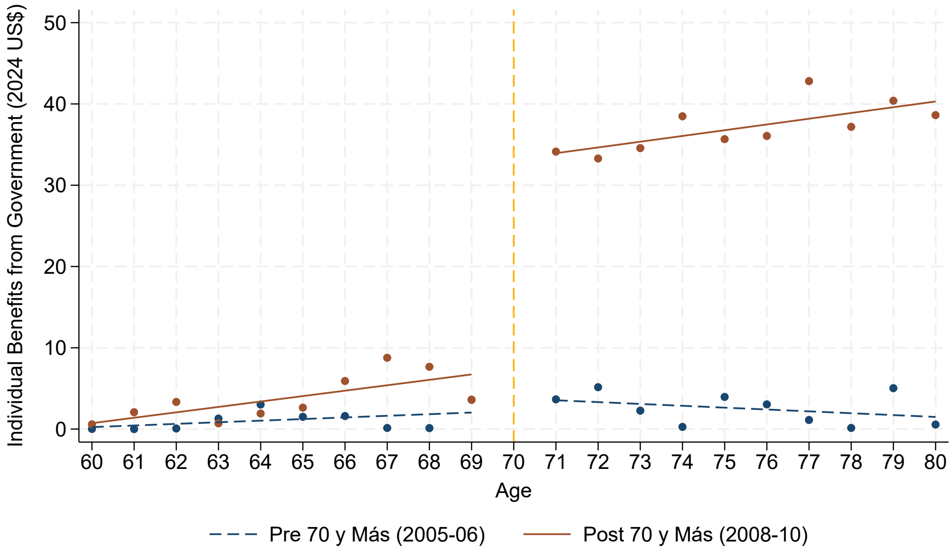
(b) Males

(c) Females Event Study By Disease Group

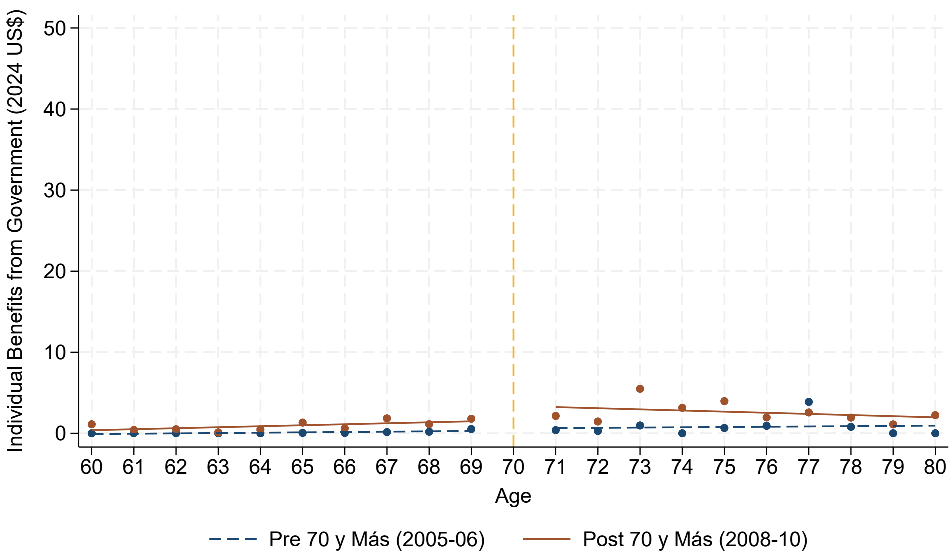
(d) Males Event Study By Disease Group

Notes: This figure shows the results obtained from estimating the dynamic difference-in-difference-in-differences using the count of deaths as the dependent variable in a Poisson regression including the logarithm of population as an offset. Age corresponds to 2 age groups: 60 to 69, and 70 to 79. It includes locality, age, and year fixed effects, as well as locality-age eligibility, year-locality eligibility, and year-age eligibility fixed effects. Standard errors are clustered at the level of treatment: locality and age. The mortality rate in the y-axis corresponds to percent changes by subtracting 1 from the rate ratio, i.e., $\hat{\theta}_{ATT\%} = exp(\hat{\beta}) - 1$. 95% confidence intervals for $\hat{\theta}_{ATT\%}$ are computed using the delta method for univariate transformations on the coefficient estimated from the Poisson regression. The period of analysis is from 2002 to 2011. All regressions are weighted using the population aged 60 to 79 in 2005. The population offset is interpolated from the 2000, 2005, and 2010 Census data at the locality-age-year level.

