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Non-contributory Pensions

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Abstract: The creation of non-contributory pension schemes is becoming increasingly common as countries struggle to reduce poverty. Drawing on data from Mexico's Adultos Mayores Program (Older Adults Program) --a cash transfer scheme aimed at rural adults over 70 years of age-- we evaluate the effects of this program on the well-being of the beneficiary population. Exploiting a quasi-experimental design whereby the program relies on exogenous geographical and age cutoffs to identify its target group, we find that the mental health of elderly adults in the program is significantly improved, as their score on the Geriatric Depression Scale decreases by 12%. We also find that the proportion of treated individuals doing paid work is reduced by 20%, with most of these people switching from their former activities to work in family businesses; treated households show higher levels of consumption expenditures (on average, an increase of 23%). Very importantly, we also rule out significant anticipation effects that might have been associated with the program transfers. Thus, overall, we find that non-contributory pension schemes target to the poor in developing countries can improve the well-being of poor older adults without having any indirect impact (through potential anticipation effects) on the earnings or savings of future program participants.

Key words: Non-contributory pensions, poverty, mental health and well-being.

JEL Codes: H4, H3, I1 and I3.

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1. Introduction

Undoubtedly, pensions are one of the most important components of a social security system. Pensions improve welfare by helping individuals to smooth out their consumption levels over their life cycle. By contributing to a pension, people consume less than they produce in the present so that they can consume more after they retire and are no longer earning an income. Pensions allow people to withdraw from the labor market without fear of falling into poverty or impoverishing their relatives.

The most common type of pension in the U.S and many other high-income countries is a contributory plan that is financed by taxing own labor income. However, contributory plans have proved to be difficult to fully scale up in economies with large informal labor markets (Dethier, 2007; Galiani and Weinschelbaum, 2012; and Levy 2007). As a result, in the developing world, large segments of the population are not covered by contributory pension schemes. In Latin America, contributory pension coverage ranges from 10% to about 60% (Dethier et al., 2010). In Mexico, the site of this study, coverage is only 23%. Most of these countries have instead turned to non-contributory pension systems targeted on the basis of age and income (Holzmann et al., 2010; and McKinnon and Sigg, 2006).[1]

In this paper we study the effect of an at-scale non-contributory pension program on the economic security and well-being of pensioners and their families. The Programa de Atención a Adultos Mayores en Zonas Rurales (Assistance for Older Rural Adults Program) provides a nationwide non-contributory universal pension scheme for seniors.[2] At the time that the data used in this study were collected, adults were eligible for the pension if they were over 70 years old and lived in communities with fewer than 2,500 inhabitants.[3] Applicants must provide documentation to prove their age and place of residence. Beneficiaries receive a cash transfer of 1,000 Mexican pesos (USD 90) every two months. The beneficiaries of the program are also invited to take part in workshops and social development activities. The program started up in 2007 and had extended coverage to 2.1 million beneficiaries living in 76,000 communities across Mexico over time. Its budget is just over 13 billion pesos, or about 0.1% of GDP, making it the second-largest social program in Mexico after the Oportunidades Program (Rubio and Garfias, 2010; Aguila et al., 2013).

We look at a number of important questions. We first ask whether the economic security afforded to beneficiaries through the program increased their well-being as measured by mental health. We then investigate the extent to which a lack of economic security prevents older adults from retiring; we do this by examining the program's impact on labor-market activities. We also seek to determine whether the program reduces the economic burden on the beneficiary's family; we do this by gauging the extent to which the transfer increases shared household consumption levels. Finally, we ask whether individuals who are nearing the age at which they will become eligible for the program start to reduce their involvement in the labor market and begin to draw on their savings ahead of time in anticipation of receiving the cash transfers from this program in the near future.

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[1] For further information on non-contributory pension schemes, see http://www.pension-watch.net.

[2] The program is known as "70 y más" (70-and-over).

[3] The program completed its expansion to all villages with less than 2500 inhabitants in late 2008. Subsequently, it started expanding to localities up to 20,000 inhabitants (Aguila et al., 2013 Rubio and Garfias, 2010, and Sedesol, 2012). However, under explicit agreement with the Government, the control localities included in this study were not incorporated nor informed about the possible incorporation until at least late 2009 well after the 2008 follow-up survey.

We begin by sketching out a conceptual framework to guide the empirical analysis and help us to interpret the results. The framework provides the theoretical underpinnings for our identification strategy as applied in the empirical work. We then test the predictions of the model using a quasi-experimental design that exploits discontinuities in the age and geographical eligibility requirements of the program to identify causal impacts.

Our results provide the first evidence that non-contributory pension systems significantly improve beneficiary mental health as measured by the Geriatric Depression Scale (GDS). These mental health effects are also present in qualitative assessments of the program, in which a large number of beneficiaries specifically commented on being "…relieved from the stress of having to work to provide support for their families" (Tellez-Rojo et al., 2012) Our results are related to the recent works of Finkelstein et al. (2012) and Baicker et al. (2013), which examine extending access to health insurance under Medicaid to a low-income, uninsured adult population. They find, within the framework of a randomized controlled experiment, that Medicaid coverage lowered self-reported depression. These results are very important since mental health is a well-accepted and critical measure of quality of life among the elderly (Campbell et al., 1976; Walker, 2005), and about 121 million people globally, many of who are older and suffer from chronic depression (World Health Organization (WHO), 2003).

We also find that beneficiaries reduced their participation in formal gainful employment outside the home in favor of less stressful and less demanding informal unpaid work within the household. The share of beneficiaries working for pay fell from 23% to 18%, while the share who were working without pay in family enterprises rose from 13% to 19%. Analogously, hours in wage work fell by 2.6 per week, and hours in unpaid work increased by 2.2 per week. These results are consistent with an international comparison of pension schemes in 11 countries that shows that increasing social security is associated with an increase in the rate of retirement of older adults from formal employment (Gruber and Wise, 1998).

