The London School of Economics and Political Science

Social Policies in Ecuador: The Effects of Minimum Wages and Cash Transfers

Wilson Guzman

A Thesis Submitted to the Department of Social Policy of the London School of Economics and Political Science for the Degree of

Doctor of Philosophy

London, October 2018

Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I declare that my thesis consists of 53,153 words.

Statement of use of third party for editorial help

I can confirm that Chapters 2, 3 and 4 of this thesis were copy edited for conventions of language, spelling and grammar. Chapter 2 was edited by Joey Ransdell and Chapters 3 and 4 were edited by Jonathan Richardson.

Wilson Guzman

Acknowledgements

I owe my deepest gratitude to my supervisors, Stephen Jenkins and Berkay Özcan, for their invaluable support and guidance.

To my wife and my sons

Abstract

Minimum wages and cash transfer (CT) programmes are two major social protection policies in developing countries aimed at alleviating poverty and redistributing income. While a lot of attention, and debate, has been put in place around the establishment of these policies, little attention has called the issue of how these policies have been implemented in practice. In the first part of this thesis, I evaluate the effects of the national minimum wage (NMW) policy of Ecuador where apart from registering increases in the legislated level of the minimum wage there are also increases in the intensity of its enforcement. In the second part, I evaluate the impact of the largest CT programme in the country, the *Bono de Desarrollo Humano* (BDH), which has no enforced conditionalities attached to it.

In Part I, I provide evidence of the effects of increases in the two components of the NMW on employment, wages and monetary poverty (Chapters 2), and on wage inequality (Chapter 3). The findings go in line with the predictions of the traditional competitive two-sector model.

In Chapter 2, I find that the increases in the two components of the NMW policy increased wages of male covered workers and reduced the probability of remaining employed in the covered sector for the less-skilled workers, the labourers. The increase in wages for male covered workers, who remained employed in the covered sector, reduced the probability of being poor for this group of workers and their families. I find that most of the labourers who lost their jobs in the covered sector migrated to the uncovered sector and others became unemployed. Additionally, I find that the increase in labour supply in the uncovered sector reduced the earnings of uncovered self-employed individuals.

In Chapter 3, I find that the increases in the two components of the NMW policy reduced wage inequality by increasing the wages of workers located up through the 60th percentile of the wage distribution. Additionally, I estimate that the increases in the two components of the NMW account for approximately 50% of the reduction in wage inequality we observe for the period 2000-2016.

In Part II, Chapter 4, I analyse the effects of the BDH on the components of food expenditure and find that the programme increased food expenditure on protein-rich products such as meat, chicken, milk, eggs, etc. Additionally, I find that the beneficiary families with under-5s spend significantly more on this kind of products and significantly less on sugar and sweets, and on meals outside the house. However, unlike other conditional CT programmes in the region, I did not find that the BDH increased expenditure on fruits and vegetables.

Table of Contents

List of figures

List of tables

List of abbreviations[1]

ATE Average Treatment Effect

BDH Bono de Desarrollo Human of Ecuador

CT Cash Transfer

CCT Conditional Cash Transfer

CONADES National Council of Salaries of Ecuador

ECLAC Economic Commission for Latin America and the Caribbean

ENEMDUR National Survey of Employment, Unemployment and Underemployment of Ecuador

ENEMDU National Urban Survey of Employment, Unemployment and Underemployment of Ecuador

IESS Ecuadorian National Security System

INEC National Institute of Statistics and Census of Ecuador

ITT Intention to Treat

NMW National Minimum Wage

OLS Ordinary Least Squares

RDD Regression Discontinuity Design

UCT Unconditional Cash Transfer

[1] Some acronyms correspond to official names in Spanish. The first time each term is mentioned in the text, the full name in Spanish is also provided.

1 Introduction

1.1 Social Protection in Latin America

Latin America remains as one of the two most unequal regions in the world (Lakner and Milanovic 2013)[2]. In the last decade, however, inequality fell in most Latin America nations (Cornia 2014). A number of studies argue that a significant part of the decline in inequality and of the reduction in absolute poverty, in the region, is explained by an expansion of the social protection schemes (Ribe et al. 2010).

Social protection includes contributory social insurance schemes as social security systems and non-contributory social assistance programmes as cash transfers (see Figure 1.1). The goals of these, contributory and non-contributory, social protection schemes is to protect individuals from felling into poverty; help those in poverty lift out of it; and, to redistribute income in favour of low-income individuals and families. Under what conditions can these protections tools accomplish these objectives? Are those conditions met in developing countries with weak institutional capabilities to enforce compliance with the regulations imposed by these policies? Who pays for poverty alleviation and redistribution? Who benefits from these policies?

In most countries of the region, employers of salaried workers (or wage workers) are required by law to register their employees to the social security system. In theory, affiliation of workers to the social security system implies that workers get paid at least the minimum wage, have a retirement pension, and received other benefits (see Figure 1.1). This system is commonly designed to be self-financing; that is, revenues come from a payroll tax that is partly paid by employees and partly paid by their employers.[3]

On the other hand, non-contributory social assistance programmes as cash transfers require the financing of the government, via the general tax system, or the economic support of other non-governmental institutions. Cash transfer programmes are usually targeted to the poor and take the form of child benefits, non-contributory social pensions for the elderly, disability pensions, and other programmes (see Figure 1.1). This kind of programmes spread widely in the region in the 2000s.

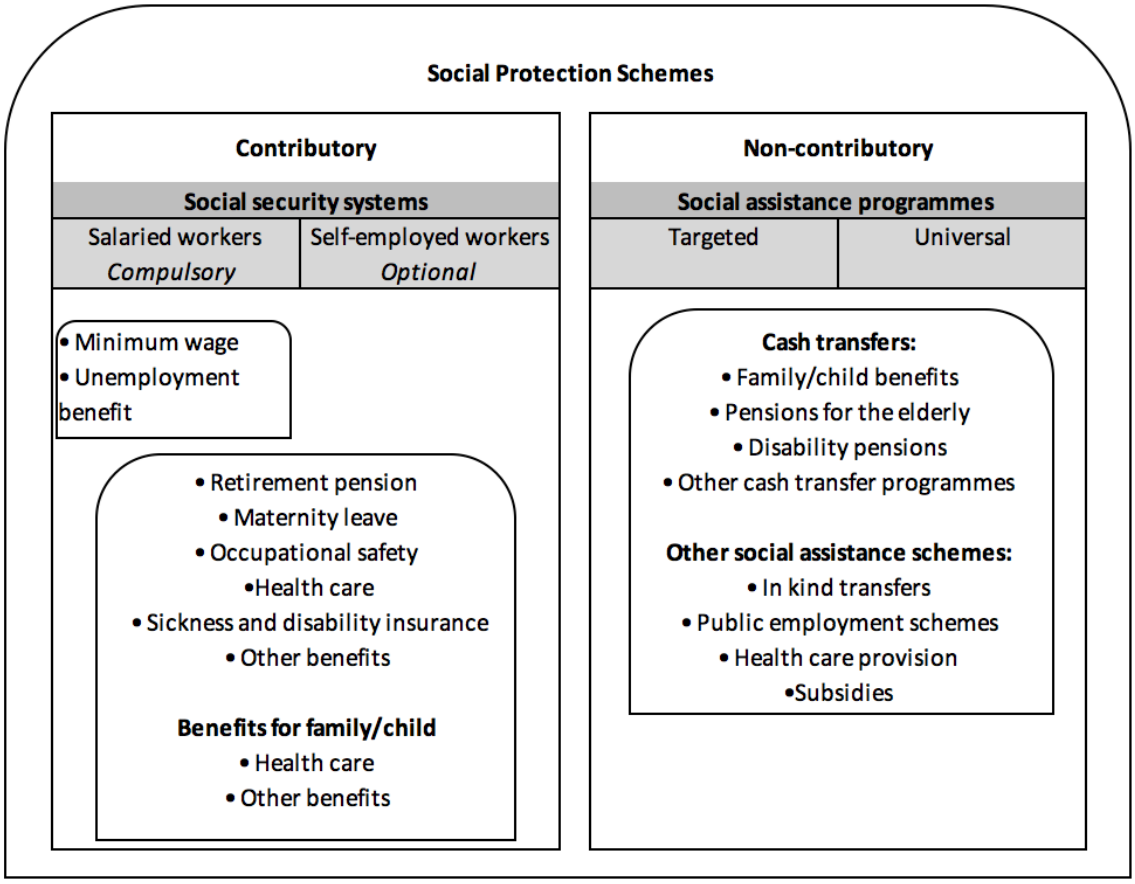
---

[2] The other most unequal region is Sub-Saharan Africa.

[3] In this thesis, I use the terms participation, affiliation and contribution as synonyms for referring to a worker that is registered in the social security system, and he and his employers made their contribution to the system.

In an increasing number of Latin American countries, social security systems coexist with non-contributory cash transfer programmes. Contributory systems have low coverage in these countries as they only cover salaried workers, for whom participation is compulsory. Additionally, not all salaried workers are affiliated to the social security system. In particular, salaried workers working in small establishments (with 5 or fewer employees) are significantly less likely to be affiliated than workers in larger firms (Auerbach et al. 2007). This dual nature of the labour markets in the region limits the scope of the social security system as a social protection tool for all workers and their families. This limitation is precisely the main driver of the rise of cash transfers programmes as a tool for social protection.

Figure 1.1 Social Protection Schemes in Latin America



Source: Author's illustration.

In Table 1.1, I show the proportion of workers that are unskilled self-employed workers and unpaid (family and non-family) workers. The proportion of unskilled self-employed individuals ranges from 17 percent, as in Argentina, Costa Rica and Chile, to around 30 percent or more, as in Bolivia, Colombia, Dominican Republic, Ecuador,

Honduras, Paraguay, Peru and Venezuela. This group of workers are not covered by the social security system because they cannot enforce themselves to contribute to the system and to paid themselves the minimum wage.

Table 1.1 The Structure of the Labour Market in Selected Latin American Countries

	Formal				Informal		
		Salaried workers					
	Entrepre-neurs	Large firms	Public sector	Self-employed profession-als	Salaried in small firms	Self-employed unskilled	Workers with zero income
Argentina	3.7	35.4	18.6	3.8	21.2	16.9	0.5
Bolivia	4.4	14.3	8.7	2.6	12.8	38.9	18.3
Brazil	3.7	35.1	12.3	2.1	19.6	20.8	6.4
Chile	2.9	50.8	12.3	3.2	12.7	17.6	0.5
Colombia	4.1	29.4	3.9	3.6	15.5	39.0	4.6
Costa Rica	4.3	41.4	14.6	1.4	19.9	17.2	1.2
Dominican Rep.	3.2	29.9	13.8	2.2	11.7	37.8	1.4
Ecuador	3.3	26.6	9.5	2.1	19.3	28.6	10.6
El Salvador	4.3	31.0	7.5	0.8	19.8	27.3	9.3
Guatemala	2.6	28.6	5.3	0.3	24.0	24.9	14.2
Honduras	9.6	17.9	6.1	0.5	19.8	31.6	14.3
Mexico	8.4	35.3	10.1	1.0	26.6	12.7	5.9
Panama	3.0	37.5	15.5	1.6	12.5	24.5	5.3
Paraguay	5.1	21.2	11.4	1.9	21.5	29.8	9.1
Peru	4.0	21.4	8.6	3.3	14.8	31.7	16.1
Uruguay	4.2	43.1	14.7	2.5	14.7	19.9	0.9
Venezuela	4.5	29.0	16.6	2.3	12.6	33.4	1.6

Source: Author's elaboration on the basis of SEDLAC (CEDLAS and The World Bank) data.
Note: data are from 2014-15. Only the data from Venezuela is from 2006, the last available information.

While self-employed workers are not obligated to contribute to the social security system, in the region, there is also a significant proportion of (covered) salaried workers that are not affiliated to the system. This issue derives from noncompliance with labour regulations, which is widespread in the region, as well as in other developing countries (see Marshall 2007; Rani et al. 2013; Ronconi 2015). To appreciate the magnitude of this issue, in Table 1.2, I show compliance rates based on affiliation of salaried workers to social security for years 2005 and 2015. In 2005, compliance rates ranged from a maximum of 65%-80%, as in Brazil, Chile, Costa Rica, and Uruguay, to a minimum of 29%-34%, as in Bolivia, Ecuador, Nicaragua, Paraguay and Peru. One decade after, in 2015, two countries

register an increase in compliance of more than 20 p.p.; in Dominican Republic compliance increased from 47 to 85 percent, and in Ecuador compliance increased from 33 to 59 percent. Finally, in 2015, seven out of fifteen countries included in Table 1.2 have compliance rates below 50 percent (Bolivia, El Salvador, Guatemala, Mexico, Nicaragua, Paraguay, and Peru).

