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

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.

JEL Codes: H55, J08, J26, O15
Keywords: Pensions, labor force participation, Colombia

Noncontributory pensions are becoming increasingly popular within the larger context of social protection systems. The noncontributory label highlights the fact that these pensions are funded not by individuals' payroll contributions—as in the case of traditional social insurance—but through other general fiscal revenues. This term is somewhat imprecise, however, as most beneficiaries of noncontributory pensions have contributed to the economy, including but not limited to paying indirect taxes.[1] Noncontributory

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1. Barrientos and Lloyd-Sherlock (2002).

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 unconditional transfers. In Latin America alone, twelve out of twenty-six countries have already implemented either a noncontributory pension or a complementary system.[2] These programs are usually directed toward older individuals, with an emphasis on residents of rural areas. Eligible individuals can receive as little as U.S. $0.10 a day in Honduras to almost U.S. $20.00 a day in Trinidad and Tobago, according to the Inter-American Development Bank.[3] Because these programs do not require a contribution to a specific fund, the costliness of their implementation and their sustainability in sometimes politically fragile developing countries have been criticized.[4] Yet unconditional cash transfers are garnering increasing attention. After the grand entrance of conditional cash transfer programs in Latin America in the late 1990s—such as *Bolsa Família* in Brazil and *Progresa/Oportunidades* (now *Prospera*) in Mexico—recent studies have questioned their conditionality aspect, turning the spotlight to unconditional programs, including noncontributory pension schemes.

This paper evaluates the effects of one such scheme, the *Colombia Mayor* noncontributory pension program. This program combines two noteworthy features: the associated benefit is quite small, and the age of eligibility is among the lowest for such programs in the region. Our analysis employs an overidentified instrumental variable estimation.[5] 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. 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, a cash transfer could potentially lead to an increase in labor force participation. Using data from the *Colombia Mayor* program,

2. Bosch and Guajardo (2012).
3. Bosch, Melguizo, and Pagés (2013).
4. Bosch and Guajardo (2012); Johnson and Williamson (2006); Rofman, Apella, and Vezza (2015).
5. The reason why other identification approaches proved ineffective is explained in detail in the online appendix to this paper (economia.lacea.org/Forthcoming%20papers/Pfutze_Rodriguez_Final_Appendices.pdf).

this paper tests 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 for conducting 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 fifties and sixties). 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. The next section provides an overview of literature relevant to the study. The paper then offers a more detailed motivation for the analysis, including a theoretical motivation. This is followed by an in-depth description of the *Colombia Mayor* program, after which we describe the data used, discuss our empirical strategy, and present our results. The final section concludes.

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 macroeconomic side, transfer programs have been studied as a potential mechanism for reducing inequality and poverty.[6] Other literature looks at the effect of these transfers on well-being at a more microeconomic level.[7] 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. Like any transfer, noncontributory pensions may influence the incentives of individuals to offer labor. Transfers can also have an effect on investment and saving, potentially freeing up resources to spend in productive activities. For the purpose of this paper, we are particularly

6. See, for example, Gasparini and Lustig (2011, p. 17); Lustig, Pessino, and Scott (2013); Lustig and Pessino (2014); Barrientos (2003, 2005); and Olivera and Zuluaga (2014).

7. See, for example, Salinas-Rodríguez, Torres-Pereda, and others (2014); Martínez (2004); and Duflo (2003).

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. Pension programs in Mexico led to a decline in overall labor supply, although the magnitude of the decline varies considerably: Aguila, Kapteyn, Robles, and Weidmer report a decrease of 4.3 percentage points in Yucatan, Mexico (work for pay), while Galiani, Gertler, and Bando 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.[8] Some studies find more nuanced effects. For the same *Adultos Mayores* program, Juárez and Pfutze show a reduction of similar magnitude only among male recipients, mainly driven by poorer beneficiaries.[9] In Mexico City, Juárez finds that the effect of a nutrition transfer among seniors (*Pensión Alimentaria para Adultos Mayores*) depends on household composition and demographics.[10] Past the age of sixty, men who live in households with qualifying members retire early. However, individuals in younger age ranges increase their labor supply if they live with an eligible man but decrease it if they live with an eligible woman. Maluccio and Flores 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.[11] Taking advantage of the initial randomized rollout of Mexico's *Progresa* program, Skoufias and di Maro do not find any significant impact on labor force participation or leisure time.[12] For the case of Chile's antipoverty program *Chile Solidario*, Galasso does not find any consistent results for labor market outcomes.[13] More recently, Banerjee, Hanna, Kreindler, and Olken review the results from seven randomized controlled trials of cash transfers (both conditional and unconditional) conducted in six countries.[14] Their reanalysis of the data from these studies reveals no impact of the transfer on either

8. Aguila, Kapteyn, Robles, and Weidmer (2012); Galiani, Gertler, and Bando (2014).
9. Juárez and Pfutze (2015).
10. Juárez (2010).
11. Maluccio and Flores (2005).
12. Skoufias and di Maro (2006).
13. Galasso (2006).
14. Banerjee, Hanna, Kreindler, and Olken (2017).

the number of hours worked or the propensity to work, for either men or women.

