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Designing Cash Transfer Programs for an Older Population: The Mexican Case

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Abstract

Aging populations and the prevalence of poverty in old age have led to the introduction of noncontributory pensions in many countries. We consider a number of alternative targeting approaches and simulate their effects in an empirical application in the State of Yucatan, Mexico. We compare the approaches with respect to leakage, under-coverage, and their effects on government budgets. We are also able to compare the simulated effects of one alternative with the observed effect of a recently introduced demogrant and find that the simulation is a close approximation of the empirical outcomes. We discuss issues of implementation and political feasibility.

Keywords

Targeting; Cash Transfer Programs; Noncontributory Pension Programs; Older Population; Mexico

1. Introduction

Many countries around the world are experiencing an increase in the proportion of older persons in their total population. The global elderly population (aged 60 years and older) is expected to double by 2050 (United Nations, 2014; Schwarz, 2003). A growing majority of the elderly live in developing countries where formal arrangements for old age support are often lacking and traditional arrangements are declining (Kakwani and Subbarao, 2005). The

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fragile nature of care arrangements for the elderly in developing countries and increases in poverty in old age has led to the introduction of noncontributory pension programs in more than 80 countries around the world (HAI, 2012).

Though raising the incomes of vulnerable populations can improve the overall welfare of society (Coady et al., 2004), programs to assist the poor require tradeoffs with financial sustainability in the face of restricted resources. Targeting recipients based on their level of need rather than applying universal eligibility serves to distribute scarce resources to those who need it most, thereby reducing “leakage” of the poverty budget to non-poor individuals. This, in turn, allows for greater generosity in benefits for a smaller eligible population or for the program to operate on a smaller budget. Effective targeting, in short, can improve resource allocation (Coady et al., 2004; Skoufias and Coady, 2007).

In many countries, particularly high-income ones, reported income is often used to identify the poor. This targeting method has a number of potential problems, including potentially high administrative costs, incentive effects, and fairness issues (Besley and Kanbur, 1990). A particularly challenging aspect of targeting recipients in developing countries is the potential for errors in identifying beneficiaries. Indicators of material wealth such as income, savings, and assets are considered the best option for targeting, but the difficulty in observing and correctly measuring these indicators in developing countries can diminish their efficacy in targeting the poor (De Wachter and Galiani, 2006; Ravallion and Chao, 1989). Income is difficult to assess and track in countries that do not have well-developed tax registries, that have large informal labor markets, or where the definition of a household is an issue (Besley and Kanbur, 1990; De Wachter and Galiani, 2006).

A second-best option for identifying the poor is to use observable characteristics of a household collected by program staff during an interview to predict household income and determine individual or household eligibility (Glewwe and Kanaan, 1989; Narayan and Yoshida, 2005; Grosh and Baker, 1995; Coady et al., 2004). This method is called proxy means testing or “tagging” (Akerlof, 1978) and it avoids issues with misreporting income when determining eligibility (Narayan and Yoshida, 2005). The success of proxy means testing depends on the ability to pick observable variables that are highly correlated with income and to reliably gather the necessary information (Coady et al., 2004). Much previous research has analyzed the effectiveness of means-testing rules for social-protection programs (see, for example, Grosh and Baker, 1995; Ahmed and Bouis, 2002; Coady et al., 2004; Castañeda, 2005; Castañeda et al., 2005). Previous research has not, however, assessed whether tagging is an accurate method for programs for older populations. The effectiveness of tagging is closely related to the accuracy with which individual income can be predicted and how precision is affected over the lifecycle by different components of income. Relevant components of individual income change considerably at the end of the lifecycle (Hood and Joyce, 2015). Therefore targeting rules may differ in their accuracy for older adults than for individuals at working age and this would apply for any policy that targets an older population.

Additionally, in developing countries, the difficulty in targeting for the elderly population, stems from the challenge of identifying sources of income for those without social-security

benefits and with sporadic sources of income, who may rely mainly on family transfers. The support of elderly generations through children, extended family, remittances, or by other members of the community poses a challenge to measuring income for recipients, especially if such support is volatile or in-kind (Kakwani and Subbarao 2005; Schwarz 2003). This issue is most pressing in developing countries due to the lack of universal coverage of social security benefits. One of the main concerns in the public debate has been the financial sustainability of programs to support the elderly.

The Mexican case is particularly noteworthy because of a rapidly aging population, lack of universal coverage of social security benefits, and high poverty rates among older persons. Moreover, we are able to compare predicted income with observable characteristics used for tagging with a comprehensive measure of income and assess the accuracy of tagging. This is relevant, as tagging for older populations may lead to important amounts of under-coverage or leakage, if it proves to be inaccurate. In this study, we evaluate various policy options for pension design (demogrant, flat-rate, sliding-scale, and perfect targeting) and assess whether targeting through proxy means testing is a sufficiently accurate method for the poor older population in a developing country. In our analysis, we exploit a rich panel data set from a field experiment in the state of Yucatan, Mexico for the population 70 years or older. The survey instrument from this field experiment is similar to that for the US Health and Retirement Study (HRS) and the Mexican Health and Aging Study (MHAS) and includes detailed information about income, wealth, and individual and household characteristics. Our income measure follows the one in HRS and MHAS surveys and captures family transfers (both monetary and in-kind), salary income (even from sporadic jobs), income from businesses or farms, pensions, income from properties, capital income, and transfers from governmental institutions. The experimental nature of the data allows us to compare our demogrant simulations with the real changes in income, which not only quantify the cash transfer but also show other behavioral changes such as crowding out of private transfers and changes on labor supply. This allows us to gauge the extent to which the simulations might be biased due to the induced behavioral effects.

Another contribution of this study is that there is scarce evidence on poverty in urban areas. Most of the prior literature focuses on rural areas (e.g. Zimmer, 2008; Barrientos et al., 2003; Kobetz et al., 2003) compared to poverty in urban settings (e.g. Ezech et al., 2006, Geronimus et al., 2015). In this study, we analyze targeting in an urban context for older adults. As one will see, it turns out that dwelling characteristics, asset ownership, and individual characteristics predict income for an older population reasonably well.