Table 1.2 Share of Salaried Workers Affiliated to Social Security in Selected Latin American Countries

	2005			2015			Absolute change 2015-2005
	Total	Gender		Total	Gender		
		Female	Male		Female	Male	Total
Argentina	52.8	49.8	63.5	67.4	68.6	74.6	14.5
Bolivia	32.7	46.4	34.9	40.6	50.7	46.9	7.9
Brazil	66.7	69.6	73.8	77.8	80.1	82.1	11.2
Chile	79.8	77.4	85.8	82.5	83.7	87.4	2.7
Colombia	—	—	—	63.1	66.0	70.4	—
Costa Rica	67.8	69.1	75.8	70.0	67.7	76.6	2.2
Dominican Rep.	46.7	57.2	47.7	77.5	84.7	80.4	30.8
Ecuador	33.1	44.5	37.5	58.5	70.2	59.3	25.4
El Salvador	52.5	74.2	51.8	48.4	57.3	52.1	-4.1
Guatemala	36.4	46.7	40.3	29.2	40.5	31.5	-7.2
Mexico	39.4	46.2	42.3	38.3	42.5	43.0	-1.1
Nicaragua	33.9	49.9	34.5	40.9	58.1	41.0	7.0
Paraguay	29.0	41.2	36.1	38.6	46.7	46.0	9.6
Peru	30.0	35.2	41.1	47.0	51.4	55.9	17.0
Uruguay	73.8	75.0	81.6	88.1	89.9	92.2	14.3
Venezuela	60.3	69.2	64.7	—	—	—	—

Source: Author's elaboration on the basis of SEDLAC (CEDLAS and The World Bank) data.
Note: the data points for 2015 come from this year for most countries, the exception is Chile, with data from 2006, and Guatemala, with data from 2004. Similarly, the data points for 2015 come from this year for most countries, the exception is Guatemala, Mexico, and Nicaragua with data from 2014.

Informal self-employed workers and a significant fraction of salaried workers, especially those working in small establishments, are not protected by the social security systems of their countries. It is fundamental to acknowledge the difference between these two groups of workers. Informal self-employed workers are not protected by the contributory social security system because the legislation does not protect them or leaves it

as optional. On the other hand, “informal” salaried workers are not protected by social security because employers and employees are not complying with the labour law. Additionally, their classification as “informal” employees, using the definition of informal employment as proposed by the International Labour Organization (ILO, see Hussmanns 2004 for a review of the definitions), derives from the fact that for these employees their employment relationship is in practice, but not in law, not subject to national labour legislation.

This distinction is important conceptually, as well as for policymaking. For informal self-employed workers, and for economically inactive individuals, CT programmes serve to protect them, and their families, because otherwise these individuals will not have access to a safety net. In contrast, CT programmes are also helping to protect informal salaried workers whose employers do not comply with the labour regulations and do not pay them at least the minimum wage. As shown in Table 1.3, families with a head of household working as a self-employed individual (including farmers), or as a salaried worker in small establishments where noncompliance with labour legislation is common, represent the largest fraction of beneficiaries of cash transfer programmes in the region. Another group with a large fraction is composed by beneficiary families with an economically inactive household head. In countries as Argentina, Chile, and Ecuador, this group represent between 19-23 percent of all beneficiary households.

The problem of noncompliance with labour regulations is the result of weak enforcement. There are three main reasons that explain this low intensity of enforcement. First, legislators usually pass the law but put little attention and efforts on the mechanisms that will be used to enforce the labour regulations they just imposed. This issue, I argue, responds to the political economy involved around labour regulations, mainly those related with the minimum wage law. While in developed countries there is a conflict between unions that promote minimum wage legislation and employer organizations that try to impede it. In countries in the region with a weak institutional capability, this conflict is less relevant since legislators can satisfy both constituencies. They can pass the law in line with workers wishes and then do nothing to enforce it in line with some employers’ interest.

Second, policy makers usually take decisions based on the cycles of the economy. They can push an increase in the intensity of enforcement of labour regulations during periods of expansion (growth) with less risk of employment losses in the covered sector. In contrast, policy makers, and the institutions in charge controlling compliance with labour regulations, will be less prone to increase the intensity of enforcement during periods of

economic contraction. What is more, during recession, they might turn a blind eye to noncompliance with the labour code, especially in small production units. They want to prevent covered workers from losing their jobs and migrating to the uncovered sector or becoming unemployed.

The third reason is the weak institutional capabilities of the institutions in charge of enforcing labour regulations. As countries develop there is an organic emergence of contract-enforcement institutions as result of the pursuit of individual interest (Greif 2005). The role of these institutions is to enforce contracts with a credible commitment of imposing sanctions to agents that do not comply with the law. However, these institutions may be limited by the lack of human and financial resources, or subject to corruption (S. Lee et al. 2014). More importantly, these weak contract-enforcement institutions may be the result of prevailing coercion-constraining institutions (Greif 2005) that are more in line with extractive institutions rather than with inclusive ones (Acemoglu and Robinson 2012).

Given the problem of noncompliance in the region, when analysing the performance of a policy or programme it is necessary to study the effects of the *effective* regulation, i.e., the combination of the of written laws, codes, rules, and conditions, and the state of enforcement efforts to achieve compliance with them. The issue of noncompliance in Latin America is so relevant that any empirical work that analyses the effects of *de jure* regulations and conditions imposed by a policy will provide a misleading picture of the effects of this policy.

The objective of this thesis is to evaluate the impact of the *effective* regulation of two of the major protection tools available in countries in the region: the minimum wage and the cash transfer programme. For this purpose, I use the case of Ecuador. This country provides a suitable setting for conducting this research.

Concerning the minimum wage policy, Ecuador is one of the few countries where the government apart from increasing the real value of its national minimum wage (NMW), it also increased the intensity of enforcement of the labour law, which includes compliance with NMW. These policy changes allow me to identify the effects of the minimum wage policy when there are increases in its two main components: the level of the minimum wage and the intensity of its enforcement.

Table 1.3 Characteristics of CTs Beneficiary Households in Selected Latin American Countries

Country	ARG	BRA	CHL	COL	CRI	ECU	GUA	MEX	PAN	PER	PRY	URU
Year	2010	2009	2009	2010	2010	2010	2011	2010	2008	2010	2010	2010
Program	FIS+AUH	BF	CS	FA	AV	BDH	MFP	Oportunidades	RDO	Juntos	Tekoporá	AF
Urban share	100.0	68.2	74.4	73.7	45.4	38.1	19.9	39.9	3.3	5.3	8.6	94.3
Family size	4.9	4.2	4.6	4.8	4.9	4.4	6.3	4.9	6.4	6.2	5.2	4.7
# of children (<12)	1.9	1.7	1.6	1.9	1.1	2.0	3.0	2.1	3.2	3.3	2.4	1.9
Household head												
Age (years)	43.4	41.5	45.5	42.7	45.9	55.3	41.2	48.7	46.7	45.7	47.7	42.9
% Male-headed	66.1	66.7	63.5	68.4	65.0	73.0	85.5	77.5	82.7	83.6	82.1	61.0
Education												
Years of schooling	8.2	4.7	7.9	5.2	6.4	4.5	2.4	4.3	4.0	4.7	4.1	6.9
% none-primary	54.2	81.9	58.1	65.8	68.7	84.2	93.6	78.4	87.6	91.4	77.0	54.8
% some/compl. secondary	38.4	14.2	39.7	30.3	28.3	13.8	6.2	20.6	12.2	8.6	20.9	43.5
% some/compl. tertiary	7.4	4.0	2.3	4.0	3.0	1.9	0.2	1.0	0.2	0.0	2.2	1.7
Labour market status												
% Inactive	19.0	16.4	23.0	15.8	16.6	22.4	7.2	9.6	12.2	1.5	15.3	14.0
Unemployed	5.5	5.3	6.7	3.6	4.5	1.3	1.5	0.3	1.0	0.0	0.9	4.5
Salaried formal	15.7	22.0	22.9	8.4	35.2	7.2	7.9	3.0	10.8	0.6	1.6	36.1
Salaried informal	34.7	22.2	19.5	20.2	19.3	26.9	39.8	40.5	17.9	5.5	4.8	17.1
Unpaid worker	0.0	4.7	0.1	0.8	0.2	0.4	0.5	0.5	2.4	0.2	0.4	0.2
Self-employed	21.2	16.2	20.1	33.6	15.3	14.1	8.1	11.5	6.0	4.1	3.2	23.2
Farmer	1.3	11.8	5.1	16.6	8.3	27.1	32.2	34.4	49.3	88.1	72.8	3.8
Employer	2.5	1.3	0.6	1.1	0.6	0.5	0.6	0.2	0.0	0.1	1.1	1.1

Source: Author's elaboration on the basis of Stampini and Tornarolli (2012).
Notes: FIS = Familias por la Inclusión Social, AUH = Asignación Universal por Hijo, BF = Bolsa Familia, CS = Chile Solidario, FA = Familias en Acción, AV = Avancemos, BDH = Bono de Desarrollo Humano, MFP = Mi Familia Progresa, RDO = Red de Oportunidades, AF = Programa de Asignaciones Familiares.

Regarding the CT programme, unlike most programmes in the region, the Ecuadorian CT, *Bono de Desarrollo Humano* (BDH), is an unconditional cash transfer (UCT) programme as their conditions were never enforced by its administrators. This characteristic of the BDH allows me to identify the effects of a CT programme with *de jure* conditions and compare the findings with the effects generated by other conditional cash transfer (CCT) programmes with monitored and enforced conditions.

A social security scheme as the minimum wage has the objective of increasing the income of low-paid salaried workers to provide them, and their families, a minimum standard of living, which includes access to adequate food and other basic needs. However, focusing on analysing the impact of changes in the legislated level of the minimum wage is not enough to capture the effects of this policy. What if the government increases the level of the minimum wage, in real terms, but makes no efforts on enforcing it? Should we expect that this policy will benefit low-paid salaried workers, acknowledging that noncompliance with the minimum wage law is particularly large among this group of workers? What if the government increases the intensity of enforcement of the labour law? Should we expect that the increased enforcement will increase wages of low-paid salaried workers, reduce employment in the covered sector, and compress the wage distribution?

Similarly, cash transfer programmes targeted to the poorest households have yet to achieve their full potential in terms of poverty alleviation and accumulation of human capital (Ribe et al. 2010). This is particularly important for countries in the region considering that CT programmes are a core social assistance tool. CT programmes usually impose conditions to beneficiary families regarding investment in children's human capital, such as sending them to school or bringing them to health centres on a regular basis. However, these conditions may not be truly enforced in practice. What if the government introduces a CT programme, promotes it as a CT with conditions attached to it, but turns a blind eye to the enforcement of the conditions? Should we expect UCTs to generate the same results in terms of their impact on the accumulation of human capital as CCTs?

1.2 The Minimum Wage Policy

Most countries in the region state the provision of a minimum wage in their constitutions as part of their social security (Alaimo et al. 2015 report that 17 out of 18 Latin American and the Caribbean countries included in their study have a minimum wage provision). Policy makers who may want to protect low-paid workers can increase the value of the minimum wage and

they can also increase the intensity of its enforcement. Increases in one or both components have the potential to alleviate poverty and redistribute income by providing a wage floor for low-paid workers. However, these changes can also reduce jobs in the covered sector and increase unemployment, by adding rigidities in the labour market.

Most countries have experienced changes in the legislated level of the minimum wage. In some countries the level of the minimum wage is particularly high. For example, the level of the minimum wage in Costa Rica, Ecuador, Guatemala, Nicaragua, Panama, Paraguay and Peru, represents more than 70 percent the average monthly wage (see Table 1.4). There are also significant differences in the level of the minimum across countries. For instance, the level of the monthly legal minimum (measured in USD, constant 2011, PPP) in Argentina, Costa Rica, Panama and Paraguay is more than two times higher than in Dominican Republic and Mexico (see Table 1.4). On the other hand, very few countries, as in the case of Ecuador and Costa Rica, have implemented programmes to increase the intensity of enforcement of the minimum wage legislation. What is more, in the case of Ecuador the government also hardened the labour law in order to reduce noncompliance with the minimum wage.

In theory, a well enforced minimum wage law, as predicted by the traditional competitive two-sector model (Harris and Todaro 1970; Welch 1974; Gramlich 1976; Mincer 1976), will reduce employment in the covered sector and some workers will migrate to the uncovered sector or to unemployment. Therefore, although it may be desirable to increase the levels in both components of the minimum wage policy to achieve its social objectives, the increases may generate job losses in the covered sector. On the other hand, under an imperfectly competitive labour market, as the monopsony (Stigler 1946), the minimum wage policy will increase wages without reducing employment. This result will hold only if the level of the minimum is set between the competitive wage and the inferior non-competitive wage imposed to workers by their monopsony employers. In this scenario, the minimum wage will be an effective policy tool to increase wages of low-paid workers by redistributing income from employers to employees.

One argument often made is that minimum wages only benefit covered workers living in non-poor households. The evidence for some developing countries, however, shows that a relatively high fraction of workers covered by the minimum wage law belongs to poor households. For example, in Ecuador, I estimate that more than 30 percent of covered workers earning below the NMW lived in poor household in 2010. Similarly, in Indonesia, more than 45 per cent of low-wage covered workers lived in poor households (Bird and Manning 2008),

and in Honduras, 71 per cent of minimum wage earners lived in poor households (Gindling and Terrell 2010).

