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

Tobias Pfutze[1]
Florida International University

Carlos Rodríguez-Castelán[2]
The World Bank

Abstract

One of the primary motivations behind the establishment of noncontributory pension programs is to allow beneficiaries to retire from the labor force. Yet, their aggregate effects may be more complex. One such program, Colombia Mayor, stands out for its low eligibility age. Given that beneficiaries are not required to leave the labor force, it practically constitutes a fixed income—an unconditional cash transfer of sorts—to a still economically-active population. Using panel data and instrumental variable techniques, this paper shows that the effect of this program has been to raise the labor force participation of relatively younger, particularly male, beneficiaries. This increase occurred precisely in the occupations with characteristics that are likely to require some up-front investment and for the comparatively poorer. We conclude that the transfer effectively loosened the liquidity constraints to remaining in these occupations. No such effect is found for older beneficiaries.

Keywords: Pensions, Labor Force Participation, Colombia

JEL Codes: H55, J08, J26, O15

The authors would like to thank Carlos Casteñeda, Sara Kotb, and Daniel Valderrama for helpful research assistance and also the participants at the 2015 Summer Initiative for Research on Poverty, Inequality, and Gender of the Poverty Global Practice at the World Bank for their useful comments and suggestions. The authors would also like to thank Juan Carlos López and Sandra Díaz at the Colombia Mayor Implementation Agency (Consorcio Colombia Mayor), and Alejandra Corchuelo and Guillermo Rivas from the Ministry of Planning (Departamento Nacional de Planeación, DNP) for providing useful data employed for some the analysis.

[1] Department of Economics, Florida International University, 11200 8th Street, Miami, FL 33199, United States, Phone: (305) 348-2318, Email: tpfutze@fiu.edu

[2] World Bank, 701 18th Street NW, Mail Stop J9-908, Washington, DC 20006, United States, Phone: (202) 473-9800, Email: crodriguezc@worldbank.org

1. Introduction

Noncontributory pensions are becoming increasingly popular within the larger context of social protection systems. The 'noncontributory' label highlights the fact that these pensions are not mainly funded by individuals' payroll contributions—as in the case of traditional social insurance—but through other general fiscal revenues. Yet, as it has been pointed out, this term is somewhat imprecise as most beneficiaries of non-contributory pension *have contributed* to the economy (Barrientos and Sherlock, 2002)—including and not limited to by paying indirect taxes. Non-contributory pensions can also be thought of (perhaps more accurately) as de facto unconditional cash transfers for old age. Indeed, noncontributory pensions account for a major share of non-conditional transfers. In Latin America alone, 12 out 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 can 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 has been criticized (Bosch and Guajardo, 2012; Johnson and Williamson, 2006; Rofman, Apella, and Vezza, 2015). Yet, unconditional cash transfers are increasingly garnering attention. After the grand entrance of conditional cash transfer programs in Latin America in the late 1990s—such as *Bolsa Familia* in Brazil and *Progresa-Oportunidades* (now *Prospera*) in Mexico—recent studies have questioned their conditionality aspect; turning the spotlight to unconditional ones, including noncontributory pension schemes.

This paper evaluates the effects of one such scheme, 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. The analysis to follow employs an overidentified instrumental variable estimation—the reason why other identification approaches proved ineffective is explained in detail in the appendix to this paper. As shown, the overidentification is crucial to our results, as it allows testing the validity of the approach taken.

This paper is specifically concerned with the effects of transfers on the labor market. As illustrated by the literature below, a cash transfer may have a negative effect on the amount of labor supplied, influencing individuals to drop out of the labor force or to cut back on the number of hours that they work. However, such a benefit may also be envisioned as a reliable additional source of income, which can lift liquidity constraints or allow individuals to engage in riskier economic activities. In this context, such a benefit could potentially lead to an increase in labor force participation. Using data from the Colombia Mayor program, this paper looks to test the hypothesis that a modest income stream can help to diversify economic activity. The low age eligibility of the program and the fact that it allows beneficiaries to stay in the labor force, which together imply that many beneficiaries remain economically active, make it a model setting to conduct this study.

Our results show that the program has had the effect of increasing labor force participation in activities that are expected to require some form of up-front investment, for male beneficiaries who are relatively young (in their 50s and 60s). However, no such effect is found among women. As discussed in more detail below, these results lend support to the notion that noncontributory pension schemes —as a type of unconditional cash transfer program—can expand the economic activity of recipients by easing liquidity constraints.

The remainder of this paper is organized as follows. Section 2, next, provides an overview of literature relevant to the study. Section 3 offers a more detailed motivation for the analysis, including a theoretical motivation. This is followed, in section 4, by an in-depth description of the Colombia Mayor Program. Section 5 describes the data used and discusses our empirical strategy; while section 6 presents results. Section 7 concludes.

2. Literature Review

A large share of the literature on noncontributory pensions focuses on their expansion of coverage and their impact on poverty—both aggregate and old age—and equity. On the macro side, transfer programs have been studied as a potential mechanism of inequality and poverty reduction.[3] Other

---

[3] For example see Gasparini and Lustig (2011, 17), Lustig et al. (2013), Lustig and Pessino (2014), Barrientos (2003, 2005),

literature has looked at the effect of these transfers on well-being at a more micro level.[4] For the matter at hand, however, we are specifically interested in another aspect of transfers, namely, the role of noncontributory pensions vis-à-vis the labor market. As any transfer, noncontributory pensions may influence the incentives of individuals to offer labor. Transfers can also have an effect on investment and savings, potentially freeing up resources to spend in productive activities. For the purpose of this paper, we are particularly concerned with the potential effects of noncontributory pensions in terms of labor force participation and the role of these transfers in easing liquidity constraints.

First, we explore the evidence on the impact of noncontributory pensions on the labor market. A significant fraction of the research shows either a negative effect of noncontributory pensions on labor supply 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. Yet, the magnitude of the decline varies considerably: while it was 4.3 percentage points in Yucatan, Mexico (Work for pay) (Aguila et al. 2012), 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. Some studies find more nuanced effects. For the same *Adultos Mayores* Program, Juarez and Pfutze (2015) show a reduction of similar magnitude only among male recipients, 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 within 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 *Red de Protección Social* cash transfer program. 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. More recently, Banerjee et al. (2017) review the results from seven randomized controlled trials of cash transfers (both condition and unconditional) conducted in six

Olivera and Zuluaga (2014).

[4] See, for example, Salinas-Rodríguez et al. (2014), Martínez (2004), and Duflo (2003).

countries. Their re-analysis of the data from these studies reveals no impact of the transfer on either the number of hours worked, or the propensity to work, for neither men nor women.

Other research finds positive effects of transfers, both conditional and unconditional, on labor supply. Soares, Ribas, and Osório (2010) evaluate Brazil's *Bolsa Família* and find that it raised female labor force participation by 4.3 percentage points. For the same program, using aggregate data of Brazilian municipalities, Fogel and Paes de Barros (2010) find that labor force participation increased by 2-4 percentage points for men. Results for women are also positive, yet not significant and smaller in magnitude. Salehi-Isfahani and Mostafavi-Dehzooei (2017) analyze the impact on the labor supply of an unconditional cash transfer program in Iran. Their results suggest no negative impact on the labor supply for the average worker, except in the subgroup of workers in their twenties. On the other hand, they find a positive effect in the labor supply of workers in the service sector. A reason behind this result, as suggested by the authors, could be the large number of credit-constrained small firms in the sector, standing to benefit from the transfer.

For Colombia, Barrientos and Villa (2013) analyze the impact on the labor market of *Familias en Acción*, a conditional cash transfer program. Using a regression discontinuity design and a large panel dataset (using two waves of SISBEN household data collected in 2006 and 2010)[5], the authors find large and positive effects on activity rates when the sample is restricted to single adult households with children aged 0-6. Additionally, they find a positive effect of the program on formal employment among women, and a positive effect on the length of job search among men. Within a series of evaluations commissioned by the Colombian government for *Familias en Acción* (Castaneda and Trujillo, 2017), several studies find positive effects on adults in the labor market. An evaluation of *Familias en Acción* for the Institute for Fiscal Studies (UT IFS-Econometría-SEI, 2007) finds positive effects on labor force participation for males and females in urban areas, and for males in rural settings. The study uses a difference in differences model (and a probit model to estimate the participation rate) and data from the program's *Second Follow up Survey* (Nov. 2005–Apr. 2006). Using the same data and methodology, and also looking at adults 18 and over, a joint evaluation by DNP-Acción Social-IADB-World Bank

---

[5] For their working dataset, the authors only include households in urban areas that joined the program in 2007. To confirm that their dataset does not include households with experience of participation in the program prior to 2007, they use a validation exercise conducted by the National Planning Department in October 2006, which cross-references the 2006 SISBEN data with program administrative records. SISBEN is a proxy means test index widely used for targeting social programs in Colombia..

(2008) finds similar results on labor force participation. No significant effect was found on hours worked.

Using the program's baseline and follow-up surveys and a mean differences analysis, CNC (2011) finds that under the program's savings modality[6], the occupation rate of beneficiaries increased by 6.5 percentage points in cities other than Bogota (and 4.1 percentage points overall).[7] There was also a positive effect on the inactivity rate, which decreased by 3.24 percentage points in the treated group, as well as on the economically active population, which increased by 3.1 percentage points. Econometría–SEI (2012) finds that the probability of having a formal job for women ages 18 to 26 years old in rural areas increases by 2.5 percentage points as a result of the program; while no significant effects are found on occupation or job quality. The study uses the program's *Baseline Survey* (Second Semester of 2002) and the *Third Follow up Survey* (Nov. 2011–Feb. 2012) and a difference in differences model. Espinosa and Nanclares (2016) use a regression discontinuity design and data from Colombia's System of Identification of Social Program (SISBEN) III at the urban level, finding that the program increases participation in the labor market by 3.5 percentage points. Conversely, they find no significant effects on unemployment rate and informality. While these studies overall reflect positive impacts, there is also evidence that the program appears to have mixed effects depending on gender, age group, and area of residence.

Other literature has looked at the specific impact that cash transfers can have on economic activity, such as in the likelihood of individuals participating in farm work. De Hoop, Groppo and Handa (2017) find that unconditional cash transfers programs in Malawi and Zambia led to an expansion of household micro-entrepreneurial agricultural activity.[8] The net participation in economic activities was unaffected, given a corresponding reduction in the likelihood that adults perform paid work. This shift (from paid work into household farm work) suggests that investments in agricultural assets increased relative returns to agricultural labor. In Zambia, Prifti et al. (2017) find that a conditional cash transfer targeted at households with children under the age of three (the Child Grant Program, CGP) causes a shift from agricultural wage labor to own-farm labor. Overall, transfers have no work disincentives

---

[6] *Familias en Acción* in urban areas works under two modalities: incremental and savings.
[7] Namely, Barranquilla, Yopal, Montería, Pasto, Pereira and Villavicencio. There was no effect in Bogotá.
[8] The two programs are targeted at "labor-constrained" households, i.e., households with high dependency ratios. Results are reported looking at the sample of households with children aged 6 to 15, though a similar pattern is observed for hours worked by all adults, and for hours worked by the subset of individuals who report positive working hours.

on-farm households. Handa et al. (2018) find that the CGP increases participation in non-farm enterprises by 14 percentage points and non-farm enterprise revenues by 125 percent or 0.33 SD. In a different line, Skoufias, Unar and González de Cossio (2013) exploit the experimental design of Mexico's Food Support Program (*Programa de Apoyo Alimentario*, PAL), finding no significant effects on total labor market participation. Yet, their estimates show that while PAL had a significant negative effect on the participation of males in agricultural activities, this translated into a shift into participation in non-agricultural activities. They argue that PAL provides partial insurance (reducing downside risks) for food consumption, which allows program beneficiaries to allocate less time into agricultural production and more towards higher-risk (and higher reward) non-agricultural activities. Finally, Martinez (2004) 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.