Other research finds positive effects of transfers, both conditional and unconditional, on labor supply. Soares, Ribas, and Osório evaluate Brazil's *Bolsa Família* and find that it raised female labor force participation by 4.3 percentage points.[15] For the same program, using aggregate data on Brazilian municipalities, Foguel and Paes de Barros find that labor force participation increased by 2–4 percentage points for men.[16] Results for women are also positive, yet not significant and smaller in magnitude. Salehi-Isfahani and Mostafavi-Dehzooei analyze the impact on the labor supply of an unconditional cash transfer program in Iran.[17] 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 on 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, which stand to benefit from the transfer.

For Colombia, Barrientos and Villa analyze the impact on the labor market of *Familias en Acción*, a conditional cash transfer program.[18] Using a regression discontinuity design and a large panel data set (based on two waves of SISBEN household data collected in 2006 and 2010), the authors find large and positive effects on activity rates when the sample is restricted to single adult households with children aged zero to six years.[19] 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*, several studies find positive effects on adults in the labor market.[20] An evaluation of *Familias en Acción* for the Institute for

15. Soares, Ribas, and Osório (2010).
16. Foguel and Paes de Barros (2010).
17. Salehi-Isfahani and Mostafavi-Dehzooei (2017).
18. Barrientos and Villa (2013).
19. For their working data set, the authors include only households in urban areas that joined the program in 2007. To confirm that their data set does not include households that participated 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. The Colombian System of Identification of Potential Social Program Beneficiaries (SISBEN) is a proxy means test index widely used for targeting social programs in Colombia.
20. Castañeda and Trujillo (2017).

Fiscal Studies finds positive effects on labor force participation for males and females in urban areas and for males in rural settings.[21] 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 (November 2005 to April 2006). Using the same data and methodology, and also looking at adults aged eighteen and over, a joint evaluation by the DNP, Acción Social, the IADB, and the World Bank finds similar results on labor force participation. No significant effect was found on hours worked.[22]

Using the program's baseline and follow-up surveys and a mean differences analysis, CNC finds that under the program's saving modality, the occupation rate of beneficiaries increased by 6.5 percentage points in cities other than Bogotá (and 4.1 percentage points overall).[23] There was also a positive effect on the inactivity rate, which decreased by 3.24 percentage points in the treated group, and on the economically active population, which increased by 3.1 percentage points. Econometría Consultores and SEI find that the probability of having a formal job for women aged eighteen to twenty-six years in rural areas increased by 2.5 percentage points as a result of the program, while no significant effects were found on occupation or job quality.[24] The study uses the program's baseline survey (second half of 2002) and the third follow-up survey (November 2011 to February 2012) and a difference-in-differences model. Espinosa and Nanclares use a regression discontinuity design and data from Colombia's System of Identification of Potential Social Program Beneficiaries (SISBEN) III at the urban level; they find that the program increased participation in the labor market by 3.5 percentage points.[25] Conversely, they find no significant effects on the 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 looks at the specific impact that cash transfers can have on economic activity, such as the likelihood of individuals participating in farm work. De Hoop, Groppo, and Handa find that unconditional cash transfer

21. IFS, Econometría Consultores, and SEI (2007).

22. DNP, Acción Social, IADB, and World Bank (2008).

23. CNC (2011). *Familias en Acción* works under two modalities in urban areas: incremental and saving. The affected cities were Barranquilla, Yopal, Montería, Pasto, Pereira, and Villavicencio. There was no effect in Bogotá.

24. Econometría Consultores and SEI (2012).

25. Espinosa and Nanclares (2016).

programs in Malawi and Zambia led to an expansion of household micro-entrepreneurial agricultural activity.[26] The net participation in economic activities was unaffected, given a corresponding reduction in the likelihood that adults performed paid work. This shift (from paid work to household farm work) suggests that investments in agricultural assets increased relative returns to agricultural labor. In Zambia, Prifti, Estruch, and others find that a conditional cash transfer targeted at households with children under the age of three years (the Child Grant Program, CGP) caused a shift from agricultural wage labor to own-farm labor.[27] Overall, transfers have no work disincentives on farm households. Handa, Natali, and others find that the CGP increased participation in nonfarm enterprises by 14 percentage points and increased nonfarm enterprise revenues by 125 percent, or 0.33 SD.[28] In a different line, Skoufias, Unar, and González-Cossío exploit the experimental design of Mexico's Food Support Program (*Programa de Apoyo Alimentario*, PAL) and find no significant effects on total labor market participation.[29] However, their estimates show that while the PAL had a significant negative effect on male participation in agricultural activities, this translated into a shift toward participation in nonagricultural activities. They argue that the PAL provides partial insurance (reducing downside risks) for food consumption, which allows program beneficiaries to allocate less time to agricultural production and more to higher-risk (and higher-reward) nonagricultural activities. Finally, Martínez 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.[30] 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 focuses on conditional transfer programs. Gertler, Martínez, and Rubio-Codina find

26. De Hoop, Groppo, and Handa (forthcoming). The two programs are targeted at "labor-constrained" households, that is, households with high dependency ratios. Results are reported looking at the sample of households with children aged six to fifteen, 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.

27. Prifti, Estruch, and others (2017).

28. Handa, Natali, and others (2018).

29. Skoufias, Unar, and González-Cossío (2013).

30. Martínez (2004).

that beneficiaries invested part of their cash transfers from *Oportunidades* in productive assets.[31] 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 find that occupational choices for treated households are more responsive to the amount of transfers expected in the future than to the amount of transfers currently received.[32] This, they suggest, shows that the program enhances willingness to bear risk, rather than just easing liquidity constraints. For *Bolsa Família*, Ribas estimates that the share of entrepreneurs among male beneficiaries with low educational attainment has grown.[33] However, the study questions the causal link between relieving financial constraints and the higher levels of investment because of the observed increase in private transfers among households.

