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Can a Small Social Pension Promote Labor Force Participation?

Evidence from the *Colombia Mayor* Program

Tobias Pfutze
Carlos Rodríguez-Castelán

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Abstract

One of the primary motivations behind the establishment of noncontributory pension programs is to allow beneficiaries to retire from the labor force. Yet, as with other unconditional cash transfer schemes, their aggregate effects may be more complex. Using panel data and instrumental variable techniques, this paper shows that the effect of one such program, Colombia Mayor, has been to raise the labor force participation of relatively younger male beneficiaries. This increase occurred precisely in the occupations with characteristics that are likely to require some up-front investment. The paper concludes that the transfer effectively loosened the liquidity constraints to remaining in these occupations. However, no such effect is found among women or older beneficiaries.

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Can a Small Social Pension Promote Labor Force Participation? Evidence from the *Colombia Mayor* Program

Tobias Pfutze[1]
Florida International University

Carlos Rodríguez-Castelán[2]
The World Bank

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[1] E-mail: tpfutze@fiu.edu (corresponding author)

[2] E-mail: crodriguezc@worldbank.org

1. Introduction

Direct cash transfers have become an increasingly popular policy tool to reduce poverty in low- and middle-income countries over the past two decades. While conditional cash transfers have made a grand entrance in Latin America through programs such as the *Bolsa Familia* conditional cash transfer program in Brazil and *Progresa-Oportunidades* (now *Prospera*) in Mexico in the late 1990s, studies have recently questioned the conditionality aspect of the transfers and put more attention on unconditional transfers. In particular, noncontributory pensions account for a major share of these nonconditional transfers. In Latin America alone, 12 of 26 countries have already implemented either a noncontributory pension or a complementary system (Bosch and Guajardo, 2012). Usually directed toward older individuals, with an emphasis on residents of rural areas, eligible individuals receive from as little as US$0.10 a day in Honduras to almost US$20.00 a day in Trinidad and Tobago according to the Inter-American Development Bank (Bosch, Melguizo, and Pagés, 2013). Because these programs do not require a contribution to a specific fund, the costliness of their implementation and their sustainability in sometimes politically fragile developing countries have been criticized by many (Bosch and Guajardo, 2012; Johnson and Williamson, 2006; Rofman, Apella, and Vezza, 2015).

This paper evaluates the effects of one such program, the Colombia Mayor noncontributory pension program. This program is especially interesting because it combines two noteworthy features: it is associated with a benefit that is quite small, and the age of eligibility is also among the lowest in such programs in the region. We show that the program has had the effect of increasing labor force participation in activities that can be expected to require some up-front investment for relatively young beneficiaries who are men (in their 50s and 60s). However, no such effect is found among women. This strongly supports the notion that unconditional cash transfer programs can expand the economic activity of recipients by easing liquidity constraints.

The literature on the role of cash transfers in easing liquidity constraints is fairly limited. Much of the literature on the use of cash transfers to promote entrepreneurship has focused on conditional transfers. Gertler, Martinez, and Rubio-Codina (2006) estimate that, for every peso transferred through *Oportunidades*, "beneficiary households used 88 cents to purchase consumption goods and services, and invested the rest," providing evidence of the use of transfers to relieve liquidity constraints in the face of investment. Similarly, in Bolsa Família, Ribas (2014) estimates that the share of entrepreneurs among men with low educational attainment who are beneficiaries has grown.

However, he questions the causal link between relieving financial constraints and the higher levels of investment because he observes a rise in private transfers among households. In regard to unconditional transfers or noncontributory pensions, there is evidence that supports the contribution of pensions to relieving credit constraints as in the case of blacks in South Africa (Berg, 2013). Moreover, the randomized control trial conducted by Haushofer and Shapiro (2013) sheds light on how regular unconditional transfers, as opposed to lump-sum transfers, are more well suited to encouraging long-term investment. There are four papers which are probably the closet to our study. First, Soares, Ribas, and Osório (2010), who evaluate Brazil's Bolsa Família and find that it raised female labor force participation by 4.3 percentage points, even though they do not discuss this result further. Fogel and Paes de Barros (2010) find for the same program, using aggregate data of Brazilian municipalities, that labor force participation increased by 2-4 percentage points for males. Results for females are also positive, yet not significant and smaller in magnitude. Next, Skoufias, Unar and González de Cossio (2013) exploit the experimental design of Mexico's Food Support Program (Programa de Apoyo Alimentario, PAL) on labor supply. Although they find no significant effects of the PAL on total labor market participation, their estimates show that PAL have a significant negative effect on the participation of males in agricultural activities which translated into a shift into participation in non-agricultural activities. They argue that PAL provide partial insurance (reduce down-side risks) for food consumption that allows program beneficiaries to allocate less time into agricultural production and more towards more risky non-agricultural activities. Finally, Martinez (2004), in an unpublished paper, finds that Bolivia's Bonosol Program boosted food consumption among rural households by more than what could be purchased with the amount of the transfers. He concludes that the additional funds are likely being used to invest in agricultural inputs.

Most other papers on the labor market impacts of noncontributory pensions find the opposite effect, or no effect at all. A decline in overall labor supply is documented in Aguila et al. (2012) and Galiani, Gertler, and Bando (2014), who explore pension programs in Mexico. However, the magnitude of the decline varied considerably; it was 4.3 percentage points in Yucatan, Mexico (work for pay) (Aguila et al. 2012), though Galiani, Gertler, and Bando (2014) find that the share of working individuals who received benefits through Mexico's Adultos Mayores Program fell by 20 percent after the expansion in coverage. Other kinds of programs produce more nuanced effects. In the same Adultos Mayores Program, Juarez and Pfutze (2015) find a reduction of similar magnitude among male recipients only, mainly driven by poorer beneficiaries. In Mexico City, Juarez (2010) finds that the effect of a nutrition transfer among seniors (Pension Alimentaria para Adultos Mayores) depends on household composition and demographics. Past age 60, men who live in households with qualified members retire early. However, individuals in younger age ranges increase their labor supply if they live with an eligible man, but decrease it if they live with an eligible woman.

Maluccio and Flores (2005) also find a statistically significant negative effect of 5.5 hours worked per week for men in response to participation in Nicaragua's Cash Transfer Program *Red de Protección Social*. Taking advantage of the initial randomized roll-out of Mexico's PROGRESA program, Skoufias and di Maro (2006) are not able to find any significant impact on labor force participation or leisure time. For the case of Chile's anti-poverty program *Chile Solidario*, Galasso (2006), in an unpublished paper, does not find any consistent results for labor market outcomes.

A different labor market–related question is whether noncontributory social protection mechanisms, of which pensions are only one type, incentivize informality. Labor force transitions between formal and informal jobs at the margin have been documented as unintended consequences of unconditional transfers. Following the implementation of Seguro Popular in Mexico, a shift toward informality was undertaken by beneficiaries (Knox and Campos-Vázquez, 2010; Aterido, Hallward-Driemeier, and Pagés; 2011; Azuara and Marinescu; 2013; Duval-Hernández and Smith Ramírez; 2011). However, Azuara and Marinescu (2013) also find that wages have remained constant, which might suggest that a wage differential did not elicit the shift toward informality. Similar results have also been documented elsewhere. In Colombia, after the expansion of a public health insurance program, informal employment rose by 2 to 5 percentage points (Camacho, Conover, and Hoyos 2014). In Argentina, Bosch and Guajarado (2012) find that the Moratorium, a social pension program, caused women working in formal jobs to retire earlier than they would have done otherwise. Untangling the demand side and supply side of the labor market invites further exploration. Although there might be evidence of less demand for formal labor among individuals receiving unconditional cash transfer benefits, Bosch and Campos-Vázquez (2010, 1) estimate that "had the program [Seguro Popular] not been in place, 31,000 more employers and 300,000 new formal jobs should have been registered with Mexican social security."

A sizable portion of the literature on noncontributory pension programs also focuses on micro level indicators of wellbeing (see Aguila et al. 2012; Barros, Ferro, and Romero 2008; Galiani, Gertler, and Bando 2014). In Yucatan, Mexico, noncontributory social security is associated with a decline in hunger rates and a rise in medical consumption spending (Aguila et al. 2012). Similarly, in rural parts of Mexico, Galiani, Gertler, and Bando (2014) and Salinas-Rodríguez et al. (2014) document lower levels of depression. In Bolivia and Mexico, recipients of noncontributory social pensions were able to raise their household consumption expenditure (Galiani, Gertler, and Bando 2014; Martínez 2004). Beyond individual indicators, Duflo (2003) explores the effect of the recipient's gender in South Africa and finds that women receiving pensions are more likely to have an effect on their daughters' nutritional status than on that of their sons. Furthermore, gender plays a secondary role in migration whereby households with a pension-receiving member are also more likely to have a woman member who is a migrant either for employment or to search for employment (Posel, Fairburn, and Lund 2006). Amuedo-Dorantes and Juarez (2015) and Juárez (2009) find that public transfers going to the elderly in Mexico (noncontributory pension programs) crowd out private transfers (mostly remittances). Their finding suggests that the effect of public transfers may extend to the support networks of program recipients.

On the macro side, transfer programs, in general, have been studied as a potential mechanism of inequality reduction. Gasparini and Lustig (2011, 17) estimate that, in Brazil, changes in public transfers "explain 49 percent of the total decline in inequality." In another paper, Lustig and Pessino (2014) show similar results in Argentina, where the pension moratorium, introduced in 2004, contributed to lowering the Gini coefficient by 4.2 percentage points. Equally important in development economics is poverty reduction. In Brazil and South Africa, noncontributory social pensions going to the elderly were found to have a significant effect on poverty reduction and prevention as well as the prevention of the transmission of intergenerational poverty (Barrientos 2005; Barrientos and Lloyd-Sherlock 2003).

In the next section, section 2, we give a more in-depth motivation for our study, including the theoretical motivation. This is followed, in section 3, by a detailed description of the Colombia Mayor Program. Section 4 describes the quality of the program's targeting, which will lead us, in section 5, to a discussion of the susceptibility of the program to evaluation. Section 6 describes the data used and examines our empirical strategy, and section 7 presents our results. Section 8 concludes.

2. Motivation and Theoretical Background

Colombia's noncontributory pension program features two particular characteristics that make it worth studying. First, in comparison with other programs in the region and beyond, it offers a fairly modest benefit. While the size of the benefit is not specifically determined by the program's rules of operation (it depends on the budgetary situation), official data show that, in 2012, the benefit paid around Col$41,000 a month (about US$23 at the time). This amounts to little more than US$0.75 a day at exchange rates or around US$1.50 a day at purchasing power parity (PPP). By comparison, similar programs pay around US$7.00 a day PPP in Argentina, US$11.00 a day PPP in Brazil, and US$6.50 a day PPP in Chile. Even the corresponding benefit in much poorer Bolivia pays around US$2 a day PPP (Bosch, Melguizo, and Pagés 2013). Second, the minimum age to qualify for the benefit is 52 for women and 57 for men, the lowest corresponding ages of eligibility in the region. In most other countries, it is 65 or 70.

At first sight, the effect on labor market decisions of such a cash transfer to the elderly should be straightforward. If one thinks of the impact as a simple income effect in a consumption-leisure decision framework, recipients would be expected to work fewer hours or drop out of the labor force altogether. As described above, several studies on similar programs have found precisely such an effect. Yet, given the paltry amount of the benefit paid out by Colombia Mayor, it would not be surprising if this effect were small and possibly statistically insignificant.

A different way to look at the benefit is as a constant income stream to a poor, but potentially economically active population. Such a stream could have two effects: (a) it may act as a form of insurance mechanism, allowing beneficiaries to engage in somewhat risky economic activities (as interpreted by Skoufias, Unar and González de Cossio, 2013); and (b) it could alleviate the liquidity constraints preventing beneficiaries from pursuing lucrative business or employment opportunities. The first effect should result primarily in a shift from employment, even if under precarious conditions, to self-employment. The second effect could result in a similar shift, but also in an increase in labor force participation or the number of hours worked. One would anticipate the increase to be concentrated among activities that require seed capital, such as small-scale commerce, food preparation, or agriculture. However, it cannot be ruled out that potential beneficiaries are prevented from taking advantage of employment opportunities if, for example, they are unable to afford transportation costs or proper attire before the first benefit payment is received.

