# Effects of Permanent Income Increases on Neighbors: Evidence from an Experiment

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## Abstract

Transitory and permanent income shocks may have varying effects on recipients and their neighbors. Among these effects are risk-sharing, in which individuals use their increase in income to provide loans and monetary transfers to neighbors as a way of insuring against future setbacks, and altruism, in which recipients of greater income use it to increase the consumption of others. Neighbors may also try to imitate consumption patterns of recipients or may feel unhappy about changes on recipient's income. To analyze these, and particularly how they may be affected by a permanent income shock, we analyze the results of a randomized control trial in an urban area of Mexico, where pension benefits were randomly assigned to some older adults and not to others. We exploit a double randomization design with differential proportions of treated households within randomly selected city blocks (treated blocks) to understand the presence of spill-over effects to non-recipients in the treated blocks, by comparing them with non-recipients in control blocks. We also estimate spillover effects by modeling spillovers as a function of distance between recipients and non-recipients. We use rich data collected before the pension as well as 14 and 26 months after its introduction. We found that non-recipients in treatment blocks report increased food availability and health care utilization as well as improved self-reported health. This evidence is consistent with benefit recipients being altruistic and sharing their resources.

*JEL Classification:* C21, C93, I38

*Keywords:* Permanent Income Shocks, Spill-over Effects, Randomized Controlled Trials

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## Introduction

The Permanent Income Hypothesis (PIH) suggests that permanent, rather than temporary, increases in income increase household consumption (Friedman 1957). This is because individuals seek to maximize their expected lifetime utility. This hypothesis fails to recognize that consumption may be determined by the consumption of those around you. Veblen (1953) states that expenditure is driven by imitation and emulation of one's community. Indeed, he coined the term "conspicuous consumption" to refer to the tendency of individuals to imitate consumption patterns of individuals with higher economic status or to demonstrate one's own higher economic status. This underlies the Relative Income Hypothesis, which states that consumption is determined not only by one's own income but also by one's income relative to that of others (Duesenberry 1949), resulting in a "demonstration effect."

Life satisfaction may also depend not only on one's own income but also on one's income relative to that of others. When one 's neighbors experience an increase in income, one's own happiness may change (Easterlin 1974). Conversely, an increase in one's own permanent income could cause unhappiness among neighbors and friends and changes in their consumption patterns For example, neighbors of lottery winners increase their consumption of durable goods, specifically cars (Kuhn et al. 2011): competitive or imitative behavior that can be explained as a reaction to an exogenous change to a neighbor's income and consumption, or, as it is popularly called, "keeping up with the Joneses."

Increased income for an individual may also increase consumption among others by inducing the sharing of resources with family and friends in other households. There are at least two motives an individual may have for sharing resources with others: risk sharing and altruism. A risk-sharing model implies that, in the absence of well-developed credit and insurance markets and in the presence of considerable income volatility, households may transfer income to each other as a way to smooth consumption (e.g. Rosenzweig 1988; Fafchamps 2008). For example, a randomized control trial (RCT) of a Mexican cash-transfer program for families with young children found that households not receiving the program increased their consumption as well as loans and money transfers received (Angelucci and De Giorgi 2009), all activities consistent with risk-sharing motives of recipient households. Whether households receiving a permanent increase in income would engage in similar risk-sharing behaviors is unclear. Altruism can induce individuals to reduce their own consumption to increase that consumption of others (Becker 1976). An increase in income would thus induce spillovers to improve neighbor's wellbeing.

In this study, we analyze the effects of an increase in permanent income of older adults on the behavior, satisfaction, and wellbeing of their neighbors. We exploit rich RCT data for a non-contributory pension program in Merida, Yucatan, Mexico. We analyze spillover effects from treated (i.e. recipients) to untreated (i.e. non-recipients) individuals. The pension program we examine is typical among those designed to support older persons in many countries that are experiencing demographic transitions characterized by aging populations (United Nations, 2015).

Spillover effects may include externalities, general equilibrium effects, peer effects, and social effects. Externalities can result when treatment of one group reduces the chances another group will be affected by an external event. An example is how giving deworming drugs to some children also reduces the chance that other untreated children are infected (Miguel and Kremer 2004). General equilibrium effects are those that affect the local economy, such as through price adjustments. Peer effects include resource sharing or cost sharing due to social norms, networks, and altruistic preferences (e.g., Angelucci and De Giorgi 2009). Social effects induce changes in neighbors' happiness or competitive behavior (e.g., Kuhn et al. 2011).

Previous literature has analyzed **transitory** income shocks such as that occurring among lottery winners and their neighbors (Kuhn et al. 2011) or through conditional government cash transfer programs for households whose young children become too old for eligibility (Angelucci and De Giorgi 2009). This is the first study to use an RCT to analyze the effects of an increase in **permanent** income on recipients' neighbors. The RCT that we analyze randomly assigns city blocks where some or all eligible households get treated (i.e. receive a pension) and city blocks where no one gets treated.

To analyze the effects of a noncontributory pension, received until death of the beneficiaries, on a recipient's older adult neighbors, we compare outcomes of non-recipients in treatment blocks and in control blocks. We exploit variations in the intensity of treatment by block, i.e. the varying proportions of households treated within a block, to identify spillover effects. We test a conspicuous consumption hypothesis by using information on expenditures for durable goods and home improvements as well as information on receipt of alternative public or private transfer programs. We test the satisfaction hypothesis that changes in one's income will affect the happiness of neighbors by using data on depression and satisfaction with household income. We test whether recipients may have risk-sharing motives for sharing resources by analyzing monetary transfers from relatives or friends and debts and loans among non-recipients. We test altruistic motives for recipients by examining self-reported health, health care utilization, food availability, and satisfaction with social contacts and friends for non-recipients. Finally, because the noncontributory pension program could induce an increase in prices for certain goods, and thereby reduce the real consumption benefit of the pension, we check price changes for possible general equilibrium effects on the local economy.