These effects of the program on labor-force participation rates are also consistent with the findings concerning improved mental health results. While the literature indicates that unemployment among adults is usually associated with lower levels of life satisfaction and higher levels of depression,[4] recent research decomposes the impact of unemployment on mental health into: (1) a "saddening" effect generated by not being able to find work, and (2) a "time-composition" effect, whereby happiness increases as people are able to devote more of their time to more pleasant activities (Knabe et al., 2010; Krueger and Muller, 2012; and Ruhm, 2001). As people age, the time-composition effect becomes more important. The economic security afforded by a pension allows older adults, who place a great deal of value on the time-composition effect, to reduce their involvement in the labor market and enjoy life.

We also show that the program is associated with a significant increase in the material well-being of the household in which the beneficiary lives. In rural Mexico, almost without exception, people over 70 years of age live with another family (usually their children or other relatives). We find that 71% of the pension is spent on shared household consumption, which translates into a 23% increase in household consumption. The marginal propensity to consume the pension is close to estimates of the marginal propensity to consume (0.78) out of Oportunidades Program cash transfers given to female heads of household (Gertler et al., 2012). This suggests that the beneficiaries of the Assistance for Older Rural Adults Program fully share their transfers with the families with which they live. These results are also

[4] See, for example, Clark and Oswald (1994), Winkelmann and Winkelmann (1998), Di Tella et al. (2001), Blanchflower and Oswald (2004) and Kahneman et al. (2004)).

consistent with evidence from South Africa that shows that the expansion of a non-contributory pension system for older black adults after the end of apartheid was effective in reducing poverty (Case and Deaton 1998).[5]

We also find no negative effects on the labor supply of other adults in the beneficiary's household. Gasparini et al. (2007) argue that pensions are essential to keep poverty among older adults low. However, this result assumes that there are low disincentive effects on labor supply and earnings. Our work provides some of the first evidence that non-contributory pension systems clearly have positive effects on material well-being without generating significant negative labor-supply effects on working-age members of the household.

Finally, one general concern about all pension systems is whether their implementation affects the work and savings behavior of the younger population in anticipation of a pension in the future (e.g., Feldstein, 1974). However, one of the key predications is that anticipation effects depend on a person having access to liquidity and/or credit. Most people who work in informal labor markets in developing countries are subject to major liquidity and credit constraints (Karlan and Murdoch, 2010), which suggests that we are unlikely to find anticipation effects in connection with this program.[6] In this paper we present evidence to support this hypothesis as applied to a poor rural population in a developing country. Our results do not provide empirical support for the presence of anticipation effects in regard to household total labor earnings or savings (i.e., consumption anticipation).

The rest of the paper is organized as follows. In the next section, we present a conceptual framework for our empirical analysis. In Section 3, we describe the strategy used to identify the causal effects of interest, while in Section 4, we describe our dataset and summary statistics. Section 5 presents the empirical results and Section 6 concludes the paper.

2. Conceptual Framework

We present a very simple model in which agents live for two periods and consume two goods: a good purchased in the market with a price normalized to 1, and a *family good* produced at home. Utility in period *t* is given by:

$$U(C_t, X_t) = \alpha ln(C_t) + (1-\alpha)\ln(X_t), \qquad \alpha \in (0,1)$$

At the beginning of each period, each person is endowed with one unit of time, which she must decide how to allocate between working in the market for a wage (*w*), which we will assume it is fixed over time, and working at home to produce good *X*. Let *h* be the amount of time that the person devotes to market work and *1-h* be the amount of time that the person devotes to working at home. For simplicity's sake, we assume that the technology used for home production is $X_t = 1 - h_t$. In the first

[5] Duflo (2000) also shows that the expansion of the pension system had positive effects on child health. See also Ardington et al. (2009), who quantify the labor supply responses of prime-aged adults to the presence of pensioners in their households, using longitudinal data collected in South Africa.

[6] In fact, there is also substantial evidence of credit constraints in rural Mexico, the site of our empirical investigation. See, for example, Angelucci (2012), Gertler et al. (2012) and Love and Sánchez (2010) for evidence suggesting the presence of credit constraints in both rural Mexican households and firms.

period, families are able to borrow (or save) at the exogenous interest rate of *r,* with the loan to be repaid in the second period. They also may receive a government pension T_t in period *t*. If she receive a pension in both periods, we assume that they are of an equal amount and denote them as *T*.[7] Finally, we assume there is no uncertainty about future realizations of the variables involved in their decision process.

Let F be the amount that a person borrows in the first period and let β be the discount rate; in that case, the dynamic optimization problem faced by the agent is $max_{h_1,h_2,F}\, U(C_1, X_1) + \beta U(C_2, X_2)$ subject to the budget constraints: $C_1 \leq wh_1 + T_1 + F$ and $C_2 \leq wh_2 + T_2 - iF$, where *i* = 1 + *r*. In an interior solution[8], the first-order conditions are:

$$\frac{\alpha w}{wh_1+T_1+F} = \frac{1-\alpha}{1-h_1} \tag{1}$$

$$\frac{\alpha w}{wh_2+T_2-iF} = \frac{1-\alpha}{1-h_2} \tag{2}$$

$$\frac{1}{wh_1+T_1+F} = \frac{i\beta}{wh_2+T_2-iF} \tag{3}$$

Equations 1 and 2 indicate that the marginal gain from working in the market must be equal to the marginal gain from working at home; equation 3 is a standard Euler equation.

For expositional simplicity, we assume that $i\beta = 1$, which enables us to isolate the incentives to borrow embodied in the differences in pension benefits across periods.[9] In this case, the solution simplifies to:

$$h_1 = \alpha - \frac{1-\alpha}{w}\left[\frac{T_2+iT_1}{1+i}\right] \quad or \quad C_1 = \alpha(w + \frac{T_2+iT_1}{1+i}) \tag{4}$$

$$h_2 = \alpha - \frac{1-\alpha}{w}\left[\frac{T_2+iT_1}{1+i}\right] \quad or \quad C_2 = \alpha(w + \frac{T_2+iT_1}{1+i}) \tag{5}$$

$$F = \frac{T_2-T_1}{1+i} \tag{6}$$

Consider now three types of individuals, who are defined by whether and when they receive a pension:

(1) *Treatment (TT):* This type receives a pension in both periods;

[7] We do not consider explicitly the possibility that a family could also rely on its initial existent assets to smooth consumption over time since that possibility would only reinforce the anticipation effects of future transfers that we highlight in the present analysis.