Table 1.4 Minimum Wages in Selected Latin American Countries

	Minimum legal monthly wage (local currency)	Minimum legal monthly wage, PPP (constant 2011 international $)	Minimum legal monthly wage (% of average monthly wage in the main occupation)
Argentina	4,133	708	67.99
Bolivia	1,440	423	59.27
Brazil	724	366	42.29
Chile	241,000	569	52.01
Colombia	616,000	475	66.25
Costa Rica	303,313	767	73.24
Dominican Rep.	6,880	296	46.61
Ecuador	340	555	72.69
El Salvador	197	358	64.63
Guatemala	2,249	518	91.94
Honduras	7,311	617	100.00
Mexico	1,946	193	34.26
Nicaragua	4,198	376	74.92
Panama	527	845	84.37
Paraguay	1,824,055	706	82.37
Peru	750	434	82.19
Uruguay	8,960	426	43.99

Source: Author's elaboration on the basis of the Labor Markets and Social Security Information System (The SIMS - IADB).
Note: data are from 2014. Only the data from Chile is from 2015.

In the debate, supporters of this policy advocate for the establishment of minimum wages as a way to contrast the inequality of bargaining power between workers and employers. They argue that imperfect labour markets with a lopsided distribution of resources and rights put employers in a dominant position in wage bargaining and the individual workers in a weaker and dependent position (Kaufman 2010).

On the other hand, supporters of the free labour market argue that the establishment of labour regulation leads to a loss of efficiency. The government is preventing workers and employers from freely negotiating employment contracts. A flexible labour market that reduces or eliminates the weight of labour regulations reduces the price of labour and induces firms to hire more people. All sides gain from free negotiation and this results in an increase in welfare.

Finally, those who are against this policy often argue that a non-enforced labour regulation can be regarded as an optimal option, as this in practice is equivalent to non-having any government intervention in the labour market.

The empirical literature on the effects of minimum wages has mostly focused on analysing the effects of changes in the level of the minimum wage. The results are mixed. While some studies find that the policy increases wages of low-paid workers and generates job losses, others find no effect on wages and employment (see Cunningham 2007; and Neumark and Wascher 2007; for reviews of the empirical literature). In most cases where authors find no effects of changes in the level of the minimum wage, they argue that this is due to the problem of noncompliance with the law.

There is a gap in the literature regarding the effects of changes in the intensity of enforcement of the minimum wage law on the labour market. Very few studies have analysed the impact of changes in this component of the minimum wage policy. See Gindling et al. (2015), for Costa Rica; Almeida and Carneiro (2009), and Almeida and Carneiro (2012), for Brazil; Ronconi (2010), for Argentina; Harrison and Scorse (2010), for Indonesia; and Soundararajan (2014), for India. Among these studies, only the analysis for the cases of Costa Rica (Gindling et al. 2015) and Indonesia (Harrison and Scorse 2010) measure the effects of an exogenous increase in the intensity of enforcement. Similar to these two cases, I exploit the exogenous increase in the intensity of enforcement of the Ecuadorian national minimum wage policy and analyse its effects on labour market. In Part I of this thesis, I contribute to this literature.

1.3 Cash Transfers Programmes

In developing countries with a large fraction of uncovered workers, who are not protected by the minimum wage policy, cash transfer (CT) programmes complement the role of the contributory system as a tool for poverty alleviation and redistribution. In Latin American countries this kind of programmes covered around a quarter of the population in 2010 (see Table 1.5). The number of CT programmes went from 6 in 2000 to 27 in 2010 (Cecchini and Atuesta 2017). By the end of the 2000s, most countries in the region had a CT programme implemented with conditionalities. This conditional cast transfer (CCT) programmes generally impose conditionalities to beneficiary families related to school attendance and medical check-ups of children in the household.

The countries with the largest CT programmes, in terms of relative coverage (beneficiaries/total population) are Bolivia, Ecuador, Dominican Republic, Argentina, and Brazil (see Table 1.5). In these countries, CT programmes cover more than a quarter of the population. On the other hand, in countries as Chile, Costa Rica, El Salvador, Paraguay, and Peru, CT programmes cover less than 10 percent of the population. Additionally, it is worth noticing that the largest programmes in terms of number of beneficiaries are Brazil's *Bolsa Familia*, reaching 52 million beneficiaries, followed by Mexico's *Oportunidades* with 27 million beneficiaries.

Table 1.5 Number of Beneficiaries of CT Programmes in Selected Latin American Countries

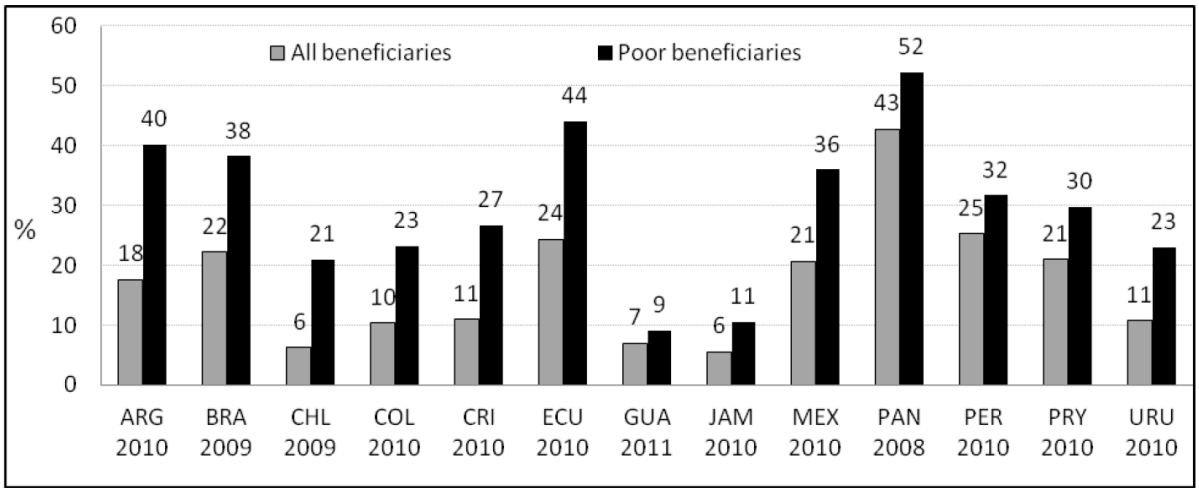
	Beneficiaries (2010, in millions)	Population (2010, in millions)	Beneficiaries / Pop.
Argentina	11.79	40.41	0.29
Bolivia	5.69	9.93	0.57
Brazil	52.39	194.95	0.27
Chile	1.30	17.11	0.08
Colombia	11.69	46.29	0.25
Costa Rica	0.19	4.66	0.04
Dominican Rep.	2.98	9.93	0.30
Ecuador	6.13	14.46	0.42
El Salvador	0.57	6.19	0.09
Guatemala	3.25	14.39	0.23
Honduras	1.07	7.60	0.14
Mexico	27.25	113.42	0.24
Nicaragua	—	5.79	—
Panama	0.36	3.52	0.10
Paraguay	0.55	6.45	0.09
Peru	2.59	29.08	0.09
Uruguay	0.76	3.36	0.23

Source: Stampini and Tornarolli (2012).

There is also great variability in the amount of the transfer across countries in the region. For instance, the transfer in Brazil, Ecuador, Mexico, Panama, Peru, and Paraguay, accounts for around 20-25 percent of the total income of beneficiary families (see Figure 1.2). Panama's *Red de Oportunidades* is the programme with the most generous transfer, it accounts for 43 percent of beneficiaries' total income. In contrast, the size of the transfer is relatively small in countries as Chile, Colombia, Costa Rica, Guatemala, Jamaica, and Uruguay. In these countries the transfer accounts for 11 percent or less of beneficiaries' total income.

The institutions in charge of the administration of CT programmes are generally the ministries or secretariats of social development or social inclusion. These institutions play an important role not only in designing the programme, targeting beneficiaries, and making the payment of the transfer, but also enforcing and controlling compliance with the conditionalities of the programme. This last responsibility, however, is sometimes not executed in practice. The administrators of the programmes may try to incentivise compliance with the conditionalities, but their success will depend of their institutional capabilities to enforce it. Additionally, they might turn a blind eye to non-complying households in order to minimize the political cost of imposing any penalties or sanctions.

Figure 1.2 Magnitude of Cash Transfers as Percentage of Recipients' Income in Selected Latin American Countries



Source: Stampini and Tornarolli (2012).
Note: ARG = Argentina, BRA = Brazil, CHL = Chile, COL = Colombia, CRI = Costa Rica, ECU = Ecuador, GUA = Guatemala, JAM = Jamaica, MEX = Mexico, PAN = Panama, PER = Peru, PRY = Paraguay, URU = Uruguay. Poor beneficiaries defined on the basis of the poverty line of USD PPP 2.5.

As highlighted by Özler (2013), CT programmes can be categorized in a spectrum from a pure UCT to a heavy-handed CCT. There can be CTs that are explicitly unconditional, CTs that have *de jure* conditions on paper, those that have *de jure* conditions and implemented a social marketing campaign to incentivise compliance with the conditions, CCTs with imperfectly monitored and minimal enforced conditions, and, in the other end of the spectrum, CCTs with well-monitored and -enforced conditions. This characterization of CT programmes is more helpful that the binary labelling of CT as CCT or UCT.

There is a debate about making or not CTs conditional. On one hand, one can attach conditionalities to a CT based on three main arguments. First, it helps direct behaviour towards

specific actions as individuals will not always behave as expected; second, conditioning on "good behaviour" may increase the support from the public, who may finance this kind of programmes through paying taxes; third, conditioning may help to achieve a social optimal investment in human capital, that otherwise will not take place (Fiszbein and Schady 2009). On the other hand, one can argue that UCT programmes allow recipients freedom of choice. If we assume individuals are informed rational agents with interest in investing in the human capital of their children, then we can expect that UCT will be an efficient way to allocate the CT. Additionally, conditionalities can be expensive to administer and ineffective in areas with an insufficient supply of services such as health care or education.

The empirical literature that compares the effects of CCTs versus UCTs shows that there are significant differences in the effects generated by these two options, mainly when the conditions attached to CCTs are well enforced and monitored. For example, in a systematic review of the effects of CCTs and UCTs on schooling outcomes, Baird et al. (2013) show that CCTs with explicit schooling conditions monitored and enforced generate substantively larger effects on school enrolment than UCTs. Similarly, in a meta-analysis, Saavedra and García (2012) find that stronger conditions were positively associated with larger secondary enrolment and attendance effects. In contrast to these two studies, Manley et al. (2012) find that when CTs have conditions attached, relating to health and education, they do not have significantly different effects on child nutritional status than UCTs. However, the authors of this last paper do not account for the differences in the intensity of enforcement of the conditionalities in the CCT programmes included in their meta-analysis.

There is a vast empirical literature of the effects of CT programmes implemented in the region, as well as in other developing countries (see Bastagli et al. 2016, for a review of the empirical literature). Most studies have focused on analysing the effects on educational and health outcomes. The results on education show, overall, that this kind of programmes incentivise school attendance, but are not conclusive in terms of improving learning outcomes and cognitive achievement. Similarly, the results of their impact on health and nutrition show, on the whole, that CTs play an important role in increasing the use of health services.

In terms of access to food and dietary diversity, CTs increased the proportion of expenditure on food as a fraction of total expenditure (the food share) in Colombia, Ecuador, Nicaragua, and Brazil (Fiszbein and Schady 2009). Additionally, the studies that report an effect of food expenditure generally find that the increases are directed toward increasing the quality of food intake. Studies for Colombia, Nicaragua, Brazil and Mexico show that the increases take place on expenditure on fruits, vegetables and products rich in proteins, such as

meat, chicken, eggs, etc. (see Hoddinott and Skoufias 2004, for *Oportunidades* in Mexico; Maluccio and Flores 2005 for *Red de Protección Social* (RPS) in Nicaragua; Attanasio and Mesnard 2006, for *Familias en Accion* in Colombia; Martins and Monteiro 2016, for *Bolsa Família* in Brazil).

For the case of Ecuador, two studies find that the BDH increases food expenditure; as a fraction of total expenditure, Schady and Rosero (2008); and in absolute value, Buser et al. (2017). The authors find, respectively, that the BDH increases the food share by about 4 percentage points, and the total amount of expenditure in food by approximately the amount of the BDH transfer, 35 USD per month. However, no study has evaluated the effects of the BDH on food expenditure patterns to identify which of the various components of food expenditure increased the most. In Part II of this thesis, I fill this gap in the literature.

1.4 The Ecuadorian Case and Thesis Contribution

The national minimum wage (NMW) law and the BDH programme are the main social protection tools in Ecuador. The primary objective of these two policies is to increase the income of low-income families to provide a minimum standard of living, which includes access to adequate food, education, health and other basic needs. By increasing the income of these families, these policies can also help to reduce income inequalities.

Achieving these goals are a central concern in Ecuador. Compared to other countries in Latin America, monetary poverty and income inequality in the country were located above the regional average by the beginning of the 2000s (see ECLAC 2012, for poverty rates; and Cornia 2014, for inequality measures). The official figures, for around 2010, show that of a population of approximately 14 million, a third lived under the poverty line; 24 percent of children under 5 years of age were affected by chronic malnutrition; and the Gini coefficient of the distribution of income was 0.51 (SIISE 2015).