For noncontributory pensions, there is evidence supporting their contribution to relieving credit constraints, as in the case of South Africa.[34] The presence of a pensioner in a household may also affect employment, including by facilitating labor migration. Ardington, Case, and Hosegood find that cash transfers to the elderly lead to increased employment among prime-working-age adults in South Africa.[35] They attribute the impact of the pension to easing liquidity constraints for migration and job search and increasing the availability of elderly household members to care for small children. Posel, Fairburn, and Lund 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.[36] Beyond the effect on migration, Alonso, Amuedo-Dorantes, and Juárez study the impact on household saving of Mexico's noncontributory pension schemes at the federal level—the *70 y Más* program—as well as state-level programs.[37] Their results show that the federal pension lowered the saving rates of certain age groups, while the combination of both programs had no effect on saving. Their findings also suggest that for some age groups, transfers had positive effects in terms of investment

31. Gertler, Martínez, and Rubio-Codina (2012).
32. Bianchi and Bobba (2013).
33. Ribas (2014).
34. Berg (2013).
35. Ardington, Case, and Hosegood (2009).
36. Posel, Fairburn, and Lund (2006).
37. Alonso, Amuedo-Dorantes, and Juárez (2016).

in human capital and in durable and financial goods. On the other hand, the randomized controlled trial conducted by Haushofer and Shapiro sheds light on how regular unconditional transfers, as opposed to lump-sum transfers, are better suited to encourage long-term investment.[38] As shown in more detail in the next sections, our paper builds on this literature, providing evidence that a small income stream allows beneficiaries to diversify economic activity, including diversifying into 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, beneficiaries appear to have shifted toward informality.[39] There might be evidence of less demand for formal labor among individuals receiving unconditional cash transfer benefits: Bosch and Campos-Vázquez 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."[40] Similar results have been documented elsewhere. In Colombia, after the expansion of a public health insurance program, informal employment rose by two to five percentage points.[41] In Argentina, Bosch and Guajarado find that the *Moratorium*, a social pension program, led women working in formal jobs to retire earlier than they would have done otherwise.[42] However, Azuara and Marinescu, 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.[43] Untangling the demand side and the 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

38. Haushofer and Shapiro (2013).

39. Knox and Campos-Vázquez (2010); Aterido, Hallward-Driemeier, and Pagés (2011); Azuara and Marinescu (2013); Duval-Hernández and Smith Ramírez (2011).

40. Bosch and Campos-Vázquez (2010, p. 1).

41. Camacho, Conover, and Hoyos (2014).

42. Bosch and Guajarado (2012).

43. Azuara and Marinescu (2013).

mostly benefit families with children and pay larger transfers, is also present for beneficiaries in their fifties and sixties when given a fairly low payment. The effect disappears, however, once beneficiaries reach their seventies. 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 employees for the latter.

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 U.S. $23 at the time). This amounts to little more than U.S. $0.75 a day at currency exchange rates or around U.S. $1.50 a day in purchasing power parity (PPP). By comparison, similar programs pay around U.S. $7.00 a day in PPP in Argentina, U.S. $11.00 a day in PPP in Brazil, and U.S. $6.50 a day in PPP in Chile. Even the corresponding benefit in more resource-constrained Bolivia is higher, paying around U.S. $2.00 a day in PPP.[44] The second notable characteristic of the program is that the minimum age to qualify is defined as three years below the stipulated age for the general public pension system. This yields a minimum age of fifty-two for women and fifty-seven for men, the lowest corresponding ages of eligibility in the region (in most other countries, it is sixty-five or seventy). Starting in January 2014, after the time period under study, all minimum pension ages were increased by two years.

At first glance, 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, in light of the paltry amount of the benefit

44. Bosch, Melguizo, and Pagés (2013).

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 thus constitutes an additional reliable income stream. Such a stream could have two effects: (1) it may act as a form of insurance, allowing beneficiaries to engage in somewhat risky economic activities (as interpreted by Skoufias, Unar, and González-Cossío), or (2) it could alleviate liquidity constraints preventing beneficiaries from pursuing lucrative business or employment opportunities.[45] 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 in the number of hours worked. One would expect the increase to be concentrated among activities that require seed capital, such as small-scale commerce, food preparation, or agriculture. Nevertheless, as with other transfers, there are transaction costs—such as the cost of transportation—that could prevent potential beneficiaries from taking advantage of such employment opportunities.

The program's characteristics thus make *Colombia Mayor* an ideal setting in which to test these effects. Beneficiaries do not have to leave the labor force, and they 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 up-front 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 up-front investment. The available data, described below, do not allow a longitudinal analysis, so we use 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 dropout rate. However, insofar as 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 is restricted.

45. Skoufias, Unar, and González-Cossío (2013).

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.[46] 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 specially established centers that cater to the needs of the elderly population and are managed and cofinanced by the municipalities.[47]

There are three main criteria for program eligibility. 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 fifty-two years among women and fifty-seven years among men. (These age thresholds were subsequently raised by two years.)