Transfers can influence saving and investment, liberating resources to spend in productive endeavors. The literature on the role of transfers in easing liquidity constraints is, however, fairly limited. Much of the literature on the use of cash transfers to promote entrepreneurship has focused on conditional ones. Gertler et al. (2012) find that beneficiaries invested part of their cash transfers from *Oportunidades* in productive assets. Their results suggest that, for each peso transferred, households consume 74 cents and invest the rest, increasing long-term consumption by 1.6 cents. Also using data from *Progresa/Oportunidades*, Bianchi and Bobba (2013) find that occupational choices for treated households are more responsive to the amount of transfers *expected* for the future than to the amount of transfers currently received. This, they suggest, shows that the program enhances willingness to bear risk, rather than just easing liquidity constraints. For *Bolsa Família*, Ribas (2014) estimates that the share of entrepreneurs among male beneficiaries with low educational attainment has grown. However, the study questions the causal link between relieving financial constraints and the higher levels of investment given the observed rise in private transfers among households.

For noncontributory pensions, there is evidence that supports their contribution to relieving credit constraints as in the case of South Africa (Berg, 2013). The presence of a pensioner in a household may also affect employment, including by facilitating labor migration. In South Africa, Ardington et al. (2009) find that cash transfers to the elderly lead to increased employment among prime-aged adults. They attribute the impact of the pension to easing liquidity constraints for migration and job search, and by increasing the availability of elderly to care for small children. Posel, Fairburn, and Lund (2006) find that gender plays a role in the effect of pensions and 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 in search of employment. Beyond the effect on migration, Alonso et al. (2016) study the impact of Mexico's noncontributory pension schemes at the federal level—the *70 y Más* program—as well as state-level programs on households' savings. Their results show that the federal pension lowered the savings rates of certain age groups; while the combination of both programs had no effect on savings. Their findings also suggest that, for some age groups, transfers had positive effects in terms of investment in human capital and in durable and financial goods. On the other hand, the randomized control trial conducted by Haushofer and Shapiro (2013) sheds light on how regular unconditional transfers, as opposed to lump-sum transfers, are better suited to encourage long-term investment. As shown in more detail in the next sections, our paper builds on this literature, providing evidence that a small income stream allows diversifying economic activity, including towards higher-risk, higher-yield investments, easing liquidity constraints.

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, evidence points to a shift toward informality undertaken by beneficiaries (Knox and Campos-Vázquez, 2010; Aterido, Hallward-Driemeier, and Pagés; 2011; Azuara and Marinescu; 2013; Duval-Hernández and Smith Ramírez; 2011). 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." Similar results have 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, led women working in formal jobs to retire earlier than they would have done otherwise. However, Azuara and Marinescu (2013), in looking at Mexico's *Seguro Popular*, find that, notwithstanding their other results, wages have remained constant, which might suggest that a wage differential did not elicit shifts toward informality. Untangling the demand side and supply side of the labor market invites further exploration.

This study falls plainly into the literature that finds positive net effects of cash transfers on labor force participation. Its main contribution lies in showing that the effect found for more traditional cash-transfer programs, which mostly benefit families with children and pay larger transfers, is still present for beneficiaries in their 50s and 60s when given a fairly low payment. It also shows that the effect disappears once beneficiaries reach their 70s. With the exception of the studies on South Africa, which find effects on other household members and look at a much higher benefit, this is the only study to find such results for a pension program. It is also the only one to suggest very different causal channels for male and female beneficiaries: As autonomous workers for the former, and as formal sector as employees for the latter.

3. Motivation and Theoretical Background

Colombia's noncontributory pension program features two particular characteristics that make it interesting to study. First, in comparison with other programs in the region, it offers a fairly modest benefit. Official data show that, in 2012, the benefit paid amounted to around Col$41,000 a month (about US$23 at the time). This comes up to little more than US$0.75 a day at currency 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 more resource-constrained Bolivia is higher, paying around US$2 a day PPP (Bosch, Melguizo, and Pagés 2013). The second notable characteristic of the program is that the minimum age to qualify is defined as three years below the one stipulated for the general public pension system. This yields a minimum age of 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). Starting in January 2014, posterior to the time period under study, all minimum pension ages were increased by two years.

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 to 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 conceptualize the benefit is as a source of a constant income stream to a poor but potentially-economically-active population. It is important to note that the program rules do not require beneficiaries to retire from the labor force. The benefit, in this way, constitutes an additional, reliable, income stream. 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/or (b) it could alleviate 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 to an increase in labor force participation, such as on 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. Notwithstanding, as with other transfers, there are transaction costs—such as the cost of transportation—which could prevent potential beneficiaries from taking advantage of such employment opportunities.

The program's characteristics thus make Colombia Mayor an ideal setting to test these effects. Beneficiaries do not have to leave the labor force and become eligible at an age at which most people are still economically active. The small benefit is unlikely to have a large effect on the consumption-leisure trade-off. However, the liquidity and insurance effects may be present at very low transfers, for example if the required upfront investment is small. We are, therefore, in a position to explore whether the benefits from the program are allowing beneficiaries to participate in activities that are risky or that require some type of upfront investment. The available data, described below, do not allow for a longitudinal analysis, but one based on pooled cross-sections. The implication is that we are only able to identify net effects on the labor market outcomes studied. We are not able to unambiguously assess the extent to which the results are driven by entry into the labor force or a lower drop-out rate. However, given that the results support the role of liquidity constraints, entry is the more probable cause. The data do allow us to identify eligible households, to which the analysis will be restricted.

4. 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, the program's 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 individuals who were living in conditions of extreme poverty.[9] The amount of the transfer is adjusted annually based on budgetary considerations, but it is never to exceed half the value of the minimum wage. The program also provides a number of indirect nonmonetary benefits that are supplied through especially established centers catering to the needs of the elderly population, managed and co-financed by the municipalities.[10]

There are three main criteria for eligibility to the program. The first is that the beneficiary must be within three years of reaching the official retirement age or older. During the period under study, this meant a minimum age of eligibility of 52 years among women and 57 years among men. (These age thresholds 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 beneficiaries to social programs, Sisben (by its Spanish acronym). 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 survey, including the quality of the dwelling, the number of dependents, disability status, income, ownership of durable goods, and so on. The score is used to determine eligibility for all the country's social programs at the national level. The system underwent two mayor modifications, such that the current, third, version is usually referred to as Sisben III. Each social program is associated with a unique maximum score to identify beneficiaries. The maximum scores usually differ depending on

---

[9] A household is defined to be in extreme poverty if its income lies below the extreme poverty line and/or if it presents multidimensional poverty as defined by the National Administrative Department of Statistics (DANE, by its acronym in Spanish).

[10] These benefits include basic social services such as food, lodging and health, as well as medicines, technical assistance, and prosthetics not included in the obligatory health plan (POS, by its acronym in Spanish) or financed through other sources. These services are offered in established centers called *Centros de Bienestar del Adulto Mayor* (CBA) and *Centros Diurnos.*

whether a household resides in one of the country's 14 major metropolitan areas, in other urban areas, or in the countryside.[11] 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, which we use in this study to proxy 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 a minimum wage. For beneficiaries living with other individuals, the entire household income may not exceed one minimum wage. Potential beneficiaries may or may not be formal workers. If they are formal, income is identified from the unique register of contributors (RUA, by its Spanish acronym). Non-formal work beneficiaries include informal workers and unemployed.

In the data described below, we are able to observe how all three criteria would function. Yet, we consider a potential beneficiary as eligible for the program if the age and Sisben score requirements are met. We have decided not to impose the income criterion as it likely suffers from substantial measurement error in our data. The income criterion is also subject to considerable changes over time. Moreover, given the difficulty of sorting out this criterion within the Sisben score, it is 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.[12] Lending 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 for the replication of Sisben scores.

In addition to eligibility, the program also employs a prioritization strategy 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

[11] It is worth noting that the only way in which assignation to the program varies by urban and rural areas is through these scores. The Colombia Mayor program gives a different score for the 14 main cities and other urban areas (from 0 to 43.63), and a different one for rural areas (from 0 to 35.26).
[12] In the case of potential beneficiaries who are formal workers, it is possible to verify that their income lies below the stipulated threshold using the administrative records mentioned (RUA). This mechanism however, is not available for those informal workers or the unemployed.

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 used to determine who receives the benefit.[13]

Finally, Colombia Mayor does not operate in a vacuum, but rather it is one of many other social programs running at the national level. It is pertinent to wonder whether other existing programs might be affecting adults' behaviors with the same cut-off point. To answer this question we consider all programs, including those that, while not directed towards elderly individuals, may target households that include Colombia Mayor beneficiaries, such as in the case of those adults who do not live by themselves, who have dependents and/or who constitute the household head. Given the period under study, we find that the *Más Familias en Acción*, *Jóvenes en Acción*, and *Red Unidos* programs may be targeted at households that include beneficiaries from Colombia Mayor. Of these programs, the first two are conditional monetary transfers with the purpose, respectively, of improving health and education outcomes of children under 18; and the competencies and work skills of youth in the case of the latter. The third, *Red Unidos* provides integrated social services. All three programs are targeted for households living in poverty and vulnerability.

Given the thresholds of Sisben scores that are used to determine eligibility, there is a segment of households for whom programs might overlap.[14] For instance, as the score threshold for *Más Familias en Acción* is lower than that of Colombia Mayor, there is a section of households who might be beneficiaries of both. This would be the case, for example, of a household living in poverty made up by an elderly individual and children. The same could happen with the *Jóvenes en Acción* program for a household living in poverty with an elderly individual and a youth attending postsecondary school.

---

[13] The allocation of resources at the municipal level is determined according to the number of elderly individuals classified in Sisben levels 1 and 2, with respect to the total number of elderly people in those levels in the country. In terms of implementation, the municipal administration presents to Consorcio the population to be covered by the project. According to the availability of resources, a "prioritized" population is selected, according to specific prioritization criteria (including age, disability, dependents) to be placed on the waiting list to become beneficiaries. After documentation is submitted, the administrative records of candidates are revised. The information received by the municipalities during the semester is updated in the main database. The prioritization process is carried out, using such database, assigning a score to each prioritized individual, which will determine the order that person will occupy in the prioritization list of each project. Once this process has taken place, if there is a tie between two individuals, the system automatically performs a tiebreaker according to specific criteria (such as whoever registered first, disability status, etc.).

[14] The required score for a family to be a beneficiary of *Más Familias en Acción* is between 0 and 30.56 for 14 cities, from 0 to 32.20 for other urban areas and from 0 to 29.03 for rural areas. Eligibility to the Colombia Mayor program requires a Sisben score of between 0 and 43.63 for the country's main 14 cities and other urban areas, and between 0 and 35.26 for rural areas. In the case of *Jóvenes en Acción*, the eligibility score for the main 14 cities is from 0 to 54.86, while for other urban areas it is from 0 to 51.56, and for rural areas it is from 0 to 37.8.

5. Data and Empirical Strategy

Finding a convincing source of exogenous variation in program receipt is a daunting task. The data used consist of four pooled yearly cross-sections of Sisben-eligible households with at least one age-eligible member. We employ the 2010–14 rounds of the yearly Encuesta Nacional de Calidad de Vida (ENCV, by its Spanish acronym), a household level survey conducted by Colombia's National Department of Statistics (DANE, by its Spanish acronym). 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. Importantly, it is the only survey that collects all the information necessary to calculate a household's Sisben score. The Ministry of Planing (DNP, by its Spanish acronym) provided us with these scores, which are based on survey responses (i.e. are not households' actual scores). 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 covers 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.