In the country, the NMW policy and the BDH programme cover a significant proportion of the population. The NMW with its value located at around the median of the wage distribution, if well-enforced, can directly influence the wages of at least half of salaried workers in the economy. Meanwhile, the BDH, is the largest social protection tool. In 2010, it covered around 1.2 million households (around 32% of the total number of households, SIISE 2015). Households with an uncovered self-employed head of household represent the largest fraction among BDH recipients; for 2010, my estimate is 41 percent. Considering the scope that

these two policies have, it is mandatory to evaluate how effective they are in achieving their objectives.

The Ecuadorian Ministry of Labour is the institution in charge of controlling and enforcing the labour law, including the NMW law. The Ecuadorian Social Security Institute (Instituto Ecuatoriano de Seguridad Social, IESS) also helps the Ministry to perform this task. Since 2010, the Ministry increased significantly the intensity of enforcement of the labour law. It put special emphasis on reducing non-compliance with the affiliation of employees, by their employers, to the IESS. Affiliation to the IESS implies that employers will pay at least the NMW to their workers. Additionally, in order to push further the increased enforcement of the labour law, in May 2011 the government approved, by referendum, that employers can go to prison if they do not affiliate their employees to the IESS.

This exogenous increase in the intensity of enforcement of the law, that took place mainly in 2010 and 2011, resulted in a significant reduction of noncompliance rates. I estimate that affiliation of salaried workers to the IESS increased by more than 10 p.p. from 40 percent in December 2009 to 53 percent in December 2011 (see Part A of Table 2.13 in Chapter 2). Similarly, I estimate that the fraction of fulltime salaried workers earning below the NMW reduced significantly from 37 percent in December 2009 to 30 percent in December 2011 (see Part B of Table 2.13 in Chapter 2).

In Part I of this thesis, Chapters 2 and 3, I exploit the increase in the intensity of enforcement of the NMW law and provide evidence on the effects of changes the minimum wage policy when there are increases in its two main components: the level of the minimum and the intensity of its enforcement. I measure the effects on employment, wages, monetary poverty, and wage inequality. Specifically, in Chapter 2, I answer the question: Has the NMW policy of Ecuador been effective in increasing the wages of low-paid workers? For this purpose, I measure the effects of increases in the two components of the NMW on wages, employment, and monetary poverty. In Chapter 3, I analyse if the changes in the NMW policy reduced wage inequality in Ecuador. For this purpose, I measure the effects of increases in the two components of the NMW policy on the dispersion of wages of workers covered by the NMW law.

The findings in these two chapters provide policy lessons of the consequences of increased enforcement of the legal minimum. These lessons can be useful for policy makers in other developing countries who seek to reduce noncompliance with the minimum wage legislation. For example, the case of Ecuador is similar to the cases of other countries such as Peru, Colombia, Paraguay, and Honduras. In these five countries wage employment in the

private sector, as a fraction of total employment, is around 35-40 percent; noncompliance with the minimum wage is above 30 percent; and, the level of the minimum wage measured by the minimum wage/average wage ratio is larger than 50 percent (Marinakis 2016). Additionally, the lessons learned can be also useful for Asian countries such a Mali, Philippines, and India, where noncompliance rates are higher than in Latin American countries (Rani et al. 2013).

In the case of the CT programme, the Ecuadorian government conceived and launched the BDH, in 2003, as a CCT. It implemented a social marketing campaign that emphasized that beneficiary families have to invest in the human capital of their children. Specifically, the campaign promoted that school-aged children of beneficiary families had to regularly attend school, and that under 5's had to be taken to health centres for growth check-ups. However, these conditions were never monitored or enforced in practice. This characteristic makes the BDH different from other CCT programmes implemented in the region. For instance, explicit CCT programmes as *Bolsa Familia* in Brazil or *Oportunidades (Progresa)* in Mexico have conditions that, although imperfectly, are monitored and enforced (Baird et al. 2013).

The non-enforced conditions of the BDH give the opportunity to evaluate the effects of this programme and compare it with the impact generated by other CCT programmes implemented in the region. The empirical literature of the effects of the BDH have focused on analysing its impact on school enrolment (Araujo and Schady 2006); on child development and cognitive achievement (Ponce and Bedi 2010; Paxson and Schady 2010; Fernald and Hidrobo 2011); and on poverty alleviation (Araujo et al. 2017). However, no study has evaluated the effect of the BDH on food expenditure patterns.

In Part II of this Thesis, Chapter 4, I fill the gap in the literature by analysing the effects of the BDH programme on total food expenditure and its composition. I answer to the question: Does the BDH programme increase the quantity and improve the quality of food consumption? Answering this question is relevant since the BDH transfer accounts for around 25 percent of recipients' total family income. Additionally, the BDH is the second largest programme in terms of relative coverage, it covers around 40 percent of the population of the country. Finally, it is not only necessary to identify if the BDH increases food expenditure or if it diversifies food intake, what is more relevant is to identify if the BDH shifts the expenditure on food toward higher-quality sources of calories. This is particularly important considering that, in the country, one in four children under 5 years of age suffers from chronic malnutrition (stunting) and that this prevalence has stagnated during the last decade (SIISE 2015).

Part I

2 The Effects of Increased Enforcement of the National Minimum Wage Law in a Developing Country: The Case of Ecuador

2.1 Introduction

The effects of minimum wages on the labour market have been, and continue to be a topic of major debate. Economists and social scientists have long debated the effectiveness of this policy as a mechanism to protect and redistribute to favour to low-paid workers. The conventional neoclassical model predicts that an increase in the minimum wage will increase wages of workers who remain employed, but will also reduce employment of some workers. In practice, minimum wages benefit low-paid workers only if the law covers them, if the law is enforced, and if workers remain employed.

The standard Welch-Gramlich-Mincer Two-sector Model (Welch 1974; Gramlich 1976; Mincer 1976) relaxes the assumption of complete coverage and considers the existence of a covered sector and an uncovered sector. Under this model, a minimum wage imposition (or increase) above the equilibrium wage will reduce employment in the covered sector, and the workers who lost their job in this sector will migrate to either the uncovered sector or to unemployment. Regarding the effect on wages, a minimum wage will increase wages of low-paid workers who keep their jobs in the covered sector, and the increase in the labour supply in the uncovered sector will pull down the earnings of uncovered workers. However, to generate these predictions, the Two-Sector Model assumes that there is perfect enforcement of minimum wage legislation within the covered sector.

Weak enforcement of labour regulations and, by implication, low compliance among covered workers is a serious problem for minimum wage policies in developing countries. In Latin America, for example, compliance rates that are based on social security contributions at the beginning of the 2000s ranged from a maximum of 70%-80% (as in Costa Rica, Chile, Uruguay and Panama) to a minimum of 25%-50% (as in Peru, Ecuador, Nicaragua, Bolivia and Paraguay) (Marshall 2007). Despite the evidence for a high rate of noncompliance, most empirical studies have focused on measuring the employment and wage effects of changes in the legislated level of the minimum wage (see Cunningham 2007 and Neumark and Wascher 2007 for reviews that cover developing countries).

There are few studies that analyse the effects of the intensity of enforcement of labour regulations on labour market outcomes in developing countries. See Gindling et al. (2015) for Costa Rica, Almeida and Carneiro (2009) and Almeida and Carneiro (2012) for Brazil, Ronconi (2010) for Argentina, Harrison and Scorse (2010) for Indonesia, and Soundararajan (2014) for India. Among these, only the studies for the cases of Costa Rica (Gindling et al. 2015) and Indonesia (Harrison and Scorse 2010) measure the effects of an exogenous increase in the intensity of enforcement that is generated through a campaign targeting wages and employment.

The other studies identify the impacts of enforcement by exploring the variation in the number of labour inspectors, or inspections, across provinces or municipalities. Nevertheless, the main obstacle in this approach is that the number of inspectors is not distributed randomly across provinces. The authors use instrumental variables such as distance to the nearest enforcement office (Almeida and Carneiro 2009, 2012), election years (Ronconi 2010), and the number of inspectors in charge of safety and health regulations (Soundararajan 2014), to deal with this problem of endogeneity of the enforcement variable.

In this article, I contribute to the literature by evaluating the impact of an enforcement programme implemented in Ecuador. I exploit the fact that in the years 2010 and 2011, the Campaign period, the Ecuadorian Ministry of Labour campaigned to increase compliance with the national minimum wage (NMW) legislation. This began in March, 2010 with an awareness campaign that targeted domestic workers and their employers. In January, 2011, the Ministry extended the scope of the programme to include all private employees and launched the Decent Work Campaign.

To further increase enforcement of the labour law, in May 2011 the government approved by referendum that employers can go to prison if they do not affiliate their employees to the Ecuador Social Security Institute (IESS is its Spanish acronym), which implies compliance with NMW. The Campaign period was not a reaction of the government to increases in noncompliance rates, rather, it was a policy change that occurred because a new government recognized that labour violations were a serious issue in the country.

I evaluate the effects of the Campaign using a difference-in-difference identification strategy with two individual-level panel data sets of workers living in urban areas. The first panel was collected before the Campaign, during June of 2008 (baseline survey) and during June of 2009 (follow-up survey), and the second panel was collected during the Campaign, during the June months of 2010 and 2011. With these data, I compare what happened during the pre-Campaign and Campaign periods to the wage growth and the employment of private

employees who were earning below the NMW (treatment group) to private employees who were earning above the NMW (control group).

The Campaign targeted the non-affiliation of private employees, by their employers, to the IESS. In principle, the affiliation of an employee to the IESS means that he/she is paid at least the NMW. Hence, in the case of Ecuador, all private employees in the economy are covered by the NMW law, irrespective of the sector where the employee works, e.g. formal sector or informal sector. Employees who work in a small establishment (with less than five or ten workers), which are regarded as informal enterprises by the definition of informality proposed by the International Labour Organization (ILO 2002), are covered by the NMW legislation. Similarly, employees of unregistered employers, such as those who are not registered with the tax authority, are covered by the NMW legislation.

I analyse the impact of the Campaign on all private employees as a group and I explore the effects splitting this group into four subgroups: salaried men, salaried women, labourers, and domestic workers. These groups represent, respectively, 50%, 28%, 14% and 8%, respectively, of all private employees (in urban areas, see Table 2.15 in the Appendix). I do this division because these groups of private covered workers may be affected differently by the Campaign. Salaried workers have monthly wages and are more likely to have permanent jobs in comparison to Labourers (mostly men) who do a temporary or casual job (mostly) in the agriculture or construction industries for wages paid on a daily or weekly basis. Domestic workers (mostly women) were the first group targeted by the Ministry of Labour and I expect that this group of workers will be the most affected by the Campaign.

I also examined the indirect effects of the increased enforcement of the labour law on the group of uncovered self-employed workers. In developing countries, there is a significant fraction of workers that are uncovered by labour protective regulations. In Ecuador, for example, during June of 2008, self-employed workers represented around 28% of the total labour force, 1.8 million self-employed workers (out of 6.6 million workers) (see Table 2.14 in the Appendix). This group of workers are uncovered by the NMW law because they cannot be forced to pay themselves the NMW, and they are not obligated to affiliate themselves to the IESS.

Finally, I evaluated the impact of the Campaign on household income and poverty. If the Campaign generates an increase in wages for low-paid covered workers, it is natural to think that there will be a reduction in the incidence of poverty for this group of workers and their families. On the other hand, the predicted decrease in earnings in the uncovered sector may lead to an increase in the incidence of poverty among self-employed workers and their families.

Especially, if the increase in labour supply in the uncovered sector push down the earnings of self-employed workers earning far below the NMW.

Ecuador is an interesting case study because of the scale of the noncompliance issue. My estimates indicate that in the pre-Campaign period, 2008 and 2009, around 34% of the workers in the private covered sector had wages that were lower than 95 percent of the value of the NMW (see Table 2.13 in the Appendix). This sector represents the largest labour sector of the economy, during June of 2008, it accounts for approximately 40% of the total labour force, 2.8 million private employees out of 6.6 million workers (see Table 2.14 in the Appendix). This reveals that around 1 million private employees that should be paid at least the NMW, have wages that were lower than the NMW just prior to the Campaign.

This pre-Campaign setting provides an opportunity to contribute to the literature by evaluating the effects of increased minimum wage policy enforcement in a country that is characterized by a high degree of noncompliance and weak enforcement of the labour regulations. Gindling et al. (2015) identified the impact of a similar campaign in Costa Rica. However, compliance rates in Costa Rica in the early 2000s were around 80%, while compliance rates in Ecuador were lower than 50% (Marshall 2007). In terms of enforcement in Costa Rica, with a labour force of around 1.5 million people, the Ministry of Labour carried out nearly 10,000 labour inspections during 2006 (Gindling and Trejos 2010). This is similar to Ecuador. With a workforce of around 6.6 million people, Ecuador carried out only 500 labour inspections during the same year (Ministry of Labour 2011).