Altogether, we find non-recipients in treatment blocks report a greater increase in food availability, health care utilization, self-reported health, and satisfaction with social contacts and friends than do non-recipients in control blocks. We also find non-recipients in treatment blocks report greater decreases in the proportion receiving emergency in-kind food transfers from government, church, and private organizations than non-recipients elsewhere. The results show a modest decline in the proportion of non-recipients reporting loans or debts. The analysis of price changes revealed no evidence of general equilibrium effects.

We do not find evidence of effects on conspicuous consumption or durable goods, home improvements, or receipt of alternative public or private transfer programs. We also do not find risk sharing to be a motive for sharing resources. Our findings are consistent with altruistic behavior from recipients towards non-recipients in the short- (14 months) and medium-term (26 months) after introduction of the noncontributory pension program.

Our findings suggest that that individuals are more likely to behave altruistically when their income increase is permanent, with risk-sharing motives being more common if the increase is temporary. Our lack of findings on imitative or competitive behavior or decreasing happiness may be a result of recipients are sharing their pension resources with non-recipients.

The rest of the paper is organized as follows. Section I describes the design of the RCT and the data. Section II describes our empirical approach and outcome variables. Section III presents the results of the spillover analysis and general equilibrium effects as well as robustness tests. Section IV concludes.

## I. Experimental Design and Data

According to the 2010 Mexican Census, Merida had a population of 777,615, with 36,782 adults 70 years or older. The large population of older adults in Merida prompted the decision to roll out the non-contributory pension program incrementally. This, in turn, allowed us to randomize which households would initially receive pension benefits.

In 2009, the city of Merida had 8,870 blocks. We selected 1,175 (13.2% of all blocks) of these randomly with inclusion probabilities proportional to the number of elderly within a block. We employed a double randomization design. Of 1,175 blocks chosen, we assigned randomly three-fourths to treatment and one-fourth to control. Once we selected the sample of blocks, the field team conducted a household census of the selected blocks using mapping and cartography methods specified by the Instituto Nacional de Estadística y Geografía (INEGI, the Mexican National Institute of Statistics and Geography) to identify households with eligible individuals. We used the same format and methods for this census as we used in an earlier experiment (see Aguila et al. 2014). Screening of each block began at its northwest corner, whose location was documented with the Global Positioning System (GPS). Within treatment blocks only a proportion of eligible households were chosen to be treated while the rest remained untreated.

Treatment blocks were assigned to two treatments that were phased in with the same monthly pension amount ($550 pesos or US$69.2 at 2014 PPP) per eligible adult (i.e. 70 years or older). The $550 pesos monthly pension amount is equivalent to 28% of the minimum wage and on average about 25% of their household income. In the first treatment, which started in December 2009, the pension was disbursed in cash. In the second treatment, which started in December 2010, the pension was paid into bank accounts that beneficiaries could access with a debit card. Beneficiaries receiving their pensions through a debit card could withdraw the money at ATMs or banks or purchase goods at some stores. Altogether, 450 blocks were assigned to the Cash intervention (to be called Merida 1 or M1), and 515 blocks to the Debit Card intervention (Merida 2 or M2). In control blocks, all households remained untreated. The probability of a household being treated within a treatment block was 50% in the Cash intervention experiment, but the actual percentage treated could vary randomly across blocks. In the debit card experiment, we set probabilities of treatment within a block at 75%, 50%, or 25%, so as to improve the efficiency of the estimates—although, again, the proportion within a block actually receiving the pension could vary from these numbers.

Figure 1 shows the percentage of blocks for different proportions of treated households within block for the Cash and Debit Card treatment blocks. It shows, for example, that more than 60 percent of the blocks selected to have at least some residents receive the pension had between 41 and 50 percent actually receiving it. In the Cash experiment the proportion of households actually receiving the pension cluster around 50%, with less variability than there is in the proportion of block households actually receiving the pension in the Debit Card experiment. The distribution of percentages of treated households in the Debit Card experiment is much closer to a uniform distribution, with more variability across blocks, allowing us to identify potential spillover effects.

[FIGURE 1]

We collected wave 1 (W1) data for the Cash experiment between September and November 2009. We conducted a follow-up survey (wave 2 or W2) of the Cash experiment between February and April 2011 and a second follow-up survey (wave 3 or W3) between March and May 2012. The take-up rate of the Cash program was 98.4%. We conducted a baseline survey (wave 1 or W1) of the Debit Card experiment in August and October 2010. We re-interviewed Debit Card experiment households between June and July 2011 (wave 2 or W2) and conducted a second follow-up survey between March and May 2012 (wave 3 or W3). The take-up rate of the Debit Card program was 96.4%. W3 was collected simultaneously in the Cash and Debit Card experiments, when we also collected data for control blocks (Merida 3 or M3). We did not collect data before W3 for control blocks in order to avoid any potential survey effects and because of time and resource constraints. Figure 2 shows the map of Merida with treatment blocks in the Cash and Debit Card experiments and control blocks.

[FIGURE 2]

For the Cash experiment, the response rates at W1 were 70.1% for treated and 71.5% for untreated households; in W2, they were 78.3% and 61.7%; and in W3 they were 85.6% and 81.3%. For the Debit Card experiment, response rates at W1 were 93.6% for treated and 94.3% for untreated households; in W2, response rates were 89.9% and 74.4%; and in W3, response rates were 80.9% and 77.9% (Aguila et. al 2015). For the control blocks the response rate was 91.0%.[1]

Our survey items were taken or adapted from other longitudinal studies including the Mexican Health and Aging Study, the U.S. Health and Retirement Study, the New Immigrant Survey in the United States, Oportunidades in Mexico, and various family life surveys. Our surveys collected detailed community, household, and individual-level data. In both baseline and follow-up surveys, we collected data on, among other topics, individual and household health, food security and availability, health care utilization and out-of-pocket expenditures, financial and in-kind transfers, and economic activity of older workers (see Aguila et al. 2014).

