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Essays in labor economics, with applications to Ecuador

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Dedication

To my beloved husband Diego, to my children Dieguito y Daniela and to my beloved parents Guido y Mercy.

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Illustration

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Quito, October 2017

Andrea Isabel Molina Vera

Abstract

The chapters in this thesis investigate different types of "interventions" that affect Ecuador's labor market and try to answer the following questions: Does fertility reduce female labor supply? Does a social security reform that extends coverage to workers' children increase formal employment? How can a positive and a negative non-labor income shock affect employment and marital status of individuals five years later? Even though these essays can be read separately they share some common aspects. First, these investigations are centered on market labor results. Second, they try to identify heterogeneous results considering different groups of the population. Third, they all focus on identifying causality using different empirical techniques. Finally, each essay uses Ecuador as a case study.

Analyzing the labor market of a country like Ecuador with the many peculiarities that exist in Latin America like high rates of informal employment and high turnover in registered employment, is important to identify what policies, could affect the labor market (like cash transfers). It is also useful to identify what policies have been successful in formalizing employment (social security reforms), particularly in population groups characterized by high informality like independent workers.

The first chapter of this dissertation investigates the effect of fertility on female labor participation in Ecuador. It uses, as source of exogenous variation in family size, parental preferences for a mixed sibling-sex composition on their children (Angrist and Evans 1998). The empirical application shows that women with two boys or two girls are 4 percentage points more likely to have a third child than women with one boy and one girl. 2SLS estimations show that a third child causes a negative impact of 8 to 9 percentage points on female labor supply.

The second chapter examines the impact on the share of formal employment in Ecuador caused by a health insurance extension to formal workers' children. The empirical analysis uses a difference-in-differences approach and repeated cross section of household surveys obtained from the Ecuadorian National Survey of Employment and Unemployment from 2005 through 2013.

I find evidence that after the reform, workers with children were more likely to become formal employees than childless workers. The impact is about 2 percentage points for three subsamples of workers (paid, independent and salaried) corresponding to an increase of 6% for paid workers, of 21% for independent workers and of 4% for salaried workers. This investigation is one of the few studies in the literature that finds a positive result on labor formalization and finds a robust impact on independent workers.

The third chapter evaluates the effects of a change in unearned income on labor supply of poor families. I exploit a change in the construction of the eligibility score for a cash transfer program in Ecuador and apply the regression discontinuity design. The analysis uses the database of the social register (three rounds of the database 2003, 2009 and 2014).

The main results are that five years later: First, women who lost the cash transfer, and had been receiving it for six years, are equally likely to have paid work than women who continued to receive the transfer. Except for those who were not married at baseline, where the impact is negative (around 4 percentage points). Second, in the case of positive income shock, five years later there is not impact on paid work for those women who began receiving the transfer in 2009 in comparison to those who never received it. Third, as part of the adjustment of these households it is found that, five years later, those women who lost the cash transfer are more likely to be married in 2014 than those who continued to receive the transfer (2 percentage points) and the women are “winners” are less likely to be married in 2014 compared to those who have never received this income (4 percentage points). This results link outcome in the labor market with variables of family composition.

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Quito-Ecuador

Introduction

This thesis is composed of three essays about different types of "interventions" that affect Ecuador's labor market and try to answer the following questions: Does fertility reduce female labor supply? Does a social security reform that extends coverage to workers' children increase formal employment? How can a positive and a negative non-labor income shock affect employment and marital status of individuals five years later? Even though these essays can be read separately they share some common aspects. First, these investigations are centered on market labor results. Second, they try to identify heterogeneous results considering different groups of the population. Third, they all focus on identifying causality using different empirical techniques. Finally, each essay uses Ecuador as a case study.

The first chapter I exploit Angrist and Evans' identification strategy. This instrument arises from the fact that some parents prefer a mixed sibling sex composition. Among parents who have at least two children, those with two boys or two girls are much more likely to go on to have a third child. Because child sex is virtually randomly assigned, a dummy for same sex sibling pairs provides a plausible instrument for further childbearing among women with at least two children. The same-sex instrument can be decomposed into two components: Two boys and Two girls. So that these variables are other possible instruments.

The empirical application shows that women with two boys or two girls are 4 percentage points more likely to have a third child than women with one boy and one girl. 2SLS estimations show that a third child causes a negative impact of 8 to 9 percentage points on female labor supply.

Analyzing the labor market of a country like Ecuador with the many peculiarities that exist in Latin America like high rates of informal employment and high turnover in registered employment, is important to identify what policies have been successful in formalizing employment (social security reforms), particularly in population groups characterized by high informality like independent workers. In October 2010, the Ecuadorian government extended its social security healthcare coverage to formal workers' children of ages from 6 to 18 years old. The objective was to increase interest

in formal employment and encourage more workers to switch into formality. This second chapter examines the impact of this reform on the share of workers in formal employment, using a difference-in-differences approach and repeated cross sections of household surveys, as well as a panel of individuals built with administrative databases of a sub-sample of the Ecuadorian population.

The second chapter presents evidence that after the reform, workers with children were more likely to become formal employees than childless workers. The impact is about 2 percentage points for three subsamples of workers (paid, independent and salaried) corresponding to an increase of 6% for paid workers, of 21% for independent workers and of 4% for salaried workers. This investigation is one of the few studies in the literature that finds a positive result on labor formalization and finds a robust impact on independent workers.

Finally, the last chapter evaluates the effect of a change in the eligibility criteria for the cash transfer program. In 2009 the Ecuadorian cash transfers program, aimed at forty percent poorest households, underwent a change in the construction of its eligibility score. This change determined different groups of eligible people: among those who used to receive the transfer prior to 2009, some continued to receive it while others stopped doing so (negative income shock); and among those who were not receiving the transfer, some began to receive it while others continued without it (positive income shock). This change constitutes an exogenous shock to the income of these households and, through an analysis of regression discontinuity, it is possible to evaluate the impact of this shock in income on labor market variables, 5 years after the change.

The main results are that five years later: First, women who lost the cash transfer, and had been receiving it for six years, are equally likely to have paid work than women who continued to receive the transfer. Except for those who were not married at baseline, where the impact is negative (around 4 percentage points). Second, in the case of positive income shock, five years later there is not impact on paid work for those women who began receiving the transfer in 2009 in comparison to those who never received it. Third, as part of the adjustment of these households it is found that, five years later, those women who lost the cash transfer are more likely to be married in 2014 than those who continued to receive the transfer (2 percentage points) and the

women are “winners” are less likely to be married in 2014 compared to those who have never received this income (4 percentage points). This results link outcome in the labor market with variables of family composition.

Chapter 1
Impact of Fertility on Female Labor Supply

1.1. Introduction

For the U.S., Angrist and Evans (1998) have found that fertility has a negative impact of about 12 percentage points on female labor supply. In Ecuador, as in the U.S., there seems to be a negative relationship between fertility and female labor supply. This gives rise to the question: can this result be extrapolated to Latin American developing countries such as in Ecuador?

The differences of labor markets and family contexts between U.S. and Ecuador motivate this question. Also, it is important to consider some differences that developing countries have compared to developed ones: high levels of underemployment and informal economy; high heterogeneity in work arrangements (Caceres-Delpiano 2012); higher fertility; lower levels of female education; fewer facilities for formal childcare (Cruces and Galiani 2005).

Also, in empirical works, it is widely known the problem of endogeneity between fertility and labor market outcomes. Angrist and Evans (1998) mention that this problem arises since there are strong theoretical reasons to believe that fertility and labor supply are jointly determined. Agüero and Marks (2008) mention the possibility of endogeneity for omitted variable bias which influences fertility and labor force participation simultaneously (e.g. ambition or talent). Thus, several studies have exploited exogenous changes in family size to identify the causal relationship between the number of children and female labor supply. Some examples include twins at first birth ((Rosenzweig and Wolpin 1980); (Bronars and Grogger 1994)) and sex of the two first children (Angrist and Evans 1998). Most of these approaches find a smaller but still significant effect of children on female labor supply.

In this paper, I exploit Angrist and Evans' identification strategy. This instrument arises from the fact that some parents prefer a mixed sibling sex composition. Among parents who have at least two children, those with two boys or two girls are much more likely to go on to have a third child. Because child sex is virtually randomly assigned, a dummy for same sex sibling pairs provides a plausible instrument for further childbearing

among women with at least two children. The same-sex instrument can be decomposed into two components: Two boys and Two girls. So that these variables are other possible instruments.

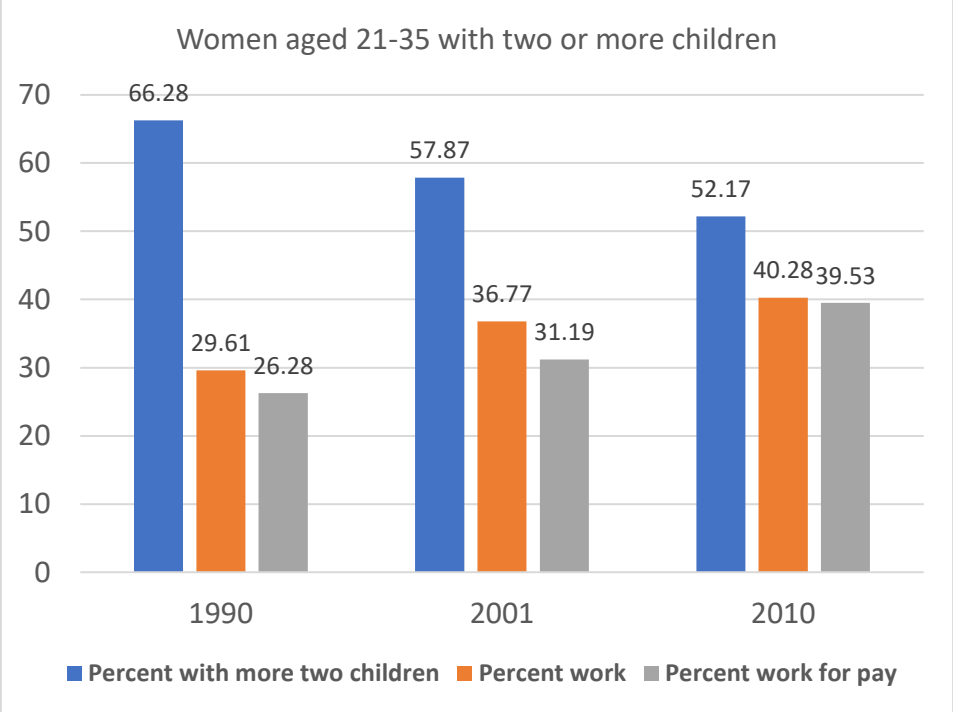
In Latin America, there are few studies which address the endogeneity of fertility decisions. Among those studies are Cruces and Galiani (2005), Agüero and Marks (2008) and Cáceres-Delpiano (2012). Cruces and Galiani (2005) study the effect of fertility on maternal labor supply in Argentina and Mexico, exploiting the source of exogenous variability in family size introduced by Angrist and Evans (1998) for the U.S., finding that the U.S. results can be generalized both qualitatively and quantitatively to Argentina and Mexico. Agüero and Marks (2008), using a subsample of Latin American countries and introducing female infertility as a source of variation in family size, do not find a significant relationship between fertility and mothers' employment. Finally, Cáceres Delpiano (2012) studies the impact of fertility on mothers' employment for a sample of developing countries using fertility shock (multiple births) and finding that children have a negative impact on female employment but with different impacts depending on the order that child of different sex are born.

All these results seem contradictory because, while Cruces and Galiani (2005), and Cáceres-Delpiano (2012) show that children have a negative impact on female employment, Agüero and Marks (2008) do not find a significant impact. One possible reason for this is that the local result depends on the instrument used and the compliers are different for the three researches.

The present study highlights two fundamental aspects. The first one is the definition of women's employment. Cáceres-Delpiano (2012) underlines this issue since in developing countries labor markets have higher levels of informality and heterogeneous payment alternatives. I contribute to this discussion by presenting the impact of fertility on the following definitions of employment: a) overall work (paid or unpaid), b) paid work (inside or outside the home), c) work outside the home (paid or unpaid) and d) work outside the home and paid. These definitions pretend to capture location and compensation issues. The second aspect highlighted here is the exploration of the external validity of results (Angrist 2004).

Using data from the Ecuadorian Population Census in the period 1990-2001-2010, Figure 1.1. shows fertility (measured as percentage of women with more than two children) and female labor supply move in opposite directions. Within the group of women aged from 21 to 35 years old and with two or more children, labor supply has increased by 13 percentage points (i.e. variation of 50% in work for pay) while the percentage of women with more than two children decreased by 14 percentage points (variation of 21%), all from 1990 to 2010. One might wonder if the decline in fertility is the cause of the sharp increase in female labor force.

Figure 1. 1. Ecuadorian Fertility and Female Labor Supply. Ecuadorian Population Census 1990,2001,2010



Thus, I find that using Angrist and Evans' instrument, IV estimations are significant and negative for married women in all employment definitions I use (between 8-9 percentage points). Meanwhile, for all women the significance of the effect on labor supply depends on the definition of employment: negative impact with "work outside the home" and with definition of "paid work outside the home" (8 percentage points). These results are confirmed using boys and girls as instrument and using the number of children as a fertility measure.

The paper has the following structure: Section 2 presents the identification strategy. Section 3 presents the data, some summary statistics and the construction of the instrumental variable. Section 4 presents the main effects of fertility on labor supply. Section 5 discusses the exclusion restriction related to the instruments used in this study. Finally, section 6 presents some conclusions.

1.2. Identification Strategy

As Rosenzweig and Wolpin (2000) indicate impact of fertility on the labor participation of married women was investigated first in the United States. Taking into account that fertility and participation on labor force are endogenous, some identification strategies have been to use twins on the first birth ((Rosenzweig and Wolpin 1980); (Bronars and Grogger 1994)) and the Angrist and Evans' natural instrument (1998) of using the sex of the first two births, specifically sex-sameness. Some recent strategies involve infertility shocks (Aguero and Marks 2008) or exploit the multiple births in higher parities (Caceres-Delpiano 2012) or research about a particular sample (Cristia 2008).

This paper employs the Angrist and Evans' instrument (1998) which exploits the parental preferences for a mixed sibling-sex composition that can be thought as randomly assigned. Then, a dummy variable that indicates whether the sex of the second child matches the sex of the first one provides a plausible instrument for further childbearing among women with at least two children (Angrist and Evans 1998, 451).

This instrument must meet the four conditions to be valid (heterogeneous effects framework): [1]

- The independence assumption should hold. That is: the instrument should be independent of potential outcomes, e.g. as good as randomly assigned.
- It must be relevant. I must find that parents who their first two children have the same sex are more likely to have an additional child compared to those who their first two children have different sex (with the sex-sameness instrument).

[1] In relation to the instrumental variables approach to revise: (Angrist and Pischke 2009), (Angrist 2004), (Bound, Jaeger and Baker 1995), (Imbens and Angrist 1994), (Angrist, Imbens and Rubin 1996).

- It must satisfy the exclusion restriction. To have children of the same sex in the first two births does not directly affect the subsequent labor supply of either parent except through its effect on having an additional birth.
- It must satisfy monotonicity assumption. This means that all those affected by the instrument are affected in the same way.

With this in mind, I propose the following regression models:

$$Y_i = \alpha + \mathrm{w}'\delta + \beta x_i + \varepsilon_i \qquad (1)$$

Where: Y_i is a measure of labor supply, w' is a covariates vector: age of women, age at first birth, indicators for the sex of first and second child, dummies by indigenous and urban area, x_i is the endogenous fertility measure of interest. In this case is third child variable or number of children variable. It is replaced in (1) by the predicted value of the following regression to obtain 2SLS estimates. Where $Samesex$ is a dummy for whether the sex of the second child matches the sex of the first child.[2]

$$x_i = \rho + \mathrm{w}'\lambda + \gamma(Samesex) + \eta_i \qquad (2)$$

1.3. Data, Summary Statistics and Variables

This research uses data on labor supply, fertility and characteristics of household members from the Ecuadorian Population Census of 2010 conducted by the National Institute of Statistics and Census (Instituto Nacional de Estadísticas y Censos INEC). Table 1.1. presents some labor force participation rates and probabilities of additional children for women of different ages and different marital status[3] for 1990, 2001 and 2010 Census.

For the sex-mix instrument, the sample is limited to women between 21 and 35 years old with at least two children and whose oldest child was, at most, 18 years old at the moment of the census.[4] Following Angrist and Evans (1998), to match women with

[2] Some references of theoretical models are found in (Browning 1992) y (Gronau 1973).
[3] It includes married women and "common law marriage".
[4] As the census does not track children across households so the simple is limited to mothers aged 21-35 whose oldest child was less than 18 years of age at the moment of the census (similarly it was developed by Angrist and Evans).

their own children, I use the sample of females who are "heads" or "spouses" in each household, then I check that the reported number of children alive coincides with the number of children in the household matched to the women, restraining the sample to women for whom both numbers were the same.

Table 1. 1. Fertility and Labor Supply Measures in Ecuador. Census 1990,2001, 2010

	All women			Indigenous women	
Fertility and Labor-Supply Measures	1990	2001	2010	2001	2010
Women aged 21-35					
Mean children ever born	2.8	2.45	2.17	3.29	2.66
Percent with 2 or more children	74.1	68.61	65.12	79.04	71.67
Percent work	32.9	40.77	44.91	59.75	59.57
Percent work for pay	29.6	35.1	44.17	46.10	58.18
Percent work outside the home			37.93		38.23
Percent paid employment outside the home			37.55		37.56
Observations	744,321	898,771	1,072,870	59,025	80,831
Women aged 36-50					
Mean children ever born	5.19	4.08	3.38	6.09	5.10
Percent with 2 or more children	90.28	87.85	85.79	91.53	89.96
Percent work	36.07	45.71	52.15	60.34	62.21
Percent work for pay	32.38	39.77	51.43	45.35	60.70
Percent work outside the home			42.21		36.16
Percent paid employment outside the home			41.89		35.47
Observations	580,660	832,587	1,052,847	49,646	62,350
Women aged 21-35 with 2 or more children					
Mean children ever born	3.55	3.13	2.94	3.85	3.42
Percent with more 2 children	66.28	57.87	52.17	73.67	63.17
Percent work	29.61	36.77	40.28	58.68	57.90
Percent work for pay	26.28	31.19	39.53	44.83	56.46
Percent work outside the home			32.64		35.34
Percent paid employment outside the home			32.28		34.66
Observations	551,520	616,683	698,674	46,656	57,935
Married women aged 21-35 with 2 or more children (married and common law marriage)					
Mean children ever born	3.55	3.13	2.93	3.87	3.45
Percent with more 2 children	66.42	57.92	52.14	74.11	63.98
Percent work	27.84	34.47	36.85	57.85	56.64
Percent work for pay	24.5	28.97	36.15	43.72	55.19
Percent work outside the home			29.22		33.73
Percent paid employment outside the home			28.9		33.05
Observations	520,635	568,594	617,362	44,096	53,587

Note: 1990 and 2001 census didn't ask about work inside or outside from home. The married samples include women married or "common law marriage" at the time of the Census. The Census 1990 don't have ethnic identification.