Figure 2: RD Scatterplot for Amount of *70 y Más* Transfer Received



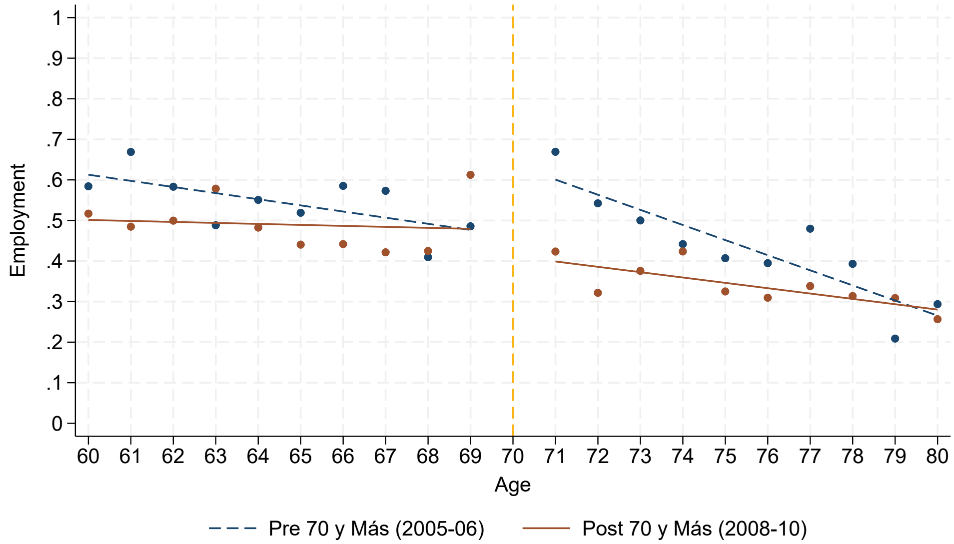
(a) Eligible Localities



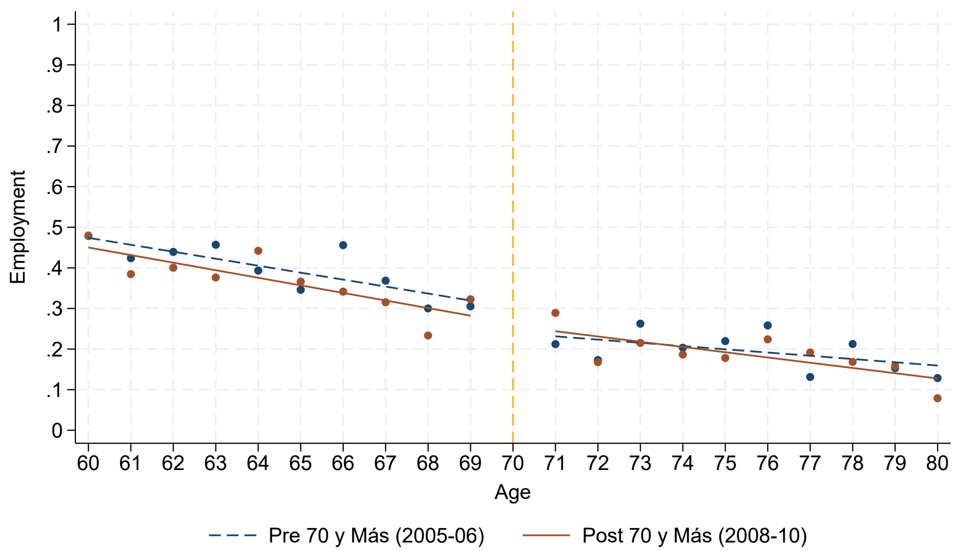
(b) Ineligible Localities

Notes: This figure visually shows the discontinuity for the amount of the *70 y Más* cash transfer reported received, disaggregated by eligible and ineligible localities, as well as by periods before and after the program's implementation. Eligibility is defined for individuals residing in localities with fewer than 2,500 inhabitants that expanded in 2007, while ineligible localities include those with populations between 30,000 and 100,000 that did not fully expand during the study period. Survey waves from 2005 and 2006 were pooled to represent the pre-program period, while waves 2008 and 2010 were pooled for the post-program period. All linear fittings are estimated at the individual level, controlling for year-of-survey fixed effects and including survey weights.

Figure 3: RD Scatterplot for Employment



(a) Eligible Localities



(b) Ineligible Localities

Notes: This figure visually shows the discontinuity for labor force participation, disaggregated by eligible and ineligible localities, as well as by periods before and after the program's implementation. Eligibility is defined for individuals residing in localities with fewer than 2,500 inhabitants that expanded in 2007, while ineligible localities include those with populations between 30,000 and 100,000 that did not fully expand during the study period. Survey waves from 2005 and 2006 were pooled to represent the pre-program period, while waves 2008 and 2010 were pooled for the post-program period. All linear fittings are estimated at the individual level, controlling for year-of-survey fixed effects and including survey weights.

