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The Unemployment Subsidy Program in Colombia: An Assessment

Por: Carlos Medina, Jairo Núñez, Jorge Andres Tamayo

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The Unemployment Subsidy Program in Colombia: An Assessment*

Carlos Medina**
Jairo Núñez***
Jorge Andres Tamayo****

Abstract

We assess the effects of the Colombian Unemployment Subsidy (US) program on future labor participation, unemployment, formality, school attendance and earnings of its beneficiaries, on household earnings and school attendance of the household members, and on weight and height of their children at birth. In addition to providing benefits, the program also provides training to some recipients. We use regression discontinuity and matching differences-in-differences estimators and find that both approaches indicate that participation in the labor market, the earnings of beneficiaries, and household income, do not increase, and for some populations decrease during the 18 months after leaving from the Unemployment Subsidy program. Enrollment in formal health insurance falls. The effects on male household heads include larger reductions in their earnings, larger decreases in their labor participation, and greater increases in their unemployment rates. We also find a small though statistically significant positive effect of the program on school attendance of the beneficiaries, but none on their childrenôs weight or height at birth. The results also are sensitive to the type of training that beneficiaries receive in the Unemployment Subsidy program. Overall, the program serves as a mechanism for smoothing consumption and providing social assistance rather than as a mechanism for promoting a more efficient labor market.

Keywords: Unemployment, Unemployment Insurance, Labor Supply
JEL Codes: J64, J65, J22

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** Researcher, *Banco de la República*
*** Researcher, Fedesarrollo.
**** Researcher, *Banco de la República*

Resumen

Se estiman los efectos del subsidio al desempleo colombiano (US) en la participación laboral, desempleo, formalidad, asistencia escolar e ingresos de sus beneficiarios, en los ingresos y la asistencia escolar de los miembros del hogar, y en el peso y la talla de los niños al nacer. El programa les transfiere efectivo a sus beneficiarios, y les provee formación para el trabajo. Resultados basados en estimadores de regresión discontinua y pareo de diferencias en diferencias, indican que la participación en el mercado laboral, los ingresos de los beneficiarios, y el ingreso del hogar, no se incrementan, y para algunas poblaciones se reducen, durante los 18 meses transcurridos después de salir del programa. El aseguramiento privado en salud se reduce. Los hombres jefes de hogar registran mayores reducciones en sus ingresos y en su participación laboral, y mayores crecimientos en tasas de desempleo. También se encuentra un efecto positivo, pequeño pero significativo, en la asistencia escolar de los beneficiarios, pero no en el peso o la talla de sus hijos al nacer. Los resultados son sensibles al tipo de formación para el trabajo que los beneficiarios reciben en el programa. En conjunto, el programa ha servido más como un mecanismo para suavizar el consumo y proveer asistencia social, que como un mecanismo para promover un mercado laboral más eficiente.

I. Introduction

During the late 1990s, Colombia experienced the highest unemployment rates in its entire history. To cope with the economic crisis, the government implemented various social and economic programs. Among those programs, Colombia sought to create a standard unemployment insurance (UI) program, with its usual requirements and characteristics. This initiative, however, gave form to substantially different from other UI programs in the region.

Unlike UI programs in other countries, Columbiaøs Unemployment Subsidy (US) provides a one time series of payments to workers displaced from either the formal or the informal sectors. The standardized benefit equals 1.5 times the monthly minimum wage paid out in six equal monthly installments. The payments are made in the form of ñvouchersò to purchase health insurance, food or education. Workers choose which type of voucher they wish to receive at the start of their (covered) unemployment spells. It is a benefit that they may receive only once during their working lives.

Part of the legislation that enabled the US program also provided funding for retraining of its beneficiaries. Program data indicate that the vast majority of unemployed formal sector workers participated simultaneously in retraining. Indeed this percentage is so high for workers from the formal sector that any evaluation of US program necessarily examines the joint effects of the US and workforce development programs, including the effects of the public employment service.

Another unusual feature of Colombiaøs UI program is that any unemployed household head is eligible to receive US benefits, and the type of benefits included depends on whether or not applicants have been previously enrolled in a Family Compensation Fund (CCF, or *Caja*).[1] The *Cajas* are private social entities formerly created to administer a family subsidy for low wage employees with children, and to provide recreation for their members. The government also allows them to provide health insurance, job training programs, etc. The *Cajas* are funded by firms with contributions from the 4 percent payroll tax on all formal sector workers.[2]

Even though the US program was implemented in 2003, there has been no in-depth evaluation of its impact. In this paper, we assess the impact of the US program on several labor market and socio-economic outcomes Because the US program targets workers from both the formal and informal sectors, and because about 50 percent of the urban labor force consists of informal workers, evaluating the US program in Colombia is different from evaluations in other countries. Our evaluation relies on two main sources of information.

[1] We understand by previous enrollment that the household head had been enrolled to a *Caja* for at least one year in the previous three years before applying for the subsidy. Entry and exit rules are established in decree 2340 of year 2003.

[2] Currently the social programs provided by the *Cajas* include: (i) health, (ii) nutrition and the marketing of food and familyøs basket products, (iii) education, (iv) housing, (v) credit for family firms (microcredit), (vi) social recreation, and (vii) the marketing of other products.

One source is data of US beneficiaries is provided by the two Family Compensation Funds (CCF), or *Cajas,* that operate in the Department of Antioquia: Comfama and Comfenalco. These institutions described below operate the US program. Data provided by these Cajaôs include nearly 70,000 individuals who received US benefits between February 2004 and December 2009. The other source are the 2002, 2003, and 2009 surveys of the System for the Selection of Beneficiaries of Social Programs (SISBEN, its acronym in Spanish), for the municipality of Medellín, the capital of Antioquia[3].

We study the effect of the US program by matching the *Cajas* data with the Sisben data. The resulting matched data base provides information on beneficiary and non beneficiary individuals at three points in time. Because the rules of the program are homogeneous across the country, we expect the results for Medellín to be roughly representative of the effect of the program in Colombia;s biggest cities. To estimate program impacts on key outcomes, we use regression discontinuity and matching differences-in-differences estimators.

We find that both approaches indicate that during the 18 months after they leave US, participation in the labor market, the earnings of beneficiaries, and household income, do not increase, and in cases they actually decrease. Enrollment in formal health insurance also declines. The effects on male household heads include a larger reduction in their earnings, a larger decrease in their labor participation, and a greater increase in their unemployment rates. We also find small positive, though statistically significant effects, of the US program on school attendance of beneficiaries. We find no effect of program participation on childrenôs weight or height at birth. These results are sensitive to the type of training beneficiaries also received in the US program. Overall, we find that the program performs better as a mechanism for smoothing consumption and providing social assistance, than as one for promoting a more efficient labor market.

In what follows, we present the empirical regularities that characterize the Colombian labor market and the characteristics of the unemployment program we evaluate. Then we proceed to present the evaluation of the program, in which we explain the programôs targeting, the data we use, the outcomes we assess, the identification strategy, and the results of our estimates. Finally, we discuss the results and conclude.

II. Facts of the Colombian Labor Market

1. Historical movements in Colombiaôs unemployment rate

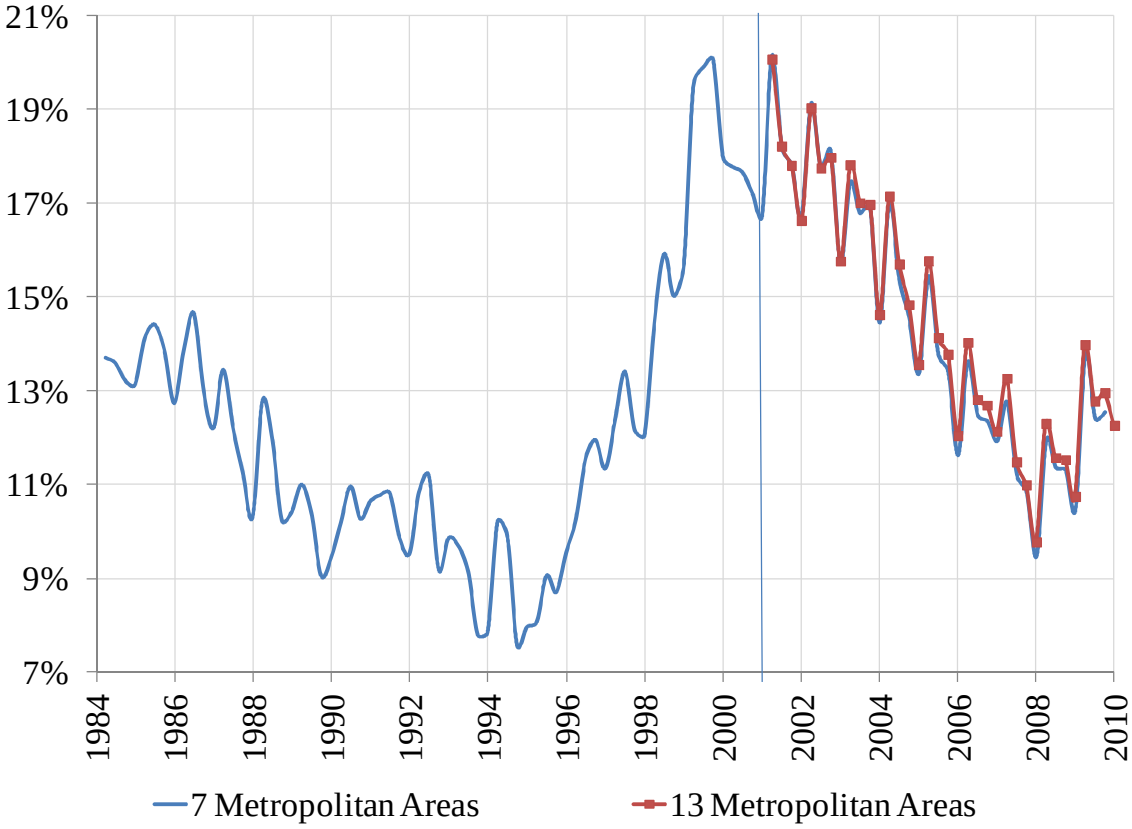
Since the early 1980s, the Colombian urban unemployment rate has experienced two important peaks: during the mid-1980s and during 1999 and 2000. Figure 1 illustrates the evolution of the quarterly unemployment rate. This information is available for the 7 largest

[3] The Sisben survey is used for the government to Rank households according to their quality of life, in order to target social public expending. It classifies people according to their socio-economic level into 6 strata, being stratum 1 homeless people and extreme poor and stratum 6 the highest level of affluence.

metropolitan areas, since 1984, and for the 13 main metropolitan areas, since 2001.[4] The figure shows that when both series became available, the unemployment rates in the two series were very similar. This fact suggests that both the level and the changes in unemployment are similar among Colombiaôs urban areas. During the late 1990s, the unemployment rate peaked at the height of the economic crisis when it nearly doubled from about 9.5 percent in 1996 to more than 18 percent by 1999. For some demographic groups, the unemployment rate exceeded 20 percent.

When the countryôs US program began in 2003, the unemployment rate was still high at levels between 16 and 17 percent. It decline steadily after that date, reaching a low of nearly 9 percent by 2008, although it has risen again during the most recent global economic crisis.

Figure 1. Evolution of Colombian urban unemployment rate in 7 and 13 MAs



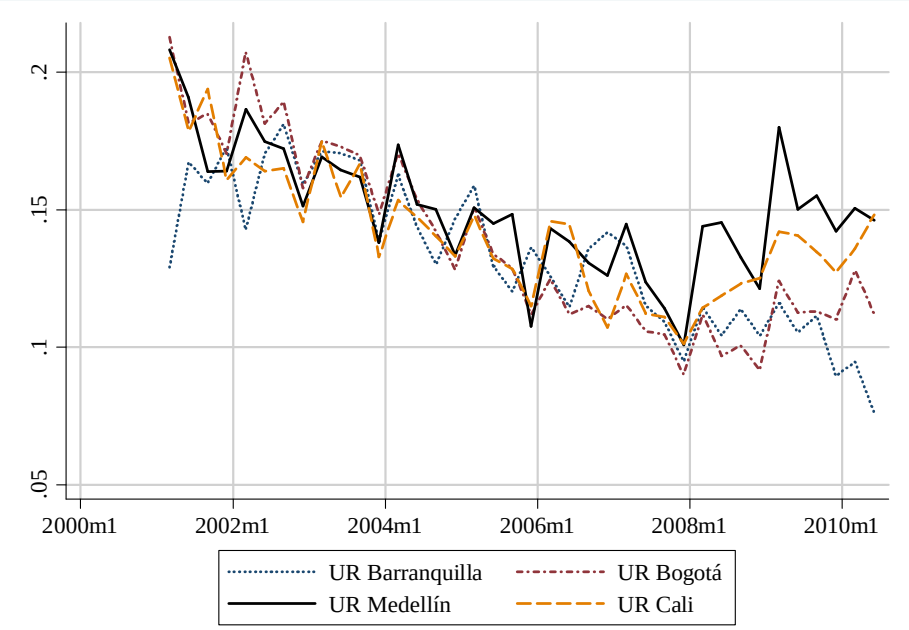
Source: López (2010). Seasonally adjusted series.

[4] The seven main metropolitan areas, MAs, are Medellín, Cali, Bogotá, Bucaramanga, Barranquilla, Manizales, and Pasto. The 13 main MAs include in addition Cúcuta, Villavicencio, Pereira, Ibagué, Montería and Cartagena.

Figure 2 illustrates the evolution of the quarterly unemployment rate for Barranquilla, Bogotá, Medellin and Cali, the four largest cities in Colombia. The figure shows that, since the 1999 economic crisis, the unemployment rate was reduced similarly in these four cities. However, during the recent financial crisis there was a marked divergence in the performance of the countryôs major cities, with Medellín and Cali experiencing the largest increases in unemployment rates.

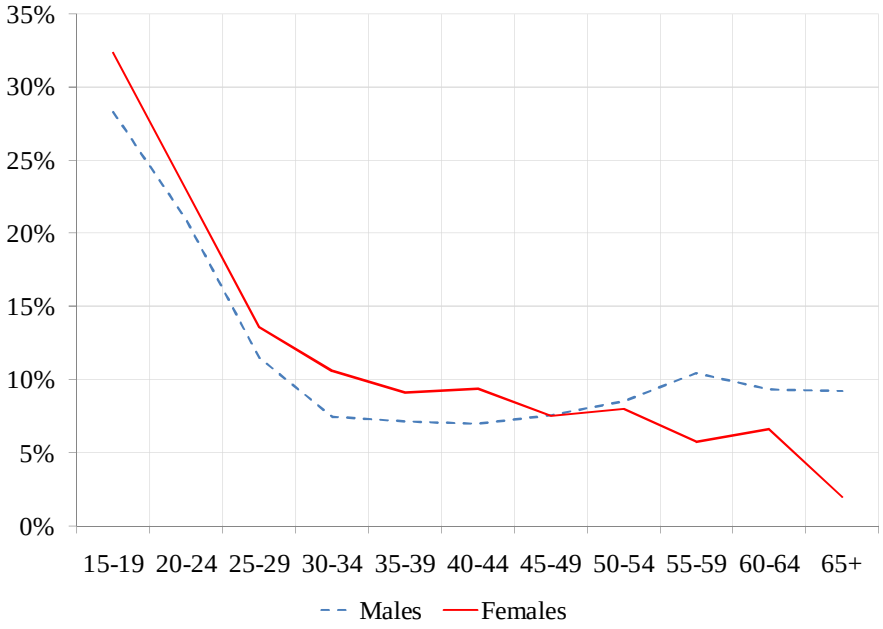
There is a close relationship between the overall unemployment rate and the share of uneducated workers either in the informal sector or unemployed. This relationship suggests that informality could be seen as the exit strategy, or outcome, for the uneducated unemployed in the country. Figure 3 shows that for both males and females, unemployment hits younger workers ï those under 25 years of age - particularly hard.

Figure 2. Evolution of unemployment rate, four major cities in Colombia



Source: López (2010).

Figure 3: Colombian Unemployment Rates by Age and Gender. 2009, 13 Largest Metropolitan Areas



2. The formal and informal sectors in urban Colombia

Because the US program targets both formal and informal workers, it is important to define the meaning of informality in Colombia and to understand its scope. According to the International Labor Organization, ILO, the type of workers considered informal are: (a) private employees or laborers in businesses or firms of up to 10 workers including their bosses or partners, (b) unsalaried family workers, (c) unsalaried workers in businesses or firms of other households, (d) domestic laborers, (e) self-employed workers without higher education, (f) employers of firms with 10 or less workers. Government employees are excluded[5]. Starting in 2009, the ILO began to classify as formal workers those who worked in a firm with more than 5 (rather than 10) workers.

Based on the ILOôs definition, to measure informality in Colombia we should recognize the following: (a) between 1986 and 2000, the Colombian household survey only allowed measuring informality in the 7 largest metropolitan areas (MAs), during the second quarter, every two years, (b) between 2001 and 2006 we can measure informality only during the second quarter, biennially, for the 13 largest MAs, and (c) between 2007 and 2009 we can estimate moving averages every three months to obtain monthly measures of informality for the 13 largest MAs.

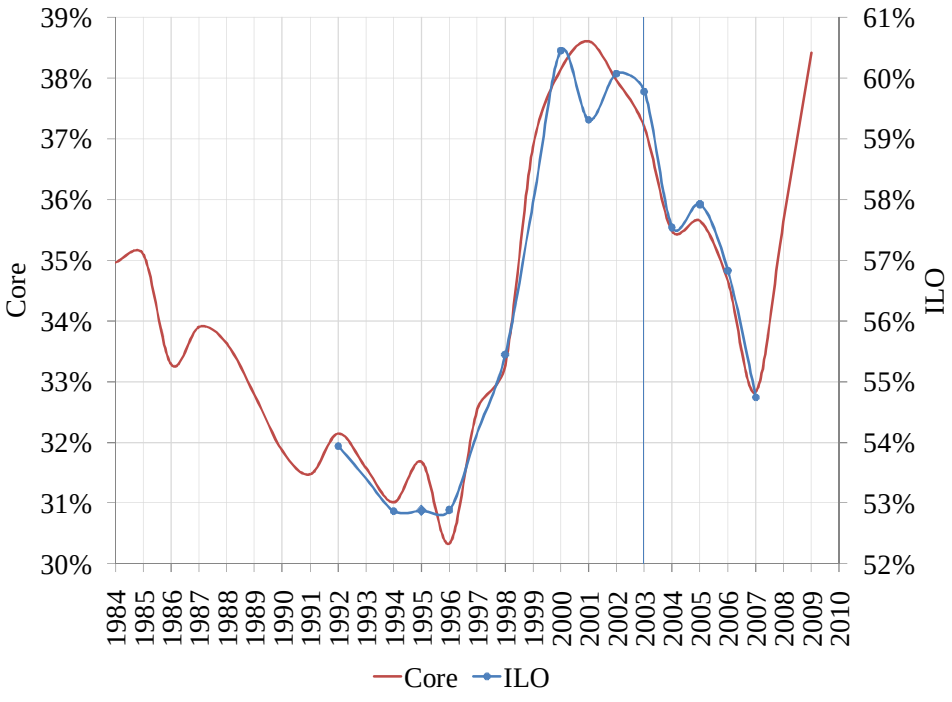
To estimate more frequent and longitudinally comparable measures of informality, we propose to include in our alternative definition of ñcore informalityò all self-employed

[5] The Administrative Department of National Statistics (DANE for its acronym in Spanish), adopted the ILOôs criteria to measure informal employment (ILO, PREALC1 78 project).

workers who have not received the benefits of higher education (excluding public or private employees and laborers). Figure 4 presents the ILOôs and our ñcore informalityò definitions. The fluctuations in the two measures are very similar, although our measure is about 20 percentage points lower than ILOôs (c.f., compare the left and right axis of the figure). Most of this difference is explained by the differing treatment of (i) wage earners and (ii) the educated self-employed working in firms of less than 10 (or 5 depending of the years considered) workers in the two measures of informality.[6]

Since our analysis below will focus on figures from Medellin, it is important to illustrate the magnitude of informality in Medellin compared to other Colombian cities. Figure 5 shows the shares of informal employment in the 13 largest Colombian MAs based on the ILO definition. As shown by the figure, the two largest cities Bogota and Medellin have the lowest levels of informality. In Cali, and in Barranquilla these rates are 5 and 10 percentage points larger, respectively, and there are even greater differences between the countryôs two largest cities and its smaller major metropolitan areas, The figure also indicates that this relationship between city size and informality did not changed much during the recent economic crisis .

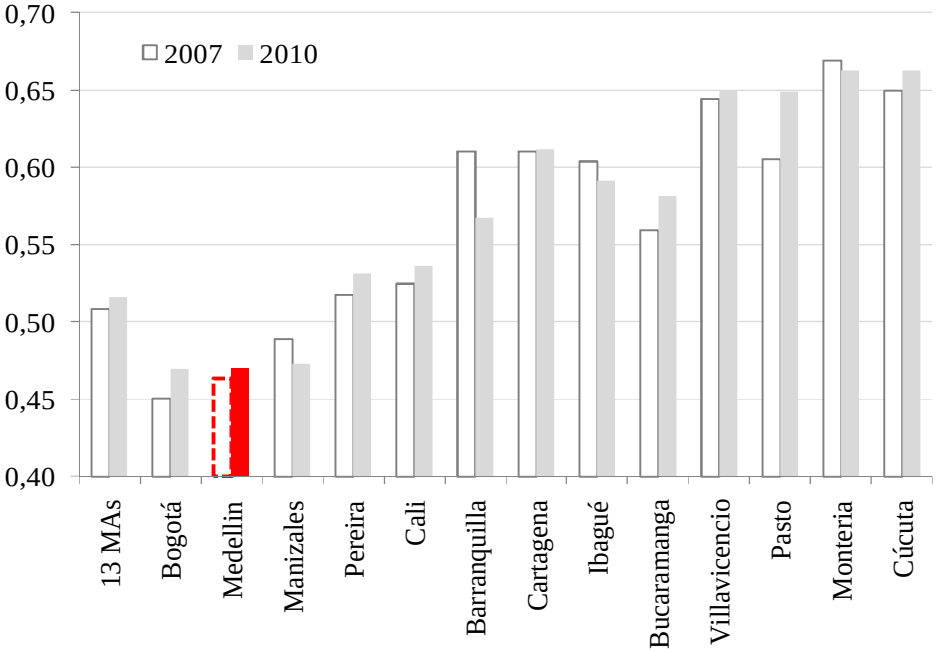
Figure 4: Informality Based on the ILOôs and the Core Informality definitions in Colombiaôs 7 Largest Metropolitan Areas, by Year from 1984 to 2010



Source: López (2010)

[6] See also Figure 9.

Figure 5: Informality in Colombiaôs 13 Largest Metropolitan Areas According to the ILOôs Definition of Informality



Source: Colombian Household Surveys, Dane.

2.1 Formality by Definition and Type of Employment

Let us analyze the composition of employment according to self-employment and the characteristics linked to formality. Figure 6 shows the share of workers with a written contract with health insurance, or who work in a job with a retirement or pension plan, by firm size and by type of worker, in the 7 largest MAs areas. In each of these categories, we know the share who are employed (either in the public or private sector), or self-employed. Self-employed workers are classified as, either educated or uneducated, and as an employer, domestic employee or unsalaried family worker.

Having a Written Contract as a Definition of Formality

Less than 40 percent of workers in Colombia have a written contract, and nearly 17 percent of employees or laborers working in the private sector do not know whether or not they have a written contract.[7]

[7] Note that although here we refer to the existence of a written contract, according to Colombiaôs Labor Code, whenever there are the following three elements: (i) personal activity of the worker, (ii) continuous subordination of the worker to an employer, and (iii) a wage as retribution to the service, the law presumes that there exists a labor contract.

Access to Health Insurance as a Definition of Formality

Colombian employers are required by law to enroll all their employees in a Health Promoting Company (EPS for its acronym in Spanish), which gives them access to health insurance of the Contributive Regime (CR).[8]

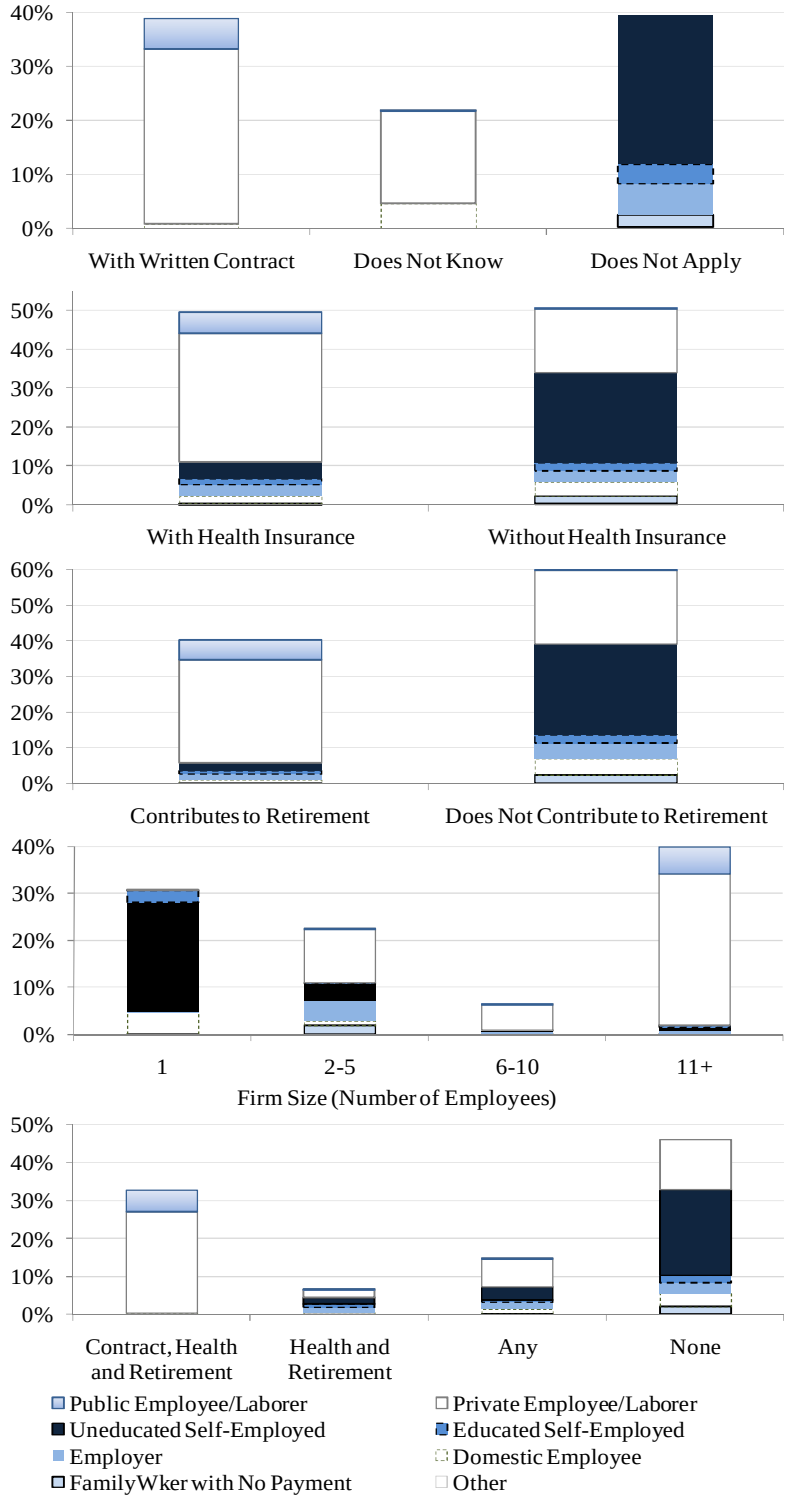
Nonetheless, some employers do not comply with the law and their employees not insured under the CR. All self-employed workers can enroll in the CR themselves by paying a monthly fixed amount based on a percentage of the monthly minimum wage, as well as employed workers whose employers did not enrolled then in the CR. Unemployed or inactive individuals can obtain health insurance through the CR, or apply for access to the Subsidized Regime (SR), a more basic basket of health services provided by the government. Its basket of services consists of about 55 percent of the basket provided by the CR.[9]

When workers are classified according to their access to health insurance by means of their contribution or that of their employers, that is, those who have access to the CR, we find that half of all workers are directly enrolled in the CR, but again, nearly 17 percent of private employees are not enrolled in the CR, nor are most self-employed workers.

[8] The CR covers most of the existent health services, except for esthetic plastic surgeries and suchlike.

[9] Actually, some employed workers, like domestic workers, apply for the SR and get it, and in some cases once they get the SR, they refuse to be enrolled in the CR by their employers just for being afraid that if they lost their job they would become uninsured, and anticipating that once unemployed, they might not be able to get access to the SR (See more on this in Camacho et al. (2009)).

Figure 6: Shares of Colombian Workers with Written Employment Contracts, Health Insurance, or Retirement Plans, by Firm Size, and Type of Worker in 7 Largest Metropolitian Areas, 2005



Source: Colombian Household Surveys, 2005, second quarter.

Contributing to Pensions as Criteria for Formality

There are even fewer workers who formally contribute to their retirement compared with the numbers of workers who are enrolled in the CR. In particular, the shares of private employees and educated self-employed who do not contribute to their retirement also are larger than the respective figures for enrollment in the CR. Nearly 60 percent of workers do not contribute to their retirement. Moreover, more than 45 percent of workers do not have a written employment contract, are not enrolled in the CR and are not contributing to their retirement. Only about one third of all workers have a written employment contract and make contributions both to the CR and to their pensions.

Firm Size as Criteria for Formality

As shown by Figure 6, the distribution of workers by firm size and type of worker reveals that most uneducated self-employed workers work one their own without (non-family) employees. Very few uneducated workers are employed even in small firms ï those with up to five employees. The difference between the ILOôs and our ñCoreò definitions of informality observed in Figure 4 are due (i) wage earners working in firms of up to 5 workers and (ii) educated self-employed. Together, these two groups constitute about 20 percent of Colombiaôs workers..

Enrollment in a *Caja* as Criteria for Formality

In the Colombian labor market , another type of worker contribution that is closely linked to the concept of formality is whether a worker contributes to the Family Compensation Funds (*Cajas de Compensación Familiar*, or ñ*Cajas*,ò see Appendix 1). Understanding which workers are enrolled in *Cajas* is relevant for our evaluation of US, because those entities are the ones that administer the program. Accordingly, enrollment in the *Cajas* by beneficiaries of the US program is a key characteristic to exploit when comparing the programôs impacts on formal and informal workers.

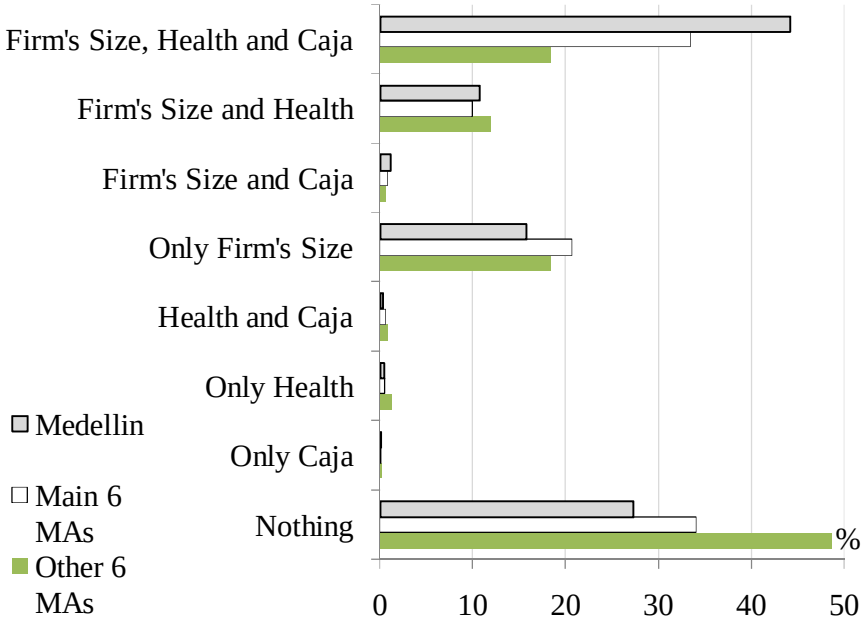
Figure 7 shows that if formality were defined according to enrollment in a *Caja*, the definition of formality would be much more demanding: most individuals enrolled in a *Caja* also are enrolled in health insurance and working in firms with at least 5 workers. If informality was defined by firm size, contributions to the CR and membership in the *Caja*, the differences across cities in the informality rates were even greater than that indicated by Figure 5.

Formality by Metropolitan Areas

Given that our evaluation examines the impact of the US program on unemployed workers in Medellin, it is important to document informality in Medellin compared with Colombiaôs other major cities. In summary, the labor market in Medellin is more formal than in other major cities in Colombia. As shown by Figure 7, Medellínôs labor market is more formal than the average of the 6 largest MAs, and those in turn more formal than the next 6 largest

MAs.[10] In particular, Medellin has a larger share of its labor force working in firms with more than 10 employees and who are enrolled in health insurance and *Cajas*, and a much smaller share working in small firms that do not enroll them in health insurance or *Cajas*. Clearly, the extent of informality is related to city size in Colombia, as shown by Figure 5, approximately one-fourth of Medellinôs workers work in small firms or do not contribute to the CR to a *Caja*. By contrast, the fraction approached one-half in the countries moderately sized cities of Villavicencio, Pereira, Cúcuta, Cartagena, Ibagué and Montería. This fact suggests that more standard measures of informality, as shown above in Figure 5, may be understating the differences between the scope of informality on Medellin compared with other cities in Colombia. As indicated by Figure 7, when informality is defined by firm size, contributions to the CR (health insurance) ad membership in the *Caja*, the differences across cities in the informality rates are larger than that indicated in Figure 5.

Figure 7: Share of Workers by Firm Size and by Contributions to Health Insurance or to *Cajas* for Medellin and the other 12 Largest Metropolitan Areas in Colombia, 2009



Source: Colombian Household Surveys, 2009. Notes: Besides Medellin, the other 6 largest MAs are Bogotá, Cali, Barranquilla, Bucaramanga, Manizales and Pasto. The next 6 largest MAs are Villavicencio, Pereira, Cúcuta, Cartagena, Ibagué and Montería.

2.2 Formality and Householdsô Socioeconomic Variables

Let us now analyze how key socioeconomic variables are related to informality. Appendix 2 shows the results of estimating the relationship between informality and household characteristics using DANEôs 2008 Living Standard Measurement Survey for the whole country, including rural areas. In each geographic domain, we estimate logit models of

[10] The 6 largest MAs are Bogotá, Cali, Barranquilla, Bucaramanga, Manizales and Pasto. The next 6 largest MAs are Villavicencio, Pereira, Cúcuta, Cartagena, Ibagué and Montería.

formality (i) under the ILOôs definition, and (ii) according to whether or not individuals contribute to both health insurance and retirement, regardless of the size of the firm they work for.