3. Description of the Program

Colombia Mayor was launched in 2003 as the Programa de Protección Social para el Adulto Mayor (PPSAM). The aim was to provide basic subsistence through noncontributory pension benefits to elderly people who had no pension income and were living in extreme poverty. In 2010, program administration was outsourced to the privately run Consorcio Colombia Mayor, which reports to the Ministry of Labor. The purpose of the change was to accelerate rollout among all elderly who were living in conditions of extreme poverty. The amount of the transfer is adjusted annually based on budgetary considerations, but it can never exceed half the value of the minimum wage. The program also provides a number of indirect nonmonetary benefits that are supplied through specially established centers catering to the needs of the elderly population and administered and cofinanced by the municipalities.

There are three main program eligibility criteria. The first is that the beneficiary must not be within three years of reaching the official retirement age. During the period under study, this meant a maximum age of eligibility for the benefit of 52 years among women and 57 years among men. (These ages were subsequently raised by two years.)

The second criterion establishes that the beneficiary's household must not score above a certain threshold in Colombia's system for the identification of potential social program beneficiaries (in the Spanish acronym, Sisben). The Sisben score represents an estimate of the living conditions of households registered in the system. It is based on information on households collected through a system survey, including the quality of the dwelling, the number of dependents, disability status, income, ownership of durable goods, and so on. The Sisben score is used to determine the eligibility for all the country's social programs at the national level. It has gone through two mayor modifications, such that the current, third, version is usually referred to as Sisben III. Each program is associated with a unique maximum score to identify beneficiaries. The maximum scores usually differ depending on whether a household resides in one of the country's 14 major metropolitan areas, in other urban areas, or in the countryside. Moreover, for many programs, eligibility is subdivided into up to three different levels, corresponding to different maximum scores. For prioritization purposes, Colombia Mayor distinguishes between two levels. The maximum scores for level 1 are 36.32 for the 14 major cities, 41.90 for other urban areas, and 32.98 for rural areas. For level 2 eligibility, which we use to determine Sisben eligibility, the respective scores are 39.32, 43.63, and 35.26.

The third criterion revolves around income. It establishes that, in the case of one-member households, a beneficiary's income may not exceed half of a minimum wage. For beneficiaries living with other individuals, the entire household income may not exceed one minimum wage. In the data described below, we are able to observe how all three criteria would function; yet, we consider a potential beneficiary eligible for the program if the age and Sisben score requirements are met. We have decided not to impose the income criterion because it likely suffers from substantial measurement error in our data. The income criterion is also subject to considerable changes over time. We believe, moreover, that, given the difficulty of sorting out this criterion within the Sisben score, it is also the criterion that is most problematic to apply in practice. In filling out the Sisben questionnaire, respondents have perverse incentives to underreport income to qualify for various government assistance initiatives. Adding support to these concerns, our own data show only a limited overlap between the Sisben scores and households that are eligible based on income. The survey data employed and described in more detail below is designed to allow construction of Sisben scores.

In addition to eligibility, the program also employs a prioritization strategy, mostly based on age, plus other criteria, such as disability status, number of dependents, and so on. The strategy represents an attempt to focus the program on the population 65 years of age and older. Every year, each municipality is given a certain number of slots for new beneficiaries. If the number of eligible petitioners exceeds the number of available slots, the prioritization strategy is intended to determine who receives the benefit.

4. Program Targeting

According to the country's Ministry of Labor, the Colombia Mayor Program is targeted at the protection of persons in conditions of extreme poverty (Colombia, Ministerio de Trabajo 2013). It is of interest to study not only the program's impact on beneficiaries, but also the extent to which these correspond to the group at which the benefit is targeted. We start by comparing our aggregate data with the data published by the government. According to the Ministry of Labor, the number of beneficiaries rose from 718,376 in December 2012 to 1,259,333 one year later (Colombia, Ministerio de Trabajo 2014). These numbers almost perfectly coincide with those given to us by the Consorcio

Colombia Mayor, which puts them at 718,376 and 1,259,004, respectively. The data of the 2013 round of the Encuesta Nacional de Calidad de Vida (National Survey of Quality of Life, ENCV) used in the analysis were collected during September and October of 2013. The Consorcio Colombia Mayor also provided us with figures for October of 2013, putting the number of beneficiaries at 1,012,724. The numbers implied by the ENCV are a little lower, but not by much. A weighted total of the number of beneficiaries declared by each household yields 828,738 beneficiaries. If most of the program's expansion occurred in the second half of the year, this calculation seems fairly reasonable.

Table 1 presents this last total, together with other estimates of the target population that are also derived as weighted totals from the 2013 round of the ENCV. We present totals for different age-groups and subgroups by socioeconomic status. The first age-groups refer to the minimum age of eligibility for the program (52 or older for women, 57 or older for men), followed by the 60–65 age-group. For socioeconomic status, we first consider whether an appropriately aged person would qualify for the benefit based on the household's Sisben score. We have also created indicators for whether a household's per capita income puts it below Colombia's poverty line and below the extreme poverty line (indigence). For this, we followed the Ministry of Labor's own methodology.[3] The main insight is that the total number of the poor in each age-group is only slightly lower than the number of people living beneath the Sisben eligibility threshold. This bodes well for the Sisben criterion in providing a good proxy for poverty. Moreover, given the enrollment numbers supplied by the ministry, the poor 65 years of age or older should be largely covered.

Table 1: Estimates of the Target Population according to ENCV 2013

Total beneficiaries	828,738
Age eligible	
Total	7,383,638
Sisben eligible	2,083,818
Poor	2,066,330
Indigent	699,141
60 years of age or older	
Total	4,947,172
Sisben eligible	1,669,819
Poor	1,476,454
Indigent	525,100
65 years of age or older	
Total	3,414,864

[3] See "Estadísticas por Tema: Pobreza y Condiciones de Vida" (database) (accessed August 4, 2015), National Administrative Department of Statistics, Bogotá, Colombia.

Sisben eligible	1,363,200
Poor	1,059,720
Indigent	385,274

A more detailed look at the data, however, yields a more complicated picture. Table 2 shows the distribution of all age-eligible individuals according to their eligibility by the Sisben criterion and their poverty status. The shares represent weighted averages of each category over all survey years. While table 1 shows that the total number of age-eligible elderly with low Sisben scores is about equal to the number of elderly living in conditions of poverty, table 2 shows that these two groups overlap only partially. Over 35 percent of the poor are not considered eligible according to their Sisben scores. The same is true of almost one-third of the indigent elderly.

Table 2: Comparison of Sisben Score and Poverty Status, percent

Eligibility	*Nonpoor*	*Poor*	*Nonindigent*	*Indigent*	*Total*
Noneligible by Sisben score	52.73	10.60	60.22	3.11	63.33
Eligible by Sisben score	17.71	18.96	28.62	8.06	36.67
Total	70.44	29.56	88.83	11.17	

The most important question is how the eligibility criteria translate into actual receipt of the benefit. Table 3 shows the share of individuals who report living in a household where at least one person receives the benefit. We present weighted averages from the 2013 round of the ENCV. As can be seen, coverage levels are fairly low in each of the target groups. Even among individuals who are older than 65 years of age, only slightly over 50 percent of the indigent and of persons living in households that are both poor and in Sisben groups 1 or 2 receive the benefit. Among all other groups, coverage is significantly lower.

Table 3: Share of Beneficiaries according to ENCV 2013, percent

	Age eligible	*60 or older*	*65 or older*
Sisben eligible	32.44	37.20	40.69
Poor	31.85	39.91	47.88
Indigent	36.72	44.83	54.65
Sisben eligible or poor	28.69	34.80	39.92
Sisben eligible and poor	40.84	46.79	52.04

A different way to address this question is shown in table 4. Here, we look at the share of program beneficiaries within each group in 2013. As becomes clear, almost 30 percent of the beneficiaries did not fulfill the Sisben requirement, and almost 40 percent are not poor in monetary terms. While only

48 percent are both poor and below the Sisben threshold (which should be close to the actual eligibility requirement), more than 15 percent of the beneficiaries are neither.

Table 4: Share among Beneficiaries in Groups according to the ENCV 2013, percent

Sisben eligible	71.65
Poor	60.59
Indigent	23.64
Sisben eligible or poor	84.25
Sisben eligible and poor	48.00

To obtain a better grasp of the role played by the eligibility criteria, we have run simple kernel regressions. Figures 1 and 2 show the relationship between the actual receipt of the program benefit by an age-eligible person and the household-level Sisben III score. Figure 1 shows the regression for each year, and figure 2 the pooled results across all years, but by area. The threshold values are 39.32 for residents of the 14 major cities, 43.63 for residents of other urban areas, and 35.26 for rural areas.

Figure 1: Kernel Regression of Program Receipt on the Sisben Score, by ENCV Round

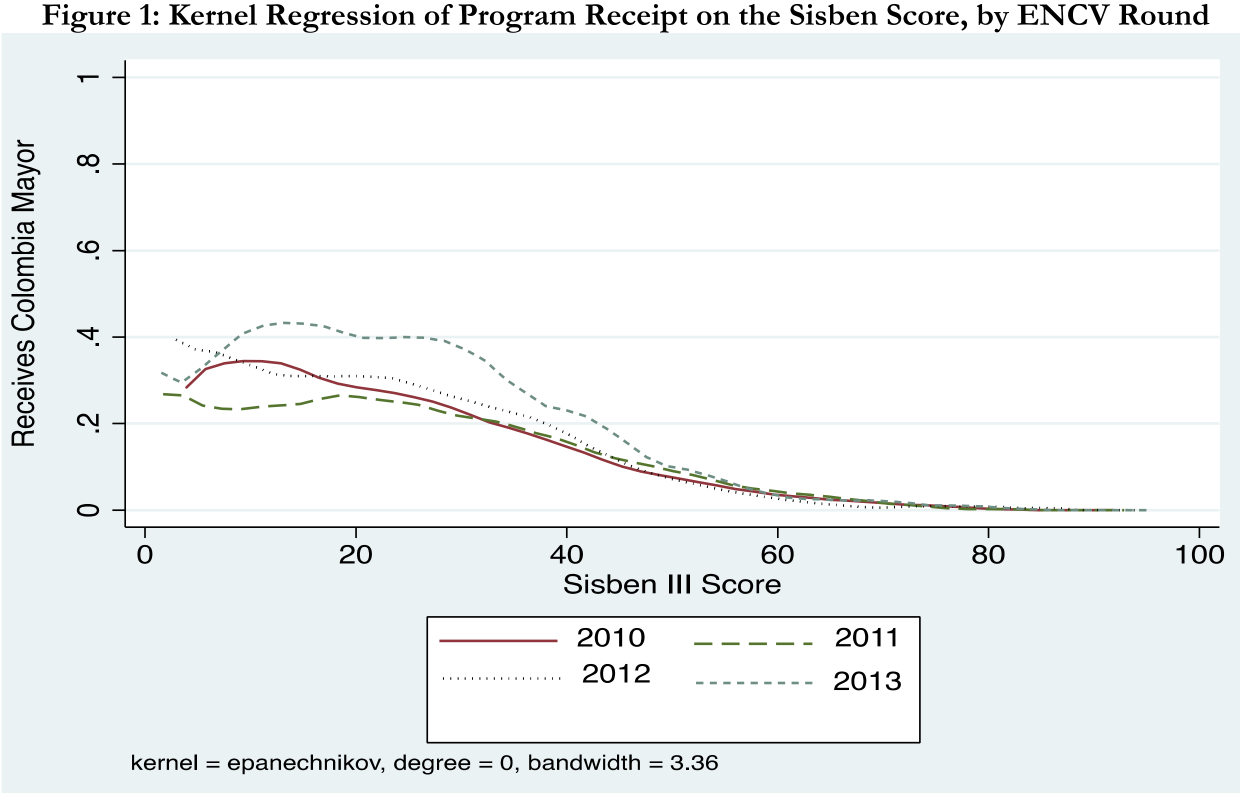
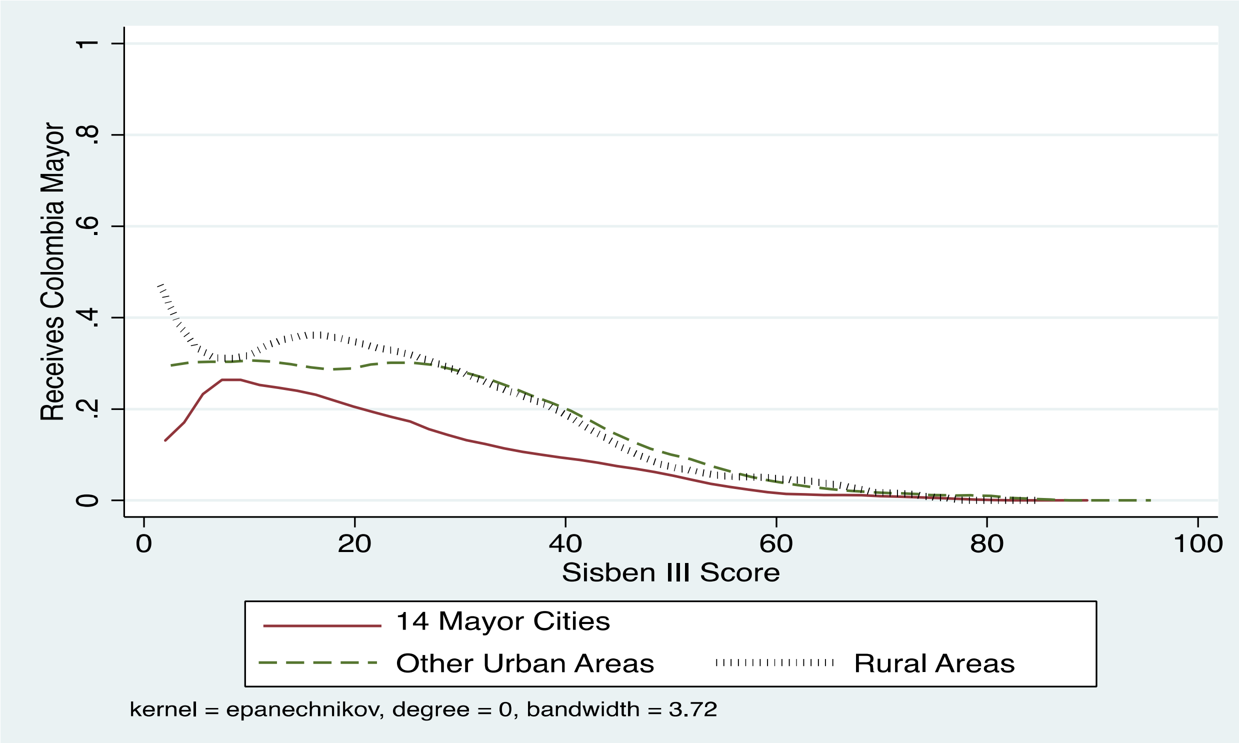