This is performed for two samples: all women and those married at the time of the census 455,125 observations fort the first one and 404,795 observations for the last. Table 1.2. shows some descriptive statistics and variable definitions for covariates, instruments and dependent variables.

Table 1. 2. Descriptive Statistics: Women aged 21-35 with 2 or more children. Census 2010

	Women		Indigenous	
Variables	All	Married	All	Married
Children ever born	2.731	2.738	3.138	3.163
	[0.965]	[0.973]	[1.294]	[1.307]
More than 2 children	0.481	0.484	0.597	0.606
	[0.5]	[0.5]	[0.490]	[0.489]
Boy first	0.512	0.513	0.507	0.508
(=1 if first child was a boy)	[0.5]	[0.5]	[0.5]	[0.5]
Boy second	0.509	0.509	0.507	0.508
(=1 if second child was a boy)	[0.5]	[0.5]	[0.5]	[0.5]
Two boys	0.263	0.263	0.258	0.258
(=1 if first two children were boys)	[0.44]	[0.44]	[0.438]	[0.438]
Two girls	0.242	0.241	0.244	0.242
(=1 if first two children were the same sex)	[0.429]	[0.428]	[0.429]	[0.428]
Same sex	0.505	0.505	0.501	0.501
(=1 if first two children were the same sex)	[0.5]	[0.5]	[0.5]	[0.5]
Age	29.477	29.42	28.833	28.790
	[3.785]	[3.8]	[3.827]	[3.831]
Age at first birth	19.327	19.384	18.993	19.008
	[3.08]	[3.101]	[2.726]	[2.719]
Schooling	8.855	8.85	5.843	5.797
	(4.36)	[4.37]	[3.724]	[3.696]
Work	0.411	0.378	0.585	0.573
(=1 if worked at least an hour in last week to census)	[0.492]	[0.485]	[0.493]	[0.495]
Worked for pay	0.403	0.371	0.572	0.559
(=1 if worked for pay in last week to census)	[0.491]	[0.458]	[0.495]	[0.496]
Worked outside the home	0.333	0.300	0.365	0.349
(=1 if worked outside the home)	[0.471]	[0.456]	[0.482]	[0.477]
Paid employment outside the home	0.329	0.297	0.359	0.343
(=1 if worked for pay and outside the home)	[0.47]	[0.457]	[0.48]	[0.475]
Number of observations	455,125	404,795	36,807	34,217

Notes: The samples include women aged 21-35 with two or more children except for women whose second child is less than a year old. Standard deviations in brackets.

The variables used as measures of fertility are the indicator of more than two children and the number of children. The first instrumental variable for fertility is the indicator Same sex. Other possible instruments are Two boys and Two girls.

The labor supply variables are:

- Work: equal 1 when a mother worked at least one-hour last week. This includes those mothers who:
    - Worked at least one hour prior to the census week, did not work but had a work which was absent, created a product or provided a service for one hour at least, helped in some business or work in the family for one hour at least, performed agriculture for one hour at least.
    - Worked for pay: equal to 1 when the mother worked for pay and is not a family worker without remuneration. This includes: employees (private or public), self-employed. owners and managers (employers, partner), laborers and domestic servants.
- Worked away from home: and equals 1 when the mother's job is out of home.
- Worked for pay outside the home: equals 1 when the mother´s job is paid and outside the home.

The dimensions that can be explored with these definitions are: location of job in relation to home, remuneration and labor participation.

1.4. Main Results

Following Angrist and Evans, Table 1.3. shows estimates of the impact of child sex and sex-mix on fertility, where 49% of all women have one girl and 51% have one boy at the first birth. The fraction of women with at least one child and who had a second child, conditional on the sex of the first child is 67% in both cases. This presents evidence there is no impact of the sex of the firstborn on fertility.

Table 1. 3. Fraction of families that had another child

	All women		Married Women	
	Sex of first child in women with one or more children			
	Fraction of sample	Fraction that had another child	Fraction of sample	Fraction that had another child
(a) one girl	0.49	0.6752	0.49	0.6860
		[0.0008]		[0.00086]
(b) one boy	0.51	0.6754	0.51	0.6870
		[0.00079]		[0.00084]
difference (b)-(a)		0.0002		00098
		[0.001141]		[0.0012]
	Sex of first two children in women with two or more children			
	Fraction of sample	Fraction that had another child	Fraction of sample	Fraction that had another child
two girls	0.242	0.503	0.242	0.507
		[0.0015]		[0.0015]
two boys	0.262	0.496	0.263	0.498
		[0.0014]		[0.0015]
(a) one boy, one girl	0.495	0.463	0.495	0.4657
		[0.00105]		[0.0011]
(b) both same sex	0.505	0.499	0.505	0.503
		[0.00104]		[0.0011]
difference (b)-(a)		0.036		0.037
		[0.0015]		[0.0015]

Notes: The samples are the same as in Table 1.2. Standard errors are reported in brackets.

For the second analysis, Table 1.3 presents the fraction of women who have a third child conditional on the sex composition of the first two children, where 46.3% of women with one boy and one girl have a third child, compared to 49.9% for women with two girls or two boys. That is a significant difference of 3.6 percentage points.

These results are confirmed in Table 1.4. which shows the first stage of the instrument for all and for married women, including controls, and results for the other possible instrument (two boys and two girls). The difference of 3.6 and 3.8 percentage points for all and for married women found here means that Ecuadorian women with two children of the same sex are 3.6 percentage points more likely to have a third child than mothers of one boy and one girl (3.8 percentage points for married women). For the United States in 1980 this difference was 6 percentage points for all women (Angrist and Evans). Cruces and Galiani found a difference of 3.5 and 3.2 percentage points (subsample of all women) for Argentina and Mexico, respectively. The instrument of

two boys and two girls is also significant for explaining fertility, with a weak preference for boys.[5]

Table 1. 4. First Stage same sex instrument. Fertility measure is third child variable

	All Women		Married Women	
	(1)	(2)	(3)	(4)
same_sex	0.0367		0.0384	
	[0.0014]***		[0.0014]***	
two_boys		0.0320		0.0337
		[0.0019]***		[0.0020]***
two_girls		0.0414		0.0432
		[0.0020]***		[0.0021]***
R2	0.14	0.14	0.15	0.15
N	455,125	455,125	404,795	404,795

* p<0.1; ** p<0.05; *** p<0.01 Robust standard errors in brackets. Covariates: age of women, age at first birth, indicators for Boy 1st, Boy 2nd and dummies by indigenous and urban status. The variable Boy 2nd is excluded from columns (2) and (4)

To check a random assignment of Same-sex instrument, Table 1.5. compares demographic characteristics of mothers among those who had a composition of same-sex (treated group) and mixed-sex (control) sibling compositions. This table includes the following variables: age of woman, age at first birth, indigenous ethnicity, years of education, residence area. None of these variables presents a significant difference (independence assumption).

Table 1. 5. Random assignment of the same sex

	By Same Sex				
Variable	Controls	sd	Treated	Sd	p-value
Age	29.482	[3.785]	29.471	[3.788]	(0.306)
Age at first birth	19.321	[3.079]	19.321	[3.080]	(0.917)
Indigenous	0.081	[0.274]	0.08	[0.272]	(0.166)
Urban area	0.608	[0.488]	0.609	[0.488]	(0.623)
Years of education	8.853	[4.365]	8.857	[4.358]	(0.752)
N	225,153		229,972		

The sample is all women. Standard deviations are reported in brackets.

[5] Coefficients for two boys and two girls are significantly different. Only for indigenous families there is a strict son preference because coefficient for Two boys is not significantly different from zero (Leung 1991).

For 2SLS estimates control for the following variables: age of women, age at first birth, a dummy variable to indicate the sex of first and second children, a dummy variable for ethnic identification and a dummy for urban area. For the cases of two boys and two girls as instrument, the covariates exclude the sex of the second children. Table 1.6. shows that OLS estimates present a negative and significant impact for all measures of work and two groups of women. The 2SLS estimates indicate there is an impact on labor supply when moving from 2-3 children for sample "all women" in two work measures (working outside from home and paid work outside home) with an impact of 8 percentage points. Finally, for married women there is negative impact of 8 to 9 percentage points in all work measures.

Table 1. 6. OLS and 2SLS. Estimates of Labor-Supply Models Using 2010 Census Data

	All Women			Married Women		
	OLS	2SLS (1)	2SLS (2)	OLS	2SLS (1)	2SLS (2)
Instrument for More than 2 children		Same sex	Twoboys, Twogirls		Same sex	Twoboys, Twogirls
Dependent Variable						
Work	-0.1120	-0.0563	-0.0499	-0.1078	-0.0794	-0.0756
	[0.0015]***	[0.0391]	[0.0389]	[0.0016]***	[0.0389]**	[0.0387]*
			(0.1185)			(0.3108)
Work for pay	-0.1118	-0.0589	-0.0521	-0.1076	-0.0831	-0.0787
	[0.0015]***	[0.0390]	[0.0388]	[0.0016]***	[0.0388]**	[0.0386]**
			(0.0932)			(0.2440)
Work outside home	-0.1084	-0.0842	-0.0792	-0.1036	-0.0930	-0.0906
	[0.0015]***	[0.0376]**	[0.0374]**	[0.0015]***	[0.0370]**	[0.0368]**
			(0.2183)			(0.5192)
Paid Work outside the home	-0.1080	-0.0888	-0.0836	-0.1031	-0.0984	-0.0956
	[0.0015]***	[0.0374]**	[0.0372]**	[0.0015]***	[0.0368]***	[0.0366]***
			(0.1850)			(0.4473)

* p<0.1; ** p<0.05; *** p<0.01. Robust standard errors in brackets. Covariates: age of women, age at first birth, indicators for Boy 1st, Boy 2nd and dummies by indigenous and urban area. In (2) the variable Boy 2nd is excluded. The p-value for the test of overidentifying (Sargan test) restriction shown in parentheses.

It is important to mention that work measures are relevant in the sample of all women since there is an impact only when the definition of work involves characteristics incompatible with child rearing. On the other hand, for married women there is an impact using all measures of work. Perhaps this happens since married women are supported with their spouses' revenues.

Also, the OLS estimates and 2SLS are similar for married women but for all women depend on the definition of work. The results are confirmed by the two boys and two girls' instrument.

These results are local for the compliers, that is, for women whose fertility decision is changed for the instrument, which is used for identifying the impact of labor supply when children move from 2 to 3, but does not identify other increases in fertility as 0 to 1 child.

1.4.1. Results: Indigenous Population

The indigenous population is a group of particular interest because this group presents higher fertility (in different measures) than others, low schooling and their work is focused on self-employment and unpaid work in the agriculture (in rural areas).

Table 1. 7. First Stage: Indigenous Women

	Indigenous Women			Married Indigenous Women		
	(1)	(2)	(3)	(4)	(5)	(6)
same_sex	0.0215			0.0239		
	[0.0047]***			[0.0049]***		
two_boys		0.0063			0.0083	
		[0.0067]			[0.0069]	
two_girls		0.0367	0.0367		0.0394	0.0394
		[0.0067]***	[0.0067]***		[0.0069]***	[0.0069]***
R2	0.14	0.14	0.14	0.15	0.15	0.15
N	36,807	36,807	36,807	34,217	34,217	34,217

* p<0.1; ** p<0.05; *** p<0.01 Robust standard errors in brackets. Covariates: age of women, age at first birth, indicators for Boy 1st, Boy 2nd and dummies by indigenous and urban status. The variable Boy 2nd is excluded from columns (2) and (5) . Specification (3) and (6) only considering "twogirls" variable as instrument.

The first stage shows that families with two boys or two girls are 2 percentage points more likely to have a third child than families with one boy and one girl (Table 1.7.). And when it considers two boys and two girls instruments the indigenous women with two boys or two girls are 0.6 (not significant) and 3.6 percentage points (significant) more likely to have a third child than families with one boy and one girl. These results suggest the indigenous families have a strict preference for boys. However, 2SLS

results for indigenous women are too imprecise to infer the causal effect of a third child on their labor supply (Table 1.8.).

Table 1. 8. OLS and 2SLS. Estimates of Labor-Supply Models Using 2010 Census Data: Indigenous Women

	All Women				Married Women			
	OLS	2SLS (1)	2SLS (2)	2SLS (3)	OLS	2SLS (1)	2SLS (2)	2SLS (3)
Instrument for More than 2 children		Same sex	Twoboys, Twogirls	Twogirls		Same sex	Twoboys, Twogirls	Twogirls
Dependent Variable								
Work	-0.0781	-0.1439	0.1691	0.2407	-0.0701	-0.1869	0.0876	0.1764
	[0.0057]***	[0.2576]	[0.2149]	[0.2213]	[0.0059]***	[0.2430]	[0.2051]	[0.2125]
			(0.0405)				(0.0380)	
Work for pay	-0.0788	-0.1233	0.1752	0.2434	-0.0706	-0.1496	0.103	0.1847
	[0.0057]***	[0.2583]	[0.2160]	[0.2223]	[0.0060]***	[0.2431]	[0.2062]	[0.2136]
			(0.0521)				(0.0577)	
Work outside	-0.0883	-0.4879	-0.2835	-0.2359	-0.0789	-0.4487	-0.2884	-0.237
home	[0.0055]***	[0.2649]*	[0.2053]	[0.2066]	[0.0057]***	[0.2443]*	[0.1972]	[0.1998]
			(0.1596)				(0.2084)	
Paid Work outside	-0.0888	-0.4563	-0.2711	-0.2288	-0.0792	-0.3995	-0.2711	-0.2296
the home	[0.0055]***	[0.2607]*	[0.2034]	[0.2048]	[0.0057]***	[0.2385]*	[0.1949]	[0.1979]
			(0.1998)				(0.3059)	

* p<0.1; ** p<0.05; *** p<0.01. Robust standard errors in brackets. Covariates: age of women, age at first birth, indicators for Boy 1st, Boy 2nd and dummies by indigenous and urban status. In (2) and (3) the variable Boy 2nd is excluded. The p-value for the test of overidentifying restriction shown in parentheses (Sargan test).

1.5. Exclusion restriction

One concern in relation to the instrument applied in this paper is raised by Rosenzweig y Wolpin (2000), who say that the same sex instrument can affect labor supply through economies of scale, and thereby reducing the cost of childcare. To evaluate this, I used data from the survey of income and expenses and I found that expenses that may involve some form of economies of scale are not different between households with two children of the same sex and households with two children of different sexes (Table 1.9.). Except for the monthly expenditure on education where the difference is statistically significant, but the sign contradicts the hypothesis of economies of scale,

since households with two children of the same sex spend more than households with two children of different sexes.[6]

Table 1. 9. Exclusion restriction

Expenditure	Share (%)	First two children Differences in means spending		
		Same-sex	Two Boys	Two Girls
Clothing and shoes	7.11	2.799	1.4388	2.4239
		[2.247]	[2.6166]	[2.6553]
Clothing	4.88	1.498	0.5381	1.5316
		[1.679]	[1.9596]	[1.9797]
Shoes	2.36	1.2614	1.1677	0.5537
		[0.7254]	[0.8430]	[0.8574]
Education	5.8	14.4895	6.3482	13.0247
		[6.5087]**	[7.6129]	[7.4987]*
Food and beverages	24.54	4.1655	6.3842	-0.7659
		[3.3524]	[3.8999]	[3.9555]
Health	4.68	-1.2670	-0.3623	-1.4072
		[2.5704]	[2.9796]	[3.0533]

National Household Survey of Income and Expenditure 2011-2012 INEC. The survey has 5,847 households with two or more children under 18 (these observations are considered in the "share" column). Differences in means spending considered only households with two children (3,154 observations). The difference in means is constructed as the mean of the population of interest (same sex, two boys, two girls) minus the mean of the rest of the population.

An additional concern that may invalidate the exclusion restriction is the existence of any instrument manipulation that is related to labor supply (the idea that boy children contribute relatively more than girl children to the parent's wellbeing) (Basu and Das Gupta 2001). In order to rule out this idea, I applied the ratio of boys to girls aged zero to four years old that for Ecuador shows that there are no forms of infant negligence that could result in higher girl mortality. On a national level this ratio is 1.07, and for the indigenous population the ratio is 1.05.[7] Also, expenditure patterns do not reveal any strong preference for boys that might indicate discrimination towards girls (Table 1.9). Except for the monthly expenditure on education, however the sign shows favoritism to girls, since households with two daughters spend more on education than households with two boys or with children different sex.

[6] Similar results were found for the indigenous population. However, this population has few observations for some items of expenditures.

[7] Results from the survey of living conditions 2014. Similar results were found with the Population Census 2010

1.6. Conclusions

OLS estimates indicate that women with more than two children are 10 percentage points less likely to work than women with two children.

To estimate the causal effect of fertility on female labor supply I use sex composition of first two children as instrumental variable. The first stage shows that families with two boys or two girls are 4 percentage points more likely to have a third child than families with one boy and one girl.

2SLS results show a causal impact of 8-9 percentage points of decrease of female labor supply (for "work outside home" and "paid work outside the home" definitions) by having a third child in all and married women respectively. Findings for Ecuador are very similar to findings for Argentina and Mexico.

Indigenous families with two boys or two girls are 0.6 (not significant) and 3.7 (significant) percentage points more likely to have a third child than families with one boy and one girl. These results suggest the indigenous have a preference for boys. However, 2SLS results for indigenous women are too imprecise to infer the causal effect of a third child in their labor supply.

Two aspects are important here: First, the results apply for the "compliers". This means that the result is local (Local Average Treatment Effect-LATE), that is, for women who changed their fertility decision due to the instrument. Second, these results refer for moving from 2 to 3 children but do not refer to other increases in fertility as in going from 0 to 1 child.

Finally, these results are interesting for the purposes of encouraging female labor force participation, among other motivations for aspects of gender equality and poverty reduction.