All study materials and informed consent documents were provided in respondents' language of choice (i.e., Spanish, Mayan, or both). More than half of participants had limited literacy and spoke the native indigenous language (i.e., Mayan), with only limited fluency in the national language (i.e., Spanish). We paid particular attention to developing a culturally appropriate data collection process and instruments that conformed to norms and regulations for conducting research involving human subjects in both the United States and Mexico. Incentives were provided to acknowledge respondents' participation. The institutional review board of RAND Corporation and the State of Yucatan approved the study procedures (Protocol approval number 2008-0513-CR07). Complete descriptions of our protocols is available elsewhere (Aguila et al. 2014, 2015a).

We deflated monetary variables with the Mexican National Consumer Price Index base for the last two weeks of December 2010 (INEGI, 2015). We adjusted expenditure variables with equivalence scales, accounting for household size by dividing the expenditure variable by the square root of the number of household members (Organisation for Economic Co-operation and Development, n.d.).

Interviewers recorded latitude and longitude coordinates with Global Positioning System (GPS) devices. We estimated ellipsoidal distances using Vincenty's (1975) formulae for the shortest distance between two points. We estimated walking distances using ArcGIS. Table 1

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[1] The response rate reported is AAPOR RR2. This is defined as the number of complete interviews (including proxy interviews) divided by the sum of the number of interviews (complete plus partial), the number of non-interviews (refusal and break-off plus non-contacts plus others), and the number of cases of unknown eligibility. This follows the guidelines of the American Association for Public Opinion Research (2011) for calculating non-response rates.

shows characteristics are balanced for age-eligible individuals not receiving the pension in treatment and non-treatment blocks for the cash and debit-card experiments.

[TABLE 1]

We surveyed of prices of 12 basic products in our survey area. These were: tortilla, French bread, sweet bread, beans, rice, eggs, milk, tomato, potato, onion, soft drinks, and noodle soup. We also collected prices of 5 basic products used at home. These were: cloth detergent, chlorine, bar soap, toilet paper, and tooth paste. We conducted our price surveys in November 2010 and June 2012 in different types of establishments (corner shops, grocery shops, convenience store, supermarkets, market, and farmers' markets) selling food staples. We conducted a census to identify the establishments selling food staples eight blocks around the selected treatment and control blocks. Many blocks do not have stores or other vendors. Therefore, we also collected price data in adjacent blocks, even if they were not part of the experiment. We surveyed 1,426 establishments in 2010 and 945 in 2012. Among the 945 establishments we surveyed in 2012, we had surveyed 805 in 2010. Among these, 140 establishments had changed their name in 2012 and were assigned a new identifier. Of the 1,426 establishments, we interviewed in 2010, 481 had closed by 2012; 77% of those closing were corner and grocery shops. Figure 3 shows the location of the establishments interviewed.

[FIGURE 3]

## II. Empirical Approach

As described above, the RCT randomized households of age-eligible individuals within randomly selected blocks. Our data includes information on non-recipients in control blocks and in treatment blocks with varying proportions of treated individuals. This design allows us to identify the effects of an unexpected increase in permanent income of older adults (treated) on their neighbors (untreated).

Mathematically, we let $Y_{ij}(A)$ be the potential outcome for untreated individual $i$ in block $j$ when the fraction treated in the block is $A$. Hence, the average effect of the pension on the untreated in treatment blocks with a fraction $A$ is

$$UTE(A) = E[Y_{ij}(A) - Y_{ij}(0)].$$

If $UTE(A) \neq 0$ for any value of $A$, this implies a spillover effect. We observe the outcome only under one level of treatment, in this case, under only one level of the fraction treated in the block. Hence, we cannot compute $UTE(A)$ directly. But under random assignment of treatment fractions to blocks as well as random assignment of treatment status to individuals within blocks (conditional on the assigned fraction treated), we can calculate an unbiased estimator of $E[Y_{ij}(A)]$. This is the sample average of the outcome for all untreated individuals in treatment blocks with a fraction treated of $A$, or, mathematically

$$\bar{Y}(A) = \frac{1}{N(A)} \sum_{i,j:\, A_{ij}=A} Y_{ij},$$

where $A_{ij}$ is the fraction treated in block $j$ in which untreated individual $i$ lives and $N(A)$ is the number of untreated individuals in blocks with treatment fraction $A$. Then, we can estimate the average effect for untreated individuals as

$$\widehat{UTE}(A) = \bar{Y}(A) - \bar{Y}(0).$$

In order to interpret this as the total spillover effect when the fraction treated is $A$, we need to assume that there are no spillovers to untreated in control blocks. That is, we need to assume that $\bar{Y}(0)$, the average outcome in control blocks, is the same as it would be if there were no program at all. This assumption is violated when untreated individuals in control blocks are affected by the program, as when spillovers cross block boundaries. In this case, the sign of the spillover effect would most likely be the same for untreated in control blocks and in treatment blocks with a positive fraction treated, with $\widehat{UTE}(A)$ underestimating the magnitude of the spillover effect.

Under the stated assumption, we can define the average spillover effect of the program (in treatment blocks that have treated individuals) as

$$UTE = E\left[UTE\left(A_{ij}\right) \mid A_{ij} > 0\right] = E\left[Y_{ij}\left(A_{ij}\right)\right] - E\left[Y_{ij}(0)\right]$$