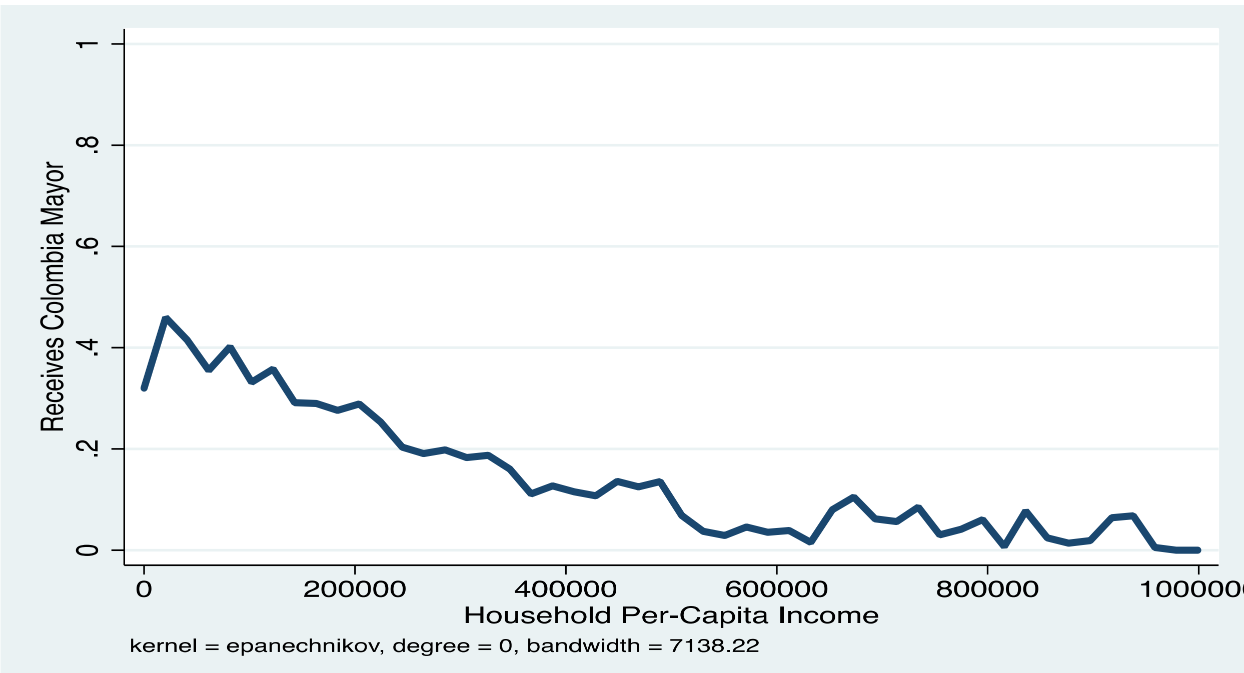
Figure 2: Kernel Regression of Program Receipt on the Sisben Score, by Place of Residence, Pooled across ENCVs 2010–13



This result is not surprising considering how the Sisben is administered. The Sisben interviews are carried out by the municipalities. While the responses are tallied centrally and the formula for calculating the score is secret, the municipality administrations have a clear interest in raising the number of social program beneficiaries in their constituencies. Camacho and Conover (2011) have analyzed the problem clearly. They show that the municipal authorities time the Sisben interviews during the electoral cycle. More importantly for the present case, when the formula for an older version of Sisben became publicly known, the distribution of the scores started to develop a discontinuity exactly at the (then universal) cutoff level for social programs, bringing more applicants into the program. Even if the formula is secret, local authorities should be able to gauge the probability of falling below the eligibility threshold based on their own experience (the final Sisben score is observable to the household). The interviews can then be manipulated accordingly because how each variable affects the overall score should be clear.

In figure 3, we present the results of a similar exercise on household per capita income in the 2013 ENCV round. To avoid the long tail of high incomes, we have included only households with a per capita income of up to Col$1,000,000 a month (around US$400 at exchange rate values). This leaves out less than 10 percent of our sample. As with the case of the Sisben score, no clear cutoff area can be defined. Rather, the probability of program receipt declines continuously and stays positive well into fairly high incomes. The actual poverty lines differ by region and municipality, reflecting differences in the cost of living. However, in 2013, they ranged roughly between Col$140,000 and Col$240,000. The corresponding line for indigence was between Col$77,000 and Col$100,000. As is apparent in the graph, there is a high probability of program receipt up to about twice the highest poverty line. And this probability seems to stay positive throughout.

Figure 3: Kernel Regression of Program Receipt on Per Capita Household Income, Pooled ENCV 2010–13



5. Evaluating the Program

Colombia Mayor presents a number of formidable obstacles in a rigorous ex post impact evaluation, all of which may be reduced to the lack of a clearly identifiable control group. In this section, we discuss three possible approaches that, based on the program design, seem feasible, but turn out not to be so.

In theory, the Sisben eligibility criterion should be amenable to evaluation through an identification strategy involving a comparison across households within some range to the left and right of the cutoff in a design inspired by regression discontinuity (RD). Given the discussion in the previous section, it should come as no surprise that this approach does not prove to be fruitful. However, even without such knowledge, we would not necessarily believe that the cutoff is followed strictly (as in a sharp RD design) because there may be differences between the ENCV data and a household's characteristics at the time of the Sisben interview. (For instance, the program benefit may have lifted a marginal household across the eligibility line.) However, this would not pose any major problems as long as the probability of program receipt were still to change discontinuously at the cutoff (as in a fuzzy RD design). A couple of unpublished working papers on the predecessor of Colombia Mayor, the Programa de Protección Social al Adulto Mayor, attempt an RD approach based on estimated Sisben scores and thresholds, but do not find any results (Rubio 2014; Rubio, Hessel, and Avendano 2015). Analyzing the effect of a different social protection program for which eligibility is based on Sisben scores (Régimen Subsidiado, a public health insurance program), Miller, Pinto, and Vera-Hernández (2013) use a similar strategy, but find only weakly significant results. We are able to observe the actual Sisben scores implied by the ENCV data and find that the official thresholds are not enforced in practice. Moreover, below, we also provide statistics on municipality-specific prioritization, which would be the principal reason to estimate municipality-specific thresholds, and show that, at least for Colombia Mayor, there is not a lot of variance across municipalities.

In table 5, we present the results of a simple RD-inspired spline regression with standard errors clustered at the municipal level. We regress the binary outcome variable of a household receiving Colombia Mayor according to whether it is Sisben eligible (another binary variable) and measure the absolute value of its distance to the cutoff, which we capture using two separate variables depending on whether the household is Sisben eligible (that is, to the left of the cutoff) or not (to the right of the cutoff). In additional specifications, the squared values of these distance measures are included. This analysis is carried out for households with at least one age-eligible member within a neighborhood of either 5 points from the cutoff (columns 1 and 2), or 10 points (columns 3 and 4). This setup corresponds roughly to a first-stage regression in a fuzzy RD design, and, for the approach to be valid, one would need a strong positive effect of the binary variable Sisben eligible (which would be interpreted as showing that the probability of program receipt changes discontinuously at the cutoff). It can be easily seen that the variable of interest is all but insignificant statistically as well as in magnitude. If we add the squared terms, it also becomes clear that the relation between the distance to the threshold and actual program receipt is best captured by a linear trend. Finally, figure 4 gives a graphical impression of the probability of receiving the benefit within the five-point window in the

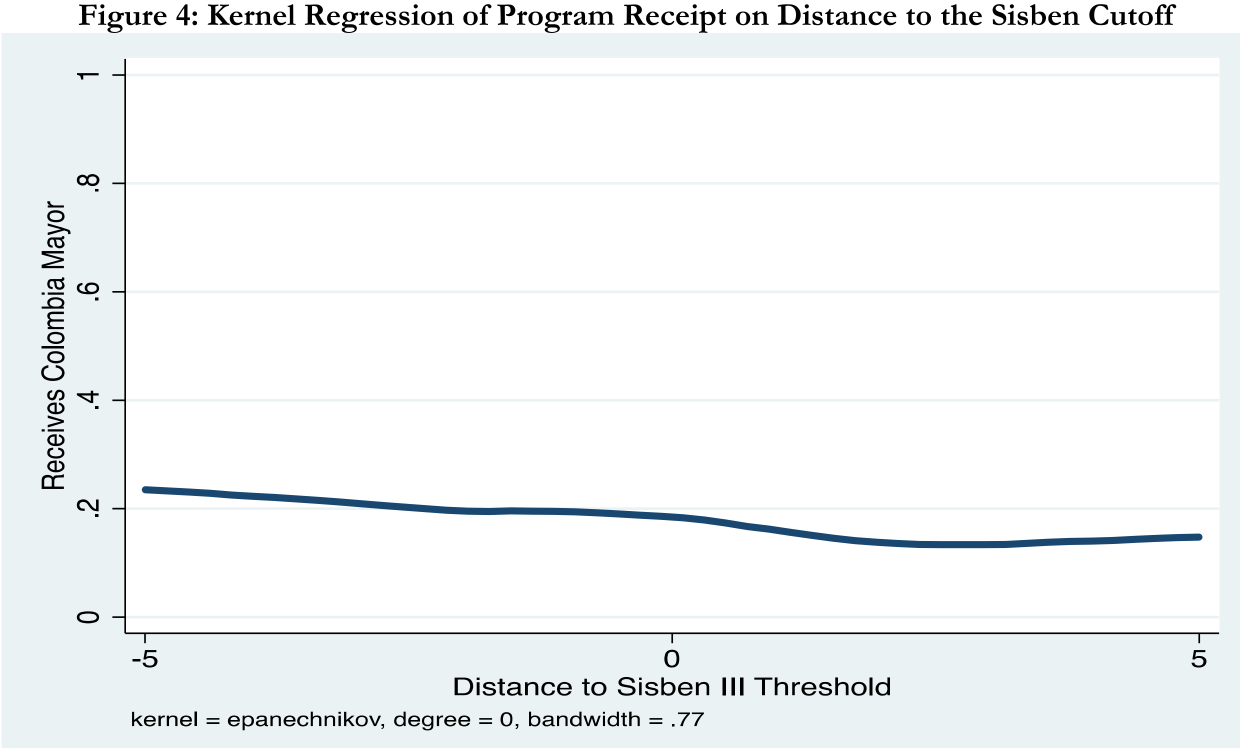
Sisben score around the corresponding threshold value. As above, we have prepared the data using a kernel regression. Figure 4 confirms the results shown in the table, that is, there is no significant change around the threshold.