Chapter 2

The impact of a benefit extension for formal workers on the share of formal employment: Evidence from Ecuador

2.1. Introduction

In October 2010, the Ecuadorian government extended its social security healthcare coverage to formal workers' children of ages from 6 to 18 years old. The objective was to increase interest in formal employment and encourage more workers to switch into formality. This paper examines the impact of this reform on the share of workers in formal employment, using a difference-in-differences approach and repeated cross sections of household surveys, as well as a panel of individuals built with administrative databases of a sub-sample of the Ecuadorian population.

The study of formal employment is important for several reasons. First, according to the definition of formal/informal employment used in this paper, high informal employment rates are concerning since informal workers do not accumulate a pension.[1]

Second, a labor market with low formal employment rates does not only imply future problems with lack of pensions but also means that precarious jobs are being created in the present. The current research helps in the understanding of the effectiveness of certain policies at the moment of increasing labor formalization and suggests important reforms for social security systems. Finally, formality is important because it usually represents a higher contribution in taxes than informal employment.

While several papers have studied the impact of social insurance reforms on US employment ((Krueger and Meyer 2002), (Currie and Madrian 1999), (Madrian 2006))[2], only few studies analyze this issue in developing countries. Some recent research addresses the impact of health insurance for informal workers as an incentive to enter informality (Seguro Popular in Mexico) (Bosch, Cobacho y Pagés 2011) and

[1] In Latin America, on average, 4 of each 10 workers contribute to any pension system (Bosch, Melguizo y Pagés 2013).

[2] Other research between health insurance and the labor market are: (Chou y Staiger 2001), (Gruber and Hanratty 1995), (Gruber and Madrian 1995). Some conceptual aspects can be reviewed in: (Gruber 1998), (Royalty and Abraham 2006), (Field 2009), (Browning 1992).

(Bosch y Campos-Vázquez 2014) and the analysis of health reform in Uruguay that benefits children of salaried workers registered with social security (Bergolo and Cruces 2014).

The present study is developed following Bergolo and Cruces' investigation, whose analysis focuses on private salaried workers who benefited from this reform. Extending coverage was financed by an increase in the contribution of workers to the health insurance fund. An eligibility criterion was to have a minimum of 25 working hours per week. The main results of this research are that reform increased benefit-elegible formal employment by 1.6 percentage points and formal employment by 1.52 percentage points.

This study diverges from Bergolo and Cruces in the following ways: In this paper, the effect of a health reform not just for salaried workers, but also for independent workers was studied. Since the latter group presents one of the largest difficulties to switch into formality, and since there is no solid evidence about effective programs for these workers, the present investigation attempts to become an important contribution to the literature. Furthermore, the new health coverage does not imply further increases in social security contribution. Therefore, since there is no additional cost in the cost-benefit analysis of individuals, this paper captures a purer effect of the reform than the research by Bergolo and Cruces.

Additionally, by using administrative data from the social security system of a subsample of the Ecuadorian population (those who belong to the database used to determine eligible families for the transfer program), this study contributes to the growing empirical literature on labor markets and social protection systems studying the interaction between conditional transfer programs and the healthcare coverage reform.

This topic is highly relevant since some transfer programs may discourage formal employment.[3]

[3] Having a formal job can be a reason for not being eligible in some social transfer programs. In Ecuador, that is not the case for the time period considered in this study.

With the repeated cross section data, the main results show that the healthcare reform increases formal employment of paid workers by about 2 percentage points, which is equivalent to a 6% increase. Similar impacts are found for salaried workers (increase of 4%) and for independent workers (increase of 21%).

With the panel data, the main result shows that the positive impact of the reform comes from individuals who had been formally employed at least once in the pre-reform period (2 percentage points for all individuals and for eligible individuals for the transfer program), and that there is no impact on those who were not formally employed in the previous period.

The rest of this paper is organized as follows: Section 2 describes the health coverage reform. Section 3 presents the methodology and describes the data sources. Section 4 presents the main effects of the reform on employment formalization in repeated cross sections of the household surveys. Section 5 presents results with a panel of individuals and administrative databases. Section 6 presents the main conclusions of the investigation.

2.2. Ecuadorian reform of the social security health provision

In October 2010, the Ecuadorian National Assembly issued a change in the social security law, increasing the age of healthcare coverage of formal workers' children from age 6 to age 18. This reform took effect in February 2011.[4] Previously, the benefit only applied to children under 6 years of age.[5]

Prior to the 2010 reform, the health insurance only covered those registered in the social security system (workers who were in a dependency relationship in either the public or private sector and independent workers), their children up until the age of 6, and retired individuals. Those who were not registered in the system, those who were informally employed and children of those who were registered but were older than 6 years of age

[4] The reformatory law passed in 2010 established that the change in health coverage would start in February 2011. Thus, coverage for children up until 18 years of age became effective in February 2011 through Resolution CD 357.

[5] Coverage for children under 6 years of age was established in the Social Security Act of 2001-55, but became effective starting in June 2009 through Resolution CD 265.

had to use the public healthcare system instead or pay for healthcare in the private healthcare system.[6]

For the time period studied in this paper the employees contribute to the social security system with 9.35% of their monthly wages while employers contribute with 11.15% of employees' wages. That is, a total contribution of 20.5% of workers' wages is paid into the system, with the employer making a direct transfer each month.[7] These contributions finance some insurance funds that benefit registered employees and constitute an indivisible bundle of benefits to which individuals have no choice on whether or not to contribute. The items that are financed include retirement pensions, health insurance, work risk insurance, severance fund, contribution to peasant social security, and administrative expenses.

Health insurance was financed with 5.71% of workers' wages. This value was included in the contributions being made by employers. Until 2010, this health fund financed healthcare coverage for employees registered in the social security system and their children if they were under 6 years of age. The extension of health coverage for children from 6 to 18 years of age was also financed from the same health insurance fund. To receive the new benefit, neither the monthly contribution to the social security system nor the rate of contribution to the health insurance were changed, and the contribution to the healthcare fund was not different between workers with and without children.[8]

[6] Peasant's social security in rural areas already granted health insurance for children under the age of 18, which is why rural areas are excluded from the analysis.

[7] Public sector workers' contributions also amount to 20.5% of their monthly wages, but the employer/employee percentage of the contribution differs from that of the private sector.

[8] The extent of this coverage was backed up by financial sustainability reports from the Ecuadorian Institute of Social Security, which do not seem to have foreseen the increased formalization caused by the reform. To address the financing of the expanded coverage, Resolution CD 357 indicates that the Social Security Institute would review and adjust contributions, if necessary, in January 2012. This setting is not seen as an increase in contributions but rather as an adjustment in the distribution of fees between various types of benefits. However, no adjustment was made during the period of study for this research. The contribution rate for health insurance was changed only in 2016; however, the total contribution to the social security system (20.6 percent of total monthly wages) remains the same. References about contribution rates can be found in the October 2005 Resolution CD 081, in the May 2009 Resolution CD 261, and in the February 2011 Resolution CD 357.

The social security contributions made by those who are self-employed was 17.5% of wages[9] during the period of this analysis. Health insurance was financed by 5.71% of wages.[10]

Therefore, during the period of analysis for this article, paid workers did not suffer from changes made to the total rate of contribution to the social security system, or to the employee/employer contributions, or to the rate that finances the health insurance fund.

The health insurance package includes activities for health promotion, preventive medicine, diagnosis and treatment of illnesses, as well as recovery and rehabilitation. This means the package includes coverage for medical exams and diagnostic procedures, medical and surgical services, hospital coverage for patients, medicine supply, dental care, emergency room care and maternity care, among other services.[11] Health benefits are provided by the social security system's medical units as well as by other public or private medical units that, in agreement with the system, have been accredited to provide care on behalf of it. This is considered as a reference system regulated by the Ecuadorian Department of Social Security Health. The billing is done directly between the medical unit and the Social Security Institute (Instituto de Seguridad Social).[12]

This reform has some unique characteristics. First, it does not imply an additional cost for registered workers, as is the case with the Uruguayan reform studied by Bergolo and Cruces. Second, independent workers who are not formally employed can benefit from this reform through voluntary membership. This way, unlike the Uruguayan reform, which applied primarily to formal employees, the impact of the Ecuadorian reform can be studied for independent workers or freelancers as well. Third, the benefit is

[9] Minimum monthly wage rates are considered for these workers so their contribution to social security is based on the set unified basic monthly salary.

[10] Contributions made by independent workers finance the same expenses described above (health insurance, pension insurance, work risk insurance, contribution to the peasant's insurance system, and administrative costs, and since 2014 the contribution was increased to 20.6% of their income to include severance fund).

[11] Treatment for chronic and degenerative diseases is included as well as treatment for catastrophic diseases that are recognized by the State as public health problems. For more detail, see Article 103 of the social security Act.

[12] Only in cases of emergency are people given a choice on medical units providing healthcare that can include units that are not in agreement with the social security system so members can obtain reimbursement of expenses.

independent from the number of children that workers have, independent of whether one or both parents are registered within the social security system. Fourth, health coverage for children is provided regardless of their preexisting health conditions.

Finally, to be eligible for the extension of the health insurance benefit for children, the worker must have contributed to the social security system for at least three consecutive months.[13]

Prior to the 2010 reform, only 11% of the children from ages 6 to 18 were covered by any type of health insurance, which corresponded to private and municipal insurance, while there was no health coverage by social security. In 2011, the rate of coverage to this age group provided by the social security system increased to 5%, with 13% of this age group having other types of health insurance, leading to a total of 18% of children from ages 6 to 18 being covered by some type of health insurance. For the year 2012, health coverage by the social security system to this age group increased to 24% while another 13% continued to receive health coverage from outside of the system, leading to a total of 37% of children in this age group having some form of health coverage.[14]

In regards to the implementation of the health fund, the social security system receives USD 1.500 million annually in health contributions, which makes up only 30% of its total budget.[15] According to figures from the Institute of Social Security, expenses for coverage amounted to USD 2.100 million (USD 700 million coming from external medical units and USD 1.400 million coming from the system itself). This means that there is a USD 600 million deficit reported for this fund. However, the Institute of Social Security noted that only USD 200 million have been spent on healthcare for the children of members.[16]

[13] For cases of emergency, just one day of membership is required. Reference regarding waiting time is found in Resolution CD 357, February 2011.

[14] Computed with microdata from the Ecuadorian National Survey of Employment and Unemployment.
[15] December 2011 Resolution CD 402 relates to the approved budget for 2012.
[16] Interview with Richard Espinoza (highest authority of the Ecuadorian Institute of Social Security) by the newspaper El Universo, November 18, 2015.

This reform is part of a set of programs that aim to increase the attractiveness of social security affiliation. Thus, the same social security reform that expanded healthcare for children of members also allowed for the extension of health coverage and affiliation to members' spouses (by paying an additional fee that accounted for 3.4% of revenue). Other reforms in this period were: In 2009, labor inspectorates increased in companies; in 2010, the "Dignified Domestic Work" (Trabajo Doméstico Digno) campaign was implemented; in 2011, this campaign, renamed "Dignified Work" (Trabajo Digno), was extended to all employees through informative talks about the rights of affiliation. In 2011, the criminalization for non-affiliation to social security was approved through popular referendum. In February 2014, the National Assembly approved legislation to issue penalties and fines for employers who do not affiliate their workers to social security. The only reform that marks a difference in coverage between individuals with and without children is the one being evaluated in the present article.

2.3. Data and methodology

2.3.1. Repeated cross sections of household surveys

The empirical analysis of this article uses repeated cross sections of household surveys[17] obtained from the Ecuadorian National Survey of Employment and Unemployment (ENEMDU in Spanish) from 2005 through 2013, corresponding to December of each year. Since the law which promoted the health reform was passed in October 2010, this study considers the years from 2005 to 2009 as the pre-reform period, and 2010 to 2013 as the post-reform period.[18]

ENEMDU is a periodic cross-sectional household survey nationwide carried out by the Ecuadorian National Institute of Statistics and Census (INEC in Spanish). This survey is the main source of information about the Ecuadorian labor market, social security, and household characteristics.

Workers are considered formal if they have contributions to social security that allow them to get a retirement pension. In the case of salaried workers, employers also

[17] References to repeated cross section in (Angrist and Pischke 2009). an important reference for dif-in-dif for panel of individuals (Bertrand, Duflo and Mullainathan 2004).

[18] Data for December 2010 is considered part of the post-reform period. However, the main results do not change if this year is excluded from the analysis.

contribute in building up their retirement pensions. In ENEMDU survey, formality of salaried workers is identified by asking whether said workers are receiving social security benefits through their employer. Meanwhile, for independent workers (self-employed and employers), formality is identified by determining whether the workers is affiliated or covered by social security.

The sample is restricted to heads of household or spouses in order to get a proper identification of their children through a question of the relationship between each household member and the head of household. Also, the sample is limited to individuals between ages 19 to 64 from urban sectors. The rural areas are excluded from the analysis, since rural areas have an exclusive social security system (known as Seguro Campesino in Spanish) which already includes health coverage for children under 18 years old. Individuals with children under 6 years old are excluded from the analysis.[19]

In this paper, results are presented for the entire sample of paid workers and for two exhaustive (and mutually exclusive) subgroups: independent and salaried workers.

In order to estimate the effect of the health reform on employment formalization, the following difference-in-differences specification is used:

$$y_i = \alpha + \beta Children_{it} + \delta Children_{it} * Post_{it} + \theta_t + \lambda_p + X'_{it}\mu + \varepsilon_{it} \qquad (1)$$

Where y_{it} equals 1 if worker *i* at time *t* is entitled to retirement savings and otherwise equals 0. $Children_{it}$ equals 1 if worker *i* at time *t* has at least one child aged between 6 to 18 years and $Children_{it}$ equals 0 if worker *i* has no children under 18 years of age.[20] $Post_{it}$ equals 1 for the post-reform years, i.e. 2010 to 2013, and for the remaining years equals 0. $Children_{it} * Post_{it}$ is an interaction term whose coefficient δ captures the impact of the healthcare coverage reform. θ_t collects fixed effects by year, λ_p collects fixed effects by province, and X_{it} contains the following covariates: age (squared),

[19] An exception when the robustness check is performed on the treatment group of individuals with children under 18 years of age.
[20] The *"Children"* variable is independent of whether the individual has or not children over 18 years old.

gender, head of household indicator, marital status, years of education (squared), number of children aged 6-18 years, number of children over 18 years old, and size categories of the firm where individual *i* works.

Huber-White robust standard errors. The estimates are obtained without using sampling weights. As a robustness check, equation (1) was also estimated considering individuals with children under 18 (instead of between 6 and 18) as the treatment group and individuals without children under 18 as the control group. The results are similar to the main results of this paper. Furthermore, several tests were conducted to verify the identifying assumptions of the difference-in-differences approach, presenting results from different specifications.

2.3.2 Panel data with administrative databases

Along with the analysis using data from household surveys (ENEMDU), panel data was built with information from administrative registers which only includes people from the database used to determine eligibility for transfer program families (this database is called the Social Register). This database collects information gathered by scanning Ecuador's poorest sectors. Survey information is used to develop a composite welfare index which determines eligible families for a cash transfer program named Human Development Bonus (BDH in Spanish). This data collects the following information from households: demographic characteristics, education, and labor status of all members (this database collects information for 2008 and 2014).

Data from the first period serves as a baseline to identify particular population according to their information in 2008 (pre-reform period). Through this database, the eligible population for the Human Development Bonus (BDH) was identified. On the other hand, the 2014 data contains the age of each household member. With this, from 2006 to 2013, for each year a treatment variable was built: individuals with children between 6 to 18 years old and individuals without children under 18 years of age.

This information is joined with an administrative register: data from of the Ecuadorian Social Security Institute (IESS in Spanish). This database has monthly information about the affiliation or non-affiliation to the social security from 2006 to 2013 of all individuals included in the Social Register of 2008 and 2014. This affiliation data was

matched with the identity number of the individuals in the social register. An outcome variable of formal or non-formal employment considering the month of December for each year was constructed.

In this analysis, a sample of individuals (head of household or spouses) from urban areas who were 19 to 60 years old in 2006 was used. Not included are individuals who are retired, disabled, or are studying in the pre-reform period (according to information from the Social Register database in 2008). Finally, individuals with children under 6 years were not considered. With these restrictions, the database consists of 407,496 individuals.

With all these sources, panel data was built. Then, a methodology of difference-in-differences was used, comparing those workers who have children between 6 to 18 years of age against workers without children aged 6 to 18 years. Due to the use of a panel data, it was possible to account for unobserved and individual specific time-invariants factors that affect employment, and to avoid bias due to (unobserved) changes in the composition of the "treatment" and "comparison" effects (Hotz, Mullin and Scholz 2005). The following difference-in-differences specification is used:

$$y_i = \alpha + \beta Children_{it} + \delta Children_{it} * Post_{it} + \theta_t + \lambda_p + X'_{it}\mu + \varepsilon_{it} \quad (2)$$

Where y_{it} equals 1 if worker *i* is entitled to retirement savings at time *t* (if he/she contributes to social security in December of year *t*), otherwise equals 0. This outcome variable y_{it} it is constructed with information from the Ecuadorian Social Security Institute (IESS). $Children_{it}$ equals 1 if worker *i* at time *t* has at least one child between 6 and 18 years old; otherwise it equals 0. Individuals with children under 6 years were removed. $Post_{it}$ equals 0 for pre-reform year and 1 for post-reform years (2010 to 2013).[21] $Children_{it}*Post_{it}$ is an interaction term whose coefficient δ captures the impact of the healthcare coverage reform. θ_t captures time fixed effects, λ_p province-fixed effects, α *i* are fixed effects by individual, and X_{it} contains the covariate: age

[21] Data for 2010 were excluded in the analyses since the reform was discussed and approved in that year.

(squared). An estimation of Eq. (2) is made using robust standard errors and clustered at the individual level.

In addition, the impact for individuals eligible and not eligible for the Human Development Bonus according to a welfare score was estimated.

2.4 Results: Repeated cross sections of household surveys

Table 2.1 presents summary statistics by treatment status before and after the reform for different variables. The last column presents the difference between the pre- and post-reform changes (unconditional difference-in-differences) for two groups: workers with children between 6 and 18 years.

For the treatment group (workers with children between 6 to 18 years old), in the pre-reform period, formality rates were: 33% for paid workers, 9% for independent workers, and 51.7% for salaried workers; in the post-reform period, these rates increased to 43%, 17%, and 63.8%, respectively. For the control group (workers without children between 6 and18 years of age), formality rates also increased, but the treatment group's rates increased to a level that decreased the gap between groups after the reform. The last row of Table 2.1 presents the preliminary evidence for formal status. The unconditional difference-in-differences estimate is positive and significant. In relation to other variables, the unconditional mean analysis shows paid workers demonstrate a difference in age, years of schooling, and head of household status between the treatment and control group, so it is important to control for these characteristics to control any possible effect of composition. Similar considerations apply to the subsamples of independent and salaried workers.