The basic empirical model can be represented as follows:

$$Y_{itm} = \alpha + \beta CM_{itm} + \gamma \bar{Y}_{tm} + \delta \bar{Y}_{mg} + \lambda X_{itm} + \varepsilon_m + \epsilon_t + u_{itm}, \tag{1}$$

where α is the intercept, β the parameter of interest, and λ other parameters of no direct interest. The parameters γ and δ control for the outcome aggregated at the same levels (municipality-year and municipality-area) as the instrumental variables. This will be explained in more detail below. The outcome is denoted by *Y*; *CM* denotes the variable of interest (living in a household with a Colombia Mayor beneficiary); and *X* denotes additional control variables. The subscripts refer to individual *i* observed in year *t* and living in municipality *m* and area *g*. 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 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 labor force participation), 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 these 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 instrumental variable (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. 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 these reasons, 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:

$$IV_{tm}^{1} = \frac{1}{N_{tm}} \sum_{i \epsilon N_{tm}} \omega_i D_i \qquad (2)$$

$$IV_{mg}^{2} = \frac{1}{N_{mg}} \sum_{i \epsilon N_{mg}} \omega_i D_i, \qquad (3)$$

where the *g* subscript represents the specific geographical area, *D* is a specific dummy variable indicating program participation in an eligible individual's household, and ω the individual's household sampling weight. The two populations denoted by N include all age- and Sisben-eligible individuals within the respective subgroups.

There are two theoretical justifications for using these instruments. The first one is simply that program roll-out proceeded unevenly across different localities over the time period under consideration. Living in a place with higher coverage in a given year would increase the probability of program participation. The second is the possibility of positive spillover effects, leading to varying coverage levels. Potential beneficiaries may learn about the program from their friends and neighbors who are participating. Having more participating households in one's geographical area will then increase the probability of knowing about the program and becoming a beneficiary oneself. This kind of reasoning can be found, among other application, in the migration literature; and the proportion of migrants at some geographical aggregate is a commonly used instrument.

While these two explanations justify the relevance of the instruments, they do not imply fulfillment of the exclusion restrictions. One may argue that the differential roll-out may be driven by unobservable sub-municipal characteristics that also affect the outcome. Or, alternatively, that similar spillover effects between households can be present for the outcomes. To address these concerns, all specifications include municipality-year and sub-municipal, time invariant, averages of the respective outcome variable that are constructed in the same manner as the instruments. These are the terms $\bar{Y}_{tm}$ and $\bar{Y}_{mg}$ in expression (1).

To fix ideas, we can think of two additional error terms (i.e. unobserved factors) in our empirical model: ξ_{tm} (municipality-year specific) and υ_{mg} (sub-municipal), which jointly determine program roll-out and the outcome. Assume we use the outcome averages to proxy for these unobserved factors. That is, the model we would like to estimate is:

$$Y_{itm} = \alpha + \beta CM_{itm} + \xi_{tm} + \upsilon_{mg} + \lambda X_{itm} + \varepsilon_m + \epsilon_t + u_{itm}$$

Taking the appropriate averages over this empirical model yields:

$$\bar{Y}_{tm} = \frac{1}{N_{tm}} \sum_{i \in N_{tm}} \widetilde{Y}_\iota + \frac{1}{N_{tm}} \sum_{i \in N_{tm}} \xi_{tm} + \frac{1}{N_{tm}} \sum_{i \in N_{tm}} \upsilon_{mg} = \overline{\widetilde{Y_{tm}}} + \xi_{tm} + \overline{\upsilon_{tm}}$$

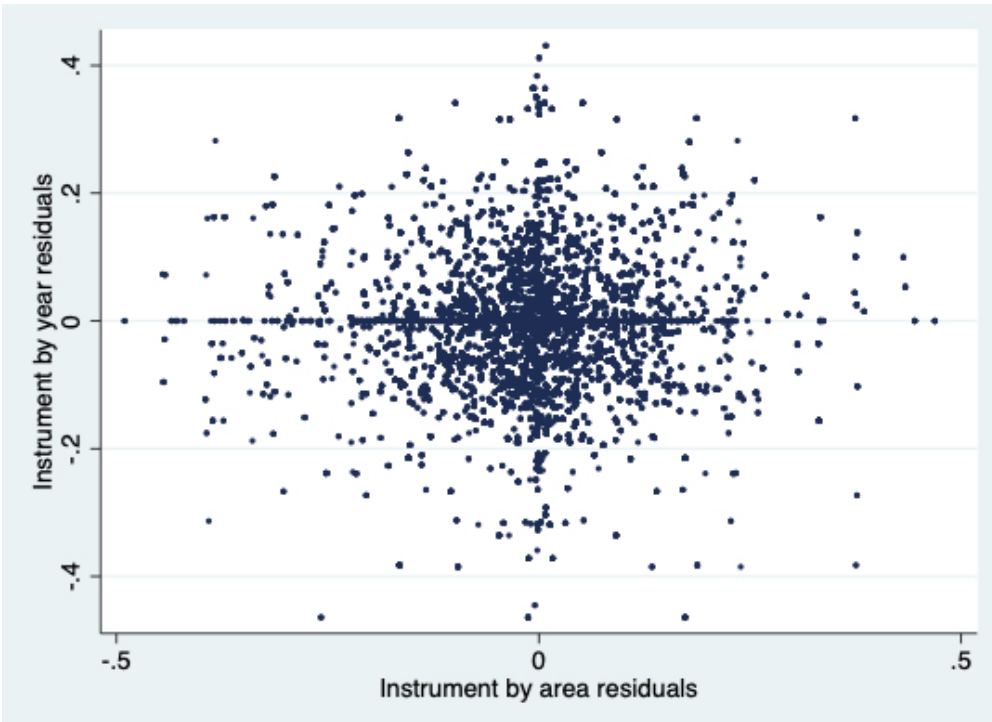
$$\bar{Y}_{mg} = \frac{1}{N_{mg}} \sum_{i \in N_{mg}} \widetilde{Y}_i + \frac{1}{N_{mg}} \sum_{i \in N_{mg}} \xi_{tm} + \frac{1}{N_{mg}} \sum_{i \in N_{mg}} \upsilon_{mg} = \overline{\widetilde{Y_{mg}}} + \overline{\xi_{mg}} + \upsilon_{mg}$$

Where $\widetilde{Y}_i$ denotes the predicted outcome for observation i when ξ_{tm} and υ_{mg} are factored out of the model. On the right-hand side of each expression, the averages are taken over the corresponding group as indicated by the subscripts. The most important point here is to show that the inclusion of the average outcomes allows to directly control for the unobserved factors ξ_{tm} and υ_{mg} that may act as omitted variables. Furthermore, in doing so, we also control for the average outcomes in the absence of these unobserved factors, $\overline{\widetilde{Y_{tm}}}$ and $\overline{\widetilde{Y_{mg}}}$. These control for the potential presence of spillover effects in the outcome variable, which may be correlated with spillover effects in program take-up and could thus act as omitted variables. Lastly, the two averaged error terms $\overline{\xi_{mg}}$ and $\overline{\upsilon_{tm}}$ (note that the averages are taken at the other error terms level of aggregation) are not expected to have any effect on the outcome, given that ξ_{tm} and υ_{mg} are controlled for. Thus far we have framed the discussion of our instruments' exclusion restrictions in terms of unobserved variables. Another, more direct, concern is that local labor market characteristics may have directly affected program roll-out. In this case, including the averages of the outcome variable at the municipality/year and municipality/area levels provides an equally direct form to control for this possibility.

This is a good point to discuss the precise causal chain behind our estimation strategy. A faster or higher level of program roll-out increases the probability that observation i receives the benefit. Given that roll-out is measured at two different aggregate levels, there is no concern that characteristics at the individual level may act as omitted variables. The inclusion of the corresponding outcome variable-averages control for any direct effect on program roll-out, unobserved characteristics at the same levels of aggregation, and spillover effects in the outcome. The instrumental variables thus isolate the partial effect of aggregate roll-out on individual take-up that is not correlated with any of these potential confounders.

One concern when using more than one instrumental variable is that a high correlation between them could result in instrument weakness and reduce the power of the OIR test. We provide statistics for instrument weakness in all our specifications. Moreover, figure 1 shows the correlation of the residuals of our instruments after regressing them on at set of municipality, year and area (i.e. locality type) fixed effects-which we control for in all our specifications. It can be clearly seen that the two instruments are almost perfectly orthogonal. The coefficient of correlation between the two residuals is 0.0148.

Figure 1: Correlation of instrument residuals after controlling for municipality, year, and area fixed effects.



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.

The ENCV sampled a total of 14,801 households in 2010, 25,364 in 2011, 21,383 in 2012, 21,565 in 2013, and 19,710 in 2014; constituting five independent cross-sections. The sampling scheme changed in 2011 and 2012 but was held constant in 2013 and 2014. The main implication of the changes in the sampling scheme is that only one municipality is exchanged each year during the last three rounds, whereas there is more change over 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 (2,483 in 2010, 5,590 in 2011, 4,081 in 2012, 3,672 in 2013, and 3,556 in 2014). The total number of potential beneficiaries is 27,724. In the sample of interest, we observe a total of 298 municipalities. Of these, 126 are only observed in one round, 2 in two, 25 in three, 76 in four, and 69 in all five rounds. The total number of municipalities sampled each year is 121 in 2010 (of which only 17 are never repeated in the following years), 220 in 2011, and 171 in 2012-2014.

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 at the household level. This makes it impossible to determine who among the various potential beneficiaries in the same household receives the benefit. Table 1 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 1: 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.

Tables 2–5 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 27,724 individual observations. Not shown in the tables, but of interest, is the fact that, of these, 43.97 percent are the sole potential beneficiaries in their households, while 47.45 percent live in households with another beneficiary, and 7.15 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. Labor force participation is determined by a battery of questions in the ENCV but defined here as either working at least one hour per week or being actively searching for work. Globally, the average labor force participation rate for the population of interest is 42 percent (table 2). 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 2: Descriptive Statistics on Labor Force Participation

	Observations, number	*Mean*	*Standard deviation*	*Minimum*	*Maximum*
All	27,724	0.42	0.49	0	1
Younger than 70					
All	15,461	0.54	0.50	0	1
Male	5,907	0.79	0.41	0	1
Female	9,554	0.38	0.48	0	1
70 or older					
All	12,263	0.27	0.45	0	1
Men	5,818	0.43	0.49	0	1
Women	6,445	0.13	0.34	0	1

With respect to the treatment variable, 29 percent of all the potential beneficiaries live in households that receive the Colombia Mayor benefit (table 3). Among household members under 70 years of age, the rate is 20 percent, whereas, among household members 70 years of age or older, it is 41 percent. These rates are fairly consistent between men and women.

Table 3: Descriptive Statistics on Receipt of the Colombia Mayor Benefit

	Observations, number	*Mean*	*Standard deviation*	*Minimum*	*Maximum*
All	27,724	0.29	0.46	0	1
Younger than 70					
All	15,461	0.20	0.40	0	1
Male	5,907	0.19	0.39	0	1
Female	9,554	0.21	0.41	0	1
70 or older					
All	12,263	0.41	0.49	0	1
Men	5,818	0.39	0.49	0	1
Women	6,445	0.43	0.49	0	1

For our control variables, we present numbers only for the whole sample (table 4). 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 69 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 3.6 members, of which 1.88 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 8 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 4: Descriptive Statistics of Control Variables

Variable	*Observations, number*	*Mean*	*Standard deviation*	*Minimum*	*Maximum*
Control variables					
Other urban	27,724	0.59	0.49	0	1
Rural	27,724	0.32	0.47	0	1
Age	27,724	68.53	9.74	52	108
Sisben	27,724	28.65	8.65	1.54	43.63
Female	27,724	0.58	0.49	0	1
Widowed	27,724	0.24	0.43	0	1
Divorced	27,724	0.14	0.34	0	1
Single	27,724	0.11	0.31	0	1
Potential beneficiaries, number	27,724	1.66	0.68	1	5
Other adults, number	27,724	0.98	1.14	0	10
Minors, number	27,724	0.99	1.36	0	12
Females, number	27,724	1.88	1.37	0	11
Primary	27,652	0.57	0.49	0	1
Secondary	27,652	0.08	0.28	0	1
Postsecondary	27,652	0.02	0.15	0	1
Incapacitated	27,724	0.13	0.33	0	1
Instrumental variables					
Municipality-year rollout	27,724	0.29	0.18	0	1
Submunicipal average rollout	27,696	0.28	0.17	0	1

For our more detailed analysis, aimed at determining causal channels, 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. In the years 2010, 2011 and 2014, it also included (e) a battery of detailed consumption and expenditure questions. 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 whether an individual works in the formal sector (measured, as is customary, as paying into the public pension program), and his/her monthly labor income and number of hours worked. For the last item, no information was collected in the 2012 round, so that the analysis for that outcome excludes this round. For the last group of outcomes, we show results for expenditures on public transportation, clothing and shoes. In each case, we consider the binary outcome whether any such expenditure has taken place[15], and the total amount spent. Ideally, we would want to be able to observe expenditures on agricultural inputs. Unfortunately, such data is not collected by the ENCV. However, expenditures on transportation and the attire required for work do constitute important up-front investments that the individual will need to meet before starting to work.