Table 5: RD Spline Regression of Program Receipt on Distance to the Sisben Cutoff

Dependent variable: receipt of Colombia Mayor	*(1)* *Within 5 points*	*(2)* *Within 5 points*	*(3)* *Within 10 points*	*(4)* *Within 10 points*
Sisben eligible	0.005	0.128	0.002	0.018
	(0.042)	(0.157)	(0.018)	(0.041)
Distance to cutoff, left	−0.009	0.063	−0.009***	−0.002
	(0.011)	(0.095)	(0.003)	(0.015)
Distance to cutoff, right	0.009***	0.019***	0.009***	0.011***
	(0.003)	(0.005)	(0.001)	(0.004)
Distance to cutoff, left squared		0.010		0.001
		(0.014)		(0.001)
Distance to cutoff, right squared		0.003**		0.000
		(0.001)		(0.000)
Constant	0.234***	0.227***	0.231***	0.232***
	(0.010)	(0.010)	(0.009)	(0.009)
Observations	10,859	10,859	21,049	21,049
R-squared	0.013	0.013	0.028	0.028
F	13.66	11.56	30.85	23.41

Note: The table shows the results of a linear probability model on a dependent variable indicating receipt of Colombia Mayor. Standard errors are in parentheses.

Figure 4: Kernel Regression of Program Receipt on Distance to the Sisben Cutoff



A second route to evaluation is offered through prioritization. In cases in which the number of eligible applicants in a municipality exceeds the number of available slots in a given year, applicants are ordered by a number of vulnerability criteria, each of which adds or subtracts a given amount to a numeric score. Applicants with the highest such score are then enrolled first, giving rise to a municipality-specific threshold value below which enrollment in the program is postponed. The score ranges from a minimum of 7 to a theoretical maximum of 22. The only component that adds negative values is age for individuals 64 years old or younger. In theory, this mechanism allows identification to be based on a comparison between closely similar households living in different municipalities with different thresholds. Closer examination, however, reveals that the prioritization score is only binding in a few cases and almost only for the relatively young (that is, applicants in their 50s or early 60s), given that age is the single most important element in the score. Table 6 shows that, of the 1,105 municipalities on which we have 2013 prioritization data, only 78 did not apply prioritization at all. However, almost all the other municipalities had low threshold values; the lion's share was between −4 and −7 (a score that can only correspond to a person in his 50s), and only 18 municipalities had a positive threshold value. The bottom line is that the prioritization threshold does not yield sufficient variance to act as a feasible identification strategy.

Table 6: Distribution of the Prioritization Threshold across Municipalities

Threshold value	*Municipalities, number*
Did not apply prioritization	78
−7	258
−6	267
−5	186
−4	133
−3	86
−2	42
−1	27
0	9
1	13
2	1
3	1
4	1
5	1
6	1

Lastly, the program's staggered rollout in municipalities could potentially be used as a source of quasi-exogenous variation. The program was implemented initially in all municipalities more or less simultaneously without any clear selection criteria. By the time the Consorcio Colombia Mayor was established, pretty much all municipalities had some beneficiaries. Unfortunately, according to the data made available to us by the consorcio, the subsequent expansion of the program with the aim of enrolling the entire target population proceeded in a parallel fashion across municipalities (meaning that there was no clearly identifiable subgroup of municipalities or other criteria that determined the speed of the expansion). Given that no objective estimate of the number of individuals in the target population—that is, individuals who fulfill the age and Sisben requirements—is available at the municipal level, we calculated an estimate of coverage by dividing the number of beneficiaries reported by the consorcio by the number of people who are age eligible according to Colombia's population projections.[4] We have complete yearly data on 1,101 municipalities in 2010–14. The first observation is that, of the 5,505 individual observations, 55 are larger than 1 and, in many cases, by a substantial amount (the largest value is over 12). This points to the unreasonably large number of beneficiaries in the consorcio data. (Keep in mind that the denominator also includes noneligible individuals of the appropriate age.)

We have run a simple regression of the estimated coverage—the share of beneficiaries in the age-eligible population—on a set of year-specific dummy variables and a set of municipality-specific dummy variables. This yields an R^2 value of 0.7305. With almost three-quarters of the variation explained by municipality and year fixed effects, the remainder may be too weak to act as a statistically strong source of variation. This problem can be visualized in figure 5 for the 298 municipalities that are included in any of the ENCVs over 2010–13 and that are used in our analysis. With the exception of a few sudden spikes in some municipalities, which are likely to be caused by errors in the data on beneficiaries made available to us by the Consorcio, the trends over time are almost parallel in all municipalities.

[4] The National Administrative Department of Statistics of Colombia provides population projections for five-year groups. (See "Demografía y Población: Proyecciones de Población" (database), National Administrative Department of Statistics, Bogotá, Colombia) The total number of age-eligible residents per municipality has been constructed by assuming a constant age distribution within each group.

This lack of municipality- and year-specific variations can be highlighted by a simple regression analysis. Table 7 shows the result of a simple regression of the binary variable indicating receipt of Colombia Mayor—taken from the four rounds of the ENCV during 2010–13—on the proportion of age-eligible residents enrolled based on administrative data. The first column runs the regression using all age-eligible observations, and the second column only shows those observations that also fulfill the Sisben criterion (which will be the sample in our analysis below). In the first group, the municipality proportion of beneficiaries is at least a statistically significant predictor of the actual receipt of the benefit (at the 5 percent level, the standard errors are clustered on the municipality), but it ceases to be a statistically significant predictor if we focus on the actual population of interest. Moreover, even in the former group, a 1 percentage point rise in the proportion of beneficiaries would only raise the likelihood of actual receipt by 0.173 percent. Among the second group, the corresponding likelihood is only 0.09 percent.

Figure 5: Rollout of Colombia Mayor across 298 ENCV 2009–13 Municipalities

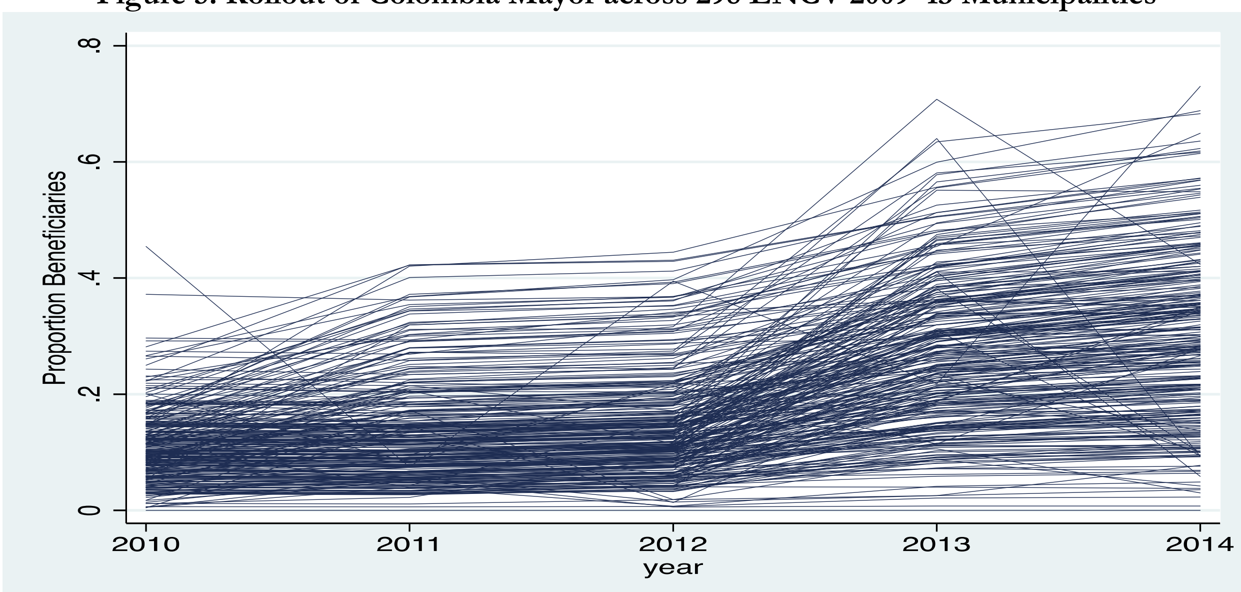


Table 7: Regression of Program Receipt on the Municipal Rollout Level, Controlling for Municipal and Year Fixed Effects

Binary dependent variable	*(1)*	*(2)*
Beneficiaries, share	0.173**	0.090
	(0.073)	(0.109)
Observations	48,271	20,172
R-squared	0.003	0.010
Municipalities, number	298	294
F	16.87	25.09

Sample	Age eligible	Age and Sisben eligible

Note: The table shows the results of a linear probability model on a dependent variable indicating the receipt of Colombia Mayor; fixed effects are omitted from the output. Standard errors are indicated in parentheses.

There is a certain contradiction in the results arising from the last two approaches. If the rollout, that is, the assigned number of beneficiaries, really evolved in a parallel fashion, one would expect to see wide variation in the prioritization score of the last person entering the program because there ought to be differences in the number and characteristics of eligible applicants. In our conversations with officials at the consorcio, we were told that the number of assigned beneficiaries is partially adjusted by the number of applicants a municipality is able to produce. This would explain the similarity in the minimum prioritization scores, but it should also result in more variation in the rollout. In our identification strategy presented in the next section, we partially assume that the official enrollment data do not properly represent actual enrollment levels, and we proceed to derive proxies for actual enrollment directly from the ENCV data.

6. Data and Empirical Strategy

We employ the 2010–13 rounds of the yearly ENCV. The principal reason we use this particular data source is that it provides us with the richest data in terms of variables, sample size, and periodicity. The other frequent large-scale and nationally representative survey is the monthly Gran Encuesta Integral de Hogares (National Integrated Household Survey). While this survey would provide us with a much larger sample size and cover almost all the municipalities in the country, it lacks the breadth of observed variables available in the ENCV. Most importantly, it does not capture whether a household receives the Colombia Mayor benefit. Furthermore, the included variables do not allow for the construction of Sisben scores.

The ENCV sampled a total of 14,801 households in 2010, 25,364 in 2011, 21,383 in 2012, and 21,565 in 2013. The sampling scheme changed in 2011 and 2012, but was held constant in 2013. The main implication of the changes in the sampling scheme is that more municipalities are repeated in the last two rounds than in the prior ones. This, however, has no bearing on the validity of our empirical approach. We restrict our sample to households that have at least one eligible member according to the age and Sisben criteria—that is, with a sufficiently low Sisben score given the place of residence—and contain either a woman of at least 52 years of age or a man of at least 57 years of age. This yields a total of 15,955 households (2,507 in 2010, 5,618 in 2011, 4,120 in 2012, and 3,710 in 2013). The total number of potential beneficiaries is 22,546. In the sample of interest, we observe a total of 294 municipalities. Of these, 124 are only observed in one round, 26 in two, 77 in three, and 67 in all four rounds. Most of the municipalities that are only observed once appear in 2011, while only one municipality that is sampled in 2012 is not observed in 2013 and vice-versa. The total number of municipalities sampled each year is 117 in 2010 (of which only 15 are never repeated in the following years), 218 in 2011, and 170 in both 2012 and 2013.

The treatment variable is binary, indicating whether or not any member in the household receives the Colombia Mayor benefit. The ENCV captures information on the receipt of government benefits only among households. This makes it impossible to determine who among the various potential beneficiaries in the same household receives the benefit. The instrumental variables (IVs) are described below. Table 8 illustrates the full set of control variables. In addition, all specifications include year- and municipality-specific fixed effects, which are omitted from the table. The first five variables (other urban, rural, age, Sisben score, and woman) are included in all specifications, while the remainder are additional control variables. The former directly determine treatment eligibility; their omission would thus result in a clear omitted variable bias.