Table 2. 1. Descriptive statistics

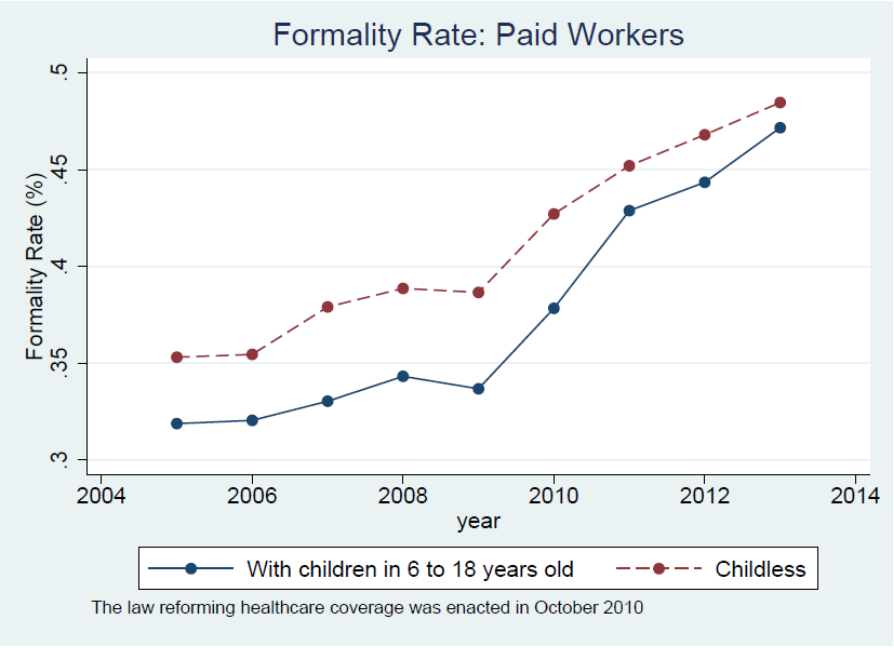
Paid workers	Parents with children of 6-18 years				Individuals without children of 6-18 years				
	Pre-reform		Post-reform		Pre-reform		Post-reform		Difference in
	Mean	sd	Mean	sd	Mean	sd	Mean	sd	Differences
Observations	21,466		16,229		17,181		15,814		
Number of children 6-18 years	1.781	0.884	1.686	0.851	0	0.000	0	0.000	-0.0950***
Number of children > 18	0.641	0.916	0.61	0.863	0.849	1.055	0.865	1.030	-0.0484***
Male	0.431	0.495	0.43	0.495	0.421	0.494	0.429	0.495	-0.0097
Age	43.454	7.897	43.821	7.942	49.111	11.104	50.138	10.606	-0.6722***
Schooling	10.195	4.907	10.329	4.732	9.896	5.172	10.244	5.064	-0.2047***
Married status	0.846	0.361	0.832	0.374	0.667	0.471	0.663	0.473	-0.0104
Head of household	0.701	0.458	0.718	0.450	0.762	0.426	0.756	0.429	0.0215***
Firm size <5	0.507	0.500	0.535	0.499	0.521	0.500	0.537	0.499	0.0116
5-9 employees	0.123	0.329	0.099	0.299	0.11	0.312	0.092	0.289	-0.0071
10-99 employees	0.117	0.321	0.113	0.317	0.103	0.304	0.103	0.304	-0.0031
>99 employees	0.253	0.435	0.253	0.435	0.266	0.442	0.268	0.443	-0.0014
Formal status	0.33	0.470	0.43	0.495	0.373	0.484	0.458	0.498	0.0152**
Independent workers	Parents with children of 6-18 years				Individuals without children of 6-18 years				
	Pre-reform		Post-reform		Pre-reform		Post-reform		DD
	Mean	sd	Mean	sd	Mean	sd	Mean	sd	
Observations	9,394		7,194		7,909		7,313		
Number of children 6-18 years	1.77	0.889	1.677	0.856	0	0.000	0	0.000	-0.0931***
Number of children > 18	0.711	0.953	0.687	0.891	0.934	1.086	0.935	1.045	-0.0283
Male	0.464	0.499	0.444	0.497	0.442	0.497	0.444	0.497	-0.0222**
Age	44.237	7.882	44.822	7.742	51.454	9.434	52.164	8.991	-0.1452
Schooling	9.189	4.616	9.389	4.372	8.454	4.755	8.821	4.650	-0.1525
Marital status	0.835	0.371	0.825	0.380	0.676	0.468	0.667	0.471	-0.0012
Head of household	0.684	0.465	0.716	0.451	0.753	0.431	0.759	0.428	0.0251**
Firm size <5	0.875	0.330	0.929	0.257	0.898	0.302	0.935	0.246	0.0158**
5-9 employees	0.104	0.305	0.058	0.234	0.081	0.273	0.051	0.220	-0.0155***
10-99 employees	0.02	0.140	0.012	0.111	0.02	0.139	0.013	0.113	-0.0004
>99 employees	0.001	0.027	0.001	0.031	0.001	0.030	0.001	0.031	0.0001
Formal status	0.09	0.287	0.17	0.376	0.142	0.349	0.207	0.405	0.0155*
Salaried workers	Parents with children of 6-18 years				Individuals without children of 6-18 years				
	Pre-reform		Post-reform		Pre-reform		Post-reform		DD
	Mean	sd	Mean	sd	Mean	sd	Mean	sd	
Observations	12,072		9,035		9,272		8,501		
Number of children 6-18 years	1.79	0.881	1.694	0.846	0	0.000	0	0.000	-0.0964***
Number of children > 18	0.586	0.883	0.549	0.834	0.777	1.022	0.804	1.012	-0.0661***
Male	0.405	0.491	0.418	0.493	0.403	0.490	0.416	0.493	-0.0005
Age	42.846	7.855	43.024	8.010	47.113	11.996	48.395	11.539	-1.1134***
Schooling	10.978	4.984	11.077	4.873	11.126	5.196	11.468	5.086	-0.2337**
Marital status	0.855	0.352	0.837	0.369	0.66	0.474	0.66	0.474	-0.0178**

Head of household	0.714	0.452	0.719	0.450	0.77	0.421	0.754	0.431	0.0196**
firm size <5 employees	0.221	0.415	0.221	0.415	0.2	0.400	0.195	0.396	0.0051
5-9 employees	0.139	0.346	0.131	0.338	0.134	0.341	0.128	0.334	-0.0012
10-99 employees	0.192	0.394	0.194	0.395	0.174	0.379	0.18	0.384	-0.0046
>99 employees	0.449	0.497	0.453	0.498	0.492	0.500	0.497	0.500	0.0008
Formal status	0.517	0.500	0.638	0.481	0.57	0.495	0.674	0.469	0.0171*

* p<0.1; ** p<0.05; *** p<0.01. "Difference in differences" (DD) refers to the impact coefficient obtained by estimating Eq.1 on each variable but without controls. Results estimated with robust errors and without sample weights. Data source: ENEMDU-INEC 2005-2013.

Figure 2.1 shows the rate of formal employment for paid workers who have children between 6 and18 years old and for paid workers without children under 18 years, both for pre- and post-reform periods.

Figure 2. 1. Formal employment rate between 2005 and 2013 for paid workers (individuals with children between 6 and18 years of age and individuals without children aged 6 and18 years).



Source ENEMDU-INEC for the months of December.

Formality rates of workers without children are higher than those of workers with children for the entire period. However, Figure 2.1 suggests that, since the reform

implemented in October 2010, the formality gap between workers without and with children has decreased.

Table 2.2 presents the estimates of the health reform impact on formal job status. Each column reports OLS estimations of Eq. (1). The first column presents results for all paid workers while the remaining columns report the heterogeneity impact for the subsamples of independent and salaried workers.

Table 2. 2. Effect of health reform on formal job

	Paid workers	Independent workers	Salaried workers
*Children*Post*	0.0219	0.0196	0.0229
	[0.0052]***	[0.0075]***	[0.0070]***
Children (=1 for age range 6-18 years)	-0.0028	-0.0124	0.003
	[0.0049]	[0.0067]*	[0.0069]
N	70,690	31,810	38,880
Percent Formal (Treatment group: 2005-2009)	33%	9%	51.7%

* p<0.1; ** p<0.05; *** p<0.01. Data source: ENEMDU-INEC 2005-2013. Dependent variable: formal status, which equals 1 if the worker is entitled to retirement savings. The sample is restricted to households without children under 6 years. *Children* equals 1 if the worker has at least one child between 6 to 18 years old. *Post* equals 1 for years 2010-2013. Robust standard errors in brackets. Covariates: age, age-squared, gender, household head status, year dummies, marital status, years of education, years of education-squared, number of children aged 6-18 years, number of children aged 18 years and older, province dummies, and full set of firm size categories.

The results evidence that the healthcare coverage reform has positive effect of about 2 percentage points on employment formalization for paid workers (this result represents an impact of 6% compared to the base rate of 33%). The paid workers can be split between salaried and independent workers. The reform has a positive effect of about 1.96 percentage points for independent workers and 2.2 percentage points for salaried workers (corresponding to an increase of 21% and 4%, respectively). The largest relative change occurs among independent workers due to the low share of formal employment this group exhibited in the pre-reform period.[22]

[22] In Latin America, there exist various treatments for independent workers and their contribution to social security. In some countries, the contribution of these workers is mandatory (Costa Rica and Chile) while in other countries it is voluntary. There are also some programs developed to achieve the affiliation of independent workers, including the reduction in contribution amounts (for instance, in Brazil through the

These effects are larger than those found by Bergolo and Cruces (impact of 1.6 percentage points on benefit-eligible employment, this is an impact of 3.5%). This result corresponds to the expected behavior of the post-reform effect because, unlike the Uruguayan reform studied by these authors, the new health coverage in Ecuador does not imply additional costs for the worker.

The difference-in-differences methodology can eliminate the effect of other possible treatments as those that occurred in the same period (information campaigns, inspectors in companies, or the penalty to the employer for not registering their worker) which were implemented for all workers, not only for those who have children. However, workers with children could value social insurance more than their childless counterparts, and so the formalization campaign could have an increase in formality in this population. It is possible that workers with children are more responsive to the campaign than workers without children. The significant and positive impact on the population of independent workers discards the possibility that the effect found in this analysis could be confounded with other "treatments", such as increasing the number of inspectors or penalizing to the employer for not registering their workers, since independent workers are their own employer.

Finally, these results suggest that healthcare coverage outside the social security system is not sufficiently attractive to mute the impact of the reform. The most common alternatives are the centers provided by the Ministry of Health (which are free of charge) or the services provided by private health insurance.

2.4.1 Common trends and dynamic specification

The difference-in-differences approach assumes that the outcome trend for the control group is what would have happened for the treatment group in absence of the reform (Angrist and Krueger 1999). Therefore, it is important to check that trends of both groups are similar previous to the reform. To do so, a slightly modified version of Eq. (1), including interaction terms between the variable *"Children"* and year dummies was estimated. Table 2.3 presents the coefficients of these terms. None of the coefficients of

Registrar Microempreendedor Individual - MEI), cost reduction and simplification of rules in Brazil (through the SIMPLES program) and Uruguay. A review of this topic for Latin America can be found in Bosch, Melguizo and Pagés (2013).

the interactions terms before 2010 are significantly different from zero. This indicates that there are no different trends between both groups before the reform. This supports the identifying assumption that the difference-in-differences estimates capture the impact of the reform on formal employment and that they are not contaminated by differential trends between groups.[23]

Table 2.3 also reports the impact of the reform in each post-reform year separately. For paid and independent workers, the coefficient for each post-reform year is statistically significant. In addition, this table presents the joint hypothesis of the coefficients of interest for pre- and post-reform years, confirming the results mentioned above. Figure 2.2 presents these estimations.

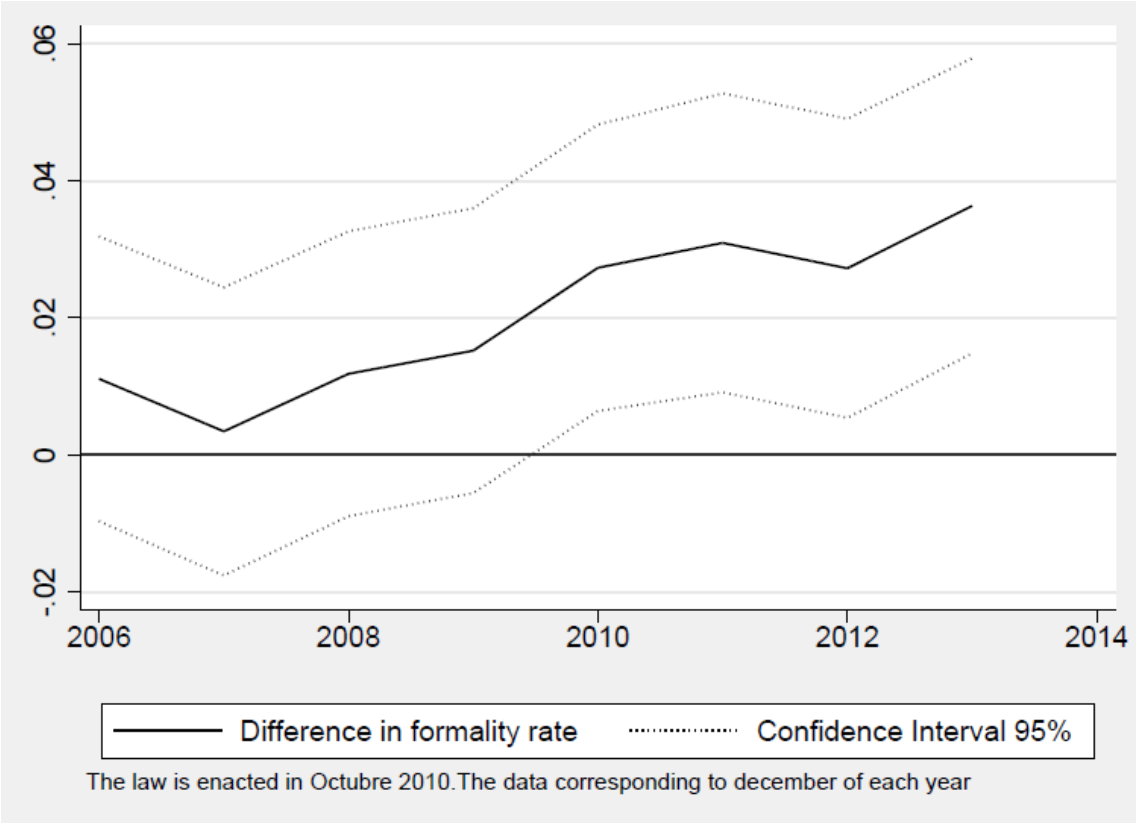
Table 2. 3. Dynamic specification: Differential trends

Children6-18*Year dummies	Paid workers	Independent workers	Salaried workers
2006	0.0112	0.0133	0.007
	[0.0106]	[0.0144]	[0.0151]
2007	0.003	0.0087	-0.0001
	[0.0107]	[0.0150]	[0.0150]
2008	0.0116	0.0215	0.0052
	[0.0106]	[0.0148]	[0.0149]
2009	0.015	0.0128	0.0181
	[0.0106]	[0.0146]	[0.0150]
2010	0.0271	0.026	0.029
	[0.0107]**	[0.0148]*	[0.0149]*
2011	0.0305	0.026	0.0323
	[0.0111]***	[0.0156]*	[0.0155]**
2012	0.0267	0.0294	0.0241
	[0.0111]**	[0.0159]*	[0.0153]
2013	0.036	0.0429	0.0305
	[0.0110]***	[0.0165]***	[0.0146]**
N	70,690	31,810	38,880
		Ho: δ 2006 = δ 2007 = δ 2008 = δ 2009 =0	
p-value	0.5848	0.6884	0.7285
	Ho: δ 2006 = δ 2007 = δ 2008 = δ 2009 =0 only considering pre-reform years		
p-value	0.5277	0.7196	0.6340
		Ho: δ 2010 = δ 2011 = δ 2012 = δ 2013 =0	
p-value	0.0107	0.1026	0.1906

Data source: ENEMDU-INEC 2005-2013. Dependent variable: formal status, which equals 1 if the worker is entitled to retirement savings. The sample is restricted to households without children under 6 years. *Children* equals 1 if the worker has at least one child between 6 to 18 years old. *Post* equals 1 for years 2010-2013. Robust standard errors in brackets. Covariates: age, age-squared, gender, head of household status, year dummies, marital status, years of education, years of education-squared, number of children aged 6-18 years, number of children aged 18 years and older, province dummies, full set of firm size categories, and *Children-year* dummies interaction terms. Omitted category: 2005.

[23] Results are similar if pre-reform trends are estimated only considering pre-reform years.

Figure 2. 2. Difference in the share of formal employment for paid workers (between individuals with children 6-18 years and individuals without children under 18 years)



Source: ENEMDU-INEC for the months of December. Robust standard errors. Covariates: age, age-squared, gender, head of household status, year dummies, marital status, years of education, years of education-squared, number of children aged 6-18 years, number of children aged 18 years and older, province dummies, full set of firm size categories, and *Children* and year dummies interaction terms. Omitted category: 2005.

2.4.2 Robustness

Placebo effect: In order to give more evidence to support that identification strategy captures the causal impact of the reform, Table 2.4 reviews the possibility of placebo effects or false experiments by estimating the impact of the reform in years prior to 2010, when there was no reform. The sample was restricted to the year in which the reform was activated and the years preceding the reform.

Table 2. 4. False experiments

	Paid workers	Independent	Salaried
*Children*Post2006*	0.0114	0.0127	0.0113
	[0.0109]	[0.0147]	[0.0157]
N	15,068	6,860	8,208
*Children*Post2007*	-0.0044	-0.0001	-0.0048
	[0.0096]	[0.0135]	[0.0135]
N	22,855	10,258	12,597
*Children*Post2008*	0.0076	0.0131	0.0061
	[0.0090]	[0.0128]	[0.0124]
N	30,861	13,773	17,088
*Children*Post2009*	-0.0015	-0.0105	0.0076
	[0.0087]	[0.0122]	[0.0121]
N	38,647	17,303	21,344

* p<0.1; ** p<0.05; *** p<0.01 Data source: ENEMDU-INEC 2005-2013. Dependent variable: formal status, which equals 1 if the worker is entitled to retirement savings. The sample is restricted to households without children under 6 years of age. *Children* equals 1 if the worker has at least one child between 6 and 18 years old. *Post* equals 1 for years 2010-2013. Robust standard errors in brackets. Covariates: age, age-squared, gender, head of household status, year dummies, marital status, years of education, years of education-squared, number of children aged 6-18 years, number of children aged 18 years and older, province dummies, full set of firm size categories, and age (three categories)*post interaction terms.