The first term can be estimated by

$$\bar{Y}^T = \frac{1}{N^T} \sum_{i,j:\, A_{ij}>0} Y_{ij},$$

where $N^T$ is the total number of untreated individuals in treatment blocks with positive treatment fractions. The estimator of $UTE$ then follows as

$$\widehat{UTE} = \bar{Y}^T - \bar{Y}(0).$$

We compare the differences of the means between untreated individuals in treatment and control blocks. We also exploit the intensity of treatment within block. We use data collected in 2012 from Merida 1 (cash) and Merida 2 (debit card) wave 3, as well as from the additional sample of untreated individuals in control blocks from Merida 3. Our basic model is:

$$Y_{ij} = \alpha + \beta_1 A_{ij} + \beta_2 X_{ij} + \varepsilon_{ij}$$

$Y_{ij}$ is the outcome of interest for individual $i$ in block $j$. We estimated $A_{ij}$ with different specifications: 1) dummy (treatment blocks=1, control blocks=0), 2) proportion of eligible households treated within block (number of households treated divided by the total number of age eligible households), and 3) proportion of potential amount of money received within block (amount of money received divided by the total amount all age eligible would have received). In the second and third definitions $0 \le A_{ij} \le 1$. The reason for the third specification is that 22% of households have at least two individuals eligible for the non-contributory pension, so that the total amount of money potentially received within a block will vary with the composition of households in that block. $X_{ij}$ includes sociodemographic characteristics uncorrelated with the treatment. These are: age, age squared, gender (male=1, female=0), couple status (yes=1, no=0), and number of years of schooling. Finally, $\varepsilon_{ij}$ is the error term. We estimate clustered standard errors at the block level, because there may be unobserved block effects (e.g., Angrist and Pischke 2009). Given that we test multiple hypotheses, we apply a Holm-Bonferroni correction (Holm 1979).

### II.1 Outcome variables

We analyze satisfaction with household income and depression (measured with the Composite International Diagnostic Interview – Short Form CIDI-SF – test that evaluates major depressive episodes), purchase of durables (cellphones, vehicle, bicycles, motorcycles, cell phones, and chickens), home improvements (dwelling type of floor), public or private transfers from other programs, monetary transfers, loans and debts, self-reported health, health care utilization, and food availability.

The different hypotheses sketched in the introduction have different observable implications (see Table 2). If we observe for non-recipients increases in the purchase of durable goods, home improvements, entrepreneurship, and receipt of public or private transfers from other government programs, we may infer the pension program led non-recipients to exhibit competitive or imitative behavior. If we observe for non-recipients decreases in satisfaction with income and an increase in depression, we may infer that an increase in permanent income for recipients also increased unhappiness in friends and neighbors. In contrast, if we observe among recipients an increase in monetary transfers, and debts and loans, we may infer pension recipients are sharing resources as an insurance mechanism.

[TABLE 2]

If we observe for non-recipients an increase in self-reported health, health care utilization, food availability, and satisfaction with social contacts and friends, we may infer that pension recipients shared their resources out of altruism. Finally, we test for general equilibrium effects by assessing changes of prices for basic products consumed by older adults. We also constructed a price index.

### II.2 Heterogeneous sharing (***work-in-progress***)

We have loosely used the term *block* to describe the unit within which spillovers (and general equilibrium effects) may occur. Yet spillovers may also occur over larger areas. Therefore, we extend our analysis to allow for spillovers within a certain radius $\Delta$, the straight-line distance or walking distance between households. We focus on straight-line and walking distances because adults 70 years or older are less likely to drive. In this case $A_{ij}$ is the proportion of treated individuals or households within a certain radius. We computed different radiuses and present the proportion of treated individuals or households within 150, 300, 500 or 800 meters. An average block in Merida is about 125 to 160 meters, approximately 500 feet.

We assume spillover effects would be stronger among non-recipients who live closest to recipients. Similarly, we assume that spillover effects will decrease at longer distances. With a geographically defined model like ours, we should correct for spatial correlation, standard errors are corrected based on a spatial autocorrelation matrix (e.g., Anselin 2001). A limitation of this approach is that data were not collected for untreated individuals in blocks that were not randomly selected in the RCT. This introduces measurement error to our analysis. Appendix A provides an analysis of the statistical implications of this.

## III. Results

Table 3 shows the results for our model with $A_{ij}$ defined as the ratio of the total amount of money received within a block divided by the total amount that could have been disbursed if everyone in the block had been treated. The results with the other two specifications of $A_{ij}$ (either a zero-one dummy or the number of treated households divided by the total number of age eligible households in the block) are very similar. We present the results for the untreated in treatment blocks of M1, M2, and for the combined sample that pools M1 and M2 in comparison to untreated in control blocks of M3. The first column shows the estimated coefficient, the second column the standard error, while the third column indicates statistical significance using the Holm-Bonferroni correction for multiple hypothesis testing. Each specification controls for age and age squared, gender, whether the respondent is single or married, and number of years of schooling.

We find that untreated individuals in treatment blocks of M1 report a decrease in the amount of food received from government programs, church, or private organizations that target low-income individuals. M1 and M2 untreated individuals in treatment blocks also reported an increase in household food availability relative to non-recipients in control blocks. That is, non-recipients in blocks where others were receiving pensions reported decreases in being hungry, running out food, cutting meals, and not eating at least one day due to lack of money. In M1 and M2 we observe an increase in health care utilization, and an improvement in self-reported health.

The non-recipients in treatment blocks in M1 and M2 were more satisfied with their social contacts and friends than were non-recipients in control blocks. Finally, in M1, there was a decline in loans or debts among non-recipients in treatment blocks.

[TABLE 3]

We did not include in Table 3 entrepreneurship, business revenues, and income from property because a very small proportion of older adults report them. We do not find evidence that an increase in permanent income of one's neighbors caused unhappiness or *conspicuous consumption*. Rather, we found neighbors of recipients were more satisfied with their friends, and did not have greater depression. We also do not observe any changes in the ownership of durable goods or effects on home improvements. Less than 1% of the untreated were beneficiaries of other government programs, indicating a lack of enrollment in such programs due to competitive behavior. We did not find evidence of risk-sharing behaviors by recipients: non-recipients in treatment blocks did not report decreases in their loans or changes in monetary transfers from relatives or friends. This is not surprising because we analyze a permanent change in income in a low-income urban setting but with more access to formal credit and insurance than present in the low-income rural settings seeing transitory changes in previous studies (e.g. Rosenzweig 1988; Angelucci and De Giorgi 2009). Our results, particularly those on greater food availability and health care use coupled with a decrease in food aid received by those not receiving the pension, are consistent with altruistic behavior by pension recipients.