The second criterion is that the beneficiary's household must not score above a certain threshold in Colombia's system for the identification of potential beneficiaries of social programs (SISBEN). The SISBEN score represents an estimate of the living conditions of households registered in the system. It is based on information on households collected through a 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 major modifications, such that the current, third version is usually referred to as SISBEN III. Each social program

46. A household is defined to be in extreme poverty if its income lies below the extreme poverty line or if it presents multidimensional poverty as defined by the National Administrative Department of Statistics (DANE).

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

is associated with a unique maximum score to identify beneficiaries. The maximum scores usually differ depending on whether a household resides in one of the country's fourteen major metropolitan areas, in other urban areas, or in the countryside.[48] 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 fourteen 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, respectively.

The third criterion revolves around income. It establishes that, in the case of single-member households, a beneficiary's income may not exceed half the 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 workers in the formal economy. If they are formal economy workers, income is identified from their tax identification number (RUA). Nonformal workers include informal sector workers and unemployed people.

In the data described below, we are able to observe how all three criteria would function, but we consider a potential beneficiary as eligible for the program if the age and SISBEN score requirements are met. We 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, because of 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.[49] Lending support to these concerns, our own data show only a limited overlap between the SISBEN scores and

48. 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 fourteen main cities and other urban areas (from 0 to 43.63) and for rural areas (from 0 to 35.26).

49. In the case of potential beneficiaries who are formal sector workers, it is possible to verify that their income lies below the stipulated threshold by using the administrative tax records mentioned (RUA). This mechanism, however, is not available for informal sector workers or the unemployed.

households that are eligible based on income. The survey data employed, and described in more detail below, are designed to allow 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 sixty-five years of age and older. Every year, each municipality is given a certain number of slots for new beneficiaries. If the number of eligible petitioners exceeds the number of available slots, the prioritization strategy is used to determine who receives the benefit.[50]

Finally, *Colombia Mayor* does not operate in a vacuum but rather is one of many other social programs running at the national level. Consequently, other existing programs could be affecting adults' behaviors with the same cut-off point. To explore this issue, we consider all programs, including those that, while not directed toward elderly individuals, may target households that include *Colombia Mayor* beneficiaries, as in the case of adults who do not live by themselves, who have dependents, or who are the household head. For the period under study, we find that *Más Familias en Acción*, *Jóvenes en Acción*, and *Red Unidos* may be targeting households that include beneficiaries of *Colombia Mayor*. Of these programs, the first two are conditional monetary transfers aimed at improving the health and educational outcomes of children under eighteen (the former) and increasing the competencies and work skills of youth (the latter). The third, *Red Unidos*, provides integrated social services. All three programs target households living in poverty and vulnerability.

50. The allocation of resources at the municipal level is determined according to the number of elderly individuals classified in SISBEN levels 1 and 2 as a share of 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. Based on the availability of resources, a "prioritized" population is selected according to specific prioritization criteria (including age, disability, and number of 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 is updated in the main database. The prioritization process is carried out using the database to assign a score to each prioritized individual, which will determine the order that person occupies 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 who registered first, disability status, and so forth).

Because of the thresholds of SISBEN scores that are used to determine eligibility, there is a segment of households for which programs might overlap.[51] For instance, as the score threshold for *Más Familias en Acción* is lower than that for *Colombia Mayor*, some households might be beneficiaries of both. This would obtain, for example, in the case of a household living in poverty and consisting of an elderly individual and children. The same could happen with the *Jóvenes en Acción* program for a household living in poverty and consisting of an elderly individual and a youth attending postsecondary school.

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 National Quality of Life Survey (ENCV), a household survey conducted by Colombia's National Administrative Department of Statistics (DANE). 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 National Planning Department (DNP) provided us with these scores, which are based on survey responses (that is, they are not households' actual scores). The other frequent large-scale and nationally representative survey is the monthly National Integrated Household Survey. While this survey would provide us with a much larger sample and covers almost all the municipalities in the country, it lacks the breadth of observed variables available in the ENCV. Most important, it does not capture whether a household receives the *Colombia Mayor* benefit.

51. The required score for a family to be a beneficiary of *Más Familias en Acción* is between 0 and 30.56 for fourteen cities, from 0 to 32.20 for other urban areas, and from 0 to 29.03 for rural areas. Eligibility for the *Colombia Mayor* program requires a SISBEN score of between 0 and 43.63 for the country's fourteen main 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 fourteen main 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.

The basic empirical model can be represented as follows:

$$(1) \qquad Y_{itm} = \alpha + \beta \mathrm{CM}_{itm} + \gamma \bar{Y}_{tm} + \delta \bar{Y}_{mg} + \lambda \mathbf{X}_{itm} + \varepsilon_m + e_t + u_{itm},$$

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 is 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 (e_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 labor force participation) but rather by selection into the program. It is unclear ex ante which unobserved individual- or household-specific factors determine enrollment in the program, but, to the extent that these factors also (partially) determine the outcome variable, the estimate of β will be biased. Insofar as 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 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:

(2) $$\mathrm{IV}^1_{tm} = \frac{1}{N_{tm}} \sum_{i \in N_{tm}} \omega_i D_i$$

and

(3) $$\mathrm{IV}^2_{mg} = \frac{1}{N_{mg}} \sum_{i \in N_{mg}} \omega_i D_i ,$$

where the g subscript represents the specific geographic area, D is a specific dummy variable indicating program participation in an eligible individual's household, and ω is 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 is simply that program rollout 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 geographic area will then increase the probability of knowing about the program and becoming a beneficiary oneself. This kind of reasoning is found in the migration literature, and the proportion of migrants at some geographic 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. The differential rollout could be driven by unobservable submunicipal characteristics that also affect the outcome, or, alternatively, similar spillover effects between households could be present for the outcomes. To address these concerns, all specifications include municipality-year and submunicipal time-invariant averages of the respective outcome variable that are constructed in the same manner as the instruments. These are the terms $\bar{Y}_{tm}$ and $\bar{Y}_{mg}$ in expression 1.