Table 2.4 shows there are no placebo effects for paid workers in any year prior to the reform (for paid workers, independent, and salaried workers.). Thus, both the absence of differential trends between groups in the pre-reform period and the absence of significant effects of placebo experiments confirm the soundness of the identification strategy.

Results robustness: As Bergolo and Cruces (2014) mention, a concern for the identification strategy applied in this paper is that the treatment and control groups may change over the study period, confounding treatment effects with composition effects. For example, since age distribution of individuals with and without children differs, trends in their labor supply may have differed by cohort. Therefore, to control for possible composition effects, Table 2.5 presents the estimated impact with a specification including interaction terms of three *age-categories* and the post-reform dummy (robustness specification 1).

Table 2. 5. Robustness and specification checks

	Paid workers	Independent workers	Salaried workers
Main Results			
*Children*Post*	0.0219***	0.0196***	0.0229***
	[0.0052]	[0.0075]	[0.0070]
N	70,690	31,810	38,880
Robustness (1): Includes interaction terms Categories of *Age*Post*			
*Children*Post*	0.0152***	0.0199**	0.0146**
	[0.0054]	[0.0079]	[0.0073]
N	70,690	31,810	38,880
Robustness (2): Includes interaction terms *Covariates*Children*			
*Children*Post*	0.0221***	0.0214***	0.0227***
	[0.0052]	[0.0075]	[0.0070]
N	70,690	31,810	38,880
Robustness (3): Includes interaction terms Categories of *Education*Post*			
*Children*Post*	0.0209***	0.0155**	0.0208***
	[0.0052]	[0.0075]	[0.0070]
N	70,690	31,810	38,880
Robustness (4): Includes interaction terms *Marital Status*Post*			
*Children*Post*	0.0194***	0.0128*	0.0249***
	[0.0053]	[0.0076]	[0.0072]
N	70,690	31,810	38,880
Robustness (5): drop year=2010			
*Children*Post*	0.0229***	0.0209**	0.0233***
	[0.0057]	[0.0084]	[0.0077]
N	62,559	28,165	34,394

* p<0.1; ** p<0.05; *** p<0.01. Data source: ENEMDU-INEC 2005-2013. Dependent variable: formal status, which equals 1 if the worker is entitled to retirement savings. The sample is restricted to households without children under 6 years. *Children* equals 1 if the worker has at least one child between 6 to 18 years old. *Post* equals 1 for years 2010-2013. Robust standard errors in brackets. Covariates: age, age-squared, gender, head of household status, year dummies, marital status, years of education, years of education-squared, number of children aged 6-18 years, number of children aged 18 years and older, province dummies, and full set of firm size categories.

Table 2.5 also reports the results of a second specification that includes interaction terms of the *Children* variable with a set of demographic controls (three age categories, four education categories, an indicator for the status of head of household, and an indicator for marital status) (robustness specification 2).

Another specification includes interaction terms of the four education categories with the post-reform dummy, which is a way to examine whether the impact effects are not

contaminated by changes in the education composition (robustness specification 3). In the same sense, the following specification includes interaction terms of the marital status indicator with the post-reform dummy (robustness specification 4).

The reform was discussed and approved in 2010. The Act was approved in October 2010 and its implementation began in February 2011. Thus, for the empirical treatment of the survey from December 2010 we have two options: the first is to define this data as post-reform period, the second is to eliminate it. Outcomes are similar to the main results when observations for 2010 are removed (robustness specification 5). Results in Table 2.5 shows that the main findings of this paper are robust to various changes.

2.4.3Alternative treatment group: parents with children under 18 years old

An alternative treatment group for the health reform studied here is the group of workers with children under 18 years old, instead of workers with children between 6 and 18 years old. Results are reported in Table 2.6. The results are in accordance with previous findings, indicating that workers with children under 6 years old have a strong response to the reform. This result makes sense because their children benefit from the reform for the entire 12-year extension, while children above 6 years old at the moment of the reform will benefit for a shorter period.[24]

Table 2. 6. Effect of health reform on formal employment: individuals with children under 18 years old as treatment group

	Paid workers	Independent workers	Salaried workers
*Children<18*Post*	0.0268	0.0193	0.0287
	[0.0047]***	[0.0068]***	[0.0063]***
Children<18 dummy	-0.0159	-0.0258	-0.0104
	[0.0040]***	[0.0055]***	[0.0055]*
N	95,703	40,961	54,742
Percent formal (avg 2005-2009)	31.3%	7.6%	48%

* p<0.1; ** p<0.05; *** p<0.01 Data source: ENEMDU-INEC 2005-2013. Dependent variable: formal status, which equals 1 if the worker is entitled to retirement savings. *Children* equals 1 if the worker has at least one child under 18 years old. *Post* equals 1 for years 2010-2013. Robust standard errors in brackets. Covariates: age, age-squared, gender, head of household status, year dummies, years of education, years of education-squared, number of children under 18 years old, number of children aged 18 and older, and full set of firm size categories.

[24] The robustness, previous trends, and placebo effect tests were performed with this group of alternative treatment, confirming that the identification strategy is appropriate.

2.4.4 Differential incentives: Estimates by age of children

The reform's incentives vary as a function of the age of an individual's dependent children in that younger children would have a longer duration of benefit.

To explore the potential heterogeneity introduced by that incentive, the model in Eq. (1) is extended to allow for different treatment effects according to the age range of those children. The specification is:

$$y_{it} = \alpha + \Sigma^k \beta^k Children_{it}^k + \Sigma^k \delta^k Children_{it}^k * Post_{it} + \lambda_p + \theta_t + x'_{it}\mu + \varepsilon_{it} \quad (3)$$

where *k* is one of the following mutually exclusive groups: children aged 0-10 years, children 11-17 years, and children in both age groups. The Table 2.7 shows the results based on Eq 3. In paid workers, there is an impact of 3.5 percentage points on formal employment for parents with some children aged 0-10 years (this result represents an impact of 10%) and 1.7 percentage points for parents with children aged 0-10 years and 11-17 years. The coefficient for the indicator of children aged 11-17 is statistically different from zero, however it is a low coefficient. In independent workers, the only group that has an impact statistically different from zero are parents with children aged 0-10 years, the impact is 2 percentage points (this result represents an impact of 24%). In salaried workers, there is an impact of 3.9 percentage points for parents with children aged 0-10 years (this result represents an impact of 8%) and 2.2 percentage points for parents with children in both age groups. These results confirm the idea that the reform represents a great incentive for parents with younger children, since the benefit that these children receive after reform is of longer duration than that of older children.

Table 2. 7. Effect of the health insurance benefit extension on formal status by age of children

	Paid work	Independent work	Salaried work
Children group dummies*PostReform			
Children, aged 0-10	0.0351	0.0221	0.0395
	[0.0067]***	[0.0097]**	[0.0090]***
Children, aged 11-17	0.0095	0.0131	0.0080
	[0.0055]*	[0.0080]	[0.0075]
Children, aged 0-10 and 11-17	0.0175	0.0112	0.0223
	[0.0064]***	[0.0087]	[0.0091]**
N	70,690	31,810	38,880
Percent Formal (2005-2009)	33%	9%	51.7%

* p<0.1; ** p<0.05; *** p<0.01. Data source: ENEMDU-INEC 2005-2013. Dependent variable: formal status, which equals 1 if the worker is entitled to retirement savings. The sample is restricted to households without children under 6 years. *Children* equals 1 if the worker has at least one child between 6 to 18 years old. *Post* equals 1 for years 2010-2013. Robust standard errors in brackets. Covariates: age, age-squared, gender, household head status, year dummies, marital status, years of education, years of education-squared, number of children aged 6-18 years, number of children 18 years and older, province dummies, and full set of firm size categories.

2.5 Results with panel data between social registers of 2008 and 2014

An analysis of the common trend in the previous period is presented in Table 2.8. The table shows that there is a previous trend common to individuals who contributed at least 1 year in the previous period. Table 2.9 shows that the reform has a significant impact of 2.6 percentage points in the sample of individuals who had been formal workers in any year during the previous period. There is no impact on those who were non-formal during the pre-reform period (the impact is significant but small).

Table 2. 8. Common trends in pre-reform period. Population of the database used for selection of beneficiaries of social programs.

Dependent variable	Formal status in December of each year					
Sample	all cases			Individuals who were formal for a minimum of one year in the previous period		
	All	BDH		all	BDH	
*Children6-18*year dummies*		elegible	non-elegible		elegible	non-elegible
2007	-0.0006	0.0032	-0.0003	-0.0048	-0.0004	-0.0088
	[0.0013]	[0.0016]*	[0.0015]	[0.0077]	[0.0341]	[0.0079]
2008	-0.0028	0.0003	-0.0025	-0.0071	-0.0263	-0.0077
	[0.0013]**	[0.0017]	[0.0015]	[0.0077]	[0.0343]	[0.0079]
2009	-0.0035	0.0021	-0.0044	0.0011	0.0106	-0.0051
	[0.0013]***	[0.0018]	[0.0017]***	[0.0080]	[0.0347]	[0.0082]
Formal post-reform (control)	17%	6.7%	21.4%	72%	59%	79%
N_Clust	407,496	123,313	284,183	65,257	9,190	56,067
N	1,629,984	493,252	1,136,732	261,028	36,760	224,268

* p<0.1; ** p<0.05; *** p<0.01 Data source: Social Register 2008 and 2014 and Administrative Data from social security 2006-2013. Dependent variable: formal status, which equals 1 if the worker is entitled to retirement savings according to data of the Social Security. *Children* equals 1 if the worker has at least one child between ages 6-18 in each year. *Post* equals 1 for years 2010-2013. Year 2010 is dropped. Robust standard errors in brackets and fixed effects at individual level. Covariates: age (squared).

Table 2. 9. Effect of Health Reform on Formal Employment using Social Register 2008-2014 with Administrative Data from Social Security.

Dependent variable	Formal status					
Sample	Non-formal pre-years			Individuals who were formal for a minimum of one year in the previous period		
	all	BDH		all	BDH	
		elegible	non-elegible		elegible	non-elegible
*Children*Post*	-0.0018	-0.0015	-0.0008	0.0263	0.0247	0.0247
	[0.0009]**	[0.0011]	[0.0012]	[0.0032]***	[0.0091]***	[0.0034]***
N (Individuals)	342,239	114,123	228,116	65,257	9,190	56,067
N (Individuals*years)	2,395,673	798,861	1,596,812	456,799	64,330	392,469

* p<0.1; ** p<0.05; *** p<0.01 Data source: Social Register 2008-2014, Administrative Data from Social Security 2006-2013 and Ecuadorian Internal Revenue Institute. Dependent variable: formal status, which equals 1 if the worker is entitled to retirement savings according to data of the social security. *Children* equals 1 if the worker has at least one child between ages 6-18 in each year. *Post* equals 1 for years 2010-2013. Year 2010 is dropped. Robust standard errors in brackets and fixed effects at individual level. Covariates: age (squared).

Results from Table 2.9 indicate that for those individuals previously non-formal, benefits of shifting from non-formality to formality are higher than its costs. According to Maloney (1999), and Maloney (2004) some of those costs are the loss of flexible schedules or the loss of being eligible for social programs.

In both populations that are eligible and ineligible for the cash transfer program, there is a positive impact of 2 percentage points.[25] This impact is identified in individuals who were formal workers at least once during the previous period. This reform is capable of counteracting any potential negative impact that BDH has on formal employment.

2.6 Conclusions

This article studies the impact on the share of formal employment caused by an extension of health coverage of formal workers' children in Ecuador based on two data sources: household surveys through a repeated cross section and a panel of individuals built with the administrative registry from social security for the population of the database used for the selection of beneficiaries of social programs.

With the repeated cross section of household surveys, this paper presents evidence of a positive impact on the share of formal employment from the extension of healthcare coverage to children aged 6-18 years old. The impact is about 2 percentage points on three sub-samples: for paid workers there is an increase of 6%, for independent workers the increase is 21%, and for salaried workers it is 4%. The results are robust for different specifications and robustness checks confirm that the identification strategy captures a causal impact.

The largest relative change occurs among independent workers due to the low share in formal employment during the pre-reform period of this group (with a formal rate of 8.9%). These workers represent one of most difficult groups to convert into formal employment, therefore these results are an important contribution to the literature.

[25] To estimate results in the population eligible for the cash transfer, the observations are divided between an eligible and ineligible population, according to welfare score in the previous period.

With the panel data, it is evident that after the reform, individuals with children aged 6-18 years are more likely to have formal jobs than childless individuals (around 2.6 percentage points for individuals who had been formal workers in any year during the previous period). In both populations that are eligible and ineligible for the cash transfer program, there is a positive impact of 2 percentage points. These impacts are identified in individuals who were formal workers at least once during the previous period.

This investigation contributes to the recent literature of the region by presenting important evidence for a country with high informality and high transition rates between informal and formal employment, where policies on the expansion of the social security coverage or similar programs (non-contributive pensions, non-contributive health insurance, among others) can change the incentives and apparently encourage a higher formal employment. In this case, the basket of benefits that the social security offers to formal workers is evaluated by individuals taking into account benefits and costs in order to choose between switching to formal employment, or to stay in a formal activity if workers are already formal.

The response of workers to these incentives has important implications when designing labor market policies and social security reforms. It is also particularly relevant in recent debates about non-contributory pension system designs, such as pensions in conditional cash transfer programs.

Chapter 3

The Impact of Positive and Negative Income Changes on Labor Supply. Evidence from Ecuador

3.1 Introduction

In 2009 the Ecuadorian cash transfers program, aimed at forty percent poorest households, underwent a change in the construction of its eligibility score. This change determined different groups of eligible people: among those who used to receive the transfer prior to 2009, some continued to receive it while others stopped doing so (negative income shock); and among those who were not receiving the transfer, some began to receive it while others continued without it (positive income shock). This change constitutes an exogenous shock to the income of these households and, through an analysis of regression discontinuity, it is possible to evaluate the impact of this shock in income on labor market variables, 5 years after the change.[1]

Cash transfer programs in Latin America have not been restricted in relation to employment. But this does not imply that they have been neutral in terms of labor supply. As Fiszbein y Schady (2009) and Alzua,et.al (2012) mention, there are channels through which a transfer program can affect labor market decisions. The first channel would explain an employment disincentive caused by an income effect (considering leisure as a normal good). However, the presence of fixed hours or of a low income-leisure elasticity may result in a zero effect on labor supply, and in some cases, it could increase when the transfer allows the poor worker to get better options to participate in the labor market.[2]

The second channel is associated to the potential decreasing of family's income associated with a reduction of child labor (many of these programs look to increase school enrollment of children and, therefore, reduce child labor)[3], pushing adults to increase their labor supply (or at least to keep it unchanged) to avoid a decrease in the

[1] A general reference on this topic is Moffitt (2002) who reviews the labor supply and other work incentive effects of welfare programs.
[2] Ardington, Case and Hoseggod (2009) found positive effects in the migration labor of adult members of eligible households of an old-age pension in Africa, and explain this through monetary restrictions and childcare, which were subsidized by mean of the transfer.
[3] (Edmons y Schday 2009).

family's income. And by the third channel, these authors indicate that the increase of children's school enrollment as a product of these transfer programs gives more time to women who were previously engaged in childcare, thus reducing their cost of working.[4] Therefore, the impact of cash transfers on the labor market outcomes becomes an empirical question.

In the framework of this research, where the results of employment are analyzed five years after the change of the eligibility index, it is important to identify if the population that stopped receiving the transfer did not have negative employment effects, which could possibly have made them return to a poverty level that qualified them for the transfer in the first place. This reflection is relevant to the population that stopped receiving the cash transfer in Ecuador. On the other hand, it is also important to know whether those who received the cash transfer had a negative impact on labor supply.

The use of a change in the eligibility score for a cash transfer to assess the impact of a shock in income is relatively new[5], Buser, et al. (2014) use this possibility and evaluate the impact of this exogenous shock of income on Ecuadorian child malnutrition indicators. In this paper, I use the regression discontinuity method within the empirical framework of Buser et. al., and I use the database of the social register (three rounds of the government database 2003, 2009 and 2014) to evaluate the impact on labor market variables in 2014 (approximately five years after the shock) of gaining the transfer (comparing those who never received the transfer with those who began receiving it in 2009) or of losing the cash transfer (comparing those receiving the transfer since 2003 and continued receiving it, with those who stopped receiving it in 2009). I also discuss possible adjustments inside the household by estimating effects on marital status.

The contributions of this research are the following: first, while most studies in the region analyze only the positive impacts of cash transfers, this article identifies the impacts of positive and negative shocks in income on labor market outcomes. Second, it has information to evaluate the impact of treatment on the type of job: salaried, independent work or unpaid work. Third, in relation to the work of Buser et. al., this

[4] Evidence on this direction can be found in Parker y Skoufias (2000).
[5] (Ardington, Case and Hoseggod 2009) and (Buser, et al. 2014).

research has the advantage of having access to three rounds of the administrative database, allowing the access to all the observations around the threshold as well as to study of heterogeneous results. In addition, this study focuses on results on labor market outcomes, unlike Buser et. al. Finally, possible outcomes are evaluated in family composition such as marital status, as part of the adjustment that takes place inside households after an income shock.

To estimate the impact of losing or gaining the transfer, all individuals need to be identified with an ID number in the three rounds of the database (Selben I, Selben II and Selben III). However, the Selben I (2003) collected this specific information mostly for women, since they were the possible direct recipients of the transfer. Because of this reason, in the gaining or losing the transfer analysis, the results are focused on women.

The main results are that five years later: First, women who lost the cash transfer, and had been receiving it for six years, are equally likely to have paid work than women who continued to receive the transfer. Except for those who were not married at baseline, where the impact is negative (around 4 percentage points). Second, in the case of positive income shock, five years later there is not impact on paid work for those women who began receiving the transfer in 2009 in comparison to those who never received it. Third, as part of the adjustment of these households it is found that, five years later, those women who lost the cash transfer are more likely to be married in 2014 than those who continued to receive the transfer (2.5 percentage points) and the women are "winners" are less likely to be married in 2014 compared to those who have never received this income (4 percentage points). These results suggest two ideas: that marrying may be a survival mechanism and the cash transfer involves empowering women in their decisions, which directly affects decisions on marital status.