We presented the results for M1 and M2 separately because there could be different spillovers for benefits disbursed by cash rather than with a debit card. Getting a monthly cash payment may lead to less consumption smoothing than with a debit card, and hence also to differing sharing of resources. Nevertheless, we found no statistically significant differences between M1 and M2 in our variables.

**III.2 General equilibrium effects**

Increased total income in treatment blocks may lead to higher equilibrium prices, thereby dampening the effect of the pension. To study this potential effect more directly, we collected data on prices of commonly purchased products. As noted, we collected wave 1 price data in November 2010, after individuals in the Merida 1 experiment had already started receiving their pensions, but just before individuals in the Merida 2 cohort started receiving their pensions. We collected wave 2 of price data in June 2012. Because not all blocks have stores, and not all stores sell all products, we constructed a measure of the prices an average household in a block faces as follows.

Interviewers used GPS to record the latitude and longitude of each household in our dataset as well as for each establishment in which prices were recorded. There were many differences in store availability and location between waves 1 and 2 of price data. Hence, we treat wave 1 stores and wave 2 stores separately. Our micro dataset contains only households that lived at the same location in all observed waves. For each household, wave, and product, we find the closest store for which we observe a product price. We use this price for what the household faces for the product in the given wave. We use prices that are deflated by the Mexico's national consumer price index (December 2010 = 100), that is, we divide raw prices by 99.74209 for wave 1 and by 107.246 for wave 2 to obtain the deflated prices. For each block, wave, and product, we average prices across households to obtain an average price at the block level.

Table 4 presents our results. Column 2 of this table shows the average deflated prices of the products in wave 1. We are interested in the extent to which the treatment affects the prices that an average household in a block faces. Hence, we study the changes in these average prices between wave 1 and wave 2. Column 3 of Table 4 shows the averages of these differences. We use this variable as the dependent variable in a linear regression analysis. The explanatory variables are dummy variables for Merida 1 and Merida 2 (with the constant omitted) the products of these dummy variables with the fraction of households on each block receiving the pension. Although we estimate this regression on the pooled data set of $N$ = 867 blocks, the estimates would be the same for separate regressions of the Merida 1 and 2 cohorts, with a constant and the treatment fraction as explanatory variables. The last four columns of Table 4 show the results.

[TABLE 4]

Of the 19 products in Table 4, 2 show a significant treatment effect (at the 5% level) for each cohort; chlorine and toothpaste for Merida 1 and tomatoes and chlorine for Merida 2. This is slightly higher than we would expected by chance alone. The signs, however, are not consistently positive, as we would expect for general equilibrium effects of the pension. Moreover, the standard errors used here assume uncorrelated errors. The outcomes of blocks that are close to each other are likely to be correlated than results for blocks further away will be, because some individuals will shop at the same store. The exact pattern of the correlations is difficult to determine, and the correlation matrix will not have a simple structure. The effect of ignoring this kind of correlation, as with more common forms of clustering, is a downward bias of the standard errors. Altogether, we conclude that there is no evidence of meaningful effects of the pension program on prices, and thus no evidence for general equilibrium effects.

### III.3 Sensitivity Analysis

We designed the rollout schedule of the program and the timing of public information campaigns in close cooperation with government to avoid announcement effects. We analyzed attrition and mortality bias but found it to be similar among individuals receiving and not receiving the pension. We also do not find any changes in labor supply among those not receiving the pension that might cause these effects.

## IV. Conclusions

In recent decades, many countries have experienced growing aging populations. Many countries have introduced non-contributory pension programs or income supplemental programs to provide income security in old age. In this study, we exploit the introduction of two RCT's of a non-contributory pension program, one disbursing benefits in cash and the other one by debit card.

We find an increase in neighbor's food availability, self-reported health, and health care utilization, consistent with *altruistic* behavior among pension recipients. Consistent with the increase in food availability, we found a decrease in the amount of food received by non-recipients from government, church, or private organizations for low-income households. The increase in health care utilization may be a mediator for the increase in self-reported health.

Our findings add to the literature on permanent income increases on neighbor's behavior. Angelucci and De Giorgi (2009) analyze a conditional government cash transfer program (*Oportunidades* in Mexico) where households lose eligibility or the subsidy declines when children are no longer of schooling age. Using data from an RCT, they find that untreated households increase their consumption, money transfers received, and loans. They suggest that their results are consistent with the risk-sharing model. Their findings show a behavior consistent with an interpretation of the cash transfer as a *transitory* income shock providing recipients with incentives to insure themselves for “rainy days.” Our findings suggest that *permanent* income shocks may bring out a pure altruistic behavior.

Our analysis could be extended in at least two ways. We find some evidence of spillover effects within blocks, but the pattern of communication of households is unlikely to be confined to a specific block. An obvious extension therefore is to consider distances between households as a measure of interaction and to parameterize spillover effects as a function of such an interaction measure. A second extension is to model heterogeneity of effects. Pre-treatment income differences between households may affect spillover effects of the pension program. In particular, one would expect households not receiving the pension but with higher income to be less likely than other non-recipients to receive transfers from households that do receive the pension. In an earlier paper (Aguila, Kapteyn, and Smith 2015), we analyzed the effects of the same non-contributory pension experiment by comparing two towns, one of which received the pension and one which did not. Plausibly, in that case, all spillovers are contained within the towns. Extending the results in the earlier paper will help us understand the treatment effects and will provide yet another way of gauging the importance of spillovers.