There are only two studies that analyse the wage and employment effects of minimum wages in Ecuador, and they only measure the impact of changes at the legislated level of the minimum wage. Canelas (2014) estimated the effects of NMW level increases during the period between 2000 and 2012 and did not find a significant increase in wages of covered workers. Furthermore, Canelas (2014) found a small increase in employment in the covered sector. This last finding contradicts the predictions of the Two-Sector Model, and the author argues that the high level of noncompliance with the minimum wage law is the main explanation for this result. Wong et al. (2016) measured the impact of increases in the sectoral minimum wages between December of 2011 and December of 2012 after the Campaign took place. Their results revealed there was a significant increase in wages of low-paid covered workers. Nevertheless, for this group of workers, they also found an increase in the probability of remaining employed and a rise in the number of hours worked. Based on these results, the authors conclude that further research is necessary to distinguish between the different working sectors of workers recorded as employed.

This paper differs from the two previous studies of Ecuador in two ways. First, I estimate the wage and employment effects of an increase in the intensity of enforcement of the NMW legislation. Second, I measure employment effects by tracking the movements of workers from the private covered sector into any other labour sector (uncovered self-employment sector, public sector) or employment status (unpaid family work, unemployment, out of the labour force). With my panel data sets, not only was I able to observe the movement of private covered workers between employment and unemployment but, I also was able to track the transition of workers from the private covered sector into any other sector or employment status. I am also able to examine the transition of workers within the private covered sector from full-time to part-time employment (working less than 40 hours a week). These distinctions are crucial if one wants to have a more comprehensive analysis of the impact of minimum wages in a developing country.

I answer these four questions about the direct effects of the Campaign: 1) What is the effect of the NMW on the changes in wages of covered workers if there is an increase in the intensity of enforcement of the NMW legislation? 2) If there is an increase in wages, does this increase reduce the likelihood of being poor among covered workers and their families? 3) Does the increase in the enforcement of the NMW law reduce the probability of remaining as a private covered worker? 4) Where do the workers who leave the covered sector move after the increase in enforcement of the NMW legislation?

In a competitive labour market, the consequences of an increase in the intensity of enforcement of a minimum wage law, for covered workers with sub-minimum wages, will be the same as those predicted by the Two-sector Model when there is a newly introduced (or increased) minimum wage. There will be an increase in wages of covered workers whose employers newly comply with the law. This result also holds during instances in which the labour market can be characterized by imperfect competition (see Basu et al. 2010). In terms of employment, the increase in enforcement will cause job losses in the covered sector and workers will migrate to the uncovered sector, or to unemployment. However, under imperfect competition, the model developed by Basu et al. (2010) predicts that at a given minimum wage, which is lower than the competitive wage, an increase in the intensity of enforcement will leave employment unchanged for firms in strict compliance and will raise employment for any other (newly compliant or noncompliant) firm. Hence, under this model, there will be an increase in employment in the covered sector.

My results provide evidence that is in line with the predictions of the Two-sector Model. I found that the Campaign increased (by approximately 12 p.p. to 16 p.p.) the real wages of

male covered workers (salaried and labourers) who were earning below the NMW and remained employed in the covered sector. This increase in wages reduced (by 7 p.p. to 10 p.p.) the likelihood that these workers and their families are poor. In terms of employment, my results show that labourers were 15 p.p. less likely to remain employed in the private covered sector during the Campaign period, compared to the pre-Campaign period, and suggest that those who lost their jobs mainly moved to the uncovered self-employment sector, or to unemployment.

For the case of salaried women, I found no significant effects on wages and employment. Domestic workers were found to be special. My results show that there was an increase of approximately 16 p.p. in the real wages of domestic workers who remain employed as full-time workers, but this increase did not reduce (significantly) the probability that these workers and their families were poor. These results also suggest that some domestic workers moved from full-time to part-time employment as consequence of the Campaign.

The structure of this paper is as follows. In Section 2.2, I outline the development of the Campaign and discuss my identification strategy. I describe the data used for the analysis in Section 2.3. Section 2.4 examines the impact of the Campaign on wages of private covered workers. The results for employment are then presented in Sections 2.5. In Section 2.6, I analyse the effects of the Campaign on the uncovered self-employment sector. Finally, in Section 2.7, I examine the impact of the Campaign on monetary poverty for covered workers and their families, and for uncovered self-employed workers and their families. In Section 2.8, I discuss the results and provide a conclusion.

2.2 Background and Identification Strategy

2.2.1 Background

In March of 2010, the Ministry of Labour in Ecuador began a comprehensive programme to promote the affiliation of private workers to the IESS and to enhance compliance with the NMW legislation. It began with the Campaña de Trabajo Doméstico Digno (Decent Domestic Work Campaign) targeting domestic workers and their employers. Building on this campaign, the Ministry extended the scope of the programme and launched (during January of 2011) the Campaña de Trabajo Digno (Decent Work Campaign). This broad campaign targeted all private employees: salaried workers, labourers, domestic workers- and their employers.

The main goal of the programme was to increase compliance with the affiliation of private workers, by their employers, to the Ecuadorian social security system (IESS). In principle, affiliation to the IESS means that employees get paid at least the national minimum

wage (NMW). Affiliation also implies that workers have legal working hours and receive other benefits that include paid vacation, the thirteenth salary, and fourteenth salary.[4]

Additionally, on May 7th, 2011, there was a referendum that asked Ecuadorians if the National Assembly should make the non-affiliation of employees, by employers, to the IESS a criminal offence.[5] Ecuadorians approved this idea with 55.02% of valid votes, and it increased the level of awareness over the non-affiliation to the IESS within Ecuadorian society. The approval of the question makes noncompliance with the IESS, and with the NMW, by employers a serious issue that can result in jail time for offender, which indicates a credible commitment of the government to enforce compliance with the labour regulations.

The first campaign in 2010 was not a reaction to increases in noncompliance with the affiliation to the IESS. Rather, it was a response by the government to the widespread non-affiliation with the IESS that characterized the country, especially among domestic workers. My estimates indicate that in December of 2009, prior to the launching of the first campaign, that only 16.6% of domestic workers were affiliated to the IESS by their employers and around 80% were paid less than the NMW (see Table 2.13 in the Appendix). Additionally, as is shown in Figure 1, my estimates indicate that compliance with the IESS for all private covered workers (in urban areas) was roughly constant at around 43% in 2007 and 2008. This compliance rate increased to 48.0% in 2009. These rates of compliance show that more than half of private covered workers were not affiliated to the IESS at the end of 2009.

After the launching of the Decent Domestic Work Campaign during March of 2010, compliance with IESS for all private covered workers increased approximately 6 p.p. from 48.0% in December of 2009 to 54.3% in December of 2010. This significant increase in compliance took place among both private salaried workers and domestic workers (see Table 2.13 in the appendix). These measurements suggest that the first campaign for domestic workers have also influenced the group of private salaried workers.

The broad campaign and the question in the referendum, in 2011, push further the enforcement of labour regulations. As shown in Figure 2.1, the highest increase in compliance with the IESS for all private covered workers took place during 2011, 7.9 p.p. from 54.3% in

---

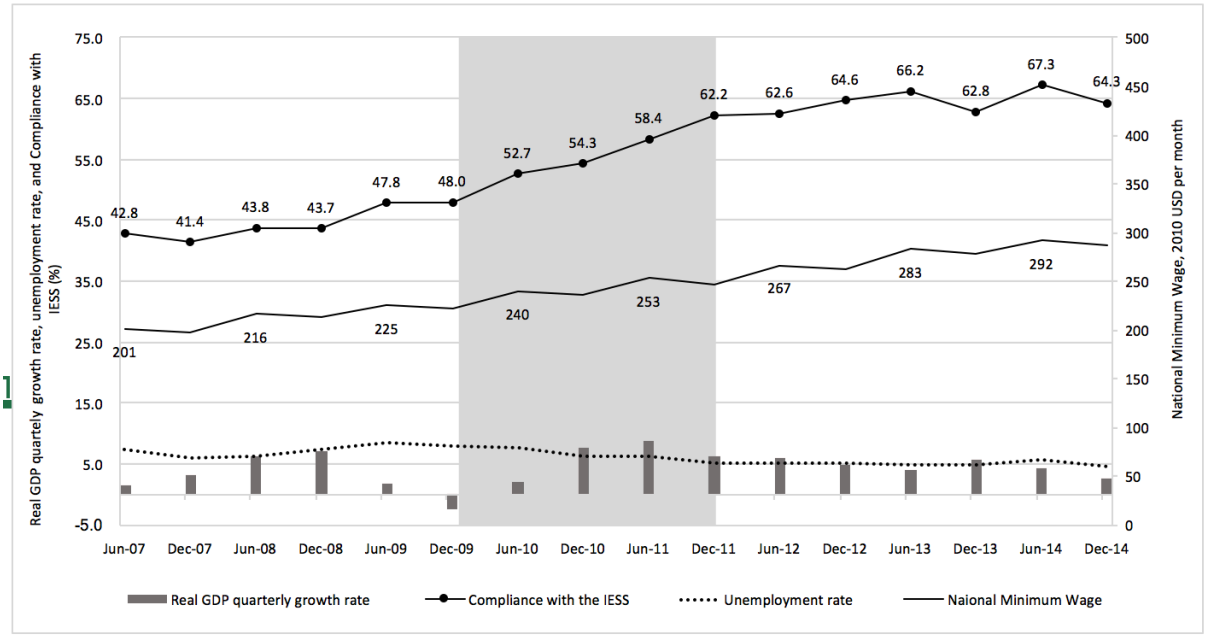
[4] Legal hours are 8 hours per day with a total of 40 hours per week, working hours above these limits imply the payment of extra hours. Employees are entitled to 15 paid days of vacation per every year worked. The thirteenth salary is equal to the sum of all the salaries received by the worker between December and November, divided by 12; it is paid in December. The fourteenth salary is equivalent to the monthly national minimum wage; it is paid on March in the coast region and on August in the highland and amazon regions.

[5] The labour code stipulates 3 to 7 days in prison and a fine of 1 to 3 NMWs for employers that do not affiliate their workers to the IESS. In the case of firms, the sanction is a fine of 3 to 5 NMWs per worker that is not affiliated to the IESS.

December of 2010 to 62.2% in December of 2011. In 2011, the increase in compliance with the IESS for domestic workers, 11.6 p.p., is higher than the increase in compliance for private salaried workers, which was 8.5 p.p. (see Table 2.13 in the appendix).

The case of labourers is different from salaried workers and domestic workers. The proportion of labourers who are affiliated to the IESS by their employers was only 2.1% in 2009. It increased to 5.4% during the campaign period, and subsequently decreased to an average of 3% after 2012 (see Table 2.13 in the appendix). The low rates of compliance with the IESS among labourers stem from differences in the entitlement of workers to labour protection. If a labourer performs a repetitive task on a regular schedule, then he/she is covered by the IESS and by the NMW. On the other hand, if a labourer performs casual work for a short time project, then he/she is not protected by the labour law. However, in both cases, labourers negotiate their wage with their employers on a daily (or a weekly) basis using the value NMW as a reference. Hence, I regard all labourers as private covered workers.

Figure 2.1 Compliance with the Affiliation to the IESS for Private Covered Workers Living in Urban Areas, and the National Minimum Wage, 2007-2014



Source: Compliance with the IESS and unemployment rates are estimated from the Urban-ENEMDU 2007-2014. The real value of the National Minimum Wage is computed using the CPI Historical Series of the INEC. Real GDP growth comes from the Monthly Statistical Reports published by the Central Bank of Ecuador

I define the period under which the campaigns and the referendum take place (2010 and 2011) as the Campaign period. During this time, the Ministry of Labour featured three components to increase the enforcement of the labour law: 1) it created awareness among

workers and employers about their duties and labour rights; 2) it prevented employers that did not affiliate their employees to the IESS to do so in order to avoid labour disputes in the future; 3) it increased the number of labour inspections targeting those not affiliated with the IESS. Under both campaigns (during 2010 and 2011) the staff of the Ministry of Labour gave personalized information to more than half a million people about their duties and rights as employers and employees (Ministry of Labour 2011). This component also included advertisements in the national media and announcements by the president of the country during his weekly Saturday radio and television show.

In 2011, the Ministry of Labour also significantly increased the number of labour inspectors targeting the non-affiliation to the IESS. Under the restructuring of Ministry, it increased the number of labour inspectors from 46 in 2010 to 240 in 2011 (see Table 2.16 in the Appendix). Regarding labour inspections (in 2011) the Ministry carried out around 26,000 inspections (Ministry of Labour 2011). This situation contrasts with the pre-Campaign period. For example, during the mid-1990s, the Quito branch of the Ministry that was in charge of roughly half of the national workforce (with 26 inspectors) carried out inspections only at the request of the workers or their unions, had no vehicle for this purpose and workers used to pay the cab fare for the inspector to perform the checks (MacIsaac and Rama 1997, p140).

Additionally, from January of 2010 to June of 2011 the Ministry inaugurated 13 of the 26 new branches it opened during 2011. As part of restructuring the Ministry, it opened new offices in cities where there was not a branch. The increase in the number of labour inspectors took place not only in the main cities where it used to have a branch (Quito, Guayaquil, and Cuenca), but also in other cities in order to provide staff for the new branches (e.g. in Santo Domingo, Riobamba, Manta, Ambato, Machala, Quevedo).