[8] A necessary and sufficient condition is $T < \frac{\alpha w}{1-\alpha}$

[9] If this assumption did not hold, then agents would save (if $i\beta > 1$) or borrow (if $i\beta < 1$) even when there were no transfers. The main results still hold but are less straightforward (see Appendix A for the complete solution to the model).

(2) *Internal Control (IC):* This type receives a pension only in the second period; and
(3) *External Control (EC):* This type never receives a pension.

This taxonomy allows us to explore two effects: (1) the treatment effect of pensions on labor supply and consumption, which we can examine by comparing the solutions for TT and EC, and (2) the anticipation effect of the program, which can be discerned by comparing EC and IC in the first period.

Treatment Effect. Assuming that neither TT nor EC will lend or borrow (*F=0)*, so, in both periods:

$$h^{EC} = \alpha \tag{7}$$

$$h^{TT} = \alpha - \left(\frac{1-\alpha}{w}\right) T \tag{8}$$

The treatment effect of giving the pension is then simply:

$$h^{TT} - h^{EC} = -\left(\frac{1-\alpha}{w}\right) T \tag{9}$$

$$C^{TT} - C^{EC} = \alpha T \tag{10}$$

As agents experience an exogenous increase in their income owing to receipt of the pension, they replace working hours in the market with hours working at home and hence consume more of both goods.

Anticipation Effect. The *IC* group knows that income will increase in the second period as a result of the pension and therefore borrows in order to spend some of the pension money in the first period. The first-period working and consumption decisions are:

$$h_1{}^{IC} = \alpha - \left(\frac{1-\alpha}{w(1+i)}\right) T_2 \tag{11}$$

$$C^{IC} = \alpha \left(w + \frac{T_2}{1+i}\right) \tag{12}$$

In this case, individuals reduce their labor in the market and increase their consumption of both goods in the first period in anticipation of receiving the pension in the second period. The higher the cost of borrowing is, the smaller the anticipation effect will be.[10] If families were completely credit-constrained

[10] This also results because we assumed that agents are liquidity constrained or directly lack initial assets. If this were not the case, families might still anticipate future transfers and increase consumption of both types of goods by using their previously accumulated assets.

because interest rates were prohibitive,[11] then the anticipation effect would be zero (at least, as we already mentioned, they could instead use previous accumulated assets to anticipate the future stream of transfers). In that case, IC and EC groups would have the same outcomes in the first period.

Alternative Treatment Effects. Suppose that we were to estimate the treatment effects of the pension by comparing TT to IC, as opposed to TT and EC, in the first period:

$$h^{TT} - h^{IC} = -\left(\frac{i(1-\alpha)}{w(1+i)}\right)T \tag{13}$$

$$C^{TT} - C^{IC} = \left(\frac{\alpha i}{1+i}\right)T \tag{14}$$

We can see from a comparison of equations (9) and (10) with equations (13) and (14) that we might underestimate those treatment effects, since IC anticipates the pension in period 2 by reducing labor supply and increasing consumption in period 1. IC is only a valid comparison group when anticipation effects are null in terms of both labor and consumption outcomes.

Thus, the theoretical model illustrates the potential effects of the program in a relatively straightforward manner: the treatment effects on labor and consumption arise from the fact that the pension transfer works as an exogenous increase in income which the beneficiary uses to consume more of both types of goods. Anticipation effects exist whenever future participants in the program can anticipate the future stream of earnings to finance current consumption and switch to non-paid work in the home. The model also provides us with a reasonable guide for our empirical work by outlining the equations that describe both types of effects.

3. Identification Strategy

The empirical challenge is to create the three types of groups described in our model. While we would like to randomly assign eligible individuals to groups given their age, we are not able to do so because the government rolled out the program for all eligible persons at the same time. Instead, we identify plausible comparison groups from the program eligibility cutoffs across two dimensions: (1) age, as people have to be at least 70 years old to take part in the program, and (2) geography, as people have to live in communities with fewer than 2,500 inhabitants (SEDESOL, 2007). We then use the panel structure of the data to control for unobserved heterogeneity.

a. Treatment and Comparison Groups

Our design exploits these two dimensions as shown in Figure 1. We assign adults between 70 and 74 years of age at baseline in localities with fewer than 2,500 inhabitants to the Treatment Group and adults between 66 and 69 years of age at baseline in the same localities to the Internal Control Group. Adults between the ages of 70 and 74 at baseline in non-treated localities are in the External Control Group 1 and those between ages 66 and 69 at baseline in the same localities are in the External Control Group 2.

[11] If we posit a very high interest rate on loans, *ceteris paribus*, the assumption being that $\beta i = 1$ will not hold, so we would have to differentiate between interest rates on loans and deposits.

Figure 1: Quasi-Experimental Design

		Locality Population	
		Treatment Localities (500 – 2,500 inhabitants)	Control Localities (2,501 – 3,300 inhabitants)
Age	70 – 74	Treatment Group (TT)	External Control Group 1 ($EC1$)
	66 – 69	Internal Control Group (IC)	External Control Group 2 ($EC2$)

We estimate the treatment effect by comparing the outcomes of TT with those of $EC1$. The people in $EC1$ are the same age as those in TT and live in localities that are right above the population cutoff, so they will not receive the transfer.[12] Our particular group structure also allows us to determine the nature of anticipation effects, if any, by comparing the outcomes of IC and $EC2$. While people in IC and $EC2$ are of the same age, those in the IC group will receive the pension in the near future, whereas those in the $EC2$ group will not.

b. Unobserved Heterogeneity

An intuitive way of calculating the treatment effect would simply be to estimate the difference between the average of the relevant indicator in the treatment group and the same average in the control group. However, for this to be a consistent estimator of the parameter of interest, there should be no difference between the characteristics of the two groups apart from their treatment status. The analysis of the baseline survey, presented below, indicates that there are some non-negligible differences between the treatment and external control groups – that is, the groups are not perfectly balanced in some pre-treatment characteristics.