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## Appendix A: Implications of Incomplete Observations on Non-recipients

For simplicity, we drop the subscripts $i$ and $j$ for the moment and first look at the model without controls. Let $S$ be the number of age-eligible individuals that are observed within a distance $\Delta$ of the target individual and let $N$ be the number of such individuals who are not observed. Because we observe all treated individuals, none of the $N$ individuals who are not observed are treated. Let $P$ be the fraction of the $S$ observed individuals within a distance $\Delta$ that are treated, i.e. receive the pension. It follows that the number of individuals within a radius of $\Delta$ that receive the pension is $P \times S$ and the total number of individuals within a radius of $\Delta$ is $S + N$. Consequently,

$$A = \frac{P \times S}{S + N} = P \left( \frac{S}{S + N} \right) = P \times r$$

and

$$Y = \alpha + \beta_1 r P + \varepsilon.$$

Let

$$\rho = E(r) = E \left( \frac{S}{S + N} \right).$$

Because both $S$ and $N$ are nonnegative, $0 \leq \rho \leq 1$. We can now write

$$Y = \alpha + \gamma P + u,$$

where $\gamma = \beta_1 \rho$ and $u = \varepsilon + P(r - \rho)$. Because of the randomization,

$$E(r - \rho \mid P) = E(r - \rho) = 0$$

and thus $E(u \mid P) = 0$. Hence, an OLS regression of $Y$ on $P$ estimates $\gamma$ consistently. We saw that $0 \leq \rho \leq 1$, which implies that $0 \leq \gamma \leq \beta_1$ if $\beta_1 \geq 0$. Otherwise, the signs are reversed. That is, $\gamma$ has the same sign as $\beta_1$ but has a smaller magnitude. In conclusion, just as with classical measurement error, the OLS estimator is biased toward zero and true effect sizes tend to be larger than estimated effect sizes. When controls $X$ are added, the conclusion remains the same, because they are independent of $P$ because of the randomization.

In order to measure the size of the measurement error, we use data from the 2005 and 2010 censuses on the number of older adults and households with older adults for all the blocks in Merida. We link the Census information at the block level to our data set and estimate $S$ and $N$. We found that $S$ in our data and the Census data is similar. This confirms that we are accurately capturing the total number of age eligible individuals per block and that we can correct for most of the measurement error in the distances analysis.