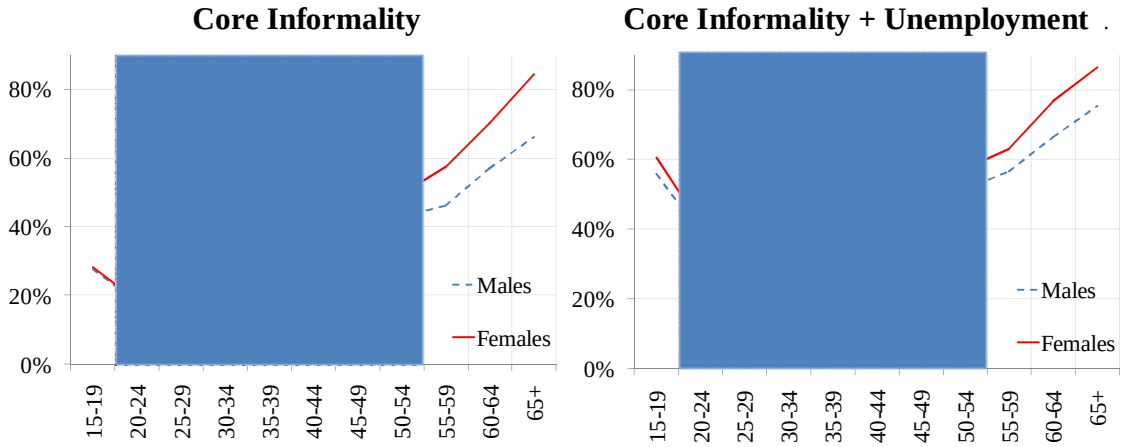
We have examined the determinants of formality, separately, for the national and for urban labor markets. In our analysis we examine these determinants for two different definitions of formality: (i) the ILO definition and (ii) our definition based on contributions to the CR (i.e, health insurance) and retirement. Results among these four groupings are very similar. As shown by Appendix Table 2, the absolute values of the (marginal) effects of these characteristics on the probability of being an informal worker are generally smaller when we use the definition of formality based on contributions to the CR and retirement. But the results based on national compared with urban labor markets are similar.

Because our analysis in this paper is of workers in an urban labor market, we focus here on our results for urban areas...Based on ILOôs definition of informality (c.f., the results presented in column vi), males are 16 percentage points more likely to work in the formal sector, and formality decreases with age at an increasing rate (i.e., informality increases with age at an increasing rate, just as Figure 8 shows). Formality increases monotonically with education. Individuals with primary education are 18 percentage points more likely to work in the formal sector than those with no education. Those workers with incomplete secondary, complete secondary, incomplete higher, complete higher, and post higher education are 28, 47, 58, 64 and 65 percentage points, respectively, more likely to work in the formal sector respectively, than the uneducated.

The estimate of the interaction term between gender and years of education implies that, other things being equal, males are less likely to work in the formal sector than females with the same years of education, depending on how much more educated they are. Individuals who are attending school are 6.5 percentage points more likely to work in the formal sector, while those born in the urban areas or who are household heads (holding gender constant) are 3.9 and 6.3 percent more likely, respectively, to work in the formal sector. Workers in small towns or rural areas are 5.5 and 14 percent less likely, respectively, to work in the formal sector (c.f., column ii). Finally, all geographic regions have higher levels of informality than Bogotá, the most informal being the *Pacific*, *Atlantic*, *Amazonía* and *Orinoquía* regions. In urban areas, individuals who receive rents from assets are 4.8 percent less likely to work in the formal sector, and those receiving subsidies are 11 percent less likely, (although this result does not necessarily reflect a causal relationship.).

ñCoreò informality in Colombia is higher among older workers. As shown by Figure 8 ñCoreò informality rates of workers 55 years old or more are above 50 percent for females, and above 40 percent for males. Since several individuals often move between informal employment and unemployment, it is worth noting that similarly, the sum of ñcoreò informality + unemployment rates of workers 55 years old or more are above 60 percent for females, and above 50 percent for males. The shaded areas refer to the population 21-54 years old, the one for which we assess the US impact below.

Figure 8. ñCoreò Informality and ñCoreò Informality Plus the Unemployment Rate, by Age and Gender. Colombiaôs 13 Largest Metropolitan Areas, 2009

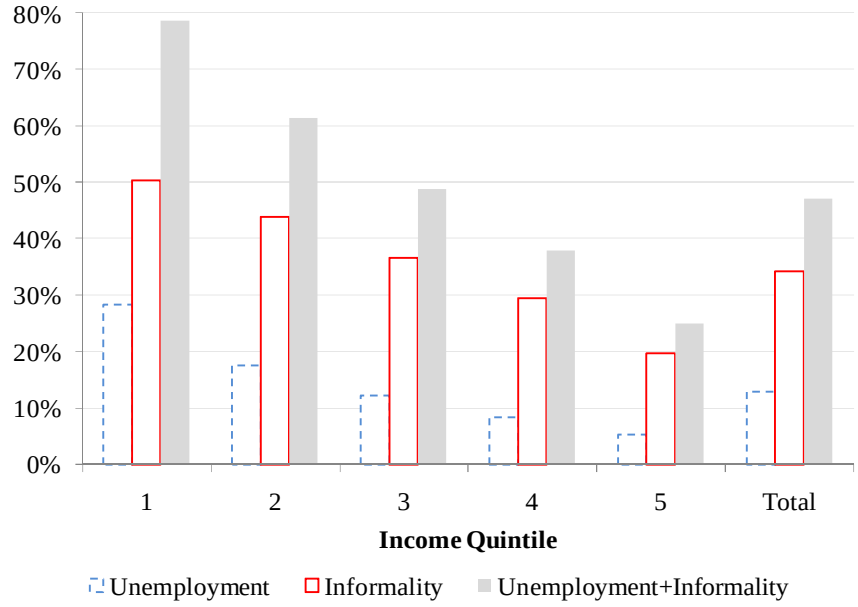


Source: Colombian Household Surveys, 2009. Notes: The 13 largest MAs are Bogotá, Medellin, Cali, Barranquilla, Bucaramanga, Manizales, Pasto, Villavicencio, Pereira, Cúcuta, Cartagena, Ibagué and Montería.

Overall, we observe that in 2009, nearly one-half of Colombiaôs workers were either unemployed or worked in the ñcoreò informal sector. Given what we have shown about the relationship been informality and education attainment, it is no surprise that these unemployment and informality rates vary sharply by household income. As shown by Figure 9, unemployment and informality rates are 28.3 and 50.4 percent, respectively, in the poorest quintile of the household income distribution, compared with 5.3 and 19.7 percent, respectively, in the richest quintile. Taken together, these percentages imply that more than three-quarters of workers in the poorest income quintile are either unemployed or informal sector workers compared with only one-quarter of their counterparts in the richest income quintile.

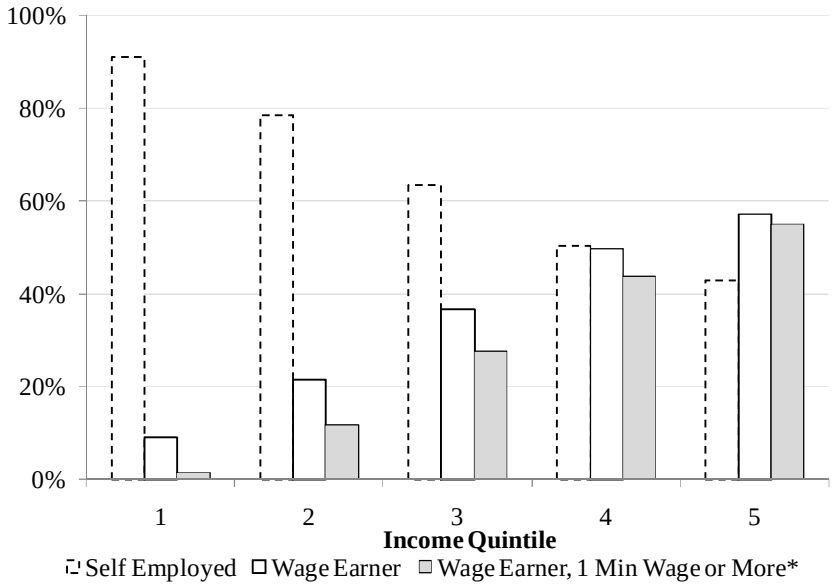
In Colombia, there is an important difference between wage earners and the self-employed. As it is shown in Figure 10, most of the population of the poorest income quintiles are self-employed, while wage earners are concentrated mostly among the countryôs richest individuals. There are almost no wage earners who earn at least one minimum wage among individuals in the first and second quintiles respectively. By contrast among individuals in the fourth and fifth quartiles, this fraction is approximately equal to one-half.

Figure 9. ñCoreò Informality and ñCoreò Informality Plus the Unemployment Rate by Income Quintile. 2009



Source: GEIH Household Survey. Quintile based on per capita householdôs income

Figure 10. Share of Total Employment by Income Quintile. Wage Earners and Self-Employed, 1st Quarter, 2009.



* Wage earners earning 1 minimum wage or more as a share of total workers in the income quintile.

Source: López (2010)

3. Unemployment duration

Since 1999, Colombia has had one of the highest unemployment rates in the region, and also has had relatively long unemployment durations.[11] To analyze in detail the duration of unemployment, we used the information of people who were working in 2009 who, if they had previously been unemployed, report the duration of their last unemployment spell.[12] This information allows us to use uncensored information to get our duration estimates.

Appendix 3 presents the cumulative hazard functions using the 2009 Colombian household survey at the national level, for different populations according to gender, age, and economic sector, type of worker, education, and geographic area. These functions allow us to estimate the effect of different characteristics on the probability of leaving unemployment by a given month.

Male workers in Colombia have shorter unemployment duration than females. The largest difference between these groups takes place around the sixth month, when 74 percent of males and only 53 percent of females have left unemployment. Younger workers also have shorter unemployment durations than older ones. By month eleventh, 85 percent of workers under 18 have left unemployment compared with only 60 percent of those aged 55 to 64 years of age.

Unemployment duration also varies across economic sectors. Workers in the economic sector of electricity, gas or water, have the shortest unemployment durations, while those in the financial services the longest. 72 percent of workers in the former sector have left unemployment by the fifth month, versus only 49 percent of those in the financial sector.

The variation of unemployment duration by type of worker is also large. Employees in rural areas are the ones with the shortest durations, followed by formal and informal employees which are very similar, while employees working for the government are the ones with longest unemployment durations. Unemployment durations are less sensitive to education differences. The average duration of unemployment in urban areas (13 main MAs and intermediate cities) is 10.6 months, while in the intermediate cities it is 10.9 months, and in the rural area 8.6 months.[13] During the first month about 14 and 20 percent of the unemployed population found a job in the urban and rural areas respectively. After three months, 44 percent (54) of the urban (rural) unemployed has found some form of occupation. Two years later there are only 10 percent of individuals looking for a job in the urban sector and 7 percent in the rural sector. We also compare unemployment duration in the main three metropolitan areas: Bogotá, Medellín and Cali. Medellín has longer unemployment durations than Bogotá, which in turn has slightly longer spells than Cali.

[11] See Ball, De Roux and Hofstetter (2011)

[12] Bear in mind that our estimates based in this survey information likely has ñretrospective biasò. It is well documented that survey respondents tend to underreport the incidence of unemployment spells that occurred more than two year prior to the survey, particularly if these were short spells of unemployment.

[13] The intermediate cities are all those cities smaller than the main 13 MAs but still urban.

III. The Unemployment Subsidy Program

The US program in Colombia was created in 2002 by Law 789, as a response to the large unemployment rates that had persisted in the country since the late 1990s (c.f., Figure 1). It was implemented starting with last quarter of 2003.[14] Although this program was initially intended to be implemented during critical economic downturns, it has operated continuously since its creation.

As is shown by Figure 11, the US program is administered by the Social Protection Ministry (MPS, for its acronym in Spanish), and its funding is carried out through the Fund to Promote Employment and Protection to the Unemployed (*Fondo para el Fomento del Empleo y la Protección al Desempleado*, FONEDE, for its acronym in Spanish). Three institutions jointly administer the program.The MPS establishes requirements for (i) eligibility, (ii) maintenance of the benefits, and (iii) the amount and duration of the benefit. The FONEDE is operated The *Cajas* operate and disperses payments to US recipients from the FONEDE. And the *Superintendencia de Subsidio Familiar* (SSF- Family Subsidy Superintendence) is responsible for program supervision and oversight.

The FONEDE is funded using revenues from the 4 percent payroll tax and its corresponding yields.[15] Thirty-five percent of FONEDEôs resources are used to pay unemployment benefits. This benefit is provided only to unemployed household heads. The grant is an in-kind benefit equal to one and a half legal minimum (monthly) wages, divided up into six equal monthly payments. This benefit is awarded through contributions to the health system, meal tickets, or educational bonds, according the beneficiaryôs choice. This benefit also does not depend on the number of people in the household.

Even though the magnitude of the benefit of the US program seems at first small, it equals nearly 100 (40) percent the 2005 baseline (before treatment) earnings of informal female (male) beneficiaries, and about 50 (30) percent the 2005 baseline earnings of female (male) formal beneficiaries, a reasonable amount given that as Nicholson and Needless (2006) affirm, for most states in the United States of America, the maximum benefit is usually between 50 and 70 percent of the earnings, with a more typical ñreplacement rateò equals to about 47 percent of prior earnings.

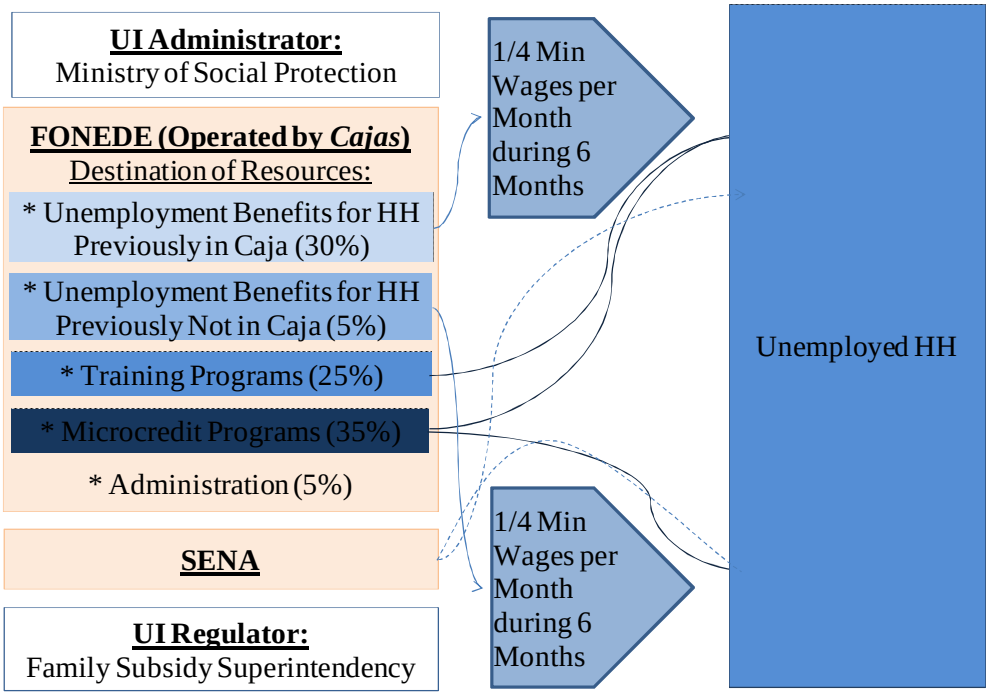
The target population of this benefit is allocated according to the previous enrollment of jobless household heads in a *Caja*. Accordingly, 30 percent of FONEDEôs resources serve unemployed household heads with previous affiliation to a *Caja*, and 5 percent to those without previous affiliation to a *Caja*.

[14] See also regulatory decrees 827 of April 2003, 2340 of August 2003, 3450 of December 2003, and 586 of March 2004.

[15] According to Law 920 of 2004, the non-executed resources during the relevant fiscal term are transferred to the FOVIS, the Fund for Housing of Social Interest (FOVIS is the acronym in Spanish for the *Fondo Obligatorio para el Subsidio Familiar de Vivienda de Interés Social*).

An additional, 25 percent of FONEDEôs resources are allocated to providing training programs for beneficiaries who previously contributed to a *Caja*, although the National Learning Service (SENA by its acronym in Spanish) has resources to provide training programs to the unemployed, regardless of whether they have previously contributed to a *Caja* or not.[16] The objective of the training program is to increase the possibility of employment among beneficiaries through better qualification and support of their job search. The training program is discretionary, and is offered by each *Caja* according to its criteria, operational schemes and management.[17]

Figure 11: Institutional Framework of the Unemployment Subsidy Program



Eligibility to the US benefit is subject to the beneficiary: (i) having no earnings, (ii) being available to work immediately, (iii) being engaged in active job search, (iv) be registered in the National System of Labor Registry (created by Law 789 of 2002), (v) attaining job training programs at a *Caja*, (vi) being an unemployed household head with people under his/her responsibility, and who, when they received the benefit, was not affiliated with an EPS or *Caja* as contributor or beneficiary.[18]

[16] Articles 10 and 12, Law 789 of 2002.

[17] Since the *Cajas* offer those services for their enrollees, what the US does is guarantee that the former beneficiaries of the *Cajas*, once unemployed, can keep their services.

[18] See paragraph 5 of article 13, Law 789 of year 2002, and Decree 2340 of 2003. Besides verifying social security participation, it is considered that the information which does not need to be ñformallyò supported is received under oath. Individuals are accepted to be unemployed household heads if they prove to have been previously affiliated, with dependent individuals, as contributors and not as beneficiaries, to an EPS, or to a *Caja*, and who at the moment of receiving the subsidy were not enrolled in an EPS or *Caja*, as contributors or

The legislation does not take into account the length of the personôs current unemployment spell, or a base period for eligibility, such as having been unemployed for a minimum period to receive the benefit. Also the benefit also is not provided to household heads who have complied with the requirements for pension (aging, surviving or disability), household heads fired due to criminal actions or other wrongdoing, or household heads who had previously been a US beneficiary.

Among US beneficiaries, reasons for losing the right to benefits include the following: when the beneficiary becomes employed; has rejected an acceptable job offer according to his/her academic qualifications; has been called to compulsory military service; receives other type of work remuneration; loss of freedom; people who have retirement plans; and death of beneficiary.

Finally, 35 percent of FONEDEôs resources are used for microcredit programs, and 5 percent for the fundôs administration. The *Cajas* spend their administrative costs in carrying out activities related to distribution of subsidies such as promotion of the US, reception of applications, verification of compliance with requirements (activity performed through information crossing of applicants with other *Cajas* and the social security system, carried out by the *Cajas*ô national association for all the *Cajas* of the country). Their activities also include providing the in-kind benefit chosen by US beneficiaries (i.e., food, educational or health support), and verifying every month compliance the programôs requirements.

Statistics on the Unemployment Subsidy Program and Workforce Training

At the national level, the unemployment rate among household heads, USôs target population, has varied around 6 percent in 2003, 2004, and 2009, and about 5.5 percent for the rest of the period (c.f., Table 1). In Medellín, the unemployment rate has averaged around 7.6 percent. By the second quarter of 2009, the number of unemployed household heads at the national level reached 611,000 and in Medellín it reached 65,000 persons.[19] The last row of Table 1 shows the ratio between the number of US program subsidies assigned and the number of unemployed household heads. Between 2004 and 2009, the program has covered an average of 16.6 percent of unemployed household heads at the national level, and 23.4 percent in Medellín

The US program is relatively small in size. In 2008, expenditures on the program amounted to approximately $153,000 million Colombian pesos (COP), or about 0.04 percent of Colombiaôs GDP.[20] This percentage is small when compared the United Statesô unemployment insurance program, which according to Nicholson and Needels (2006) was about $34 billion in 2004, or nearly 0.23 percent of that nationôs GDP.

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beneficiaries. When the applicant applies as having been previously enrolled in a *Caja*, he or she is required to have been enrolled in a *Caja* for at least one year during the last three years since the moment of application.

[19] At that time, there were 2.37 million unemployed at the national level, 265,000 of whom resided in Medellín.

[20] See Carrasco (2009) for more details. The average exchange rate between 2005 and 2006 was $2,340/USD.

Program records show that the training benefit has not been fully used, and additionally, it has had a dropout rate of 20 percent (c.f., Table 3). Nonetheless, it appears the most beneficiaries who previously contributed to a *Caja* received training in connection with being unemployed. In addition, other beneficiaries who were not previously enrolled in a *Caja* received training funded by SENA.[21]

Table 1. Household Heads Assigned US Subsidies, by Previous Enrollment in a *Caja*. Nationwide and Medellín. May ï July 2003 to 2009.

Concept	Colombia							Medellín						
	2004	2005	2006	2007	2008	2009	Average 2004-09	2004	2005	2006	2007	2008	2009	Average 2004-09
Overall participation rate (%)	81.0	79.9	80.5	80.3	79.4	81.2	80.4	74.5	72.3	72.2	72.8	72.0	74.9	73.1
Unemployment rate (%)	6.7	5.3	5.4	5.6	5.8	6.3	5.9	8.0	7.3	8.0	5.9	7.5	8.9	7.6
Economically active pop., EAP♦	8,243	8,259	8,671	9,050	9,242	9,708	8,862	577	579	599	650	670	726	634
Unemployed♦	551	441	464	507	536	611	518.3	46	42	48	38	50	65	48
Subsidies assigned♦														
Previously enrolled in Comfama								5.4	5.0	5.4	6.0	6.3	7.1	5.9
Prev. enrolled in Comfenalco								0.68	1.20	1.10	1.20	1.65	2.32	1.4
Prev. enrolled in *Cajas* Total	49.7	59.5	58.6	63.7	69.6	46.3	57.9	6.1	6.2	6.5	7.2	7.9	9.4	7.2
Previously not enrolled in *Cajas*	15.8	51.3	18.1	16.9	17.5	9.9	21.6	2.7	9.5	2.9	4.2	3.9	1.3	4.1
Total subsidies assigned*	65.5	110.8	76.8	80.6	87.1	56.2 **	86.2	8.8	15.7	9.3	11.4	11.8	10.7	11.3
Tot. subsidies assigned/EAP (%	0.8	1.3	0.9	0.9	0.9	0.6	0.9	1.5	2.7	1.6	1.7	1.8	1.5	1.8
Total subsidies assigned/Unemployed (%)	11.9	25.1	16.6	15.9	16.3	9.2	16.6	19.1	37.2	19.4	29.7	23.5	16.5	23.4

Source: DANE ï Continuous Households Survey (2003-05), Great Integrated Households Survey (2006-09). Mobile Quarter Series 01 - 08. Note: Results expressed in thousands. Due to rounding in thousands, totals may differ slightly. ♦ EAP, Unemployed and subsidies assigned are in Thousands, and only

*Source: Social Protection Ministry (Information on subsidies at the national level is on an annual basis; it does not correspond to the quarter May-July), Comfama, Comfenalco, and household surveys.

** Number of subsidies assigned between January and July of year 2009.

Table 2. Beneficiaries of the Training Program and Percentage Being Trained

Year	Beneficiaries	% of Beneficiaries Being Trained	Training Drop Outs
2004	40,508		
2005	72,596		
2006	75,542	67.9	26.4
2007	75,181	74.5	22.9
2008	85,460	78.0	18.7
2009*	37,894	78.2	16.1
Average 2006-09	77,993	74.8	20.7

Source: Family subsidy Superintendence (Superintendencia del Subsidio Familiar). Estimates from the General Management for Labor Promotion (Dirección General de Promoción del Trabajo), Social protection Ministry. * Data until June 2009.

Although the legislation allows *Cajas* to use FONEDEôs resources to provide training to their US beneficiaries, there have also been alliances between the National Association of Family Equalization Funds (*Asociación Nacional de Cajas de Compensación Familiar* -

[21] FONEDEôs training resources only target former *Caja* enrollees.

ASOCAJAS) and *SENA*. As a result, the *Cajas* may use SENAôs public employment service (SPE) to give their users access to registered employment vacancies. The objective of the SPE is to ease and quicken the transitions of the unemployed into employment.[22]

According to official FONEDE data, during the period between years 2004-2008, the fund has received about 6.2 percent of the revenunes from the payroll tax. (C.f., Table 3). In other words for every $100 collected by *Cajas* through the 4 percent payroll tax, $6.2 have been assigned to FONEDE. Accordingly, this implies that about $2.0 is spent on the US program.

Table 3: FONEDE Revenues as a Percentage of the 4 Percent Payroll Tax

Year	% Fonede/ 4%
2004	6.5%
2005	6.5%
2006	6.1%
2007	6.1%
2008	5.9%

Source: Supersubsidy, Statistical Report, population, contributions and monetary subsidy, year 2008.

However, these data also show that through 2008, resources appropriated for these programs have not been fully spent.[23] Table 4 shows the share of FONEDEôs resources that have been expended by the program as a proportion of the amount appropriated by law.[24] As shown by the table, since 2005 total expenditures on US benefits have been near to the limit allowed by law. Out of the 35 percent of FONEDEôs resources that are annually budgeted to US benefits, the *Cajas* have spent more than 96.5 percent. By contrast, the microcredit program has had expenditure levels of less than 50 percent of what were intended under the legislation. Since 2005, *Cajas*ô microcredit expenditures have been about 30 percent of FONEDEôs budgeted resources.

Data from last quarter of year 2003 through July 2009, indicate there have been 495,078 US claimants. Of this total, 72.5 percent corresponded to assignments to household heads with previous *Caja* enrollment, and the remaining 27.5 percent to household heads without previous *Caja* enrollment (c.f., Table 5). During this period, female household heads received larger proportion FONEDE assignments of US benefits than males. Women received about 290,000 (or 58.6 percent) of these assignments compared with 205,000 (or 41.4 percent) of the assignments for men (c.f., Table 6).

[22] See the Cooperation Agreement No. 7 of 2009 between ASOCAJAS and the National Direction of SENA. The SENA is the national public entity in charge to provide training programs (*Servicio Nacional de Aprendizaje*).

[23] As was discussed above, FONEDEôs non executed resources during each fiscal year are transferred to FOVIS.

[24] Fractions may exceed 1 when resources from previous years are carried over to the current year.

Table 4. FONEDEôs Expenditures as a Share of the Disposable Budget by Program

Year	Microcredit	Benefits	Training	Total*
Share of FONEDE's Resources by Concept →	0.35	0.35	0.25	0.95
Share Actually Executed				
2003	0.03	0.344	0.141	0.176
2004	0.166	0.647	0.405	0.406
2005	0.236	0.963	0.773	0.754
2006	0.204	0.965	0.88	0.727
2007	0.238	0.976	0.897	0.749
2008	0.447	0.975	0.853	0.779
2009	0.371	1.156	0.954	0.839
Simple Average 2005-2009	0.30	1.01	0.87	0.77

Source: Supersubsidy, Statistical Report. Population, contributions and monetary Subsidy, year 2008, 2009.

* The remaining 5 percent if for administration.

As shown by Table 7, administrative records demonstrate that US beneficiaries chose to receive their benefits almost entirely in the form of food vouchers. They opted for this modality 97.8 percent of the time. The other modalities, health and education, were chosen by 1.7 and 0.5 percent of beneficiaries, respectively.

Table 5. Assignment of Number of US Beneficiaries, by Year and Prior Enrollment in a *Caja*

Year	Previously enrolled in a CCF	Previously not enrolled in a CCF	Total
2003	11,748	6,499	18,247
2004	49,653	15,809	65,462
2005	59,504	51,270	110,774
2006	58,619	18,142	76,761
2007	63,714	16,886	80,600
2008	69,575	17,480	87,055
2009	46,288	9,891	56,179
Total	**359,101**	**135,977**	**495,078**
Participation	**72.5**	**27.5**	**100.0**

Source: *Superintendencia del Subsidio Familiar* (Family Subsidy Superintendence). Estimates by General Management on Labor Promotion, Social Protection Ministry.

The programôs administrative records also indicate that the waiting times of the unemployed to receive US benefits varied considerably. Depending on the unemployed state and whether they had previously been a member of a *Caja*, these times varied from between two months (minimum waiting time recorded) and 19 months (maximum waiting time). On average, people with no previous enrollment in *Cajas* showed higher waiting times, mainly in small states, where it took beneficiaries 26 months in 2007; 28 months in

2008; and 27 months during the first six months of 2009. In contrast, applicants with previous enrollment in *Cajas* showed lower waiting times, especially in the smaller states (Table 8).[25]

Table 6. Distribution of Number of US Assignments, by Gender

Year	Females		Males		Total
	N	%	N	%	N
2003	11,283	61.8	6,964	38.2	18,247
2004	41,290	63.1	24,172	36.9	65,462
2005	67,129	60.6	43,645	39.4	110,774
2006	35,716	46.5	41,045	53.5	76,761
2007	51,404	63.8	29,196	36.2	80,600
2008	50,245	57.7	36,810	42.3	87,055
2009	32,949	58.7	23,230	41.4	56,179
Total Period	**290,016**	**58.6**	**205,062**	**41.4**	**495,078**

Source: Superintendencia del Subsidio Familiar (Family subsidy Superintendence). Estimates, General Management on Labor Promotion, Social Protection Ministry (Ministerio de la Protección Social)

Table 7. Distribution of US Beneficiaries, by Modality of Use

Year	Subsidies Per Modality			
	Health	Food	Education	Total
2003	453	17,504	290	18,247
2004	1,746	62,813	903	65,462
2005	1,230	108,959	585	110,774
2006	1,088	75,429	244	76,761
2007	1,215	79,157	228	80,600
2008	1,723	85,059	273	87,055
2009	762	55,230	187	56,179
Total	**8,217**	**484,151**	**2,710**	**495,078**
%	**1.7**	**97.8**	**0.5**	**100.0**

Source: Superintendencia del Subsidio Familiar (Family subsidy Superintendence). Estimates, General Management on Labor Promotion, Social Protection Ministry (Ministerio de la Protección Social)

Most US beneficiaries have been under 45 years of age: 35-44 years old constitute 36.9 percent of beneficiaries, and 25-34 year olds make up 28.3 percent of beneficiaries. By contract 45-54 year olds constitute only 21.2 percent of beneficiaries.[26] Young adults and youths are under represented among US beneficiaries, even though young people constitute a disproportionate share of the unemployed, This underrepresentation arises by design because they are less likely than other unemployed persons (i) to be household heads or (ii) to have previously enrolled in a *Caja*. Likewise the oldest unemployed also are

[25] Medellín is located in Antioquia, which is classified as a large state.
[26] Data from 2005 to June 2009.

underrepresented among US because they are often eligible to receive benefits from a retirement plan. (C.f., Table 9).

Table 8. Waiting Times Average Waiting Times in Months before US Beneficiary Receives Subsidy, by Prior Enrollment in a *Caja*

Year	Previously enrolled in CCF			Previously not enrolled in CCF			Average
	Big States	Small States	Total	Big States	Small States	Total	
2003	3.0	2.0	2.5	3.0	1.0	2.0	2.25
2004	5.0	3.0	4.0	4.0	2.0	3.0	3.50
2005	8.0	5.0	6.5	6.0	2.0	4.0	5.25
2006	11.0	6.0	8.5	9.0	2.0	5.5	7.00
2007	3.0	6.0	4.5	12.0	26.0	19.0	11.75
2008	2.0	2.0	2.0	5.0	28.0	16.5	9.25
2009	2.0	2.0	2.0	4.0	27.0	15.5	8.75

Source: *Superintendencia del Subsidio Familiar* (Family Subsidy Superintendence). Estimates, General Management on Labor Promotion, Social Protection Ministry.

Administrative records for the program show a difference between distributions of resources according to whether US beneficiaries were previously enrolled in a *Caja* and their prior education. For beneficiaries previously enrolled in a *Caja*, most concentration of resources was seen in people who had finished secondary school, followed by people who only finished primary school or did not have any educational studies. For beneficiaries with no previous enrollment, more than 70 percent of the subsidies were distributed to people with no education, or no more than primary school (c.f., Table 10).

Table 9. Distribution of US Beneficiaries, by Age

Age Range	Previously Enrolled to CCF	Previously Not Enrolled to CCF	Total
15-24	0.042	0.088	0.055
25-34	0.300	0.237	0.283
35-44	0.377	0.346	0.369
45-54	0.212	0.212	0.212
55+	0.069	0.117	0.082
All	1.00	1.00	1.00

Source: *Superintendencia del Subsidio Familiar* (Family subsidy Superintendence). Estimates, General Management on Labor Promotion, Social Protection Ministry.

Table 10. Distribution of US Beneficiaries, by Previous *Caja* Enrollment and Education

Education	Previously Enrolled to CCF	Previously Not Enrolled to CCF
None	21%	45%
Primary	21%	28%
Secondary	44%	23%
Technical	9%	2%
Undergraduate or More	4%	1%
Other	1%	1%

Source: *Superintendencia del Subsidio Familiar* (Family subsidy Superintendence). Estimates, General Management on Labor Promotion, Social Protection Ministry.

As we would expect because we use prior enrollment in a *Caja* as an indicator of whether the worker is in the formal sector, that these workers were better paid prior to becoming unemployed compared with their peers who had not been members of a *Caja*. Among people with previous *Caja* enrollment and who received US benefits during the 2003 to 2009 period, the wages of 77 percent of them ranged from between 1 and 2 minimum wages. By contrast, among people with no prior *Caja* enrollment, who received US benefits during the 2003 to 2009 period, 90.8 percent had earned less than the minimum wage (c.f., Table 11).

Information about provision of resources distributed among applicants with or without previous enrollment to a *Caja*, discriminated by state, indicates that greater provisions to beneficiaries previously enrolled in a *Caja*, near to 85 percent, were provided by *Cajas* from the states of Caldas, Cesar, Cauca and Casanare. Those with less provision (less than 50 percent) assignment were *Cajas* from Chocó, Sucre, Amazonas and Arauca. Antioquia, the state where Medellín is located, assigned 77 percent to beneficiaries with previous enrollment in *Cajas* (Appendix 4).[27]

Table 11. Distribution of US Beneficiaries, by Previous Wage

Wage Range in Minimum Wages	Previously Enrolled to CCF	Previously Not Enrolled to CCF
<1	16.8%	90.8%
1-2	77.3%	8.9%
3-4	4.5%	0.2%
5-6	1.0%	0.0%
>7	0.3%	0.0%
Other	0.1%	0.1%

Source: Superintendencia del Subsidio Familiar (Family subsidy Superintendence). Estimates, General Management on Labor Promotion, Social Protection Ministry.

[27] If *Cajas* executed all their available resources to fund subsidies, the share for those beneficiaries previously enrolled in a *Caja* would be the share of resources located by FONEDE to beneficiaries previously enrolled to a *Caja* (30 percent) divided by the total share of resources located to beneficiaries (35 percent), that is, 30/35 ≅ 85.7, but *Cajas* usually execute less of one or other type of subsidy, thus explaining the observed variation in the percentages shown in Appendix 4.