We use a difference-in-differences (DID) approach to control for these differences. Using DID, we can compare the differences in *changes* in means between the two groups for the period between the baseline and the follow-up survey. We estimate the DID regression models using the individual or household as the unit of observation and conditioning on unit and year fixed effects. Specifically, in this way, we control for individual and locality characteristics that are time invariant, as well as for secular trends that are common to both treatment and comparison groups.

Specifically, we estimate the following empirical model:

$$Y_{ijt} = \alpha_i + \gamma_t + \beta\, TT_{jt} + e_{ijt} \qquad (15)$$

[12] An alternative that other studies use is to rely on IC as the comparison group. However, in that case, a bias could be generated by anticipation effects and by the nonlinear aging effects of the passage of time on both groups.

where Y_{ijt} is the outcomes for individual i living in locality j in year t, α_i is an individual fixed effect, γ_t is the year fixed effect, TT_{jt} indicates treatment status and varies only by locality and year, and β is our parameter of interest, measuring the treatment effect of the pension program on the outcomes of interest. To assess the sample variability of our estimates, we cluster the standard errors both at the locality-year and locality level.

c. Robustness Tests

The maintained assumption needed for this approach to yield consistent estimates of the causal impacts of the intervention is that the changes observed in the treatment and control groups would have been the same in the absence of the program. While this assumption is not directly testable, we can test it indirectly by evaluating whether indicators that should not be affected by the intervention change by the same amount in the treatment and comparison communities. We implement this test by estimating versions of the DID specification in equation (15) with placebo outcomes.

We consider food prices and wages. On the one hand, the disbursement of pensions could have a direct effect on prices and wages, e.g., the transfers could increase market demand for food, thereby raising local market food prices (Angelucci and De Georgi, 2009; and Lehmann, 2013). On the other hand, prices and wages could also change differentially between the treated and control areas if there were differential secular trends other than equilibrium market effects induced by the program. We provide empirical evidence that prices and wages do not correlate with the introduction of the Assistance for Older Rural Adults Program in treatment communities, which therefore supports the identifying assumption used in this study.

4. Data

a. Sample

The data for our entire analysis come from two surveys that were carried out by the Mexican National Public Health Institute) in the early stages of the program's implementation (Instituto Nacional de Salud Pública, 2007 and 2008). The first survey –which we will call the baseline survey– was carried out between September and November 2007. This survey collected information on individuals and households before the disbursement of cash transfers took place. The second –or follow-up survey– was carried out between November and December 2008, once the program had been operating for almost a year. Both surveys have a household module and a module for which the older adult was interviewed individually. The data come from surveys that were collected in the states of Guerrero, Querétaro, Michoacán, San Luis Potosí, Puebla, Veracruz and Hidalgo.

The surveys collected detailed information from female heads of household concerning household demographic structure, household members' labor activities and outcomes, and household consumption. The surveys were also used to collect information directly from the older adults in the relevant age range about their labor-market activities and mental health.

We measured mental health using the Geriatric Depression Scale (GDS) developed by Sheikh and Yesavage (1986). The index is based on a 15-item yes/no questionnaire that contains queries about whether one feels satisfied with life, whether one is bored or lacks attention from other people, whether one prefers to stay at home rather than going outside or feels full of energy, and so on. The answers to each question are then compared with those corresponding to a person with no trace of depression. Each opposite answer is assigned a value of 1. The GDS score is simply the sum of the

points assigned to the answers, with a higher score reflecting the presence of more symptoms of depression. See Table B1 in Appendix B for a full definition of all the variables used in this paper.

From the baseline survey, we retain in our sample the households with at least one adult between 66 and 74 years of age. We have at the baseline survey 3,792 individuals in that age range. From this sample, we use data for persons for whom we observe all outcomes both in 2007 and 2008 so that we can form a panel. Our final dataset contains a panel of 3,556 individuals for which we have complete data on labor-market outcomes, out of which 168 live in a household with another person over 65 years of age. Our dataset includes complete data for both adults for 158 cases and for one of the two adults for 10 cases. Thus, we have a total of 3,477 households in 463 localities.

Out of the 3,388 households with one adult, a shorter version of the questionnaire without a module on expenditures was applied to 444 randomly chosen households in localities smaller than 2500 inhabitants to reduce survey costs and stay within the limited budget. Out of the 2,944 for which a module on expenditures was applied, we have complete information on total expenditures on both waves of the survey only for 2,873. Total expenditure is defined as the sum of food and non-food expenditures by the household. Non-food expenditures include transportation, tobacco, cigarettes and alcohol, newspapers and magazines, hygiene products, medicine, energy, home utensils, clothes, expenditures on education and on social events.

b. Descriptive Statistics

Tables 1 and 2 present descriptive statistics for the individuals and their households, respectively, for the four groups described in Figure 1 using the 2007 baseline survey. Panel A reports the statistics for the 70-74 age group, disaggregated by treatment and control locality, and Panel B reports the same statistics for the 66-69 age group. There appears to be no difference in depression symptoms between treatment and control localities for either age group. Persons in the older age group in treatment localities are more likely to engage in household activities for no pay and have slightly more years of schooling than their counterparts in the control localities. While the differences in individual characteristics between treatment and control localities are small, households in the control localities do appear to be wealthier in terms of both labor income and consumption.