To fix ideas, we can think of two additional error terms (that is, unobserved factors) in our empirical model: ξ_{tm} (municipality-year specific) and υ_{mg} (submunicipal), which jointly determine program rollout 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

$$Y_{itm} = \alpha + \beta \text{CM}_{itm} + \xi_{tm} + \upsilon_{mg} + \lambda \mathbf{X}_{itm} + \varepsilon_m + e_t + u_{itm}.$$

Taking the appropriate averages over this empirical model yields

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

and

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

where $\tilde{Y}_\iota$ 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 us 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, $\overline{\tilde{Y}_{tm}}$ and $\overline{\tilde{Y}_{mg}}$. 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 thus could act as omitted variables. Last, 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, insofar as we control for ξ_{tm} and υ_{mg}. 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 rollout. In this case, including the averages of the outcome variable at the municipality-year and municipality-area levels provides an equally direct way to control for this possibility.

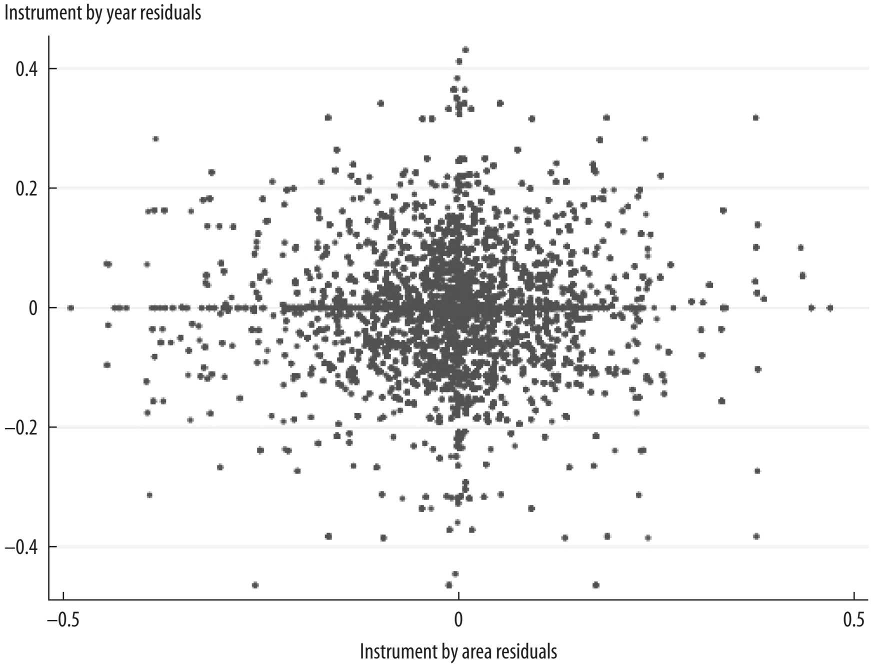
This is a good point to discuss the precise causal chain behind our estimation strategy. A faster or higher level of program rollout increases the probability that observation *i* receives the benefit. Because the rollout is measured at two different aggregate levels, there is no concern that characteristics at the individual level may act as omitted variables. The inclusion of the corresponding outcome variable averages controls for any direct effect on program rollout, unobserved characteristics at the same levels of aggregation, and spillover effects in the outcome. The instrumental variables thus isolate the partial effect of aggregate rollout 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 overidentifying restrictions (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 a set of municipality, year, and area (that is, locality-type) fixed effects, which we control for in all our specifications. The two instruments are almost perfectly orthogonal. The coefficient of correlation between the two residuals is 0.0148.

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 one. Unfortunately, these predicted values cannot be used to substitute for treatment in the model of interest because of 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.[52] We show the results and most

52. Wooldridge (2010) offers a detailed discussion of the procedure, which has also been used in many applied studies (for example, Adams, Almeida, and Ferreira, 2009).

FIGURE 1. Correlation of Instrument Residuals after Controlling for Municipality, Year, and Area Fixed Effects



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, which constitute 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 at

TABLE 1. List of Included Control Variables

Control variable	*Description*
Other urban Rural	Categorical variables indicating two of the three geographic 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, which 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 Other adults Minors	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 years.
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.

least fifty-two years old or a man at least fifty-seven years old. 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 observed in only one round, two in two rounds, twenty-five in three rounds, seventy-six in four rounds, and sixty-nine in all five rounds. The total number of municipalities sampled each year is 121 in 2010 (of which only seventeen are never repeated in the following years), 220 in 2011, and 171 in 2012–14.

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 (namely, other urban, rural, age, SISBEN score, and woman) are included in all specifications, while the remainder are additional control

TABLE 2. Descriptive Statistics on Labor Force Participation

Sample	*No. observations*	*Mean*	*Standard deviation*	*Minimum*	*Maximum*
Full sample	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

variables. The former directly determine treatment eligibility; their omission would thus result in a clear omitted-variable bias.