Other Instruments to Protect Workers in Colombia

A. Severance Payments

There are other mechanisms to protect workers from the risk of being unemployed. The main one is severance payments, known in Colombia as *Cesantías*. This mechanism works as a saving method, funded annually by the employer, which corresponds to a monthly average income accrued during the prior year. The employer also has to pay 12 percent interest on the total balance of the deposits the worker had accumulated by December 31st of the previous year, which in practice means that the employee has a net saving rate of (1/12)*1.12, that is, near 9.3 percent of last yearôs earnings. The resources are allocated in individual accounts, and the accumulated deposits (the one month wage plus its 12 percent interests) of the individual accounts earn returns above the fixed term deposit interest rate (DTF for its acronym in Spanish); therefore there is a guarantee of a minimum profitability.

The main goal of these severance payments is for the worker to use these resources whenever he loses his job or his labor contract ends. However, the employee can also use this resource to acquire a new house, to pay for home improvements, and to pay for college or higher education of the beneficiary, spouse or children. Once workers become unemployed, they may withdraw the whole balance regardless of the reason for becoming unemployed. In fact, workers tend to use most of this fund for the other reasons mentioned above rather than have it available whenever they become unemployed. Requirements for its use make *cesantías* more similar to an individual savings account for current spending than a long-run saving to cover the risk of unemployment.

B. Active Labor Market Policies

Apart from severance payments and the US, there are also training programs. The public strategy in this field is mostly in the hands of the SENA, which is funded with a 2 percent payroll tax. *SENA* is in charge of executing these resources by providing training in its branches, monitoring and regulating training courses across the nation. Previous evaluations of the impact of its program in urban areas provide mixed evidence of its effectiveness. Gaviria and Núñez (2003), Medina and Núñez (2005), and Estancio and others (2009), find nil to modest effects of its courses on earnings. Nonetheless, the program has been estimated to be effective for women. In particular, Attanasio, Kugler and Meghir (2011) present experimental evidence that the Program *Jóvenes en Acción* increased the earnings of women by 19.6 percent. These effects partly results from a 6.8 percent increase in the employment than those not offered training, mainly in formal-sector jobs. In addition, their cost-benefit analysis leads them to conclude that the program generates much larger net gains than those available in developed countries. Sarmiento and others (2007) find non experimental evidence of positive effects on earnings.

IV. Impact Evaluation

1. Establishing Eligibility for the Unemployment Subsidy

As it we explained above, enrollment in a *Caja* is closely linked to formality. Therefore, we define formal workers as potential beneficiaries who are unemployed household heads and who have contributed to any *Caja* for at least one year during the previous three years

before losing their jobs. We define informal workers to be potential beneficiaries who were unemployed household heads without earnings and who did not contribute to a *Caja* for at least one year during the last three years.[28]

In accordance with these definitions, easily observable characteristics like age, education, marital status, household size, etc., are not directly used to target eligibility for the US. Nonetheless, we know that self-selection ends up generating differences in those characteristics among beneficiary and non-beneficiary populations. We return to this point below.

An additional requirement of the US program is that in order to receive US benefits, the claimant may not be currently a beneficiary or a contributor to an *EPS* or to a *Caja*. Policymakers established this restriction in order to prevent employed workers from applying for and obtaining the US benefit. Because Colombian law requires employers in either (i) to enroll their employees in the Contributive Regime and in a *Caja*, or (ii) requires their employees to enroll in the CR themselves, this requirement allows the *Cajas* to prevent free-riding by employed individuals.

This restriction also seeks to target the US benefit to the most vulnerable part of the unemployed population. Anyone being enrolled in an *EPS* or *Caja* would signal that he or she or a member of his household, was able and willing to become enrolled.[29] This limitation implies that unemployed informal workers who wanted to claim US benefit, but who had enrolled in the CR on their own, would have to stop contributing to the CR. By contrast, had they enrolled in the SR rather than in the CR, this same person could have applied for a US benefit.

As shown in Table 12, between 2003 and October 2009, nearly 20 percent of US claimants were either denied or lost their US benefits, because they had been enrolled in an EPS (the Contributive Regime (CR).[30]

The importance of this no EPS - no CR - requirement becomes even more apparent once we understand how Colombia targets health insurance for the poor through the Subsidized Regime (SR). Prior to 1993, only workers affiliated with the Colombian Institute of Social Insurance, ISS, were beneficiaries of privately provided health insurance, while uninsured individuals were treated by the network of public hospitals. In 1993, Law 100 established two tiers of health insurance: the Contributive Regime, CR, and the Subsidized Regime, SR. The CR covers formal workers with a very comprehensive set of health services that includes nearly all of the most common illnesses. The SR covers the poorest informal workers with a plan that encompasses about 55 (initially 50 percent) of the illnesses

[28] Among these informal workers, the program gives priority to artists, sportsmen and writers. That is, anyone in this group would become beneficiary before other comparable candidates from other professions who applied with the same date (Paragraph 2 of article 13 of Decree 2340 of 2003).

[29] Paragraph 5º, Article 13 of Law 789, 2002. As explained by Synergia (2009), this requirement is actually enforced by some of the most important *Cajas*.

[30] Ramírez (2009) uses only the information of applicants and beneficiaries of Comfama, one of the two *Cajas* operating in Medellín.

covered by the CR. Formal workers and their employers fund workersô insurance premiums for coverage by the CR. Several public funds (national transfers, municipalitiesô budgets, lottery contributions, etc.) and the Solidarity Fund, FOSYGA, collect resources to fund the SR.

Table 12. Reasons that Unemployed Applicants are Denied or Lose the Right to Receive US Benefits

Reason	2003		2004		2005		2006		2007		2008		Oct 2009		Average	
	N	%	N	%	N	%	N	%	N	%	N	%	N	%	N	%
Enrolled in any *Caja*	71	48	606	41	1,725	50	1,596	59	1,585	68	732	69	2,256	51	1,289	54
Resigned the benefit/ becomes employed	7	5	51	3	343	10	382	14	334	14	80	8	289	6	221	9
Beneficiary of EPS*	54	36	821	55	909	26	438	16	297	13	166	16	596	13	486	21
Other	16	11	18	1	487	14	289	11	125	5	88	8	1,311	29	371	16
Total	**148**	**100**	**1,496**	**100**	**3,464**	**100**	**2,705**	**100**	**2,341**	**100**	**1,066**	**100**	**4,452**	**100**	**2,366**	**100**
Benefits for Previously:	1,472		7,845		10,893		8,355		9,442		10,961		9,330		8,595	
Enrrolled in *Caja*	749		6,690		6,804		7,230		7,804		8,617		7,977		6,781	
Not Enrrolled in *Caja*	723		1,155		4,089		1,125		1,638		2,344		1,353		1,814	
Rejection Rate (%)	**10.1**		**19.1**		**31.8**		**32.4**		**24.8**		**9.7**		**47.7**		**26.4**	

Source: Ramírez (2009). * Includes both beneficiaries by their own contribution or of that of a third party.

A key aspect of the 2003 US reform is its requirement that potential beneficiaries not be beneficiaries of the CR regime, In addition this restriction interacts with the existing way that policy establishes eligibility for the SR. To target people for the SR, officials first interview about 70 percent of the poorest households. Secondly, using the data gathered from these interviews, they construct a welfare index. Finally, officials used this index ï known as a ñSisben scoreò to classify households into one out of six levels. Only households classified in the two lowest levels of Sisben scores were eligible to become beneficiaries of the SR. Additionally, any household that was beneficiary of the CR could not become beneficiary of the SR.

As observed by Camacho and Conover (2008), there are beneficiaries of the SR at both sides of the Sisben ñcutoff score.ò This point occurs between levels two and three. But the share of beneficiaries changes discontinuously at this score. In theory, knowing that enrollment in the SR changes discontinuously at this rhreshold does not guarantee that the percentages (i) of non-CR beneficiaries or (ii) of US beneficiaries also change discontinuously at this cutoff score. Nonetheless, because households at Sisben levels one and two are more likely to benefit from the SR than those in levels three or above, the expected benefit of being a beneficiary of the CR should be lower for households to the left the threshold than for those to the right of it.

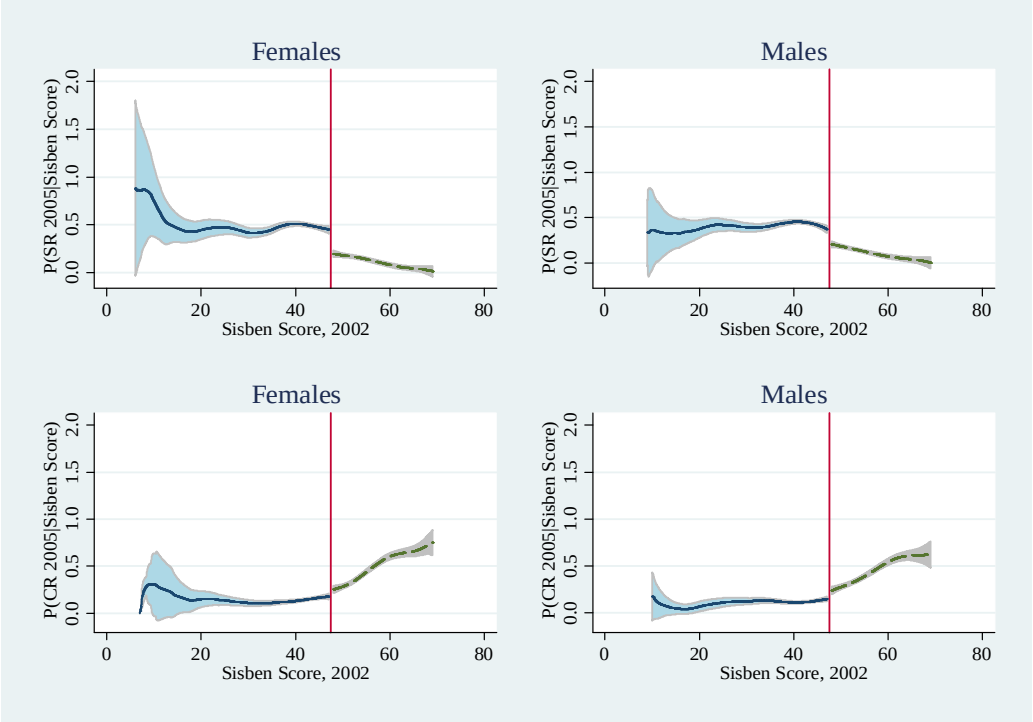
We find evidence of the foregoing relationship in our data. The graphs at the top of Figure 12 show the 2005 probability of enrollment in the SR as a function of individualsô 2002 Sisben score. The graphs at the bottom of the figure show the probability of enrollment in the CR. The graphs include a vertical line at the ñcutoffò score of 47 between Sisben levels 2 and 3.

As shown by Figure 12, the probability of enrollment in the SR (CR) declines (increases) discontinuously at the cutoff. Below we illustrate the change in the probability of US enrollment around the cutoff.

As previously mentioned, there is anecdotal evidence that some former informal workers who became formal employees have asked their employers not to enroll them in the CR so that they would not lose their affiliation in the SR, and there is quantitative evidence that the SR decreases formality by almost 4 percent.[31] This type of situation is more likely the less stable is the formal job of the worker. These workers recognize that if they lose their job, they have to reapply to the SR, and would not be covered for any health insurance until the government enrolls them again in the program.

Gaviria et al. (2007) demonstrate that the SR program adversely affects womenôs labor force participation in the formal sector. Because women face greater risk of losing their formal jobs, they also are at greater risk of being without health insurance. As a result, some women either opt for the sure thing by remaining in the SR instead of allowing their formal employer to enroll them in the CR. Consequently, they have less incentive than their male peers to become formally employed.

Figure 12: Discontinuity in the Probability of Enrollment in the SR and CR Around The Cutoff Sisben Score of 47 between Sisben Levels 2 and 3



Source: Sisben Surveys 2002 and 2005

[31] See Camacho et al. (2009)

Although we have shown the existence of a discontinuity at the Sisben cutoff score for enrollments in the SR and in the CR, whether there is a discontinuity in the share of beneficiaries of the US at this cutoff score, is an empirical question. We next turn to assess the evidence for this discontinuity using data for both beneficiaries, and non-beneficiaries.

2. Data

We have two sources of data available for our evaluation of the impact of the US. One source of data is provided by two *Cajas*: Comfama and Comfenalco. These are the only *Cajas* that operate in the state of Antioquia, a state with a total population of nearly 6 million people. The stateôs capital is the city of Medellín. Data provided by Comfama includes 47,600 household heads who were US beneficiaries. These *Caja* participants received US benefits at some point between September 2003 and December 2009. Data provided by Comfenalco includes nearly 23,000 individuals. These *Caja* participants received US benefits at some point between February 2004 and December 2008.

The second source of data is from successive Censuses of the whole population from Sisben surveys of Medellín for three years: 2002, 2005 and 2009.[32] The Sisben dataset is not a panel of households. Instead it consists of three cross-sections from a census of roughly the poorest 70 percent of the population. To create a panel data set, we matched household records across the three years.[33].

As is shown by Appendix 5, although the 2002 Sisben survey was implemented around 1994, most individuals were interviewed in 2002. Between 2003 and 2005, the country updated the methodology used to estimate the Sisben score, which determines eligibility for social benefits, and then, updated information for all individuals both in 2005 and 2009. Our final sample of beneficiaries consists of 6,004 beneficiaries who were matched to both the 2002 and 2005 Sisben surveys, and 14,364 beneficiaries who were matched to both the 2005 and 2009 Sisben surveys.[34]

It is important to highlight that the information contained in the Sisben survey is used to calculate the Sisben score, based on which households are classified in one out of six Sisben levels. Individuals belonging to Sisben levels 1 or 2 become eligible to be enrolled in the Subsidized Regime, as was explained above, but they are not automatically enrolled.

The survey includes a question that asks whether individuals are enrolled in the SR or the CR. We use that question to determine whether these individuals were CR beneficiaries in the baseline years, 2002 and 2005 and in the follow up years, 2005 and 2009.[35]

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[32] The Sisben data for Medellín is available every three months. Nonetheless, it is only rarely updated by the households (see more below). The data might become valuable if we were to use Sisben data much closer to the moment that individualsô enroll in the program. However, the endogenous updating of information would pose additional challenges to identification.

[33] We have the identification number of each household member to do the match.

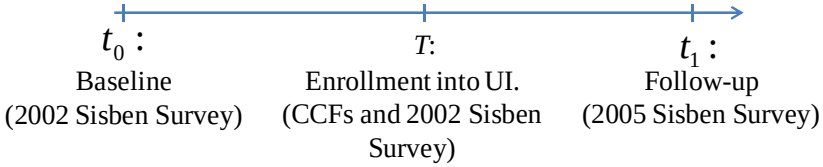
[34] See Appendix 5 for additional details of the way our final sample was constructed.

[35] The few observations of the 2005 Sisben survey not collected in 2005 are of people who asked the municipality of Medellín to update their information. Note that only households whose standard of living

By matching the *Cajas* data with the Sisben data, we have information of US beneficiaries and non-beneficiaries at three points in time.

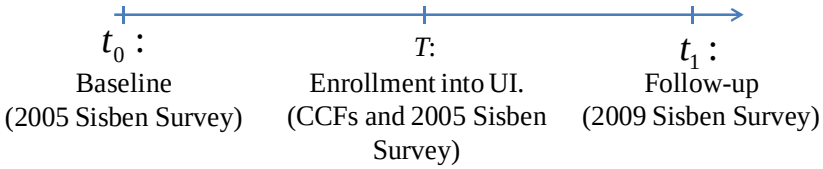
Figure 13 shows the timeline considered in our exercise. We use 2002 Sisben survey for our baseline data, which takes place at t_0 in the figure. Individuals enroll into the unemployment subsidy at *T*, which we know from data provided by the *Cajas*. Then we observe individuals again in the 2005 Sisben survey, which takes place at t_1 in the figure (Period 2002-2005).[36]

Figure 13. Timing of the key events and data used at each moment



Similarly, we use 2005 Sisben survey for baseline data and the 2009 Sisben survey as follow-up, for those individuals enrolled into the unemployment subsidy at *T*, between those two dates (Figure 14) (Period 2005-2009)

Figure 14. Timing of the key events and data used at each moment



To clarify the content of these figures, first note that the subsidy lasts for six months after enrollment, for which we exclude from the sample those beneficiaries who were matched to the Sisben survey less than six months after their enrollment. Second, to limit the possibility of outcomes being affected by other interventions different from, the US we limit the length of time between the baseline and the enrollment in the US, and we also focus on the impacts of the program in a limited period of time, namely within the following 1.5 years after they exit from the US program. Thus, we exclude from the sample those beneficiaries whose differences in time, between the date of enrollment and both, the baseline and follow up (plus six months of subsidy), are larger than 24 months. That is we exclude those for whom,

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deteriorated would be willing to ask for a new interview to update their status, and lower their Sisben score. The same follows for people whose data was not collected in 2002 but between 2003 and 2004. All individuals in the last round were interviewed in a short period of time between late 2009 and early 2010.

[36] We use Sisben survey for Medellín (second largest city in Colombia) because the data provided by the *Cajas* (Comfama and Comfenalco), only cover municipalities of Antioquia. Among the subsidies granted by these two *Cajas*, a large share of those, were for people who at the moment of the subsidy were living in Medellín.

$$T - t_0 > 24\ months$$

$$t_1 - T > 24\ months$$

However, we repeat the exercises that will be presented later, covering only 18 months in order to assess the robustness of the results.[37]

Third, there might be differences between the way individuals present themselves as household heads to the *Cajas* and the way they self-classify as such in the Sisben survey, or their parenthood status might change between the time they were interviewed for the Sisben survey and the time they enrolled in the US. To address this issue, first, we separately estimate the impacts of the US for men and women. Second, we use as comparison group people selected from the whole sample of men (or women) at the baseline years (2002 or 2005), in case beneficiaries were household heads at the moment they enrolled in the US, but not necessarily at the baseline or follow up (2005 or 2009 respectively). Third, alternatively we use as a comparison group those who were household heads at the baseline.

Outcomes to Study

The Sisben survey includes key outcomes of interest for this evaluation; these outcomes are available for both of the baseline surveys, 2004 and 2007; and both of the follow up surveys, 2007 and 2009. The outcomes that we use are the following:

- Labor Market Participation (LMP): The Sisben survey reports whether individuals are working, looking for a job, or inactive. In the latter case, it tells us whether individuals are studying, working in any home production activity, handicapped, or doing nothing. This variable is equal to one if the individual is either working or unemployed, and zero otherwise.
- Unemployment: This variable is equal to one if the individual is unemployed, and zero otherwise.[38]
- Formality (EPS): we know if the beneficiary was enrolled in any EPS. This variable is equal to one if the individual is enrolled in an EPS, and zero otherwise.
- School attendance: This variable is equal to one if the individual is attending any academic institution, and zero otherwise.
- Individual earnings
- Household earnings: Total earnings of all household members.
- School Index: we construct an index defined as the ratio between the number of children of the household between 5 and 12 years old at the baseline, who are attending school and the total of children between 5 and 12 years old.
- Weight, Height and Body Mass Index (BMI) and Apgar at Birth: we match Comfama data with Vital Statistics Records of births to assess these outcomes. The BMI is the ratio between the weight of the children in kilograms to their squared height in meters.

[37] Those exercises are available upon request but are not included in this article.

[38] Someone is considered unemployed in Colombia if he or she searched for a job during the last month and did not find one.

The Apgar is determined by evaluating the newborn on five simple criteria on a scale from zero to two, then summing up the five values obtained. The resulting Apgar ranges from zero to 10. The five criteria are Appearance, Pulse, Grimace, Activity and Respiration. Apgar1 and Apgar5 refer to the same concept assessed 1 and 5 minutes after the child was born.

Descriptive Statistics

The table in Appendix 6 includes descriptive statistics of the variables from the Sisben survey that we use in our matching estimations. Some of these variables are school attendance, earnings of household, earnings of the individual, labor market participation, unemployment, household headôs gender, number of children under 6 and 18 years old, householdsô size, etc. They also include a panel with the descriptive statistics for the complete sample of individuals who became US beneficiaries between 2002 and 2005, and another panel for non-beneficiaries during the same period. Each panel contains information for females and males, and for formal and informal workers, by gender. The table includes the mean and standard deviation of the outcomes of the individuals based on the information included in the 2005 Sisben survey, and their baseline characteristics from the 2002 Sisben survey.

According to the baseline information, non-beneficiaries are better off than beneficiaries, contrary to the finding by Mazza (2000) who found that unemployment insurance beneficiaries from several countries she analyzed ï including Argentina, Barbados, and Brazil ï are middle income workers rather than poor workers. She reported that unemployment insurance beneficiaries in these countries had higher rates of school attendance, higher household and individual earnings, lower unemployment rates. Futher they were more likely to have secondary education, their households are less likely to be headed by a woman, have less children under 6 and 18, have fewer members, are less likely to own the house they live in, and are less likely to live in socioeconomic stratum 1 (i.e., the poorest stratum)[39].

Similar conclusions are arrived at by studying Appendix 7, which presents the results of the whole sample for the period between 2005 and 2009; and from appendices 8 and 9 that present the statistics for individuals who were household heads during the baseline years covered in appendices 6 and 7, respectively.

3. Identification Strategy and Estimation

In this section we propose several different ways to identify the effects of the US program on a variety of outcomes. Each method solves the selection problem in a different way. The

[39] Urban areas in Colombia are split into six socioeconomic strata in which the first has the lowest income levels (the poorest). The strata are used by authorities to spatially target social spending like that in the supply of public services (water, electricity), housing, health insurance for the poor, etc. Note that socioeconomic stratification is assigned to the housing units, and it is a method of spatial targeting which is a function of the housing characteristics and its amenities, while the Sisben levels are assigned to the households, and it is a function of the household and housing characteristics.

estimators that we consider are based on (i) regression discontinuity designs (RDD), matching difference-in-differences estimators and matching cross-sectional estimators.

In what follows we will refer to the impact of the ñtreatment on the treatedò as our parameter of interest. Treatment status is denoted by the binary variable *D*,

D=1 for treated individuals, and
D=0 for untreated individuals.

The untreated individuals comprise the comparison group. We estimate the effect of *D* on an outcome *Y*, where Y_1 denotes the treated outcome and Y_0 denotes the untreated outcome, After we condition on a set of observed variables *X*, we define the impact of the treatment on the treated as follows:

$TT=E(Y_1-Y_0|D=1,X)$.

3.1 Regression Discontinuity Design

RDD is an appropriate identification strategy whenever assignment to treatment is based on individualsô score on a continuous variable, and also when those individuals with a score at or below a clearly defined cutoff are more likely to become enrolled that those whose scores fall beyond that cutoff. Since individualsô characteristics change continuously along the assignment variable, individual characteristics on both sides of the cutoff are nearly identical. The only difference (in the limit) between the two groups around the cutoff score is on whether or not it is likely they enrolled in or received the treatment. This design allows the evaluator to use individuals close to the cutoff score as if they were drawn from an experimental design.

As shown above in section IV.1, the targeting of the SR implies that the probability of enrollment to the SR, and to the CR, changes discontinuously at the cutoff between Sisben levels 2 and 3. Since the US requires its applicants not to be enrolled in the CR, in this section we assess whether such requirement is also implying a discontinuity in the enrollment to the US at the cutoff between Sisben levels 2 and 3, in order to apply RDD to identify the impact of the US on a subset of outcomes around the cutoff point.

Strategy

First, let us analyze how this approach allows us to identify the impact of the US for individuals whose Sisben score is close to the cutoff score. According to this approach, selection for treatment depends either deterministically or probabilistically on a continuous variable *z*, the Sisben score, so that either we say that the design is *sharp* because selection for treatment is determined deterministically as a function of *z*, and changes discontinuously at the cutoff z_0; or the design is *fuzzy* because selection for treatment changes probabilistically, and the probability of treatment changes discontinuously at the cutoff score.

In this context, the outcome Y can be expressed as a function of the treatment D and the controls X: $Y_i = \alpha \cdot X_i + \beta \cdot D_i$.[40] Note that β identifies the impact of the US only around the cutoff score. So this regression is run locally using only treated and untreated individuals whose Sisben score is close to the cutoff score.

We now provide empirical evidence that support the standard assumptions required in a RDD. According to our rationale, the system used to target the SR regime coupled with the eligibility requirements for US claimants imply that probability of enrollment into US should change discontinuously at the cutoff between Sisben levels two and three. This threshold determines the boundary between the eligible and non eligible population to the SR. This test would ensure that assumption (i) above is satisfied.

Figures 15 and 16 show local linear regressions of estimates of the probability of enrollment in the US conditional on the Sisben score. For these figures, we use data for individuals who became beneficiaries between 2002 and 2005, and matched it to their responses in 2002 (baseline) and 2005 (follow-up) Sisben surveys. The analysis depicted in the figures is based on samples restricted to individuals whose outcomes are observed in the 2005 Sisben survey (t_1), two years after the enrollment date in the US (T), that is, as given above, individuals for whom $t_1 - T < 24$ months.

The figures include results for the sample of formal and informal individuals according to two alternative definitions of formality. (See Table 13) Administrative data provided by the *Cajas* allow us to know whether or not US beneficiaries were previously enrolled in a *Caja*, or in an EPS. By contrast, for individuals in the comparison group, the Sisben survey data do not allow us to know whether individuals in the baseline were enrolled in a *Caja*, but only whether they were enrolled in an EPS. This distinction explains why we consider two groups of formal individuals: one (A in the table) we denote ñEPSò that takes as formal anyone who was enrolled in an EPS in the 2002 Sisben survey (baseline), regardless of whether he or she was enrolled in a *Caja*. A second group (B in the table) we denote ñ*Caja*ò that takes as formal in the treatment group (US beneficiaries) only those who were enrolled in a *Caja* at the baseline, whereas formality in the comparison group are those previously enrolled in an EPS.

The four graphs at the top in Figure 15 summarize our results for females. The two graphs at the top assess the discontinuity for the samples of formal females based on previous enrollment in the EPS (left) or the Caja (right). The two graphs at the bottom assess the discontinuity of informal females based on the same respective definitions. The four graphs at the bottom include the same information for females, but with the additional restriction that individuals had enrolled in the US within two years after they were observed in the 2002 Sisben survey (t0). Figure 16 contains the same respective information for males. The vertical lines in all of the figures specify the cutoff values between Sisben levels 2 and 3.

[40] The identifying assumptions underlying this method are three (Hahn et al. (2001): (i) $lim_{z \to z0^-} E(D=1 \mid z_i = z)$ and $lim_{z \to z0^+} E(D=1 \mid z_i = z)$ exist and they are different; (ii) $E(X \mid z_i = z)$ is continuous at $z = z_0$.; and (iii) $E(\beta \mid z_i = z)$ regarded as a function of z, is continuous at z_0.

Table 13. Definitions of formality according to previous enrollment in an EPS or *Caja*.

A. Formality based on previous enrollment to EPS (Sisben database)	Both Beneficiaries and Non Beneficiaries of the UI (D=1) are formal if previously (at the baseline) enrolled to EPS

B. Formality based on previous enrollment to *Cajas* (*Cajas* database)	Beneficiaries of the UI (D=1) are formal if previously (at the baseline) enrolled to *Cajas*
	Non beneficiaries of the UI (D=0) are formal if previously (at the baseline) enrolled to *EPS*

To obtain estimates of the probability of enrollment in the US from Figure 15, we use the data for people on the left of the cutoff between Sisben levels two and three as the treatment group. We use only people on the right of the same cutoff as a comparison group. For females, the figures show the existence of a discontinuity in the probability of enrollment in the US between Sisben levels two and three in all graphs of Figure 15 and that of informal females (based on previous EPS enrollment) for the restricted sample. Figure 16 shows the existence of discontinuity only in the samples of formal males, being somewhat weaker under the definition of formality based on EPS enrollment.[41] The lack of discontinuity among informal males might be explained by the few number of beneficiaries available in our data. As it is shown in Figure 17 there are much fewer observations of informal males than females. We have nearly 340 and 250 informal male beneficiaries based on our EPS and *Caja* definitions respectively, while for informal females we have almost five times as many.

Overall, the results summarized in Figures 15 and 16 indicate that the conditions for a valid RDD hold better for formal than for informal workers, and better for females than for males.[42]

A valid RDD also requires that individuals cannot strategically manipulate their Sisben scores to affect their probability of receiving US should they become unemployed. This could be done by strategic response, cheating in response, corrupting officials, or any other means. Bottia et al. (2008) provide evidence that the denominated old Sisben scores, based on the mechanism that was used from 1993 until 2003, had serious signals of these sorts of limitations. However, they report that the new Sisben scores, (the one we use in our

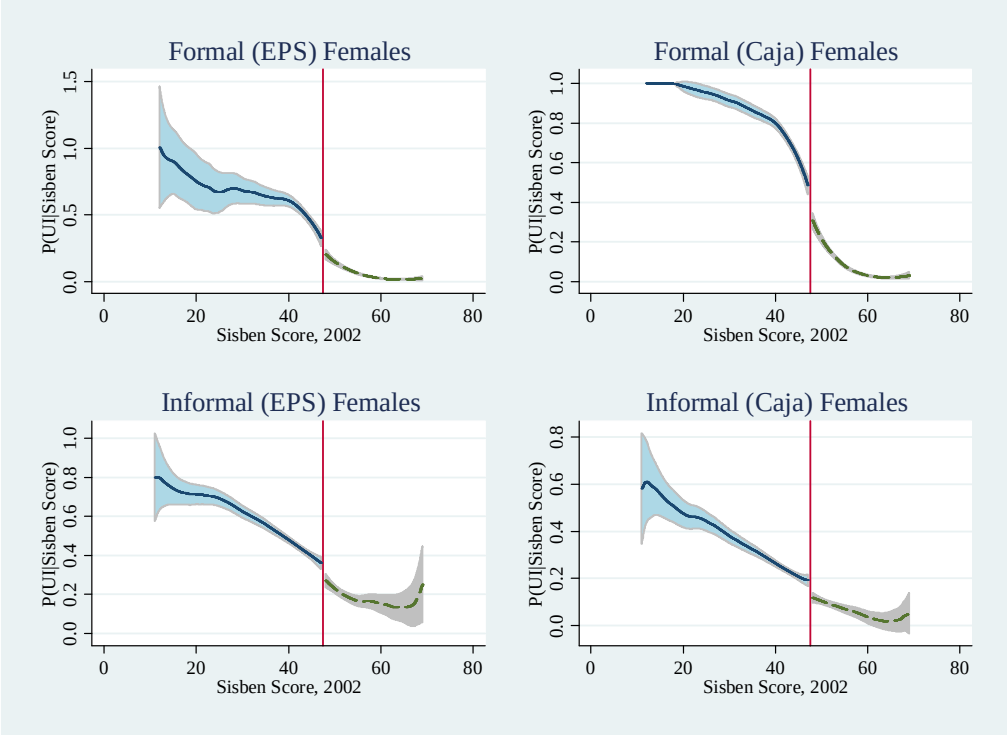
[41] We also assessed the existence of discontinuity between Sisben levels 1 and 2 (not shown here) but we found none. A similar exercise was done with individuals who became beneficiaries between 2005 and 2009, and were matched with the 2005 and 2009 Sisben surveys. The exercise sought to assess whether the discontinuity observed based on data of beneficiaries between 2002 and 2005 was also found for individuals who became beneficiaries between 2005 and 2009. We found no clear discontinuity in the FONEDE enrollment rate around the cutoff between Sisben levels two and three, most likely due to the changes introduced in 2004 the way the Sisben score was estimated. Those changes had not yet distorted the way the SR was targeted for individuals observed between 2002 and 2005, as it did for individuals observed between 2005 and 2009.

[42] A similar exploration (not included here) was performed on the population of household heads and we found a higher discontinuity among formal males than females, although in the informal population there was only discontinuity among females, and among them it was higher based on the *Cajas* definition.

estimations), which was implemented as from 2004, performed significantly better. Camacho and Conover (2008) also provide evidence of these limitations with the old Sisben scores, but contend that in some of the larger municipalities the system performed well.

To check to see whether such ñgamingò of the Sisben scores took place on a wide enough scale to potentially invalidate our RDD, we test whether there is a smooth distribution of individuals around the cutoff z_0. As shown by Figure 17, the distributions of non-beneficiary households changes smoothly around the cutoff between Sisben levels 2 and 3, signaling that individuals did not systematically manipulate their Sisben scores in order to gain access to the SR, the US or other subsidies. By contrast, the distribution of US beneficiaries by Sisben scores changes much less smoothly at the cutoff score. This difference is consistent with the anticipated discontinuity in the targeting of unemployment subsidies.[43]

Figure 15. The Probability of Enrollment around the Cutoff for Females: Individuals whose outcome is observed within two years of enrollment in the US



[43] A similar exploration (not included here) was performed on the population of household heads and we found similar results supporting the requirement of no manipulation of the Sisben score.