5. Results

In this section first present our main findings and then the results of the robustness tests.

a. Mental Health

The first row of Table 3 shows that the program has a significant negative effect in terms of the GDS score – that is, treated individuals are less depressed than non-treated individuals. The average GDS score for treated persons is 0.424 points less than the one for people in External Control Group 1 –a decrease of 12%. As mentioned above, we present two standard errors for our point estimates. Within parentheses, we report standard errors clustered at the locality-year level, while, within brackets, we present standard errors clustered at the locality level. The effect is significant at conventional levels for both approaches used to assess the sample variability of our point estimators. Table 3 also shows no significant anticipation effects in terms of mental health. As a robustness check, we also estimate the anticipation effect when excluding adults who were 69 years of age at baseline, which is a pertinent robustness check to this test since some of those individuals might have turned 70 between both surveys. The results remain unchanged.

b. Labor Supply

Table 3 also reports the results for participation in the labor market. The program does not appear to have an effect on overall labor-force participation or hours worked, which remain stable at around 0.37 for participation and 14 hours worked per week. However, the program does have a significant effect on the composition of work. The pension is associated with a reduction in paid work outside the house and an increase in unpaid work on a family farm or in a family business. Specifically, the proportion of individuals in paid work decreases by 18% in relation to the baseline level (from 0.23 to 0.18), while unpaid work rises from 0.13 to 0.19 (a 48% increase from baseline levels). Similarly, the level of substitution is also equal in terms of hours per week, as beneficiaries engaged in 2.6 fewer hours of paid work and 2.2 more hours of unpaid work. All these substitution effects are statistically significant at conventional levels. A similar pattern of behavior is seen for the anticipation effects, although it is less pronounced for all these outcomes in relation to baseline levels and in most cases is not statistically significant at conventional levels. In fact, if the group of persons of 69 years of age is excluded from the analysis then none of the anticipation results are statistically robust.

c. Household Income and Consumption

In Table 4 we present the estimates of treatment and anticipation effects for household labor income and consumption expenditures per adult equivalent.[13] Since we want to interpret the results on this section through the lens of our theoretical model, and check that the changes in earnings and consumption satisfy the change in the budget constraint of the households, we do not include the analysis the households with two beneficiaries of the program.

Expenditure and labor income data is subject to large measurement errors. Therefore, before conducting the analysis, we decided first to drop potential outliers. We compute the changes in total consumption and labor income and drop the observations for which the absolute difference between baseline and follow up values for either variable is at the top 5% of the corresponding distribution. We end up with 2,577 households in the usable dataset. This induced attrition in the usable dataset (2,944 to the 2,577) does not correlate with treatment status and demographic baseline characteristics. It is positively correlated with baseline household income, baseline household consumption and household size. This suggest that the large changes in consumption and income discarded for the analysis are not the result of the program and are potentially the result of large measurement errors among the households at the top of the distribution (see Table B2 in Appendix B).

The results show that the program had a negative effect on household labor income per adult equivalent of 34 pesos, which amounts to 17% of household labor income. Most of the effect vanishes when the income of the treated adult is not considered. On the other hand, consumption increases by 63 pesos per adult equivalent in the treated households, which amounts to a 23% rise in consumption. The reduction in income plus the increase in consumption is equivalent to about 97 pesos, which is very close to --and not statistically different from (P-value = 0.80)-- the pension transfer amount per adult equivalent of 89 pesos per month.[14] Hence, 71% of the transfer is used for consumption and 28% is

[13] The adult equivalence scale weights each person older than 12 as 1 and those aged 12 or younger as 0.5.

[14] Our results are at odds with Juárez (2009) who examines the effect of public pension transfers on private transfers by exploiting an increase in public pensions in Mexico City in 2001. She uses is a set of repeated cross-sections to estimate the effect of income on private transfers instrumented by a dummy representing pension eligibility post 2001. Identification is driven off the cross-sectional difference in eligibility in Mexico City compared to individuals outside Mexico City. Her analysis does not include either time fixed effects or location fixed effects. She finds that a one-peso increase in the income is associated with a reduction of private transfers by 86 cents. In

taken in the form of increased leisure (reduced labor supply). We find no evidence of anticipation effects either on labor earnings or on consumption.[15]

d. Robustness Tests

In this section we test whether the trend in food prices and wages was different across treated and control localities. We estimate DID regressions to test whether there is a treatment effect, i.e., do wages or prices change over the study period at a different rate in treatment localities than in control localities.

Using the sample of working people, we estimate the DID model in equation (16) for log wages of people aged 18 - 65 (see Table B4 in Appendix B) and for people aged between 66 and 88 years (see Table B5 in Appendix B). The individual fixed effects control for bias from the usual socio-demographics included in wage regressions. We estimate separate models for males and females and a number of specifications, including a single treatment effect and separate treatment effects by age and level of education. Overall, there appear to be no changes in hourly wages as a result of the implementation of the program.

We measure food prices at the locality level using the module on consumption expenses. We examine both prices for individual food items and a price index where the weights are the average budget shares for household consumption collected at baseline using a model similar to the one in equation (15) but estimated with data aggregated to the locality level, and where individual fixed effects are therefore replaced by locality fixed effects. Thus, we only present standard errors clustered at the locality level. The results show that the program has no effect on prices, since by large there was no difference in the change in prices between treatment and control localities over the study period (see Table B6 in Appendix B).

The estimates for the price and wage equations rule out the possibility of equilibrium effects and other differential secular trends in prices and wages that could have potentially invalidated the identification assumptions underlying our econometric model. While these results do not completely eliminate the potential sources of differential secular trends, they do provide substantial reassurance about the validity of our identification strategy.

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contrast, our results suggest that there is no crowding-out. Otherwise, the effect on consumption and income would not add up to the pension transfer. One explanation for the difference maybe that our sample is comprised of rural villages with less than 2500 inhabitants for which such transfers are substantially smaller than the large urban population of 20 million inhabitants in Mexico City. In our sample, only 8% of households report a positive private transfer at baseline compared to 19% for the 70+ group in Juárez (2009). Unfortunately, the 2008 survey did not collect private transfer information.

[15] These results are robust to including the few households with two treated adults. The results are also robust to excluding the 1% top of the distribution of the changes in total consumption; labor income and labor income excluding the senior (see Table B3 in Appendix B). Though the point estimates are somewhat larger, and the drop in household income is statistically significant, qualitatively, the results are not different. We still find that the reduction in income plus the increase in consumption is not statistically different from 89 pesos -the pension transfer amount per adult equivalent per month- (P-value = 0.15).