The Campaign period represents a major change in the institution of the Ministry of Labour and in its capacity to enforce compliance with labour regulations. The Campaign period involved the promotion of workers' duties and rights, a significant increase in labour inspector and inspections, new infrastructure and new vehicles to perform the controls. All these changes represent an increase in enforcement of the labour law and can be considered as an exogenous "treatment" event for private covered workers that earn below the NMW. This fact allows me to address the problem of endogeneity of the enforcement variable that affects other studies that use state variation in the number of labour inspections (or inspectors) to identify the effects of increased enforcement.

2.2.2 Identification Strategy

The typical approach to measuring the effects of minimum wage legislation is to identify a group of workers who are most likely to be directly affected by the minimum wage and compare their wage growth and employment changes to workers who are less likely to be directly affected. This methodology is in the spirit of the conventional non-experimental programme evaluation, where trends in other groups can be used to infer what would have happened to the outcome variables in the absence of treatment. A notable and influential example of this approach is the study done by Card and Krueger (1994). The authors compare the change in employment in New Jersey and Pennsylvania before and after the rise in the minimum wage in New Jersey (treatment group). Under the assumption that in the absence of the treatment, employment trends in New Jersey would have been equal to the ones observe in Pennsylvania (where the minimum wage was constant), the authors regard their difference-in-difference estimate as the causal effect of the rise in the minimum wage.

In this study, I use this difference-in-difference methodology and adapt it to evaluate the effects of the Campaign. I borrow from the empirical literature that analyses the effects of changes in the legislated level of the minimum wage and identifies the impact based on the position of covered workers across the wage distribution (see Currie and Fallick 1996; Fajnzylber 2001; Neumark et al. 2004; Stewart 2004; Stewart and Swaffield 2008). Specifically, I follow the adaptation of the difference-in-difference methodology implemented by Stewart (2004). The author estimates the impact of the introduction of the national minimum wage (NMW) in the U.K by comparing the wage growth and employment change of covered workers with wages below and above the NMW, prior the introduction of the law.

My identification strategy exploits the fact that the group of private covered workers whose wages had to be raised to comply with the NMW (i.e. those initially below the NMW) will be more affected by the Campaign than the group of private covered workers with wages above the NMW. Stewart (2004) defines those earning below the new NMW as the treated group and those earning at, or slightly above the new NMW as the control group. In my case, I am analysing the effects of increased enforcement of a minimum wage law that is already in place. This fact implies that there is a significant proportion of covered workers earning at the NMW, even before the Campaign started (see Panels A and B of Figure 2.4 in the Appendix). Therefore, in this study there are two treated groups. Private covered workers who were earning below the NMW in 2010 are the group of workers that are mostly affected by the Campaign. This group of workers is my treatment group 1 and it is the focus of this study. The group of

workers who were earning at the NMW, in 2010, will be affected mostly by the increase in the value of the NMW and it is my treatment group 2. Finally, my control group are all private covered workers who earned above the NMW during 2010.

I use a bound of 5% to allow for some measurement error in my earnings variable. The group of workers earning at the NMW includes those with wages within 0.95 and 1.05 of the value of the NMW. Workers below the NMW are those earning less than 0.95 of the NMW, and workers above the NMW are those earning more than 1.05 of NMW. In addition, I restrict the analysis to workers with wages within 25% of the NMW to make the comparison between treatment and control groups as similar as possible, in terms of wage. As shown in Figure 2.2, treated group 1 are private covered workers earning below the NMW in 2010 (240.0 USD per month) with wages that range from 180.0 USD (per month) to 228.0 USD. Treated group 2 are private covered workers earning at around the NMW in 2010, from 228.0 USD to 252.0 USD. Finally, the control group contains private covered workers earning above the NMW in 2010 whose wages ranged from 252.0 USD to 300.0 USD.

Figure 2.2 Definitions of "Treated" and "Control" groups

	Private Covered Sector	Uncovered Self-employed Sector
Earning Below the NMW ($\$180.0 \leq W_{(2010)} < \$228.0$)	Treated_1	Indirectly_Affected_1
Earning At the NMW ($\$228.0 \leq W_{(2010)} \leq \$252.0$)	Treated_2	Indirectly_Affected_2
Earning Above the NMW ($\$252.0 < W_{(2010)} \leq \$300$)	Control_Covered	Control_Uncovered

Source: Author's illustration.
Note: The classification of workers into those earning Below, At, and Above the NMW uses a bound of 5% around the NMW.

With my identification strategy, I move upward in the wage distribution and chose a group of covered workers that is not directly affected by the Campaign. This approach makes my control group as similar to my treatment group as possible, in terms of unobservables. Nevertheless, the existence of spillover effects and measurement error in the wage variable can obscure the identification of any effect of the Campaign on my treatment group. These threats

to my identification strategy are reduced when I regard also as treatment group the group of covered workers who were earning at around 5% of the NMW (treated group 2).

Another option is to move to another sector of the labour market and use those workers that are uncovered by the NMW law, the self-employed workers, as a control group. With this approach, I can choose a control group that is as similar to the treatment groups as possible in terms of initial earnings. However, using the self-employed workers as a control group threatens my identification, which states that the treatment does not affect our untreated comparison group. As predicted by the Two-sector Model, the empirical literature for developing countries frequently reports that minimum wage laws also affect the earnings of workers in the uncovered self-employed sector (Maloney and Mendez 2004, Lemos 2009). This suggests that using the self-employed workers as the control group not only reduces the similarity in unobservable characteristics between treatment and comparison groups, but also threatens my identification assumption. For these reasons, I prefer to use the group of covered workers with wages slightly above the NMW as my control group.

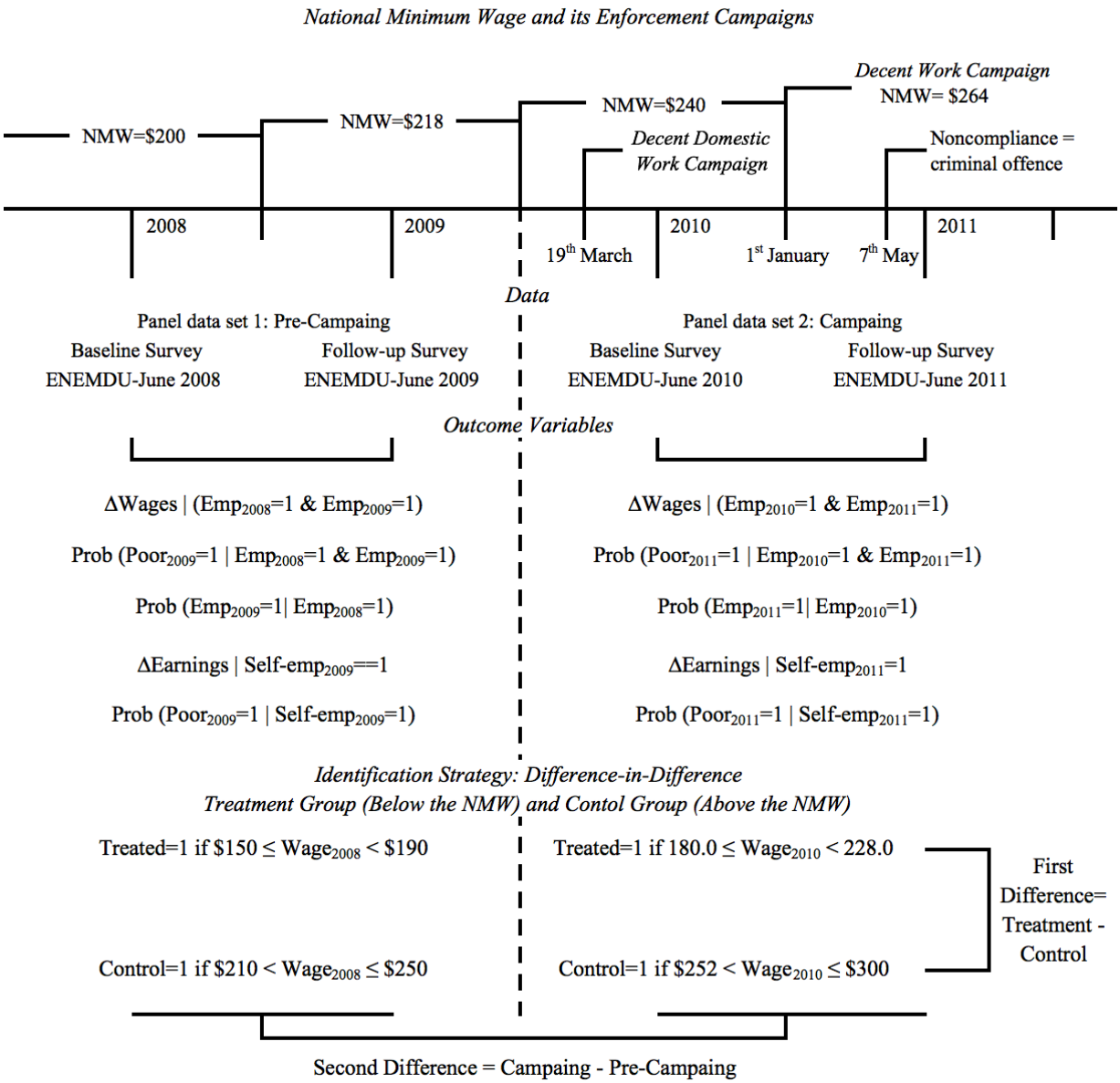
A direct comparison of the wage growth and employment change of covered workers earning below the NMW and those earning above the NMW will not be appropriate to identify any causal effect because even in the absence of the Campaign, there can be significant differences in the outcome variables. The differences in the outcome variables between treatment and control groups during the Campaign period can be compared to the equivalent differences found during the pre-Campaign period. This is my difference-in-differences estimator of the effect of the Campaign. It generates a more reliable estimate of the causal effect of the Campaign because with this approach, I can account for any pre-existing differences between the treatment and control groups.

Ideally, a panel data set of workers interviewed four times to implement my difference-in-difference estimation strategy is needed: two times before the Campaign started, during December of 2008 and December of 2009, and two times during the Campaign period, during December of 2010 and December of 2011. Unfortunately, there is not such a panel data set. The only existing data are two separate panel data sets: one with information from workers collected before the Campaign period, during June of 2008 and June of 2009, and the other panel with information from workers collected during the Campaign, during June of 2010 and June of 2011.

Figure 2.3 outlines the timing of both panel data sets and the timing of the changes in the NMW and its enforcement campaigns. The Figure also shows how my outcome variables are computed and illustrates my identification strategy. As we can see in Figure 2.3, the last survey

of my second panel dataset was conducted in June, 2011, only one month after the question in the referendum and only six months after the broad Decent Work Campaign was launched. This implies that my results represent the short-term effects of the increased enforcement of the NMW.

Figure 2.3 Chronology of Changes in National Minimum Wage and its Enforcement Campaigns, Data, Outcome Variables, and Identification Strategy



Source: Author's illustration.

I begin by comparing the year-on-year (real) wage change (hereafter wage growth) for the Campaign period, from June of 2010 to June of 2011 between my treatment and control groups and then compare this difference with the corresponding difference for the pre-

Campaign period from June of 2008 to June of 2009. Real wages are computed by deflating the nominal wage by the consumer price index (CPI), where CPI equals 100 in June of 2010. One would expect that the wages of covered workers whose wages had to be raised to comply with the NMW will increase more than the wages of covered workers who already earned above the NMW.

For those directly affected by the Campaign (my treatment group), what would their employment status be if the Campaign had not been implemented? To try to answer this question, I estimate the difference in the conditional probability of remaining employed in the private covered sector, between workers in my treatment and control group during the Campaign period, and compare it to the corresponding difference for the pre-Campaign period. For workers who lost their jobs in the covered sector, I try to identify to which other sector or employment status they migrated. Their transition options are: move to the uncovered self-employment sector, go to the public sector, become unemployed, move out of the labour force, or for full-time private employees, become part-time workers in the same covered sector.

Finally, I compare the incidence of monetary poverty in the follow-up surveys among covered workers who remain employed in the covered sector and are divided in to treatment and control groups. I also perform this analysis for all workers who report being self-employed workers in the follow-up surveys; this includes those covered workers in the baseline surveys who became self-employed workers in the follow-up surveys. If the Campaign increases the wages of low-pay covered workers, I expected will expect to find a significant reduction in poverty among these workers and their families. On the other hand, if the Campaign reduces the earnings of workers in the uncovered sector, I will expect to find an increase in poverty among self-employed workers and their families.

2.3 Data

I use two panel data sets of individuals constructed from four survey rounds of the Encuesta Nacional de Empleo, Desempleo y Subempleo, ENEMDU (national survey of employment, unemployment and underemployment). The institution in charge of the ENEMDU is the Instituto Nacional de Estadística y Censos, INEC (National Institute of Statistics and Census). The ENEMDU is carried out quarterly in urban areas using a rotating sampling strategy. Under this design, 25% of the households are interviewed for two consecutive quarters, rest for two quarters, and are interviewed again for the two last quarters (see Table 2.17 in the Appendix). The sample is renewed every two years.