Table 8: List of Included Control Variables

Control variable	*Description*
Other urban Rural	Categorical variables indicating two of the three geographical areas that define eligibility for the Colombia Mayor benefit. The omitted category is residence in one of the 14 major cities.
Age	Age in years
Sisben	The household's Sisben III score
Woman	A binary variable indicating gender. It is omitted from estimations by gender.
Widowed Divorced Single	Categorical variable indicating each of the three civil states. The omitted variable is married.
Potential beneficiaries, number Other adults, number Minors, number	The number of household members in each group: members old enough to qualify for Colombia Mayor; other household members 18 years of age or older; members younger than 18
Number of women	The number of women household members
Primary Secondary Postsecondary	Set of categorical variables indicating the highest attained certificate or degree. The omitted category is no completed level of education.
Incapacitated	A binary variable indicating that a person is permanently unable to work.

In light of the discussion in section 5, finding a convincing source of exogenous variation in program receipt is a daunting task. The data used consist of four pooled yearly cross-sections (2010–13) of Sisben-eligible households with at least one age-eligible member. The basic setup may be represented as follows:

$$Y_{itm} = \alpha + \beta CM_{itm} + \gamma X_{itm} + \varepsilon_m + \epsilon_t + u_{itm}, \qquad (6.1)$$

where α is the intercept, β the parameter of interest, and γ other parameters of no direct interest. The outcome is denoted by Y; CM denotes the variable of interest (living in a household with a Colombia Mayor beneficiary); and X denotes an additional control variable. The subscripts refer to individual i observed in year t and living in municipality m. The error term has three components: municipality-specific, time invariant unobserved factors (ε_m); year-specific unobserved shocks (ϵ_t); and individual specific factors (u_{itm}). We directly control the first two of these through municipality and year fixed effects so that the only error term of concern is the last one. The individual in question is not restricted to the potential beneficiary, but the effect of the program on other household members is also analyzed.

The main concern with this model is the endogeneity of the variable of interest (*CM*). This seems to arise not so much because of reverse causation (that is, being a beneficiary is partially determined by the outcome), but rather by selection into the program. It is ex ante unclear which unobserved individual- or household-specific factors determine enrollment into the program, but, to the extent that the factors also (partially) determine the outcome variable, the estimate of β will be biased. Given that other identification strategies are not available, the most common way to address this concern is to find an IV for *CM*. Because we have controlled for municipal and year fixed effects, the natural choice for the IV would be a measure of the level of program rollout in the municipality in each year. Yet, in section 5, we explain at great length that the available administrative data do not provide a valid instrument. The problem is that, according to the official data, the program was rolled out in similar increments in almost all municipalities each year so that almost all the variation in the data is accounted for by the two sets of fixed effects.

We do, however, have serious concerns about the quality of the official data because there appear to be only two types of municipalities: those that have the same increments as almost all others, and those that exhibit unreasonable jumps. While the latter are clearly errors in the data, the former seem to reflect administrative targets rather than actual enrollment levels. For this reason, we have created two additional instruments from the ENCV data. The first attempts to replicate the level of program rollout at the municipal level. It consists of a weighted average at the municipal level of all age- and Sisben-eligible individuals who actually receive the benefit in each of the four years. This provides us with an estimate of the effective year-specific level of rollout in municipalities. A second instrument can be constructed in similar fashion at a lower level of aggregation. In addition to the location in specific municipalities, we also observe whether a household resides in a municipality seat (*cabecera*), some other urban setting (defined as anything that resembles a town), or in a dispersed rural environment. We then construct the same weighted instrument for each of these subareas across all four years. More formally, our instruments are as follows:

$$IV_{tm}^{1} = \frac{1}{N_{tm}} \sum_{i \in N_{tm}} D_i \qquad (6.2)$$

$$IV_{mg}^{2} = \frac{1}{N_{mg}} \sum_{i \in N_{mg}} D_i, \qquad (6.3)$$

where the g subscript represents the specific geographical area, and D is a specific dummy variable indicating program participation. The two populations denoted by N include all age- and Sisben-eligible individuals within the respective subgroups.

Neither of the two instruments is perfect on its own, and there are differences between the two. The first is municipality and year specific. Given that we control for municipality and year fixed effects, we are concerned with potential endogeneity because of unobserved time variant factors in the municipalities. In terms of the empirical model presented above, the instrument may be correlated with an additional error term, say, ξ_{tm}. The second instrument is constant across all four years (if it were also year specific it would be highly collinear with the first instrument), but specific to an area at the submunicipal level. Any endogeneity would thus derive from unobserved time invariant characteristics that are idiosyncratic to the specific geographical group within a municipality once we have controlled for time invariant municipal characteristics. We can think of this as an additional error term, υ_{mg}. The advantage of having two instruments available for a single potentially endogenous variable lies in the possibility of testing for validity through an overidentification restrictions (OIR) test, which gives us additional confidence in our strategy.

Moreover, all specifications include municipality year and submunicipal time invariant averages of the respective outcome variable that are constructed in the same manner as the instruments. These enhance the above model by including the terms $\bar{Y}_{tm}$ and $\bar{Y}_{mg}$. This indirectly controls for the additional error terms mentioned by controlling for any possible direct effects of the instruments on the individual outcome through spatial correlations.

The two-stage least squares model could simply be run by estimating a linear probability model in each stage. While this would be consistent, it is broadly reckoned to be an inefficient approach that is likely to exhibit considerable small sample bias even in fairly large samples. For this reason, we follow a standard approach in dealing with binary endogenous variables and estimate a three-stage model. We first estimate a probit model for the first stage, including all exogenous control variables, and obtain predicted values for the probability that the treatment variable is equal to 1. Unfortunately, these predicted values cannot be used to substitute for treatment in the model of interest given their nonlinear nature. But they can be used as instruments for actual treatment, running a second first stage that again includes all exogenous control variables. This procedure has been shown to be more efficient than using the instruments directly. Wooldridge (2010) offers a detailed discussion on the procedure, which has also been used in many applied studies (for example, Adams, Almeida, and Ferreira 2009). We show the results and most statistics for this procedure. Because it reduces the number of instruments to one, we also show OIR test statistics, where appropriate, for a simple two-stage least squares model when we directly instrument for the binary endogenous variable.

Tables 9–11 show descriptive statistics for our sample of potential beneficiaries, that is, individuals who would qualify for the benefit based on their age and their household's Sisben score. These statistics represent individuals, not households. Also, all the descriptive statistics are unweighted. We have a total of 22,546 individual observations. Not shown in the tables, but of interest, is the fact that, of these, 44.6 percent are the sole potential beneficiaries in their households, while 47.1 percent live in households with another beneficiary, and 6.9 percent live with two other potential beneficiaries. (Few live in households with four or five beneficiaries.)

For our main outcome of interest—labor force participation—and our treatment variable, we show results for the various subgroups that we analyze separately. Globally, the average labor force participation rate for the population of interest is 42 percent (table 9). As would be expected, this share is higher among individuals younger than 70 years of age and lower among individuals 70 years of age or older. Also unsurprisingly, participation rates are much higher among men than among women. Our sample consists of more women than men, especially among the relatively younger group. This merely reflects the five-year lower minimum age for women to qualify for the benefit.

Table 9: Descriptive Statistics on Labor Force Participation

	Observations, number	*Mean*	*Standard deviation*	*Minimum*	*Maximum*
All	22,546	0.42	0.49	0	1
Younger than 70					
All	12,644	0.53	0.50	0	1
Male	4,805	0.79	0.41	0	1
Female	7,839	0.38	0.48	0	1
70 or older					
All	9,902	0.28	0.45	0	1
Men	4,686	0.44	0.50	0	1
Women	5,216	0.13	0.34	0	1

Globally, with respect to the treatment variable, 26 percent of the potential beneficiaries live in households that receive the Colombia Mayor benefit (table 10). Among household members under 70 years of age, the rate is 18 percent, whereas, among household members 70 years of age or older, it is 37 percent. These rates are fairly consistent between men and women.

Table 10: Descriptive Statistics on Receipt of the Colombia Mayor Benefit

Labor force participation	*Observations, number*	*Mean*	*Standard deviation*	*Minimum*	*Maximum*
All	22,546	0.26	0.44	0	1
Younger than 70					
All	12,644	0.18	0.38	0	1
Male	4,805	0.17	0.37	0	1
Female	7,839	0.18	0.39	0	1
70 or older					
All	9,902	0.37	0.48	0	1
Men	4,686	0.36	0.48	0	1
Women	5,216	0.39	0.49	0	1

For our control variables, we present numbers only for the whole sample (table 11). A few observations are lost because of missing values in the education-related variables. Most of our observations are in urban areas outside the 14 major cities (59 percent), followed by individuals living in rural settings (32 percent), which leaves a remainder of less than 9 percent who reside in one of Colombia's major cities. This low number mostly reflects the lower incidence of poverty in the big cities, but also partially arises because of the stratification of the sample. The average potential beneficiary is around 68 years old, and 58 percent are women (mainly because of the lower eligibility age of women). Around half the potential beneficiaries are married; 24 percent are widowed; and 14 percent and 11 percent, respectively, are divorced or single. The average number of household members in each group yields an average household size of around 3.7 members, of which 1.9 are females. About two-thirds of the potential beneficiaries have at least finished primary education, but the number who have secondary education or beyond is fairly low (about 9 percent and 2 percent, respectively); 13 percent are incapacitated and cannot work. By construction, the average of the IVs is almost equal to the global average of program participation.

Table 11: Descriptive Statistics of Control Variables

Variable	*Observations, number*	*Mean*	*Standard deviation*	*Minimum*	*Maximum*
Control variables					
Other urban	22,546	0.59	0.49	0	1
Rural	22,546	0.32	0.47	0	1
Age	22,546	68.42	9.73	52	108
Sisben	22,546	28.48	8.68	1.54	43.63
Female	22,546	0.58	0.49	0	1
Widowed	22,546	0.24	0.43	0	1
Divorced	22,546	0.14	0.34	0	1
Single	22,546	0.11	0.31	0	1
Potential beneficiaries, number	22,546	1.65	0.68	1	5
Other adults, number	22,546	1.00	1.15	0	10
Minors, number	22,546	1.01	1.38	0	12
Females, number	22,546	1.90	1.37	0	11
Primary	22,484	0.57	0.50	0	1
Secondary	22,484	0.09	0.28	0	1
Postsecondary	22,484	0.02	0.15	0	1
Incapacitated	22,546	0.13	0.33	0	1
Instrumental variables					
Municipality-year rollout	22,546	0.26	0.17	0	1
Submunicipal average rollout	22,546	0.26	0.17	0	1

For our more detailed analysis, we also look at a number of more granular outcomes. The ENCV has detailed questions on (a) the size of the workplace, (b) the location of work, (c) the type of (self-)employment, and (d) the sector of occupation. We are particularly interested in whether the monetary benefit plays a role in easing liquidity constraints and thus provides seed money for small-scale self-employment. For this reason, we are interested in whether, among the first group of outcomes, there is an increase in people working alone. In the second group, we have created a category consisting of work that takes places at home or work as a street vendor (either from a fixed stall or selling door to door); a second category of interest captures whether a person works in a different household; and a third category covers whether work takes place in the countryside, on a river, or on the sea. Among the types of employment, we show results on whether individuals are employed in the private sector or public sector, work independently, work their own land (including land that may be rented or sharecropped), or work without pay. We also show results on the primary sector (including related industries), manufacturing, commerce and trade, and other services. In addition, we present results on monthly labor income and number of hours worked. For the latter, no information was collected in the 2012 round so that analysis on that particular outcome is excluded. Table 12 shows the means for each of these outcomes across the various samples of interest. These are averages across all observations, not merely the economically active. It can be seen that a comparatively large share of each sample work alone on agricultural land, rivers, or the sea, as independent workers, or in the primary sector. The categories associated with agricultural or livestock activities are particularly prominent among men, who also enjoy higher incomes and work more hours. Women are more likely to work at home, as street vendors, or in the service sector. They also account for a much larger share of independent workers than men relative to overall participation rates. These averages should be kept in mind in interpreting the results presented hereafter.