Women whose outcomes are observed within two years of enrollment in the US, and whose baseline data also was observed within two years of enrollment

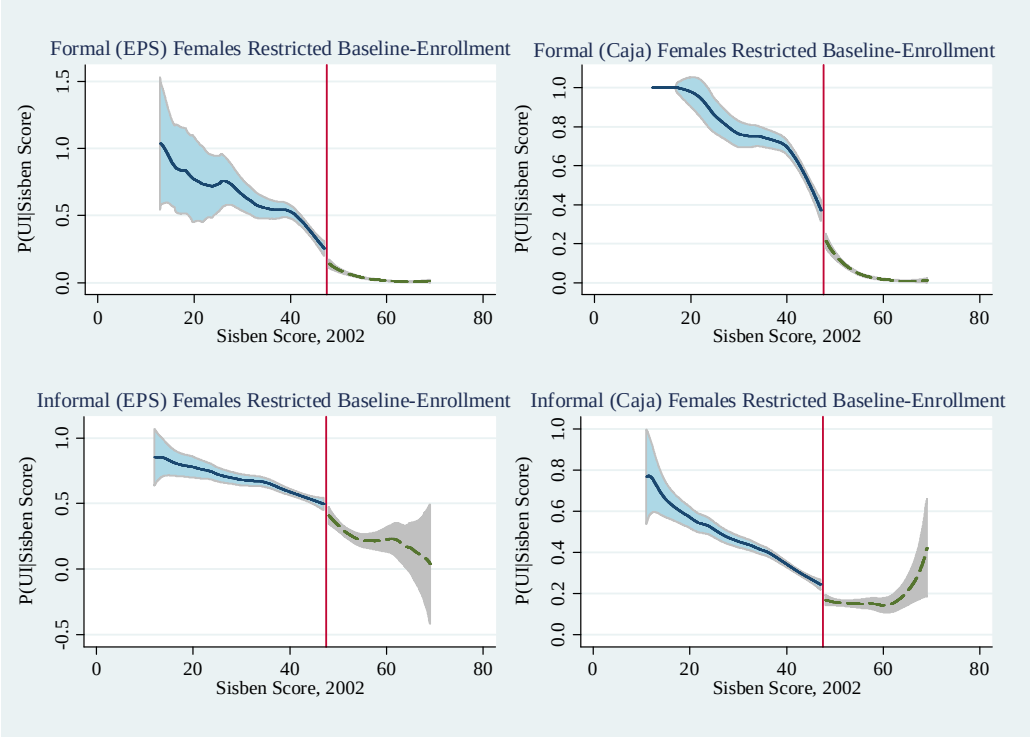
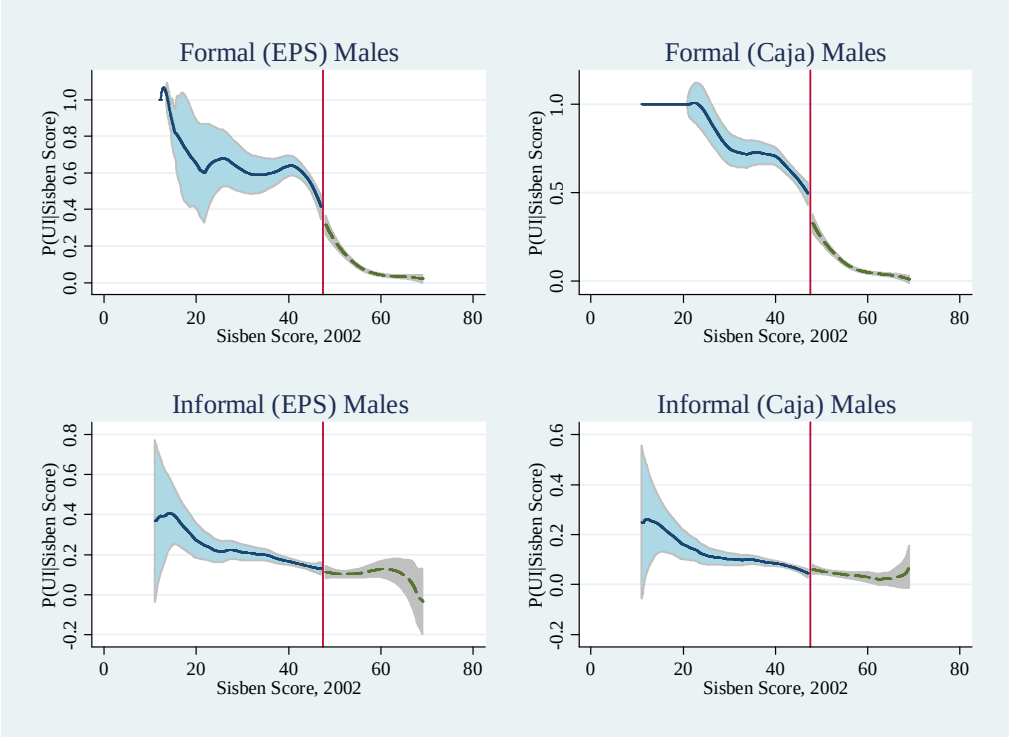


Figure 16. The Probability of Enrollment Around the Cutoff for Males: Individuals whose outcome is observed within two years of enrollment in the US



Men whose outcomes are observed within two years of enrollment in the US, and whose baseline data also was observed within two years of enrollment

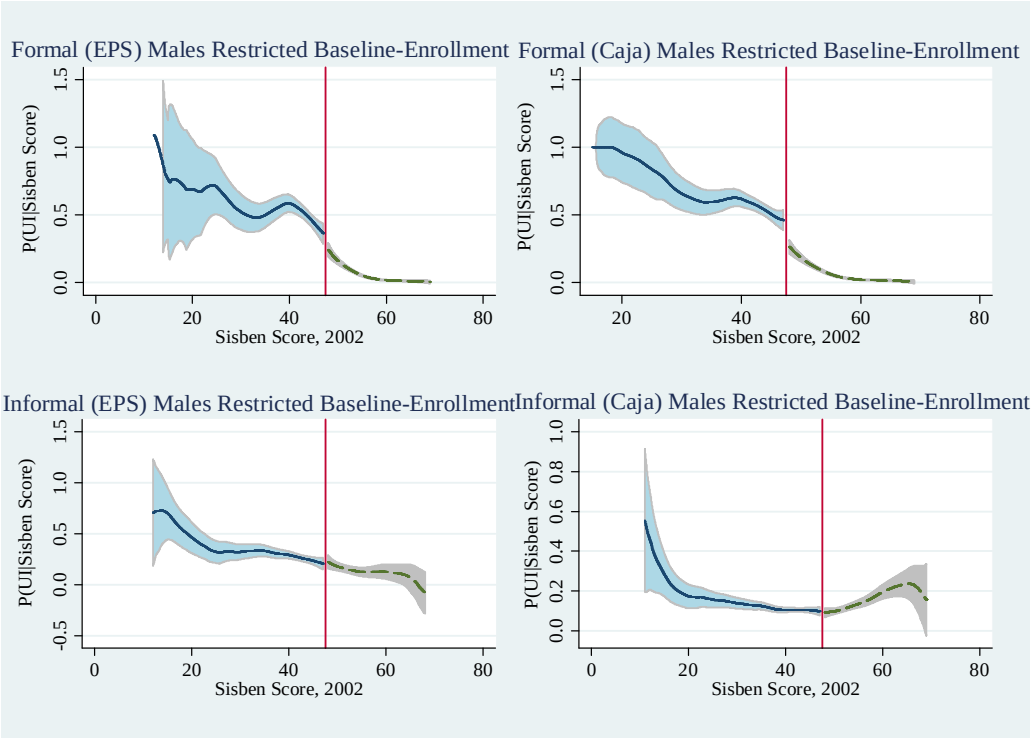
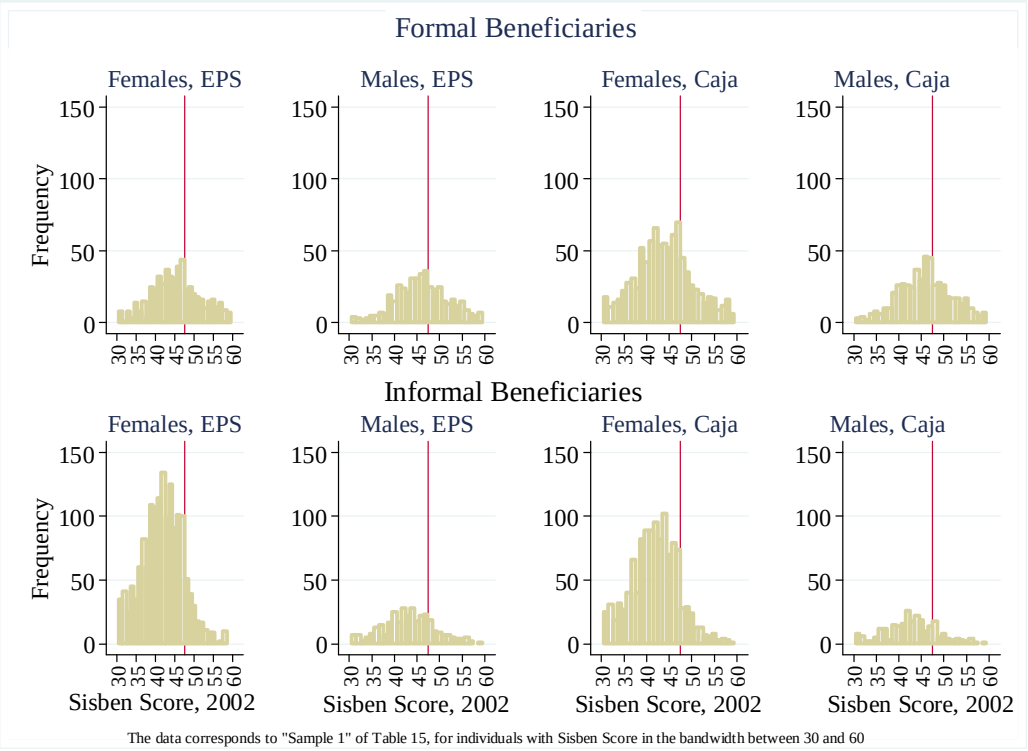


Figure 17. Distribution of Individuals, by Sisben Score



Source: Comfama, Comfenalco, and Sisben 2002.

Finally, we test for discontinuity at the cutoff of the outcomes of interest both at the follow up and at the baseline. We need to find a discontinuity of the follow up outcomes at the cutoff if there was a non-zero effect of the intervention, and ideally, we should find no discontinuity at the cutoff on lagged outcomes, since those are not supposed to have been affected by the subsequent intervention.[44] Figures in Appendixes 6 and 7 show local polynomial regressions of the expected follow up and lagged outcomes respectively, conditional on the Sisben Score; using data on each side of the cutoff. We only included the figures obtained with the definitions of formality based on enrollment to an EPS since those based on enrollment to a *Caja* were very similar.

Let us first analyze the figures in Appendixes 6 and 7. When we consider the probability of enrollment in the Contributive Regime (EPS) we find that in the follow up it is always highly discontinuous at the cutoff for all socioeconomic groups, and it is also discontinuous at the baseline for formal employees, both males and females. Labor participation does not show a discontinuity at the follow up in any case. Furthermore, it is discontinuous at the baseline for informal females. Unemployment is not discontinuous at the follow up or baseline. School attendance is only discontinuous at the follow up for informal males. Household income is discontinuous both at the follow up and the baseline in all cases. Finally, the earnings are discontinuous at the follow up for informal females and males, and

[44] See Lee and Lemieux (2010), and Lee, Moretti and Butler (2004).

somewhat for formal males; while they are discontinuous in all cases but for formal females, at the baseline.

Based on these figures we can expect RDD estimates to be more likely to correctly identify the impact of the US on enrollment to an EPS of informal employees, on school attendance for informal males, and on earnings for formal females. The presence of discontinuities at the baseline of some of the outcomes considered suggest the potential existence of other intervention that would be affecting them at that point simultaneously with the US. The main intervention to consider is the Subsidized Regime, which case we analyze below.

To identify the effect of the US on an outcome Y_i, α, we get both Wald and regression RDD estimates. We get Wald RDD estimates according to Hahn, Todd and Van der Klaauw (2001), estimating the ratio

$$\hat{\alpha} = \frac{\lim_{S \to \bar{S}^+} E[Y_i \mid S_i = \bar{S}] - \lim_{S \to \bar{S}^-} E[Y_i \mid S_i = \bar{S}]}{\lim_{S \to \bar{S}^+} E[US_i \mid S_i = \bar{S}] - \lim_{S \to \bar{S}^-} E[US_i \mid S_i = \bar{S}]} \quad (1)$$

Where $\lim_{S \to \bar{S}^{+(-)}} E[Y_i|S_i = S]$ is the expected value of outcome Y_i at the right (-: left) of the cutoff $\bar{S}$, and $\lim_{S \to \bar{S}^{+(-)}} E[US_i|S_i = S]$ is the probability of being enrolled in *US* at the right (-: left) of the cutoff $\bar{S}$. We also follow van der Klaauwôs (2002) approach for a ñfuzzyò RDD, and estimate the following equation:

$$Y_i = X_i\beta + \alpha \cdot E(US|S_i) + k(S_i) + w_i \quad (2)$$

In (2), Y_i is again the outcome of interest, X_i is a vector of control variables, US_i is a dummy variable indicating whether individual *i* was beneficiary of the US, S_i is the Sisben score, and $k(S_i)$ is a polynomial on S_i.[45]

The expected value in (2) is obtained from the following first-stage estimation:

$$US_i = X_i\beta + f(S_i) + \gamma \cdot 1[S_i \geq \bar{S}] + \varepsilon_i \quad (3)$$

In (3), $f(S) = \Sigma_0^3 \psi_{0k} S^k + \Sigma_1^3 \psi_{1k} (S - \bar{S})^k \cdot 1[S \geq \bar{S}]$, where $1[S \geq \bar{S}]$ is an indicator function equal to one if the term in brackets is true, and zero otherwise. Significance of the γ coefficient guarantees discontinuity of the probability of enrollment in the US at the cutoff between Sisben levels 2 and 3.[46] As explained by van der Klaauw (2002), if $k(S)$ and $f(S)$ are correctly specified, this two-stage procedure leads to consistent estimates of the effect of US, α, on our outcomes.

[45] Specifically, $k(S) = \Sigma_1^3 \beta_j S^j$.

[46] Note that in this case, some subsamples that were discontinuous in the figures above might not be discontinuous according to equation (1), because this equation is controlling for the polynomial in *S* and additional variables.

In addition, provided that X vary smoothly around the cutoff, the TT estimate $\delta\tilde{E}$ would lead to the same estimate shown in equation (1)

Therefore, if $\gamma \neq 0$, the denominator in the foregoing expression is non-zero. This, guarantees an estimable value of α. Finally, it is important to say that the 2002 Sisben Score, our forcing variable, was estimated that year based on a weighting of variables that led to discrete values. We follow Lee and Card (2008) to correct for the lack of continuity in that variable, by estimating equation (2) using robust and clustered (on the individual values of the Sisben Score) standard errors.[47]

Notice again that identification also requires that no other factors, different to the program of interest, cause the discontinuity. As it was explained above, enrollment to the Subsidized Regime also changes discontinuously around the same cutoff enrollment to the US changes. If the SR has any impact on the outcomes that are being considered to be affected by the US, then estimates omitting that effect would be biased, and in that case, it is important to know the direction of such bias. Consider a model with two treatments and assume for simplicity that individuals are never treated simultaneously for both interventions, then our model above would become $Y_i = X_i\beta + \alpha_{US} \cdot E(US|S_i) + \alpha_{SR} \cdot E(SR|S_i) + k(S_i) + w_i$. It is straightforward to show that in this case,

$$\delta\tilde{E}_{US} = [Y_i^+ - Y_i^- - (x_{SRi}^+ - x_{SRi}^-)\ \alpha_{SR}\ ]/(\ x_{USi}^+ - x_{USi}^-) = \delta\tilde{E} - (x_{SRi}^+ - x_{SRi}^-)\ \alpha_{SR}\ /(\ x_{USi}^+ - x_{USi}^-). \quad (4)$$

Where $(Y_i^+ - Y_i^-)$ is equal to the numerator in (1), $(x_{USi}^+ - x_{USi}^-)$ is its denominator, $(x_{SRi}^+ - x_{SRi}^-)$ is the respective term for the Subsidized Regime, and α_{SR} is the impact of the Subsidized Regime on Y_i. In other words, by using RDD to estimate α_{US} in the presence of other intervention like the SR, according to (4) we would be overestimating (underestimating) α_{US} if α_{SR} is positive (negative), in a magnitude equal to the second term in the right hand side of equation (4).

By 2005, there were in Medellín more than 150,000 beneficiaries of the Subsidized Regime, but only about 7,000 beneficaries of the Unemployment Subsidy. That is, beneficiaries of the US were nearly 4.5 percent of the beneficiaries of the Subsidized Regime. Since the number of beneficiaries of the US is small relative to the number of beneficiaries of the SR, we can drop the beneficiaries of the US from our data and use equation (2) with the rest of the data to get an unbiased estimate of $(Y_i^+ - Y_i^-)$ and $(x_{SRi}^+ - x_{SRi}^-)$ within that population, to estimate the impact of the SR on our outcomes of interest. We present those results below.

[47] See also Lee and Lemieux (2010).

In the following two subsections, we present RDD estimates of the effect of the unemployment subsidy on a subset of outcomes, focusing on individuals between 20 to 55 years of age.

3.1.1 Results

Table 14 presents the effects of the US on our set of outcomes based on the Wald and Regression RDD estimates defined in equations (1) and (2). There are five panels in the table, one for each outcome: Labor Force Participation, Unemployment, School Attendance, Household Income, and Earnings; and eight columns, one for each population considered: four for females and four for males. For each gender we have two panels for formal employees, one based on their enrollment at the baseline in an EPS (Contributive Regime) and the other based on their enrollment to a *Caja*. The definitions of formal and informal workers were explained above in Table 13. The other two panels per gender are for informal employees based on the same two concepts, EPS and *Caja*. For each outcome, we obtained Wald estimates using bandwidths of 2, 4 and 8 points of the Sisben score, and for each bandwidth, we report the estimated numerator and denominator of equation (1), and its corresponding ratio, which is the parameter of interest. For the regression estimates, we the bandwidths used in the regression were of 20 points without control variables (row A), 20 with control variables (row B) and 30 with control variables (row C).

The shadow areas of our Wald estimates are those in which we did not find evidence of discontinuity, that is, where the coefficient of the denominator was not statistically different from zero. The blank areas in our regression estimates are those in which we did not find evidence of a discontinuity using the regression estimates of equation (2), that is, when the γ coefficient was not statistically different from zero.

According to our Wald estimates, no ratio between the numerators and denominator are statistically different from zero, and thus, there is no effect of the US on the assessed outcomes for any of the populations.

When we focus on or regression RDD estimates, we omit the results for informal males since we did not find the required discontinuity at the cutoff for that group. Evidence of the discontinuities is presented in the last three rows of the table, where we present, for each bandwidth considered (A, B or C), the estimated γ coefficients. Row A only includes as control variables in equations (2) and (3), the polynomials in S, $k(S)$ and $f(S)$, and individuals within a ñbandwidthò of 20 Sisben points, 10 on each side of the cutoff; row B uses the same bandwidth, and in addition to the polynomials in S, it also includes a set of control variables which descriptive statistics are presented in appendices 6 to 9. Finally, row C also includes the polynomials in S and the control variables from row B, but in that case it includes individuals with Sisben scores in the interval $30 < S < 60$. The sample considered consisted of individuals whose outcome was observed within two years of receiving the US.[48]

[48] Similar results were obtained when we considered individuals whose baseline survey also occurred within two years prior to enrollment in the US.

Since in the population studied, the subset of beneficiaries at t_1 who are enrolled in an EPS and also belong to Sisben levels 1 or 2 is negligible, we are unable to use RDD to identify the effect of the US on EPS enrollment.

Overall, the only regression RDD results we include are those in which we found a discontinuity, with at least the 5 percent of significance. We find a significant discontinuity for formal and informal females. For males, we only find the discontinuity for the formal workers. Our estimates consistently imply a negative impact of the US participation on the earning of beneficiaries, and a somewhat weaker negative impact on household earnings. Although the results show a strong and robust reduction of earnings of beneficiary females and males, the magnitude of the estimated coefficients seems too large. To see this point bear in mind that the baseline earnings of beneficiary females vary between $50,000 and $90,000 Colombian pesos, while those of beneficiary males vary between $90,000 and $150,000 Colombian pesos (See Appendix 6). The negative effect nonetheless with be partly explained by the so called earnings dip pointed at by Ashenfelter (1978), and by Heckman and Smith (1999). In particular, if individuals experienced a drop in earnings before applying to the program, it would be very difficult to find a comparison group able to resemble the earnings pattern of the hypothetical ideal control group. Since individuals in the treated group in this program are required to have experienced unemployment, their earnings before entering are very likely to have dropped and thus, if their baseline earnings are measured relatively before the drop takes place, and the follow up is measured when the earnings have not recuperated their former level, then the estimated effect is very likely to be underestimated if we are not able to simulate a comparison group that would experience a similar earnings pattern. In the next section we perform a matching differences-in-differences estimation with a similar sample of individuals we are using here, but also, with a sample that only considers individuals who were unemployed at the baseline.

We find no effect on labor participation or unemployment. Once we use a larger bandwidth, as we do in panel C of the Tables, the discontinuity becomes significant in a greater number of cases. The larger the bandwidth, nonetheless, the more important it is for our polynomial $k(S)$ and $f(S)$ to be correctly specified.

Finally, the table shows a positive effect on formal femalesô school attendance and a positive on formal malesô. There is also a positive effect in the case of informal females, although those coefficients are incredibly large.

Altogether, the results reveal some limitations in the power of the regression RDD to correctly identify the impact of the intervention. We tried several other specifications, most of them leading to similar results in terms of signs and magnitude of the coefficients. The different specification included variations in the polynomials in the Sisben Score and its piecewise components. We also obtained estimations with the outcomes in differences (follow up minus baseline), and in levels controlling for all the baseline outcomes. None of those led us to obtain reasonable magnitudes in all household income and earnings

coefficients.[49] In addition, we used another definition of formality according to which individuals were classified as formal if they were employees at the baseline. We only found discontinuity for the population of informal females, with no effect in any outcome but household income and earnings, which were again too large.

[49] We also obtained estimates for the sample of household heads (not included here) are similar to the ones included in Table 14.

Table 14. Wald and Regression RDD estimates of the Unemployment Subsidy

			Females								Males							
			Formal				Informal				Formal				Informal			
			(i) EPS		(ii) Caja		(iii) EPS		(iv) Caja		(v) EPS		(vi) Caja		(vii) EPS		(viii) Caja	
			Coeff.	*z*	*Coeff.*	*z*	*Coeff.*	*z*	*Coeff.*	*z*	*Coeff.*	*z*	*Coeff.*	*z*	*Coeff.*	*z*	*Coeff.*	*z*
	Bandwidth	***Variable***	**Labor Force Participation**															
Wald Estimates	2	*numer*	0.051	1.61	0.045 *	1.66	-0.011	-1.61	-0.012 *	-1.84	-0.010	-0.36	-0.010	-0.38	-0.007	-1.09	-0.007	-1.00
		denom	0.038 **	2.34	0.056 **	3.04	0.007 **	3.98	0.002	1.37	0.042	1.64	0.069 **	2.35	0.001	0.54	0.003 **	2.24
		lwald	1.346	0.11	0.817	1.12	-1.502	-1.17	-5.024	-0.41	-0.225	-0.04	-0.141	-0.12	-10.488	-0.02	-2.526	-0.31
	4	*numer*	0.046	1.25	0.039	1.13	-0.010	-1.12	-0.010	-1.06	0.012	0.34	0.010	0.32	-0.011	-1.32	-0.012	-1.33
		denom	0.028 *	1.70	0.022	1.02	0.006 **	2.21	0.001	0.47	0.031	1.05	0.056	1.62	0.000	0.24	0.004 **	2.53
		lwald	1.649	0.62	1.764	0.14	-1.686	-0.21	-8.756	-0.25	0.382	0.12	0.174	0.07	-33.094	-0.53	-3.019	-0.57
	8	*numer*	0.026	1.18	0.019	0.86	-0.004	-0.58	-0.004	-0.57	0.001	0.04	0.004	0.20	-0.006	-1.18	-0.006	-0.99
		denom	0.031 **	2.72	0.046 **	3.63	0.004 **	2.71	0.001	0.97	0.045 **	2.24	0.070 **	3.84	0.000	0.38	0.003 **	3.35
		lwald	0.846	0.99	0.419	0.74	-0.869	-0.30	-2.427	-0.09	0.021	0.01	0.058	0.17	-15.011	-0.07	-1.912	-0.77
Regression Estimates	20	A			0.571	1.03	-0.309	-0.24	-0.332	-0.19	0.106	0.44	0.047	0.25				
	20	B					-0.482	-0.38	-0.471	-0.28	0.258	0.95	0.126	0.58				
	30	C	-0.283	-0.96	-0.180	-1.33	-0.016	-0.01	0.183	0.11	-0.041	-0.28	-0.036	-0.31				
			Unemployment															
Wald Estimates	2	*numer*	0.002	0.20	-0.003	-0.39	0.000	-0.14	0.000	0.00	0.010	0.44	0.018	1.03	-0.008 *	-1.65	-0.008	-1.27
		denom	0.038 **	2.67	0.056 **	3.60	0.007 **	3.89	0.002	1.27	0.042	1.61	0.069 **	3.16	0.001	0.60	0.003 **	2.21
		lwald	0.044	0.17	-0.058	-0.31	-0.060	-0.11	-0.001	0.00	0.242	0.20	0.267	0.86	-13.049	-0.17	-2.932	-0.73
	4	*numer*	-0.001	-0.15	-0.006	-0.59	0.001	0.14	0.001	0.29	0.005	0.19	0.010	0.51	-0.002	-0.26	-0.002	-0.25
		denom	0.028 *	1.67	0.022	1.24	0.006 **	2.59	0.001	0.54	0.031	1.00	0.056 **	1.97	0.000	0.23	0.004 **	2.39
		lwald	-0.053	-0.03	-0.292	-0.05	0.113	0.06	0.963	0.01	0.169	0.02	0.182	0.29	-5.145	-0.21	-0.528	-0.04
	8	*numer*	0.007	1.07	0.003	0.41	0.001	0.48	0.002	0.65	0.001	0.07	0.008	0.51	0.000	-0.11	0.000	-0.05
		denom	0.031 **	2.79	0.046 **	3.63	0.004 **	2.48	0.001	1.07	0.045 **	2.30	0.070 **	4.23	0.000	0.45	0.003 **	2.57
		lwald	0.214	0.88	0.056	0.35	0.328	0.33	1.226	0.03	0.031	0.02	0.107	0.47	-1.209	-0.04	-0.091	-0.04
Regression Estimates	20	A			-0.031	-0.19	0.177	0.34	0.196	0.27	-0.088	-0.49	-0.058	-0.40				
	20	B					0.003	0.01	-0.019	-0.03	-0.169	-0.78	-0.142	-0.79				
	30	C	-0.087	-0.91	-0.023	-0.50	0.594	1.23	0.872	1.19	-0.014	-0.12	0.017	0.18				
			School Attendance															
Wald Estimates	2	*numer*	-0.005	-0.59	-0.007	-0.88	-0.002	-0.61	-0.002	-0.89	0.006	0.68	0.006	0.59	0.002	1.18	0.002	0.93
		denom	0.038 **	2.72	0.056 **	2.85	0.007 **	4.42	0.002	1.54	0.042 *	1.75	0.069 **	3.46	0.001	0.54	0.003 *	1.91
		lwald	-0.124	-0.43	-0.127	-0.27	-0.233	-0.48	-0.770	-0.07	0.131	0.22	0.086	0.53	3.827	0.08	0.820	0.08
	4	*numer*	-0.004	-0.50	-0.005	-0.60	-0.002	-0.69	-0.002	-0.89	0.003	0.29	0.003	0.27	0.001	0.33	0.001	0.27
		denom	0.028 *	1.83	0.022	0.94	0.006 **	2.76	0.001	0.64	0.031	1.09	0.056 **	2.53	0.000	0.22	0.004 **	2.08
		lwald	-0.157	-0.15	-0.247	-0.02	-0.415	-0.15	-2.088	-0.13	0.103	0.09	0.058	0.16	2.979	0.16	0.250	0.00
	8	*numer*	-0.006	-0.86	-0.008	-1.15	-0.004 *	-1.68	-0.004 **	-2.01	0.004	0.48	0.004	0.53	0.002	0.73	0.001	0.66
		denom	0.031 **	2.84	0.046 **	3.25	0.004 **	3.45	0.001	1.09	0.045 **	2.28	0.070 **	4.86	0.000	0.35	0.003 **	2.47
		lwald	-0.190	-0.36	-0.164	-0.86	-0.882	-0.84	-2.609	-0.16	0.079	0.14	0.060	0.54	3.967	0.20	0.507	0.10
Regression Estimates	20	A			0.200	1.15	0.837 **	2.51	1.072 **	2.32	-0.141 *	-1.91	-0.110 *	-2				
	20	B					0.801 **	2.34	1.000 **	2.17	-0.190 **	-2.09	-0.156 **	-2				
	30	C	0.094	0.99	0.088 **	2.01	0.978 **	3.16	1.400 **	2.96	-0.117 **	-2.15	-0.097 **	-2				

Table 14. Wald and Regression RDD estimates of the Unemployment Subsidy (Continuation)

			Females												Males							
			Formal				Informal				Formal				Informal							
			(i) EPS			(ii) Caja			(iii) EPS			(iv) Caja			(v) EPS			(vi) Caja			(vii) EPS			(viii) Caja		
			Coeff.	*p*	*z*	*Coeff.*	*p*	*z*	*Coeff.*	*p*	*z*	*Coeff.*	*p*	*z*	*Coeff.*	*p*	*z*	*Coeff.*	*p*	*z*	*Coeff.*	*p*	*z*	*Coeff.*	*p*	*z*
			Household Income																							
Wald Estimates	2	*numer*	-2884		-0.18	-3322		-0.21	-14047	**	-3.09	-13867	**	-4.04	20836		1.06	16598		1.01	-9612	##	-2.09	-9812	**	-2.13
		denom	0.038	**	2.96	0.056	**	2.74	0.007	**	3.49	0.002	*	1.70	0.042	*	1.90	0.069	**	3.12	0.001	##	0.52	0.003	*	1.92
		lwald	-75454		-0.11	-59822		-0.16	-1940903		-1.12	-5990592		-0.61	490359		0.06	241144		0.78	-15000000	##	-0.01	-3419492		-0.91
	4	*numer*	11634		0.54	16495		0.80	-7670		-1.33	-7736		-1.56	24893		1.06	22946		1.35	2355	##	0.37	1939		0.29
		denom	0.028	**	2.11	0.022		0.99	0.006	**	2.53	0.001		0.68	0.031		0.99	0.056	**	2.22	0.000	##	0.20	0.004	*	1.95
		lwald	418689		0.01	749210		0.04	-1335728		-0.15	-6743727		-0.65	813690		0.10	408495		0.31	7058207	##	0.15	507587		0.07
	8	*numer*	-4315		-0.34	-1841		-0.16	1663		0.41	1571		0.50	-4488		-0.26	-8523		-0.57	6511	##	1.57	6183		1.54
		denom	0.031	**	3.09	0.046	**	2.95	0.004	**	2.70	0.001		1.13	0.045	**	3.19	0.070	**	4.50	0.000	##	0.41	0.003	**	2.30
		lwald	-140608		-0.26	-39979		-0.14	393444		0.21	1062414		0.20	-99451		-0.23	-121043		-0.52	17000000	##	0.29	2104759		0.30
Regression Estimates	20	A				-921667	**	-3.31	-476353		-0.87	-601611		-0.80	-110160		-0.84	-147548		-1						
	20	B							-925932	*	-1.86	-1215877	*	-1.81	-12031		-0.08	-83193		-0.68						
	30	C	-281212	**	-2	-159239	**	-2.53	-621705		-1.39	-893254		-1.31	-83372		-0.98	-100583		-2						
			Earnings																							
Wald Estimates	2	*numer*	8305		1.04	5634		0.84	-4743	*	-1.94	-4880	**	-2.55	-12743		-0.94	-17288		-1.40	-4423	##	-1.59	-4558		-1.25
		denom	0.038	**	2.75	0.056	**	3.25	0.007	**	4.05	0.002	*	1.72	0.042	*	1.76	0.069	**	2.93	0.001	##	0.58	0.003	**	2.08
		lwald	217306		0.10	101453		0.53	-655390		-1.24	-2108061		-0.05	-299884		-0.28	-251167		-0.59	-6908176	##	-0.36	-1588358		-0.30
	4	*numer*	14847	*	1.65	12066		1.40	-2744		-0.83	-2832		-1.08	-3488		-0.20	-6985		-0.44	-3992	##	-1.14	-4103		-0.84
		denom	0.028	*	1.67	0.022		1.07	0.006	**	2.73	0.001		0.74	0.031		1.16	0.056	**	2.00	0.000	##	0.22	0.004	**	2.13
		lwald	534356		0.22	548074		0.17	-477781		-0.35	-2468545		-0.34	-114008		-0.06	-124347		-0.16	-12000000	##	-0.55	-1074003		-0.15
	8	*numer*	-461		-0.08	-2377		-0.39	-2447		-1.16	-2524		-1.63	-11388		-1.02	-14821	*	-1.67	-3166	##	-1.27	-3215		-1.16
		denom	0.031	**	2.54	0.046	**	3.68	0.004	**	3.02	0.001		1.23	0.045	**	2.29	0.070	**	4.14	0.000	##	0.38	0.003	**	2.72
		lwald	-15024		-0.05	-51629		-0.33	-579127		-0.92	-1707110		-0.36	-252335		-0.22	-210481		-0.95	-8256146	##	-0.38	-1094375		-0.64
Regression Estimates	20	A				-125002		-0.71	-697681	**	-2.04	-941398	**	-1.99	-697681	**	-2.04	-941398	**	-2						
	20	B							-686736	**	-2.03	-888819	*	-1.95	-96313		-0.76	-124977		-1						
	30	C	-203125	**	-2	-130312	**	-2.96	-534156	*	-1.75	-772516	*	-1.66	-152577	**	-2.20	-141594	**	-3						
			1st Stage																							
Regression Estimates	A	1[S<â], γ				0.030	**	2.34	0.004	**	3.30	0.003	**	3.78	0.068	**	4.30	0.084	**	5						
	B	1[S<â], γ							0.004	**	3.22	0.003	**	3.77	0.055	**	3.77	0.067	**	4						
	C	1[S<â], γ	0.045	**	5	0.094	**	9.32	0.004	**	3.77	0.003	**	3.99	0.084	**	6.70	0.108	**	8						

A. Only Polynomial term in S, Bandwidth: 20 (38 < S < 57); B. Polynomial term in S and Control Variables, Bandwidth: 20 (38 < S < 57); C. Polynomial term in S and Control Variables, Bandwidth: 30 (30 < S < 60)

The average exchange rate between 2005 and 2006 was $2,340/USD.

3.1.3 Regression RDD Estimates of the Effect of the Subsidized Regime

Regarding the possibility that the impact of the Subsidized Regime would be limiting the possibility for us to obtain unbiased estimates of the effect of the US, we obtained RDD estimates of the impact of the SR on our set of outcomes which are included in appendix A.12. The results in Tables A.12 (a) and (b) are very consistent across samples, bandwidths, the inclusion of control variables, and the definition of formality. Overall, we find a robust discontinuity at the cutoff of the probability of being enrolled in the SR. In addition, the magnitudes of most coefficients seem very reasonable when compared with those found when estimating the effect of the US.

Very importantly, there is no positive effect of the SR on household income or earnings, which means that the negative effects of the US on those variables would not be explained by the effect of the SR, because the effect of the SR if something, would overestimate our estimate of the impact of the US. This implies that there would be other reasons different to the concurrence of another intervention at the cutoff that should be explaining the huge negative US effects we estimated. Lack of consistency across specifications and in some cases, across definitions of formality, plus the limitations posed by the discontinuities of the lagged outcomes we found, might be among the causes of that result.

3.1.4 RDD: Synthesis

We obtained both Wald and Regression RDD estimates of the impact of the US on a battery of outcomes. We also present evidence that supports the identifying assumptions underlying RDD. According to our Wald estimates, the US program had no significant effect on any of the outcomes considered, while our regression RDD estimates do point to a negative effect on earnings and household income, and a positive effect on school attendance of females. There were nonetheless some facts that prevent us from considering the regression RDD estimates robust enough.

We now proceed to complement these results with matching estimates, which as we will see, will lead us to estimates of much more reasonable magnitudes, in particular, on variables like household income and earnings.