Panel 1 comes from the ENEMDUs carried out during June of 2008 (baseline survey) and June of 2009 (follow-up survey). Similarly, panel 2 originates from the ENEMDUs conducted during June of 2010 and June of 2011. To construct these panel data sets, the INEC matched the households and the individuals that were interviewed during both June rounds; it matched the households that remained at the same address and the individuals who remained within the same households.

The baseline and follow-up surveys of Panel 1 were collected before any enforcement campaign was implemented and I use this data as my pre-Campaign panel data set (see Figure 2.3). The baseline survey of Panel 1 is a subsample of the ENEMDU from June of 2008. It contains information from 11,144 individuals aged 15 to 70, which represents 64.1% (11,144/ 17,398) of the total working-age individuals recorded in the complete urban sample of the ENEMDU. Table 2.18 in the Appendix compares the distribution of the working-age population across sectors (or employment status) between both data sets and shows that the proportion of workers that fell into each category are around the same.

The baseline and follow-up surveys of Panel 2 were gathered during the Campaign period. Specifically, the baseline survey is a subsample of the ENEMDU from June of 2010 and it was collected two months after the Ministry launched the Decent Domestic Work Campaign. This baseline survey contains information from 10,974 individuals aged 15 to 70, which represent 37.4% (10,974/ 29,325) of the total working-age population registered in the complete urban sample of the ENEMDU. As is shown in Table 2.18, the proportions of workers that fell into each category between both data sets are practically equal.

The information of the national minimum wage comes from the Ministry of Labour, which publishes (by the end of each year) the NMW that will apply for the following year. The NMW is registered in the labour law as the minimum monthly payment for full-time private salaried workers. The labour law does not specify an hourly minimum wage for part-time workers. In this case, the Ministry of Labour applies a corresponding hourly minimum wage by dividing the monthly NMW by the legal numbers of hours per week (40) multiplied by 4.35 weeks per month. In addition to the NMW, in Ecuador there are wage Councils that use the NMW as a floor to fix the sectoral minimum wages, but its values do not differ significantly from the NMW. This form of setting the minimum wages in this country implies, in principle, that all full-time salaried workers in the economy should earn at least the NMW.

To determine which workers fell into my treatment and control groups during the Campaign period, I assigned each worker in Panel 2 the value of NMW during the year 2010, 240.0 USD per month. Similarly, to do so for the pre-Campaign period, I assigned each worker

in Panel 1 the value of NMW in 2008, 200.0 USD. For both full-time and part-time workers, I compared their reported monthly gross earnings from their main job with the value of the monthly NMW. In the ENEMDU surveys, workers report their monthly earnings for the previous month, but they usually report hours worked per week. Thus, to reduce any potential measurement error that may arise from using hourly earnings, I used the reported monthly gross earnings for the analysis and present results for full-time workers only, and for both full-time and part-time workers.

Table 2.1 shows the descriptive statistics of private covered workers in my treatment and control groups in the baseline survey of Panel 2 for June, 2010. While males represent 55.6% of the workers earning below the NMW, they represent 73% of the workers earning more than the NMW. Workers earning below, or at the NMW are younger than workers with wages above the NMW. Compared to workers in the treatment groups, workers in the control group are better educated, more likely to work in medium and large-size firms, are more likely to work in the construction industry, and are less likely to work in agriculture and commerce.

These differences show that my control group is not an ideal counterfactual, and suggests that I should account for these characteristics to determine if these differences are not driving my results. Finally, when I compare covered workers that earn below the NMW with uncovered self-employed workers earning below the NMW, the differences in these characteristics are even greater, and this is the primary reason why I do not use the uncovered self-employed workers as my control group (see Table 2.19 in the Appendix).

2.4 Effects on Wages of Covered Workers

2.4.1 Difference in Difference

Table 2.2 illustrates my difference-in-difference estimation of the effect of increased enforcement of the NMW on wages. The sample is restricted to workers who were employed in the private covered sector in the baseline survey and remain employed in this sector in the follow-up survey (for each panel data set). In Panel A of Table 2.2, I compare the pre-Campaign change in wages of covered workers who were earning below, and at, the NMW to the pre-Campaign change in wages of covered workers who were earning above the NMW. Each cell reports the average change in log (real) monthly wages for the group labelled on the axes, along with the standard error and the number of observations. In Panel B of Table 2.2, I performed the same exercise for the Campaign period, June of 2010-June of 2011.

Table 2.1 Descriptive Statistics for Private Covered Workers with Wages Below, At, and Above the NMW during June of 2010

Characteristics (% in each category)	Full-time Workers Only			Both Full-time and Part-		
	Below the NMW	At the NMW	Above the NMW	Below the NMW	At the NMW	Above the NMW
Gender						
Male	55.6	60.9	73.0	57.4	60.0	72.0
Female	44.4	39.1	27.0	42.6	40.0	28.0
Ethnicity						
Mestizo / White / others	93.2	92.3	94.2	92.6	92.7	94.3
Indigenous / Afro	6.8	7.7	5.8	7.4	7.3	5.7
Age						
15-24	23.4	23.3	17.0	23.4	23.4	17.4
25-34	30.7	33.2	32.5	30.1	32.4	31.5
35-44	21.0	24.8	29.1	20.3	24.3	29.5
45-54	13.6	12.8	15.7	13.6	13.3	15.6
54-64	6.8	4.4	5.2	8.0	5.1	5.5
65+	4.5	1.5	0.5	4.5	1.6	0.5
Education						
Primary (1–6years)	38.1	26.9	29.3	37.3	26.4	27.8
Secondary (7–12years)	48.8	51.5	45.5	48.0	51.7	44.7
University (13–21 years)	13.1	21.6	25.1	14.7	22.0	27.5
Firm size						
Small-firms (1–10 workers)	67.2	48.8	50.0	68.5	49.3	50.4
Medium/Large-firms (11+)	32.8	51.2	50.0	31.5	50.7	49.6
Industry						
Agriculture	11.0	4.9	7.3	12.3	5.5	6.9
Manufacturing	21.8	24.3	22.5	20.8	23.9	22.3
Service	19.7	14.6	12.6	20.8	16.1	13.6
Construction	10.0	10.2	18.6	10.0	10.0	17.9
Commerce	29.7	31.1	23.0	27.5	29.5	23.3
Transportation	4.2	5.3	6.3	4.7	5.7	6.2
Finance	3.7	9.5	9.7	4.0	9.3	9.7
Number of Observations	381	588	382	448	633	403

Source: Author's calculations from the ENEMDU panel data set June2010-June2011.
Note: The sample is restricted to individuals aged 15-70 who reported working in the private covered sector during June of 2010. The classification of workers into those earning Below, At, and Above the NMW uses a bound of 5% around the NMW.

During the pre-Campaign period, June of 2008–June of 2009, there was a 12.9 percent (0.122 log points) increase in real wages of (full-time) covered workers who were earning below the NMW. For covered workers earning above the NMW, the average change in (real) wages was an increase of 3.4 percent (0.034 log points). Thus, before any enforcement campaign of the NMW took place, the wage growth of (full-time) covered workers earning below the NMW was approximately 9 p.p. higher than the wage growth of covered workers earning above the NMW. This reveals that even in the absence of our "treatment", those at the bottom of the wage distribution have larger increases in wages, which suggests that I should take into account this pre-existing difference to appropriately identify the effects of the Campaign on wages.

During the Campaign period, workers in both of my treatment groups, those earning below and at the NMW, experience significantly larger real wage growths compared to workers in my control group. Specifically, the wage growth of (full-time) covered workers earning below the NMW was 16.5 p.p. higher than the wage growth of covered workers that earn above the NMW. Likewise, the wage growth of (full-time) covered workers earning at the NMW was 10.7 p.p. higher than the wage growth of covered workers earning above the NMW.

Finally, I compute my difference-in-difference estimates by taking the difference between the single-difference estimates of Panels A and B and present the results at the bottom of the table. The *relative* wage growth for full-time workers earning below, or at, the NMW increased by approximately 7 p.p. more during the Campaign period, compared to the *relative* wage growth during the pre-Campaign period. The difference-in-difference estimates for workers earning at the NMW are statistically significant at the 5% level, in both specifications (full-time workers only, and both full-time and part-time workers). The difference-in-difference estimate for workers who were earning below the NMW is statistically significant at the 10% level in the specification that includes full-time workers only, and it is not significant at conventional levels when I include both full-time and part-time workers.

The results of Table 2.2 give some evidence that the Campaign increased the wages of covered workers who were earning below or at the NMW. The magnitude of the effects are similar to the wage effect of the enforcement campaigns implemented in Costa Rica (Gindling et al. 2015) and Indonesia (Harrison and Scorse 2010). The increase in wages implies that the Campaign effectively increased compliance with the NMW. However, these results could be driven by differences in the characteristics of covered workers earning below, or at, the NMW with that of covered workers earning above the NMW. In the next section, I perform the analysis in a regression framework and control for individual characteristic of workers.

Table 2.2 Difference-in-Difference Estimates of the Effect of the Campaign on Wage Growth for Private Covered Workers

	Full-time Workers Only		Both Full-time and Part-time	
A. Pre-Campaign Period				
	Obs.	*Change in log monthly wage (2008-2009)*	*Obs.*	*Change in log monthly wage (2008-2009)*
Covered Workers in June 2008:				
Below the NMW	281	0.122	352	0.078
		(0.021)		(0.02)
At the NMW	368	0.061	418	0.039
		(0.016)		(0.016)
Above the NMW	286	0.034	316	0.026
		(0.019)		(0.018)
Difference: Below - Above		0.088***		0.052*
		(0.028)		(0.028)
Difference: At - Above		0.027		0.013
		(0.025)		(0.024)
B. Campaign Period				
	Obs.	*Change in log monthly wage (2010-2011)*	*Obs.*	*Change in log monthly wage (2010-2011)*
Covered Workers in June 2010:				
Below the NMW	247	0.183	321	0.135
		(0.016)		(0.018)
At the NMW	450	0.132	493	0.113
		(0.012)		(0.012)
Above the NMW	295	0.030	314	0.028
		(0.018)		(0.018)
Difference: Below - Above		0.152***		0.107***
		(0.025)		(0.026)
Difference: At - Above		0.102***		0.086***
		(0.021)		(0.021)
Difference-in-Difference (Below-Above)		0.064*		0.056
		(0.036)		(0.035)
Difference-in-Difference (At-Above)		0.075**		0.073**
		(0.033)		(0.033)

Source: Calculations from the ENEMDU panel data sets: June 2008-June 2009 and June2010-June 2011.
Note: Standard errors are shown in parentheses. The sample is restricted to (full-time) private covered workers in the baseline surveys who remain employed as (full-time) private covered workers in the follow-up surveys.
*10%, **5%, and ***1% significance level.

2.4.2 Regression-Adjusted Models

The raw comparisons in Table 2.2 do not control for other factors that might explain the increase in wages. One factor is, for example, the industry in which the employee was working during June of 2010. This and other characteristics are taken into account (controlled for) in the estimates presented in Table 2.3. I estimate the following wage equation on all workers who remained employed in the private covered sector and whose initial wages were within 25% of the NMW:

$$\begin{aligned}\Delta lnW_{i,t} = \alpha + \gamma_1 Below_{i,t} + \gamma_2 At_{i,t} + \lambda C_t + \beta_1\left(C_t * Below_{i,t}\right) \\ + \beta_2\left(C_t * At_{i,t}\right) + \boldsymbol{X}'_{i,t}\delta + \varepsilon_{i,t}\end{aligned} \tag{2.1}$$

where the dependent variable, $\Delta lnW_{i,t}$, is the change in the log of real wage of worker *i* in the period *t* (1 during the Campaign, 0 for the pre-Campaign period). $Below_{i,t} = 1$ if worker *i* reports a salary in the baseline survey of period *t* that is below 95% of the NMW (treatment group 1), and =0 otherwise. Similarly, $At_{i,t} = 1$ if worker *i* reports a wage in the baseline survey of period *t* that is within 5% of the NMW (treatment group 2), and =0 otherwise. For example, for the Campaign period, $Below$ includes those with initial monthly wages that range from 180.0 USD to less than 228.0 USD, and At includes those with wages that range from 228.0 USD to 252.0 USD. *Above* (the control group) includes those with wages above 252.0 USD, but below or equal to 300.0 USD.

C_t=1 if time *t* corresponds to the Campaign period, and equals 0 for the pre-Campaign period. The coefficients β_1 and β_2, on the interaction terms $\left(C_t * Below_{i,t}\right)$ and $\left(C_t * At_{i,t}\right)$, respectively, capture all the variation in wage growth that is specific to the treatment groups (relative to the control group) during the Campaign period (relative to the pre-Campaign period). These two coefficients are my difference-in-difference estimators of the effect of the Campaign on wages. Finally, $\boldsymbol{X}_{i,t}$ is a vector of individual specific characteristics such as gender, ethnicity, age, years of schooling, and industry, that is reported in the baseline survey for each period *t*.