**FIGURE 1-- Frequency of Proportion of Treatment by Block**

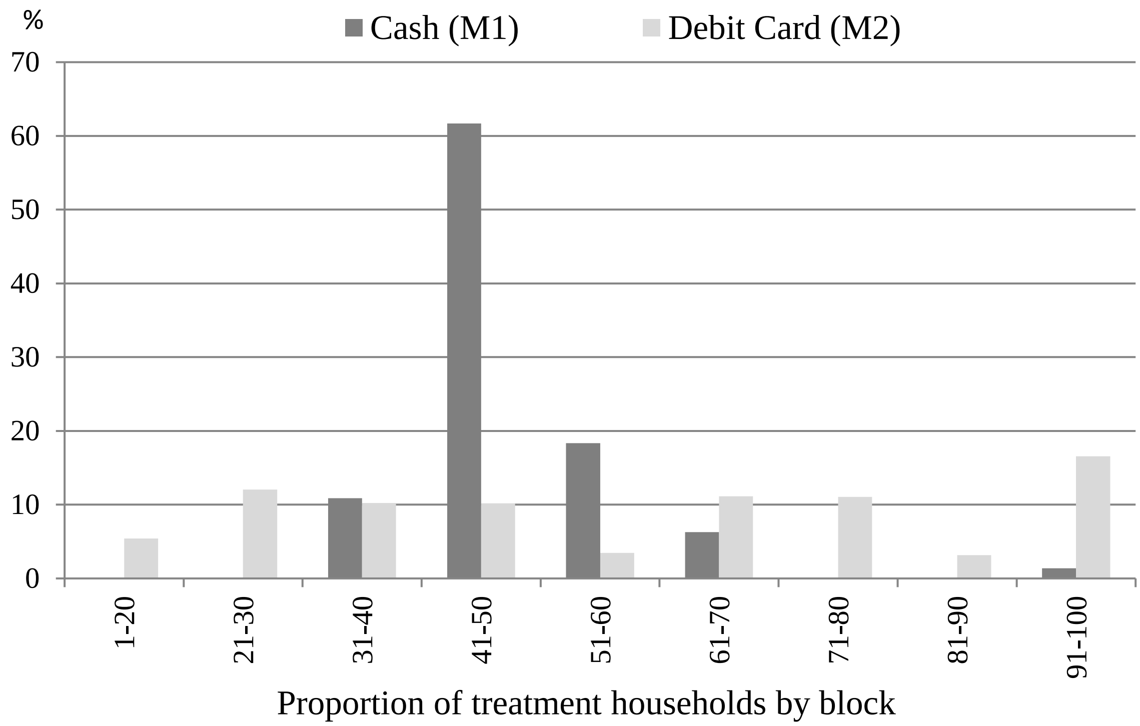

**Figure 2. Maps of Treatment and Control Households, Merida 1 and Merida 2**

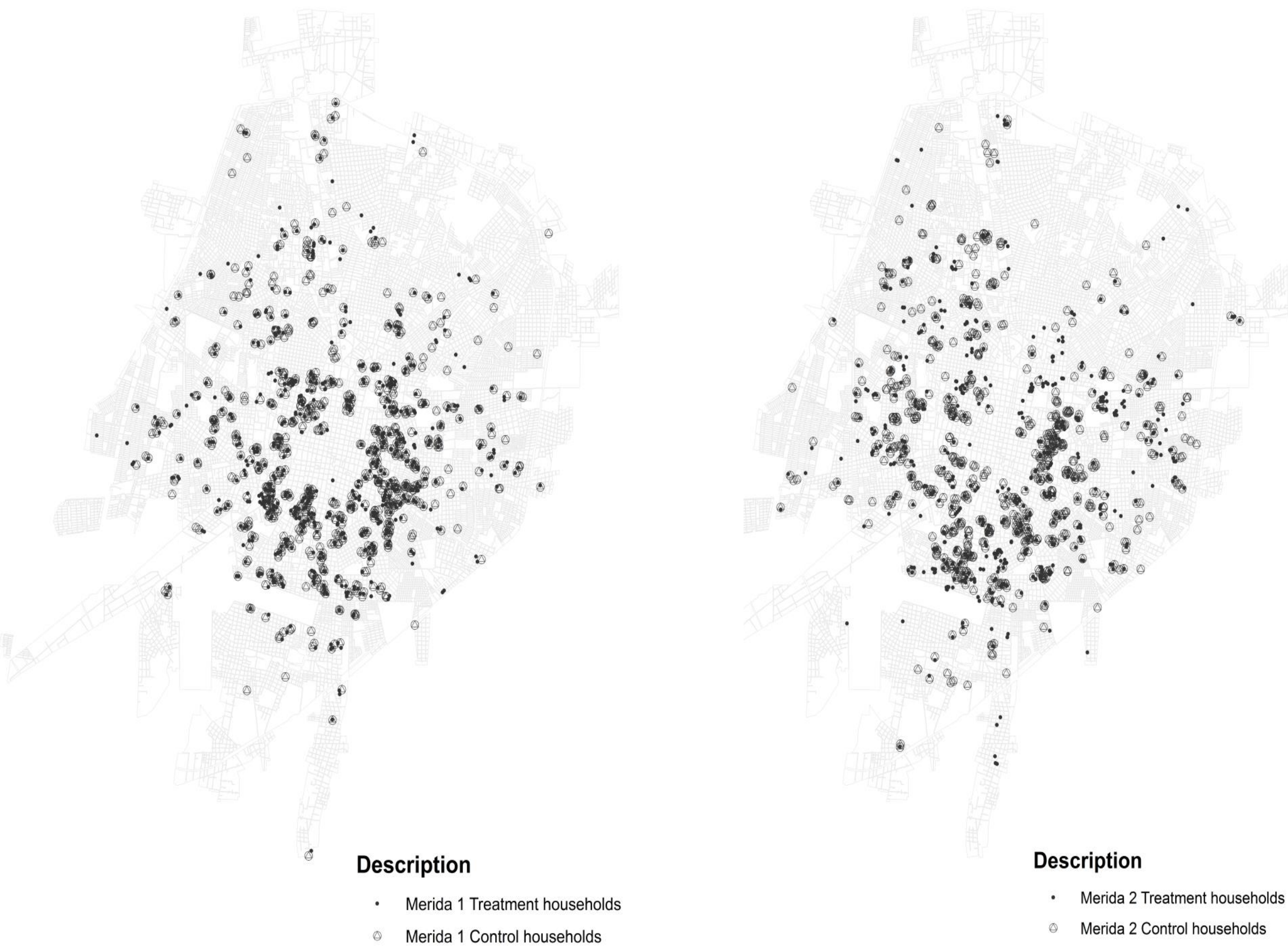

**Figure 3. Map of Establishments Interviewed for Prices Surveys, Merida**

**TABLE 1— Descriptive Statistics Baseline**

| | Treatment Blocks | | Control Blocks | | | |
|---|---|---|---|---|---|---|
| | **Cash (M1)** | **Debit Card (M2)** | **(M3)** | | | |
| | Untreated | Untreated | Untreated | p-value (M1-M2) | p-value (M1-M3) | p-value (M2-M3) |
| Age | 77.88 | 77.41 | 77.602 | 0.13 | 0.43 | 0.58 |
| | (6.488) | (6.467) | (6.231) | | | |
| Male (%) | 37.20 | 38.77 | 38.02 | 0.50 | 0.76 | 0.78 |
| Years of schooling | 4.13 | 4.09 | 4.27 | 0.84 | 0.45 | 0.36 |
| | (3.324) | (3.333) | (3.368) | | | |
| No. Observations | 829 | 796 | 605 | | | |

**TABLE 2—Hypotheses and Outcomes**

| If (…) for untreated in treatment blocks | Hypothesized behavior |
| --- | --- |
| • Increase in **durables** (e.g. cellphones, bicycles, and chickens)<br>• **Home improvements** (dwelling floor)<br>• Increase in **public or private transfers** from other programs | Neighbors' *competitive* or *imitative* behavior |
| • Decline in **satisfaction with family household income**<br>• Increase in **depression** | Neighbors' *unhappiness* due to unfavorable comparison |
| • Increase in **monetary transfers** from relatives or friends<br>• Increase in **Debts** and **loans** | Recipients' may share resources as an *insurance mechanism* |
| • Increase in **self-reported health** and **health care utilization**<br>• Increase in **food availability**<br>• Increase in **satisfaction with social contacts and friends** | Recipients' may share resources out of *altruism* |

**TABLE 3—DID Regressions**

| Variables | Cash (M1) | | | Debit Card (M2) | | | Cash and Debit Card | | |
|---|---|---|---|---|---|---|---|---|---|
| | 26 months after | | | 14 months after | | | (M1 and M2) | | |
| | **b** | **(SE)** | **HB** | **b** | **(SE)** | **HB** | **b** | **(SE)** | **HB** |
| **Transfers** | | | | | | | | | |
| Transfers to the older adult (index) | -0.042 | (0.149) | | -0.067 | (0.160) | | -0.070 | (0.117) | |
| Transfers from the older adult (index) | -0.156 * | (0.091) | | 0.043 | (0.232) | | -0.117 | (0.093) | |
| Receiving food from government, church or private (index) | -0.270 *** | (0.103) | †† | 0.091 | (0.194) | | -0.092 | (0.103) | |
| **Loans and Debts** | | | | | | | | | |
| Credit card, other debts or loans, % | -3.600 ** | (1.510) | †† | -1.160 | (2.150) | | -2.030 | (1.320) | |
| **Durables, %** | | | | | | | | | |
| Telephone | 3.580 | (7.040) | | -0.079 | (8.680) | | 3.890 | (5.950) | |
| Cellphone | -6.970 | (7.700) | | -10.300 | (8.040) | | -9.570 | (6.180) | |
| Bicycle | -1.410 | (5.430) | | -7.240 | (5.780) | | -1.260 | (4.390) | |
| Chickens | 0.287 | (4.110) | | -0.338 | (4.370) | | 1.420 | (3.220) | |
| **Dwelling floor type, %** | | | | | | | | | |
| Cement | -2.550 | (7.530) | | 1.630 | (9.100) | | -4.290 | (6.340) | |
| Tile | 0.029 | (7.550) | | -1.650 | (9.080) | | 2.510 | (6.350) | |