7. Conclusions

In many developing countries, the recent large increase in life expectancy has resulted in large increases in poverty among the elderly. Until recently, countries relied on traditional contributory pension schemes to cater for the needs of people upon retirement. However, high levels of labor market informality have limited the effectiveness of these systems as a means of providing adequately for older adults. Faced with rising poverty among the elderly and the inadequacy of contributory pension systems, countries such as South Africa, Brazil and Mexico have adopted non-contributory pension schemes in an effort to extend coverage to all members of their older population. Although very popular, these non-contributory plans have received little attention from empirical economists.

Our paper is one of the first to provide conclusive, comprehensive evidence about the effect of these schemes in terms of various outcomes in the case of Mexico. We first sketched out a theoretical framework to guide our analysis using a model that allows for possible anticipation effects in response to future benefits. The model predicts a shift from paid formal work to unpaid informal work and an increase in consumption. The model also identifies early reductions in participation in the labor force and increased consumption (lower savings) in anticipation of future benefits, depending on the ability to borrow. The anticipation effects disappear in the presence of liquidity and credit constraints. We use a quasi-experimental design that relies on the exogeneity of the age and geographical eligibility requirements to which we applied the DID analysis.

Our results are encouraging in a number of different ways. Most importantly, mental health appears to improve substantially, as is indicated by the 12% decrease in the group's score on the Geriatric Depression Scale. This result should not be taken lightly, since good mental health helps to improve happiness and is a key determinant of overall well-being. The program also appears to be effective in allowing older poor people to exit the formal labor market; it should be noted, however, that they did not completely retire but instead continued to work in the delivery of unpaid services on family farms or in family businesses. We find that 71% of the pension was used to finance an increase in household consumption of about 23%. However, family labor earnings fell, which indicates that 38% of the pension was used to offset reductions in labor earnings. Finally, we find no evidence of anticipation effects in respect of labor earnings or consumption, which makes sense in view of the lack of liquidity and the credit constraints existing in rural Mexico.

Overall, the results are very positive, since the program appears to lead to a substantial improvement in the material and psychological living standards of older people residing in rural areas. Moreover, the absence of notable anticipation effects suggests that the equilibrium costs of the policy may not be so sizeable. Thus, the program appears to be an effective tool for improving the living conditions of older people who are living in poverty.

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Table 1: Baseline Means of Individual Variables

	Panel A: Individuals Age 70-74			Panel B: Individuals Age 66-69		
	Treatment Locality (<2500 residents)	Control Locality (2500-3300 residents)	P (value for test of equality)	Treatment Locality (<2500 residents)	Control Locality (2500-3300 residents)	P (value for test of equality)
Age	71.90	71.93	0.621	67.29	67.35	0.337
Male	0.50	0.35	0.000	0.59	0.51	0.006
Years of Schooling	1.86	1.39	0.005	2.00	1.67	0.140
Married	0.66	0.46	0.000	0.61	0.58	0.017
Geriatric Depression Scale	3.52	3.69	0.339	3.55	3.97	0.070
Worked last week	0.36	0.31	0.078	0.49	0.47	0.290
Worked last week for pay	0.23	0.23	0.926	0.34	0.31	0.343
Worked last week for no pay	0.13	0.09	0.029	0.16	0.16	0.992
Hours worked last week	14.20	10.93	0.003	19.84	16.73	0.013
Hours worked last week for pay	9.28	7.72	0.074	13.37	11.27	0.086
Hours worked last week for no pay	4.92	3.21	0.036	6.48	5.46	0.299
Labor Earnings	176.81	200.38	0.509	303.95	291.60	0.838
Sample Size	1,144	806		954	652	

Notes: P-values are for tests of the null hypothesis of equality of means and account for correlated errors within locality.

Table 2: Baseline Means of Household Variables

	Panel A: Individuals Age 70-74			Panel B: Individuals Age 66-69		
	Treatment Locality (<2500 residents)	Control Locality (2500-3300 residents)	P (value for test of equality)	Treatment Locality (<2500 residents)	Control Locality (2500-3300 residents)	P (value for test of equality)
Income per adult equivalent	198.83	212.23	0.515	167.56	202.09	0.289
Consumption per adult equivalent	270.72	422.91	0.000	267.71	366.97	0.000
Household size in adult equivalents	5.60	4.02	0.000	6.10	4.55	0.000
Age of household head	68.99	69.62	0.353	64.75	67.14	0.000
Male household head	0.74	0.57	0.000	0.79	0.67	0.000
Indigenous household Head	0.05	0.09	0.058	0.07	0.10	0.13
Sample Size	724	693		605	555	

Notes: P-values are tests of the null hypothesis of equality of means and account for correlated errors within locality.

Table 3: Impact on Individual Mental Health and Labor Supply

	Treatment Effect	Anticipation Effect	Anticipation Effect excluding 69 year olds
Geriatric Depression Scale	-0.424	0.035	0.100
	(0.17)**	(0.19)	(0.188)
	[0.241]*	[0.268]	[0.266]
Worked last week	0.014	0.018	0.028
	(0.016)	(0.018)	(0.02)
	[0.023]	[0.025]	[0.029]
Worked last week for pay	-0.047	-0.037	-0.014
	(0.016)***	(0.026)	(0.029)
	[0.023]**	[0.036]	[0.041]
Worked last week for no pay	0.061	0.055	0.042
	(0.014)***	(0.022)**	(0.025)*
	[0.02]***	[0.031]*	[0.035]
Hours worked last week	-0.44	-1.02	-0.43
	(0.81)	(0.93)	(1.07)
	[1.145]	[1.31]	[1.51]
Hours worked last week for pay	-2.61	-2.00	-1.19
	(0.73)***	(1.09)*	(1.23)
	[1.03]**	[1.54]	[1.74]
Hours worked last week for no pay	2.17	0.98	0.75
	(0.59)***	(0.81)	(0.95)
	[0.83]**	[1.15]	[1.35]
Sample Size	1950	1606	1267

Notes: Row represent different dependent variable in a regression model. The first column reports the difference-in-difference estimated effects of treatment (i.e. pension) on the dependent variable from a regression that also controls for individual fixed effects and year fixed effects using the data from the T and $EC2$ groups. The second and third columns reports the difference-in-difference estimated effects of treatment on the dependent variable from a regression that also controls for individual fixed effects and year fixed effects using data from the IC and $EC1$ groups. Standard errors clustered at the locality-year level in parenthesis. Standard errors clustered at the locality level in brackets. *, **, *** indicate that the estimates coefficient is significantly statistically different from zero at the 0.10, 0.05 and 0.01 level, respectively.