The remainder of the paper is organized as follows. In Section two, we describe the experimental design, data, and variables. In Section three, we describe the tagging methods and define various poverty and inequality measures. Section four presents the results, including descriptive statistics of the data, regression results of the Heckman selection model used to model household income, and the results of the simulated targeting mechanisms. We present poverty and inequality measures, and compare the costs of the different types of mechanisms. In addition, we compare the simulated effects of the demogrant with the observed effects of a demogrant that was implemented as part of the experiment. Section five provides a discussion, conclusion, and policy recommendations.

2. Data

The data we analyze are from the evaluation of a social-security program implemented in the State of Yucatan. This program provides benefits to individuals 70 and older in localities with more than 20,000 inhabitants. The program was designed to provide a non-contributory pension payment of MXN\$550 or US\$70.2 at 2013 PPP per month to any individual 70 or older in semi-urban and urban areas (see Aguila et al. 2014). The monthly benefit is equal to almost one third of the monthly minimum wage in Yucatan (MXN\$1,865.95 in January 2013 or US\$238.2 at 2013 PPP).¹ The program was implemented in phases throughout the state. To be able to evaluate the effect of the provision of the old age pension, we chose two towns of more than 20,000 inhabitants with similar demographic and economic characteristics, one of which would receive the pension, while the other would be the control town and only receive the pension at a later stage. Both towns had similar federal government programs and state government programs. Valladolid (45,868 inhabitants according to the 2005 Census) was randomly chosen to start receiving the pension in 2008, while Motul (21,508 according to the 2005 Census) was chosen as a control town. This paper is not about the experiment itself, but we take advantage of the availability of the extensive data collection that was part of the experiment.

For the experiment, surveys were conducted at baseline in both the control and treatment town and then in follow-ups after implementation of a social security program in the treatment town. The baseline survey collected data among persons at least 70 years of age and their households in the treatment town in August and September 2008 and identical data in the control town in October and November 2008. Evaluation staff conducted a follow-up survey in June and July 2009, in both municipalities approximately six months after the first pension payments in December 2008 in the treatment town. In this paper, we analyze baseline data collected for this program from August to November 2008, as well as data from the first follow-up in July-August 2009. For more information about the implementation of the non-contributory pension program, see Aguila et al. (2014). The baseline data sample includes 1,656 individuals, 1,146 of the treatment group and 510 of the control group. See Aguila et al. (2015) for a more detailed explanation of the sample design.

We define income as the sum of all wages, income from businesses or farms, pensions, income from properties, other capital income², and transfers (both monetary and in-kind) from relatives, friends, and governmental institutions. Governmental transfers include those from Oportunidades/Progres, Programa de Atención a los Adultos Mayores, and Procampo³ programs.⁴ Income components at the household level are divided between the

¹We denote Mexican pesos as MXN\$. The 2013 PPP exchange rate from Mexican pesos to U.S. dollars is taken from the Organisation for Economic Co-Operation and Development (n.d.).

²Including income from checking and savings accounts, fixed investments, stocks, bonds, and shares.

³Oportunidades, originally named Progres, is one of the main anti-poverty programs of the Mexican government targeting poor households in rural and urban communities and aiming to improve the education, health, and nutrition of children. It is a cash-transfer program conditional on regular school attendance and health-clinic visits. Programa de Atención a los Adultos Mayores is the federal-government noncontributory pension program for persons 65 years or older without social security benefits. Procampo is a cash-transfer program, launched in 1993, to provide support for agricultural producers in Mexico.

⁴Our definition of income does not include self-produced goods but we include in-kind transfers from relatives or friends, which include self-produced goods given to the respondent. We did not include questions about self-produced goods in the survey because Valladolid and Motul are urban areas with regosol soil that is mostly rocky and less suitable for agriculture (INAFED, n.d.). Therefore, self-production is less prevalent in these areas.

respondent and the spouse equally with the exception of government programs. Only 3.2% of respondents report receiving income from government transfers at the household level. Appendix A.1, Table A.1, shows descriptive statistics of those with and without reported income by source of income.

We also include in our analysis variables to predict income. These include individual characteristics (marital status, level of education, age, literacy, work status, household size), asset ownership (real estate, business or farm, car or bicycle, and other assets such as breed animals), dwelling characteristics (presence of toilet, sewer connection, running water, dirt floor, number of rooms, and whether respondent sleeps in the kitchen). In general, variables for predicting income should be few enough to allow the application of the proxy-means test to a large population and relatively difficult for a household to manipulate (Coady et al., 2004). There may be leakage errors from under-reporting or under-coverage errors from over-reporting (Martinelli and Parker, 2009). In the case discussed in this paper, the survey we use to simulate the effects of various means testing programs was collected a month before the program was announced and there generally was no incentive for respondents to over- or under report⁵.

3. Methods

In this study, we explore the tagging approach to predict income (Akerlof, 1978). Consider the following simple model:

$$y = x' \beta + \varepsilon \quad (1)$$

where y is some measure of economic well-being (which we will take to be individual income) and x is a vector of observables. As noted earlier, it may be easier to include a vector of observable characteristics x than to observe income y . When analyzing an older population in a developing country, we expect to observe a large proportion of individuals reporting no income. We assume that individuals not reporting income receive support through in-kind transfers (food, clothing, among others). To deal with this issue, we use a Heckman selection model (Heckman, 1979).

Let there be some cut-off point y_0 such that an individual is only eligible for the program when $y \geq y_0$. The estimation of a model like (1) also yields an estimate of the distribution of ε . We can then calculate the probability that $y \geq y_0$ for each individual for whom we observe x . Given that y is not observable, we may adopt a rule $x' \beta \geq z_0$, where the cut-off point z_0 may or may not be equal to y_0 . We can then calculate

$$\Pr[y \geq y_0 | x' \beta \geq z_0] \quad (2)$$

⁵The Social Security Program that was implemented is a demogrant and hence independent of respondents' incomes.