Table 1: *70 y Más* DDD Effect on Mortality Rates

				By Disease Status			
		By Sex		CVD		non-CVD	
	Pooled	Females	Males	Females	Males	Females	Males
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
After 70 y Más	-0.0233***	-0.0549***	0.0111	-0.0229	0.0545**	-0.0641***	-0.0058
	(0.0037)	(0.0090)	(0.0095)	(0.0194)	(0.0229)	(0.0086)	(0.0100)
No. Deaths	412,266	187,767	224,499	50,913	59,671	136,854	164,828
No. Deaths Eligibles	148,565	67,686	80,879	20,886	23,127	46,800	57,752
No. Deaths Ineligibles	263,701	120,081	143,620	30,027	36,544	90,054	107,076
No. Deaths Eligibles (year before coverage)	13,755	6,341	7,414	1,972	2,075	4,369	5,339
No. Localities	18,797	18,797	18,797	12,873	13,318	17,495	17,860
No. Locality-Age cells (obs.)	375,940	375,940	375,940	257,460	266,360	349,900	357,200
No. Locality-Age Eligible cells (obs.)	186,340	186,340	186,340	127,110	131,550	173,320	176,970
No. Locality-Age Ineligible cells (obs.)	189,600	189,600	189,600	130,350	134,810	176,580	180,230
No. Locality-Age non-zero cells (obs.)	80,368	49,283	56,136	18,676	20,471	37,098	43,925
Locality Controls	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Locality FE	Y	Y	Y	Y	Y	Y	Y
Age FE	Y	Y	Y	Y	Y	Y	Y
Year x Age Eligible FE	Y	Y	Y	Y	Y	Y	Y
Year x Locality Eligible FE	Y	Y	Y	Y	Y	Y	Y
Age x Locality Eligible FE	Y	Y	Y	Y	Y	Y	Y

Notes: This table shows the results obtained from estimating the dynamic difference-in-difference-in-differences using the count of deaths as the dependent variable in a Poisson regression including the logarithm of population as an offset. Age corresponds to 2 age groups: 60 to 69, and 70 to 79. It includes locality, age, and year fixed effects, as well as locality-age eligibility, year-locality eligibility, and year-age eligibility fixed effects. Standard errors are clustered at the level of treatment: locality and age. The reported coefficient corresponds to percent changes by subtracting 1 from the rate ratio, i.e., $\widehat{\theta}_{ATT\%} = \exp(\widehat{\beta}) - 1$. Each estimate captures the effect post-treatment for those groups in eligible localities above 70 years of age relative to deaths in non-eligible localities. The period of analysis is from 2002 to 2011. All regressions are weighted using the population aged 60 to 79 in 2005, and control for locality-level time-varying covariates: Marginality index, *Progresa* penetration, Percentage of deaths medically certified, and lag of death registration. The population offset is interpolated from the 2000, 2005, and 2010 Census data at the locality-age-year level. Number of Deaths Eligible corresponds to those deaths reported as residing in eligible localities above or equal to age 70, a year before the coverage. ***p<0.01,**p<0.05,*p<0.1

Table 2: Difference in Differences in Discontinuities Results for Individual Outcomes

	70 y Mas	Earnings	Income	Employment	Hours Worked	Hours Worked (if >0)	Seguro Popular
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Pooled							
*1(Age ≥ 70) * Eligible * Post*	26.163***	-20.483	5.639	-0.218**	-6.351*	-0.900	0.173***
	(3.129)	(18.687)	(42.024)	(0.086)	(3.343)	(4.765)	(0.065)
R-W p-value	0.010	0.525	0.970	0.050	0.099	0.970	0.050
R-squared	0.270	0.051	0.050	0.063	0.055	0.020	0.171
Mean Dep. Var	4.90	54.41	255.05	0.39	17.54	41.29	0.22
Observations	15,860	15,860	15,867	15,860	16,017	6,804	12,457
Panel B: Male							
*1(Age ≥ 70) * Eligible * Post*	25.901***	-31.936	22.865	-0.240**	-7.068	1.243	0.215***
	(4.371)	(40.058)	(76.151)	(0.099)	(5.720)	(6.339)	(0.069)
R-W p-value	0.010	0.554	0.693	0.069	0.515	0.554	0.010
R-squared	0.214	0.072	0.094	0.121	0.103	0.031	0.169
Mean Dep. Var	5.62	89.33	353.29	0.58	27.19	44.49	0.22
Observations	7,719	7,719	7,728	7,719	7,853	4,799	6,068
Panel C: Female							
*1(Age ≥ 70) * Eligible * Post*	26.593***	-6.910	0.729	-0.247**	-7.598*	-5.293	0.156
	(3.888)	(6.183)	(37.592)	(0.122)	(4.263)	(7.820)	(0.133)
R-W p-value	0.010	0.545	0.960	0.149	0.208	0.713	0.545
R-squared	0.389	0.045	0.029	0.038	0.030	0.023	0.177
Mean Dep. Var	4.22	21.29	161.78	0.22	8.26	33.63	0.22
Observations	8,141	8,141	8,139	8,141	8,164	2,005	6,389
Weights	Y	Y	Y	Y	Y	Y	Y
Year of Survey FE	Y	Y	Y	Y	Y	Y	Y