Table 5 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. Expenditures are fairly similar between men and women, but somewhat lower for older individuals. These averages should be kept in mind in interpreting the results presented hereafter.

[15] For clothing and shoes this refers to expenditures during the three months prior to the interview. For public transportation, we merge outcomes for local transportation (past week) ad inter-urban transportation (past year). For the amount spent, the figure for the latter is put into weekly terms.

Table 5: Detailed Labor Market Outcomes, Means

Indicator	*All*	*Younger than 70*			*70 or older*		
		All	*Male*	*Female*	*All*	*Male*	*Female*
Size of business							
Works alone	0.25	0.31	0.42	0.23	0.17	0.25	0.09
Place of work							
Home or street	0.13	0.17	0.14	0.18	0.09	0.09	0.08
Other homes	0.03	0.05	0.04	0.06	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	0.07	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	0.30	0.39	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	0.01	0.02	0.01	0.02	0.01	0.01	0.01
Sector of work							
Agriculture & related	0.19	0.22	0.47	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	0.05	0.04
Service & tourism	0.08	0.12	0.10	0.14	0.03	0.04	0.03
Other outcomes							
Formal	0.03	0.04	0.05	0.04	0.01	0.01	0.00
Labor income	138,951	194,111	307,107	124,249	69,406	123,472	20,601
Hours worked	12.41	16.29	26.08	10.24	7.52	12.60	2.94
Expenditures							
Transport	0.31	0.33	0.32	0.34	0.29	0.30	0.29
Transport Amount	5,295	5,812	5,356	6,092	4,627	4,451	4,785
Clothing	0.21	0.22	0.22	0.22	0.18	0.18	0.19
Clothing Amount	27,308	30,081	30,263	29,969	23,729	21,389	25,835
Shoes	0.19	0.20	0.19	0.21	0.17	0.16	0.18
Shoes Amount	12,805	14,086	13,665	14,344	11,152	10,191	12,016

6. Results

We start by presenting the results for the first-stage probit regressions for our most important specifications. This allows us, first, to 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 6, which presents marginal effects evaluated at the mean (not parameter estimates) for the first stage probit model for the estimation on labor force participation. 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 0.9–1.2 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 4-6 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 in most cases; but this effect is only barely statistically significant among comparatively younger individuals (under 70 years of age) and negative for women. 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 6: Regression of the Treatment Variable on Instruments and Exogenous Control

Variable	*All*	*All*	*< 70 All*	*>= 70 All*	*Males*	*Females*
Instrument by year	0.924***	0.912***	0.649***	1.161***	0.959***	0.894***
	0.036	*0.036*	*0.033*	*0.064*	*0.043*	*0.045*
Instrument by area	0.996***	0.974***	0.631***	1.322***	0.958***	0.997***
	0.027	*0.030*	*0.035*	*0.056*	*0.046*	*0.040*
Labor force part. by year	0.009	0.018	-0.008	0.055	0.007	0.026
	0.029	*0.032*	*0.036*	*0.056*	*0.054*	*0.041*
Labor force part. by area	0.076***	0.062**	0.091**	0.043	0.107**	0.046
	0.029	*0.031*	*0.042*	*0.066*	*0.052*	*0.050*
Other urban	0.001	0.000	0.002	0.016	0.004	0.001
	0.024	*0.022*	*0.018*	*0.039*	*0.030*	*0.028*
Rural	-0.039	-0.052**	-0.038*	-0.044	-0.056*	-0.044
	0.024	*0.022*	*0.020*	*0.041*	*0.030*	*0.029*
Age	0.012***	0.010***	0.015***	0.004***	0.011***	0.010***
	0.000	*0.000*	*0.001*	*0.001*	*0.001*	*0.000*
Female	0.048***	0.044***	0.060***	0.040***		
	0.006	*0.006*	*0.006*	*0.013*		
Sisben	-0.006***	-0.006***	-0.004***	-0.007***	-0.006***	-0.006***
	0.001	*0.001*	*0.000*	*0.001*	*0.001*	*0.001*
Potential beneficiaries, number		0.042***	0.073***	0.014	0.052***	0.031**
		0.012	*0.016*	*0.018*	*0.018*	*0.014*
Other adults, number		0.039***	0.062***	0.008	0.063***	0.016
		0.014	*0.015*	*0.026*	*0.020*	*0.016*
Minors, number		0.069***	0.066***	0.059**	0.091***	0.050***
		0.016	*0.016*	*0.023*	*0.021*	*0.019*
Females, number		0.091***	0.125***	0.037***	0.112***	0.079***
		0.009	*0.008*	*0.013*	*0.011*	*0.010*
Widowed		0.006	0.006	-0.002	-0.003	0.010*
		0.005	*0.004*	*0.008*	*0.006*	*0.005*
Divorced		-0.031***	-0.019***	-0.031***	-0.033***	-0.032***
		0.005	*0.004*	*0.009*	*0.008*	*0.006*
Single		0.002	0.001	0.005	0.005	0.006
		0.006	*0.005*	*0.009*	*0.009*	*0.007*
Primary		-0.035***	-0.016**	-0.059***	-0.035***	-0.037***
		0.007	*0.008*	*0.012*	*0.011*	*0.010*
Secondary		-0.106***	-0.047***	-0.222***	-0.062***	-0.136***
		0.011	*0.010*	*0.026*	*0.021*	*0.012*
Postsecondary		-0.180***	-0.102***	-0.345***	-0.184***	-0.178***

		0.014	*0.011*	*0.024*	*0.019*	*0.019*
Incapacitated		0.000	0.030**	0.018	0.024*	-0.022*
		0.009	*0.015*	*0.014*	*0.014*	*0.012*
Observations	27,605	27,535	15,109	12,159	11,572	15,862

Note: The table illustrates a probit model on dependent variable indicating that at least one household member receives Colombia Mayor benefit. Parameters shoe estimated marginal effects at mean. Standard errors are clustered at the municipal level and shown in italics.

6.1. Main Results

Moving on to our actual results, we start by presenting the results for the total labor force participation of potential beneficiaries. Table 7 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 group 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 7: Results for Labor Force Participation, Entire Sample

Variable	*OLS All*	*OLS All*	*OLS Males*	*OLS Females*	*IV All*	*IV All*	*IV Males*	*IV Females*
Colombia Mayor	-0.020***	-0.010*	0.002	-0.006	0.056***	0.069***	0.057**	0.090***
	0.006	*0.006*	*0.009*	*0.007*	*0.015*	*0.016*	*0.023*	*0.026*
Dependent variable by year	0.626***	0.559***	0.499***	0.577***	0.627***	0.559***	0.499***	0.577***
	0.029	*0.028*	*0.045*	*0.040*	*0.030*	*0.029*	*0.045*	*0.041*
Dependent variable by area	0.713***	0.657***	0.577***	0.629***	0.725***	0.671***	0.584***	0.648***
	0.024	*0.025*	*0.043*	*0.051*	*0.024*	*0.025*	*0.044*	*0.051*
Observations	27,605	27,535	11,572	15,862	27,605	27,535	11,572	15,862
R-squared	0.257	0.324	0.414	0.160	0.253	0.319	0.412	0.152
Municipalities, number	288	288	276	285	288	288	276	285
F	822.9	452.4	507.4	136.0	799.2	411.4	484.5	130.8
Partial R-squared					0.0889	0.0952	0.102	0.0933
Kleinbergen-Paap statistic					2561	2574	1481	1660
OIR test					0.0657	0.0657	0.127	0.231
Endogeneity test					3.98e-06	1.96e-06	0.0153	0.000344
First stage								
Dependent variable by year					0.004	0.002	0.004	0.001
					0.023	*0.025*	*0.039*	*0.033*
Dependent variable by area					-0.002	-0.002	-0.001	-0.005
					0.021	*0.022*	*0.036*	*0.037*
Instrument from probit					0.984***	0.999***	0.997***	0.999***
					0.019	*0.020*	*0.026*	*0.025*

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 clustered at the municipal level and shown in italics.

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, as explained above, the OIR test is crucial to assess the validity of our approach. It allows testing for whether the exclusion restrictions are fulfilled 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.

As to the results in table 7, 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 negative effect of program participation by one-half and reduces the significance level from the 1 percent to the 10 percent level. 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. Importantly, they do not change by much once the additional controls are included (comparing columns 5 and 6). This provides additional support for the validity of our approach to identification. They imply that program participation has the effect of boosting labor force participation (or reducing the retreat of beneficiaries from the labor force). The magnitude of the point estimate for women is particularly striking given their lower level of overall labor force participation. 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 somewhat low for the joint sample, but for the separate male and female subsamples instrument exogeneity cannot 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 put the cutoff at age 70. We then also divide each age-group by gender. The first three columns of table 8 present the results for the relatively younger cohort. The results are positive, and statistically significant at the 5 percent level for men and at the 10 percent level for women. The point estimate is only slightly larger for men; but given women's generally lower participation rate, their result constitutes a larger increase in percentage terms. The next three columns show the corresponding results for individuals 70 years of age or older. All the results are much smaller in magnitude and statistically insignificant. 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. The p-value of the OIR test statistic is very high for younger men, but somewhat lower (at 0.15) for females. Instrument weakness is not a concern.

Our results suggest that labor force participation increases for men beneficiaries by 8.1 percentage points, and for women beneficiaries by 7.4 percentage points. To put these results in context, there is evidence suggesting that *Bolsa Família* raised female labor force participation by 4.3 percentage points (Soares et al., 2010); and that male labor force participation increased with the same program by 2-4 percentage points using aggregate data for Brazilian municipalities, while results for women in this study, though positive, were smaller and not significant (Fogel and Paes de Barros, 2010). In Colombia, looking at beneficiaries of *Familias en Accion*, Barrientos and Villa (2013) find that eligible adults in single adult households with children aged 0-6 have 9 percent higher rates of participation than non-eligible ones; the difference for females is positive at 6 percent. They find effects on labor force participation of 2.3 percent for all adult males, and 2.9 percent for males aged 21 to 35. Other evaluations of *Familias en Accion* suggest comparable results. UT IFS-Econometría-SEI (2007) find that the program increased the participation of adult women by 4.1 percentage points in urban areas (significant at 10 percent), of adult males in rural areas by 2.7 percentage points (significant at 1 percent), and by 3.1 percent for women in rural areas (where this last result is not significant). DNP-Acción Social-IADB-World Bank (2008) finds that women's participation rate in urban areas increased by 4.1 percentage points; while CNC (2011) indicates that, at the urban level, the program leads to an increase of 4.1 percentage points in beneficiaries' occupation rate. Espinosa and Nanclares (2016) find that the occupation rate increases with the program by 2.8 percentage points; where women's labor participation and occupation rate are lower than those for men.