This gives us the probability that a potentially eligible individual does not get the pension (under-coverage), or that an individual is incorrectly deemed eligible and receives a pension (leakage). Once the model has been estimated, it can be used to simulate income distributions under different targeting scenarios: (1) a *demogrant* approach or universal coverage, whereby no targeting scheme is applied and all individuals in the sample would receive the same cash transfer; (2) a *flat rate* that would provide some level m0 if the predicted income falls below an eligibility cutoff or poverty line, A0 ; (3) a *sliding-scale*, whereby the benefit makes up the shortfall of the predicted income relative to the cut-off point with the same eligibility criteria as (2); (4) the “best-case” scenario in which income is observed for *perfect targeting*, with transfers made to provide a pension equal to the shortfall between reported income (excluding government transfers) and the poverty line. While options (2) and (3) are based on the same sample of eligible individuals targeted based on their predicted income, they vary in the generosity of benefits they represent.

The pension paid as part of the social security experiment in the State of Yucatan, is equal to MXN \$550 per month or US\$70.20 at 2013 PPP.⁶ For the purpose of our analysis we take this amount as the poverty line. In scenario (3), we therefore compute the pension P as follows:

$$P = \max \left\{ \min \left\{ 550, 550 \frac{z_0 - x' \beta}{z_0} \right\}, 0 \right\} \quad (3)$$

If we take $z_0 = 550$ then the formula for the pension simplifies to

$$P = \max \{ \min \{ z_0, z_0 - x' \beta \}, 0 \} \quad (4)$$

We will simulate income distributions under different targeting scenarios and estimate the effect of the targeting schemes on poverty and inequality. Various poverty metrics will be used. The first one is the poverty gap index, i.e. the total amount of money that would be needed to raise the income of all poor individuals exactly to the poverty line, divided by the total number of individuals. The poverty gap index is a special case of the Foster-Greer-Thorbecke poverty measure:

$$\text{Poverty}_\alpha = \frac{1}{N} \sum_{i=1}^q \left(\frac{y_0 - y_i}{y_0} \right)^\alpha \quad (5)$$

where N represents the total number of individuals, y_0 is the poverty line, and q is the number of poor individuals. The parameter α is a measure of the sensitivity of the index to

⁶The PPP exchange rate from Mexican pesos to U.S. dollars is for 2013.

poverty. The poverty gap index represents the case $\alpha = 1$. A second measure, the poverty headcount index, is obtained for $\alpha = 0$. The poverty headcount index is the number of poor as a proportion of total population size. Finally, the squared poverty gap is obtained for $\alpha = 2$. The squared poverty gap is more difficult to interpret. It places the highest weight on the largest income deficit, making it sensitive to the income distribution among the poor (Foster et al., 1984).

To analyze the effects of the targeting approaches on inequality we will use two inequality measures: the Gini coefficient and the Theil index. The Gini coefficient is defined as:

$$\text{Gini} = 1 + \frac{1}{N} - \left(\frac{2}{\mu N^2} \right) \left(\sum_{i=1}^N (N - i + 1) y_i \right) \quad (6)$$

where N represents the total number of individuals, and individuals are ranked in ascending order of y_i . $(N - i + 1)$ is the income rank of person i with income y_i , such that the richest person receives a rank of 1 and the poorest a rank of N , and μ represents mean income. The Gini coefficient runs from 0 (perfect equality) to 1 (maximal inequality).

The Theil index is an entropy measure. The maximum entropy represents perfect equality and the index takes the value of 0. The index is defined as:

$$T_r = \sum_{i=1}^N \frac{1}{N} \frac{y_i}{\mu} \log \left(\frac{y_i}{\mu} \right) \quad (7)$$

where N represents the total number of individuals, y_i represents the income of person i , and μ represents mean income.

To further analyze the relation between poverty reduction and budgetary costs, we will estimate how much poverty reduction is possible with a given budget. To that end, we specify different budget levels and determine the effect of these budgets on the various poverty measures. The approach we suggest is similar to Glewwe's solution of the targeting problem (1992); a similar approach was taken by De Wachter and Galiani (2006). Glewwe (1992) considers an explicit welfare maximization (poverty minimization) problem based on a parameterization of the poverty index subject to a government budget constraint. In estimating the parameter vector β he chooses the value of β that minimizes the poverty index subject to the constraint. Rather than following that approach strictly, we will consider somewhat simpler approaches as sketched above and investigate sensitivities to functional specification and provide estimates of leakage and under-coverage.

4. Results

Table 1 displays descriptive statistics of the sample that includes baseline respondents in Valladolid and Motul. The proportion of individuals who do not report any income other

than transfers from government programs is high, 47.0%. By contrast, the MHAS, which surveyed persons 50 or older in 2003, found 13.8% of the respondents reported no individual income (Wong and Espinoza, 2004). There are two potential reasons for older individuals not to report income. First, as discussed above, respondents may live with other family members and not have any income. Second, respondents may refuse to provide or may not know their income. This second reason, however, was of limited importance in our survey; we found that less than 0.5% of our respondents refused to provide or did not know their income. We also note that our data, unlike the MHAS, do not include imputations of income.

In Table 1, we observe that women and less-educated individuals are more prevalent among those reporting no income than they are among those reporting income. Persons 80 or older are less likely to report income than younger age groups. Individuals reporting zero income are less likely than those with income to report having health insurance. Persons with income are more likely to report owning real estate, while those without income are more likely to live in a dwelling with dirt floors.

4.1 Predicting Income

To achieve a parsimonious model, we used pretesting to select the variables that contribute significantly to the explanatory power of the model. We further reviewed the literature on existing algorithms for proxy targeting to decide on the final set of explanatory variables. Appendix A.2 describes the specification of the Heckman selection model we have estimated to predict individual incomes, as well as our method for simulating incomes.