Table 12: Detailed Labor Market Outcomes, Means

Indicator	*All*	*Younger than 70*			*70 or older*		
		All	*Male*	*Female*	*All*	*Male*	*Female*
Size of business							
Works alone	0.24	0.30	0.42	0.23	0.17	0.25	0.09
Place of work							
Home or street	0.13	0.16	0.14	0.18	0.08	0.09	0.08
Other homes	0.03	0.05	0.04	0.05	0.01	0.01	0.01
Land, river, or sea	0.18	0.21	0.45	0.06	0.15	0.28	0.03
Type of occupation							
Private employee	0.03	0.05	0.08	0.03	0.01	0.02	0.00
Public employee	0.01	0.02	0.01	0.02	0.00	0.00	0.00
Independent worker	0.23	0.29	0.38	0.24	0.14	0.20	0.09
Own land	0.07	0.08	0.17	0.02	0.07	0.13	0.01
Unpaid	0.02	0.02	0.01	0.02	0.01	0.01	0.01
Sector of work							
Agriculture & related	0.19	0.21	0.46	0.06	0.16	0.29	0.04
Manufacturing	0.04	0.05	0.04	0.05	0.02	0.02	0.03
Trade & commerce	0.07	0.09	0.10	0.09	0.04	0.06	0.03
Service & tourism	0.08	0.12	0.11	0.13	0.03	0.04	0.02
Other outcomes							
Labor income	139,527	193,204	304,945	124,712	70,985	126,083	21,485
Hours worked	11.82	15.42	24.69	9.75	7.21	12.10	2.83

7. Results

We start by presenting the results for the first-stage probit regressions for our most important specifications. This allows us to, first, establish the significance of the two instruments statistically as well as in terms of magnitude. Second, by including the results for all control variables, we are also able to gauge the extent of selection in the program across individuals. These results are omitted from the tables on the outcomes of interest, which only present first-stage results and other statistics on the predicted probability of treatment used as the sole instrument.

This is illustrated in table 13, which presents marginal effects evaluated at the mean (not parameter estimates). It can readily be established that both instruments are highly significant. In terms of magnitude, a 1 percentage point rise in each measure increases the probability of receiving the benefit by around 0.9–1.1 and 0.6–1.3 percentage points, respectively. Statistically, both are significant well below the 1 percent level, and, in some cases, the t-statistics are above 20. Unsurprisingly, a higher age or Sisben score has a significant effect with the expected sign. If all else is equal, women are approximately 4 percent more likely to be beneficiaries than men. The positive effect of the higher number of potential beneficiaries may reflect only the greater probability that at least one household member receives the benefit (treatment is only observed at the household level). However, if more household members of any other kind receive the benefit, this also has a positive effect. Divorce lowers the probability of benefit receipt. The higher the level of educational attainment, the lower the probability of benefit receipt. Incapacitation for work raises the probability of receiving the benefit, but this effect is only statistically significant among comparatively younger individuals (under 70 years of age) and men. Overall, the results strongly suggest that selection into treatment is important at the individual level, and it can therefore not be safely assumed that other, unobserved characteristics could act as omitted variables.

Table 13: Regression of the Treatment Variable on Instruments and Exogenous Control Variables

Variable	*(1) All*	*(2) All*	*(3) < 70 All*	*(4) >= 70 All*	*(5) <70 Males*	*(6) <70 Females*
Instrument by year	0.907***	0.890***	0.643***	1.155***	0.924***	0.887***
	(0.034)	(0.036)	(0.035)	(0.075)	(0.050)	(0.042)
Instrument by area	0.951***	0.932***	0.612***	1.287***	0.916***	0.954***
	(0.026)	(0.028)	(0.032)	(0.053)	(0.041)	(0.038)
Other urban	0.009	0.009	0.010	0.035	0.021	0.004
	(0.028)	(0.024)	(0.016)	(0.040)	(0.031)	(0.034)
Rural	−0.022	−0.033	−0.022	−0.013	−0.028	−0.029
	(0.028)	(0.024)	(0.018)	(0.042)	(0.032)	(0.033)
Age	0.011***	0.009***	0.012***	0.004***	0.010***	0.009***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)
Female	0.043***	0.037***	0.054***	0.024*		
	(0.006)	(0.006)	(0.006)	(0.013)		
Sisben	−0.005***	−0.005***	−0.003***	−0.006***	−0.006***	−0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Potential beneficiaries, number		0.049***	0.067***	0.031*	0.062***	0.039***
		(0.013)	(0.016)	(0.019)	(0.020)	(0.014)
Other adults, number		0.047***	0.063***	0.027	0.071***	0.025
		(0.014)	(0.015)	(0.026)	(0.020)	(0.017)
Minors, number		0.070***	0.054***	0.070***	0.086***	0.058***
		(0.016)	(0.016)	(0.024)	(0.021)	(0.018)
Females, number		0.085***	0.116***	0.036***	0.100***	0.077***
		(0.009)	(0.007)	(0.013)	(0.012)	(0.010)
Widowed		0.003	0.004	−0.006	−0.005	0.007
		(0.005)	(0.004)	(0.008)	(0.006)	(0.005)
Divorced		−0.031***	−0.018***	−0.034***	−0.033***	−0.032***
		(0.006)	(0.005)	(0.009)	(0.008)	(0.006)
Single		0.005	0.002	0.010	0.009	0.007
		(0.006)	(0.005)	(0.009)	(0.009)	(0.007)
Primary		−0.034***	−0.014*	−0.057***	−0.030***	−0.039***
		(0.007)	(0.008)	(0.013)	(0.011)	(0.010)
Secondary		−0.096***	−0.042***	−0.207***	−0.052**	−0.125***
		(0.012)	(0.011)	(0.024)	(0.023)	(0.012)
Postsecondary		−0.156***	−0.087***	−0.312***	−0.166***	−0.150***
		(0.012)	(0.011)	(0.025)	(0.016)	(0.019)
Incapacitated		0.008	0.039**	0.022	0.028*	−0.013
		(0.009)	(0.016)	(0.015)	(0.014)	(0.013)
Observations	22,358	22,297	12,093	9,778	9,298	12,892

Note: The table illustrates a linear probability model on dependent variables indicating receipt of the Colombia Mayor benefit. Standard errors are shown in parentheses.

Moving on to our actual results, we start by presenting the results for the total labor force participation of potential beneficiaries. Table 14 has eight columns. The first four show the ordinary least squares (OLS) regression with municipality and year fixed effects, and the second four present the IV results.

The first column in each groups presents the results for a parsimonious specification that includes only a binary variable for the geographical area (other urban and rural), age, the Sisben score, and gender as control variables. These are included because they have a direct effect on Sisben eligibility and therefore act as omitted variables. The next column includes a full set of control variables. A comparison of the first two columns gives, above all, an idea of the extent to which the additional controls act as omitted variables in the first specification. This can be seen as an (imperfect) exogeneity test on the treatment variable.

Table 14: Results for Labor Force Participation, Entire Sample

Variable	*(1) OLS All*	*(2) OLS All*	*(3) OLS Males*	*(4) OLS Females*	*(5) IV All*	*(6) IV All*	*(7) IV Males*	*(8) IV Females*
Colombia Mayor	−0.018***	−0.007	0.004	−0.003	0.069***	0.090***	0.116***	0.076***
	(0.006)	(0.006)	(0.010)	(0.008)	(0.017)	(0.017)	(0.027)	(0.028)
Dependent variable by year	0.629***	0.571***	0.469***	0.636***	0.625***	0.565***	0.472***	0.627***
	(0.033)	(0.031)	(0.052)	(0.045)	(0.035)	(0.033)	(0.053)	(0.047)
Dependent variable by area	0.730***	0.679***	0.598***	0.668***	0.739***	0.691***	0.614***	0.669***
	(0.024)	(0.025)	(0.045)	(0.050)	(0.024)	(0.025)	(0.047)	(0.051)
Observations	22,546	22,484	9,472	13,007	22,358	22,297	9,298	12,892
R-squared	0.259	0.327	0.415	0.163	0.253	0.321	0.407	0.157
Municipalities, number	296	296	293	294	286	286	273	283
F	915.7	453.7	547.7	120.3	865.8	408.4	512.5	114.1
Partial R-squared					0.0886	0.0957	0.102	0.0951
Kleinbergen-Paap statistic					2,679	2,495	1,346	1,650
OIR test					0.358	0.358	0.786	0.0929
Endogeneity test					0.000	0.000	0.000	0.00470
First stage								
Dependent variable by year					0.002	−0.000	0.006	−0.002
					(0.028)	(0.030)	(0.044)	(0.041)
Dependent variable by area					−0.000	−0.001	−0.001	−0.004
					(0.021)	(0.023)	(0.040)	(0.035)
Instrument from probit					0.977***	0.993***	0.989***	0.993***
					(0.019)	(0.020)	(0.027)	(0.024)

Note: The table illustrates a linear probability model on dependent variables indicating participation in the labor force. The IV specification relies on a three-stage procedure for binary treatment variables. Only the OIR test is derived from a two-step procedure. Columns (1) and (5) do not include additional control variables. Standard errors are shown in parenthesis.

The bottom of this table and of the following tables includes a number of additional statistics. While those shown only under the OLS specification (number of observations, R^2, number of municipalities, and the F-test statistic for joint significance) should be self-explanatory, the statistics in the IV specifications warrant additional explanation. The first two—partial R^2 (Shea 1997) and the Kleinbergen-Paap statistics (Stock, Wright, and Yogo 2002)—are weak instrument tests. Simply put, in the case of a single endogenous regressor and no control variables, the partial R^2 is reduced to the standard R^2, and the Kleinbergen-Paap is reduced to the F-test in the first-stage regression. They thus provide measures of the magnitude and statistical significance, respectively, of the effect of the instrument on the endogenous variable. It can be shown that a higher partial R^2 results in a lower asymptotic bias in the IV estimator relative to the OLS if the exclusion restriction is violated. The Kleinbergen-Paap statistics are a panel data variant of the Cragg-Donald statistic, which has been introduced as a weak instrument test by Stock, Wright, and Yogo (2002). Critical values for different tests can be derived for the Kleinbergen-Paap statistic, the most conservative of which for our case has values of around 20. Next, the OIR test allows testing for the validity of the exclusion restrictions if the endogenous variables are overidentified (which is why we wanted to have two instruments in the first place). The null hypothesis is that the exclusion restrictions are met, that is, the instruments are valid. Although the test is regarded as low powered, it provides a useful check on the identification strategy. While all the other IV-related test statistics refer to a first stage in which the sole instrument is derived from the probit model, the OIR test is presented for a linear first stage using the two IVs directly. Lastly, under the assumption that we have a set of valid instruments, we can test whether or not the variable we instrumented for is indeed endogenous. The null hypothesis here is the exogeneity of that variable.

Starting with table 15, we look at the effect of the program on the labor force participation of beneficiaries. The important result in the first two columns is that the inclusion of the additional control variables reduces the estimated effect of program participation by more than one-half and reduces the significance level from 1 percent to insignificance. This is a strong indicator that the treatment variable is endogenous and that the OLS results are likely biased. The IV results, in contrast, are highly significant and have a positive sign. They imply that program participation has the effect of boosting labor force participation or, probably more likely, reducing the retreat of beneficiaries from the labor force. The likely implication of these results is that, while individuals who are not in the labor force are more likely to receive the benefit in the first place, the actual effect of the program is that beneficiaries tend to join the labor force. The OIR test p-values are not close to rejection of the H_0, except in the case of women, and the exogeneity of the treatment variable can be rejected.

We now analyze the counterintuitive result of an overall positive effect on the labor force participation of beneficiaries. We start by dividing the sample into two age-groups: beneficiaries who can be expected to be active in the labor market and beneficiaries who have probably dropped out of the labor market because of old age. We have decided to make the cutoff at age 70. We then divide each age-group also by gender. The first three columns of table 15 present the results for the relatively younger cohort. The overall result is still positive, but only statistically significant among men. The point estimate is much smaller among women and, hence, not significant. (The standard errors are the same.) The next three columns show the corresponding results for individuals 70 years of age or older. All the results are much smaller in magnitude—in the case of women, they have a negative sign—and statistically insignificant. (Among men, the result is similar to the result among younger women.) This establishes that the positive effect on total labor force participation is driven by beneficiaries younger than 70, who, based on their age, can be expected to be still economically active, and, in particular, by men.