Table 8: Results on Labor Force Participation, by Age-Group

Variable		*< 70 years*			*≥ 70 years*	
	All	*Males*	*Females*	*All*	*Men*	*Women*
Colombia Mayor	0.071**	0.081**	0.074*	-0.014	0.027	0.014
	0.030	*0.038*	*0.040*	*0.021*	*0.036*	*0.028*
Dependent variable by year	0.716***	0.362***	0.746***	0.474***	0.528***	0.337***
	0.042	*0.064*	*0.058*	*0.045*	*0.065*	*0.052*
Dependent variable by area	0.825***	0.487***	0.775***	0.678***	0.673***	0.363***
	0.038	*0.056*	*0.075*	*0.046*	*0.071*	*0.068*
Observations	15,109	5,436	9,226	12,159	5,667	6,262
R-squared	0.112	0.317	0.121	0.194	0.338	0.068
Municipalities, number	274	231	260	282	259	264
F	213.0	488.2	97.83	93.75	111.2	22.21
Partial R-squared	0.102	0.121	0.0994	0.0953	0.0981	0.0911
Kleinbergen-Paap statistic	1167	832.3	755.0	1090	541.9	663.5
OIR test	0.292	0.962	0.150	0.971	0.0598	0.660
Endogeneity test	0.000207	0.0638	0.0812	0.708	0.427	0.367
First stage						
Dependent variable by year	0.002	0.001	0.001	0.008	0.005	0.005
	0.033	*0.053*	*0.040*	*0.043*	*0.057*	*0.066*
Dependent variable by area	0.002	0.004	-0.001	-0.003	-0.001	-0.007
	0.036	*0.054*	*0.047*	*0.046*	*0.070*	*0.076*
Instrument from probit	1.019***	1.020***	1.006***	0.994***	0.993***	0.989***
	0.030	*0.035*	*0.037*	*0.030*	*0.043*	*0.038*

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 and full set of fixed effects. Standard errors are clustered at the municipal level and shown in italics.

6.2. Causal Channels

In order to get a better idea of the possible causal channels for these results, we have to cut deeper and look at which economic activities are particularly affected. This is done in table 9, where we explore the results for two age-groups and for gender-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 present results for working in the formal sector, 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 results have important implications.

In the first three columns of table 9, we show the results for potential beneficiaries younger than 70. We do have important results for males and females, but of a somewhat different nature. For males, we find positive and statistically significant results for working alone, for working on agricultural land, rivers, or the sea, and for working independently or working on one's own land (even if only at the 10 percent significance level). Correspondingly, we also find an increase of males younger than 70 working in the primary sector (at the 10 percent level). In terms of magnitude, we find that such males are 10 percentage points more likely to work alone, 13.2 percentage points more likely to work on agricultural land, 13.3 percentage points more likely to work independently, 4.9 percentage points more likely to work on their own land, and 7.3 percentage points more likely to work in the agricultural sector. These results are consistent with international evidence. Under Malawi's unconditional cash transfer program, individuals (aged 18 to 55 years) were 12 percentage points more likely to work in agriculture in the household farm or in herding activities in program villages. (De Hoop et al. 2017). In addition, the likelihood that beneficiary households sold any crop or owned any livestock increased with unconditional cash transfers in Malawi and Zambia by 10 and 17 percentage points, and by 26 and 34 percent, respectively (idem). In Zambia, recent evidence suggests that the Child Grant Program led to an increase participation in non-farm enterprises by 14 percentage points and in non-farm enterprise revenues by 125 percent (Handa et al. 2018). In Mexico, there is evidence that beneficiaries invested 26 cents out of each peso transferred from *Oportunidades* in productive assets, increasing agricultural income by almost 10 percent (Gertler et al. 2012); while another study suggests that the likelihood of becoming an entrepreneur with the program increases by about 20 percent (Bianchi and Bobba, 2013).

In our results, we also find that the benefit is estimated to increase monthly labor income by around Col$172,000 (around US$55) among this group and that the number of weekly hours worked rose by 5.3 hours. To put these numbers into perspective, we can think about two extreme scenarios. Under the first, if the entire increase in income came from the additional hours worked at the extensive margin (assuming 22 working days in a month), this would imply hourly earnings of Col$7,376, or a little less than two and a half times the official minimum wage. If, on the other hand, beneficiaries shifted all their working hours towards more remunerative activities, a back of the envelope calculation based on an average of 26.1 weekly hours worked, and a participation rate of 19% in the program (taken from tables 3 and 5) would indicate an average increase for that group from 25.1 to 30.4 hours worked. Average hourly earnings would increase from Col$2,485 to Col$3,339, or by less than a third of a minimum wage (corresponding to an increase of 35%). While we find an expansion in labor force participation in the categories just discussed, 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. It follows that the estimates discussed here assume that beneficiaries and non-beneficiaries would, on average, work the same hours for the same pay.

Table 9: 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	0.037	0.100**	-0.009	0.009	0.043	0.030
	0.027	*0.048*	*0.033*	*0.020*	*0.032*	*0.025*
Place of work						
Home or street	-0.019	-0.027	-0.020	0.014	-0.004	0.036
	0.020	*0.036*	*0.029*	*0.015*	*0.028*	*0.022*
Other homes	-0.001	-0.011	0.014	-0.011*	-0.015*	-0.007
	0.011	*0.018*	*0.015*	*0.006*	*0.009*	*0.009*
Land, river, or sea	0.062***	0.132***	0.018	-0.036**	0.021	-0.015
	0.022	*0.042*	*0.022*	*0.018*	*0.033*	*0.011*
Type of occupation						
Private employee	0.015	0.018	0.030**	-0.002	-0.006	0.001

	0.011	*0.021*	*0.013*	*0.007*	*0.012*	*0.006*
Public employee	0.020**	0.011	0.022**	0.002	0.002	0.001
	0.008	*0.009*	*0.010*	*0.002*	*0.004*	*0.001*
Independent worker	0.037	0.133***	-0.026	0.008	0.051	-0.002
	0.025	*0.050*	*0.033*	*0.020*	*0.037*	*0.024*
Own land	0.019	0.049*	-0.013	-0.008	0.006	0.011
	0.016	*0.030*	*0.016*	*0.014*	*0.028*	*0.009*
Unpaid	0.007	-0.007	0.015	0.002	0.004	-0.006
	0.008	*0.011*	*0.012*	*0.005*	*0.009*	*0.007*
Sector of work						
Agriculture & related	0.047**	0.073*	0.025	-0.029	0.030	-0.006
	0.023	*0.040*	*0.024*	*0.019*	*0.034*	*0.014*
Manufacturing	0.011	0.008	0.022	0.000	-0.016	0.007
	0.010	*0.016*	*0.015*	*0.009*	*0.014*	*0.012*
Trade & commerce	-0.013	0.009	-0.050**	0.008	0.021	-0.004
	0.016	*0.030*	*0.024*	*0.011*	*0.020*	*0.015*
Service & tourism	0.042***	0.043	0.048*	0.004	0.004	0.004
	0.016	*0.026*	*0.025*	*0.009*	*0.016*	*0.013*
Other outcomes						
Formal Sector	0.032***	0.021	0.042***	0.001	0.001	0.006
	0.012	*0.017*	*0.016*	*0.004*	*0.008*	*0.004*
Labor income	107,993.946***	172,150.173***	86,581.274**	-28,498.555**	-19,452.727	-12,824.117
	35,888.669	*57,689.294*	*42,431.410*	*14,502.378*	*27,847.370*	*8,787.479*
Hours worked	2.200*	5.334**	-0.349	-1.721*	-1.228	-0.207
	1.332	*2.276*	*1.557*	*0.923*	*1.731*	*0.911*

Note: Only parameters on treatment variable shown. Linear probability model with three-stage procedure for binary treatment variables on respective dependent variable. For "Labor Income" and "Hours Worked" dependent variables are continuous. All specifications include complete set of control variables and full set of fixed effects. Standard errors are clustered at the municipal level and shown in italics.

For females the picture is different. We find strongly statistically significant effects on private and public employment, and work in the formal sector (the latter at the 1 percent level). In terms of magnitude, we find increases of 3 percentage points for private, 2.2 percentage points for public sector employment, and 4.2 percentage points for employment in the formal sector. Moreover, there is a shift from the trade and commerce to the service and tourism sector. This magnitude of this shift is around 5 percentage points (though only significant at the 10 percent level for the latter). While hours worked stay unaltered, incomes increase by an estimated Col$1,922 per hour, or a little less than two-thirds of a minimum wage.

The other three columns in table 9 show the corresponding results among individuals 70 years of age or older. The results are mostly statistically insignificant, and the few results that are have the opposite sign compared to the younger cohort. There is a lower participation for individuals working in other homes or on land, rivers or sea; fewer hours worked and a lower labor income. While these results point to some effect of Colombia Mayor to lower labor force participation for people older than 70 years in some occupations, the important conclusion for our purpose is that the identified positive effects can only be found for the relatively younger cohort.

Considering the discussion in section 3, the results above suggest that the principal effect of the benefit on individuals younger than 70 years was to increase their labor supply by easing budget constraints that prevented entry into the labor force. For males, this increase manifested itself primarily as an entry into independent agricultural production. For females, the results suggest not only some entry, but also a shift from less lucrative activities (possibly as informal vendors) into formal employment. To test for these channels, we directly look at changes in expenditure patterns for items that may constitute up-front investments for labor market entry (or change in activity). As mentioned above, the ENCV included a consumption and expenditure module only in the years 2010, 2011, and 2014, leaving us with a smaller sample size and hence possibly less statistical power. Moreover, we are unfortunately not able to observe spending on any agricultural inputs.

Among the observable expenditures, we are interested in public transportation (local and inter-municipal), clothing and shoes. These results are presented in table 10. For each case, we present results on the binary outcome of whether any spending in the category has taken place, followed by the amount spent. The first result is that we only find statistically (and economically) significant effects for the relatively younger cohorts. This should rule out the concern that our estimates merely pick up increased consumption due the additional income provided by the benefit. For the younger cohort, we find a strong increase in transportation expenditure for males, on the binary as well as on the continuous outcome (though, in the latter case only significant at the 10 percent level). Why we are unable to observe agricultural inputs, higher expenditure on transportation likely reflects the cost of bringing the production to market. For poor women, finding formal employment in the private or public sector likely requires proper attire as an important up-front investment. For clothing and shoes, we do find an increase on the binary measure for both males and females. However, the effect is larger and/or statistically more significant for females. In particular, the amount spent on clothing is statistically significant for females at the 1 percent level, but insignificant for males. We are of course unable to distinguish between spending on professional attire and other work-related clothing. The results on shoes for men may, for example, be driven by investments in rubber boots necessary for agricultural work. While these results lend further support to our hypothesized role of up-front expenditures, it has to be pointed out that they may also reflect increased spending due to the higher labor income shown in the previous table.

Table 10: Results for up-front expenditures

Variable	< 70 years			≥ 70 years		
	All	*Males*	*Females*	*All*	*Males*	*Females*
Transport	0.070	0.183***	0.018	-0.013	0.015	-0.003
	0.044	*0.058*	*0.052*	*0.042*	*0.058*	*0.049*
Transport Amount	728.133	2,773.905*	-27.223	-190.768	-274.347	109.044
	1,324.362	*1,533.874*	*1,707.643*	*859.361*	*1,214.943*	*1,150.417*
Clothing	0.122***	0.110**	0.130***	-0.026	-0.034	-0.013
	0.035	*0.050*	*0.043*	*0.030*	*0.048*	*0.037*
Clothing Amount	20,656.358***	16,880.413	26,643.208***	717.298	3106.614	861.564
	7667.42	*13339.938*	*9737.977*	*5691.535*	*9028.301*	*7607.251*
Shoes	0.095***	0.119**	0.110***	-0.043	-0.025	-0.055
	0.030	*0.049*	*0.040*	*0.027*	*0.038*	*0.037*
Shoes Amount	12,142.688***	16,275.845**	12,861.858***	-2,460.568	-4,955.818	-1,238.901
	3,897.678	*7,254.158*	*4,595.713*	*3,248.812*	*4,572.115*	*4,696.896*

Note: The table illustrates a linear probability model relying on a three-stage procedure for binary treatment variables on the respective dependent variable. Dependent variables are either binary (whether any expenditure has taken place), or based on the continuous amount spent in the category. All specifications include a complete set of control variables and full set of fixed effects. Standard errors are clustered at the municipal level and shown in italics.