Our aim is to predict individual incomes excluding government transfers (monetary or in-kind transfers of food or clothing from the government or charitable or social-welfare institutions). The likelihood of not reporting income may be related to a respondent's work history, with those in the informal labor market possibly more likely to not report income. Given the rules establishing eligibility for health insurance, having health insurance can be used as a proxy for a respondent's work history. In particular, coverage by health insurance under the Mexican Social Security Institute (IMSS) reflects a formal, salaried work history in the private sector, and coverage by the Institute of Social Services and Security for Civil Servants (ISSSTE) reflects such a history in the public sector (Knaul and Frenk, 2005). The selection equation thus differs from the income equation by the addition of an indicator on whether the respondent has health insurance.

Table 2 shows that marriage (being in a couple) and car ownership are associated with higher income. Real-estate ownership, whether of one's home or other property (including business), does not appear significantly related to the reported level of income. Working for pay is negatively associated with level of reported income but positively related to the probability of reporting a non-zero income. This may point to poor elderly having to engage in work that is often irregular and may generate little formal income. There is a positive relationship between income and number of rooms but no other significant associations between income and housing characteristics.

The likelihood of reporting any income is negatively related to the lack of health insurance, as expected, and having incomplete primary or no education. Working for pay, owning real

estate other than home, and living with at least one more person are all associated with a greater likelihood of reporting income. The estimated coefficient of the hazard rate λ suggests indeed that incomes are not missing at random.

4.2 Actual and Simulated Income Distributions

Figure 1 shows the actual income distribution, both including and excluding government transfers. As mentioned above, a large fraction of the sample reported no income. Including government transfers results in more persons reporting income, as one would expect. The distribution of reported income including government transfers is similar to reported income without government transfers for individuals with an income above MXN \$1,000 or US \$127.60 PPP. This is as expected given that government transfer programs such as Oportunidades, Procampo, and the Programa de Atencion a los Adultos Mayores all target the poor. Figure 1 also shows the simulated (predicted) distribution of incomes without government transfers. One sees that predicted incomes show a similar but smoother distribution than reported income without government transfers.

Figure 2 shows the simulated income distributions for the four types of targeting options, i.e. the demogrant, flat rate, sliding scale and perfect targeting. We calculated income distributions by adding the transfer under each targeting option to the total reported income, including government transfers. The demogrant option implies universal coverage. Hence there is no under-coverage error, but 32% of individuals under the demogrant scenario are receiving transfers without being poor.

To appreciate the effect of the various policies, these distributions should be compared to Figure 1. For example, 35.3% under the flat-rate mechanism and 27.7% under the sliding-scale mechanisms would have an income between MXN \$550 and MXN \$749, that is, at or just above the poverty line set at MXN \$550 (Figure 2). Prior to those transfers, this particular group represented about 5.0% of the sample or 3.3% of the sample when excluding government transfers (Figure 1). The increase seen in Figure 2 corresponds to the proportion of individuals initially classified as poor who received a benefit that lifted them above the poverty threshold.

As noted above, the eligibility criterion for the flat-rate and the sliding-scale is the same, as the predicted incomes are used to determine whether an individual falls below the poverty threshold of MXN \$550. Both for the sliding scale and the flat-rate scheme, some poor individuals may not receive benefits because of imperfect targeting. In the case of the sliding scale, moreover, some correctly targeted individuals may receive an amount that is not sufficient to lift them above the MXN \$550 threshold. The perfect-targeting alternative shows the counterfactual of eligibility based on respondents' reported income, with transfers equal to the poverty gap if the individuals were below the MXN \$550 threshold.

We calculate under-coverage and leakage errors by looking at the difference between eligibility based on predicted income and that based on reported income. The under-coverage rate is calculated by dividing the number of under-covered individuals (i.e., individuals classified as living above the poverty line but whose “true” income, excluding government transfers, falls below the poverty line) by the total number of “truly” poor

individuals (Grosh and Baker, 1995). We calculate the leakage rate by dividing the number of individuals whose true income is above the poverty line, but identified as eligible for a transfer, by the total number of individuals receiving transfers. We calculate an under-coverage error rate of 21.7% for the flat-rate and sliding-scale mechanisms, and a leakage rate of 17.7%.

To see how our simulations compare to reality, we compare our simulations of the demogrant scenario with the actual outcomes in the treatment town. Figure 3 shows the distribution of individual incomes including government transfers in the treatment and control towns as reported at baseline and in the first follow-up in the summer of 2009, as well as the simulated distribution based on the demogrant. The demogrant was implemented in the treatment town shortly after the baseline survey.

We observe a major shift in the distribution of income in the treatment town: while about 40% of individuals reported no income in the baseline survey, only 5% did so in the follow-up. The distribution has shifted instead to the poverty threshold at MXN \$550 or US\$70.2 PPP. The simulated effect of the demogrant in the treatment town is rather close to the observed distribution at follow-up, but with a somewhat larger dispersion. The differences between the simulated demogrant distribution and the actual income distribution of the elderly in the treatment town in the follow-up survey could be due to other behavioral changes such as crowding out effects of family transfers, recipient labor supply, family composition changes, changes in take-up rates of other government programs, among others. Aguilu et al. (2016) using data from the same experiment, find no changes in living arrangements, but they do find modest changes in take-up rates of other government programs. Aguilu et al. (2015) also using data from the same experiment find some leakage in the total recipient income due to a reduction in labor supply and fewer financial transfers from relatives. The authors found that work for pay decreased by 4.5 percentage points, and the crowding-out effects on family transfers represented on average 36% of the noncontributory pension. The limited effects on labor supply can be explained by a small proportion of older adults 70 or older still working for pay (16.5% at baseline).

Thus most of the difference between the simulated demogrant distribution and the actual income distribution of the treatment group in the follow-up survey is likely due to a decline in family transfers after the introduction of the noncontributory pension program. The five percent in the treatment town reporting no income in the follow-up survey may be the result of non-take-up of the pension. We also observe a decrease on the order of 5 percentage points in the control town in the percentage of the sample reporting zero income, while the rest of the distribution has changed very little.