Table 4: Impact on Household Income and Consumption Expenditure

	Treatment Effect	Anticipation Effect	Anticipation Effect excluding 69 year olds
Income per adult equivalent	-33.998	-4.804	4.008
	(15.823)**	(17.838)	(19.991)
	[22.394]	[25.247]	[28.295]
Income per adult equivalent excluding senior	-13.125	-4.558	-3.981
	(9.856)	(11.726)	(13.943)
	[13.949]	[16.596]	[19.735]
Consumption per adult equivalent	63.342	9.469	7.356
	(16.401)***	(15.143)	(17.328)
	[23.212]***	[21.432]	[24.526]
Sample Size	1417	1160	914

Notes: Row represent different dependent variable in a regression model. The first column reports the difference-in-difference estimated effects of treatment (i.e. pension) on the dependent variable from a regression that also controls for individual fixed effects and year fixed effects using the data from the T and $EC2$ groups. The second and third columns reports the difference-in-difference estimated effects of treatment on the dependent variable from a regression that also controls for individual fixed effects and year fixed effects using data from the IC and $EC1$ groups. Standard errors clustered at the locality-year level in parenthesis. Standard errors clustered at the locality level in brackets. *, **, *** indicate that the estimates coefficient is significantly statistically different from zero at the 0.10, 0.05 and 0.01 level, respectively.

Appendix A: Generalized Model

In this appendix we present the model that we have used while relaxing the assumption that $1 - \beta i = 0$ and show that the model's predictions still hold. In this case, the solution to the first-order conditions (1) – (3) is:

$$h_1 = \alpha - (1 - \beta i)\frac{1-\alpha}{i(1+\beta)} - \frac{1-\alpha}{i(1+\beta)w}[T_2 + iT_1]$$

$$h_2 = \alpha + (1 - \beta i)\frac{1-\alpha}{1+\beta} - \frac{1-\alpha}{(1+\beta)w}\beta[T_2 + iT_1]$$

$$F = \frac{w(1-\beta i)}{i(1+\beta)} + \frac{T_2 - \beta i T_1}{i(1+\beta)}$$

Then the treatment and anticipation effects are:

$${h_1}^{TT} - {h_1}^{EC} = -\frac{(1-\alpha)(1+i)}{(1+\beta)wi}T \quad \text{(A1)}$$

$${h_1}^{IC} - {h_1}^{EC} = -\frac{(1-\alpha)}{(1+\beta)wi}T \quad \text{(A2)}$$

Again, the treatment (A1) and anticipation (A2) effects are negative, and the anticipation effect is smaller the higher interest rates are.

Appendix B: Tables

Table B1: Definitions of Variables Used in Tables in Text

Individual outcomes	Definition
Geriatric Depression Scale	Definition in measurement section of the text.
Worked last week	Worked last week for at least one hour for pay or uncompensated in a family business
Worked last week for pay	Main job last week was for commission, fixed salary, business owner, self-employed, or member of a cooperative
Worked last week for no pay	Main job last week is family worker with no pay.
Hours worked last week	Hours devoted to main and secondary job last week.
Hours worked last week for pay	Hours devoted to main and secondary job last week.
Hours worked last week for no pay	Sum of hours for last week for pay and nopay
Household outcomes	
Household labor income per adult equivalent	Monthly earnings for all household members per adult equivalent.
Household labor income per adult equivalent excluding the senior	Monthly earnings for all household members 65 years old and younger per adult equivalent.
Household consumption per adult equivalent	Sum of food and non-food expenditures plus value of home produced food
Individual characteristics	
Age	Age in years.
Male	Gender (=1 if male, 0 if female).
Years of Schooling	Years of education approved in school starting with preschools
Married	Individual is currently married or living with a domestic partner
Earnings	Payment for work last week.
Household characteristics	
Household size in adult equivalents	Weight sum of individuals. Weight for children 12 years old or younger is 0.5 and for individuals 13 year olds and older is 1.
Age of household head	Age of the household head.
Male household head	Gender of the household head =1 if male, 0 if female
Household head is literate	Household head can read and write a note
Indigenous household head	Household head can only speak an indigenous language

Appendix Table B2: Baseline Means of Households Included and Excluded in Table 4

	Households with Member Age 66-69			Households with Member Age 70-74		
	Included	Excluded	*P* (Value)	Included	Excluded	*P* (Value)
Treatment locality	0.52	0.48	0.712	0.51	0.46	0.652
Household consumption per adult equivalent	315.20	398.37	0.018	345.15	475.35	0.000
Household income per adult equivalent	145.760	442.56	0.000	165.57	475.88	0.000
Household income excluding the senior	105.75	331.17	0.000	121.15	394.42	0.000
Household size per adult equivalent	5.36	6.322	0.006	4.83	6.12	0.001
Age of household head	65.89	65.280	0.504	69.30	67.63	0.09
Married household head	0.67	0.66	0.848	0.60	0.63	0.49
Male household head	0.73	0.77	0.294	0.66	0.70	0.262
Household head worked last week	0.58	0.61	0.571	0.48	0.52	0.425
Household head is literate	0.41	0.47	0.289	0.43	0.46	0.460
Indigenous household head	0.09	0.06	0.401	0.08	0.08	0.840

Notes: P-values are tests of the null hypothesis of equality of means and account for correlated errors within locality.

Table B3: Impact on Household Income and Consumption Trimming the Top 1% of Values.