TABLE 3—DID Regressions (*continued*)

| Variables | Cash (M1) 26 months after | | | Debit Card (M2) 14 months after | | | Cash and Debit Card (M1 and M2) | | |
|---|---|---|---|---|---|---|---|---|---|
| | b | (SE) | HB | b | (SE) | HB | b | (SE) | HB |
| **Food Availability** | | | | | | | | | |
| Food Availability (index) | 0.669 *** | (0.113) | †† | 0.430 *** | (0.106) | †† | 0.522 *** | (0.089) | †† |
| **Satisfaction (1-very unsatisfied- 5 very satisfied)** | | | | | | | | | |
| Family household income | 0.226 | (0.150) | | 0.263 | (0.170) | | 0.166 | (0.124) | |
| Social contacts and friends | 0.353 *** | (0.095) | †† | 0.250 ** | (0.109) | † | 0.285 *** | (0.081) | †† |
| **Health and Health Care Use** | | | | | | | | | |
| Self-reported health (0-poor,3-excellent) | 0.280 *** | (0.100) | †† | 0.303 ** | (0.117) | † | 0.277 *** | (0.082) | †† |
| Depression score (CIDI-SF) | 0.198 | (0.165) | | 0.161 | (0.210) | | 0.121 | (0.147) | |
| Health care utilization (index) | 0.372 *** | (0.131) | †† | 0.068 | (0.159) | | 0.281 *** | (0.108) | †† |
| Number of observations | 975 | | | 938 | | | 1,409 | | |

*Notes:* *** indicates significance at 1%, **indicates significance at 5%, and *indicates significance at 10%. HB refers to Holm-Bonferroni correction. ††indicates significance at 5%, and † indicates significance at 10% after HB correction. Additional independent variables: age, age squared, gender, couple, number of years of schooling, living alone, and household size. Robust standard errors clustered at the block level.

**TABLE 4. Effects of Treatment on Deflated Prices of Common Products**

| Product | Means | | Regression | | | |
|---|---|---|---|---|---|---|
| | | | Merida 1 | | Merida 2 | |
| | Wave 1 | Difference w2 - w1 | Constant | Treatment fraction | Constant | Treatment fraction |
| Tortilla | 11.775 | 0.954 | 0.807*** | 0.272 | 1.007*** | -0.096 |
| | | | (0.112) | (0.208) | (0.064) | (0.111) |
| French bread | 2.736 | -0.069 | -0.054 | -0.021 | -0.052* | -0.049 |
| | | | (0.046) | (0.085) | (0.026) | (0.045) |
| Beans | 18.351 | 1.644 | 1.919*** | -0.667 | 1.746*** | -0.108 |
| | | | (0.441) | (0.818) | (0.250) | (0.434) |
| White rice | 13.847 | -0.358 | 0.070 | -0.879 | -0.224 | -0.275 |
| | | | (0.416) | (0.771) | (0.236) | (0.409) |
| White egg | 21.976 | 2.142 | 2.187*** | -0.107 | 2.415*** | -0.596 |
| | | | (0.492) | (0.913) | (0.279) | (0.484) |
| Milk | 12.585 | -0.514 | -0.047 | -0.773 | -0.648*** | 0.134 |
| | | | (0.217) | (0.403) | (0.123) | (0.214) |
| Milk powder | 26.705 | -1.546 | -3.143 | 2.158 | -0.641 | -0.946 |
| | | | (1.949) | (3.616) | (1.107) | (1.917) |
| Tomatoes | 16.305 | -3.304 | -3.227*** | -0.033 | -4.112*** | 1.692* |
| | | | (0.761) | (1.412) | (0.432) | (0.749) |
| Onion | 10.779 | -0.676 | -0.255 | -0.977 | -1.024*** | 0.924 |
| | | | (0.517) | (0.959) | (0.294) | (0.509) |
| Potato | 15.496 | -2.868 | -2.649*** | -0.710 | -2.742*** | 0.002 |
| | | | (0.405) | (0.751) | (0.230) | (0.398) |
| Soda | 16.942 | 0.209 | 0.138 | 0.125 | 0.207* | 0.024 |
| | | | (0.144) | (0.267) | (0.082) | (0.142) |
| Sweet bread | 2.819 | -0.054 | -0.140 | 0.209 | -0.054 | -0.040 |
| | | | (0.089) | (0.165) | (0.051) | (0.087) |
| Soup | 5.435 | 0.007 | -0.085 | 0.092 | 0.073 | -0.053 |
| | | | (0.131) | (0.242) | (0.074) | (0.128) |
| Detergent | 19.918 | -0.081 | 0.761 | -1.380 | 0.226 | -1.014 |
| | | | (0.759) | (1.407) | (0.431) | (0.746) |

| | | | | | | |
|---|---|---|---|---|---|---|
| Chlorine | 9.330 | 0.067 | -0.338 | 0.792* | -0.139 | 0.483* |
| | | | (0.194) | (0.360) | (0.110) | (0.191) |
| Cloth soap | 5.832 | 1.179 | 0.979** | 0.350 | 1.194*** | 0.018 |
| | | | (0.304) | (0.563) | (0.172) | (0.299) |
| Soap | 7.567 | 0.881 | 0.665* | 0.010 | 1.086*** | -0.018 |
| | | | (0.335) | (0.621) | (0.190) | (0.329) |
| Toilet paper | 15.795 | -2.161 | -1.532 | -1.425 | -2.561*** | 1.077 |
| | | | (0.985) | (1.828) | (0.560) | (0.969) |
| Toothpaste | 14.288 | -1.685 | -0.825 | -1.846* | -1.862*** | 0.531 |
| | | | (0.470) | (0.871) | (0.267) | (0.462) |

*Notes:* * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. $N = 867$. The dependent variable in the regressions is the change in the deflated price. The unit of analysis is the city block. Treatment fraction= fraction of eligible households that are treated.