To gauge somewhat more formally how close the simulated distribution of income in the treatment town is to the observed distribution at follow-up, we construct a simple measure of the differences between distributions. The histograms shown in Figure 3 classify income in 11 cells. To compare two income distributions, we calculate the squared difference in observed percentages in each of the cell between the two distributions and then take the average of these squared differences across the 11 cells. For instance, if we compare the income distribution in Valladolid between baseline and follow-up (i.e. before and after the

introduction of the demogrant), this measure equals 346.4. The same measure comparing baseline and follow-up in the control town equals 7.3. Comparing the simulated distribution after the introduction of the demogrant with the actual observed distribution at follow-up in Valladolid yields a value of 10.9. These comparisons suggest that the simulated distribution explains the bulk of the changes between baseline and follow-up, which in turn suggests that neglecting behavioral effects of the demogrant (reduction in family transfers and in labor supply) has a rather minor effect on the accuracy of the simulated outcomes. Since the other targeting approaches generally provide lower benefits than the demogrant (and hence may be expected to have smaller behavioral impacts), we tentatively conclude that also for the other approaches, our simulations probably provide a reasonable approximation of what would happen if these alternative programs were implemented.

4.3 Impact on Poverty and Inequality

Table 3 shows the poverty and inequality measures for each of the four targeting options as well as for a situation without a pension. In the latter case - not implementing any transfer program and not including government transfers - the poverty-gap index is 59.8%, the Gini Coefficient is 0.61 and the Theil index is 0.75. Using the sliding-scale would result in the highest poverty rate and inequality among the targeting options, with a poverty gap equal to 20.2%, a Gini of 0.52 and a Theil index of 0.59. The flat-rate scheme would reduce the poverty gap to 16.0%, the Gini Coefficient to 0.50 and the Theil index to 0.54. The difference between the flat rate scheme and the sliding scale reflects an inadequacy in the transfer amount computed through the sliding scale rather than under-coverage, leading to transfers too small to lift some eligible elderly out of poverty and hence a higher post-transfer poverty gap and inequality. Both the flat-rate and sliding-scale schemes reduce the poverty-gap index to about one-third of the no-transfer policy and inequality is only reduced by about one-fifth. Using simulations of the demogrant or perfect targeting, as noted earlier, would eliminate poverty and reduce the poverty-gap index to zero. In contrast, inequality is not eliminated but decreases to a Gini of 0.47 for the demogrant and 0.46 for perfect targeting. Results are similar for the other poverty or inequality measures.

Our results are sensitive to the choice of poverty line. To shed some light on this, we repeat the analysis but now use the official Mexican national poverty line of MXN \$709.69 (Table 4). Now, none of the approaches results in complete eradication of poverty, which is the obvious consequence of the fact that the official poverty line is higher than the MXN \$550 that is provided under perfect targeting or under the demogrant policy. In particular the poverty head count remains high under perfect targeting. This is not surprising as the perfect targeting approach makes sure that everyone has an income that is at least equal to MXN \$550, which is below the CONEVAL poverty line. Apart from this, we see that various poverty indicators fall under each of the policies considered. As before, the sliding scale shows the smallest effect. It turns out that the under-coverage and leakage rates are not very sensitive to the poverty line chosen.

To further compare our simulations with the observed changes in the treatment town between baseline and first follow-up survey, Table 5 shows the various poverty and inequality indicators at baseline and at the first follow-up survey for both the treatment and

control town. The proportion of the treatment sample living in poverty decreased from 68.9% in the baseline survey to 9.2% at follow-up and the Gini Coefficient decreased from 0.65 to 0.49 (and the Theil index from 0.88 to 0.53). The cash-transfer program implemented with no targeting mechanism thus helped reduce poverty by about two-thirds and inequality by about two-fifth to one-fourth. Poverty rates and inequality measures don't change much between baseline and follow-up for the control town, as one would expect given that only the treatment group received the non-contributory pension program.

Half of the individuals who remained in poverty after the implementation of the current demogrant program reported being enrolled in the program, which may indicate survey measurement error or underreporting. The other half did not report receiving the program which suggests a less than 100% take-up rate, as noted before.

4.4 Costs and Budget constraints

The choice of a particular targeting scheme will involve considerations of both the impact on poverty reduction and costs of the scheme. To compare the various targeting options, we estimate the relative costs incurred by the targeting programs and compare them to the demogrant approach, the most costly option. Figures 4-6 show the effect of available budgets on remaining poverty for the different poverty alleviation schemes.

For instance, the black area in the south west corner of Figure 4 shows that under perfect targeting, the poverty gap goes from about 8% when the budget is 50% of the demogrant budget to zero at about 59% of the demogrant budget. The dotted area indicates the poverty-gap index without transfers, which is 59.8%. The interpretation of the other areas in Figures 4-6 is analogous.

The least expensive policy option is the sliding scale, which would require 54.6% of the demogrant budget, while the flat rate option would require about 64.2% of the demogrant budget. The sliding-scale option, however, leaves, as Figures 4 and 5 illustrate, a poverty gap of 20.2% and a head count of 30.4% of individuals below the MXN \$550 threshold even after transfers.

With a budget equal to half the demogrant budget, the flat-rate scheme would allow a monthly pension of MXN\$433 per recipient.

5. Discussion and Conclusion

Aging populations lacking formal sources of income have led low and middle-income countries around the world to implement noncontributory pensions. In this paper, we explore the implications of various eligibility and cash transfer mechanisms, in particular regarding their impact on alleviating poverty, inequality, and their budgetary consequences, using data from an experimental noncontributory pension program in the State of Yucatan, Mexico. Due to underreporting of income among the elderly population, we estimated a Heckman selection model to predict income, using demographic characteristics, asset ownership and dwelling characteristics as explanatory variables. Comparing individual eligibility for these programs based on observed versus predicted income enables us to evaluate the targeting

accuracy of our model. We found that dwelling characteristics, asset ownership, and individual characteristics predict income for an older population with reasonable accuracy.