3.2 Matching Estimators

Since the RDD strategy only allows us to identify program impacts near the cutoff $\overline{S}$, it can be useful to complement those estimates with additional ones that could give us mean impacts for a broader population of US participants. We now obtain them by using the matching method.

This method assumes that selection into the program is based on the observed variables in the data set. The crux of this approach is that treatments and controls with the same observed characteristics are assumed to be allocated randomly between program and non-program status. Even though the sample of beneficiaries seems very different to that of non-beneficiaries, that should not pose significant limits to applying the matching estimators, since there is a large set of people in the comparison group from which to get

the matches for each beneficiary in the treatment group. Appendix 11 includes descriptive statistics of the variables on which the matching was performed between beneficiaries and non- beneficiaries of the US (treated and comparison group). Prior to matching, the mean differences between these groupsô characteristics are in most cases statistically significant. After matching, these mean differences are negligible and never statistically significant. Similar results follow for the other subsamples.

We obtain matching differences-in-differences and cross-section estimates for all outcomes of interest, except for those outcomes for which we only have information at the follow-up survey such as unemployment duration, or ñenrolled in an EPS.ò For these variables we can only obtain cross section estimates.

3.2.1 From RDD to Matching Estimates

In this section we depart from the fact that if both the RDD and Matching estimates were able to identify the impact of the US, then they should be similar around the cutoff. So we could get the Matching estimates using data just on the left of the cutoff within a narrow bandwidth, and if we got similar results to those found using the Wald RDD, then we could argue that Matching estimates are being able to correctly identify the impact of the US. If that was the case, and being aware that the impact of the US does not have to be homogeneous in a range beyond the cutoff, we could obtain Matching estimates of the impact of the US over the whole sample. We compare these Matching estimates with the RDD Wald estimates, since as we argued previously; with the exception of the estimates of the US effect on earnings, the regression RDD estimates were not as robust.

Table 15 shows the Differences-in-Differences Matching estimates using data on the left of the cutoff within a 5 Sisben score points bandwidth. Most estimates are not statistically significant at the 5 percent level. Furthermore, with the exception of the estimate of the effect on the unemployment rate of informal females, the only significant coefficients are those of earnings and earnings of households, the ones that were most robustly significant among the regression RDD estimates.

Overall, we take the results in Table 15 as a proof of the consistency among our RDD and Matching estimates, and proceed in the next subsection to get Matching estimates for the whole sample.

Table 15 Matching Estimates on the left of cutoff between Sisben levels 2 and 3[*]

Outcome		Females				Males			
		Number of observations		Diff-in-Diff		Number of observations		Diff-in-Diff	
		Total	Treated	ATT	z	Total	Treated	ATT	z
Formal (EPS=1)	LMP	2,346	293	0.083	1.81	1,461	245	-0.005	-0.14
	Unemployment	2,346	293	0.015	0.65	1,461	245	0.054	1.41
	School Attendance	2,346	293	0.020	1.55	1,461	245	0.006	1.26
	Earnings of Household	2,346	293	-3,785	-0.26	1,461	245	**-67,963**	-3.24
	Earnings	2,346	293	17,158	1.49	1,461	245	**-56,367**	-3.61
Informal (EPS=0)	LMP	49,857	904	0.020	0.95	39,646	188	-0.066	-1.72
	Unemployment	49,857	904	**0.024**	2.55	39,646	188	0.002	0.06
	School Attendance	49,857	904	0.009	1.51	39,646	188	0.008	1.15
	Earnings of Household	49,857	904	**-34,798**	-5.33	39,646	188	**-78,166**	-5.12
	Earnings	49,857	904	**-11,341**	-2.46	39,646	188	**-60,271**	-5.05

* Includes observations in the bandwidth from Sisben score 42 to 47 (on the left of the cutt-off).

3.2.2 Matching Estimates

We present our results by gender, formality status, and for the periods between 2002 and 2005, and between 2005 and 2009. For all of these cases we estimate the effect on the seven outcomes mentioned above. Remember, we could not present RDD estimates for the 2005 to 2009 period, because during this period we could not find sufficient evidence of a discontinuity in enrollment.

Labor Market Outcomes

Table 16 presents all our matching estimators. The columns are divided by gender, and within each gender we include columns for formal and informal workers between 21 and 54 years of age, in each case, according to enrollment in an EPS or a *Caja*. In the rows we include the estimated coefficients per each of the period of time considered, namely 2002 to 2005, and 2005 to 2009. Within each period of time we include results for all workers, and also for workers who were household heads at the baseline. Within each population we include both cross section and differences-in-differences estimates of the impact of the US on labor market participation, unemployment, enrollment in an EPS, earnings of the household, and earnings of the worker.[50] The table also presents estimates obtained when we split the beneficiaries according to the type of training courses they took when in the US program, namely those related to industrial affairs, management and services, technology and software, and other courses.

Here we describe our DID estimates obtained for the period between 2005 and 2009, which is the period during which our sample included the largest number of US beneficiaries. For women in the formal sector, US participation lead to a slight fall in labor force participation when formality is measured by previous enrollment in a *Caja* (0.0 to 3.7 percent), and a larger one when measured by previous enrollment in a EPS (7.8 to 8.5 percent); had no effect on unemployment; and caused a fall in both individual and household earnings.

[50] See Appendix 6 to 9 for summary of the variables employed in the estimations presented in this section, for the period between 2002 and 2005, and between 2005 and 2009.

Our DID-matching estimates are much smaller and more reasonable than those found with RDD. Among women from the formal sector, the estimated impacts of the US are very large relative to the treatment group baseline earnings reported in appendices 7 and 9, accounting for a reduction of earnings between 18 and 49 percent ($21,993 and $82,387, respectively). The large magnitude of the impact is partly explained by the low levels of the beneficiariesô earnings at the baseline. In fact, the magnitude of these impacts is small when compared with the monthly minimum wage, with respect to which the effect is between 4 and 17 percent.

Among women in the informal sector, US participation had no effect on labor force participation and led to a slight increase in unemployment. In addition, both individual and household earnings fell, but this earnings decline was smaller than the earnings declines experienced by women from the formal sector. In this case, the fall in earning is between 22 and 27 percent of baseline earnings ($26,687 and $25,583), but only 5 percent of the monthly minimum wage.

Our DID estimates indicate that the impact of the US on labor market outcomes also varied by participation in training. For women in the formal sector, labor force participation fell regardless of the type of training they took while they were US beneficiaries. This estimated decline was larger than the estimated declines we found when females were not split by the type of training courses they took.

Unemployment fell for formal females who took courses in industrial affairs and other topics, while it remained unchanged for those who took courses in management and services, and in technology and software. Both individual and household earnings fell, although by a smaller magnitude for formal females who took courses in technology and software.

Among informal females, the US program seemed not to affect labor participation, and there was a slight increase in unemployment (1.2 percent). Although unemployment durations increased and individual earnings always fell for these women, for females who took courses in technology and software topics household earnings fell, whereas they remained unchanged for informal women who took courses on management and services.

Among males from the formal sector, we found that US participation caused labor force participation to fall by more than it fell for informal females (between 5.7 and 9.9 percent). Unemployment increases for the subsample of household head males (4.3 to 5.5 percent), while it remains unchanged for the whole sample of males. Both individual and household earnings fall. Informal males also reduce their labor force participation, but by half as much as do formal males. Their unemployment rates are unaffected, although their unemployment duration increases. That is, the unemployed become fewer but for longer spells. Finally, and as was the case for the females, the decrease in earnings is larger among formal compared to informal males. This decline in the earnings of formal males is between 34 and 61 percent of their baseline earnings ($74,985 and $138,733), and between 15 and 28 percent of the monthly minimum wage. Among informal males the fall is between 18 and 22 percent of their baseline earnings ($40,287 and $46,030), and between 8 and 9 percent of the minimum wage.

The labor force participation of formal males falls regardless of the type of training courses they took, although the reduction is smaller for those who took courses in technology and software. Unemployment of formal males is not affected by the type of training courses they took. Individual earnings fall regardless of the type of training they took, but household earnings fall for all types of training, except for management and services courses. The results included in the table for informal males are not robust due to the small size of the sample of beneficiaries.

Human Capital and Nutrition Outcomes

An important function of unemployment insurance is that it helps individuals and their families to ñsmoothò their consumption, and in particular, their human capital investment, when they are unemployed and their earnings are low. Our data contains no information on household consumption, but it does include several other variables that are known to be related. These variables are the participantsô school attendance, the school attendance rate of all household members 6 to 18 years old - defined as the ratio of those members attending school to the total number of household members in that age range -, and we also have the weight, height, Body Mass Index (BMI), and the Apgar score of the beneficiary femalesô children at birth.[51] These variables are all imperfect indicators of smoothing consumption since one the one hand, the US might allow individuals to prevent their household members from dropping out of school, and on the other hand, it might help pregnant women to maintain minimum nutrition standards. BMI for example, may take many months to influence. But we believe looking at these variables collectively may provide indirect evidence on whether the US program is achieving its key objective: To smooth human capital investment and nutrition of Colombiaôs poorest (urban) families when they become unemployed.

Formal and informal femalesô school attendance increases with the US for the whole sample of females, although on a small scale (1.0 percent). The school index is not affected by the US but only for informal females, where it is negative.

The US has no effect on these outcomes for females who are household heads. Although the results based on the 2005 to 2009 data imply negative effects of the US on weight and height at birth, once we control for the education of the mother and the father (only available for the years 2006 and 2007), we find no effect of the US on weight, height or BMI of beneficiariesô children at birth.

[51] The BMI is the ratio of the childrenôs weight to the square of their height, and it is expressed in kilograms per square meter. The Apgar score is only available in the 2006 and 2007 surveys, and it is determined by evaluating the newborn on five simple criteria on a scale from zero to two, then summing up the five values obtained. The resulting Apgar ranges from zero to 10. The five criteria are Appearance, Pulse, Grimace, Activity and Respiration. Apgar1 and Apgar5 refer to the same concept assessed after 1 and 5 minutes the child was born. We defined each Apgar as 1 if the score was 7 or more, and zero otherwise. See descriptive statistics in Appendix 10, where beneficiariesô socioeconomic variables suggest they are worse off than non beneficiaries.

The US has a positive effect on school attendance of the formal and informal males of around 2.0 percent, while for those who are household heads there is no effect. On the contrary, it has a negative effect on the school index, especially on informal workers.

These results on school attendance suggest that individuals who were not household heads at the baseline in 2005, who are presumably relatively younger and with smaller family sizes than those who were already household heads by then, face liquidity constraints that prevent them from attending school. In addition, the fact that such a small economic benefit received by them makes a difference that allows them to attend school, signals their precarious economic conditions. The result is consistent with the one found for Indonesia by Chetty and Looney (2006), and with evidence showing the effects of US benefits on smoothing consumption by Gruber (1997, 1998), Browning and Crossley (2001), and Bloemen and Stancanelli (2005).

Matching DiD Estimators using only the unemployed at Baseline

Although we consider our matching estimates are reasonable on the whole, we must bear in mind that it is not easy for the method to correctly resemble all the characteristics and facts of the treated population with the universe of individuals in the comparison group. In particular, notice that at the baseline we know that in theory, all treated individuals are supposed to suffer a shock that would lead us to apply for the US, while we do not know which among the comparison group will suffer a comparable shock, and information used to match people from both groups seems limited to predict the likelihood of such events.

In an attempt to assess if not having information for the comparison group on whether people in that group suffered a shock in the analyzed period, we obtain matching differences-in-differences estimates for the sample of all individuals who were unemployed at the baseline, and also for the sample of all individuals in the treatment group (regardless of their employment status at the baseline) and the subset of individuals in the comparison group who were unemployed at the baseline. By including individuals in the comparison group who were unemployed at the baseline, we make sure that at least at that moment they suffered an employment shock, and thus we would expect those estimates would not underestimate the impacts, but rather, they might actually provide an upper bound of them.

When we compare the estimates obtained for females in Table 16 with those in Table A.13 of the Appendix, we find that the effect on household income becomes non significant for the sample of individuals who were unemployed at the baseline. A similar result is found for formal females, although the effect of the US on informal females becomes more negative. In the sample of formal females other estimates become much larger, as it is the case for the impact on labor participation and unemployment, in particular, when we consider the sample of all formal females. Notice that the sample of females who were unemployed household heads at the baseline becomes too small.

When we compare the results obtained for males in Table 16 with those in Table A13, we find that only the effects on earnings become of similar magnitude and significance, while all other estimates in Table A.13 become non significant, presumably because of the fewer number of observations available.

In short, when we use the sample of unemployed individuals at the baseline, the effects of the US on household income and earnings become much less negative, or in the worst case, of similar magnitude to those obtained when we used all individuals available at the baseline. For females, we found larger effects on labor participation and unemployment, while those become negligible for males.

Table 16: Matching Estimators. Whole Sample and Household Heads.

				Females								Males							
				Formal				Informal				Formal				Informal			
				(i) EPS				(ii) Caja				(iii) EPS				(iv) Caja			
	Training	Sample	Method	*Coeff.*		*z*		*Coeff.*		*z*		*Coeff.*		*z*		*Coeff.*		*z*	
Labor Force Participation																			
2002/2005		All	CS	-0.063	**	-3.08		-0.074	**	-4.91		0.014		1.16		0.022	**	2.28	
			DiD	0.004		0.21		0.001		0.07		0.007		0.66		-0.009		-0.65	
		HH	DiD	-0.018		-0.72		-0.044	**	-2.21		-0.011		-0.88		-0.017		-1.07	
2005/2009		All	CS	-0.005		-0.46		-0.016	*	-1.88		0.029	**	3.43					
			DiD	-0.085	**	-6.56		-0.003		-0.34		-0.010		-1.02					
		HH	DiD	-0.018		-0.72		-0.044	**	-2.21		-0.011		-0.88					
	Not Defined	All	CS	-0.128	**	-6.98		-0.049	**	-3.67		-0.043	**	-2.50					
	Industrial	All	CS	-0.125	**	-5.90		-0.023		-1.54		0.001		0.07					
	Manag & Serv	All	CS	-0.152	**	-6.13		-0.089	**	-4.50		-0.055	*	-1.72					
	Tech & Soft	All	CS	-0.116	**	-3.67		-0.038		-1.45		-0.031		-0.76					
Unemployment																			
2002/2005		All	CS	0.030	**	3.35		0.023	**	3.89		0.011	**	2.12		0.002		0.57	
			DiD	-0.037	**	-2.8		0.001		0.07		0.002		0.32		0.011		1.35	
		HH	DiD	-0.032	*	-1.76		-0.036	**	-2.75		0.001		0.14		0.002		0.22	
2005/2009		All	CS	0.049	**	9.95		0.035	**	10.65		0.006	**	1.99					
			DiD	-0.009		-1.44		-0.013	**	-2.97		0.012	**	2.48					
		HH	DiD	-0.032	*	-1.76		-0.036	**	-2.75		0.001		0.14					
	Not Defined	All	CS	-0.023	**	-2.52		-0.021	**	-3.29		0.003		0.29					
	Industrial	All	CS	-0.047	**	-4.53		-0.021	**	-3.13		0.025	**	2.85					
	Manag & Serv	All	CS	-0.009		-0.72		-0.017	*	-1.68		-0.011		-0.62					
	Tech & Soft	All	CS	-0.022		-1.32		-0.008		-0.59		0.000		0.00					
School Attendance																			
2002/2005		All	CS	-0.001		-0.14		-0.004		-0.98		0.0004		0.14		-0.006		-1.59	
			DiD	0.017	*	1.8		0.010	*	1.66		0.004		1.24		-0.001		-0.29	
		HH	DiD	0.010		0.88		0.002		0.23		0.000		-0.05		-0.013	**	-2.21	
2005/2009		All	CS	0.006		1.53		0.007	**	2.8		0.009	**	3.9					
			DiD	0.011	**	2.11		0.009	**	2.54		0.016	**	4.92					
		HH	DiD	0.010		0.88		0.002		0.23		0.000		-0.05					
	Not Defined	All	CS	-0.030	**	-2.05		-0.003		-0.33		0.008		1.58					
	Industrial	All	CS	-0.004		-0.32		0.004		0.49		0.006		1.02					
	Manag & Serv	All	CS	-0.060	**	-2.83		-0.033	**	-2.36		-0.003		-0.28					
	Tech & Soft	All	CS	0.034	**	2.44		0.016		1.27		0.008		0.41					
School Index																			
2002/2005		All	DiD	-0.023		-1.18		0.040	**	2.092		-0.009		-0.7		-0.018	*	-1.69	
2005/2009			DiD	-0.002		-0.16		-0.004		-0.47		-0.032	**	-3.81					

				Males							
				Formal				Informal			
				(v) EPS				(vi) Caja			
	Training	Sample	Method	*Coeff.*		*z*		*Coeff.*		*z*	
Labor Force Participation											
2002/2005		All	CS	-0.096	**	-5.73		-0.094	**	-6.25	
			DiD	-0.080	**	-4.53		-0.081	**	-5.16	
		HH	DiD	-0.087	**	-4.70		-0.086	**	-5.07	
2005/2009		All	CS	-0.102	**	-9.87		-0.086	**	-9.43	
			DiD	-0.078	**	-6.83		-0.057	**	-5.77	
		HH	DiD	-0.017		-1.07		-0.087	**	-4.7	
	Not Defined	All	CS	-0.127	**	-6.12		-0.115	**	-6.12	
	Industrial	All	CS	-0.110	**	-4.37		-0.095	**	-4.28	
	Manag & Serv	All	CS	-0.109	**	-3.29		-0.101	**	-3.21	
	Tech & Soft	All	CS	-0.053		-1.55		-0.067	**	-2.15	
Unemployment											
2002/2005		All	CS	0.102	**	6.62		0.109	**	7.94	
			DiD	-0.009		-0.42		0.007		0.36	
		HH	DiD	0.040	*	1.90		0.050	**	2.57	
2005/2009		All	CS	0.088	**	10.1		0.080	**	10.6	
			DiD	0.008		0.76		0.001		0.14	
		HH	DiD	0.002		0.22		0.040	*	1.9	
	Not Defined	All	CS	0.045	**	2.50		0.024		1.48	
	Industrial	All	CS	0.001		0.02		-0.013		-0.61	
	Manag & Serv	All	CS	0.008		0.24		0.013		0.43	
	Tech & Soft	All	CS	0.034		1.31		0.000		0.01	
School Attendance											
2002/2005		All	CS	0.002		0.41		0.004		0.89	
			DiD	0.009		1.22		0.008		1.24	
		HH	DiD	-0.002		-0.32		0.000		-0.07	
2005/2009		All	CS	0.007	*	1.83		0.008	**	2.23	
			DiD	0.019	**	3.25		0.018	**	3.51	
		HH	DiD	-0.013	**	-2.21		-0.002		-0.32	
	Not Defined	All	CS	-0.028	**	-2.53		-0.023	**	-2.29	
	Industrial	All	CS	-0.014		-1.23		-0.011		-1.03	
	Manag & Serv	All	CS	-0.039	*	-1.73		-0.036	*	-1.73	
	Tech & Soft	All	CS	0.006		0.35		0.000		-0.02	
School Index											
2002/2005		All	DiD	-0.022		-1.07		-0.010		-0.53	
2005/2009			DiD	-0.024		-1.41		-0.036	**	-2.48	

				Males							
				Informal							
				(vii) EPS				(viii) Caja			
	Training	Sample	Method	*Coeff.*		*z*		*Coeff.*		*z*	
Labor Force Participation											
2002/2005		All	CS	-0.070	**	-4.09		-0.067	**	-3.25	
			DiD	-0.064	**	-3.46		-0.056	**	-2.48	
		HH	DiD	-0.085	**	-4.72		-0.076	**	-3.51	
2005/2009		All	CS	-0.011		-0.72					
			DiD	-0.045	**	-2.55					
		HH	DiD	-0.086	**	-5.07					
	Not Defined	All	CS	-0.116	**	-2.71					
	Industrial	All	CS	-0.092	*	-1.89					
	Manag & Serv	All	CS	-0.247	**	-2.53					
	Tech & Soft	All	CS	-0.269	**	-3.42					
Unemployment											
2002/2005		All	CS	0.052	**	3.44		0.034	*	1.92	
			DiD	0.040	**	1.99		0.022		0.95	
		HH	DiD	0.024		1.19		0.007		0.28	
2005/2009		All	CS	-0.003		-0.21					
			DiD	0.024		1.344					
		HH	DiD	0.050	**	2.57					
	Not Defined	All	CS	-0.038		-0.97					
	Industrial	All	CS	-0.047		-0.92					
	Manag & Serv	All	CS	0.024		0.26					
	Tech & Soft	All	CS	-0.063		-0.83					
School Attendance											
2002/2005		All	CS	-0.001		-0.29		0.002		0.27	
			DiD	0.001		0.19		0.002		0.26	
		HH	DiD	-0.007		-1.34		-0.005		-0.94	
2005/2009		All	CS	0.010	*	1.85					
			DiD	0.022	**	2.84					
		HH	DiD	0.000		-0.07					
	Not Defined	All	CS	-0.006		-0.38					
	Industrial	All	CS	-0.013		-0.70					
	Manag & Serv	All	CS	0.002		0.22					
	Tech & Soft	All	CS	-0.019		-0.78					
School Index											
2002/2005		All	DiD	0.057	**	2.879		0.054	**	2.342	
2005/2009			DiD	-0.081	**	-4.41					

Table 16: Matching Estimators. Whole Sample and Household Heads (Continuation).

				Females								Males							
				Formal				Informal				Formal				Informal			
				(i) EPS		(ii) Caja		(iii) EPS		(iv) Caja		(v) EPS		(vi) Caja		(vii) EPS		(viii) Caja	
	Training	Sample	Method	*Coeff.*	*z*	*Coeff.*	*z*	*Coeff.*	*z*	*Coeff.*	*z*	*Coeff.*	*z*	*Coeff.*	*z*	*Coeff.*	*z*	*Coeff.*	*z*
Earnings of Household																			
2002/2005		All	CS	-208,205	** -15.5	-200,129	** -21.58	-52,723	** -11	-56,871	** -8.6	-196,595	** -15.7	-207,495	** -18.5	-99,195	** -8.84	-92,555	** -6.97
			DiD	-105,400	** -7.71	-85,675	** -9.27	-51,178	** -10	-61,295	** -9.26	-61,949	** -4.65	-80,661	** -6.77	-96,742	** -8.4	-82,920	** -6.11
		HH	DiD	-117,407	** -6.99	-102,923	** -8.14	-52,829	** -7.79	-51,085	** -5.82	-84,388	** -6.07	-97,245	** -7.57	-88,627	** -7.13	-81,873	** -5.56
2005/2009		All	CS	-209,186	** -16.6	-227,448	** -25.17	-72,429	** -9.2			-218,600	** -14.8	-213,199	** -16.4	-32,922	* -1.69		
			DiD	-84,500	** -6.3	-39,097	** -4.06	-52,857	** -6.51			-77,508	** -4.98	-63,511	** -4.66	-32,248	-1.62		
		HH	DiD	-117,407	** -6.99	-102,923	** -8.14	-52,829	** -7.79			-51,085	** -5.82	-84,388	** -6.07	-97,245	** -7.57		
	Not Defined	All	CS	-154,316	** -4.97	-92,293	** -4.20	-52,975	** -2.57			-122,242	** -4.43	-124,135	** -4.91	-72,673	-1.52		
	Industrial	All	CS	-158,236	** -4.87	-104,826	** -4.61	-65,700	** -3.15			-154,725	** -4.15	-145,452	** -4.38	-106,318	-1.53		
	Manag & Serv	All	CS	-123,890	** -2.90	-95,141	** -2.92	-58,040	-1.52			-36,927	-0.76	-54,428	-1.21	-96,433	-1.16		
	Tech & Soft	All	CS	-51,636	-0.89	-63,950	-1.47	-90,205	* -1.90			-91,498	* -1.80	-101,060	** -2.16	-214,227	** -2.17		
Earnings																			
2002/2005		All	CS	-79,716	** -11.6	-77,521	** -16.91	-9,526	** -4.1	-12,448	** -4	-127,043	** -17.0	-133,058	** -19.5	-64,550	** -10.1	-59,403	** -7.88
			DiD	-27,941	** -4.05	-21,935	** -4.99	-13,180	** -5.5	-22,535	** -6.98	-66,733	** -8.37	-74,157	** -10.2	-67,835	** -10.3	-56,364	** -7.48
		HH	DiD	-46,046	** -5.39	-37,346	** -6.43	-17,942	** -5.57	-23,203	** -5.56	-93,222	** -10.9	-96,239	** -12.1	-65,073	** -9.06	-59,822	** -7.23
2005/2009		All	CS	-75,714	** -12.4	-76,941	** -18.93	-11,796	** -3.6			-140,100	** -16.1	-135,904	** -18.2	-16,009	-1.55		
			DiD	-57,966	** -8.92	-21,993	** -5.16	-26,687	** -7.64			-84,487	** -9.08	-74,985	** -9.44	-46,030	** -4.32		
		HH	DiD	-46,046	** -5.39	-37,346	** -6.43	-17,942	** -5.57			-23,203	** -5.56	-93,222	** -10.9	-96,239	** -12.1		
	Not Defined	All	CS	-103,696	** -6.74	-64,651	** -6.56	-29,596	** -3.25			-171,521	** -9.55	-166,012	** -10.2	-53,748	** -1.98		
	Industrial	All	CS	-116,665	** -6.81	-68,265	** -6.47	-22,812	** -2.45			-186,888	** -7.48	-169,984	** -7.76	-35,976	-1.04		
	Manag & Serv	All	CS	-123,082	** -5.98	-93,439	** -6.63	-45,528	** -2.90			-135,328	** -4.56	-135,509	** -4.92	-119,806	** -2.37		
	Tech & Soft	All	CS	-94,714	** -3.65	-68,283	** -3.69	-17,861	-0.76			-139,878	** -4.13	-129,367	** -4.24	-69,173	-1.17		
EPS (Health Insurance)																			
2002/2005		All	CS	-0.532	** -31.3	-0.537	** -44.83	-0.090	** -12.0	-0.096	** -19	-0.506	** -27.2	-0.521	** -31	-0.097	** -7.54	-0.101	** -7.24
			DiD	0.000	0	0.000	0	0.000	0	0.000	0.00	0.000	0.0	0.000	0	0.000	0	0.000	0
		HH	DiD	0.000	0	0.000	0	0.000	0.00	0.000	0.00	0.000	0.0	0.000	0	0.000	0	0.000	0
2005/2009		All	CS	-0.473	** -45.7	-0.507	** -70.0	-0.124	** -19.1			-0.414	** -31.9	-0.432	** -38.1	-0.072	** -4.25		
			DiD	0.000	0	0.000	0.00	0.000	0.00			0.000	0.0	0.000	0	0.000	0		
		HH	DiD	0.000	0	0.000	0.00	0.000	0.00			0.000	0.0	0.000	0	0.000	0		
				Weight at Birth		**Height at Birth**		**BMI at Birth (Kg/m^2)**											
2006-2009***		All	CS	-92.4	** -2.24	0.029	0.08	-0.365	** -2.6										
2006-2007***		HH	DiD	-13.8	-0.32	-0.058	-0.22	-0.042	-0.32										

Source: Authors calculations using 2002, 2005 and 2009 Sisben Surveys, and Comfama and Comfenalco information for beneficiaries. Variables in the propensity score include information at the baseline of whether the individual attended school, his education, the gender of HH head, his main economic activity, his earnings, number of children under 6, number of children under 18, HH size, house ownership, socioeconomic stratum, length of pregnancy, type of birth, age of mother, number of children born alive, number of pregnancies, age of father. ** In addition to the previous variables, it includes the education of the mother and that of the father. *** In the period 2006-2007 we control for the education of the mother and the father, which were only available for these years. All: Everyone at the baseline; HH: Only people who were household heads at the baseline. Average exchange rate between 2005 and 2006 was $2,340/USD.

V. Discussion and Conclusions

The establishment and implementation of protection programs for the unemployed such as the Colombian Unemployment Subsidy program and job training is a huge advance to serve Colombiaôs vulnerable population. In this paper, we assess the effects of this program on labor market outcomes and outcomes related to householdsô consumption.

We obtain both regression discontinuity and matching differences-in-differences (DID) estimates, and find that overall, according to both estimates, formal and informal beneficiaries of both genders experience a reduction in their future individual earnings as well as their future household earnings. Based on our matched estimates for the whole sample, we find that individual earnings of formal females fall between 4 and 17 percent of the minimum wage, and for the informal only 5 percent of the minimum wage. Individual earnings of formal males fall between 18 and 22 percent of the minimum wage, and for informal males they fall between 8 and 9 percent of the minimum wage. The reduction in the individual earnings of US beneficiaries is a much larger share of their baseline earnings, since those are less than half the minimum wage. Those effects on earnings nonetheless, are likely to be a lower bound for formal females, for whom we found no effect of the US once we consider only the sample of unemployed formal females at the baseline. In no case we found a positive effect of the US on household income or earnings.

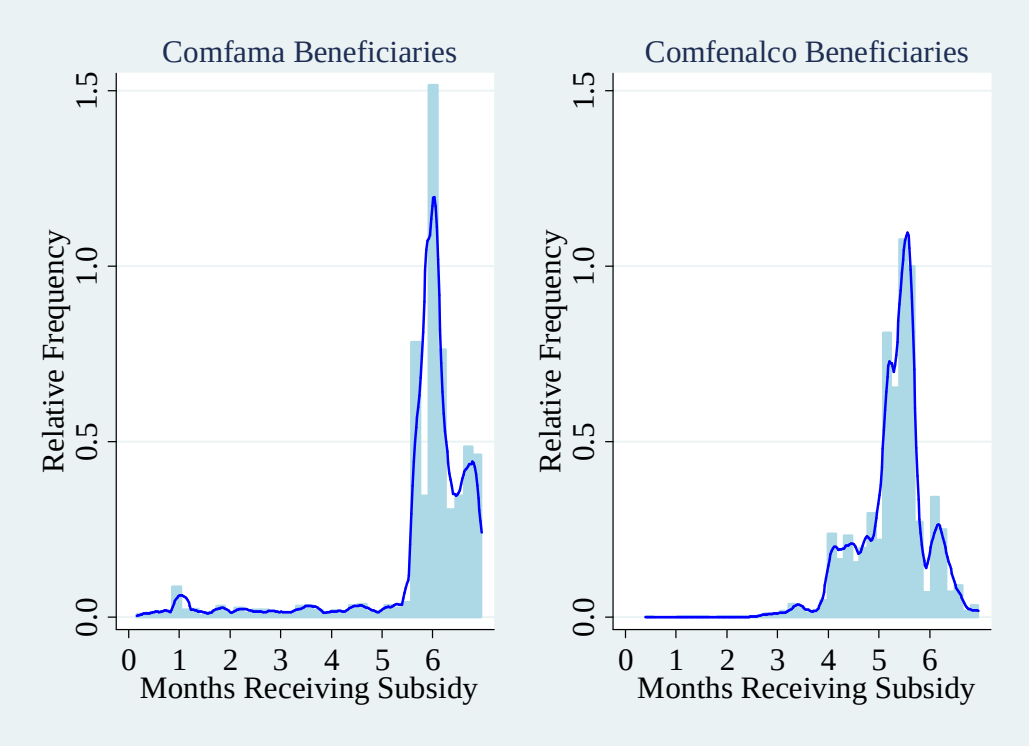
We consider the nil to slightly negative effects of the US on earnings consistent with a model like the one presented by Akin and Platt (2011). In their model, the benefits of unemployment insurance increase the workerôs reservation wage, nonetheless, as the subsidy received draws closer to its expiration date, the workerôs reservation wage falls, making him more desperate for a job. In their model, an increase in the benefit decreases wages since that encourages workers to delay their acceptance of jobs, moving them closer to the expiration date, and allowing firms to offer much lower wages. This result is also consistent with Hansen and Imrohoroglu (1992), who emphasize the moral hazard effect the subsidy has on the unemployed whose job search is not required or monitored, encouraging them to remain unemployed longer.[52] In our case, think of a US beneficiary living out of the benefit and working less (lowering his earnings) once he becomes beneficiary; some not even searching for a job since it is not always enforced; some searching, but rejecting offers because of both having a higher reservation wage due to the benefit, and not having to accept any specific offer because they have guaranteed the reception of the benefit; and finally, as the expiration date of the benefit approaches, the beneficiary would rush to get a job, but because of the much lower reservation wage, he would be willing to accept one with a lower wage than the one before becoming a US beneficiary.

[52] See also Fishe (1982), Feldstein and Poterba (1984), and Shimer and Werning (2006), among others. Search is particularly discouraged in Colombianô US program, since payments are constant rather than decreasing in time, making inefficient the individualô job choices (Baily, 1978, Fleming, 1987, Shavell and Weiss, 1979, Fredriksson and Holmlund, 2001).

In addition, although in Akin and Platt (2011) increases in the length of the benefit increase wages, that effect is much less than the one caused by increases in the amount of the subsidy, leading in the net to a reduction of earnings.

Their model is particularly applicable to a setup like the Colombian one, since the US program is well-targeted on the poor low-skilled individuals, who are more likely to rely on unemployment benefits than on the low levels of accumulated assets of their own (if any), and who are more likely to experience unemployment spells between jobs. As it is shown in Figure 19, most beneficiaries in both *Cajas* keep their subsidies until very close to their expiration dates, that is, they receive the benefits for nearly 6 months, the maximum length. Remember that in addition to the mentioned effects in the Akin and Platt (2011) unemployment insurance model, in Colombiaôs case, an unemployment subsidy does not require individuals to search or be eligible for jobs. Some individuals, those with the lowest earnings (and thus with a relatively much higher replacement rate), might be willing to quit their jobs in order to benefit from the US, as they do it in a similar setup presented by Hansen and Imrohoroglu (1992).

Figure 19. Distribution of the Effective Length of the Unemployment Subsidy



In addition, according to our matching estimates, their enrollment in the CR also falls. The foregoing result suggests that one of the objectives of a standard unemployment insurance, namely, allowing beneficiaries to make efficient job choices while job searching, would not be achieved, since these CR results point to their obtaining more precarious or informal jobs than the ones obtained by non beneficiaries. Furthermore, the reduction in earnings is

higher in the case of formal workers, both for males and females, although less strong for females.

Our DID matching estimates also imply that for males, the US does not increase labor participation, it either reduces it or does not affect it depending on whether we consider the whole sample of males or only those unemployed at the baseline respectively. The concurrence of lower earnings and lower participation in the labor market is consistent with poorer labor opportunities discouraging workers, and consequently, leading to a fall in labor force participation. A similar mechanism to that described above that could lead beneficiaries to end up with lower wages, might also lead them to get lower quality jobs, reinforcing in this way the perverse effects on formality caused by the subsidized regime reported by Camacho and Conover (2009). In particular, household head males experience the following: no increase in the earnings, and possibly a reduction, no increase in their labor-force participation (and possibly some reduction), and higher unemployment rates.