Tables 2 through 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, which we analyze separately. Labor force participation is determined by a battery of questions in the ENCV but is defined here as either working at least one hour per week or 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 seventy years of age and lower among individuals seventy 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.

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 seventy years of age, the rate is 20 percent,

TABLE 3. Descriptive Statistics on Receipt of the *Colombia Mayor* Benefit

Sample	*No. observations*	*Mean*	*Standard deviation*	*Minimum*	*Maximum*
Full sample	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

whereas among household members seventy years of age or older, it is 41 percent. These rates are fairly consistent between men and women.

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 fourteen 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 sixty-nine years old, and 58 percent are women (mainly because of the lower eligibility age for women). Around half the potential beneficiaries are married; 24 percent are widowed; and 14 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 female. About two-thirds of the potential beneficiaries have finished at least primary education, but the number who have completed 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.

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 (1) the size of the workplace, (2) the location of work, (3) the type of (self-)employment, and (4) the sector of occupation. In the years 2010, 2011, and 2014, it also included (5) a battery of detailed consumption and expenditure questions. We are particularly interested in whether the

TABLE 4. Descriptive Statistics of Control Variables

Variable	*No. observations*	*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	27,724	1.66	0.68	1	5
Other adults	27,724	0.98	1.14	0	10
Minors	27,724	0.99	1.36	0	12
Women	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

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 at 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 or her monthly labor income and number of hours worked. For the last item, no information was collected in the 2012 round, which is therefore excluded from the analysis for that outcome. 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 of whether any such expenditure has taken place and the total amount spent.[53] Ideally, we would want to be able to observe expenditures on agricultural inputs. Unfortunately, such data are 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 make before starting 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. A comparatively large share of each sample works alone on agricultural land, rivers, or at 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.

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 present only 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 of labor force participation. Both instruments are highly significant.

53. 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) and interurban transportation (past year). For the amount spent, the figure for the latter is put into weekly terms.

TABLE 5. Detailed Labor Market Outcomes: Means

		Younger than 70			70 or older		
Indicator	*Full sample*	*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 and 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 and commerce	0.07	0.09	0.10	0.09	0.04	0.05	0.04
Service and 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 (COL$, monthly)	138,951	194,111	307,107	124,249	69,406	123,472	20,601
Hours worked	12.41	16.29	26.08	10.24	7.52	12.60	2.94
Expenditures							
Transport	0.31	0.33	0.32	0.34	0.29	0.30	0.29
Transport amount (COL$, annual)	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 (COL$, annual)	27,308	30,081	30,263	29,969	23,729	21,389	25,835
Shoes	0.19	0.20	0.19	0.21	0.17	0.16	0.18
Shoes amount (COL$, annual)	12,805	14,086	13,665	14,344	11,152	10,191	12,016

In terms of magnitude, a one-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 (since treatment is observed only 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

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		0.042***	0.073***	0.014	0.052***	0.031**
		(0.012)	(0.016)	(0.018)	(0.018)	(0.014)
Other adults		0.039***	0.062***	0.008	0.063***	0.016
		(0.014)	(0.015)	(0.026)	(0.020)	(0.016)
Minors		0.069***	0.066***	0.059**	0.091***	0.050***
		(0.016)	(0.016)	(0.023)	(0.021)	(0.019)
Women		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)
No. observations	27,605	27,535	15,109	12,159	11,572	15,862

Notes: The table illustrates a probit model on dependent variable indicating that at least one household member receives *Colombia Mayor* benefit. Parameters show estimated marginal effects at mean. Standard errors clustered at the municipal level are in parentheses.

* Statistically significant at the 10 percent level.

** Statistically significant at the 5 percent level.

*** Statistically significant at the 1 percent level.

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 seventy 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.

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 geographic 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.

The bottom of this table and of the following tables includes a number of additional statistics. While those shown only under the OLS specification (namely, the 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 and the Kleibergen-Paap statistic—are weak instrument tests.[54] 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 Kleibergen-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. A higher partial R^2 results in a lower asymptotic bias in the IV estimator relative to the OLS if the exclusion

54. On the partial R^2, see Shea (1997). On the Kleibergen-Paap statistic, see Stock, Wright, and Yogo (2002).

TABLE 7. Results for Labor Force Participation: Full Sample

Variable	*OLS* All (parsimonious) (1)	All (full) (2)	Males (3)	Females (4)	*IV* All (parsimonious) (5)	All (full) (6)	Males (7)	Females (8)
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)
Summary statistics								
No. observations	27,605	27,535	11,572	15,862	27,605	27,535	11,572	15,862
R^2	(0.257)	(0.324)	(0.414)	(0.160)	(0.253)	(0.319)	(0.412)	(0.152)
No. municipalities	288	288	276	285	288	288	276	285
F statistic	(822.9)	(452.4)	(507.4)	(136.0)	(799.2)	(411.4)	(484.5)	(130.8)
Partial R^2					0.089	0.095	0.102	0.093
Kleibergen-Paap statistic					2,561	2,574	1,481	1,660
OIR test					0.066	0.066	0.127	0.231
Endogeneity test					0.000	0.000	0.015	0.000
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)

Notes: 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 clustered at the municipal level are in parentheses.