Using the flat-rate and sliding-scale mechanisms result in an under-coverage rate of about 21.7%, i.e., about one in five individuals who should receive a benefit would not under these schemes. Note that this only reflects an individual's eligibility to receive a transfer, which, in combination with the actual amount of the transfers and their adequacy, determines whether individuals remain in poverty. These scenarios result in a leakage rate of 17.7%, i.e., about one in six individuals that should not be eligible to receive a poverty benefit under these schemes would do so. The demogrant approach would have a leakage rate of 32%.

In our sample, the initial poverty rate was 59.8% and initial inequality measured by the Gini Coefficient was 0.61 (and 0.75 according to the Theil index). Using the sliding-scale would still leave 30.4% in poverty and the Gini would decrease to 0.53, while the Theil index would decrease to 0.59; under the flat-rate scheme 20.2% would remain below the poverty line and the Gini would fall to 0.50 (and the Theil index to 0.54), while poverty would be eliminated under the Demogrant and perfect targeting options and inequality declines to 0.46 or 0.47 (and Theil to 0.49 or 0.53). We have also evaluated the sensitivity of our results with respect to the choice of poverty line by using a different threshold –the CONEVAL poverty line (CONEVAL, n.d.). Our analysis shows that, at a cost similar to that for perfect targeting, the flat-rate option would be more effective in alleviating poverty and reducing inequality than the sliding-scale, but the sliding-scale remains the least-costly policy option. Overall, weighing leakage and under-coverage of each policy option remains a difficult task. The tradeoff between raising welfare for the poor and saving limited resources by prioritizing low costs for a program remains a challenge for policymakers. Moreover, all targeted programs may have unintended incentive effects on labor supply and on reporting of income or other targeted characteristics (Besley and Kanbur, 1990).

We analyzed the actual distribution of income following the introduction of a universal pension to all individuals aged 70 and older in the treatment town. We found that the poverty-gap index was 54.7%, the Gini Coefficient 0.65, and the Theil index 0.88 among baseline survey respondents in Valladolid before the implementation of non-contributory pension program. Six months after program implementation, the poverty gap was 7.0%, the Gini 0.49, and the Theil index 0.53. The proportion of individuals still in poverty six months after the implementation of the program likely reflects a take-up rate below 100% or survey measurement error. The non-take-up causes some discrepancy between the simulated effect of the demogrant (which assumes a 100% take-up) and the observed distribution in the treatment town. Nevertheless, as we have shown in Figure 3, the simulated income distribution under the demogrant appears quite close to what is observed in the treatment town after the non-contributory pension has been implemented.

In addition to the design of an effective targeting mechanism, one must consider the feasibility of different methods and the costs of applying them. Specifically, political feasibility may play an important role. For example, targeting poverty-alleviation programs would have a political cost if excluding the middle and higher income brackets were to undermine political support. At the same time, if a targeting program proves to be efficient

and thus minimizes tax outlays, then political support for it might increase, with non-beneficiaries realizing indirect benefits, such as political stability and social justice, from reduced poverty (Coady et al., 2004). In terms of costs, the literature identifies two types of costs as possible barriers to targeting: monitoring costs and participation costs for potential beneficiaries. First, monitoring costs may be high, as frequent monitoring of individuals and administrative training of auditors are required to ensure successful means-testing (Besley and Kanbur, 1990). Consequently, when choosing data-collection methods, policymakers should weigh the costs of collecting information against the likelihood of misreporting. Second, participation costs may prove too burdensome to intended beneficiaries, who might refuse to participate in extensive interviews or detailed assessments necessary to obtain the benefit (Besley and Kanbur, 1990, Coady et al., 2004). Such non-monetary costs may also include the time spent filing necessary documents or obtaining required certifications (such as identity cards or proof of residency).

By comparing the budgetary cost of a demogrant and that of perfect targeting, one can easily calculate what level of administrative costs would be allowable to make targeting more efficient. In our setting the total cost of a program using perfect targeting would be equal to 59.8% of the demogrant budget. Thus, total administrative costs equal to 40.2% of the demogrant budget per month would make perfect targeting equally expensive as a demogrant program.

We can apply the same logic to transfers based on predicted income. We found that a sliding-scale would be the least-costly option for transfers, but would have targeting errors similar to those for the flat-rate scheme. Comparing the sliding-scale to the demogrant approach, we conclude that the sliding-scale would be less expensive than the demogrant if monthly administrative costs would be less than 45.4% of the demogrant budget, but with the obvious added disadvantage of both under-coverage and leakage.

For further research, one could imagine extending the experimental design of a social security program as was done in our project in Yucatan, by implementing different means testing schemes and comparing outcomes. The government agency would have to collect the variables we found relevant for tagging in this study (dwelling characteristics, asset ownership, and individual characteristics) and analyze carefully the quality of the data collected. Improving methods to collect accurate data required to predict income will facilitate the policy decision process to assess more accurately the most suitable targeting method. Policymakers could consider implementing targeting depending on budgetary constraints, costs, and political feasibility.

Acknowledgments

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Appendix

A.1 Income Variable

Table A.1. shows the sources of income of individuals 70 years or older for those with and without reported income. For those who report income, 59.6% is from family transfers, 27.9% from salary income, 25.8% from pensions, and 16.3% from government transfers; only 2.7% derive income from a business or farm.

Table A.1
Sources of Income for Individuals 70 years or older

Source: Authors' calculation based on baseline Valladolid and Motul, 2008

	Reported income		No reported income		Difference
	Mean	SD	Mean	SD	
<i>Sources of Income (%)</i>					
Salary	27.90	44.90	0.00	0.00	27.9***
Business/farm	2.74	16.30	0.00	0.00	2.74***
Financial assets	0.91	9.51	0.00	0.00	0.91**
Family transfers	59.60	49.10	0.00	0.00	59.60***
Gov. Transfers	16.30	37.00	19.90	39.90	-3.57*
Pension	25.80	43.80	0.00	0.00	25.80***
Property income	1.14	10.60	0.00	0.00	1.14***
<i>Amount of Income (MXN)</i>					
Income	1564.00	2833.00	0.00	0.00	1564***
Income w/ gov.transfers	1628.00	2858.00	80.00	205.00	1548***
Salary	291.00	832.00	0.00	0.00	291***
Business/farm	133.00	1706.00	0.00	0.00	133**
Financial assets	9.01	203.00	0.00	0.00	9.01
Family transfers	479.00	1669.00	0.00	0.00	479***
Gov. Transfers	63.60	193.00	80.00	205.00	-16.30*
Pension	633.00	1462.00	0.00	0.00	633
Property income	20.90	372.00	0.00	0.00	20.90
No. of observations	877		780		

Notes: The classification of individuals with positive income and no reported income is based on total income including government transfers. SD refers to standard deviation.

p<0.01,
**
p<0.05,
*
p<0.1.