We also find that school attendance of formal female beneficiaries increases, and simultaneously, their unemployment rates either fall or remain unchanged, which might be due to a the possibility of the US allowing them to overcome liquidity constraints that would have prevented them from attending school, and shift some of their labor supply that would have ended up in unemployment, into a higher demand for education. Finally, we find that the US does not affect their infantôs weight or height at birth. The increase in school attendance provides evidence in favor of the US achieving other of its objectives, namely, enabling the unemployed to smooth consumption. The mechanism identified here would be similar to, although different from the one pointed out by Chetty (2008), since in Chettyôs model either the subsidy or severance payments allow liquidity-constrained unemployed individuals to wait longer for a job, while here the subsidy would prompt individuals not only to wait longer (in particular in the case of household head males whose unemployment increases) but also to move out from the labor force and enroll in the education system.

The overall negative effects obtained on labor-market outcomes point to potential deficiencies of the programsô design. The very fact that the program can benefit unemployed workers coming from both the formal and informal sectors, rather than just those coming from the formal sector, poses several challenges and distortions on what a standard unemployment insurance should look like in terms of the financing of the system, the monitoring of the conditions of eligibility, the prioritization of assistance versus labor-market efficiency, and its targeting.

First, only formal employees are contributing to the US program, although informal workers also benefit from it, and there are serious limitations to the possibility and ability to demand the informal workers to contribute to it and comply with standard requirement imposed to beneficiaries of this type of programs.

Secondly, the program does not prevent workers who voluntarily quit their jobs or are fired for some cause to benefit from the US, and although it theoretically requires, it does not enforce ñcontinuing eligibility standardsò like the ones listed by Nicholson and Needless (2006), aimed to guarantee that beneficiaries remain available for work as they benefit from

the US, which includes availability for work and active job search.[53] There are cases in which active job search is required even if it might affect the beneficiariesô performance under self-employment. Actually, given the large size of the informal sector in Colombia, self-employment might affect the US program for both formal and informal beneficiaries by discouraging them from active job search, and as Mazza (2000) finds, by providing perverse incentives to increase the informal sector. Alvarez-Parra and Sánchez (2009), and Bardey and Jaramillo (2011), refer to labor-market opportunities in the informal sector as a hidden labor market, and to participation in such market as a factor that increases the cost of search effort.

By not strictly enforcing the eligibility standards, the US program ends up working more as a social subsidy aimed to assist households in hard times, than as a standard unemployment insurance scheme. Another characteristic designed to ensure that the US gives a higher priority to the assistance component of the program than to its component that seeks to promote a more efficient labor market, is the additional requirement, aimed at targeting the most vulnerable section of the unemployed population, namely, that applicants cannot be enrolled in neither a *Caja* or the contributive regime at the moment of enrollment in the US. Furthermore, that targeting mechanism, aimed at benefiting the most vulnerable, not only leaves labor market issues as a secondary goal behind social assistance, but also provides such social assistance merely targeted on unemployment status to people well off when compared to potential beneficiaries of assistance if such assistance was targeted based on conventional mechanisms, such as individualsôSisben score.

Thirdly, the condition that individuals can benefit from the US only once, motivated in part by the inability of the regulator to prevent informal workers for continuously free-riding the program, also eliminates a key component of the standard unemployment insurance programs, namely the risk coverage against job loss on the part of risk-averse individuals, who would not accept several risky job offers had they not had the ability to get the unemployment insurance in case of job loss. Since risk-averse beneficiary individuals know they will not be eligible anymore for the US, they will now be more willing to reject the more risky job offers than before, even though under availability of the US it would be optimal for them to accept some, as it has been previously explained in the literature by Acemoglu and Shimer (1999, 2000), and Hopenhayn and Nicolini (1997) among others.

The promotion of job training among the beneficiaries is nonetheless evidence that, at the moment of creating the US program, the policymakers not only thought of assistance, but also of the performance of beneficiaries in the labor market. Actually, we found that beneficiaries enrolled in job-training programs, like that on technology and software, achieved better outcomes. Nonetheless, the high dropout rate from the training programs, suggests the need of reallocating beneficiaries among courses, or making compulsory attendance at training for those who choose to receive it, at least during the term people receive the benefit.

[53] Although Decree 2340 of 2003 requires applicants to be available to work, and it also requires beneficiaries to prove they are looking for a job, these conditions are not actually strictly enforced.

No evaluation is available on the courses provided: their quality, costs of attendance per beneficiary, and dropout status among other things. There is no assessment of the effects of implementing the US programs on the operation of other actions to promote entrepreneurship or on the national system of job training promoted by the SENA. The integration of this program with the labor intermediation schemes currently existing in the country does not go far from implementing labor insertion programs provided by *Cajas*, and there is no evidence regarding the results of the recent alliance between *Asocajas* and *SENA*, so that *Cajas* which want to work with *SENA* may have access to the Public Employment Service. There is also the need for more articulation between training programs, labor insertion programs, and labor intermediation services.

The US program has the potential to promote a more efficient labor market, but to do so it would need to modify its design, adopting some requirements of more standard unemployment insurance programs.

Another issue is that the US program shows an unbalance against the unemployed with no previous enrollment to *Cajas*, as well as a restriction to the benefit of job training programs, only available to the unemployed previously enrolled in a *Caja*. Access to the unemployment benefits by unemployed people with no previous enrollment seems very restricted when compared to the provisions assigned to those with previous enrollment in *Cajas*. In addition, the waiting times to get the unemployment benefits are longer for the unemployed with no previous *Cajas* enrollment.

Nonetheless, beneficiaries previously not enrolled in a *Caja* not only benefit from the program without having had to contribute to it, but also are benefiting from the contributions of those previously enrolled, mostly from the formal sector, who have been contributing to the US program.

In addition, beneficiaries coming from the informal sector pose several challenges to the US program for which standard unemployment insurance programs are not designed. For example, if one wanted them to contribute to the US program, their earnings would be very difficult to observe in order to determine the level of their contribution. They have incentives to keep working in the informal sector becoming discouraged from active job search in the formal sector, their unemployment status is difficult to verify, and thus, if beneficiaries could receive the benefit again, it would be very difficult for the regulator to prevent them from continuously free-riding the program, etc.

The program might be split in one with more standard requirements targeted to formal employees, and another oriented to assist the most vulnerable targeted with instruments like the Sisben score. Both programs could be permanent, with the magnitude of their resources varying inversely to economic conditions.

In this regard, Reyes (2005) proposes a scheme that could be considered for formal employees. The target workers of his proposal are: (i) household heads (males: 18-59, females: 18-54) and their spouses, and (ii) formal employees (those with work contract). His program is designed to benefit workers once a year or, at most, six months per year, provided they were working the previous year and had contributed to the Fund for one year;

or at least for six months if there was a commitment of the government to cover any shortage. Finally, with a replacement rate of 50 percent, the study finds that to fund the program, eligible workers would have to contribute 2.5 percent of their wages. If the program were targeted to all wage earners, regardless of whether or not they are household heads, each eligible worker would have to contribute nearly twice as much, since the other wage earners are much harder hit by unemployment. Reyesôs proposal, which is endorsed by López and Núñez (2007), also includes decreasing contributions with employment duration, and replacement rates decreasing with unemployment duration.

If an unemployment insurance program like the one proposed by Reyes (2005) and López and Núñez (2007) was implemented, the current program, as Reyes suggests, could be exclusively targeted to the most vulnerable population. Its target mechanism could be based on the Sisben score.

More recently, Tenjo (2010) reviewed previous proposals of unemployment insurances for Colombia, and proposed to replace the current system, with one funded by both individual accounts (nurtured with about 50 percent of individualsô severance payments) and a solidarity fund (mostly funded with one out of the four points received by the *Cajas*), targeted to the unemployed who had cumulated enough savings to fund 6 months at a 50 percent replacement rate.[54]

Other programs suited for a labor market with a large share of informal sector might as well be considered to complement the US program, such as those designed to promote self-employment. As Kosanovich et al. (2001) find, there are successful programs to assist US applicants to become self-employed, by allowing them to receive unemployment benefits with the requirement to devote themselves to self-employment in lieu of the standard unemployment insurance job search requirements. In the future, it might be worthwhile assessing whether or not those approaches are suitable in the Colombian case.

Finally, it is important to highlight that although we have pointed to several caveats of the program that prevents it from getting better outcomes in the labor market, the positive effects it seems to be exerting in lessening liquidity constraints of beneficiaries might actually surpass, in terms of welfare, those deficiencies, since the positive effects on smoothing consumption could be affecting not only the individuals, but also its human capital accumulation through education, as well as through health and other means, that can potentially have long-term permanent effects on the individualsô productivity, as Low, Meghir and Pistaferri (2010) find.

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Appendix 1: The Familiar Subsidy and the Family Compensation Funds (CCF)

In 1957, the Colombian government established the family subsidy, paid by firms with at least 20 workers, and targeted to full-time workers with dependent children less than 18 years old, or unable to work. The subsidy is funded by firms with contributions of 4 percent of the wages they pay. The law also ordered employers who were required to pay this subsidy, to form Family Compensation Funds (*Cajas de Compensación Familiar*, *Cajas*).

In 1982 the subsidy was targeted to workers of medium or low wages (up to four minimum wages) working at least part-time or 96 hours per month. Its magnitude was determined to be proportional to the number of dependents, and all employers were demanded to pay it regardless of their firmôs size. Finally, 55 percent of the funds collected by the *Cajas* had to be invested on the family subsidy, up to 10 percent for installment, administration and operation expenses, up to 3 percent for legal reserves, and the balance for social programs to pay the subsidies in the form of services, or in kind. At that time, the social programs provided by the *Cajas* for these payments were required to be included in the fields of: (i) Health, (ii) Nutrition and the marketing of food and familyôs basket products. (The *Cajas* are monitored by the Family Subsidy Superintendence.) (iii) Education, (iv) Housing, (v) Credit for family firms, (vi) Social recreation, and (vii) the Marketing of other products.

In 1993, Law 100 ordered the *Cajas* to fund the subsidiary regime in health with 5 to 10 percent of their resources collected to fund the family subsidy, and permitted then to administer those resources. Later Law 789 allowed them, as from 2002, to invest in the subsidiary regime, the system of professional risks, the system of pensions, and to participate in the market of microcredit. It also allowed them to participate in a wide variety of activities like recreation, sports, tourism, culture, social housing, credit, childcare, schooling, job training, attention to the aged, nutrition, etc.

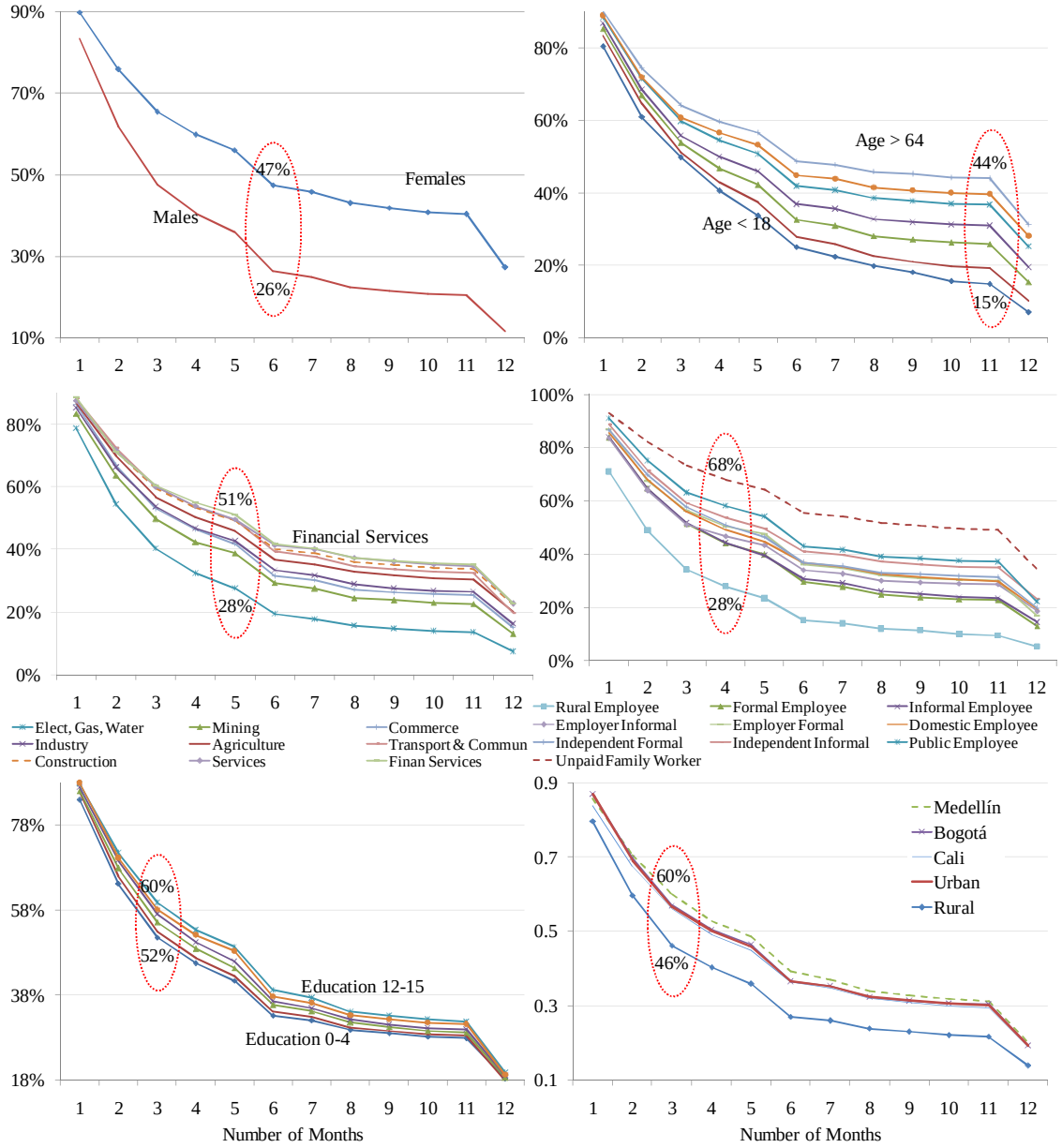
Appendix 2. Determinants of Formality in Colombia, by National and Urban Labor Markets and Definition of Formality

Variable	National				Urban			
	ILO		Health-Pension		ILO		Health-Pension	
	Coeff.	Marg. Effect	Coeff.	Marg. Effect	Coeff.	Marg. Effect	Coeff.	Marg. Effect
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
Male	0.6775	0.1109	0.6057	0.0652	0.6983	0.1618	0.4487	0.0796
	7.24	*7.63*	*5.86*	*6.14*	*6.16*	*6.33*	*3.66*	*3.71*
Age	0.0974	0.0166	0.1977	0.0222	0.0836	0.0197	0.1894	0.0340
	11.23	*11.4*	*18.34*	*19.5*	*8.16*	*8.19*	*15.15*	*15.7*
Age Squared	-0.0013	-0.0002	-0.0024	-0.0003	-0.0012	-0.0003	-0.0024	-0.0004
	-12.42	*-12.67*	*-18.25*	*-19.51*	*-9.33*	*-9.37*	*-15.23*	*-15.83*
Primary*	0.6870	0.1219	0.9671	0.1183	0.7397	0.1784	0.7369	0.1435
	5.37	*5.23*	*5.72*	*5.35*	*3.98*	*3.96*	*3.16*	*2.97*
Incomp. Secondary	1.25	0.2536	1.55	0.2431	1.16	0.2813	1.14	0.2367
	8.91	*8.07*	*8.65*	*6.96*	*5.96*	*6.15*	*4.77*	*4.38*
Complete Secondary	2.13	0.4550	2.53	0.4515	2.07	0.4749	2.12	0.4484
	14.51	*14.44*	*13.68*	*11.65*	*10.26*	*12.27*	*8.71*	*8.76*
Incomplete Higher	2.94	0.6260	3.14	0.6193	2.84	0.5842	2.69	0.5820
	18.87	*25.57*	*16.31*	*17.21*	*13.57*	*22.38*	*10.75*	*13.08*
Complete Higher	4.10	0.7441	3.57	0.7054	4.00	0.6447	3.12	0.6500
	23.08	*60.7*	*17.5*	*23.9*	*17.5*	*51.82*	*11.98*	*18.98*
Post University	4.95	0.7749	3.57	0.7079	4.90	0.6537	3.13	0.6484
	22.44	*100.84*	*16.58*	*23.37*	*18.23*	*81.5*	*11.59*	*19.42*
Male*Yrs of Educ.	-0.0367	-0.0063	-0.0340	-0.0038	-0.0395	-0.0093	-0.0231	-0.0041
	-4.08	*-4.1*	*-3.72*	*-3.74*	*-3.71*	*-3.72*	*-2.18*	*-2.18*
School Atendance	0.2735	0.0497	0.2039	0.0244	0.2711	0.0653	0.2319	0.0436
	3.92	*3.7*	*2.69*	*2.53*	*3.47*	*3.4*	*2.81*	*2.69*
Born in Urban Area	0.2079	0.0354	0.1590	0.0178	0.1673	0.0389	0.1493	0.0262
	4.56	*4.58*	*3.17*	*3.18*	*3.04*	*3.08*	*2.49*	*2.55*
Household Head	0.3024	0.0516	0.3169	0.0356	0.2689	0.0632	0.2663	0.0478
	6.77	*6.77*	*6.71*	*6.68*	*5.26*	*5.27*	*5.02*	*5.02*
Small Town	-0.3436	-0.0546	-0.2274	-0.0241				
	-5.61	*-6.05*	*-3.32*	*-3.53*				
Rural	-0.9271	-0.1406	-0.7024	-0.0709				
	-14.89	*-17.29*	*-10.01*	*-11.34*				
Geographic Region								
Atlantic**	-0.9788	-0.1343	-1.4749	-0.1127	-0.9361	-0.1945	-1.4120	-0.1848
	-12.24	*-15.47*	*-17.29*	*-24.11*	*-10.92*	*-12.88*	*-15.47*	*-22.11*
Eastern	-0.6379	-0.0952	-0.7726	-0.0711	-0.7959	-0.1692	-0.9516	-0.1384
	-8.15	*-9.38*	*-9.8*	*-11.83*	*-9.35*	*-10.66*	*-10.99*	*-13.89*
Central	-0.8287	-0.1190	-0.9728	-0.0855	-0.7833	-0.1667	-0.8765	-0.1294
	-10.52	*-12.63*	*-12.14*	*-15.21*	*-9.12*	*-10.39*	*-10.09*	*-12.54*
Pacific	-1.1084	-0.1519	-1.5054	-0.1197	-1.0187	-0.2098	-1.4316	-0.1887
	-14.01	*-17.72*	*-18.06*	*-24.19*	*-12.03*	*-14.38*	*-16*	*-22.64*
San Andrés	-0.4094	-0.0624	-0.3598	-0.0356	-0.3923	-0.0876	-0.3447	-0.0567
	-3.82	*-4.33*	*-3.38*	*-3.86*	*-3.68*	*-3.91*	*-3.25*	*-3.57*
Amazon., Orinoq.	-0.8932	-0.1186	-1.3565	-0.0966	-0.9047	-0.1856	-1.3573	-0.1717
	-8.65	*-11.59*	*-12.08*	*-19.65*	*-8.84*	*-10.65*	*-12.15*	*-18.55*
Antioquia***	-0.3146	-0.0501	-0.3331	-0.0341	-0.3327	-0.0755	-0.2993	-0.0504
	-4.06	*-4.35*	*-4.31*	*-4.71*	*-3.98*	*-4.15*	*-3.6*	*-3.85*
Valle	-0.2295	-0.0373	-0.3478	-0.0356	-0.2785	-0.0637	-0.4162	-0.0686
	-3.08	*-3.24*	*-4.72*	*-5.17*	*-3.51*	*-3.62*	*-5.3*	*-5.79*
Receives Rents	-0.1910	-0.0310	-0.0354	-0.0039	-0.2072	-0.0475	-0.0723	-0.0127
	-1.85	*-1.96*	*-0.35*	*-0.35*	*-1.88*	*-1.94*	*-0.66*	*-0.67*
Receives Subsidies****	-0.5434	-0.0807	-0.4574	-0.0444	-0.5125	-0.1127	-0.4590	-0.0736
	-6.23	*-7.31*	*-4.79*	*-5.62*	*-5.31*	*-5.79*	*-4.38*	*-4.96*
Constant	-3.81		-6.45		-3.42		-5.80	
	-16.79		*-22.94*		*-12.06*		*-16.89*	
N	20,705				12,013			
Pseudo R2	0.2656		0.2358		0.2038		0.1790	

Source: DANE-ECV-2008. Author's calculation. * Education level comparison is zero years. ** Regional comparison is Bogota. *t* -statistics in italics.
*** In this region is located Medellin. **** Government subsidies like conditional cash transfers (Familias en Accion), social housing, etc.

Logit estimates of formality by definition. Dependent variable is 1 if employed in formal sector.

Appendix 3. Kaplan-Meier Survival Estimates. All Colombian Workers, 2009.



Source: GEIH 2009, DANE

Appendix 4. Share of Subsidies by Status of Enrollment to *Cajas* for Each State

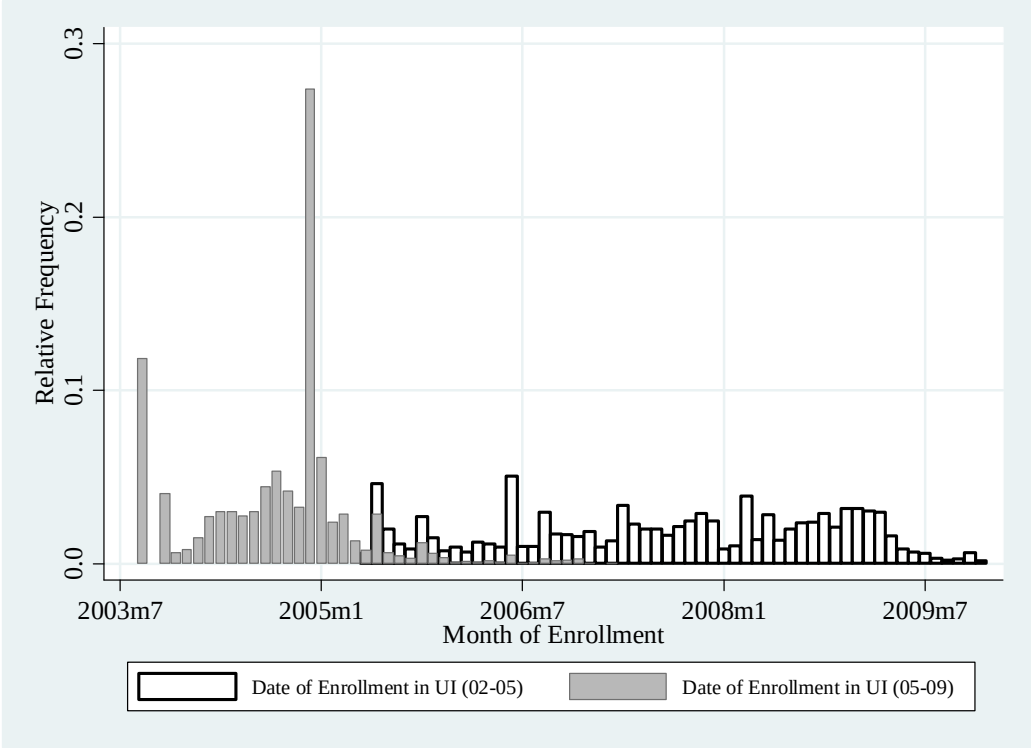
State	Previously Enrolled to CCF	Previously Not Enrolled to CCF
Caldas	0.83	0.17
Cesar	0.83	0.17
Cauca	0.81	0.19
Casanare	0.80	0.20
Cajas Nacionales	0.79	0.21
Nariño	0.79	0.21
San Andrés	0.78	0.22
Tolima	0.77	0.23
Cundinamarca and Bogot	0.77	0.23
Putumayo	0.77	0.23
Antioquia	0.77	0.23
Quindio	0.76	0.24
Atlántico	0.75	0.25
Magdalena	0.72	0.28
Córdoba	0.71	0.29
Santander	0.70	0.30
Valle	0.68	0.32
Boyaca	0.64	0.36
Huila	0.64	0.36
Risaralda	0.62	0.38
Norte de Santander	0.62	0.38
Guajira	0.59	0.41
Bolivar	0.59	0.42
Caquetá	0.55	0.45
Meta	0.54	0.46
Arauca	0.45	0.55
Amazonas	0.31	0.69
Sucre	0.27	0.73
Chocó	0.19	0.81

Source: *Superintendencia del Subsidio Familiar* (Family subsidy Superintendence). Estimates, General Management on Labor Promotion, Social Protection Ministry.

Appendix 5: Sample Construction

Our samples are built by merging two kinds of datasets, one of beneficiaries provided by the two *Cajas* that operate in Antioquia, namely Comfama and Comfenalco, and the other with householdsô and individualsô characteristics, the Sisben surveys. The information of beneficiaries of the unemployment insurance was provided for the period 2003 to 2009, and it is distributed by month according to Figure A5.1. This figure shows the way our beneficiaries are distributed in time according to the date in which they became beneficiaries of the US. In addition, the gray bars show the subsample of beneficiaries matched to the Sisben surveys of 2002 (their source of baseline characteristics) and 2005 (their source of outcomes), while the white bars show the subsample of beneficiaries matched to the Sisben surveys of 2005 (their source of baseline characteristics) and 2009 (their source of outcomes).

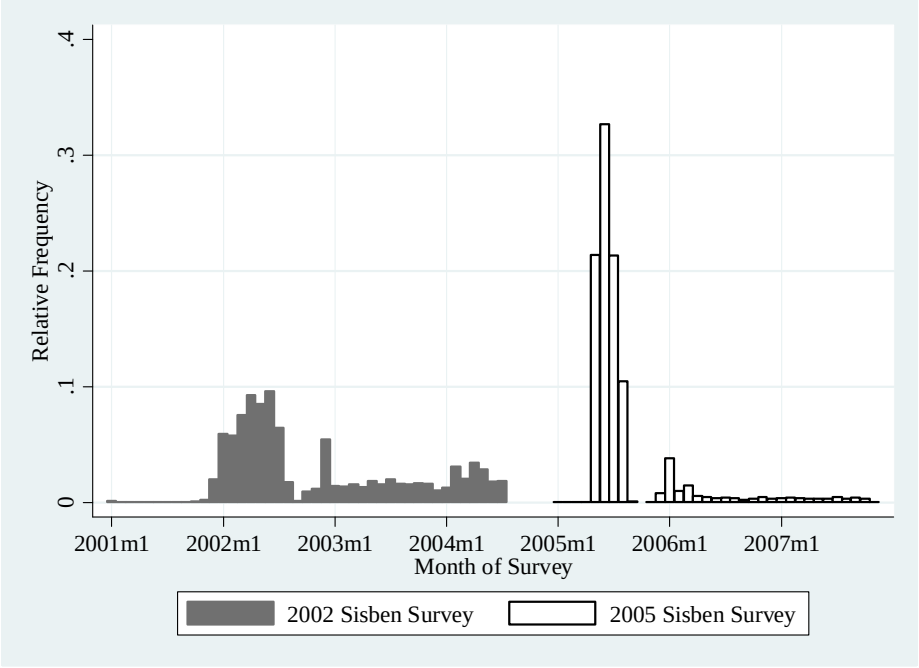
Figure A5.1. Distribution of Beneficiaries According to the Sisben Data They Are Matched To



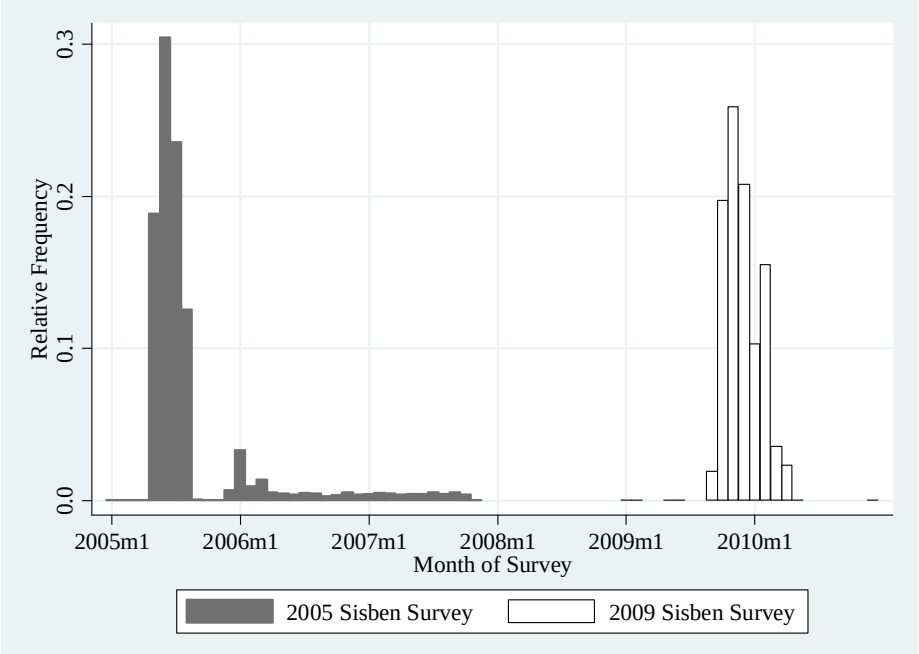
Beneficiaries in the gray bars of Figure A5.1 are included among those accounted for in the 2002 and 2005 Sisben surveys of Figure A5.2, along with the non beneficiaries included in the Sisben surveys of those years. Similarly, beneficiaries in the white bars of Figure A5.1 are included among those accounted for in the 2005 and 2009 Sisben surveys of Figure A5.2, along with the non beneficiaries included in the Sisben surveys of those years.

Note that the 2002 Sisben survey actually includes some households that were interviewed in 2001, 2003 and 2004, but we labeled them as 2002 since most were interviewed that year. The 2005 Sisben survey includes a few individuals in 2006 and 2007; and the 2009 Sisben survey includes about a third of its individuals interviewed in 2010.

Figure A5.2. Distribution of Individuals in the Sisben Surveys
2002 and 2005



2005 and 2009

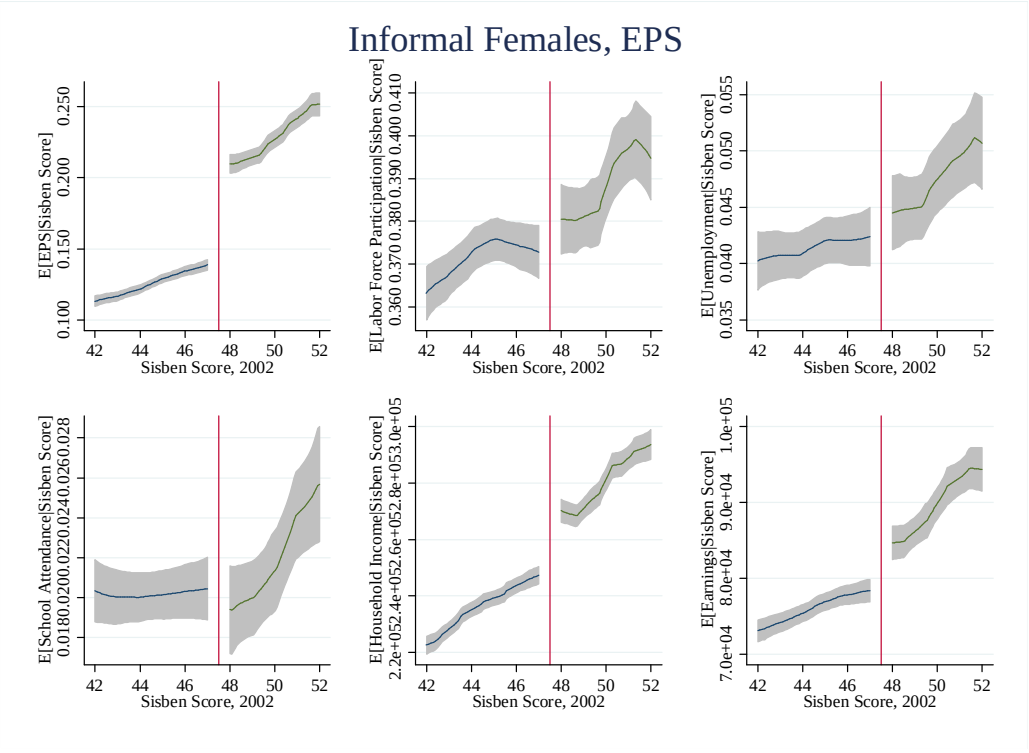


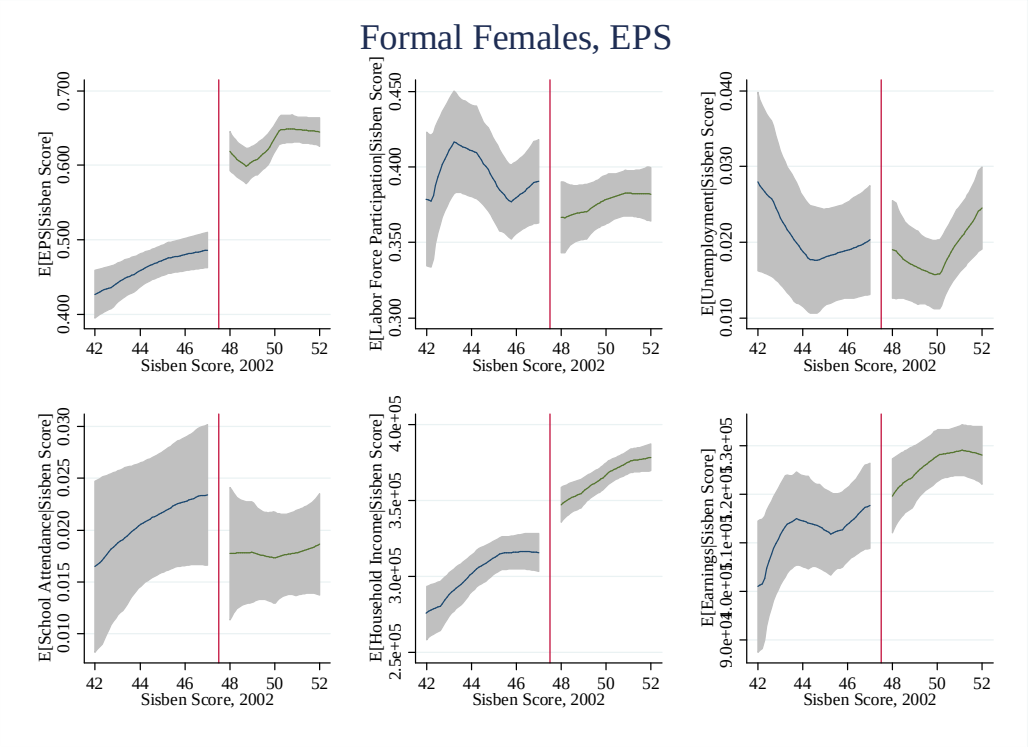
Note that we have 47,604 beneficiaries provided by Comfama and 23,106 beneficiaries provided by Comfenalco for a total of 70,710 beneficiaries for the analyzed period. 6,004 of those beneficiaries were matched to both 2002 and 2005 Sisben surveys, and in addition, we matched between the 2002 and 2005 Sisben surveys 438,565 individuals to create our comparison group in that period. We also matched 14,364 beneficiaries to both 2005 and 2009 Sisben surveys, and additionally, we matched between the 2005 and 2009 Sisben surveys 578,919 individuals to form our comparison group in that period. These figures are not comparable to the ones in annexes 2 to 5 since, in those tables, an individual might be included in more than one column.