The rest of this paper is organized as follows: Section 2 presents the context and data sources. Section 3 introduces the methodology. Section 4 presents and discusses the main results. Finally, section 5 summarizes and concludes.

3.2 Context and Cash Transfer Program

Ecuador is a country that has about 16 million inhabitants, 68% live in urban areas and 32% in rural areas. In 2015 GDP was 100 million dollars (current dollars) and per capita

GDP is about 6,196 dollars. Regarding the labor market, in 2015 the economically active population are 7 million people, only 46% of that population has a salary above the minimum wage, 14% are underemployed; while the workers without social security are 64%.[6]

One of the most important social protection program, according to the number of beneficiaries is the cash transfer program called the Human Development Bonus. The cash transfer has three types of beneficiaries: eligible households (where mainly female head or spouse is the recipient of the transfer), elderly people and disabled people living in poverty. In 2004 around 840,305 women (and their households) were beneficiaries of the program, between 2009 and 2013 beneficiaries increased to 1 million women[7] and in 2015, fell to 444,150.[8] The transfer program budget was 0.86% of GDP from 2003 to 2006, and 1% from 2007 to 2012. The main objective of the program was to improve the human capital of the poor population (education and health).

From 2003 to 2007 the transfer was $ 11 monthly per household. From 2007 to July 2009 transfer increases to $ 30. Then transfer rose to $ 35 until January 2013 where it was again increased to $ 50. To understand the potential magnitude of this transfer, it should be noted that from 2003 to 2013, the transfer represents about 11% of household income.[9]

This cash transfer was implemented in 2003 through a poverty score and a threshold for eligibility that worked until 2009. The poverty score is computed using nonlinear principal components based on information about the household and its members, education, employment, housing, access to services, etc. The eligibility threshold was set to reach 40% of the poorest population.[10] From 2008 to 2009 there was gathering of

[6] Own estimates through employment surveys, are consistent with the official INEC figures for December 2015. Economically active population (EAP): 7 million people, 46% of the EAP are people with adequate employment (population has a salary above the minimum wage), 14% of the EAP are underemployed, 34% people without adequate employment, 4.7% of the EAP are unemployed.
[7] The total number of beneficiaries was 1 million people in 2004, 1.6 million in 2009 and 1.7 million in 2013, adding to those elderly and disabled.
[8] During 2014 and 2015, a new poverty score was built and a new eligibility threshold was determined, this led to a reduction the number of beneficiaries.
[9] Estimated average from the survey of employment and unemployment, in households receiving transfer.
[10] Poverty score scale of 0 (poorest) to 100 (the least poor).

new information and the methodology for creating a new poverty score changed. With this some individuals changed their eligibility status while others did not.

Following Buser, et al. (2014), I used the change in the poverty index (Selben index) or eligibility for a cash transfer program called Bono de Desarrollo Humano (BDH, in its Spanish acronym) as an exogenous shock in the income of households.

From 2003-2008, the beneficiaries of the cash transfer have been selected through a database (the name of this database is Selben, for this paper I refer to this first round of data as Selben I). From 2008 and 2009's first semester, there was new gathering of data so as to update the information and change the methodology for the poverty index (the name of this database is Social Registry 2008-2009, I refer to this second round as Selben II). This new poverty index was used from August 2009 up until 2014. At the beginning of 2014 there was another gathering of data (Social Registry 2013-2014, I refer to this third round as Selben III). The impacts on the labor market and the results of other variables can be identified through the new information in 2014, which was collected five years after the exogenous change in income in 2009.

The change in the poverty score in 2008-2009 results in four groups of individuals: a) those who continued to receive the cash transfer (always winners), b) those who ceased to receive the transfer (losers), c) those who began receiving the transfer (winners), and those who continued without receiving the transfer (always losers). Therefore, if the individuals who are "always winners" were to be compared to those identified as "losers," the impact of a negative change in income will be estimated. Whereas, if the "winners" individuals are compared with the "always losers", a positive change in income will be estimated.

The advantage of this paper, in comparison to that of Buser et. al. is that it uses government databases for selecting beneficiaries (including Selben I, Selben II and Selben III) such that it contains all the observations across the threshold of eligibility. Additionally, the paper contains an analysis of the payment database received in cash by individuals. This allows to estimate both ITT (reduced form) effects as LATE (instrumental variable) effects.

The outcome variables of the labor market were collected in 2013 and part of 2014. This means that individuals who were "always winners" received a cash transfer as maximum for 11 years (2003 to 2014), while individuals who were “losers" received money transfers for six years at most (2003 to 2009) and then ceased to receive it. And those individuals who are "winners" received the transfer of money for five years at most (2009-2014).

3.3 Empirical Strategy

The changes in eligibility to receive the cash transfer, generated by a change in the construction of the poverty index in 2009, simulate an exogenous change in income, as expressed by Buser et. al. (2014), allowing an opportunity to assess the impact of a change in income on labor market outcomes.

The analysis is restricted to individuals who are around the new eligibility threshold (Selben II). I use a regression discontinuity approach, identifying the impact of a negative change in income (comparing those who are “always winners” with those who are “losers”) and a positive change in income (comparing “winners” with the “always losers”) on different labor market outcomes.[11]

The following specification is used to identify the impact of an exogenous change in income. First, I analyze a negative change in income restricting the sample to individuals who continued to receive the cash transfer (always winners) and those who ceased to receive the transfer (losers). Second, I analyze a positive change in income restricting the sample to individuals who began receiving the transfer (winners) and those who continued without receiving the transfer (always losers).

$$y_i = \alpha + \beta T_i + \delta f(e_i) + \lambda T_i + X'_{i\mu+}\varepsilon_i \qquad (1)$$

Where y_i is a dummy variable equal 1 if the individual has paid employment and 0 otherwise (similar for labor participation, work, salaried work, independent work,

[11] References of this approach: (Angrist and Pischke 2009), (Gelman and Imbens 2014), (Imbens and Angrist 1994). (Imbens, Rubin and Sacerdote 2001), (Lee and Lemieux 2010)].

unpaid work and marital status)[12], T_i is a variable equal to 1 if the individual is eligible to receive the transfer (that is to say, if the individual's poverty score is below a threshold) and 0 otherwise, e_i is the poverty score and $f(e)$ is a polynomial form of the poverty score. Control variables X i include: age (squared), gender, years of schooling, marital status, household head status, dummy variables by provinces and a dummy variable for the year in which the information was gathered (2013 or 2014). I use the variables of the Selben II survey as baseline variables just before the change on income. For this analysis I restrict the sample to individuals older than age 17 and younger than age 65 at Selben III database (2013-2014).

One concern with this identification strategy is that the discontinuity regression can be invalidated if there is manipulation of the score around the threshold, which would invalidate the quasi-random assignment. As noted by Buser et. al. "this could occur if households know how the poverty index is constructed and know the location of the program threshold at the moment that they are visited by the enumerators that collect information for Selben II. But the variables used in the construction of this score, the weights of the variables, and the eligibility threshold was determined ex post to the collection of information", therefore manipulation by individuals would not be feasible.[13]

To determine the individuals of the four analysis groups and analyze labor market outcomes in 2014, merged databases between Selben I, Selben II and Selben III are employed.[14] Previously it is necessary to analyze the discontinuity test around the threshold from Selben II.

For the sample of women, the McCrary test do not reject equal density around the eligibility threshold (Appendix A3). For the sample of unmarried and married women in baseline, the same test is also performed, which indicates that results can be identified

[12] Unfortunately, in these databases there is no information on hours worked or labor income.

[13] This is confirmed by doing the McCrary test, (-0.0045 with standard error of 0.047 in Selben I and -0.00105 with standard error of 0.0056 in Selben II) which confirms there is no evidence of manipulation (Appendix A1).

[14] I restrict the observations Selben I database that were collected until 2004 (inclusive), this is because in the original data included special surveys conducted in 2007.

for these groups. Additionally, it is important to note that the data gathering for Selben II was conducted by two institutions: The National Institute of Statistics and Census (INEC) and the Ministry of Social Development (MCDS). INEC gathered the information mainly by scanning (visiting each house) in poor sectors of the country, while the MCDS conducted surveys on demand.[15] Therefore, I use merged databases between Selben I, Selben II and Selben III restricted to the INEC sample.[16]

To estimate the impact of losing or gaining the transfer, all individuals need to be identified with an ID number in the three rounds of the database (Selben I, Selben II and Selben III). However, the Selben I (2003) collected these specific information mostly for women, since they were the possible direct recipients of the transfer (mainly heads and spouses of household).[17] For that reason, the results are focused on women.

Women who are in the three joined databases are 840,722. Of those who are in the sample of INEC are 628,359. Finally, around the threshold of eligibility selben II, there are 164,906 women (+/-5 points). This is the sample that is used with different bandwidth in the analysis of gaining or losing the transfer.

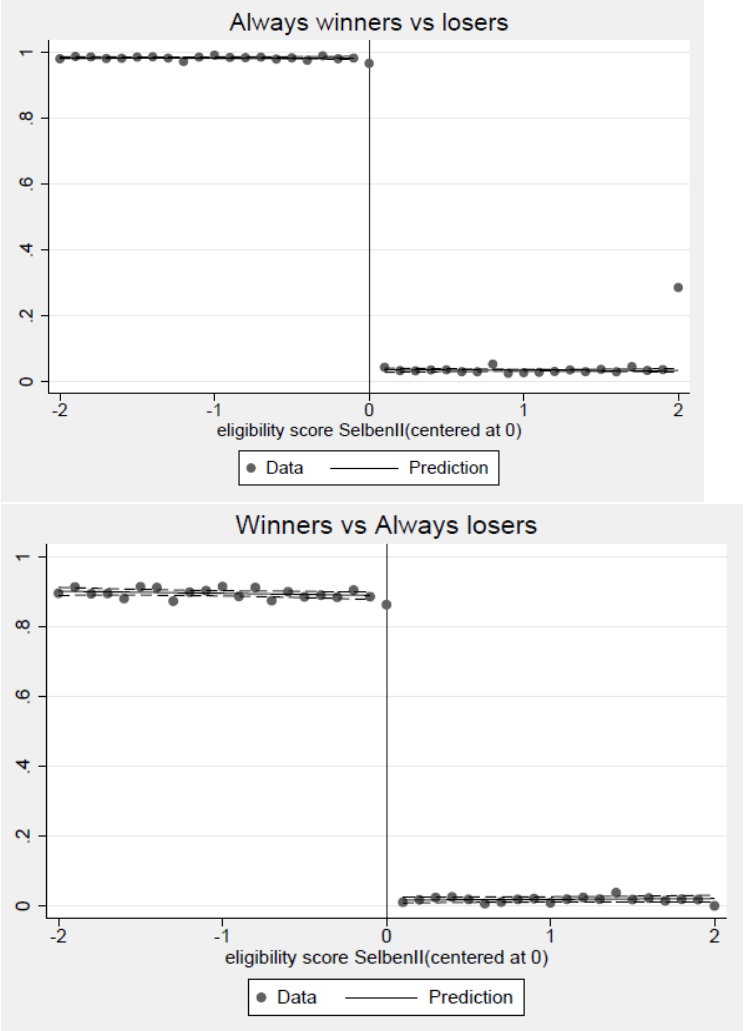
Both estimates of the ITT (reduced form) as the LATE estimates (the latter are shown in appendix) are reported.

[15] These results are showed in A2 of the appendix.

[16] The test of the appendix A3 is considering the INEC sample.

[17] At the database of Selben I (2003) from women aged 18 and over, 80% have ID. And from women aged 18 and over who are heads or spouses of the household, 90% have ID. These figures for the men are: men aged 18 and over, 38% have ID. And men aged 18 and over who are heads or spouses of the household, 42% have ID.

Figure 3. 1. First Stage. The households received at least one payment between 2009 and 2012



The figure shows the proportion of households who collect the BDH (cash transfer) at least once above and below the cutoff for two sample (Always-winners vs losers and winners vs Always-losers). Observations are divided into bins with a width of 0.10 and Selben II score is centered to be zero at the cutoff. Households to the left of the cutoff are eligible to receive the transfer while those to the right are not.

3.4 Results

3.4.1 Descriptive statistics

Table 3.1 shows some characteristics between eligible and ineligible women at baseline. It uses data just before the modification of the Selben index (information from Selben II) for individuals who are close to the eligibility threshold and confirms that the change experienced by individuals in their eligibility status was random conditional on the running variable in the whole sample (column "all") and in the sample of "Always winners" vs "losers" for different bandwidth around the threshold. In the sample of "winners" vs "always losers", the treatment group and the control group are similar in the baseline, except for the probability of being married in 2008 for the bandwidths

furthest from the eligibility threshold (this probability was lower for the "winners" than for the "always losers"). For the different exceptions conditioning each variable in the baseline is necessary.

Table 3. 1.Differences between eligible and non-eligible for each sample at baseline (2008). Women in the three rounds of the Selben base

Variables in 2008	Always winners vs losers				Winners vs Always losers				All			
	+/-1	+/-1.5	+/-2	+/-2.5	+/-1	+/-1.5	+/-2	+/-2.5	+/-1	+/-1.5	+/-2	+/-2.5
Observations	18,325	27,531	36,447	45,498	9,276	13,879	18,628	23,341	27,601	41,410	55,075	68,839
age	-0.1956	-0.0885	0.1851	0.0426	-0.5888	0.1633	0.4748	0.4421	-0.3056	0.0088	0.2887*	0.1895
	(0.2996)	(0.2288)	(0.2105)	(0.1874)	(0.4508)	(0.3356)	(0.3153)	(0.2979)	(0.2686)	(0.1888)	(0.1644)	(0.1566)
mean in non-eligible	38.4844	38.4965	38.6141	38.6230	37.1442	37.4130	37.5839	37.7110	38.0278	38.1229	38.2501	38.2987
schooling	0.0192	0.0491	0.0532	0.0552	0.0486	0.0377	0.0396	-0.0197	0.0190	0.0404	0.0478	0.0280
	(0.1222)	(0.0934)	(0.0802)	(0.0674)	(0.2365)	(0.1468)	(0.1028)	(0.0901)	(0.1232)	(0.0833)	(0.0602)	(0.0533)
mean in non-eligible	6.9338	6.9603	7.0003	7.0222	7.6019	7.5752	7.6121	7.6373	7.1614	7.1723	7.2165	7.2409
head of household	-0.0223	-0.0106	-0.0033	-0.0020	0.0100	0.0065	0.0091	0.0143	-0.0115	-0.0048	0.0009	0.0032
	(0.0145)	(0.0096)	(0.0093)	(0.0077)	(0.0274)	(0.0208)	(0.0184)	(0.0156)	(0.0157)	(0.0101)	(0.0084)	(0.0072)
Percent in non-eligible	0.3683	0.3624	0.3640	0.3645	0.3783	0.3728	0.3741	0.3729	0.3717	0.3660	0.3676	0.3675
Married	0.0181	0.0128	0.0052	0.0038	-0.0162	-0.0197	-0.0244*	-0.0253**	0.0068	0.0018	-0.0048	-0.0057
	(0.0142)	(0.0097)	(0.0090)	(0.0077)	(0.0188)	(0.0158)	(0.0144)	(0.0114)	(0.0125)	(0.0094)	(0.0080)	(0.0067)
Percent in non-eligible	0.6412	0.6483	0.6474	0.6468	0.6202	0.6234	0.6199	0.6198	0.6341	0.6397	0.6376	0.6372
Paid Work	-0.0046	-0.0055	-0.0022	0.0036	0.0325	0.0185	0.0189	0.0225**	0.0079	0.0027	0.0051	0.0100
	(0.0141)	(0.0103)	(0.0086)	(0.0078)	(0.0214)	(0.0182)	(0.0151)	(0.0113)	(0.0132)	(0.0095)	(0.0078)	(0.0066)
Percent in non-eligible	0.3717	0.3684	0.3700	0.3718	0.3856	0.3858	0.3895	0.3926	0.3765	0.3744	0.3769	0.3792

To estimate the differences, I run specification (1) considering as regressors: eligibility status, score (1st. and 2nd. polynomial order) and score*eligibility status. With robust standard errors in parentheses and clustered by canton. Dependent variables are in baseline 2008. Score centered at zero. Source: merged databases between Selben I, Selben II and Selben II.

3.4.2 The first stage

In Figure 3.1, I present the level of household' likelihood to receive any payments during 2009-2012 for both sides of eligibility cutoff. The figure shows compliance rates are high for "always-winners" (95% of eligible households received the transfer in the period between 2009-2012 while 4% of "not eligible" household received transfers). The figure also shows that, 81% of the winners received the transfer during the time the study was conducted, while 2% of "always losers" received the payment.[18] These results are reported in A3 of the appendix.

[18] The dependent variable is equal to 1 if the household received BDH at least once in the period from August 2009 to December 2012 and is 0 if the household never received the transfer in this period.

3.4.3 Results 2014: The effects of gaining and losing a cash transfer on labor market outcomes and on probability of being married in 2014.