A.2 Predicting Income

We employ a Heckman Selection model to account for zero income. This model involves two equations: the regression and selection equations. The first equation considers the mechanisms determining the outcome variable, and the second one considers only the proportion of the sample with an observable outcome. The model assumes that the error

terms in both equations are normally distributed, that the error terms are independent of control variables, and assumes that the variance of the error term from the probit regression equals 1. The likelihood of not having zero or missing values is determined by:

$$I^* = X' \beta + \varepsilon \quad (\text{A.1})$$

and

$$I = 1(I^* > 0) \quad (\text{A.2})$$

where ε is a standard normally distributed error and I is a dummy, indicating positive income ($I = 1$) or zero or missing income ($I = 0$). The latent income process is

$$y^* = Z\gamma + u \quad (\text{A.3})$$

Observed log-income $\ln(y) = y^*$ if $I = 1$ and unobserved if $I = 0$. For the errors we assume

$$\begin{pmatrix} \varepsilon \\ u \end{pmatrix} \sim N \left[\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} 1 & \sigma_{u\varepsilon} \\ \sigma_{\varepsilon u} & \sigma_u^2 \end{pmatrix} \right] \quad (\text{A.4})$$

The conditional expectation of log-income, given that it is observed is

$$E(\ln(y)|X, I=1) = Z'\gamma + E(u|X, I=1) = Z'\gamma + \rho\sigma_u\lambda(X'\beta) \quad (\text{A.5})$$

Here ρ is the correlation between the error terms u and ε and λ is the Mills ratio.

The unconditional prediction of income is:

$$E(\ln(y)|X, Z) = Z'\gamma \quad (\text{A.6})$$

The simplest procedure to simulate incomes is to draw from the joint distribution of the error terms. If the simulated $I=0$, this generates a zero income. If $I=1$ we calculate y^* which is then the simulated observed log-income. We simulate five values per observation in order to obtain a stable distribution of predictions, using the following definitions of the error terms:

$$\varepsilon = e_1 \frac{\sigma_{\varepsilon u}}{\sigma_u} + e_2 \sqrt{1 - \frac{\sigma_{\varepsilon u}^2}{\sigma_u^2}} \quad (\text{A.7})$$

and

$$u = e_1 \sigma_u \quad (\text{A.8})$$

where e_1 and e_2 are drawn independently from standard normal distributions.

The Heckman model in its basic form relies on the assumption that error terms are normally distributed; it has been found in the literature that estimates based on that assumption may be quite sensitive to deviations from normality (e.g. Goldberger, 1983; Vella, 1998). It has become customary to sharpen identification by imposing exclusion restrictions. We have included a health insurance dummy in the selection equation, but excluded it from the income equation. Our choice of the health insurance variable reflects the extent to which households are connected to the formal sector. The outcome variable (income) is explained by a number of demographic and socio-economic variables, which by themselves should be a sufficient set of variables to explain the level of one's income. However, the connection to the formal sector bears direct relevance for whether one will have a source of monetary income at all.

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Highlights

- We design and simulate the effects of means testing benefit schemes for elderly.
- Simulations show pronounced beneficial effects on poverty and income inequality.
- For validation we use data from a unique field experiment in Yucatan, Mexico.
- The simulations provide a good forecast of observed effects in the experiment.

Figure 1. Reported individual income distribution with and without government transfers and predicted income

Source: Authors' calculation based on baseline survey Valladolid and Motul, 2008.

Figure 2. Individual income distributions after receiving the non-contributory pension
Source: Authors' calculation based on baseline survey Valladolid and Motul, 2008.

Figure 3. Reported Individual Income Including Government Transfers at Baseline and Follow-up and Simulated Demigrant Income

Source: Authors' calculation based on baseline and follow-up surveys Valladolid and Motul, 2008.

Figure 4. Poverty gap index by targeting mechanism

Source: Authors' calculation based on baseline surveys Valladolid and Motul, 2008.

Figure 5. Poverty headcounts by targeting mechanism

Source: Authors' calculation based on baseline surveys Valladolid and Motul, 2008.

Figure 6. Poverty gap index squared by targeting mechanism

Source: Authors' calculation based on baseline surveys Valladolid and Motul, 2008.

Table 1

Descriptive characteristics of the sample

Source: Authors' calculation based on baseline survey Valladolid and Motul, 2008

	Reported income		No reported income		Difference
	Mean	SD	Mean	SD	
Individual Characteristics (%)					
Male	59.90	49.00	52.40	50.00	7.43***
Couple	52.70	50.00	52.20	50.00	0.49
Education: No school	29.20	45.50	39.20	48.90	-10.00***
Education: Incomplete Primary	47.30	50.00	44.40	49.70	2.96*
Education: Primary or more	12.30	32.90	5.64	23.10	6.67***
Age 70-79	71.00	45.40	63.30	48.20	7.72***
Age 80-89	25.00	43.30	30.70	46.10	- 5.68**
Age 90-99	3.77	19.10	5.65	23.10	-1.88*
Age 100-109	0.23	4.78	0.39	6.20	-0.16
Read and write a message	62.60	48.40	50.90	50.00	11.70***
Lives alone	14.50	35.20	11.90	32.40	2.56
Lives w/ 1 other person	32.00	46.70	30.50	46.10	1.53
Live w/ 2+ other persons	53.50	49.90	57.60	49.50	-4.09*
No health insurance	23.40	42.30	33.20	47.10	-9.83***
Work for pay	28.20	45.00	2.31	15.00	25.90***
Assets Ownership (%)					
Car	3.99	19.60	2.95	16.90	1.04
Bike	0.91	9.51	0.64	7.99	0.27
Breeding animals	39.90	49.00	39.10	48.80	0.81
Home	78.70	41.00	77.90	41.50	0.73
Business/farm	3.76	19.00	2.56	15.80	1.20
Other real estate	3.65	18.80	1.54	12.30	2.11***
Income (MXN)					