Note also that, even though the benefit is meant to be targeted only to household heads, as we mentioned previously and is illustrated in the figures included above, our data covers a period of time from 2003 to 2009, thus many individuals who were household heads at the moment they applied for the US, might not have been at the moment our baseline surveys were collected.

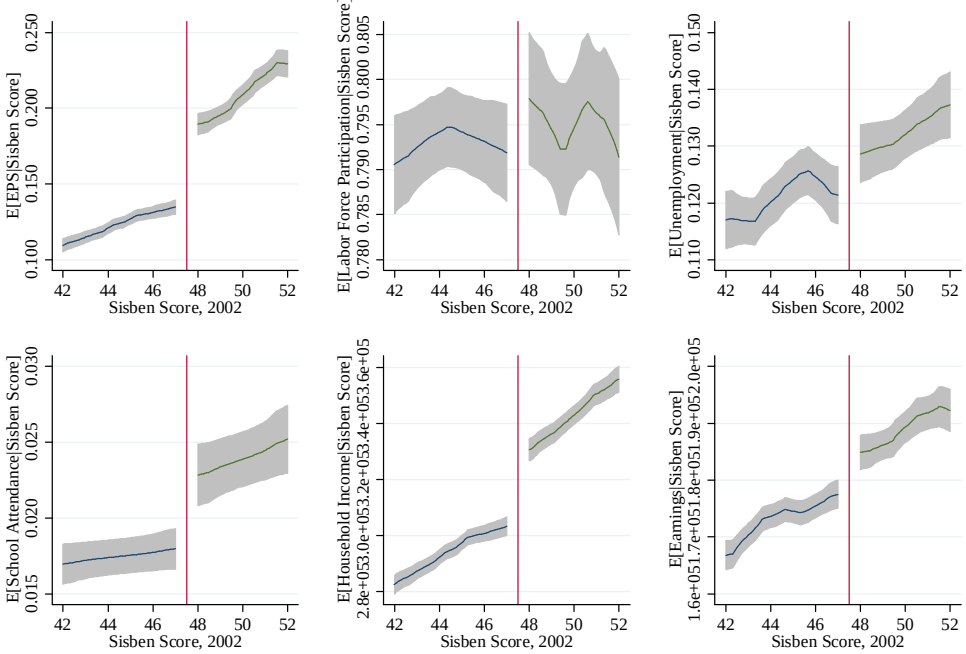
Our analysis is limited to Medellín, since the quality of the Sisben surveys and Vital Statistics Records are better there than in the rest of Antioquia, minimizing potential problems arising from having a censored dataset.

A.6 Testing for Discontinuities in the Outcomes

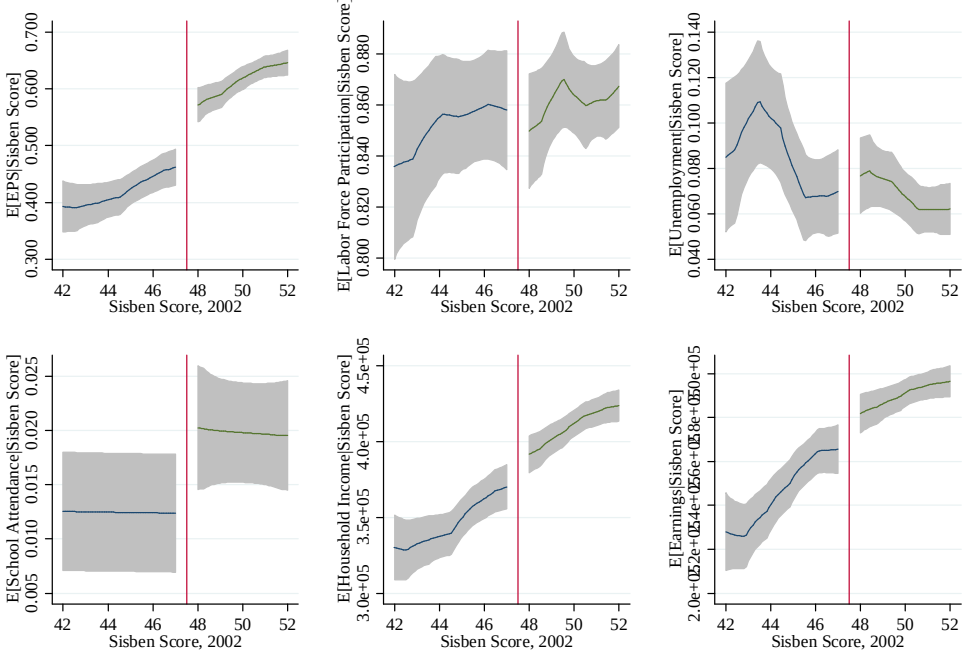




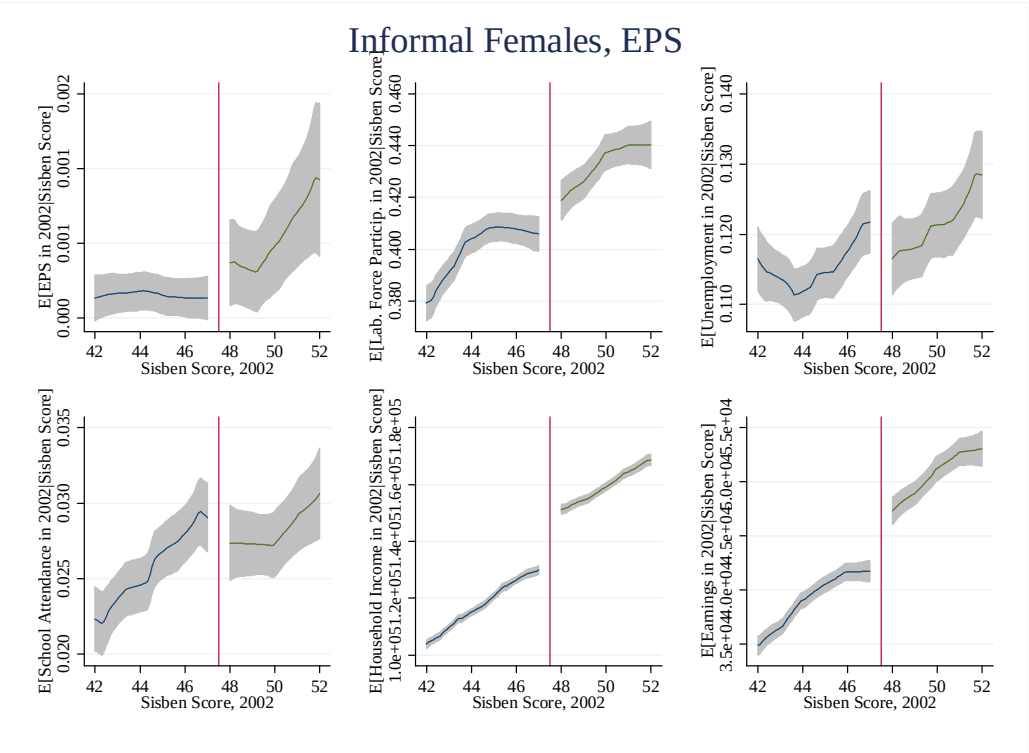
Informal Males, EPS

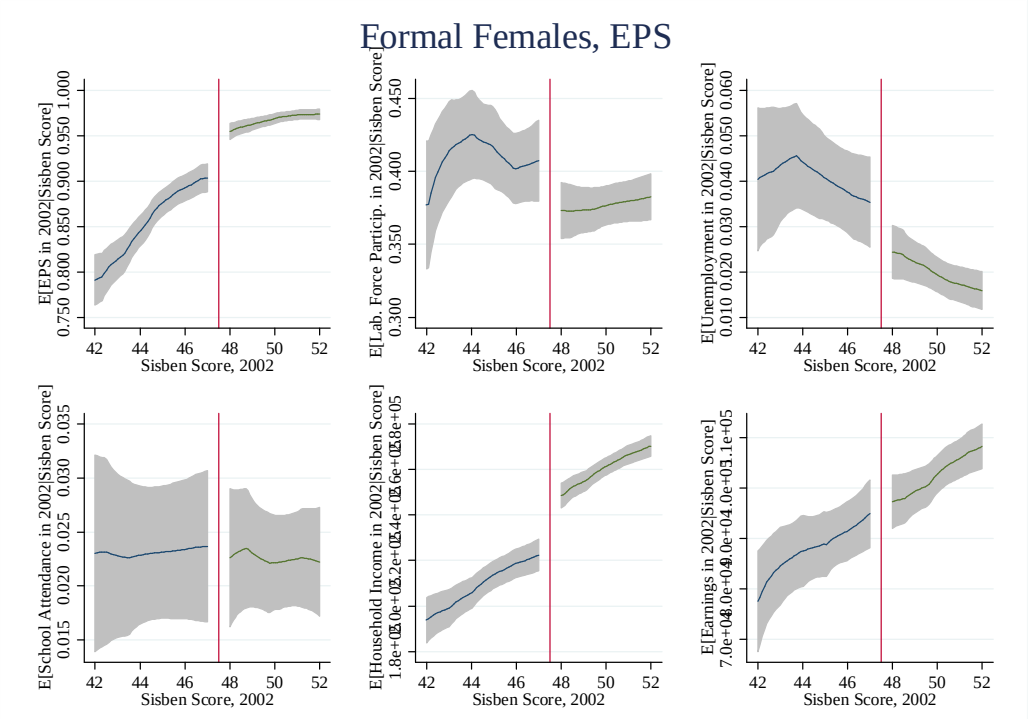


Formal Males, EPS

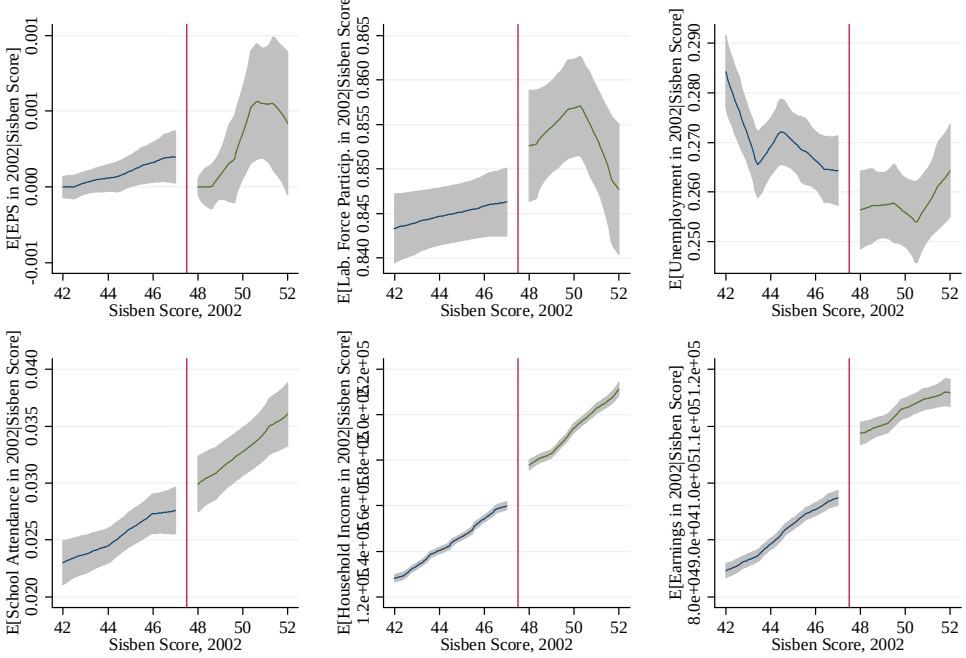


A.7 Testing for Discontinuities in the Lagged Outcomes

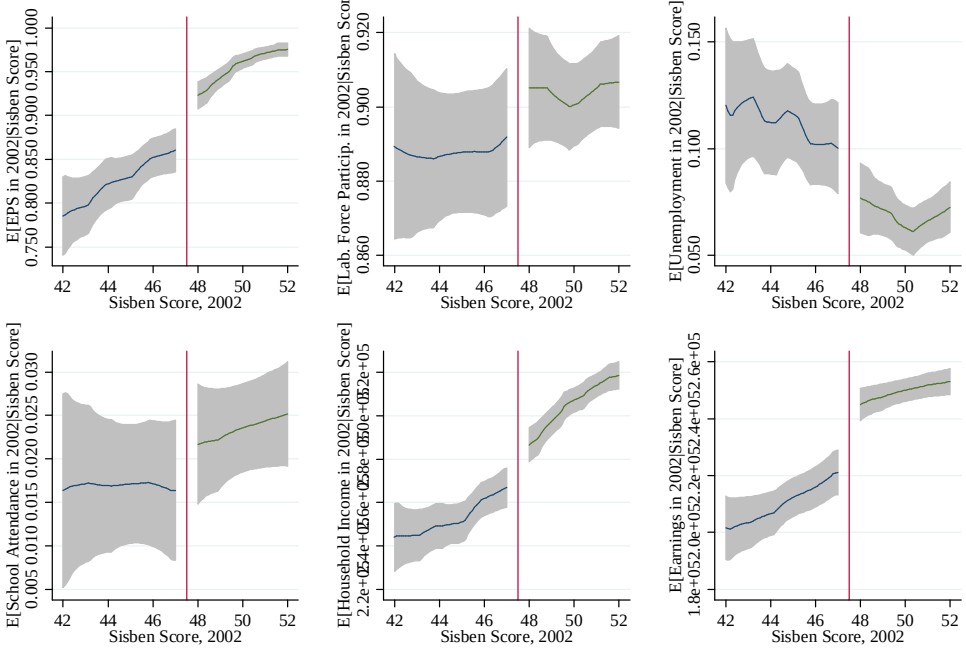




Informal Males, EPS



Formal Males, EPS



Appendix 6: Descriptive Statistics Based on Sisben Survey of 2002 and 2005. All Sample.

Variable	st.	Beneficiaries of Unemployment Insurance								Non Beneficiaries of Unemployment Insurance								Treated vs. Comparison*							
		Males				Females				Males				Females				Males				Females			
		Formal		Informal		Formal		Informal		Formal		Informal		Formal		Informal		Form.		Inf.		Form.		Inf.	
		EPSF	CajaF	EPSI	CajaI	EPSF	CajaF	EPSI	CajaI	EPSF	CajaF	EPSI	CajaI	EPSF	CajaF	EPSI	CajaI								
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School Attendance	**Mean**	0.013	0.012	0.009	0.011	0.021	0.023	0.016	0.023	0.024	0.024	0.019	0.019	0.022	0.022	0.021	0.021	*	*	*				*	
(Output 2005)	**s.d.**	0.114	0.108	0.096	0.104	0.143	0.151	0.125	0.151	0.152	0.152	0.138	0.138	0.147	0.147	0.144	0.144								
Earnings of Household	**Mean**	295,636	285,884	249,043	250,556	288,488	274,130	263,370	274,130	535,493	535,493	376,116	376,116	530,261	530,261	357,835	357,835	*	*	*	*	*	*	*	*
(Outcome 2005)	**s.d.**	238,251	232,560	214,000	218,483	240,799	242,361	232,116	242,361	341,404	341,404	304,180	304,180	353,761	353,761	293,321	293,321								
Earnings	**Mean**	160,930	156,967	133,053	130,138	94,982	79,480	74,061	79,480	332,072	332,072	176,121	176,121	139,796	139,796	78,076	78,076	*	*	*	*	*	*	*	
(Output 2005)	**s.d.**	151,866	149,241	129,817	126,941	120,905	114,466	110,583	114,466	193,300	193,300	166,080	166,080	191,372	191,372	131,221	131,221								
LMP	**Mean**	0.788	0.794	0.792	0.781	0.503	0.449	0.437	0.449	0.897	0.897	0.799	0.799	0.385	0.385	0.374	0.374	*	*			*	*	*	*
(Output 2005)	**s.d.**	0.409	0.405	0.406	0.414	0.500	0.497	0.496	0.497	0.304	0.304	0.401	0.401	0.487	0.487	0.484	0.484								
EPS (Health Insurance)	**Mean**	0.134	0.121	0.039	0.028	0.079	0.048	0.021	0.048	0.719	0.719	0.147	0.147	0.719	0.719	0.153	0.153	*	*	*	*	*	*	*	*
(Output 2005)	**s.d.**	0.341	0.326	0.193	0.166	0.269	0.214	0.143	0.214	0.449	0.449	0.354	0.354	0.450	0.450	0.360	0.360								
Unemployment	**Mean**	0.179	0.182	0.151	0.134	0.059	0.050	0.033	0.050	0.061	0.061	0.124	0.124	0.018	0.018	0.043	0.043	*	*			*	*	*	
(Output 2005)	**s.d.**	0.384	0.386	0.358	0.342	0.236	0.219	0.180	0.219	0.239	0.239	0.330	0.330	0.134	0.134	0.202	0.202								
School Attendance	**Mean**	0.009	0.010	0.008	0.007	0.016	0.011	0.009	0.011	0.036	0.036	0.026	0.026	0.031	0.031	0.025	0.025	*	*	*	*	*	*	*	*
(Baseline 2002)	**s.d.**	0.096	0.097	0.088	0.080	0.126	0.107	0.093	0.107	0.187	0.187	0.160	0.160	0.172	0.172	0.155	0.155								
Earnings of Household	**Mean**	192,812	192,986	146,110	127,319	140,240	126,809	110,271	126,809	386,362	386,362	176,568	176,568	360,568	360,568	148,983	148,983	*	*	*	*	*	*	*	*
(Baseline 2002)	**s.d.**	191,091	184,655	113,158	84,350	121,884	116,896	105,763	116,896	184,859	184,859	170,450	170,450	185,186	185,186	156,670	156,670								
Earnings	**Mean**	153,414	154,167	110,143	91,523	92,967	71,156	51,764	71,156	286,449	286,449	96,011	96,011	118,261	118,261	42,013	42,013	*	*	*		*	*	*	*
(Baseline 2002)	**s.d.**	172,013	164,510	102,823	83,582	111,596	97,261	76,833	97,261	132,640	132,640	136,654	136,654	151,360	151,360	75,286	75,286								
LMP	**Mean**	0.926	0.931	0.896	0.874	0.599	0.521	0.465	0.521	0.927	0.927	0.850	0.850	0.395	0.395	0.405	0.405			*		*	*	*	*
(B 2002)	**s.d.**	0.262	0.254	0.306	0.332	0.490	0.500	0.499	0.500	0.260	0.260	0.357	0.357	0.489	0.489	0.491	0.491								
Unemployment	**Mean**	0.246	0.232	0.227	0.249	0.119	0.100	0.085	0.100	0.047	0.047	0.272	0.272	0.015	0.015	0.120	0.120	*	*	*		*	*	*	*
(Baseline 2002)	**s.d.**	0.431	0.422	0.419	0.433	0.324	0.300	0.279	0.300	0.211	0.211	0.445	0.445	0.121	0.121	0.325	0.325								
Household Head Gender	**Mean**	0.864	0.857	0.834	0.837	0.322	0.367	0.388	0.367	0.864	0.864	0.761	0.761	0.659	0.659	0.520	0.520			*	*	*	*	*	*
(Baseline 2002)	**s.d.**	0.343	0.351	0.373	0.369	0.468	0.482	0.487	0.482	0.343	0.343	0.426	0.426	0.474	0.474	0.500	0.500								
HH Labor Participation	**Mean**	0.827	0.831	0.818	0.807	0.784	0.764	0.743	0.764	0.831	0.831	0.760	0.760	0.799	0.799	0.749	0.749			*	*		*		
(Baseline 2002)	**s.d.**	0.378	0.375	0.386	0.395	0.412	0.425	0.437	0.425	0.375	0.375	0.427	0.427	0.401	0.401	0.434	0.434								
Household Head Income	**Mean**	149,217	147,192	107,054	94,542	111,413	101,345	89,994	101,345	267,788	267,788	122,193	122,193	260,139	260,139	112,188	112,188	*	*	*	*	*	*	*	*
(Baseline 2002)	**s.d.**	171,753	162,201	94,956	83,138	108,006	100,865	89,383	100,865	149,365	149,365	143,147	143,147	152,436	152,436	134,087	134,087								
Childern under 6 years	**Mean**	0.410	0.473	0.571	0.505	0.462	0.625	0.677	0.625	0.371	0.371	0.374	0.374	0.330	0.330	0.447	0.447		*	*	*	*	*	*	*
(Baseline)	**s.d.**	0.645	0.712	0.805	0.756	0.659	0.775	0.810	0.775	0.597	0.597	0.659	0.659	0.565	0.565	0.683	0.683								
Childern under 18 years	**Mean**	1.758	1.802	1.871	1.824	1.678	2.091	2.256	2.091	1.390	1.390	1.247	1.247	1.341	1.341	1.422	1.422	*	*	*	*	*	*	*	*
(Baseline)	**s.d.**	1.301	1.313	1.426	1.454	1.105	1.378	1.408	1.378	1.117	1.117	1.292	1.292	1.081	1.081	1.225	1.225								
Household's size	**Mean**	1.854	1.878	1.949	1.937	2.632	2.653	2.572	2.653	1.743	1.743	2.623	2.623	2.288	2.288	2.898	2.898		*	*	*	*	*	*	*
(Baseline 2002)	**s.d.**	1.794	1.817	1.828	1.797	2.092	2.223	2.195	2.223	1.456	1.456	2.158	2.158	1.451	1.451	2.122	2.122								
House Ownership	**Mean**	0.337	0.336	0.246	0.213	0.202	0.231	0.235	0.231	0.465	0.465	0.307	0.307	0.438	0.438	0.251	0.251	*	*	*	*	*	*	*	*
(Baseline 2002)	**s.d.**	0.473	0.473	0.431	0.410	0.402	0.422	0.424	0.422	0.499	0.499	0.461	0.461	0.496	0.496	0.434	0.434								
House Rented	**Mean**	0.358	0.360	0.412	0.430	0.328	0.296	0.299	0.296	0.356	0.356	0.371	0.371	0.341	0.341	0.358	0.358			*	*		*	*	*
(Baseline 2002)	**s.d.**	0.480	0.480	0.493	0.496	0.470	0.457	0.458	0.457	0.479	0.479	0.483	0.483	0.474	0.474	0.479	0.479								
House not Owned/Rented	**Mean**	0.304	0.304	0.342	0.358	0.470	0.473	0.467	0.473	0.179	0.179	0.322	0.322	0.221	0.221	0.391	0.391	*	*			*	*	*	*
(Baseline 2002)	**s.d.**	0.460	0.460	0.475	0.480	0.499	0.499	0.499	0.499	0.383	0.383	0.467	0.467	0.415	0.415	0.488	0.488								
Socioeconomic Stratum 1	**Mean**	0.223	0.236	0.311	0.319	0.237	0.355	0.406	0.355	0.169	0.169	0.277	0.277	0.160	0.160	0.272	0.272	*	*			*	*	*	*
(Baseline 2002)	**s.d.**	0.416	0.425	0.463	0.467	0.425	0.479	0.491	0.479	0.375	0.375	0.448	0.448	0.366	0.366	0.445	0.445								
Socioeconomic Stratum is 2	**Mean**	0.672	0.659	0.582	0.573	0.654	0.563	0.523	0.563	0.722	0.722	0.621	0.621	0.719	0.719	0.627	0.627	*	*	*	*	*	*	*	*
(Baseline 2002)	**s.d.**	0.470	0.474	0.494	0.495	0.476	0.496	0.500	0.496	0.448	0.448	0.485	0.485	0.449	0.449	0.484	0.484								
Socioeconomic Stratum 3	**Mean**	0.105	0.104	0.104	0.106	0.107	0.078	0.067	0.078	0.109	0.109	0.101	0.101	0.121	0.121	0.100	0.100						*	*	*
(Baseline 2002)	**s.d.**	0.307	0.306	0.306	0.309	0.310	0.268	0.249	0.268	0.311	0.311	0.301	0.301	0.326	0.326	0.301	0.301								
Individuals with at least	**Mean**	0.455	0.427	0.255	0.232	0.498	0.375	0.262	0.375	0.596	0.596	0.372	0.372	0.573	0.573	0.398	0.398	*	*	*	*	*	*	*	*
Secondary (Baseline 2002)	**s.d.**	0.498	0.495	0.436	0.423	0.500	0.484	0.440	0.484	0.491	0.491	0.483	0.483	0.495	0.495	0.489	0.489								
Individuals with at least	**Mean**	0.979	0.971	0.946	0.948	0.980	0.958	0.930	0.958	0.979	0.979	0.948	0.948	0.980	0.980	0.955	0.955						*	*	
Primary (Baseline 2002)	**s.d.**	0.144	0.167	0.227	0.222	0.139	0.202	0.255	0.202	0.143	0.143	0.222	0.222	0.140	0.140	0.208	0.208								
Number of Observations		759	941	643	461	866	2,004	3,348	2,004	23,352	23,352	124,556	124,556	34,438	34,438	156,299	156,299								

Source: Authors calculations using 2002 and 2005 Sisben surveys, and Comfama and Comfenalco information for beneficiaries. **EPSF**: EPS=1 ∀ D; **CajaF**: Caja=1 for D=1, EPS=1 for D=0; **EPSI**: EPS=0 ∀ D; **CajaI**: Caja=0 for D=1, EPS=0 for D=0. * The asterisc means that the means difference between treatment and comparison are statistically significant.

Appendix 7: Descriptive Statistics Based on Sisben Survey of 2005 and 2009. All Sample.

Variable	st.	Beneficiaries of Unemployment Insurance						Non Beneficiaries of Unemployment Insurance						Treated vs. Comparison*					
		Males			Females			Males			Females			Males			Females		
		Formal		Informal	Formal		Informal	Formal		Informal	Formal		Informal	Form.		Inf.	Form.		Inf.
		EPSF	CajaF	EPSI	EPSF	CajaF	EPSI	EPSF	CajaF	EPSI	EPSF	CajaF	EPSI						
		(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
School Attendance	**Mean**	0.017	0.016	0.011	0.025	0.019	0.014	0.033	0.033	0.026	0.031	0.031	0.028	*	*	*	*	*	*
(Output 2009)	**s.d.**	0.130	0.124	0.104	0.155	0.138	0.119	0.178	0.178	0.159	0.173	0.173	0.164						
Earnings of Household	**Mean**	434,162	441,429	436,752	463,502	425,077	391,351	702,921	702,921	518,924	686,904	686,904	491,135	*	*	*	*	*	*
(Outcome 2009)	**s.d.**	437,771	456,616	482,222	461,487	450,172	440,893	646,162	646,162	537,797	647,360	647,360	515,529						
Earnings	**Mean**	218,493	219,670	204,621	145,609	120,515	98,541	382,277	382,277	206,870	186,389	186,389	108,650	*	*		*	*	*
(Output 2009)	**s.d.**	249,877	247,084	233,206	215,999	193,212	178,891	381,546	381,546	266,972	314,512	314,512	213,556						
LMP	**Mean**	0.791	0.800	0.814	0.539	0.492	0.449	0.876	0.876	0.776	0.417	0.417	0.399	*	*	*	*	*	*
(Output 2009)	**s.d.**	0.407	0.400	0.389	0.499	0.500	0.497	0.330	0.330	0.417	0.493	0.493	0.490						
EPS (Health Insurance)	**Mean**	0.316	0.296	0.207	0.235	0.174	0.116	0.766	0.766	0.255	0.772	0.772	0.270	*	*	*	*	*	*
(Output 2009)	**s.d.**	0.465	0.457	0.405	0.424	0.379	0.321	0.424	0.424	0.436	0.419	0.419	0.444						
Unemployment	**Mean**	0.147	0.137	0.112	0.070	0.054	0.038	0.053	0.053	0.100	0.019	0.019	0.034	*	*		*	*	
(Output 2009)	**s.d.**	0.355	0.344	0.315	0.255	0.225	0.192	0.224	0.224	0.300	0.135	0.135	0.181						
School Attendance	**Mean**	0.011	0.011	0.010	0.020	0.017	0.013	0.036	0.036	0.029	0.031	0.031	0.026	*	*	*	*	*	*
(Baseline 2005)	**s.d.**	0.103	0.106	0.098	0.139	0.131	0.115	0.185	0.185	0.167	0.174	0.174	0.158						
Earnings of Household	**Mean**	441,202	420,113	344,379	472,242	381,406	299,853	670,528	670,528	380,740	654,155	654,155	352,021	*	*	*	*	*	*
(Baseline 2005)	**s.d.**	305,397	301,224	275,495	341,911	318,025	267,634	446,411	446,411	311,971	658,011	658,011	292,207						
Earnings	**Mean**	285,890	269,229	205,572	214,226	146,241	86,669	398,683	398,683	145,749	187,730	187,730	65,104	*	*	*	*	*	*
(Baseline 2005)	**s.d.**	195,044	190,614	166,443	185,079	171,702	131,728	233,109	233,109	159,618	239,723	239,723	116,609						
LMP	**Mean**	0.921	0.907	0.858	0.714	0.577	0.458	0.918	0.918	0.753	0.436	0.436	0.359			*	*	*	*
(B 2005)	**s.d.**	0.269	0.290	0.349	0.452	0.494	0.498	0.274	0.274	0.431	0.496	0.496	0.480						
Unemployment	**Mean**	0.123	0.125	0.129	0.069	0.061	0.049	0.022	0.022	0.165	0.007	0.007	0.062	*	*	*	*	*	*
(Baseline 2005)	**s.d.**	0.329	0.330	0.336	0.254	0.238	0.216	0.146	0.146	0.371	0.084	0.084	0.240						
Household Head Gender	**Mean**	0.794	0.789	0.778	0.395	0.390	0.393	0.808	0.808	0.663	0.640	0.640	0.506		*	*	*	*	*
(Baseline 2005)	**s.d.**	0.405	0.408	0.416	0.489	0.488	0.489	0.394	0.394	0.473	0.480	0.480	0.500						
HH Labor Participation	**Mean**	0.764	0.756	0.731	0.638	0.629	0.632	0.766	0.766	0.638	0.705	0.705	0.639			*	*	*	
(Baseline 2005)	**s.d.**	0.425	0.429	0.444	0.481	0.483	0.482	0.423	0.423	0.481	0.456	0.456	0.480						
Household Head Income	**Mean**	276,860	260,951	203,447	212,743	181,295	153,405	366,606	366,606	186,687	350,566	350,566	181,200	*	*	*	*	*	*
(Baseline 2005)	**s.d.**	206,856	202,533	176,655	196,913	181,425	159,630	279,583	279,583	179,876	556,659	556,659	172,819						
Childern under 6 years	**Mean**	0.338	0.327	0.293	0.191	0.222	0.255	0.225	0.225	0.197	0.186	0.186	0.211	*	*	*		*	*
(Baseline)	**s.d.**	0.587	0.587	0.597	0.448	0.492	0.537	0.475	0.475	0.488	0.439	0.439	0.500						
Childern under 18 years	**Mean**	1.470	1.497	1.510	1.039	1.229	1.422	1.093	1.093	0.912	1.008	1.008	1.019	*	*	*		*	*
(Baseline)	**s.d.**	1.193	1.231	1.335	1.080	1.190	1.296	1.064	1.064	1.149	1.043	1.043	1.169						
Household's size	**Mean**	4.973	4.998	5.135	5.169	5.470	5.762	4.421	4.421	5.256	4.441	4.441	5.344	*	*		*	*	*
(Baseline 2005)	**s.d.**	2.191	2.214	2.374	2.573	2.719	2.849	1.811	1.811	2.676	1.890	1.890	2.687						
House Ownership	**Mean**	0.451	0.456	0.461	0.441	0.457	0.467	0.395	0.395	0.469	0.390	0.390	0.500	*	*		*	*	*
(Baseline 2005)	**s.d.**	0.498	0.498	0.499	0.497	0.498	0.499	0.489	0.489	0.499	0.488	0.488	0.500						
House Rented	**Mean**	0.058	0.058	0.048	0.051	0.047	0.043	0.073	0.073	0.039	0.072	0.072	0.037	*	*		*	*	
(Baseline 2005)	**s.d.**	0.235	0.234	0.214	0.220	0.213	0.203	0.261	0.261	0.193	0.258	0.258	0.190						
House not Owned/Rented	**Mean**	0.369	0.359	0.344	0.387	0.352	0.332	0.457	0.457	0.361	0.463	0.463	0.333	*	*		*	*	
(Baseline 2005)	**s.d.**	0.483	0.480	0.475	0.487	0.478	0.471	0.498	0.498	0.480	0.499	0.499	0.471						
Socioeconomic Stratum 1	**Mean**	0.061	0.049	0.018	0.058	0.033	0.012	0.081	0.081	0.012	0.074	0.074	0.012	*	*		*	*	
(Baseline 2005)	**s.d.**	0.239	0.216	0.133	0.234	0.178	0.108	0.272	0.272	0.111	0.262	0.262	0.109						
Socioeconomic Stratum is 2	**Mean**	0.691	0.704	0.763	0.688	0.759	0.832	0.560	0.560	0.758	0.548	0.548	0.763	*	*		*	*	*
(Baseline 2005)	**s.d.**	0.462	0.456	0.425	0.463	0.428	0.374	0.496	0.496	0.428	0.498	0.498	0.425						
Socioeconomic Stratum 3	**Mean**	0.248	0.246	0.219	0.253	0.208	0.156	0.359	0.359	0.229	0.378	0.378	0.225	*	*		*	*	*
(Baseline 2005)	**s.d.**	0.432	0.431	0.414	0.435	0.406	0.363	0.480	0.480	0.420	0.485	0.485	0.417						
Individuals with at least	**Mean**	0.671	0.639	0.479	0.690	0.541	0.399	0.726	0.726	0.479	0.714	0.714	0.499	*	*		*	*	*
Secondary (Baseline 2005)	**s.d.**	0.470	0.480	0.500	0.463	0.498	0.490	0.446	0.446	0.500	0.452	0.452	0.500						
Individuals with at least	**Mean**	0.992	0.990	0.977	0.998	0.982	0.969	0.987	0.987	0.962	0.988	0.988	0.968	*		*	*	*	
Primary (Baseline 2005)	**s.d.**	0.091	0.099	0.151	0.049	0.132	0.173	0.115	0.115	0.191	0.110	0.110	0.175						
Number of Observations		1,575	2,127	726	2,470	5,666	4,388	54,939	54,939	89,621	77,955	77,955	11,326						

Source: Authors calculations using 2002 and 2005 Sisben surveys, and Comfama and Comfenalco information for beneficiaries. **EPSF**: EPS=1 $\forall$ D; **CajaF**: Caja=1 for D=1, EPS=1 for D=0; **EPSI**: EPS=0 $\forall$ D; **CajaI**: Caja=0 for D=1, EPS=0 for D=0. * The asterisc means that the means difference between treatment and comparison are statistically significant.