Table 15: Results on Labor Force Participation, by Age-Group

Variable		*< 70 years*			*≥ 70 years*	
	All	*Males*	*Females*	*All*	*Men*	*Women*
Colombia Mayor	0.091***	0.108**	0.066	0.008	0.060	−0.021
	(0.032)	(0.044)	(0.044)	(0.022)	(0.040)	(0.032)
Dependent variable by year	0.646***	0.313***	0.821***	0.429***	0.492***	0.349***
	(0.046)	(0.079)	(0.071)	(0.056)	(0.085)	(0.061)
Dependent variable by area	0.713***	0.501***	0.792***	0.616***	0.676***	0.385***
	(0.049)	(0.064)	(0.079)	(0.045)	(0.077)	(0.066)
Observations	12,093	3,977	7,356	9,778	4,530	5,042
R-squared	0.300	0.333	0.123	0.287	0.342	0.070
Municipalities, number	270	214	254	280	256	262
F	236.0	446.7	80.55	103.8	105.2	18.35
Partial R-squared	0.105	0.119	0.102	0.0945	0.0976	0.0917
Kleinbergen-Paap statistic	1,045	624.9	665.0	1024	495.9	721.1
OIR test	0.927	0.679	0.428	0.469	0.966	0.448
Endogeneity test	0.00702	0.0384	0.212	0.383	0.104	0.810
First stage						
Dependent variable by year	0.001	0.005	−0.004	0.003	0.001	0.002
	(0.042)	(0.074)	(0.052)	(0.053)	(0.070)	(0.079)
Dependent variable by area	0.004	0.008	0.002	−0.001	−0.001	−0.007
	(0.037)	(0.060)	(0.050)	(0.048)	(0.069)	(0.087)
Instrument from probit	1.011***	1.015***	1.009***	0.987***	0.981***	0.983***
	(0.031)	(0.041)	(0.039)	(0.031)	(0.044)	(0.037)

Note: The table illustrates a linear probability model relying on a three-stage procedure for binary treatment variables on the dependent variable indicating participation in the labor force. Only the OIR test is derived from a two-step procedure. All specifications include a complete set of control variables. Standard errors are shown in parenthesis.

To understand the dynamics that drive the positive effect of the benefit on labor force participation, we have to delve deeper and take a more granular look at the kind of economic activities that are particularly affected. In table 16, we explore the results for two age-groups and for gender and location-specific subgroups within each of these. The method employed is the same as above, that is, we have estimated a three-stage linear probability model for the binary outcome. One could argue that a multinomial model would be more appropriate; yet, no such models for IV estimation are available.

At the bottom of the table, we also present results for monthly labor income and hours worked. These are estimated by a simple linear regression with values equal to zero for individuals not in the labor force. Although we admit that a Tobit or Heckman selection model may be more appropriate, as with the multinomial models, no IV techniques exist for these methods. Also, as mentioned above, for hours worked, no information is available from the 2012 round, reducing the sample by more than 25 percent. With these caveats in mind, we still believe that these result have important implications.

Table 16: Results for the Treatment Effect of Program Receipt on Various Labor Force Outcomes

Variable		*< 70 years*			*≥ 70 years*	
	All	*Males*	*Females*	*All*	*Males*	*Females*
Size of business						
Works alone	0.038 (0.030)	0.138** (0.056)	−0.017 (0.039)	0.023 (0.023)	0.080** (0.040)	−0.007 (0.028)
Place of work						
Home or street	0.020 (0.022)	0.025 (0.042)	−0.007 (0.034)	0.001 (0.016)	−0.027 (0.032)	0.018 (0.024)
Other homes	−0.002 (0.011)	−0.009 (0.019)	0.010 (0.016)	−0.008 (0.007)	−0.010 (0.011)	−0.009 (0.011)
Land, river, or sea	0.042* (0.023)	0.093* (0.048)	0.029 (0.026)	−0.003 (0.019)	0.071* (0.038)	−0.031** (0.015)
Type of occupation						
Private employee	0.027** (0.010)	0.014 (0.023)	0.040*** (0.013)	−0.001 (0.008)	−0.006 (0.015)	−0.000 (0.006)
Public employee	0.006 (0.007)	0.000 (0.009)	0.009 (0.010)	0.002 (0.002)	0.003 (0.004)	0.000 (0.001)
Independent worker	0.060** (0.029)	0.147** (0.059)	−0.025 (0.039)	0.019 (0.023)	0.067 (0.046)	−0.020 (0.026)
Own land	−0.002 (0.015)	0.065** (0.033)	−0.020 (0.016)	0.007 (0.013)	0.020 (0.030)	0.011 (0.010)
Unpaid	0.012 (0.008)	0.000 (0.015)	0.017 (0.011)	0.001 (0.006)	0.003 (0.011)	−0.003 (0.008)
Sector of work						
Agriculture & related	0.038 (0.024)	0.057 (0.049)	0.039 (0.027)	−0.009 (0.020)	0.060 (0.038)	−0.029* (0.017)
Manufacturing	0.029** (0.012)	0.017 (0.018)	0.034* (0.018)	−0.010 (0.010)	−0.029** (0.014)	−0.001 (0.013)
Trade & commerce	0.004 (0.018)	0.032 (0.033)	−0.046* (0.025)	0.021* (0.012)	0.037* (0.022)	0.004 (0.017)
Service & tourism	0.031* (0.016)	0.045 (0.029)	0.024 (0.026)	0.003 (0.010)	0.007 (0.018)	−0.004 (0.016)
Other outcomes						
Labor income	137,327.628*** (38,036.743)	152,043.474** (61,568.069)	89,104.746** (42,852.815)	−28,262.658 (18,488.245)	−31,913.834 (32,228.232)	−20,480.467* (10,770.416)
Hours worked	4.474*** (1.416)	7.416*** (2.369)	1.029 (1.682)	−0.943 (0.858)	−0.957 (1.764)	−0.618 (0.861)

Note: Only parameters on treatment variable shown. Linear probability model with three-stage procedure for binary treatment variables on respective dependent variable. Only the OIR test is derived from two-step procedure. For "Other Outcomes" dependent variables are continuous. All specifications include complete set of control variables and full set of fixed effects. Standard error in parenthesis.

In the first three columns of table 16, we show the results for potential beneficiaries younger than 70. The most important results can be found in column 2 for males. We find positive and statistically significant results for working alone, for working on agricultural land, rivers, or the sea (even if only at the 10 percent significance level), and for working independently or working on one's own land. In terms of magnitude, we find that such males are 13.8 percentage points more likely to work alone, 9.3 percentage points more likely to work on agricultural land, 14.7 percentage points more likely to work independently, and 6.5 percentage points more likely to work on their own land. None of the results on the four sectors are statistically significant, which is probably because each sector includes many different working arrangements. However, the highest point estimate is found in the agricultural sector, followed by services. We also find that the benefit is estimated to increase labor income by around Col$150,000 (around US$50) among this group and that the number of hours worked rose by more than seven hours. While we find an expansion in labor force participation in these categories, there seems to be no reduction in other categories, such as private sector employment. This implies that the benefit transfer does not have the effect of shifting the economically active from safe to more risky occupations. The results suggest, rather, that the transfer enables some beneficiaries to become economically active. It would, of course, be of interest to learn whether these labor market entrants only work a few hours, but the lack of proper panel data makes this infeasible.

For females, we find no such effect, and the picture is different. The only strongly statistically significant effect (at the 1 percent level) can be found in employment in the private sector. The magnitude of this effect, at 4 percentage points, has to be set beside the overall mean of 3 percent presented above; it is comparatively large and represents the lion's share of the overall 6.6 percentage point increase in labor force participation presented above. Moreover, there is a small effect through the reduction of work in commerce and trade and the increase in work in manufacturing.

The other three columns in table 16 show the corresponding results among individuals 70 years of age or older. Some parallels exist with the previous results. In particular, we still find a positive effect among men in the probability of working alone (8 percentage points) and working on agricultural land, rivers, or the sea (7.1 percentage points). Men are also 3.7 percentage points more likely to work in commerce and trade, which is significant at the 10 percent level. These results are pretty much in line with our prior findings, albeit weaker. This indicates that the same forces continue to be exerted even as men age. The other statistically significant results are all negative, indicating that, in this age-group, the benefit may have the expected effect despite its small amount. Men are working less in manufacturing, while women are moving out of the primary sector and are working less on agricultural land. Labor income is declining, though mostly this effect is not statistically significant, and the same is true for the number of hours worked. However, the decline in labor income is smaller than the amount of the benefit.

Table 17: Results of Labor Force Participation among Other Adults Living in a Household with a Potential Beneficiary

Variable	*(1) OLS All*	*(2) OLS All*	*(3) OLS Males*	*(4) OLS Females*	*(5) IV All*	*(6) IV All*	*(7) IV Males*	*(8) IV Females*
Colombia Mayor	0.029***	0.035***	0.019***	0.052***	0.017	0.032	0.007	0.094*
	(0.008)	(0.007)	(0.007)	(0.014)	(0.020)	(0.026)	(0.024)	(0.055)
Dependent variable by year	0.705***	0.570***	0.276***	0.812***	0.714***	0.575***	0.301***	0.806***
	(0.028)	(0.031)	(0.040)	(0.055)	(0.029)	(0.032)	(0.041)	(0.058)
Dependent variable by area	0.769***	0.653***	0.322***	0.925***	0.767***	0.650***	0.310***	0.926***
	(0.024)	(0.027)	(0.040)	(0.059)	(0.024)	(0.028)	(0.042)	(0.062)
Observations	16,563	15,145	7,898	7,233	16,171	14,707	7,502	6,771
R-squared	0.188	0.327	0.384	0.172	0.187	0.326	0.381	0.167
Municipalities, number	292	292	283	284	264	262	237	243
F	343.5	478.7	1549	152.0	335.3	459.5	1456	138.2
Partial R-squared					0.0595	0.0670	0.0753	0.0599
Kleinbergen-Paap statistic					431.3	467.7	342.3	247.1
OIR test					0.996	0.996	0.0281	0.0330
Endogeneity test					0.557	0.919	0.597	0.440
First stage								
Dependent variable by year					0.005	0.005	0.008	−0.002
					(0.049)	(0.048)	(0.064)	(0.062)
Dependent variable by area					0.003	−0.004	−0.011	0.006
					(0.047)	(0.047)	(0.071)	(0.060)
Instrument from probit					0.961***	0.990***	1.007***	0.967***
					(0.046)	(0.046)	(0.054)	(0.062)

Note: The table illustrates a linear probability model on a dependent variable indicating participation in the labor force. The IV specification uses a three-stage procedure for binary treatment variables. Only the OIR test is derived from a two-step procedure. Columns (1) and (5) do not include additional control variables. Standard errors are shown in parenthesis.

It is also of interest to take a quick look at the situation among other household members. Table 17 shows the results for other adult household members. The potential for the endogeneity of treatment here is far smaller because we are not looking at the actual beneficiary, but at someone else. Yet, it cannot be completely ruled out that unobserved household-level characteristics act as omitted variables. The effect is positive and statistically significant for all four OLS estimations. The inclusion of the additional control variables has only a marginal effect on our estimates. While statistically insignificant, the point estimates on the full sample are only slightly lower under the IV estimation. The lack of statistical significance can be explained by larger standard errors. However, there is some concern about instrument validity, as shown by the OIR test, once the sample is divided according to gender. Overall, these results indicate that the program had a positive effect on other adults in the household, similar to its effect on the actual beneficiaries.

All the results on labor force participation among 13–17-year-olds, shown in table 18, can be dealt with quickly. There is not a hint of evidence on any effect because all the results are statistically insignificant and small in magnitude. All the IV test statistics are strong, making us more confident in the absence of an effect. However, the sample size is somewhat reduced given the higher incidence of municipalities with no variation in the outcome among this subgroup. This is so because of the smaller number of minors in our sample (and, hence, the fewer observations in each municipality) and the high incidence of zeros in the outcome variable because most minors do not work.