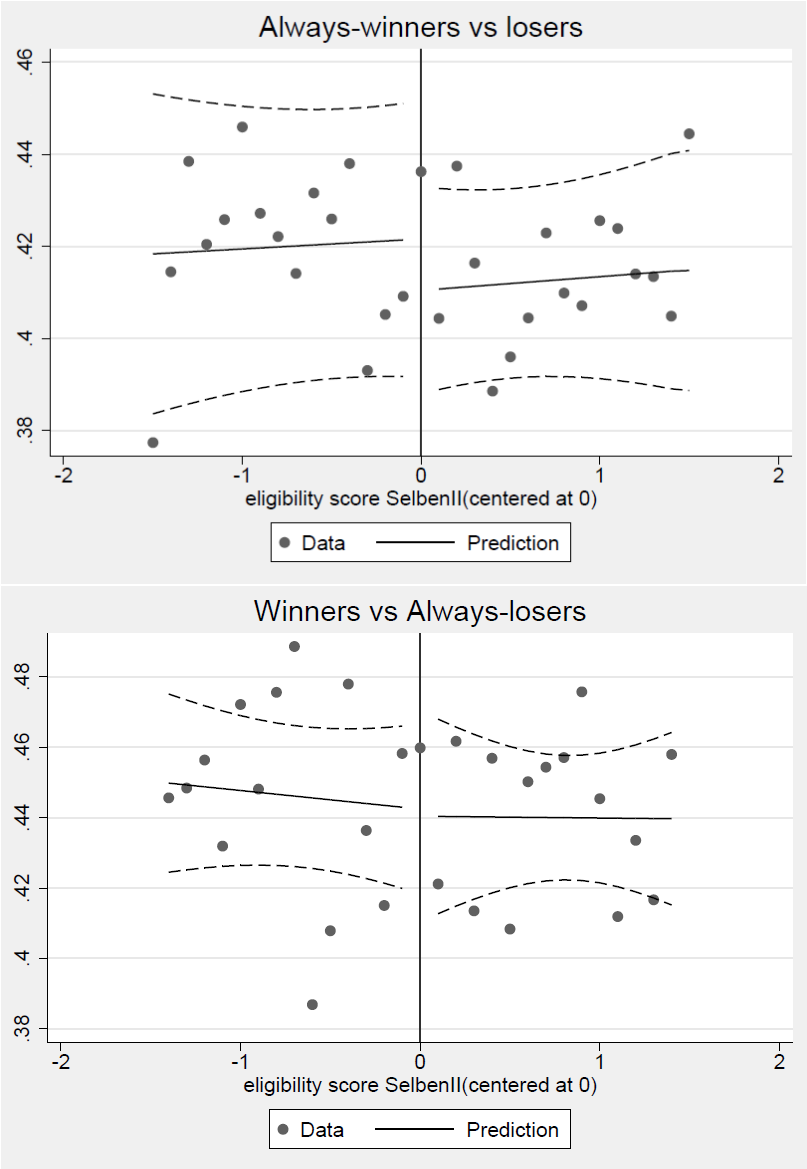
Figure 3.2 shows the effect of losing the transfer (always-winners vs. losers) and winning the transfer (winners vs. always-losers) on labor market outcomes. The Table 3.2 and 3.3 show the results for different bandwidth around the threshold and different specifications polynomial (linear and second order).

Table 3.2 indicates that loss of income transfer has no effect on labor market status (considering that there is no problem of number of observations and the result closest to the threshold is more credible). But the question arises is how women who lost their transfer offset this loss of income if they do not work more? The last column in the table shows that those women who stopped receiving the transfer are more likely to be married in 2014 than those who continued to receive the transfer (around of 2.5 percentage points). These results suggest that marrying may be a survival mechanism for women who lost their transfer. Therefore, we can think economic reasons for marital unions are strong, and also transfers can be associated with women's empowerment within the household.

Table 3.3 shows the effect of gaining the transfer. Those women who started receiving the transfer in 2009 are equally likely to work in 2014 than those who never received it (paid work and salaried work). There is effect on the probability of being an unpaid worker (it increases around 2 percentage points). In relation to the probability of being married in 2014, results closeness to the eligibility threshold (+/- 1 point) indicate that there is no impact. However, that result it may be caused due to the small number of observations. Results with data from +/- 1.5 points around eligibility threshold show that gaining the cash transfer has a significant and negative impact on the probability of being married in 2014 (4 percentage points).[19] Again, these results can be explained thinking that the cash transfer empowers women in their decisions, including their marital status.

[19] Bobonis (2011) carries out an investigation in this sense, with the transfer of the Progresa program and the transition from marital status.

Figure 3. 2. Probability of getting a paid work in 2013-2014. Women



Observations are +/-1 points around of cutoff. Discontinuity at threshold: Always-winners vs losers 0.015 (p=0.218), Winners vs always-losers -0.063 (p=0.027). In this figure observations are divided into bins with a width of 0.1. Each dot represents the average outcome of the individuals in each bin. The solid lines represent the best linear fits through the dots. Individuals to the left of the cutoff are eligible to receive the transfer while those to the right are not. This does not include controls apart from a eligibility status (Ti), Selben II score (ei), and the interaction between the score and the status of eligibility. Robust standard errors are clustered by canton. These estimates are from the reduced form of the intention to treat.

3.4.4 Heterogenous result for sample of women "Always-winners vs losers": Women unmarried at baseline.

A particularly interesting result obtained when comparing women of sample "always winners" vs "losers" is that of unmarried women in 2008/2009 (Table 3.4), 6 years later

those who lost their transfer are more likely to reduce salaried work (4 percentage points)[20] and more likely to be married (6-7 percentage points). These results suggest that cash transfer received by women who are unmarried at the baseline helped to reduce the cost of working.[21]

[20] An exception for this result is in the bandwidth closest to the threshold that may be due to the size of the sample.

[21] In the sample of women "winners" and "always losers" there is not a robust result analogous to this, for this reason these results are not presented.

Table 3. 2. Results on Labor Market and Probability of being married in 2013-2014: Always Winners vs Losers.

Always Winners vs Losers (Women aged 18-64 years in 2014)													points
Dependent Variable	Labor Participation		Paid Work		Salaried Work		Independent Work		Unpaid Work		Married		around the
polynomial order	1st.	2nd.	1st.	2nd.	1st.	2nd.	1st.	2nd.	1st.	2nd.	1st.	2nd.	threshold
Eligible	-0.0148	-0.0133	-0.0034	-0.0058	0.0108	0.0051	-0.0141	-0.0109	-0.0050	-0.0018	-0.0253**	-0.0260**	
	(0.0135)	(0.0144)	(0.0132)	(0.0146)	(0.0105)	(0.0109)	(0.0112)	(0.0127)	(0.0051)	(0.0050)	(0.0106)	(0.0114)	+/-1
N	18,325	18,325	18,325	18,325	18,325	18,325	18,325	18,325	18,325	18,325	18,325	18,325	
Eligible	0.0057	0.0091	0.0117	0.0148	0.0164*	0.0179**	-0.0047	-0.0032	-0.0034	-0.0038	-0.0228**	-0.0222**	
	(0.0118)	(0.0122)	(0.0114)	(0.0114)	(0.0086)	(0.0087)	(0.0091)	(0.0087)	(0.0041)	(0.0042)	(0.0104)	(0.0099)	+/-1.5
N	27,531	27,531	27,531	27,531	27,531	27,531	27,531	27,531	27,531	27,531	27,531	27,531	
Eligible	0.0102	0.0123	0.0154	0.0177*	0.0139*	0.0159**	0.0015	0.0018	-0.0034	-0.0036	-0.0240**	-0.0243***	
	(0.0103)	(0.0111)	(0.0098)	(0.0104)	(0.0072)	(0.0072)	(0.0084)	(0.0083)	(0.0037)	(0.0037)	(0.0094)	(0.0090)	+/-2
N	36,447	36,447	36,447	36,447	36,447	36,447	36,447	36,447	36,447	36,447	36,447	36,447	
Eligible	0.0151*	0.0162**	0.0158**	0.0171**	0.0083	0.0103	0.0075	0.0068	-0.0003	-0.0002	-0.0247***	-0.0255***	
	(0.0078)	(0.0083)	(0.0077)	(0.0080)	(0.0065)	(0.0063)	(0.0084)	(0.0083)	(0.0028)	(0.0029)	(0.0084)	(0.0084)	+/-2.5
N	45,498	45,498	45,498	45,498	45,498	45,498	45,498	45,498	45,498	45,498	45,498	45,498	
Mean-ineligible	45%		42%		22%		20%		2.2%		70%		

* p<0.1; ** p<0.05; *** p<0.01 Dependent variable are: Labor Participation (=1 if individuals work or unemployment, 0 otherwise), Paid work (=1 if individuals work for pay, 0 otherwise), Salaried work (=1 if the individual works as employees, 0 otherwise), Independent work (=1 if individual works as a self-employed or employer, 0 otherwise), Unpaid work (=1 if individual is unpaid worker, usually work in the countryside or in the city when it is unpaid assistant, 0 otherwise), Married (=1 if the individual is united or married, 0 otherwise). Covariates in baseline: eligibility status, score, score*eligibility status interaction term, age, age squared, a dummy for head of household, marital status, years of education, province dummies in baseline. And a dummy for the year in which the information was gathered (2013-2014). Selben II score centered at zero. Results using score around the cutoff (+/-1, +/-1.5,+/-2, +/-2.5 points). Standard errors are in parentheses, clustered by canton.

Table 3. 3. Results on Labor Market and Probability of being married in 2013-2014: Winners vs Always Losers.

Winners vs Always Losers (Women aged 18-64 years in 2014)													points
Dependent Variable	Labor Participation		Paid Work		Salaried Work		Independent Work		Unpaid Work		Married		around the
polynomial order	1st.	2nd.	1st.	2nd.	1st.	2nd.	1st.	2nd.	1st.	2nd.	1st.	2nd.	threshold
Eligible	0.0228	0.0118	0.0075	-0.0073	-0.0003	-0.0065	0.0078	-0.0008	0.0199***	0.0198***	-0.0380*	-0.0303	
	(0.0193)	(0.0220)	(0.0222)	(0.0244)	(0.0189)	(0.0187)	(0.0164)	(0.0180)	(0.0068)	(0.0066)	(0.0217)	(0.0209)	+/-1
N	9,276	9,276	9,276	9,276	9,276	9,276	9,276	9,276	9,276	9,276	9,276	9,276	
Eligible	0.0084	0.0093	-0.0006	-0.0000	0.0011	-0.0002	-0.0016	0.0002	0.0098*	0.0091*	-0.0449***	-0.0411***	
	(0.0133)	(0.0133)	(0.0138)	(0.0141)	(0.0166)	(0.0165)	(0.0145)	(0.0138)	(0.0051)	(0.0050)	(0.0151)	(0.0143)	+/-1.5
N	13,879	13,879	13,879	13,879	13,879	13,879	13,879	13,879	13,879	13,879	13,879	13,879	
Eligible	0.0086	0.0093	0.0034	0.0044	0.0031	0.0029	0.0004	0.0016	0.0065	0.0055	-0.0402***	-0.0411***	
	(0.0131)	(0.0126)	(0.0124)	(0.0126)	(0.0126)	(0.0123)	(0.0134)	(0.0126)	(0.0043)	(0.0041)	(0.0125)	(0.0123)	+/-2
N	18,628	18,628	18,628	18,628	18,628	18,628	18,628	18,628	18,628	18,628	18,628	18,628	
Eligible	0.0141	0.0160	0.0081	0.0104	-0.0005	-0.0001	0.0086	0.0105	0.0069**	0.0068**	-0.0332***	-0.0341***	
	(0.0110)	(0.0110)	(0.0110)	(0.0111)	(0.0103)	(0.0099)	(0.0114)	(0.0108)	(0.0034)	(0.0033)	(0.0107)	(0.0114)	+/-2.5
N	23,341	23,341	23,341	23,341	23,341	23,341	23,341	23,341	23,341	23,341	23,341	23,341	
Mean-ineligible	48%		44%		24%		20%		2.4%		65%		

* p<0.1; ** p<0.05; *** p<0.01 Dependent variable are: Labor Participation (=1 if individuals work or unemployment, 0 otherwise), Paid work (=1 if individuals work for pay, 0 otherwise), Salaried work (=1 if the individual works as employees, 0 otherwise), Independent work (=1 if individual works as a self-employed or employer, 0 otherwise), Unpaid work (=1 if individual is unpaid worker, usually work in the countryside or in the city when it is unpaid assistant, 0 otherwise), Married (=1 if the individual is united or married, 0 otherwise). Covariates in baseline: eligibility status, score, score*eligibility status interaction term, age, age squared, a dummy for head of household, marital status, years of education, province dummies in baseline. And a dummy for the year in which the information was gathered (2013-2014). Selben II score centered at zero. Results using score around the cutoff (+/-1, +/-1.5,+/-2, +/-2.5 points). Standard errors are in parentheses, clustered by canton.

Table 3. 4. Always Winners vs Losers: Work and Marital Status in 2013-2014. Marital Status at the baseline.

Always Winners vs Losers (Women aged 18-64 years in 2014)													points around the threshold
	Women who are unmarried at the baseline						Women who are married at the baseline						
Dependent Variable	Paid Work		Salaried Work		Married		Paid Work		Salaried Work		Married		
polynomial order	1st.	2nd.	1st.	2nd.	1st.	2nd.	1st.	2nd.	1st.	2nd.	1st.	2nd.	
Eligible	0.0252	0.0310	0.0375*	0.0228	-0.0660***	-0.0698***	-0.0201	-0.0273	-0.0045	-0.0051	-0.0011	0.0018	
	(0.0295)	(0.0322)	(0.0226)	(0.0251)	(0.0215)	(0.0238)	(0.0191)	(0.0209)	(0.0143)	(0.0153)	(0.0102)	(0.0103)	+/-1
N	6,524	6,524	6,524	6,524	6,524	6,524	11,801	11,801	11,801	11,801	11,801	11,801	
Eligible	0.0378*	0.0391*	0.0403**	0.0393**	-0.0506***	-0.0500***	-0.0030	0.0009	0.0032	0.0061	-0.0070	-0.0069	
	(0.0208)	(0.0217)	(0.0166)	(0.0165)	(0.0174)	(0.0184)	(0.0152)	(0.0151)	(0.0113)	(0.0120)	(0.0103)	(0.0094)	+/-1.5
N	9,710	9,710	9,710	9,710	9,710	9,710	17,821	17,821	17,821	17,821	17,821	17,821	
Eligible	0.0359**	0.0378**	0.0364**	0.0385***	-0.0515***	-0.0522***	0.0040	0.0063	0.0015	0.0035	-0.0098	-0.0098	
	(0.0179)	(0.0182)	(0.0146)	(0.0144)	(0.0160)	(0.0155)	(0.0123)	(0.0127)	(0.0093)	(0.0092)	(0.0083)	(0.0083)	+/-2
N	12,865	12,865	12,865	12,865	12,865	12,865	23,582	23,582	23,582	23,582	23,582	23,582	
Eligible	0.0340**	0.0348**	0.0213*	0.0233**	-0.0630***	-0.0624***	0.0056	0.0074	0.0011	0.0033	-0.0046	-0.0062	
	(0.0159)	(0.0158)	(0.0115)	(0.0114)	(0.0139)	(0.0137)	(0.0098)	(0.0100)	(0.0086)	(0.0084)	(0.0075)	(0.0075)	+/-2.5
N	16,060	16,060	16,060	16,060	16,060	16,060	29,438	29,438	29,438	29,438	29,438	29,438	

* p<0.1; ** p<0.05; *** p<0.01 Dependent variable are: Paid work (=1 if individuals work for pay, 0 otherwise), Married (=1 if the individual is united or married, 0 otherwise). Covariates in baseline: eligibility status, score, score*eligibility status interaction term, age, age squared, a dummy for head of household, marital status, years of education, province dummies in baseline. And a dummy for the year in which the information was gathered (2013-2014). Selben II score centered at zero. Results using score around the cutoff (+/-1, +/-1.5,+/-2, +/-2.5 points). Standard errors are in parentheses, clustered by canton.

3.5 Conclusions

This paper researches the medium-term effects of a change in unearned income on labor supply of poor families in a lower middle-income country. It exploits a change in the construction of the eligibility score for a cash transfer program in Ecuador in 2009 to estimate the response of labor supply. I use the eligibility database used by the Ecuadorian government in three rounds (2003, 2008-2009 and 2013-2014) and applies the regression discontinuity approach. The impact of a negative income shock is estimated, comparing people who received the transfer and continued receiving it afterwards with those who used to receive the transfer before but stopped receiving it after 2009. The impact of a positive shock on income is also estimated, when comparing people who never received the transfer with those who started to receive it in 2009.

The main results are that five years later: First, women who lost the cash transfer, and had been receiving it for six years, are equally likely to have paid work than women who continued to receive the transfer. Except for those who were not married at baseline, where the impact is negative. Second, there is not impact on paid work or salaried work for those women who began receiving the transfer in 2009 in comparison to those who never received it. Third, as part of the adjustment of these households it is found that, women who lost the cash transfer are more likely to be married in 2014 than those who continued to receive the transfer (3 percentage points), and women who began receiving the transfer in 2009 are less likely to be married in 2014 compared to those who have never received this income (4 percentage points). These results link outcomes in the labor market with variables of family composition.

In relation with other studies performed in the region, Alzua, Cruces and Ripani (2012) look at the effect of welfare programs on work incentives of three programs implemented in rural areas in Mexico, Nicaragua and Honduras. They have not found effect of the cash transfer on the labor supply of participating adults. Others results ((Parker y Skoufias 2000), (Skoufias and Di Maro 2008)) find a small positive effect on the number of hours worked by female beneficiaries in the case of PROGRESA program in Mexico. One of the fundamental differences is that the present paper shows results of longer term (about 5-6 years) than previous studies (about 2 years) it can affect decisions based on the theory of permanent income.

Lastly, these results are relevant for managing cash transfer programs, particularly in the case of Ecuador. Considering individuals who were receiving the transfer but managed to overcome the threshold and were no longer eligible for the transfer, one concern is that without the transfers these people might return to the poverty levels that qualified them to receive the transfer in the first place. In this case, the evidence shows that, five years later, the loss of transfer has an impact on a particular population: women who were not married at baseline and one of its solutions to offset the loss of income is to increase the probability of being married.

On the other hand, the concern that cash transfer cause adverse effects on the labor supply is confirmed particularly in women. Unfortunately, there is no information to identify the impact on total household labor income or total hours worked.

Conclusions

The chapters in this thesis investigate different types of "interventions" that affect Ecuador's labor market and try to answer the following questions: Does fertility reduce female labor supply? Does a social security reform that extends coverage to workers' children increase formal employment? How can a positive and a negative non-labor income shock affect employment and marital status of individuals five years later? Even though these essays can be read separately they share some common aspects. First, these investigations are centered on market labor results. Second, they try to identify heterogeneous results considering different groups of the population. Third, they all focus on identifying causality using different empirical techniques. Finally, each essay uses Ecuador as a case study.

Analyzing the labor market of a country like Ecuador with the many peculiarities that exist in Latin America like high rates of informal employment and high turnover in registered employment, is important to identify what policies, could affect the labor market (like cash transfers). It is also useful to identify what policies have been successful in formalizing employment (social security reforms), particularly in population groups characterized by high informality like independent workers.

The first chapter of this dissertation investigates the effect of fertility on female labor participation in Ecuador. It uses, as source of exogenous variation in family size, parental preferences for a mixed sibling-sex composition on their children (Angrist and Evans 1998). The empirical application shows that women with two boys or two girls are 4 percentage points more likely to have a third child than women with one boy and one girl. 2SLS estimations show that a third child causes a negative impact of 8 to 9 percentage points on female labor supply.