* Statistically significant at the 10 percent level.

** Statistically significant at the 5 percent level.

*** Statistically significant at the 1 percent level.

restriction is violated. The Kleibergen-Paap statistics are a panel-data variant of the Cragg-Donald statistic, which was introduced as a weak instrument test by Stock, Wright, and Yogo.[55] Critical values for different tests can be derived for the Kleibergen-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 over-identified (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. Last, under the assumption that we have a set of valid instruments, we can test whether or not the variable for which we instrumented 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. Of note, 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 in light of 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.

55. Stock, Wright, and Yogo (2002).

TABLE 8. Results for Labor Force Participation, by Age Group

	Younger than 70			70 or older		
Variable	*All*	*Male*	*Female*	*All*	*Male*	*Female*
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)
Summary statistics						
No. observations	15,109	5,436	9,226	12,159	5,667	6,262
R^2	0.112	0.317	0.121	0.194	0.338	0.068
No. municipalities	274	231	260	282	259	264
F statistic	213.0	488.2	97.83	93.75	111.2	22.21
Partial R^2	0.102	0.121	0.099	0.095	0.098	0.091
Kleibergen-Paap statistic	1,167	832	755	1,090	542	664
OIR test	0.292	0.962	0.150	0.971	0.060	0.660
Endogeneity test	0.000	0.064	0.081	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)

Notes: 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 clustered at the municipal level are in parentheses.

* Statistically significant at the 10 percent level.

** Statistically significant at the 5 percent level.

*** Statistically significant at the 1 percent level.

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 decided to put the cutoff at age seventy. We then also divided 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 the 10 percent level for women. The point estimate is only slightly larger for men, but because of 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 seventy 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 seventy, who, based on their age, can be expected to still be 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 male beneficiaries by 8.1 percentage points and for female beneficiaries by 7.4 percentage points. To put these results in context, evidence for Brazil suggests that *Bolsa Família* raised female labor force participation by 4.3 percentage points.[56] A second study of the same program, using aggregate data for Brazilian municipalities, finds that male labor force participation increased by 2–4 percentage points, while results for women were smaller and not significant.[57] In Colombia, looking at beneficiaries of *Familias en Acción*, Barrientos and Villa find that eligible adults in single-adult households with children aged zero to six years have 9 percent higher rates of participation than ineligible adults; the difference for females is positive, at 6 percent.[58] They find effects on labor force participation of 2.3 percent for all adult males and 2.9 percent for males aged twenty-one to thirty-five. Other evaluations of *Familias en Acción* suggest comparable results. A joint study by IFS, Econometría Consultores, and SEI finds that the program increased the participation rate by 4.1 percentage points for adult women in urban areas (significant at 10 percent), by 2.7 percentage points for adult males in rural areas (significant at 1 percent), and by 3.1 percent for women in rural areas (not significant).[59] A joint assessment by the DNP, Acción Social, the IADB, and the World Bank finds that women's participation rate in urban areas increased by 4.1 percentage points, while CNC indicates that, at the urban level, the program leads to an increase of 4.1 percentage points in beneficiaries' occupation rate.[60] Espinosa and Nanclares 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 of men.[61]

56. Soares, Perez Ribas, and Guerreiro Osório (2010).
57. Foguel and Paes de Barros (2010).
58. Barrientos and Villa (2013).
59. IFS, Econometría Consultores, and SEI (2007).
60. DNP, Acción Social, IADB, and World Bank (2008); CNC (2011).
61. Espinosa and Nanclares (2016).

Causal Channels

To explore the possible causal channels for these results, we have to cut deeper and look at which economic activities are particularly affected. Table 9 shows 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. Although a multinomial model might be more appropriate, no such models are available for IV estimation. 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. Here again, a tobit or Heckman selection model might be more appropriate, but, 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, which reduces the sample by more than 25 percent. With these caveats in mind, we still believe that these results have important implications.

The first three columns of table 9 show the results for potential beneficiaries younger than seventy. 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 on one's own land (even if only at the 10 percent significance level). Correspondingly, we also find an increase in males younger than seventy working in the primary sector (at the 10 percent level). In terms of magnitude, we find that such males are ten 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 eighteen to fifty-five years) were twelve percentage points more likely to work in agriculture on the household farm or in herding activities in program villages.[62] In addition, the likelihood that beneficiary households sold any crop or owned any livestock increased with unconditional cash transfers by ten and seventeen percentage points, respectively, in Malawi and by 26 and 34 percent in Zambia.[63]

62. De Hoop, Groppo, and Handa (Forthcoming).
63. De Hoop, Groppo, and Handa (Forthcoming).

TABLE 9. Results for the Treatment Effect of Program Receipt on Various Labor Force Outcomes

	Younger than 70			*70 or older*		
Variable	*All*	*Male*	*Female*	*All*	*Male*	*Female*
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 and 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 and 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 and 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 (COL$, monthly)	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)

Notes: Only parameters on the treatment variable are shown. The table illustrates a linear probability model with three-stage procedure for binary treatment variables on respective dependent variable. For labor income and hours worked, the dependent variables are continuous. All specifications include a complete set of control variables and a full set of fixed effects. Standard errors clustered at the municipal level are in parentheses.

* Statistically significant at the 10 percent level.