Appendix 8: Descriptive Statistics Based on Sisben Survey of 2002 and 2005. Household Heads.

Variable	st.	Beneficiaries of Unemployment Insurance								Non Beneficiaries of Unemployment Insurance								Treated vs. Comparison*							
		Males				Females				Males				Females				Males				Females			
		Formal		Informal		Formal		Informal		Formal		Informal		Formal		Informal		Form.		Inf.		Form.		Inf.	
		EPSF	CajaF	EPSI	CajaI	EPSF	CajaF	EPSI	CajaI	EPSF	CajaF	EPSI	CajaI	EPSF	CajaF	EPSI	CajaI								
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School Attendance	**Mean**	0.008	0.008	0.008	0.008	0.014	0.022	0.017	0.022	0.012	0.012	0.006	0.006	0.014	0.014	0.015	0.015								
(Output 2005)	**s.d.**	0.089	0.088	0.088	0.090	0.118	0.148	0.128	0.148	0.107	0.107	0.077	0.077	0.118	0.118	0.121	0.121								
Earnings of Household	**Mean**	286,144	277,799	242,149	242,633	289,828	277,536	260,817	277,536	505,849	505,849	345,928	345,928	537,799	537,799	347,519	347,519	*	*	*	*	*	*	*	*
(Outcome 2005)	**s.d.**	223,763	221,070	207,643	208,755	250,810	253,189	236,905	253,189	303,852	303,852	267,266	267,266	378,676	378,676	296,065	296,065								
Earnings	**Mean**	168,275	165,837	143,004	138,365	111,321	97,023	89,424	97,023	348,730	348,730	206,934	206,934	229,042	229,042	105,365	105,365	*	*	*	*	*	*	*	*
(Output 2005)	**s.d.**	151,130	149,117	130,385	126,441	124,724	119,791	116,033	119,791	184,161	184,161	159,818	159,818	193,285	193,285	137,646	137,646								
LMP	**Mean**	0.796	0.801	0.808	0.802	0.590	0.538	0.526	0.538	0.923	0.923	0.878	0.878	0.599	0.599	0.507	0.507	*	*	*	*		*		*
(Output 2005)	**s.d.**	0.403	0.399	0.394	0.399	0.492	0.499	0.499	0.499	0.267	0.267	0.328	0.328	0.490	0.490	0.500	0.500								
EPS (Health Insurance)	**Mean**	0.134	0.121	0.039	0.030	0.070	0.046	0.019	0.046	0.741	0.741	0.157	0.157	0.669	0.669	0.132	0.132	*	*	*	*	*	*	*	*
(Output 2005)	**s.d.**	0.341	0.326	0.194	0.170	0.255	0.211	0.137	0.211	0.438	0.438	0.364	0.364	0.470	0.470	0.338	0.338								
Unemployment	**Mean**	0.162	0.162	0.135	0.125	0.070	0.062	0.044	0.062	0.052	0.052	0.096	0.096	0.024	0.024	0.047	0.047	*	*	*		*	*		*
(Output 2005)	**s.d.**	0.369	0.369	0.342	0.331	0.255	0.241	0.206	0.241	0.222	0.222	0.295	0.295	0.154	0.154	0.212	0.212								
School Attendance	**Mean**	0.008	0.009	0.006	0.003	0.023	0.015	0.010	0.015	0.014	0.014	0.006	0.006	0.021	0.021	0.015	0.015								
(Baseline 2002)	**s.d.**	0.089	0.095	0.076	0.052	0.149	0.122	0.101	0.122	0.118	0.118	0.076	0.076	0.142	0.142	0.120	0.120								
Earnings of Household	**Mean**	193,092	191,409	144,509	129,325	124,706	104,914	84,340	104,914	361,906	361,906	159,676	159,676	294,892	294,892	98,503	98,503	*	*	*	*	*	*	*	*
(Baseline 2002)	**s.d.**	190,078	181,894	104,940	82,923	116,518	106,752	87,988	106,752	150,575	150,575	165,702	165,702	148,905	148,905	102,624	102,624								
Earnings	**Mean**	165,946	165,080	120,237	104,453	114,969	93,064	70,877	93,064	302,018	302,018	134,667	134,667	219,797	219,797	75,237	75,237	*	*	*	*	*	*	*	*
(Baseline 2002)	**s.d.**	177,159	167,226	94,584	80,897	112,170	99,663	76,252	99,663	123,292	123,292	154,161	154,161	142,371	142,371	86,047	86,047								
LMP	**Mean**	0.939	0.947	0.930	0.911	0.739	0.684	0.635	0.684	0.959	0.959	0.940	0.940	0.679	0.679	0.626	0.626	*			*	*			*
(B 2002)	**s.d.**	0.239	0.225	0.256	0.286	0.440	0.465	0.481	0.465	0.198	0.198	0.237	0.237	0.467	0.467	0.484	0.484								
Unemployment	**Mean**	0.210	0.196	0.188	0.209	0.142	0.131	0.114	0.131	0.040	0.040	0.153	0.153	0.017	0.017	0.122	0.122	*	*	*	*	*	*		
(Baseline 2002)	**s.d.**	0.408	0.397	0.391	0.407	0.349	0.338	0.318	0.338	0.195	0.195	0.360	0.360	0.130	0.130	0.327	0.327								
Household Head Gender	**Mean**	0.995	0.994	0.980	0.978	0.004	0.004	0.005	0.004	0.994	0.994	0.993	0.993	0.009	0.009	0.008	0.008			*		*	*		*
(Baseline 2002)	**s.d.**	0.069	0.080	0.139	0.146	0.059	0.063	0.073	0.063	0.078	0.078	0.083	0.083	0.095	0.095	0.090	0.090								
HH Labor Participation	**Mean**	0.939	0.947	0.930	0.911	0.739	0.684	0.635	0.684	0.959	0.959	0.940	0.940	0.679	0.679	0.626	0.626	*			*	*			*
(Baseline 2002)	**s.d.**	0.239	0.225	0.256	0.286	0.440	0.465	0.481	0.465	0.198	0.198	0.237	0.237	0.467	0.467	0.484	0.484								
Household Head Income	**Mean**	165,946	165,080	120,237	104,453	114,969	93,064	70,877	93,064	302,018	302,018	134,667	134,667	219,797	219,797	75,237	75,237	*	*	*	*	*	*	*	*
(Baseline 2002)	**s.d.**	177,159	167,226	94,584	80,897	112,170	99,663	76,252	99,663	123,292	123,292	154,161	154,161	142,371	142,371	86,047	86,047								
Childern under 6 years	**Mean**	0.449	0.513	0.636	0.575	0.426	0.567	0.619	0.567	0.440	0.440	0.536	0.536	0.268	0.268	0.471	0.471		*	*		*	*	*	*
(Baseline)	**s.d.**	0.668	0.727	0.822	0.784	0.632	0.741	0.774	0.741	0.627	0.627	0.735	0.735	0.505	0.505	0.670	0.670								
Childern under 18 years	**Mean**	1.939	1.957	2.072	2.087	1.664	1.970	2.088	1.970	1.582	1.582	1.591	1.591	1.134	1.134	1.355	1.355	*	*	*	*	*	*	*	*
(Baseline)	**s.d.**	1.275	1.259	1.352	1.412	1.044	1.266	1.284	1.266	1.070	1.070	1.303	1.303	0.988	0.988	1.103	1.103								
Household's size	**Mean**	1.519	1.557	1.589	1.537	2.557	2.626	2.589	2.626	1.360	1.360	1.912	1.912	2.214	2.214	2.891	2.891	*	*	*	*	*	*	*	*
(Baseline 2002)	**s.d.**	1.569	1.626	1.609	1.503	2.261	2.420	2.491	2.420	1.237	1.237	2.025	2.025	2.088	2.088	2.525	2.525								
House Ownership	**Mean**	0.325	0.323	0.241	0.211	0.173	0.191	0.194	0.191	0.434	0.434	0.234	0.234	0.294	0.294	0.150	0.150	*	*			*	*	*	*
(Baseline 2002)	**s.d.**	0.469	0.468	0.428	0.409	0.379	0.393	0.395	0.393	0.496	0.496	0.424	0.424	0.455	0.455	0.357	0.357								
House Rented	**Mean**	0.371	0.371	0.429	0.450	0.294	0.260	0.256	0.260	0.382	0.382	0.416	0.416	0.313	0.313	0.297	0.297						*	*	*
(Baseline 2002)	**s.d.**	0.483	0.484	0.495	0.498	0.456	0.439	0.437	0.439	0.486	0.486	0.493	0.493	0.464	0.464	0.457	0.457								
House not Owned/Rented	**Mean**	0.304	0.305	0.331	0.339	0.532	0.549	0.550	0.549	0.184	0.184	0.350	0.350	0.393	0.393	0.553	0.553	*	*			*	*		
(Baseline 2002)	**s.d.**	0.460	0.461	0.471	0.474	0.499	0.498	0.498	0.498	0.387	0.387	0.477	0.477	0.489	0.489	0.497	0.497								
Socioeconomic Stratum 1	**Mean**	0.229	0.242	0.297	0.298	0.221	0.331	0.385	0.331	0.177	0.177	0.308	0.308	0.165	0.165	0.265	0.265	*	*			*	*	*	*
(Baseline 2002)	**s.d.**	0.421	0.428	0.458	0.458	0.415	0.471	0.487	0.471	0.382	0.382	0.461	0.461	0.371	0.371	0.441	0.441								
Socioeconomic Stratum is 2	**Mean**	0.667	0.649	0.595	0.604	0.669	0.585	0.537	0.585	0.721	0.721	0.606	0.606	0.705	0.705	0.626	0.626	*	*				*	*	*
(Baseline 2002)	**s.d.**	0.472	0.477	0.491	0.490	0.471	0.493	0.499	0.493	0.449	0.449	0.489	0.489	0.456	0.456	0.484	0.484								
Socioeconomic Stratum 3	**Mean**	0.104	0.108	0.106	0.098	0.109	0.081	0.074	0.081	0.102	0.102	0.085	0.085	0.130	0.130	0.109	0.109						*	*	*
(Baseline 2002)	**s.d.**	0.305	0.310	0.308	0.297	0.311	0.273	0.262	0.273	0.302	0.302	0.279	0.279	0.336	0.336	0.311	0.311								
Individuals with at least	**Mean**	0.443	0.414	0.247	0.230	0.510	0.385	0.262	0.385	0.565	0.565	0.315	0.315	0.544	0.544	0.375	0.375	*	*	*	*		*	*	
Secondary (Baseline 2002)	**s.d.**	0.497	0.493	0.431	0.422	0.500	0.487	0.440	0.487	0.496	0.496	0.464	0.464	0.498	0.498	0.484	0.484								
Individuals with at least	**Mean**	0.981	0.977	0.955	0.954	0.982	0.958	0.927	0.958	0.983	0.983	0.950	0.950	0.979	0.979	0.955	0.955						*	*	
Primary (Baseline 2002)	**s.d.**	0.137	0.151	0.208	0.210	0.131	0.200	0.259	0.200	0.128	0.128	0.219	0.219	0.143	0.143	0.207	0.207								
Number of Observations		628	770	511	369	571	1,249	2,027	1,249	18,070	18,070	72,609	72,609	10,455	10,455	62,434	62,434								

Source: Authors calculations using 2002 and 2005 Sisben surveys, and Comfama and Comfenalco information for beneficiaries. **EPSF**: EPS=1 ∀ D; **CajaF**: Caja=1 for D=1, EPS=1 for D=0; **EPSI**: EPS=0 ∀ D; **CajaI**: Caja=0 for D=1, EPS=0 for D=0. * The asterisc means that the means difference between treatment and comparison are statistically significant.

Appendix 9: Descriptive Statistics Based on Sisben Survey of 2005 and 2009. Household Heads.

Variable	st.	Beneficiaries of Unemployment Insurance						Non Beneficiaries of Unemployment Insurance						Treated vs. Comparison*					
		Males			Females			Males			Females			Males			Females		
		Formal		Informal	Formal		Informal	Formal		Informal	Formal		Informal	Form.		Inf.	Form.		Inf.
		EPSF	CajaF	EPSI	EPSF	CajaF	EPSI	EPSF	CajaF	EPSI	EPSF	CajaF	EPSI						
		(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
School Attendance	**Mean**	0.018	0.015	0.006	0.021	0.013	0.011	0.016	0.016	0.009	0.018	0.018	0.016						
(Output 2009)	**s.d.**	0.132	0.122	0.077	0.143	0.114	0.105	0.126	0.126	0.096	0.132	0.132	0.125						
Earnings of Household	**Mean**	399,509	409,523	413,306	412,763	374,583	338,330	658,042	658,042	459,837	632,611	632,611	433,408	*	*		*	*	*
(Outcome 2009)	**s.d.**	388,270	409,722	442,685	438,950	417,148	392,330	597,814	597,814	469,551	613,647	613,647	478,962						
Earnings	**Mean**	226,021	230,147	221,628	168,427	138,803	115,234	409,835	409,835	249,002	256,101	256,101	134,326	*	*		*	*	*
(Output 2009)	**s.d.**	248,814	247,069	236,376	233,700	204,089	173,707	389,223	389,223	277,559	344,476	344,476	229,605						
LMP	**Mean**	0.810	0.823	0.851	0.589	0.546	0.518	0.900	0.900	0.859	0.505	0.505	0.481	*	*		*	*	*
(Output 2009)	**s.d.**	0.393	0.382	0.357	0.492	0.498	0.500	0.300	0.300	0.348	0.500	0.500	0.500						
EPS (Health Insurance)	**Mean**	0.344	0.317	0.213	0.227	0.166	0.116	0.790	0.790	0.275	0.754	0.754	0.248	*	*	*	*	*	*
(Output 2009)	**s.d.**	0.475	0.465	0.410	0.419	0.373	0.320	0.407	0.407	0.447	0.431	0.431	0.432						
Unemployment	**Mean**	0.147	0.135	0.105	0.051	0.043	0.034	0.046	0.046	0.081	0.019	0.019	0.027	*	*		*	*	
(Output 2009)	**s.d.**	0.355	0.342	0.307	0.219	0.203	0.181	0.210	0.210	0.273	0.138	0.138	0.162						
School Attendance	**Mean**	0.008	0.009	0.008	0.015	0.008	0.005	0.015	0.015	0.007	0.015	0.015	0.010					*	
(Baseline 2005)	**s.d.**	0.091	0.094	0.089	0.121	0.090	0.070	0.120	0.120	0.083	0.122	0.122	0.098						
Earnings of Household	**Mean**	388,900	369,792	301,678	359,009	283,187	227,616	586,559	586,559	296,735	537,567	537,567	260,730	*	*		*	*	*
(Baseline 2005)	**s.d.**	234,372	231,811	217,985	249,007	245,220	220,353	363,434	363,434	218,585	368,700	368,700	226,131						
Earnings	**Mean**	323,935	303,362	233,102	265,480	185,708	128,706	425,388	425,388	214,839	287,019	287,019	127,121	*	*		*	*	
(Baseline 2005)	**s.d.**	184,954	183,643	164,060	169,002	166,596	137,559	239,124	239,124	156,026	246,500	246,500	135,365						
LMP	**Mean**	0.961	0.946	0.905	0.854	0.730	0.650	0.946	0.946	0.909	0.606	0.606	0.588				*	*	*
(B 2005)	**s.d.**	0.194	0.226	0.294	0.353	0.444	0.477	0.227	0.227	0.288	0.489	0.489	0.492						
Unemployment	**Mean**	0.086	0.091	0.105	0.058	0.048	0.039	0.020	0.020	0.103	0.007	0.007	0.044	*	*		*	*	
(Baseline 2005)	**s.d.**	0.281	0.288	0.307	0.233	0.214	0.194	0.141	0.141	0.304	0.085	0.085	0.204						
Household Head Gender	**Mean**	0.993	0.993	0.996	0.009	0.008	0.009	0.998	0.998	0.996	0.014	0.014	0.011					*	
(Baseline 2005)	**s.d.**	0.086	0.082	0.063	0.096	0.088	0.097	0.047	0.047	0.062	0.119	0.119	0.106						
HH Labor Participation	**Mean**	0.961	0.946	0.905	0.854	0.730	0.650	0.946	0.946	0.909	0.606	0.606	0.588				*	*	*
(Baseline 2005)	**s.d.**	0.194	0.226	0.294	0.353	0.444	0.477	0.227	0.227	0.288	0.489	0.489	0.492						
Household Head Income	**Mean**	323,935	303,362	233,102	265,480	185,708	128,706	425,388	425,388	214,839	287,019	287,019	127,121	*	*		*	*	
(Baseline 2005)	**s.d.**	184,954	183,643	164,060	169,002	166,596	137,559	239,124	239,124	156,026	246,500	246,500	135,365						
Childern under 6 years	**Mean**	0.429	0.413	0.368	0.247	0.261	0.283	0.294	0.294	0.337	0.134	0.134	0.220	*	*		*	*	*
(Baseline)	**s.d.**	0.630	0.634	0.651	0.487	0.514	0.550	0.524	0.524	0.602	0.378	0.378	0.506						
Childern under 18 years	**Mean**	1.813	1.834	1.855	1.501	1.651	1.805	1.375	1.375	1.372	0.914	0.914	1.159	*	*	*	*	*	*
(Baseline)	**s.d.**	1.076	1.129	1.286	0.988	1.079	1.178	1.039	1.039	1.210	0.984	0.984	1.175						
Household's size	**Mean**	4.543	4.600	4.759	3.971	4.419	4.762	4.122	4.122	4.331	3.703	3.703	4.206	*	*	*	*	*	*
(Baseline 2005)	**s.d.**	1.667	1.767	2.083	1.943	2.265	2.453	1.495	1.495	2.046	1.789	1.789	2.261						
House Ownership	**Mean**	0.490	0.493	0.501	0.507	0.508	0.514	0.430	0.430	0.575	0.452	0.452	0.568	*	*	*	*	*	*
(Baseline 2005)	**s.d.**	0.500	0.500	0.500	0.500	0.500	0.500	0.495	0.495	0.494	0.498	0.498	0.495						
House Rented	**Mean**	0.069	0.067	0.052	0.055	0.051	0.047	0.080	0.080	0.037	0.062	0.062	0.032						*
(Baseline 2005)	**s.d.**	0.253	0.250	0.222	0.229	0.221	0.211	0.272	0.272	0.189	0.241	0.241	0.176						
House not Owned/Rented	**Mean**	0.325	0.318	0.300	0.305	0.269	0.255	0.414	0.414	0.250	0.397	0.397	0.241	*	*		*	*	
(Baseline 2005)	**s.d.**	0.468	0.466	0.459	0.461	0.444	0.436	0.493	0.493	0.433	0.489	0.489	0.428						
Socioeconomic Stratum 1	**Mean**	0.072	0.059	0.026	0.067	0.034	0.010	0.093	0.093	0.016	0.069	0.069	0.011		*			*	
(Baseline 2005)	**s.d.**	0.259	0.235	0.159	0.250	0.182	0.099	0.290	0.290	0.124	0.253	0.253	0.105						
Socioeconomic Stratum is 2	**Mean**	0.702	0.718	0.767	0.692	0.769	0.837	0.570	0.570	0.788	0.542	0.542	0.755	*	*		*	*	*
(Baseline 2005)	**s.d.**	0.457	0.450	0.423	0.462	0.421	0.370	0.495	0.495	0.408	0.498	0.498	0.430						
Socioeconomic Stratum 3	**Mean**	0.224	0.223	0.207	0.242	0.196	0.153	0.337	0.337	0.196	0.389	0.389	0.234	*	*		*	*	*
(Baseline 2005)	**s.d.**	0.417	0.416	0.405	0.428	0.397	0.360	0.473	0.473	0.397	0.488	0.488	0.423						
Individuals with at least	**Mean**	0.650	0.624	0.483	0.644	0.468	0.333	0.697	0.697	0.418	0.645	0.645	0.421	*	*	*		*	*
Secondary (Baseline 2005)	**s.d.**	0.477	0.485	0.500	0.479	0.499	0.471	0.460	0.460	0.493	0.478	0.478	0.494						
Individuals with at least	**Mean**	0.993	0.992	0.980	0.999	0.977	0.963	0.991	0.991	0.972	0.987	0.987	0.969				*	*	
Primary (Baseline 2005)	**s.d.**	0.086	0.090	0.140	0.034	0.151	0.189	0.095	0.095	0.166	0.114	0.114	0.172						
Number of Observations		1,078	1,468	503	869	2,199	1,806	37,374	37,374	39,201	18,436	18,436	30,387						

Source: Authors calculations using 2002 and 2005 Sisben surveys, and Comfama and Comfenalco information for beneficiaries. **EPSF**: EPS=1 ∀ D; **CajaF**: Caja=1 for D=1, EPS=1 for D=0; **EPSI**: EPS=0 ∀ D; **CajaI**: Caja=0 for D=1, EPS=0 for D=0. * The asterisc means that the means difference between treatment and comparison are statistically significant.

Appendix 10: Descriptive Statistics Based on Sisben Survey of 2010 and Vital Stats. Women Giving Birth.

Variable	Non Beneficiaries			Beneficiaries			Treated vs. Comparison*
	N	Mean	Std. Dev.	N	Mean	Std. Dev.	
Child's Weight at Birth	12,389	3,009	561	167	2,870	629	*
Child's Height at Birth	12,389	48.8	4.0	167	48.5	5.2	
Child's Body Mass Index at Birth, BMI	12,389	12.6	2.0	167	12.1	1.9	*
Gender of HH	12,389	0.568	0.495	167	0.563	0.498	
School Attendance	12,389	0.105	0.307	167	0.030	0.171	*
HH Active in Labor Market	12,389	0.676	0.468	167	0.641	0.481	
Earnings of HH	12,306	191,654	171,022	166	150,358	145,707	*
Children Under 6	12,389	0.457	0.766	167	0.443	0.749	
Children Under 18	12,389	1.319	1.409	167	1.347	1.617	
HH Size	12,389	6.370	3.109	167	6.467	2.690	
House Ownership	12,389	0.463	0.499	167	0.431	0.497	
Socioeconomic Stratum 1	12,389	0.312	0.463	167	0.365	0.483	
Socioeconomic Stratum 2	12,389	0.586	0.492	167	0.539	0.500	
House Rented	12,389	0.042	0.200	167	0.042	0.201	
Number of Households in Housing	12,389	6.287	3.068	167	6.401	2.671	
Secondary Education	12,389	0.647	0.478	167	0.509	0.501	*
Primary Education	12,389	0.982	0.134	167	0.952	0.214	
Year of Survey	12,389	2005	0.573	167	2005	0.452	*
Gestation Length up to 36 Months	12,389	0.129	0.335	167	0.192	0.394	*
Expontaneous Childbirth	12,388	0.671	0.470	167	0.671	0.470	
Mother Between 20 and 40 Years	12,389	0.846	0.361	167	0.939	0.239	*
3 or More Children Born Alive	12,384	0.268	0.443	167	0.437	0.496	*
3 or More Pregnancies Including Current	12,384	0.340	0.474	167	0.503	0.500	*
Father Between 20 and 40 Years	12,388	0.819	0.385	166	0.819	0.385	
Mother Married or in Free Union	10,724	0.678	0.467	125	0.688	0.463	
Mother with Complete Secondary or More	10,731	0.448	0.497	125	0.368	0.482	
Father with Complete Secondary or More	10,734	0.385	0.487	125	0.256	0.436	*

Source: Authors calculations using the 2010 Sisben Survey and Vital Statistics Records (Birth Certificates). * The asterisc means that the means difference between treatment and comparison are statistically significant.

Appendix 11. Mean Differences between Treatment and Comparison Groups, and between Treatment and Matched Comparison for Formal (EPS) Females.

Variable	Treated		Comparison*		*t* of Difference	Difference (Treated-Matched Comparison*)		
	Mean	S.D.	Mean	S.D.		Mean	S.D.	*t*
Household Head Gender (Baseline 2002)	0.325	0.469	0.656	0.475	-20.9	-0.005	0.086	-0.06
Household Head School Attendance (Baseline 2002)	0.015	0.124	0.031	0.174	-3.6	0.000	0.002	0.00
Household Head Labor Participation (Baseline 2002)	0.789	0.408	0.795	0.404	-0.4	-0.001	0.060	-0.01
Household Head Income (Baseline 2002)	112,051	108,205	260,287	152,231	-39.2	-45,575	68,131	-0.67
Childern under 6 years old (Baseline)	0.460	0.654	0.332	0.571	5.8	0.072	0.230	0.31
Childern under 18 years old (Baseline)	1.685	1.108	1.341	1.072	9.2	0.226	0.470	0.48
Household's size (No. Members) (Baseline 2002)	2.630	2.101	2.277	1.442	5.1	0.108	0.559	0.19
House Owned (Baseline 2002)	0.202	0.402	0.437	0.496	-17.0	-0.005	0.069	-0.07
Socioeconomic Stratum is 1 (Baseline)	0.243	0.429	0.160	0.367	5.7	0.006	0.048	0.13
Socioeconomic Stratum is 2 (Baseline)	0.646	0.478	0.719	0.449	-4.5	-0.007	0.055	-0.14
House Rented (Baseline 2002)	0.330	0.470	0.345	0.475	-0.9	-0.001	0.057	-0.01
Individuals with at least Secondary	0.496	0.500	0.577	0.494	-4.8	-0.008	0.102	-0.08
Individuals with at least Primary	0.980	0.141	0.981	0.136	-0.3	0.0000	0.0001	0.10
Year of the Baseline Sisben Survey	2,003	0.793	2,002	0.627	12.3	0.107	0.296	0.36
Number of Observations	942		12,612					

* The number of observations of the comparison group is a random subsample of the universe used to obtain the matching estimates.

A.12 Impact Estimates of the Effect of the Subsidized Regime
(a) With Formality Defined Based on EPS Enrollment and Outomes in Levels

Variable	Females						Males					
	Formal			Informal			Formal			Informal		
	Coeff.		Std. Err.	Coeff.		Std. Err.	Coeff.		Std. Err.	Coeff.		Std. Err.
A. Only Polynomial term in S, Bandwidth: 20 (38 < S < 57)												
1[S<â], γ	0.094	**	0.024	0.278	**	0.007	0.048	**	0.024	0.283	**	0.006
LFP	0.110		0.185	-0.007		0.020	0.196	*	0.133	0.002		0.020
Unemployment	0.008		0.040	-0.001		0.006	-0.035		0.150	-0.025	*	0.021
School Attendance	0.031	*	0.020	0.011	**	0.005	-0.110	*	0.071	-0.004		0.005
Household Income	-195,127	**	84,115	-19,703	*	13,427	-34,749		88,475	-4,415		7,355
Earnings	-62,321		63,485	-8,458	**	2,704	-8,458	**	2,704	4,011		5,704
N. Observations	19,555			128,797			11,765			103,605		
B. Polynomial term in S and Control Variables, Bandwidth: 20 (38 < S < 57)												
1[S<â], γ	0.093	**	0.025	0.280	**	0.007	0.044	*	0.025	0.282	**	0.006
LFP	0.029		0.192	-0.018		0.022	0.182	*	0.156	0.002		0.021
Unemployment	0.009		0.040	-0.007	*	0.004	-0.029		0.148	-0.030	*	0.020
School Attendance	0.036	*	0.021	0.008	*	0.005	-0.105	*	0.075	-0.005	*	0.004
Household Income	-227,910	**	68,048	-15,266	**	4,925	-30,731		92,948	-11,682	*	8,810
Earnings	-69,325		65,134	-10,610	**	3,892	-74,041		89,065	4,301		4,669
N. Observations	19,555			128,797			11,765			103,605		
C. Polynomial term in *S* and Control Variables, Bandwidth: 30 (30 < *S* < 60)												
1[S<â], γ	0.090	**	0.022	0.260	**	0.012	0.058	**	0.025	0.249	**	0.011
LFP	-0.146		0.189	0.012		0.022	-0.010		0.127	0.009		0.019
Unemployment	-0.040		0.044	0.004		0.006	-0.030		0.124	-0.018		0.016
School Attendance	0.011		0.027	0.014	**	0.004	-0.115	*	0.065	0.000		0.004
Household Income	-246,265	**	59,092	-5,223		6,793	-20,798		91,799	-1,553		10,180
Earnings	-123,847	**	61,007	-6,077	*	4,208	-77,420	*	68,670	10,301	*	6,215
N. Observations	27,236			148,334			16,481			120,058		

(b) With Formality Defined Based on EPS Enrollment and Outomes in Differences

Variable	Females						Males					
	Formal			Informal			Formal			Informal		
	Coeff.		Std. Err.	Coeff.		Std. Err.	Coeff.		Std. Err.	Coeff.		Std. Err.
A. Only Polynomial term in S, Bandwidth: 20 (38 < S < 57)												
1[S<â], γ	0.094	**	0.022	0.277	**	0.010	0.047		0.029	0.282	**	0.011
LFP	-0.157	*	0.120	-0.011		0.034	0.266	*	0.194	0.030	*	0.025
Unemployment	-0.039		0.053	-0.034	*	0.021	-0.166		0.168	-0.037		0.039
School Attendance	-0.009		0.034	-0.008		0.009	-0.069		0.093	-0.016	**	0.006
Household Income	-106,794		96,976	-7,860		15,001	11,579		100,496	-9,358		13,176
Earnings	-66,046	*	45,432	-4,053		5,359	-4,053		5,359	16,071	*	8,449
N. Observations	19,555			130,028			12,412			105,554		
B. Polynomial term in S and Control Variables, Bandwidth: 20 (38 < S < 57)												
1[S<â], γ	0.093	**	0.022	0.279	**	0.010	0.044		0.029	0.282	**	0.011
LFP	-0.045		0.111	-0.004		0.024	0.278	*	0.187	0.031	*	0.021
Unemployment	-0.028		0.051	-0.004		0.009	-0.140		0.155	0.004		0.030
School Attendance	-0.059	*	0.036	0.004		0.008	-0.127	*	0.097	-0.012	*	0.006
Household Income	-195,367	**	87,891	-16,416	**	7,789	6,098		105,140	-21,380	**	9,343
Earnings	-75,346	*	47,514	-8,094	*	4,932	-11,685		83,692	8,843		8,055
N. Observations	19,555			130,028			12,412			105,554		
C. Polynomial term in *S* and Control Variables, Bandwidth: 30 (30 < *S* < 60)												
1[S<â], γ	0.083	**	0.020	0.264	**	0.009	0.052	**	0.025	0.256	**	0.010
LFP	-0.106		0.112	-0.001		0.021	0.273	*	0.142	0.015		0.017
Unemployment	-0.034		0.056	0.009		0.009	-0.120		0.128	-0.004		0.022
School Attendance	-0.071	*	0.040	0.008	*	0.007	-0.125	*	0.086	-0.010	*	0.006
Household Income	-212,659	**	68,730	-10,661	*	8,160	-63,912		97,194	-5,836		13,009
Earnings	-90,015	*	46,817	-9,511	**	4,673	-46,151		72,796	16,414	**	7,858
N. Observations	24,808			147,863			16,018			120,511		

A.13 Matching Estimates Obtained with the Sample of Unemployed Individuals at the Baseline

Population		Outcome	All Females in 2005				HH Females in 2005			
			Number of Observations	Number Treated	Diff-in-Diff ATT	z	Number of Observations	Number Treated	Diff-in-Diff ATT	z
Formal	**EPS=1 ∀ D**	LMP	718	171	**0.165**	2.88	184	50	0.202	1.91
		Unemployment	718	171	**0.075**	2.56	184	50	0.037	1.33
		Earnings of Household	718	171	-50,463	-0.88	184	50	29,755	0.35
		Earnings	718	171	40,085	1.46	184	50	45,467	0.94
		School Attendance	718	171	0.014	0.56	184	50	0.006	0.13
	D=1: Caja=1, D=0: EPS=1	LMP	889	343	**0.128**	2.75	240	106	**0.138**	1.58
		Unemployment	889	343	**0.073**	3.58	240	106	**0.056**	1.99
		Earnings of Household	889	343	-75,098	-1.36	240	106	-4,886	-0.07
		Earnings	889	343	-165	-0.01	240	106	-14,057	-0.44
		School Attendance	889	343	0.011	0.52	240	106	0.066	1.36
Informal	**EPS=0 ∀ D**	LMP	7,112	215	-0.059	-1.42	1,418	71	**-0.151**	-1.96
		Unemployment	7,112	215	0.026	1.28	1,418	71	0.035	1.04
		Earnings of Household	7,112	215	-37,680	-1.03	1,418	71	-69,087	-1.33
		Earnings	7,112	215	**-38,135**	-2.42	1,418	71	**-67,096**	-2.82
		School Attendance	7,112	215	0.015	0.92	1,418	71	-0.005	-0.19

Population		Outcome	All Males in 2007				HH Males in 2007			
			Number of Observations	Number Treated	Diff-in-Diff ATT	z	Number of Observations	Number Treated	Diff-in-Diff ATT	z
Formal	**EPS=1 ∀ D**	LMP	1,385	194	0.013	0.32	851	93	0.003	0.06
		Unemployment	1,385	194	0.019	0.64	851	93	0.065	1.63
		Earnings of Household	1,385	194	-33,112	-0.67	851	93	70,659	1.33
		Earnings	1,385	194	**-74,041**	-2.62	851	93	-43,631	-1.20
		School Attendance	1,385	194	**0.043**	3.25	851	93	-0.004	-0.35
	D=1: Caja=1, D=0: EPS=1	LMP	1,456	265	0.001	0.01	892	134	-0.027	-0.59
		Unemployment	1,456	265	0.017	0.65	892	134	0.043	1.38
		Earnings of Household	1,456	265	-37,090	-0.84	892	134	59,055	1.3
		Earnings	1,456	265	**-77,965**	-3.26	892	134	-38,851	-1.25
		School Attendance	1,456	265	**0.032**	2.34	892	134	-0.006	-0.45
Informal	**EPS=0 ∀ D**	LMP	14,751	94	-0.042	-0.81	4,091	53	-0.106	-1.42
		Unemployment	14,751	94	-0.035	-0.88	4,091	53	-0.036	-0.78
		Earnings of Household	14,751	94			4,091	53	12,084	0.2
		Earnings	14,751	94			4,091	53	-15,615	-0.37
		School Attendance	14,751	94			4,091	53	-0.013	-0.81

Source: Authors calculations using 2005 and 2009 Sisben Surveys, and Comfama and Comfenalco information for beneficiaries.