Table 18: Labor Force Participation, 13–17-Year-Olds in Households with a Potential Beneficiary

Variable	*(1) OLS All*	*(2) OLS All*	*(3) OLS Males*	*(4) OLS Females*	*(5) IV All*	*(6) IV All*	*(7) IV Males*	*(8) IV Females*
Colombia Mayor	0.007	0.004	0.012	−0.004	0.032	0.031	0.062	0.055
	(0.013)	(0.013)	(0.023)	(0.016)	(0.028)	(0.028)	(0.048)	(0.045)
Dependent variable by year	0.753***	0.755***	0.854***	0.565***	0.750***	0.751***	0.858***	0.593***
	(0.033)	(0.033)	(0.053)	(0.068)	(0.037)	(0.038)	(0.065)	(0.076)
Dependent variable by area	0.759***	0.753***	0.914***	0.446***	0.750***	0.746***	0.901***	0.528***
	(0.028)	(0.028)	(0.049)	(0.060)	(0.031	(0.031)	(0.059)	(0.066)
Observations	5,870	5,866	3,074	2,767	5,175	5,172	2,348	2,209
R-squared	0.207	0.210	0.250	0.104	0.201	0.204	0.240	0.102
Municipalities, number	280	280	266	261	214	214	169	157
F	221.8	121.1	65.50	10.30	173.5	95.64	52.06	10.63
Partial R-squared					0.0575	0.0675	0.0855	0.0660
Kleinbergen-Paap statistic					189.9	241.5	190.3	123.9
OIR test					0.697	0.540	0.429	0.397
Endogeneity test					0.385	0.360	0.259	0.190
First stage								
Dependent variable by year					0.002	−0.004	0.005	−0.006
					(0.061)	(0.058)	(0.079)	(0.116)
Dependent variable by area					0.002	−0.001	−0.003	−0.004
					(0.065)	(0.065)	(0.100)	(0.100)
Instrument from probit					0.975***	0.999***	1.044***	1.039***
					(0.071)	(0.064)	(0.076)	(0.093)

Note: The table illustrates a linear probability model on a dependent variable indicating participation in the labor force. The IV specification uses a three-stage procedure for binary treatment variables. Only the OIR test is derived from a two-step procedure. Columns (1) and (5) do not include additional control variables. Standard errors are shown in parenthesis.

We are also interested in whether the increase in labor force participation among the elderly in response to the benefit reduces other activities, in particular household-related work. In table 19, we present the results for our model with the binary dependent variable indicating that potential beneficiaries declared that household-related work was their primary activity during the week prior to the interview. This outcome is not mutually exclusive with labor force participation; the latter is still the case if the respondent had worked for at least an hour or had been actively looking for work. Nonetheless, the results are almost a mirror image of the results on labor force participation. We find a statistically significant positive effect in the OLS, but a negative one in the IV results, though the latter are not statistically significant among the gender-specific samples and are associated with generally lower point estimates, which is to be expected if some respondents spend only a few hours a week on paid work. Overall, these results are in line with our principal findings.

Table 19: Household Work as the Primary Occupation among Potential Beneficiaries

Variable	*(1)* *OLS* *All*	*(2)* *OLS* *All*	*(3)* *OLS* *Males*	*(4)* *OLS* *Females*	*(5)* *IV* *All*	*(6)* *IV* *All*	*(7)* *IV* *Males*	*(8)* *IV* *Females*
Colombia Mayor	0.017**	0.025***	0.017**	0.017*	−0.059***	−0.041**	−0.026	−0.043
	(0.007)	(0.006)	(0.008)	(0.009)	(0.020)	(0.019)	(0.026)	(0.028)
Dependent variable by year	0.732***	0.590***	0.484***	0.617***	0.735***	0.588***	0.477***	0.619***
	(0.026)	(0.026)	(0.044)	(0.041)	(0.027)	(0.027)	(0.042)	(0.042)
Dependent variable by area	0.685***	0.592***	0.498***	0.548***	0.686***	0.597***	0.496***	0.553***
	(0.030)	(0.029)	(0.042)	(0.048)	(0.031)	(0.029)	(0.044)	(0.048)
Observations	22,546	22,484	9,472	13,007	22,358	22,297	9,298	12,892
R-squared	0.316	0.426	0.100	0.322	0.312	0.423	0.095	0.318
Municipalities, number	296	296	293	294	286	286	273	283
F	561.8	745.9	30.64	773.5	556.6	729.8	31.29	756.7
Partial R-squared					0.0890	0.0961	0.102	0.0957
Kleinbergen-Paap statistic					2705	2493	1370	1635
OIR test					0.657	0.657	0.657	0.529
Endogeneity test					0.000136	0.000317	0.0877	0.0266
First stage								
Dependent variable by year					0.005	0.006	0.003	0.003
					(0.028)	(0.031)	(0.046)	(0.036)
Dependent variable by area					0.005	0.005	0.006	0.005
					(0.027)	(0.028)	(0.044)	(0.040)
Instrument by year					0.977***	0.993***	0.989***	0.994***
					(0.019)	(0.020)	(0.027)	(0.025)

Note: The table illustrates a linear probability model on a dependent variable indicating household work as a primary occupation. The IV specification uses a three-stage procedure for binary treatment variables. Only the OIR test is derived from a two-step procedure. Columns (1) and (5) do not include additional control variables. Standard errors are shown in parenthesis.

A large part of the literature on noncontributory pensions concerns the question of whether such a benefit crowds out the receipt of transfers from other family members or close friends. In table 20, we show the results for our standard estimation on the binary outcome of whether a potential beneficiary receives any monetary transfers from other households or organizations such as nongovernmental organizations or churches. A comparison of the OLS and the IV results shows a clear selection. The elderly who receive such third-party transfers are more likely also to receive the Colombia Mayor benefit. The IV results show a mostly negative effect, which is, however, small in magnitude and statistically insignificant. While small, it is in line with other research showing that the negative point estimate is driven by women, rather than men beneficiaries. Taken together, this probably indicates that the third parties that help the elderly financially also help them by informing them about the Colombia Mayor benefit and, perhaps, assist in the application process. Yet, the third parties do not reduce their own transfers.

Table 20: The Receipt of Monetary Transfers from Third Parties to Potential Beneficiaries

Variable	*(1) OLS All*	*(2) OLS All*	*(3) OLS Males*	*(4) OLS Females*	*(5) IV All*	*(6) IV All*	*(7) IV Males*	*(8) IV Females*
Colombia Mayor	0.043***	0.050***	0.055***	0.044***	−0.012	−0.014	0.005	−0.017
	(0.008)	(0.008	(0.012)	(0.010)	(0.012)	(0.014)	(0.027)	(0.023)
Dependent variable by year	0.804***	0.765***	0.657***	0.824***	0.802***	0.765***	0.665***	0.816***
	(0.030)	(0.031	(0.044)	(0.046)	(0.030)	(0.032)	(0.044)	(0.046)
Dependent variable by area	0.853***	0.810***	0.766***	0.801***	0.855***	0.814***	0.772***	0.800***
	(0.022)	(0.024)	(0.049)	(0.044)	(0.022)	(0.024)	(0.051)	(0.044)
Observations	22,546	22,484	9,472	13,007	22,358	22,297	9,298	12,892
R-squared	0.076	0.105	0.105	0.106	0.072	0.100	0.102	0.102
Municipalities, number	296	296	293	294	286	286	273	283
F	209.6	117.9	44.97	87.48	201.1	109.5	43.04	80.22
Partial R-squared					0.0889	0.0961	0.103	0.0950
Kleinbergen-Paap statistic					2683	2495	1407	1553
OIR test					0.966	0.966	0.311	0.160
Endogeneity test					0.000122	0.000	0.0525	0.00920
First stage								
Dependent variable by year					−0.005	−0.003	0.003	−0.005
					(0.027)	(0.028)	(0.038)	(0.034)
Dependent variable by area					0.005	0.005	0.011	0.002
					(0.024)	(0.026)	(0.045)	(0.040)
Instrument from probit					0.976***	0.992***	0.988***	0.994***
					(0.019)	(0.020)	(0.026)	(0.025)

Note: The table illustrates a linear probability model on a dependent variable indicating receipt of monetary transfers from third parties. The IV specification uses a three-stage procedure for binary treatment variables. Only the OIR test is derived from a two-step procedure. Columns (1) and (5) do not include additional control variables. Standard errors are shown in parenthesis.

Another question of interest revolves around whether the benefit changes the living arrangements of the elderly, in particular whether they are either more or less likely to live with other family members in multigenerational households. If this is the case, the results on labor force participation may be indirectly driven by the program's effect on household composition. In table 21, we thus show the results for the binary dependent variable of whether a potential beneficiary lives in a household with at least one adult below the program eligibility age. Under the OLS, the effect is negative and highly statistically significant. However, this is most likely because elderly people who live with younger household members are less likely to enroll in the program. In the IV specification, point estimates turn positive, but are small in magnitude and statistically insignificant. We therefore rule out that household composition may be the driver behind our prior results.

Table 21: Living with Another Adult below the Eligibility Age of Potential Beneficiaries

Variable	*(1)* *OLS* *All*	*(2)* *OLS* *All*	*(3)* *OLS* *Males*	*(4)* *OLS* *Females*	*(5)* *IV* *All*	*(6)* *IV* *All*	*(7)* *IV* *Males*	*(8)* *IV* *Females*
Colombia Mayor	−0.052***	−0.050***	−0.053***	−0.043***	0.030*	0.029	0.030	0.023
	(0.011)	(0.011)	(0.013)	(0.012)	(0.017)	(0.018)	(0.030)	(0.025)
Dependent variable by year	0.783***	0.772***	0.780***	0.777***	0.786***	0.775***	0.789***	0.775***
	(0.029)	(0.029)	(0.042)	(0.037)	(0.029)	(0.030)	(0.042)	(0.038)
Dependent variable by area	0.899***	0.887***	0.924***	0.848***	0.905***	0.893***	0.928***	0.854***
	(0.018)	(0.018)	(0.028)	(0.028)	(0.018)	(0.018)	(0.030)	(0.028)
Observations	22,546	22,484	9,472	13,007	22,358	22,297	9,298	12,892
R-squared	0.072	0.089	0.118	0.077	0.066	0.083	0.112	0.073
Municipalities, number	296	296	293	294	286	286	273	283
F	328.6	216.1	140.6	92.54	351.2	226.1	138.7	88.22
Partial R-squared					0.0889	0.0914	0.0956	0.0918
Kleinbergen-Paap statistic					2621	2689	1330	1626
OIR test					0.670	0.670	0.265	0.929
Endogeneity test					0.000	0.000	0.00564	0.00987
First stage								
Dependent variable by year					0.008	0.006	0.009	(0.004
					(0.024)	(0.024)	(0.032)	(0.031)
Dependent variable by area					−0.006	−0.006	−0.009	−0.006
					(0.017)	(0.017)	(0.029)	(0.027)
Instrument from probit					0.976***	0.983***	0.979***	0.990***
					(0.019)	(0.019)	(0.027)	(0.025)

Note: The table illustrates a linear probability model on a dependent variable indicating that an adult below eligibility age lives in the same household. The IV specification uses a three-stage procedure for binary treatment variables. Only the OIR test is derived from a two-step procedure. Columns (1) and (5) do not include additional control variables. Standard errors are shown in parenthesis.

8. Conclusion

The literature on the labor market effects of noncontributory pensions has so far almost exclusively focused on income effects, showing that they incentivize beneficiaries to leave the labor force or reduce the number of hours they work. Our study offers a different perspective. Because noncontributory pensions are essentially a nonconditional cash transfer to the elderly, their effects should, overall, be much more nuanced. In particular, a reliable, even if small, additional income stream could allow beneficiaries to engage in economic activities that require some up-front investment or that are generally more risky. The Colombia Mayor Program provides the ideal scenario to test these hypotheses, given the small amount of the associated benefit, which would imply a small income effect, and the low age of eligibility. Using IV techniques, we find that the benefit increased labor force participation among men, especially men in their 60s. This effect is particularly pronounced in occupations that require working alone as independent workers or in the cultivation of agricultural land over which the beneficiaries have tenure rights. These results support the idea that liquidity constraints prevented some beneficiaries from engaging in these economic activities. However, we could not find any evidence for a shift from less risky to more risky activities. Moreover, we could find no such results among women. Among men age 70 or older, some of these results persist, albeit at a much lower magnitude.

Our findings have two important implications. First, they indicate that studies demonstrating support for the negative aggregate effect of noncontributory pensions on beneficiary labor force participation may confound two separate effects. Second, they add another outcome to the still understudied area of the potential of cash transfer programs to ease liquidity constraints and increase economic activity. The last point warrants more attention than it has thus far received.

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