Another possibility to test for the hypothesis that our results on labor force participation are driven by budget constraints is to check whether the effects are more pronounced for poorer individuals. This is done in table 11. We take the first three columns from table 8 (i.e for individuals younger than 70) and divide the sample into the top 50% and bottom 50% according to their Sisben score[16]. We indeed find large and statically significant effects for the group with lower socio-economic status. Results for the comparatively richer group are still positive, yet insignificant. Omitted from the table, but available upon request, we do not find any statistically significant results for individuals older than 70.

Table 11: Results on Labor Force Participation for Individuals Younger Than 70 Years by Sisben Score.

Variable	Bottom 50% Sisben Score			Top 50% Sisben Score		
	All	*Males*	*Females*	*All*	*Men*	*Women*
Colombia Mayor	0.099**	0.123***	0.126**	0.046	0.064	0.033
	0.041	*0.045*	*0.054*	*0.040*	*0.053*	*0.051*
Dependent variable by year	0.674***	0.254***	0.767***	0.725***	0.402***	0.721***
	0.072	*0.099*	*0.088*	*0.062*	*0.087*	*0.088*
Dependent variable by area	0.775***	0.418***	0.742***	0.882***	0.556***	0.813***
	0.066	*0.079*	*0.107*	*0.057*	*0.099*	*0.089*

[16] To do so we first calculated the median Sisben score for the three areas (14 principal cities, other urban areas, rural) and assigned each household to the top or bottom 50% according to its position relative to the corresponding median.

Observations	7,085	2,502	4,258	7,449	2,417	4,532
R-squared	0.112	0.362	0.113	0.111	0.282	0.130
Municipalities, number	236	179	219	236	179	220
F	187.6	240.7	53.36	89.81	177.5	47.76
Partial R-squared	0.0995	0.117	0.0981	0.117	0.157	0.108
Kleinbergen-Paap statistic	614.4	370.1	385.7	521.5	354.3	358.5
OIR test	0.265	0.892	0.145	0.943	0.92	0.661
Endogeneity test	0.000442	0.00870	0.0298	0.0306	0.333	0.473
First stage						
Dependent variable by year	-0.005	-0.005	0.003	0.003	-0.003	0.002
	0.056	*0.105*	*0.062*	*0.055*	*0.085*	*0.067*
Dependent variable by area	0.007	0.005	0.005	0.000	0.001	-0.007
	0.059	*0.100*	*0.075*	*0.054*	*0.096*	*0.067*
Instrument from probit	1.006***	1.022***	0.982***	1.036***	1.026***	1.021***
	0.041	*0.053*	*0.050*	*0.045*	*0.055*	*0.054*

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 and a full set of fixed effects. Standard errors are clustered at the municipal level and shown in italics.

7. 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 non-conditional 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 are generally riskier. 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 beneficiaries under the age of 70. For men, this effect is particularly noticeable in occupations that require working alone as independent workers or in the cultivation of agricultural land. Women, on the other hand, moved into formal employment as a result of the benefit. The additional results on expenditures 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.

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.

References

Adams, Renée, Heitor Almeida, and Daniel Ferreira. 2009. "Understanding the Relationship between Founder-CEOs and Firm Performance." *Journal of Empirical Finance* 16 (1): 136–50.

Aguila, Emma, Arie Kapteyn, Rosalba Robles, and Beverly Weidmer. 2012. "Experimental Analysis of the Health and Well-Being Effects of a Non-Contributory Social Security Program." Rand Corporation, Santa Monica, CA.

Amuedo-Dorantes, Catalina, and Laura Juarez. 2015. "Old-Age Government Transfers and the Crowding Out of Private Gifts: The 70 and Above Program for the Rural Elderly in Mexico." *Southern Economic Journal* 81 (3): 782–802.

Ardington Cally, Anne Case, and Victoria Hosegood V. 2009. "Labor supply responses to large social transfers: longitudinal evidence from South Africa". *American Economic Journal: Applied Economics* 1(1):22–48.

Aterido, Reyes, Mary Hallward-Driemeier, and Carmen Pagés. 2011. "Does Expanding Health Insurance beyond Formal-Sector Workers Encourage Informality? Measuring the Impact of Mexico's Seguro Popular." Policy Research Working Paper 5785, World Bank, Washington, DC.

Azuara, Oliver, and Ioana Marinescu. 2013. "Informality and the Expansion of Social Protection Programs: Evidence from Mexico." *Journal of Health Economics* 32 (5): 938–50.

Banerjee Abhijit V., Rema Hanna, Gabriel E. Kreindler, and Benjamin A. Olken. 2017. "Debunking the Stereotype of the Lazy Welfare Recipient: Evidence from Cash Transfer Programs." *The World Bank Research Observer* 32 (2): 155–184.

Barrientos, Armando. 2003. "What Is the Impact of Non-Contributory Pensions on Poverty? Estimates from Brazil and South Africa." Development Economics and Public Policy Working Paper 30556, Institute for Development Policy and Management, University of Manchester, Manchester, U.K.

———. 2005. "Non-contributory Pensions and Poverty Reduction in Brazil and South Africa." Working paper, Institute for Development Policy and Management, University of Manchester, Manchester, U.K.

Barrientos, Armando, and Peter Lloyd-Sherlock. 2003. "Non-contributory Pensions and Social Protection." Issues in Social Protection Discussion Paper 12, International Labour Organization, Geneva.

Barrientos, Armando, and Juan M. Villa. 2013. "Antipoverty transfers and labour force participation effects". The University of Manchester, UK: Brooks World Poverty Institute.

Berg, Erlend. 2013. "Are Poor People Credit-Constrained or Myopic? Evidence from a South African Panel." *Journal of Development Economics* 101 (March): 195–205.

Bianchi, Milo, and Matteo Bobba. 2013. "Liquidity, Risk, and Occupational Choices." *Review of Economic Studies* 80(2): 491-511.

Bosch, Mariano, and Raymundo M. Campos-Vázquez. 2010. "The Trade-Offs of Social Assistance Programs in the Labor Market: The Case of the 'Seguro Popular' Program in Mexico." Documento de Trabajo 12–2010, Centro de Estudios Económicos, El Colegio de México, Mexico City.

Bosch, Mariano, and Jarret Guajardo. 2012. "Labor Market Impacts of Non-Contributory Pensions: The Case of Argentina's Moratorium." IDB Working Paper 366, Inter-American Development Bank, Washington, DC.

Bosch, Mariano, Ángel Melguizo, and Carmen Pagés. 2013. *Better Pensions, Better Jobs: Towards Universal Coverage in Latin America and the Caribbean*. Washington, DC: Inter-American Development Bank.

Camacho, Adriana, Emily Conover, and Alejandro Hoyos. 2014. "Effects of Colombia's Social Protection System on Workers' Choice between Formal and Informal Employment." *World Bank Economic Review* 28 (3): 446–66.

Castañeda, Carlos, and Lorena Trujillo. 2017. "Evaluación del impacto de los subsidios sobre el mercado laboral colombiano". Dirección de Seguimiento y Evaluación de Políticas Públicas, Bogotá D.C.: DNP.

Centro Nacional de Consultoría (CNC). 2011. "Evaluación del Programa Familias en Acción en grandes centros urbanos". Bogotá D.C.: DNP.

de Hoop, Jacobus, Valeria Groppo, and Sudhanshu Handa. 2017. "Cash Transfers, Microentrepreneurial Activity, and Child Work: Evidence from Malawi and Zambia". [Accepted for publication at World Bank Economic Review, Working paper].

DNP-Acción Social-IADB-World Bank. 2008. "Programa Familias en Acción: Impactos en capital humano y evaluación beneficio-costo del programa". Bogotá D.C.: DNP.

Duflo, Esther. 2003. "Grandmothers and Granddaughters: Old-Age Pensions and Intrahousehold Allocation in South Africa". Washington, DC: World Bank.

Duval-Hernández, Robert, and Ricardo Smith Ramírez. 2011. "Informality and Seguro Popular under Segmented Labor Markets." Working paper, Centro de Investigación y Docencia Económicas, Mexico City.

Econometría-SEI. 2012. "Impactos de largo plazo del programa Familias en Acción en municipios de menos de 100 mil habitantes en los aspectos claves del desarrollo del capital humano". Bogotá D.C.: DPS-DNP.

Espinosa, Francisco Alejandro, and Elixon F. Nanclares. 2016. "Efectos del programa Más Familias en Acción sobre variables del mercado laboral". Bogotá D.C.: mimeo.

Fogel, Nathan M. and Paes de Barros, Ricardo. 2010. "The Effect of Conditional Cash Transfer Programmes on Adult Labor Supply: An Empirical Analysis Using a Time-Series-Cross-Section Sample of Brazilian Municipalities." *Estudos Econômicos (São Paulo)*, 40(2), pages 259-293.

Galasso, Emanuela. 2006. "`With Their Effort and One Opportunity': Alleviating Extreme Poverty in Chile." Unpublished Working Paper.

Galiani, Sebastián, Paul Gertler, and Rosangela Bando. 2014. "Non-contributory Pensions." IDB Working Paper 517, Inter-American Development Bank, Washington, DC.

Gasparini, Leonardo, and Nora Lustig. 2011. "The Rise and Fall of Income Inequality in Latin America." ECINEQ Working Paper 213, Society for the Study of Economic Inequality, Dipartimento de Scienze Economiche, Università di Verona, Verona, Italy.

Gertler Paul J., Sebastian W. Martinez, and Marta Rubio-Codina. 2012. "Investing cash transfers to raise long-term living standards." *American Economic Journal: Applied Economics* 4(1):164–92.

Handa, Sudhanshu, Luisa Natali, David Seidenfeld, Gelson Tembo, and Benjamin Davis. 2018. "Can unconditional cash transfers raise long-term living standards? Evidence from Zambia". *Journal of Development Economics* 133 (July): 42-65.

Haushofer, Johannes, and Jeremy Shapiro. 2013. "Household Response to Income Changes: Evidence from an Unconditional Cash Transfer Program in Kenya." Working paper, Massachusetts Institute of Technology, Cambridge, MA.

Johnson, Jessica K. M., and John B. Williamson. 2006. "Do Universal Non-Contributory Old-Age Pensions Make Sense for Rural Areas in Low-Income Countries?" *International Social Security Review* 59 (4): 47–65.

Juárez, Laura. 2010. "The Effect of an Old-Age Demogrant on the Labor Supply and Time Use of the Elderly and Non-Elderly in Mexico." *B. E. Journal of Economic Analysis & Policy* 10 (1): 1–27.

Juárez, Laura, and Tobias Pfutze. 2015. "The Effects of a Non-Contributory Pension Program on Labor Force Participation: The Case of 70 y Más in Mexico." *Economic Development and Cultural Change* 63 (4): 685–713.

Knox, Melissa A., and Raymundo M. Campos-Vázquez. 2010. "Social Protection Programs and Employment: The Case of Mexico's Seguro Popular Program." Documento de Trabajo 14–2010, Centro de Estudios Económicos, El Colegio de México, Mexico City.

Lustig, Nora, Carola Pessino and John Scott. 2013. "The Impact of Taxes and Social Spending on Inequality and Poverty in Argentina, Bolivia, Brazil, Mexico, Peru and Uruguay: An Overview." Working Papers 1313, Tulane University, New Orleans, Louisiana.