** Statistically significant at the 5 percent level.

*** Statistically significant at the 1 percent level.

In Zambia, recent evidence suggests that the Child Grant Program led to an increased participation in nonfarm enterprises by fourteen percentage points and increased nonfarm enterprise revenues by 125 percent.[64] 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.[65] Another study suggests that the likelihood of becoming an entrepreneur with the same program increases by about 20 percent.[66]

In our results, we also find that the benefit is estimated to increase monthly labor income by around Col$172,000 (around U.S. $55) among this group and that the number of weekly hours worked rose by 5.3 hours. To put these numbers into perspective, we consider two extreme scenarios. Under the first, if the entire increase in income came from the additional hours worked at the extensive margin (assuming twenty-two workdays 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 toward 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 percent in the program (taken from tables 3 and 5) indicates 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 percent). 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 riskier 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 work only a few hours, but the lack of proper panel data makes this infeasible. It follows that the estimates discussed here assume that beneficiaries and nonbeneficiaries would, on average, work the same hours for the same pay.

For females the picture is different. We find strongly statistically significant effects on private and public employment and on work in the formal sector (the latter at the 1 percent level). In terms of magnitude, we find increases of

64. Handa, Natali, and others (2018).
65. Gertler, Martínez, and Rubio-Codina (2012).
66. Bianchi and Bobba (2013).

three percentage points for private sector employment, 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 sector to the service and tourism sector. The magnitude of this shift is around five percentage points (though only significant at the 10 percent level for the latter). While hours worked are 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 seventy 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 rate for individuals working in other homes or on land, rivers, or at sea; fewer hours worked; and a lower labor income. While these results point to some effect of *Colombia Mayor* in lowering labor force participation rates for people older than seventy years in some occupations, the important conclusion for our purpose is that the identified positive effects can be found only for the relatively younger cohort.

In light of our earlier discussion, the above results suggest that the principal effect of the benefit among individuals younger than seventy years was to increase their labor supply by easing budget constraints that prevented entry into the labor force. For males, this increase manifested primarily as 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) to 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 a 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 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 intermunicipal), 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 find statistically (and economically) significant effects only among the relatively younger cohorts. This should rule out the concern that our estimates merely pick up increased consumption owing to the additional income provided by the benefit. For the younger cohort, we find a strong increase in transportation expenditure for males, on

TABLE 10. Results for Up-Front Expenditures

	Younger than 70			70 or older		
Variable	*All*	*Male*	*Female*	*All*	*Male*	*Female*
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
(COL$, annual)	(1,324.362)	(1,533.874)	(1,707.643)	(859.361)	(1,214.943)	(1,150.417)
Clothing	0.122***	0.110**	0.130***	−0.026	−0.034	−0.013
	(0.035)	(0.050)	(0.043)	(0.030)	(0.048)	(0.037)
Clothing amount	20,656.358***	16,880.413	26,643.208***	717.298	3,106.614	861.564
(COL$, annual)	(7,667.42)	(13,339.938)	(9,737.977)	(5,691.535)	(9,028.301)	(7,607.251)
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
(COL$, annual)	(3,897.678)	(7,254.158)	(4,595.713)	(3,248.812)	(4,572.115)	(4,696.896)

Notes: 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 a full set of fixed effects. Standard errors clustered at the municipal level are in parentheses.

* Statistically significant at the 10 percent level.

** Statistically significant at the 5 percent level.

*** Statistically significant at the 1 percent level.

the binary as well as on the continuous outcome (though in the latter case, it is only significant at the 10 percent level). While we are unable to observe agricultural inputs, higher expenditure on transportation likely reflects the cost of bringing the product 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 in the binary measure for both males and females. However, the effect is larger and 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, they may also reflect increased spending due to the higher labor income shown in the previous table.

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 (that is, for individuals younger than

TABLE 11. Results on Labor Force Participation for Individuals Younger than 70 Years, by SISBEN Score

	Bottom 50% SISBEN Score			*Top 50% SISBEN Score*		
Variable	*All*	*Male*	*Female*	*All*	*Male*	*Female*
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)
Summary statistic						
No. observations	7,085	2,502	4,258	7,449	2,417	4,532
R^2	0.112	0.362	0.113	0.111	0.282	0.130
No. municipalities	236	179	219	236	179	220
F statistic	187.6	240.7	53.36	89.81	177.5	47.76
Partial R^2	0.100	0.117	0.098	0.117	0.157	0.108
Kleibergen-Paap statistic	614	370	386	522	354	359
OIR test	0.265	0.892	0.145	0.943	0.92	0.661
Endogeneity test	0.000	0.009	0.030	0.031	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)

Notes: 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 clustered at the municipal level are in parentheses.

* Statistically significant at the 10 percent level.

** Statistically significant at the 5 percent level.

*** Statistically significant at the 1 percent level.

seventy) and divide the sample into the top 50 percent and bottom 50 percent according to their SISBEN score.[67] We indeed find large and statically significant effects for the group with lower socioeconomic status. Results for the comparatively richer group are still positive, yet insignificant. Omitted from the table, but available on request, we do not find any statistically significant results for individuals older than seventy.

67. To do so, we first calculated the median SISBEN score for the three areas (fourteen principal cities, other urban areas, and rural areas) and assigned each household to the top or bottom 50 percent according to its position relative to the corresponding median.

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 an unconditional 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 an ideal scenario in which to test these hypotheses because of 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 seventy. 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 of a shift from less risky to riskier 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.

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