	Treatment Effect	Anticipation Effect	Anticipation Effect excluding 69 year olds
Income per adult equivalent	-39.222	-31.947	-25.981
	(16.558)**	(19.787)	(22.553)
	[23.433]*	[28.004]	[31.921]
Income per adult equivalent excluding senior	-25.139	-16.386	-19.534
	(10.944)**	(13.268)	(15.633)
	[15.489]	[18.778]	[22.126]
Consumption per adult equivalent	95.143	13.913	11.141
	(16.797)***	(16.470)	(18.617)
	[23.772]***	[23.310]	[26.350]
Sample Size	1532	1265	998

Notes: Row represent different dependent variable in a regression model. The first column reports the difference-in-difference estimated effects of treatment (i.e. pension) on the dependent variable from a regression that also controls for individual fixed effects and year fixed effects using the data from the T and $EC2$ groups. The second and third columns reports the difference-in-difference estimated effects of treatment on the dependent variable from a regression that also controls for individual fixed effects and year fixed effects using data from the IC and $EC1$ groups. Standard errors clustered at the locality-year level in parenthesis. Standard errors clustered at the locality level in brackets. *, **, *** indicate that the estimates coefficient is significantly statistically different from zero at the 0.10, 0.05 and 0.01 level, respectively.

Appendix Table B4: Impact on Ln Wages of Persons Aged 18-64

	Males			Females		
	(2)	(3)	(4)	(5)	(6)	(7)
Treatment*2008	0.05	-.07	0.1	0.05	0.2	0.16
	(0.06)	(0.14)	(0.07)	(0.11)	(0.28)	(0.16)
	[0.08]	[0.16]	(0.09]	[0.15]	[0.36]	[0.19]
T*08* <Primary School		0.15			-.10	
		(0.15)			(0.29)	
		[0.16]			[0.36]	
T*08*Complete Prim		0.15			-.19	
		(0.15)			(0.29)	
		[0.16]			[0.35]	
T*08*Complete Second		0.17			-.37	
		(0.16)			(0.3)	
		[0.17]			[0.35]	
T* 2008*> Secondary		0.05			0.11	
		(0.19)			(0.46)	
		[0.19]			[0.56]	
T* 2008*Age 30-39			0.03			-.25
			(0.08)			(0.2)
			[0.09]			[0.22]
T* 2008 * Age 40-49			-.12			0.06
			(0.1)			(0.23)
			[0.11]			[0.29]
T * 2008*Age 50-59			-.23			-.13
			(0.12) *			(0.29)
			[0.14] *			[0.36]
T * 2008 * Age 60-64			-.13			-.23
			(0.16)			(0.35)
			[0.16]			[0.36]
Mean of Dep Variable		2.48			2.38	
Sample Size		1640			756	

Notes: Each column presents the treatment effects on log Wages from a difference in difference regression model that also controls for individual fixed effects and year fixed effects. Standard errors clustered at the locality-year level in parenthesis. Standard errors clustered at the locality level in brackets. *, **, *** Indicate that the estimates coefficient is significantly statistically different from zero at the 0.10, 0.05 and 0.01 levels, respectively.

Appendix Table B5: Impact on Ln Wages of Persons Aged 65-88

	Males		Females	
	(1)	(2)	(3)	(4)
Treatment*Year 2008	0.07	0.12	-.06	-.04
	(0.1)	(0.13)	(0.23)	(0.27)
	[0.13]	[0.17]	[0.32]	[0.36]
Treatment* Year 2008*Incomplete primary		-0.06		0.13
		(0.14)		(0.41)
		[0.16]		[0.48]
Treatment * Year 2008*Completed primary		-.40		-.80
		(0.32)		(0.54)
		[0.37]		[0.54]
Treatment * Year 2008*Completed secondary		-.22		
		(1.35)		
		[1.43]		
Mean of the dependent variable	2.28		2.01	
Sample Size	1022		307	

Notes: Each column presents the treatment effects on log Wages from a difference in difference regression model that also controls for individual fixed effects and year fixed effects. Standard errors clustered at the locality-year level in parenthesis. Standard errors clustered at the locality level in brackets. *, **, *** Indicate that the estimates coefficient is significantly statistically different from zero at the 0.10, 0.05 and 0.01 levels, respectively.

Appendix Table B6: Impact on Food Prices

	Index	Tomatoes	Onions	Potatoes	Carrots	Veggies	Oranges
Treatment	-0.30	-1.23*	-0.07	-0.11	-0.28	24.67	5.20
	(0.50)	(0.74)	(0.66)	(0.37)	(0.98)	(15.89)	(5.36)
Observations	924	738	710	658	318	206	262
# of localities	462	369	355	329	159	103	131
Mean	14.28	12.12	10.02	9.283	8.787	13.88	7.082
	Bananas	Apples	Lemons	Nopales	Tortillas	Corn	Rice
Treatment	0.21	-1.10	0.55	-1.96	-0.43	3.63	-0.51
	(0.36)	(1.06)	(0.80)	(6.51)	(0.47)	(5.96)	(0.46)
Observations	542	390	306	200	410	188	634
# of localities	271	195	153	100	205	94	317
Mean	6.757	13.3	8.03	9.882	8.151	5.727	10.74
	Beans	Chicken	Beef	Eggs	Milk	Cheese	Coffee
Treatment	-0.25	-0.74	1.45	2.38	0.31	1.00	-11.25
	(0.51)	(2.29)	(3.55)	(2.38)	(1.38)	(6.08)	(16.69)
Observations	712	568	388	558	480	380	408
# of localities	356	284	194	279	240	190	204
Mean	13.52	29.08	46.45	14.15	10.13	38.37	46.82

Notes: The Price of each food item is calculated as the median of expenditures reported over quantities reported by households. Municipality-year median of a given item is imputed to localities that do not report expenditure in a given item. The price index is a weighted average of items for which at least 500 localities report prices in both years. A locality is said to have reported price if at least one household reported price. Weights are defined as average shares among households of expenditure on a given item over the expenditure on the items included in the index in 2007. Standard errors clustered at the locality level. *, **, *** Indicate that the estimates coefficient is significantly statistically different from zero at the 0.10, 0.05 and 0.01 levels, respectively.