Lustig, Nora, and Carola Pessino. 2014. "Social Spending and Income Redistribution in Argentina during the 2000s: The Increasing Role of Noncontributory Pensions." *Public Finance Review* 42 (3): 304–25.

Maluccio, John A. and Flores, Rafael. 2005. "Impact Evaluation of a Conditional Cash Transfer Program: The Nicaraguan *Red de Protección Social*." *International Food Policy Research Institute Research Report* 141.

Martinez, Sebastian. 2004. "Pensions, Poverty, and Household Investments in Bolivia." Doctoral dissertation, University of California at Berkeley, Berkeley, CA.

Miller, Grant, Diana Pinto, and Marcos Vera-Hernández. 2013. "Risk Protection, Service Use, and Health Outcomes under Colombia's Health Insurance Program for the Poor." *American Economic Journal: Applied Economics* 5 (4): 61–91.

Olivera, Javier, and Blanca Zuluaga. 2014. "The Ex-Ante Effects of Non-Contributory Pensions in Colombia and Peru." *Journal of International Development* 26 (7): 949–973.

Posel, Dorrit, James A. Fairburn, and Frances Lund. 2006. "Labour Migration and Households: A Reconsideration of the Effects of the Social Pension on Labour Supply in South Africa." *Economic Modelling* 23 (5): 836–53.

Prifti Ervin, Elisenda Estruch, Silvio Daidone, Benjamin Davis, Paul van Ufford, Michelo Stanfeld, Sudhanshu Handa, David Seidenfeld, Tembo Gelson. 2017. "Learning about labour impacts of cash transfers in Zambia." *Journal of African Economies* 26(4):433–42.

Ribas, Rafael P. 2014. "Direct and Indirect Effects of Cash Transfers on Entrepreneurship." Paper presented at the Agricultural and Applied Economics Association's 2014 Annual Meeting, Minneapolis, July 27–29.

Rofman, Rafael, Ignacio Apella, and Evelyn Vezza, eds. 2015. *Beyond Contributory Pensions: Fourteen Experiences with Coverage Expansion in Latin America*. Report 92872. Directions in Development: Human Development. Washington, DC: World Bank.

Rubio, Jose. 2014. "The Effect of Non-contributory Pensions on the Health and Welfare of the Elderly Poor in Colombia." Master's Dissertation SA4A6, London School of Economics and Political Science, London.

Salehi-Isfahani, Djavad, and Mohammad H. Mostafavi-Dehzooei. 2017. "Cash Transfers and Labor Supply: Evidence from a Large-Scale Program in Iran." WP 1090. Economic Research Forum.

Salinas-Rodríguez, Aarón, Ma Del Pilar Torres-Pereda, Betty Manrique-Espinoza, Karla Moreno-Tamayo, and Martha María Téllez-Rojo Solís. 2014. "Impact of the Non-Contributory Social Pension Program *70 y más* on Older Adults' Mental Well-Being." PLoS One 9 (11): 1–10.

Shea, John. 1997. "Instrument Relevance in Multivariate Linear Models: A Simple Measure." *Review of Economics and Statistics* 79 (2): 348–52.

Skoufias, Emmanuel and di Maro, Vincenzo. 2006. "Conditional Cash Transfers, Adult Work Incentives, and Poverty." *World Bank Policy Research Working Paper* 3973.

Skoufias, Emmanuel, Mishel Unar, and Teresa Gonzalez de Cossio.2013. "The Poverty Impacts of Cash and In-Kind Transfers: Experimental Evidence from Rural Mexico." *Journal of Development Effectiveness. D*OI: 10.1080/19439342.2013.843578

Soares, Fábio Veras, Rafael Perez Ribas, and Rafael Guerreiro Osório. 2010. "Evaluating the Impact of Brazil's Bolsa Família: Cash Transfer Programs in Comparative Perspective." *Latin American Research Review* 45 (2): 173–90.

Stock, James H., Jonathan Wright, and Motohiro Yogo. 2002. "A Survey of Weak Instruments and Weak Identification in Generalized Method of Moments." *Journal of Business and Economic Statistics* 20 (4): 518–29.

UT IFS-Econometría-SEI. 2007. 'Evaluación de Impacto del Programa Familias en Acción". Bogotá D.C.: DNP.

Wooldridge, Jeffrey M. 2010. *Econometric Analysis of Cross Section and Panel Data*. 2nd ed. Cambridge, MA: MIT Press.

Appendix I: Results for Other Outcomes and Household Members

It is also of interest to look at the effect of the program on outcomes for other individuals living in a beneficiary household. Table 12 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 much less significant, 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.

Table A1: 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	0.024***	0.027***	0.020***	0.037***	0.020	0.043**	0.035*	0.066
	0.007	*0.006*	*0.006*	*0.013*	*0.017*	*0.020*	*0.020*	*0.044*
Dependent variable by year	0.724***	0.579***	0.285***	0.829***	0.724***	0.578***	0.285***	0.828***
	0.025	*0.026*	*0.034*	*0.046*	*0.025*	*0.027*	*0.034*	*0.046*
Dependent variable by area	0.763***	0.664***	0.268***	0.960***	0.763***	0.664***	0.269***	0.957***
	0.025	*0.028*	*0.041*	*0.064*	*0.025*	*0.028*	*0.041*	*0.063*
Observations	19,628	17,907	9,157	8,391	19,628	17,907	9,157	8,391
R-squared	0.188	0.323	0.374	0.171	0.188	0.323	0.374	0.170
Municipalities, number	269	267	245	250	269	267	245	250
F	316.6	501.2	1799	147.1	316.0	496.2	1818	147.3
Partial R-squared					0.0655	0.0733	0.0797	0.0665
Kleinbergen-Paap statistic					752.1	767.4	532.9	412.0
OIR test					0.866	0.866	0.0693	0.0313
Endogeneity test					0.820	0.419	0.434	0.488
First stage								
Dependent variable by year					0.002	0.005	0.008	0.000
					0.044	*0.044*	*0.057*	*0.055*
Dependent variable by area					0.004	-0.001	-0.005	0.000
					0.044	*0.044*	*0.066*	*0.053*
Instrument from probit					0.966***	0.997***	1.006***	0.983***
					0.035	*0.036*	*0.044*	*0.048*

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 clustered at the municipal level and shown in italics.

The results on labor force participation among 13–17-year-olds, shown in table 13, 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 A2: 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	-0.003	-0.005	0.002	-0.018	0.037	0.036	0.018	0.070
	0.012	*0.012*	*0.019*	*0.015*	*0.025*	*0.026*	*0.041*	*0.043*
Dependent variable by year	0.733***	0.731***	0.858***	0.523***	0.736***	0.734***	0.860***	0.525***
	0.030	*0.031*	*0.045*	*0.063*	*0.029*	*0.029*	*0.045*	*0.061*
Dependent variable by area	0.707***	0.700***	0.856***	0.514***	0.709***	0.701***	0.858***	0.507***
	0.032	*0.032*	*0.059*	*0.062*	*0.032*	*0.032*	*0.059*	*0.064*
Observations	6,326	6,323	3,105	2,749	6,326	6,323	3,105	2,749
R-squared	0.215	0.218	0.266	0.110	0.213	0.216	0.266	0.096
Municipalities, number	224	224	185	172	224	224	185	172
F	208.3	120.7	73.50	10.92	214.0	120.1	73.42	11.10
Partial R-squared					0.0612	0.0730	0.0932	0.0696
Kleinbergen-Paap statistic					291.4	339.7	264.4	162.1
OIR test					0.351	0.263	0.419	0.820
Endogeneity test					0.143	0.144	0.667	0.0391
First stage								
Dependent variable by year					-0.003	-0.004	0.004	-0.007
					0.047	*0.044*	*0.064*	*0.075*
Dependent variable by area					0.000	0.004	-0.004	-0.001
					0.063	*0.062*	*0.086*	*0.096*
Instrument from probit					0.969***	1.006***	1.028***	1.018***
					0.057	*0.055*	*0.063*	*0.080*

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 clustered at the municipal level and shown in italics.

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 14, 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. 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 A3: 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	0.027***	0.031***	0.022***	0.024***	-0.049***	-0.029*	-0.003	-0.056**
	0.006	*0.006*	*0.007*	*0.008*	*0.018*	*0.017*	*0.022*	*0.026*
Dependent variable by year	0.733***	0.593***	0.522***	0.618***	0.736***	0.596***	0.524***	0.620***
	0.022	*0.022*	*0.038*	*0.035*	*0.022*	*0.022*	*0.039*	*0.035*
Dependent variable by area	0.701***	0.598***	0.460***	0.577***	0.711***	0.607***	0.464***	0.588***
	0.031	*0.028*	*0.044*	*0.047*	*0.031*	*0.028*	*0.045*	*0.0480*
Observations	27,605	27,535	11,572	15,862	27,605	27,535	11,572	15,862
R-squared	0.318	0.426	0.099	0.322	0.314	0.424	0.098	0.317
Municipalities, number	288	288	276	285	288	288	276	285
F	581.8	856.6	32.24	959.9	561.4	849.5	32.42	958.4
Partial R-squared					0.0892	0.0956	0.102	0.0941
Kleinbergen-Paap statistic					2529	2573	1456	1692
OIR test					0.17	0.17	0.189	0.0396
Endogeneity test					4.02e-05	0.000449	0.227	0.00204
First stage								
Dependent variable by year					0.002	0.003	0.001	0.003
					0.023	*0.025*	*0.036*	*0.030*
Dependent variable by area					0.006	0.005	0.006	0.006
					0.026	*0.029*	*0.042*	*0.041*
Instrument by year					0.984***	0.999***	0.997***	1.000***
					0.020	*0.020*	*0.026*	*0.024*

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 clustered at the municipal level and shown in italics.

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 15, 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. 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 A4: 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***	0.051***	0.058***	0.043***	-0.002	0.001	0.028	-0.010
	0.007	*0.006*	*0.010*	*0.009*	*0.011*	*0.013*	*0.024*	*0.020*
Dependent variable by year	0.803***	0.770***	0.661***	0.838***	0.807***	0.775***	0.665***	0.841***
	0.025	*0.026*	*0.038*	*0.036*	*0.026*	*0.026*	*0.038*	*0.036*
Dependent variable by area	0.862***	0.805***	0.776***	0.787***	0.868***	0.813***	0.778***	0.798***
	0.020	*0.023*	*0.048*	*0.044*	*0.020*	*0.023*	*0.048*	*0.044*
Observations	27,605	27,535	11,572	15,862	27,605	27,535	11,572	15,862
R-squared	0.078	0.108	0.104	0.110	0.076	0.106	0.103	0.108
Municipalities, number	288	288	276	285	288	288	276	285
F	262.7	139.0	53.47	100.3	262.6	137.1	52.44	95.94
Partial R-squared					0.0890	0.0952	0.101	0.0934
Kleinbergen-Paap statistic					2597	2632	1463	1710
OIR test					0.454	0.454	0.821	0.318
Endogeneity test					0.000100	0.000131	0.174	0.00869
First stage								
Dependent variable by year					-0.002	-0.003	0.002	-0.004
					0.021	*0.022*	*0.031*	*0.027*
Dependent variable by area					0.008	0.007	0.012	0.005
					0.025	*0.026*	*0.049*	*0.039*
Instrument from probit					0.983***	0.999***	0.995***	1.000***
					0.019	*0.019*	*0.026*	*0.024*

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 clustered at the municipal level and shown in italics.

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 was the case, the results on labor force participation may be indirectly driven by the program's effect on household composition. In table 16, 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. Using OLS, the effect is negative and mostly statistically significant. Probably 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, except in the first column. We therefore rule out that household composition may be the driver behind our prior results.