Notes: This table shows the results obtained from estimating a difference-in-difference-in-discontinuities regression using individual-level outcomes as the dependent variable. The $1(Age \geq 70) * Eligible * Post$ estimates show results after the *70 y Más* program started between eligible and ineligible localities and before and after for those above the cut off. *70 y Más* is the self-reported amount of cash transfer received. Hours Worked (if >0) only includes non-zero hours worked. Income includes wages from job, wages from business, and secondary jobs, wages from previous months, all transfers (private and government, including *70 y Más*), income from business and property, and other income not from work. Means are weighted using survey weights. Standard errors are clustered at the household level. The period of analysis before the program started is 2005 and 2006 surveys, and after the program started is the 2008 and 2010 surveys. All regressions use survey weights and year of survey fixed effects. Observations across columns differ because we drop the upper 1% outliers for each outcome of interest. ***p<0.01,**p<0.05,*p<0.1

Table 3: Difference in Differences in Discontinuities Results for Household Outcomes

	70 y Más	Earnings	Income	Spending	Food Spending	Health Expenditure	No. Members
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Pooled							
*1(Age ≥ 70) * Eligible * Post*	27.526***	4.282	-21.452	103.633	-10.008	-9.078	0.042
	(4.129)	(79.623)	(94.842)	(67.363)	(18.109)	(6.829)	(0.330)
R-W p-value	0.010	0.980	0.980	0.366	0.861	0.564	0.980
R-squared	0.275	0.108	0.116	0.127	0.100	0.019	0.011
Mean Dep. Var	6.84	404.22	762.66	567.82	192.91	17.29	3.63
Observations	11,964	11,964	11,975	11,964	11,974	11,972	11,923
Panel B: Male							
*1(Age ≥ 70) * Eligible * Post*	26.919***	-8.522	38.532	80.617	-5.534	-6.397	-0.518
	(5.492)	(102.484)	(139.017)	(98.610)	(24.261)	(12.698)	(0.447)
R-W p-value	0.010	0.950	0.950	0.713	0.950	0.911	0.554
R-squared	0.267	0.124	0.143	0.152	0.127	0.023	0.014
Mean Dep. Var	7.95	391.31	781.00	573.18	197.02	17.35	3.74
Observations	7,493	7,493	7,495	7,499	7,493	7,509	7,437
Panel C: Female							
*1(Age ≥ 70) * Eligible * Post*	27.440***	13.726	-119.728	123.017	-12.373	-11.612	1.154**
	(5.574)	(112.876)	(142.511)	(103.084)	(31.509)	(16.406)	(0.511)
R-W p-value	0.010	0.931	0.782	0.455	0.832	0.782	0.059
R-squared	0.275	0.088	0.085	0.098	0.068	0.022	0.011
Mean Dep. Var	4.97	425.84	731.99	558.82	186.04	17.17	3.45
Observations	4,471	4,471	4,480	4,465	4,481	4,463	4,486
Weights	Y	Y	Y	Y	Y	Y	Y
Year of Survey FE	Y	Y	Y	Y	Y	Y	Y

Notes: This table shows the results obtained from estimating a difference-in-difference-in-discontinuities regression using household-level outcomes as the dependent variable. The $1(Age \geq 70) * Eligible * Post$ estimates show results after the *70 y Más* program started between eligible and ineligible localities and before and after for those above the cut off. 70 y Más is the self-reported amount of cash transfer received. Hours Worked (if >0) only includes non-zero hours worked. Income includes wages from job, wages from business, and secondary jobs, wages from previous months, all transfers (private and government, including *70 y Más*), income from business and property, and other income not from work. Means are weighted using survey weights. Standard errors are clustered at the household level. The period of analysis before the program started is 2005 and 2006 surveys, and after the program started is the 2008 and 2010 surveys. All regressions use survey weights and year of survey fixed effects. Observations across columns differ because we drop the upper 1% outliers for each outcome of interest. ***p<0.01,**p<0.05,*p<0.1