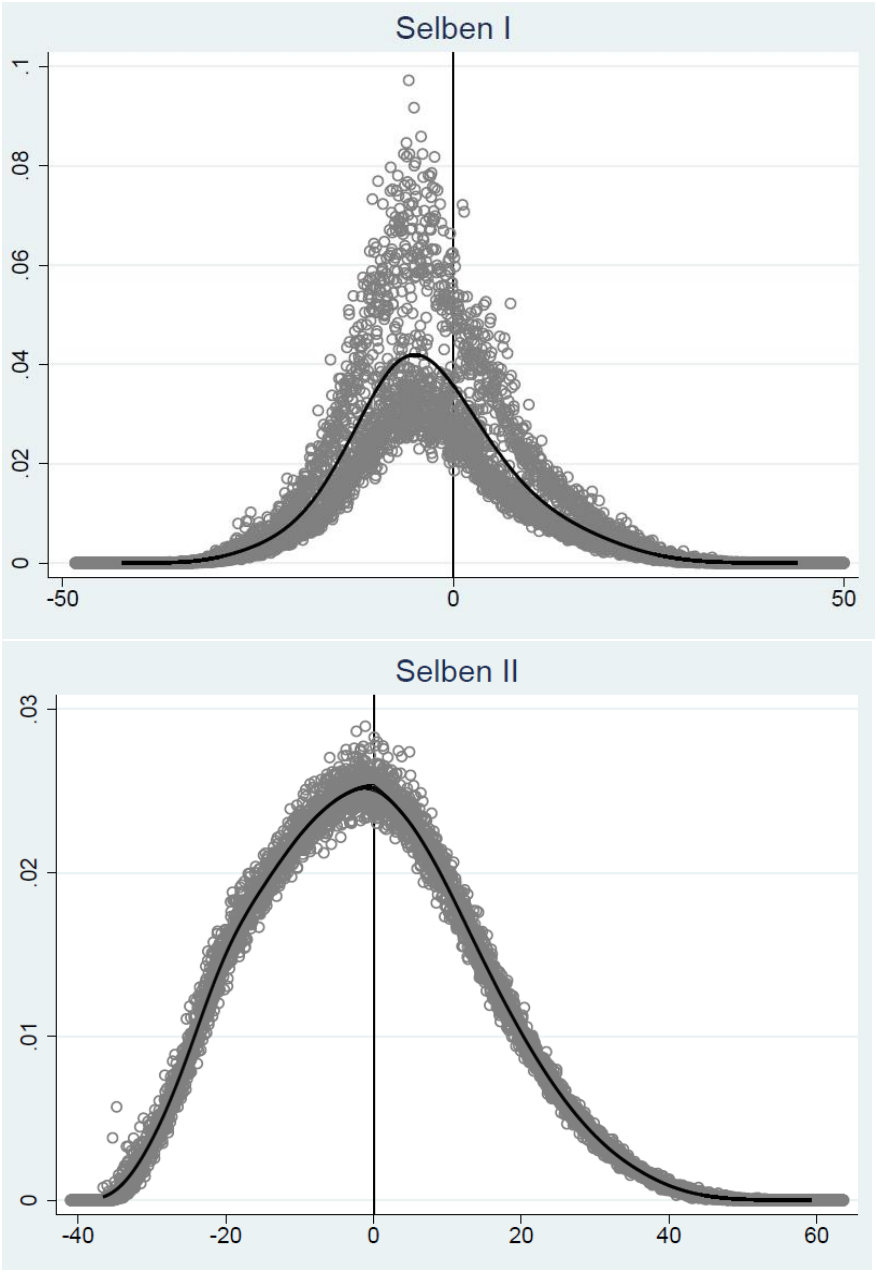
The second chapter examines the impact on the share of formal employment in Ecuador caused by a health insurance extension to formal workers' children. The empirical analysis uses a difference-in-differences approach and repeated cross section of household surveys obtained from the Ecuadorian National Survey of Employment and Unemployment from 2005 through 2013.

I find evidence that after the reform, workers with children were more likely to become formal employees than childless workers. The impact is about 2 percentage points for three subsamples of workers (paid, independent and salaried) corresponding to an increase of 6% for paid workers, of 21% for independent workers and of 4% for salaried workers. This investigation is one of the few studies in the literature that finds a positive result on labor formalization and finds a robust impact on independent workers.

The third chapter evaluates the effects of a change in unearned income on labor supply of poor families. I exploit a change in the construction of the eligibility score for a cash transfer program in Ecuador and apply the regression discontinuity design. The analysis uses the database of the social register (three rounds of the database 2003, 2009 and 2014). The main results are that five years later: First, women who lost the cash transfer, and had been receiving it for six years, are equally likely to have paid work than women who continued to receive the transfer. Except for those who were not married at baseline, where the impact is negative (around 4 percentage points). Second, in the case of positive income shock, five years later there is not impact on paid work for those women who began receiving the transfer in 2009 in comparison to those who never received it. Third, as part of the adjustment of these households it is found that, five years later, those women who lost the cash transfer are more likely to be married in 2014 than those who continued to receive the transfer (2 percentage points) and the women are "winners" are less likely to be married in 2014 compared to those who have never received this income (4 percentage points). This results link outcome in the labor market with variables of family composition.

Appendix

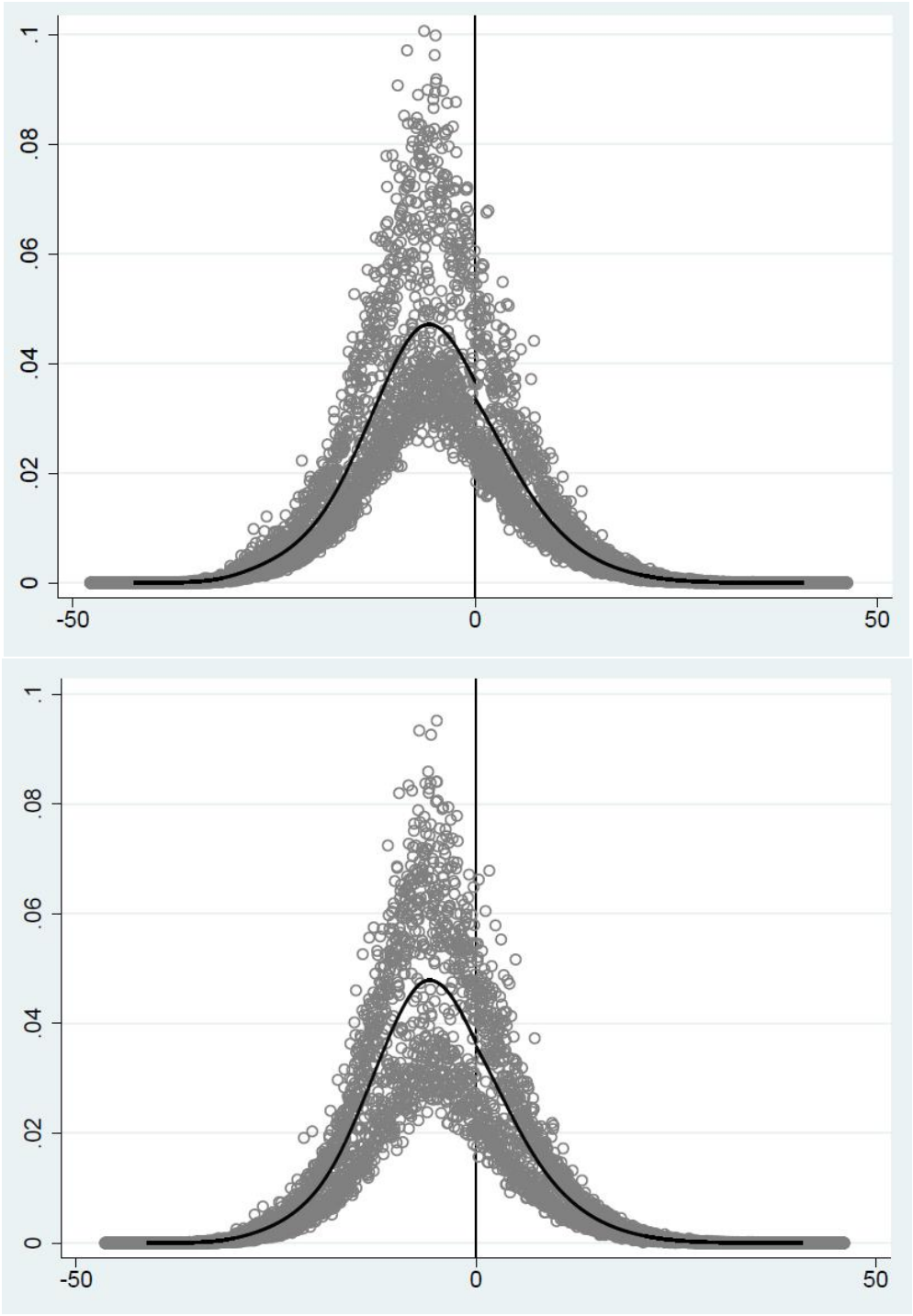
A1. Frequency distribution of Selben I y II (households). McCrary test.



Note: The figures are generated using the full SELBEN I and II database (1,962,417 and 2,718,562 households, respectively). The cutoff is normalised to zero.

(McCrary test: -0.0045 with standard error of 0.0047 in Selben I and -0.00105 with standard error of 0.0056 in Selben II). In the sample of selben 1 only the households whose information was collected before 2005 are considered.

A2. McCrary test in merger databases between SelbenI and SelbenII. Is the random attrition in the merged database?. Entire sample and INEC sample.



Note: The figures are generated using the merged databases between SELBEN I and SELBEN II (Entire Sample 1,498,433 and INEC Sample 1,091,208 households). The cutoff is normalised to zero.

(McCrary test: -0.08716 with standard error of 0.00578 in Entire Sample and -0.016 with standard error of 0.0067 in INEC Sample)

A3. McCrary test in merger databases (Selben I, Selben II and Selben III) used in the analysis of this paper: Sample of women aged 18-64 in 2014, +/-5 points around Selben II eligibility threshold.

	All	Always Winners-Losers	Winners-Always losers
Discontinuity estimate	-0.0277	-0.0165	-0.0225
s.e.	(0.0167)	(0.0237)	(0.0372)
Unmarried women in the baseline			
Discontinuity estimate	-0.0134	-0.0021	-0.0770
s.e.	(0.0323)	(0.0356)	(0.0548)
Married women in the baseline			
Discontinuity estimate	-0.0231	-0.0422	0.0014
s.e.	(0.0226)	(0.0317)	(0.0408)

A4. First Stage. The households received at least one payment between 2009 and 2012.

	Whole sample	Always winners vs Losers	Always Losers vs Winners	
Eligible for transfer	0.8895***	0.9479***	0.7897***	+/-1
	(0.0058)	(0.0041)	(0.0115)	
Eligible for transfer	0.8890***	0.9482***	0.7911***	+/-1.5
	(0.0051)	(0.0038)	(0.0091)	
Eligible for transfer	0.8897***	0.9509***	0.7867***	+/-2
	(0.0050)	(0.0041)	(0.0088)	
Eligible for transfer	0.8897***	0.9508***	0.7844***	+/-2.5
	(0.0045)	(0.0034)	(0.0083)	

Regressions of actual transfer collection on an eligibility dummy. The controls are: score (the SELBEN ii score) , eligibility status and score* eligibility status. Selben II score centered at zero. Results using score around the cutoff (+/-1, +/-1.5,+/-2, +/-2.5 points). Standard errors are in parentheses, clustered by canton.

A5. LATE: Results on Labor Market and Probability of being married in 2013-2014. Always Winners vs Losers.

Always Winners vs Losers (Women aged 18-64 years in 2014)													points
Dependent Variable	Labor Participation		Paid Work		Salaried Work		Independent Work		Unpaid Work		Married		around the
polynomial order	1st.	2nd.	1st.	2nd.	1st.	2nd.	1st.	2nd.	1st.	2nd.	1st.	2nd.	threshold
Eligible	-0.0157	-0.0141	-0.0035	-0.0061	0.0114	0.0054	-0.0149	-0.0115	-0.0053	-0.0019	-0.0267**	-0.0276**	
	(0.0143)	(0.0153)	(0.0140)	(0.0155)	(0.0111)	(0.0116)	(0.0119)	(0.0135)	(0.0054)	(0.0053)	(0.0112)	(0.0121)	+/-1
N	18,325	18,325	18,325	18,325	18,325	18,325	18,325	18,325	18,325	18,325	18,325	18,325	
Eligible	0.0061	0.0096	0.0124	0.0156	0.0173*	0.0190**	-0.0049	-0.0034	-0.0036	-0.0041	-0.0241**	-0.0235**	
	(0.0124)	(0.0129)	(0.0120)	(0.0120)	(0.0091)	(0.0092)	(0.0097)	(0.0092)	(0.0044)	(0.0044)	(0.0110)	(0.0105)	+/-1.5
N	27,531	27,531	27,531	27,531	27,531	27,531	27,531	27,531	27,531	27,531	27,531	27,531	
Eligible	0.0107	0.0129	0.0162	0.0186*	0.0146*	0.0168**	0.0016	0.0018	-0.0035	-0.0038	-0.0253**	-0.0256***	
	(0.0109)	(0.0116)	(0.0103)	(0.0110)	(0.0076)	(0.0076)	(0.0088)	(0.0087)	(0.0039)	(0.0039)	(0.0099)	(0.0094)	+/-2
N	36,447	36,447	36,447	36,447	36,447	36,447	36,447	36,447	36,447	36,447	36,447	36,447	
Eligible	0.0159*	0.0171**	0.0166**	0.0180**	0.0087	0.0109	0.0079	0.0072	-0.0003	-0.0002	-0.0260***	-0.0269***	
	(0.0082)	(0.0087)	(0.0081)	(0.0084)	(0.0069)	(0.0066)	(0.0089)	(0.0087)	(0.0029)	(0.0031)	(0.0088)	(0.0088)	+/-2.5
N	45,498	45,498	45,498	45,498	45,498	45,498	45,498	45,498	45,498	45,498	45,498	45,498	

* p<0.1; ** p<0.05; *** p<0.01. Coefficients are from IV - regressions of transfer receipt on the outcome variable. Dependent variable are: Labor Participation (=1 if individuals work or unemployment, 0 otherwise), Paid work (=1 if individuals work for pay, 0 otherwise), Salaried work (=1 if the individual works as employees, 0 otherwise), Independent work (=1 if individual works as a self-employed or employer, 0 otherwise), Unpaid work (=1 if individual is unpaid worker, usually work in the countryside or in the city when it is unpaid assistant, 0 otherwise), Married (=1 if the individual is united or married, 0 otherwise). Covariates in baseline: eligibility status, score, score*eligibility status interaction term, age, age squared, a dummy for head of household, marital status, years of education, province dummies in baseline. And a dummy for the year in which the information was gathered (2013-2014). Selben II score centered at zero. Results using score around the cutoff (+/-1, +/-1.5,+/-2, +/-2.5 points). Standard errors are in parentheses, clustered by canton.

A6. LATE:Results on Labor Market and Probability of being married in 2013-2014. Winners vs Always Losers.

Winners vs Always Losers (Women aged 18-64 years in 2014)													points
Dependent Variable	Labor Participation		Paid Work		Salaried Work		Independent Work		Unpaid Work		Married		around the
polynomial order	1st.	2nd.	1st.	2nd.	1st.	2nd.	1st.	2nd.	1st.	2nd.	1st.	2nd.	threshold
Eligible	0.0262	0.0136	0.0087	-0.0084	-0.0003	-0.0074	0.0090	-0.0009	0.0228***	0.0227***	-0.0436*	-0.0349	
	(0.0221)	(0.0254)	(0.0255)	(0.0281)	(0.0217)	(0.0215)	(0.0189)	(0.0207)	(0.0079)	(0.0077)	(0.0246)	(0.0240)	+/-1
N	9,276	9,276	9,276	9,276	9,276	9,276	9,276	9,276	9,276	9,276	9,276	9,276	
Eligible	0.0096	0.0106	-0.0007	-0.0000	0.0012	-0.0002	-0.0019	0.0002	0.0112*	0.0104*	-0.0513***	-0.0469***	
	(0.0152)	(0.0152)	(0.0158)	(0.0161)	(0.0189)	(0.0189)	(0.0166)	(0.0157)	(0.0058)	(0.0057)	(0.0171)	(0.0163)	+/-1.5
N	13,879	13,879	13,879	13,879	13,879	13,879	13,879	13,879	13,879	13,879	13,879	13,879	
Eligible	0.0096	0.0106	-0.0007	-0.0000	0.0012	-0.0002	-0.0019	0.0002	0.0112*	0.0104*	-0.0513***	-0.0469***	
	(0.0152)	(0.0152)	(0.0158)	(0.0161)	(0.0189)	(0.0189)	(0.0166)	(0.0157)	(0.0058)	(0.0057)	(0.0171)	(0.0163)	+/-2
N	13,879	13,879	13,879	13,879	13,879	13,879	13,879	13,879	13,879	13,879	13,879	13,879	
Eligible	0.0099	0.0106	0.0039	0.0051	0.0035	0.0033	0.0004	0.0018	0.0075	0.0062	-0.0458***	-0.0468***	
	(0.0149)	(0.0143)	(0.0141)	(0.0143)	(0.0144)	(0.0140)	(0.0153)	(0.0144)	(0.0049)	(0.0047)	(0.0141)	(0.0140)	+/-2.5
N	18,628	18,628	18,628	18,628	18,628	18,628	18,628	18,628	18,628	18,628	18,628	18,628	

* p<0.1; ** p<0.05; *** p<0.01. Coefficients are from IV - regressions of transfer receipt on the outcome variable. Dependent variable are: Labor Participation (=1 if individuals work or unemployment, 0 otherwise), Paid work (=1 if individuals work for pay, 0 otherwise), Salaried work (=1 if the individual works as employees, 0 otherwise), Independent work (=1 if individual works as a self-employed or employer, 0 otherwise), Unpaid work (=1 if individual is unpaid worker, usually work in the countryside or in the city when it is unpaid assistant, 0 otherwise), Married (=1 if the individual is united or married, 0 otherwise). Covariates in baseline: eligibility status, score, score*eligibility status interaction term, age, age squared, a dummy for head of household, marital status, years of education, province dummies in baseline. And a dummy for the year in which the information was gathered (2013-2014). Selben II score centered at zero. Results using score around the cutoff (+/-1, +/-1.5,+/-2, +/-2.5 points). Standard errors are in parentheses, clustered by canton.

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