	Reported income		No reported income		Difference
	Mean	SD	Mean	SD	
Amount of income	1564	2833	0	0	1564 ***
Amount income w/ gov. transfers	1628	2858	80	205	1548 ***
Dwelling Characteristics (%)					
Toilet in dwelling	72.20	44.80	66.80	47.10	5.38 **
Sewage in dwelling	81.50	38.80	79.10	40.70	2.43
Dirt floor	4.45	20.60	6.79	25.20	-2.35 **
Sleep in kitchen	14.50	35.20	16.80	37.40	-2.31
Number of rooms (mean)	2.69	1.21	2.55	1.25	0.14 **
No running water	10.60	30.80	10.50	30.70	0.09
No. of observations	877		780		

Notes: The classification of individuals with positive income and no reported income is based on total income including government transfers. SD refers to standard deviation.

p<0.01,

**
p<0.05,

*
p<0.1.

Table 2
Income and selection equation results

Source: Authors' calculation based on baseline survey Valladolid and Motul, 2008.

	(1) Log income	(2) Reporting income
<i>Individual Characteristics</i>		
Male	-0.027 (0.13)	0.095 (0.084)
Couple	0.311 ** (0.14)	-0.253 *** (0.091)
Education: No school	-0.465 * (0.24)	-0.449 *** (0.149)
Education: Incomplete primary	-0.317 * (0.19)	-0.329 *** (0.124)
Omitted category: Primary or more		
Age 70-79	1.227 (1.34)	0.341 (0.754)
Age 80-89	1.196 (1.33)	0.28 (0.755)
Age 90-99	1.241 (1.35)	0.281 (0.767)
Omitted category: Age 100-109		
Read and write a message	0.145 (0.15)	0.049 (0.094)
Lives alone	-0.012 (0.17)	0.118 (0.112)
Lives w/ 1 other person	-0.135 (0.13)	0.200 ** (0.080)
Omitted category: lives w/2 other persons		
No health insurance		-0.410 *** (0.081)
Work for pay	-0.871 ** (0.36)	1.676 *** (0.131)
<i>Assets Ownership</i>		
Car	0.977 *** (0.30)	-0.257 (0.206)
Bike	0.704 (0.59)	-0.136 (0.403)
Breeding animals	-0.141 (0.11)	0.077 (0.075)
Home	-0.145 (0.15)	-0.007 (0.096)
Business/farm	0.288 (0.30)	0.109 (0.198)
Other real estate	-0.057 (0.33)	0.701 *** (0.243)
<i>Dwelling Characteristics</i>		
Toilet in dwelling	-0.028 (0.20)	0.214 * (0.119)
Sewage in dwelling	0.261 (0.19)	-0.039 (0.120)
Dirt floor	0.273 (0.26)	-0.274 * (0.162)
Sleep in kitchen	0.08 (0.15)	-0.017 (0.095)
Number of rooms	0.111 ** (0.05)	0.029 (0.032)
No running water	-0.064 (0.28)	0.335 * (0.186)
Mills Ratio (λ)	-1.668 *** -0.46	
Constant	6.501 *** (1.47)	-0.224 (0.780)
No. of observations	1,538	1,538

Notes: The dependent variable in the income equation is the log of individual income without government transfers; the dependent variable for the selection equation is an income dummy variable indicating whether the respondent reported any income excluding government transfers. Standard errors in parentheses.

p<0.01,

**
p<0.05,

*
p<0.1

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Table 3
Impact on poverty with selected targeting options, unlimited budget

Source: Authors' calculation based on baseline survey Valladolid and Motul, 2008.

	Poverty headcount index	Poverty gap index	Squared Poverty Gap index	Theil Index	Gini Coefficient
Without transfers	69	59.8	55.9	0.75	0.61
Demogrant	0	0	0	0.48	0.47
Flat rate	20.2	16	14.4	0.54	0.50
Sliding scale	30.4	20.2	16.8	0.59	0.53
Perfect targeting	0	0	0	0.53	0.46

Table 4
Impact on poverty with selected targeting options, unlimited budget, and using the CONEVAL poverty line
Source: Authors' calculation based on baseline survey Valladolid and Motul, 2008.

	Poverty headcount Index	Poverty gap Index	Squared Poverty Gap Index	Theil Index	Gini Coefficient
Without transfers	72.2	62.2	57.9	0.75	0.61
Demogrant	44.9	9.1	1.9	0.48	0.47
Flat rate	52.4	21.9	15.1	0.53	0.50
Sliding scale	56.4	27.5	19.8	0.59	0.53
Perfect Targeting	63.5	21.3	7.8	0.53	0.46

Table 5
Poverty Indicators at Baseline and Follow-up by City
Source: Authors' calculation based on baseline survey and Follow-up Valladolid and Motul, 2008

	Poverty headcount Index		Poverty Gap Index		Squared Poverty Gap Index		Theil Index		Gini Coefficient	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
Including government transfer	Baseline	68.9	55.4	54.7	44.4	49.4	40.3	0.88	0.41	0.65
	Follow-up	9.2	53.7	7.0	43.7	6.4	40.5	0.53	0.42	0.49
Excluding government transfers	Baseline	71.8	61.9	61.4	54.9	57.2	51.9	0.88	0.41	0.66
	Follow-up	74.0	59.9	66.2	53.1	63.1	50.6	0.69	0.42	0.61