Table A5: 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	-0.046***	-0.014**	-0.005	-0.019***	0.035**	0.006	0.025	-0.011
	0.010	*0.006*	*0.007*	*0.007*	*0.015*	*0.018*	*0.023*	*0.020*
Dependent variable by year	0.783***	0.342***	0.332***	0.354***	0.779***	0.341***	0.331***	0.354***
	0.029	*0.023*	*0.029*	*0.026*	*0.028*	*0.023*	*0.029*	*0.026*
Dependent variable by area	0.898***	0.368***	0.356***	0.372***	0.905***	0.369***	0.358***	0.373***
	0.018	*0.021*	*0.030*	*0.025*	*0.018*	*0.021*	*0.031*	*0.025*
Observations	27,605	27,535	11,572	15,862	27,605	27,535	11,572	15,862
R-squared	0.070	0.578	0.606	0.559	0.065	0.578	0.605	0.559
Municipalities, number	288	288	276	285	288	288	276	285
F	295.2	256.1	250.4	173.2	285.0	254.0	254.5	170.7
Partial R-squared					0.0893	0.0963	0.102	0.0948
Kleinbergen-Paap statistic					2602	2683	1514	1744
OIR test					0.626	0.626	0.0957	0.711
Endogeneity test					3.51e-07	0.259	0.158	0.704
First stage								
Dependent variable by year					0.003	0.002	0.004	0.000
					0.019	*0.021*	*0.026*	*0.029*
Dependent variable by area					-0.006	-0.003	-0.006	-0.002
					0.018	*0.019*	*0.029*	*0.028*
Instrument from probit					0.983***	0.998***	0.996***	0.999***
					0.019	*0.019*	*0.026*	*0.024*

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 clustered at the municipal level and shown in italics.

Appendix II: 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 appendix, 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). 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. This should not come as too much of a surprise, given municipalities' incentives and ability to manipulate the Sisben (see Camacho and Conover 2011). 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.

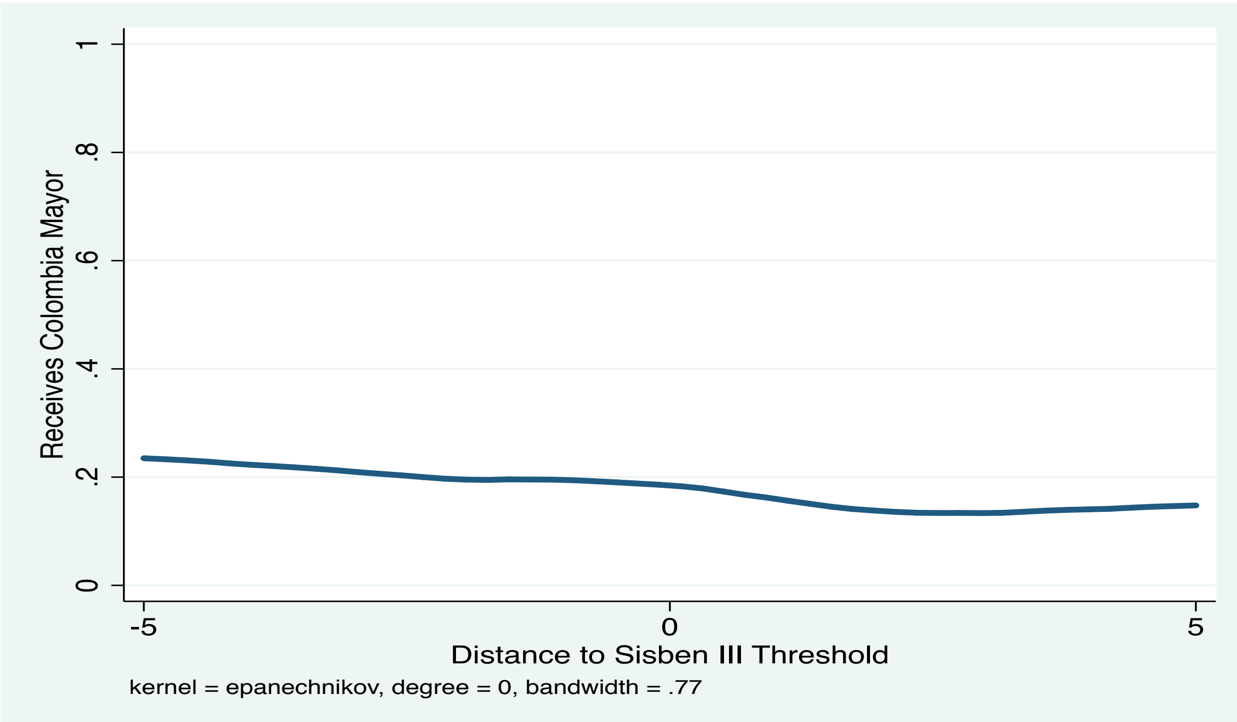
In Table 1A, we present the results of a simple RD-inspired spline regression with standard errors clustered at the municipal level. We regress the binary outcome variable of a household receiving Colombia Mayor according to whether it is Sisben eligible (another binary variable) and measure the absolute 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. In addition, figure 1A 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. We ran a simple kernel regression of program receipt on the Sisben score in the +/- 5 point interval around the eligibility cutoff. Figure 1A confirms the results shown in the table, that is, there is no significant change around the threshold.

Table A6: 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 italics

Figure A1: 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 2A 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 A7: 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.[17] 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

[17] 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, http://www.dane.gov.co/index.php/poblacion-y-demografia/proyecciones-de-poblacion.) The total number of age-eligible residents per municipality has been constructed by assuming a constant age distribution within each group.

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 2A 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.

This lack of municipality- and year-specific variations can be highlighted by a simple regression analysis. Table 3A 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 A2: Rollout of Colombia Mayor across 298 ENCV 2009–13 Municipalities

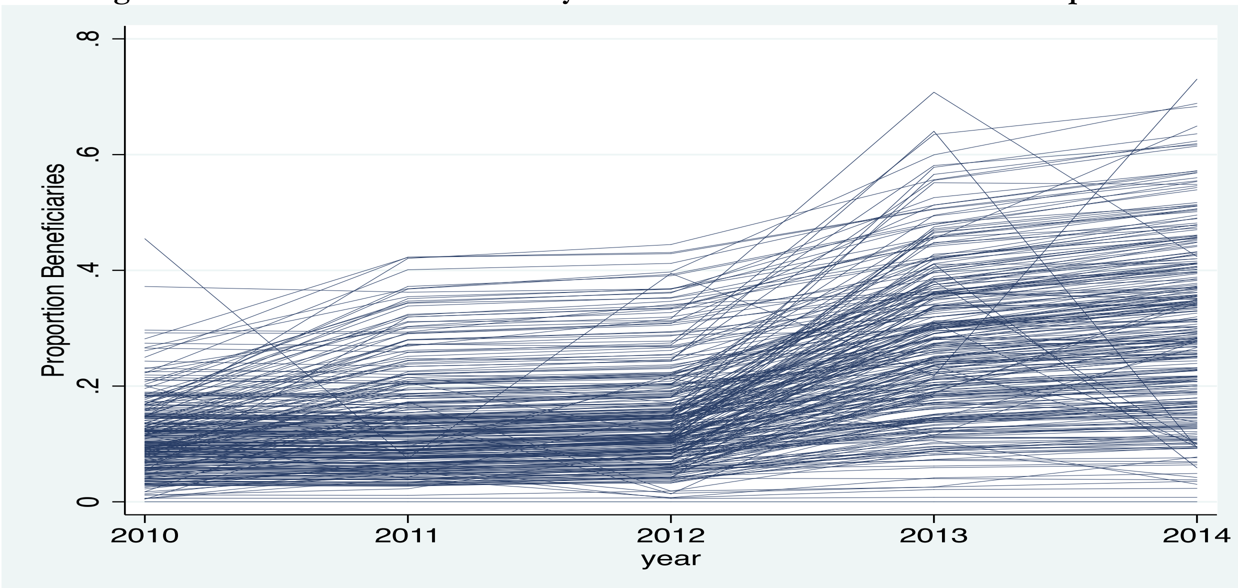


Table A8: Regression of Program Receipt on the Municipal Rollout Level, Controlling for Municipal and Year Fixed Effects

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 in italics.

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.

Appendix III: Results for Control Function Approach on IV

In this appendix, we present results for an alternative approach to our instrumental variable strategy. This addresses the concern that program roll-out at the levels of aggregation used for the instrumental variables may have been directly driven by labor market outcomes. We employ a control function approach on our two instrumental variables by regressing them first on all the observed labor market outcomes in the data, their squared terms, and also their pairwise interaction terms. We then use the residuals from that regression as our new IVs. The rational behind this methodology is that we want to purge our IV from all possible correlation with labor market outcomes. By including the squared and interaction terms, we extent this purge to possible non-linear relationships. We use the averages of beneficiaries over the outcomes presented in table 9 in the main text, leaving out, where necessary, one category as baseline. Additionally, we divide labor force participation up into being occupied and being unemployed. A number of the resulting pairwise interaction terms are subsequently dropped to avoid perfect collinearity.

In table A4 below we show the results from this exercise for the whole sample and the six subgroups from table 8. It has to be kept in mind that by using the residuals we end up with a considerably weaker instrument than we had before. Their standard deviation drops from 0.18 and 0.17, to 0.12 and 0.14, respectively. Referring to table A4, the statistics capturing instrument strength are reduced accordingly. The Kleinbergen-Paap statistic is in some cases cut in half, and the partial R-squared is also considerably reduced (comparing table A4 to column 6 in table 7 and to table 8). Reduced strength in the instruments translates into larger standard errors in the estimations. Comparing the results below to those in the paper it, the point estimates for the whole sample and for younger males are almost identical. The results for beneficiaries 70 years of age or older continue being statistically insignificant. The larger standard errors reduce the level of statistical significance for males younger than 70, our principal result of interest, though the point estimate is even slightly higher than before. For women of the same age, results lose their marginal significance (before at the 10%-level) and the point estimates are slightly lower. However, the bottom line is that this approach does not alter our principal findings.

Table A9: Results for estimation with control function.

	All	*Younger than 70*			*70 or older*		
		All	*Male*	*Female*	*All*	*Male*	*Female*
Colombia Mayor	0.072***	0.054	0.085*	0.055	-0.014	0.053	-0.022
	0.020	0.036	0.045	0.048	0.027	0.045	0.036
Dependent variable by year	0.559***	0.715***	0.362***	0.746***	0.474***	0.527***	0.339***
	0.029	0.042	0.065	0.058	0.045	0.066	0.051
Dependent variable by area	0.672***	0.822***	0.488***	0.773***	0.678***	0.677***	0.354***
	0.025	0.038	0.056	0.075	0.046	0.071	0.069
Observations	27,535	15,109	5,436	9,226	12,159	5,667	6,262
R-squared	0.319	0.114	0.317	0.122	0.194	0.336	0.069
Municipalities, number	288	274	231	260	282	259	264
F	408.7	212.1	489.4	97.06	93.75	109.7	22.48
Partial R-squared	0.0641	0.0713	0.0833	0.0692	0.0615	0.0618	0.0602
Kleinbergen-Paap statistic	1285	846.0	422.4	635.7	531.4	284.0	420.0
OIR test	0.119	0.224	0.528	0.0382	0.987	0.426	0.587
Endogeneity test	9.59e-05	0.0105	0.107	0.311	0.760	0.249	0.769
First stage							
Dependent variable by year	0.005	0.004	0.002	0.003	0.007	0.007	0.003
	0.041	0.041	0.062	0.047	0.060	0.071	0.079
Dependent variable by area	-0.002	0.003	0.003	-0.000	-0.005	-0.003	-0.006
	0.038	0.045	0.069	0.053	0.060	0.078	0.091
Instrument from probit	1.001***	1.013***	1.007***	0.996***	0.995***	0.994***	0.980***
	0.028	0.035	0.049	0.04	0.043	0.059	0.048