Table 2.3 presents the estimates of the β_1 and β_2 from Eq. (2.1). The estimates shown in columns 1 and 3 are directly comparable to the raw difference-in-difference estimates presented at the bottom of Table 2.2. Columns 2 and 4 report the estimates of the regression-adjusted models that include the set of control variables. Column 2 shows the regression-

adjusted estimates for full-time covered workers only, and Column 4 reports the estimates for both full-time and part-time covered workers. In both cases, the introduction of individual characteristics into the models do not have a sizeable impact on the coefficients: the magnitude of the coefficients changes marginally and they remain statistically significant. Additionally, the coefficient on the interaction term $(C_t * Below_{i,t})$ for the model that includes both full-time and part-time covered workers becomes statistically significant with the inclusion of the control variables. The results of the regression-adjusted models suggest that the significant wage effects, presented in the previous section, are not caused by differences in individual characteristics between workers in my treatment and control groups.

Table 2.3 OLS Difference-in-Difference Estimates of the Effect of the Campaign on Wage Growth for Private Covered Workers

	Full-time Workers Only		Both Full-time and Part-time Workers	
	(1)	(2)	(3)	(4)
Campaign*Below	0.064*	0.079**	0.056	0.066*
	(0.038)	(0.037)	(0.037)	(0.036)
Campaign*At	0.075**	0.069**	0.073**	0.065**
	(0.033)	(0.032)	(0.033)	(0.031)
Individual controls		Yes		Yes
R^2	0.031	0.094	0.017	0.082
N	1,927	1,927	2,214	2,214

Source: Author's calculations from the ENEMDU panel data sets: June 2008-June 2009 and June2010-June2011.
Note: Robust standard errors are shown in parentheses. The sample is restricted to (full-time) private covered workers in the baseline surveys who remain employed as (full-time) private covered workers in the follow-up surveys. Control variables are: gender, ethnicity, age, years of schooling, and industry.
*10%, **5%, and ***1% significance level.

To determine which group of workers felt the impact of the Campaign (on wages) the most, I ran the difference-in-difference regression model for each subgroup of private covered workers: salaried men, salaried women, labourers (mostly men), and domestic workers (mostly women). Table 2.4 presents the estimates of the coefficients for the interaction terms, β_1 and β_2, from Eq. (1). In this case, the treatment group is the subgroup private employees indicated in each column of Table 2.4. The comparison group is the same as those for Tables 2.2 and 2.3: workers who were earning above the NMW and who remained employed in the private covered sector. I report the raw OLS estimate at the top of the table and show OLS estimates with controls (my preferred specification) at the bottom of the table.

The results in Table 2.4 show that the Campaign increased by approximately 12 p.p. the wages of salaried men who were earning below the NMW. In contrast, the estimated coefficients for female salaried workers with sub-minimum wages are not statistically different from zero. Labourers and domestic workers are the groups of covered workers who benefited most from the Campaign with regards to wage growth. The *relative* wage growth of full-time labourers and domestic workers who were earning below the NMW increased by approximately 16 p.p. more during the Campaign period, compared to the *relative* wage growth during the pre-Campaign period. Likewise, the *relative* wage growth for those who were earning at the NMW increased by approximately 14 p.p. more during the Campaign period, compared to the pre-Campaign period. As I mentioned in Section 2, some labourers are not covered because they do a form of casual work, but they negotiate their wage using the NMW as reference. The results in Table 4 for this group of workers shows evidence that is in line with this affirmation.

Table 2.4 OLS Difference-in-Difference Estimates of the Effect of the Campaign on Wage Growth for Different Groups of Private Covered Workers

	Full-time Workers Only				Both Full-time and Part-time Workers			
	Salaried	Salaried	Labourer	Domestic	Salaried	Salaried	Labourer	Domestic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Raw OLS difference-in-differences estimates								
C*Below	0.106**	-0.019	0.115	0.125*	0.124**	-0.019	0.098	0.011
	(0.049)	(0.047)	(0.083)	(0.072)	(0.050)	(0.051)	(0.071)	(0.081)
C*At	0.054	0.073	0.130**	0.128*	0.051	0.065	0.115*	0.158**
	(0.037)	(0.047)	(0.064)	(0.076)	(0.037)	(0.044)	(0.065)	(0.072)
OLS difference-in-differences estimates with controls								
C*Below	0.112**	-0.001	0.148*	0.145**	0.117**	-0.003	0.124*	0.008
	(0.048)	(0.047)	(0.084)	(0.068)	(0.049)	(0.050)	(0.072)	(0.079)
C*At	0.05	0.076*	0.137**	0.127*	0.039	0.068	0.130**	0.142**
	(0.036)	(0.046)	(0.063)	(0.075)	(0.035)	(0.043)	(0.063)	(0.071)
N	1,228	981	772	689	1,364	1,104	874	762

Source: Author's calculations from the ENEMDU panel data sets: June 2008-June 2009 and June2010-June2011.
Note: Robust standard errors are shown in parentheses. The sample is restricted to (full-time) private covered workers in the baseline surveys who remain employed as (full-time) private covered workers in the follow-up surveys. The treatment group is the subgroup of covered workers indicated in each column. The comparison group is the same as in Table 3 (all covered workers with wages above the NMW). Control variables are: gender, ethnicity, age, years of schooling, and industry.
*10%, **5%, and ***1% significance level.

The effects for male covered worker (salaried and labourers) who were earning below the NMW is statistically significant in both specifications for full-time workers, and for both full-time and part-time workers. This is not the case for domestic workers who were earning below the NMW. The difference-in-difference estimate for full-time domestic workers who remained as full-time workers in the covered sector is 0.145 log points, and it is statistically significant at the 10% level. However, in the specification that includes both full-time and part-time domestic workers, the magnitude of the coefficient is reduced to nearly zero, and it becomes not significant.

These findings suggest that some full-time domestic workers who were earning less than the NMW moved to work part-time in the same covered sector during the Campaign period. This reduces their monthly wages (from their main job), which in turn pushes down the average increase in wages. The results in Table 2.20 in the Appendix confirm this conclusion. In this table, I restrict the sample to full-time covered workers in the baseline surveys who report being part-time or full-time covered workers in the follow-up surveys. The estimate of β_1 = 0.018. It is lower than β_1 = 0.145 from column 4 of Table 2.4, and it is not significantly different from zero.

In summary, the analysis of the effects on wages suggests that the Campaign has increased the wages of salaried men, labourers, and domestic workers who earned below the NMW. The significant effects on wages imply that the Campaign has increased compliance with the NMW among these groups of workers. However, the results also reveal the Campaign had no significant effect on the wages of salaried women who were earning less than the NMW.

2.5 Effects on Employment of Covered Workers

2.5.1 Impact on the Probability of Remaining Employed in the Covered Sector

In this section, I examine the effect of the Campaign on the probability of remaining employed in the private covered sector. Using the panel data sets for the pre-Campaign and Campaign, I estimate the following employment equation:

$$Prob(EMP_{i,t} = 1) = \Phi\big[\alpha + \gamma_1 Below_{i,t} + \gamma_2 At_{i,t} + \lambda C_t + \beta_1\big(C_t * Below_{i,t}\big) + \beta_2\big(C_t * At_{i,t}\big) + \boldsymbol{X}'_{i,t}\delta + \varepsilon_{i,t}\big] \quad (2.2)$$

where the binary response variable $EMP_{i,t}$ = 1 if individual *i* is working as a private covered worker in the baseline survey and he/she remains employed in the private covered sector in the follow-up survey for each period *t* (pre-Campaign and Campaign). $EMP_{i,t}$ = 0 if individual *i* is working as a private covered worker in the baseline survey and he/she moves to another sector (e.g. uncovered self-employment sector, public sector) or employment status (e.g. unemployment, out of the labour force) in the follow-up survey. For the analysis of full-time workers only, $EMP_{i,t}$ = 1 if individual *i* remains as a full-time private covered worker in the follow-up survey, and equals zero otherwise. In this last case, $EMP_{i,t}$ = 0 also when individual *i* remains employed in the same private covered sector but he/she moves from full-time to part-time employment.

The explanatory variables are the same as those in the wage equation, and Φ is the probit transformation. As in the wage equation, workers with wages above the NMW in the baseline surveys act as the control group. The coefficient on the interaction terms capture the effect of the Campaign. Specifically, the probability difference-in-differences estimates of the effect of the Campaign are the *marginal effects* of the interactions $(C_t * Below_{i,t})$ and $(C_t * At_{i,t})$, derived from the probit coefficient estimates and evaluated at the sample means of the explanatory variables.

Table 2.5 presents the difference-in-differences estimates of the probit model with controls in columns 2 and 4. For comparison, Table 2.5 also reports raw (i.e., without control variables) OLS difference-in-differences estimates in columns 1 and 3. Table 2.5 shows the results for full-time workers only, and for both full-time and part-time workers. All difference-in-differences estimates in Table 2.5 are not significantly different from zero. However, for the group of full-time workers only (see columns 1 and 2), the sign and magnitude of the coefficients on the interaction $(C_t * Below_{i,t})$ suggest that they may be a negative impact of the Campaign on the probability of remaining employed for a sub-group of covered workers.

Table 2.6 reports the result for the four sub-groups of private covered workers: salaried men, salaried women, labourers, and domestic workers. The comparison group is the same as that for Table 2.5: private covered workers who were earning above the NMW in the baseline surveys. The first block of Table 2.6 provides the raw OLS difference-in-differences estimates. The second block presents the probit difference-in-differences estimates, which are the *marginal effects* of the interactions $(C_t * Below_{i,t})$ and $(C_t * At_{i,t})$. These estimates are probability difference-in-differences and can be interpreted as the effect of the Campaign on the probability of remaining employed in the private covered sector for each group of workers.

Table 2.5 Difference-in-Difference Estimates of the Effect of the Campaign on the Probability of Subsequent Employment for Private Covered Workers

	Full-time Workers Only		Both Full-time and Part-time Workers	
	OLS	Probit	OLS	Probit
	(1)	(2)	(3)	(4)
Campaign*Below	-0.049	-0.054	0.004	0.000
	(0.045)	(0.044)	(0.040)	(0.040)
Campaign*At	0.009	0.006	0.024	0.022
	(0.039)	(0.039)	(0.037)	(0.036)
Individual controls		Yes		Yes
N	2,632	2,632	2,912	2,912

Source: Author's calculations from the ENEMDU panel data sets: June 2008-June 2009 and June2010-June2011.
Note: Robust standard errors are shown in parentheses. The sample is restricted to (full-time) private covered workers in the baseline surveys. Control variables are: gender, ethnicity, age, years of schooling, and industry. The marginal effect of interactions terms derived from the probit coefficient estimate and are evaluated at the sample means of the explanatory variables.
*10%, **5%, and ***1% significance level.

The OLS and probit difference-in-differences estimates on the interaction $(C_t * Below_{i,t})$ are negative and statistically different from zero for the group of labourers (see Columns 3 and 7 of Table 2.6). The results suggest that there is a reduction, during the Campaign period, of approximately 15 p.p. in the *relative* probability of remaining employed in the covered sector for labourers who were earning below the NMW, compared to the *relative* probability during the pre-Campaign period. Additionally, the signs of the estimates for the groups of salaried men and domestic workers who were working full-time and were earning below the NMW are negative, but they are not statistically significant. This is interesting because these workers, with the group of labourers, were the ones who saw their wages increase as consequences of the Campaign.

Although not significant, the estimates on the interaction $(C_t * Below_{i,t})$ for full-time domestic workers are negative and larger (in absolute magnitude) than the positive estimates for both full-time and part-time domestic workers (see Columns 4 and 8 of Table 2.6). These results, with the results of the effects on wages, suggest that domestic workers remained employed in the private covered sector, but some of them moved from full-time to part-time employment within this sector.

Table 2.6 Difference-in-Difference Estimates of the Effect of the Campaign on the Probability of Subsequent Employment for Different Groups of Private Covered Workers

	Full-time Workers Only				Both Full-time and Part-time Workers			
	Salaried Men	Salaried Women	Labourer	Domestic Worker	Salaried Men	Salaried Women	Labourer	Domestic Worker
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Raw OLS difference-in-differences estimates								
C*Below	-0.041	0.025	-0.161*	-0.117	0.009	0.056	-0.136**	0.036
	(0.059)	(0.066)	(0.085)	(0.115)	(0.053)	(0.058)	(0.069)	(0.101)
C*At	0.035	-0.041	-0.035	0.113	0.034	0.021	-0.024	0.073
	(0.046)	(0.057)	(0.079)	(0.103)	(0.043)	(0.054)	(0.068)	(0.092)
Probit difference-in-differences estimates with controls								
C*Below	-0.053	0.007	-0.163*	-0.104	0.005	0.042	-0.151**	0.048
	(0.060)	(0.069)	(0.084)	(0.110)	(0.054)	(0.061)	(0.071)	(0.104)
C*At	0.04	-0.064	-0.031	0.102	0.039	0.005	-0.02	0.068
	(0.046)	(0.059)	(0.080)	(0.098)	(0.043)	(0.055)	(0.071)	(0.094)
N	1,635	1,331	1,034	915	1,772	1,459	1,118	978

Source: Author's calculations from the ENEMDU panel data sets: June 2008-June 2009 and June2010-June2011.
Note: Robust standard errors are shown in parentheses. The sample is restricted to (full-time) private covered workers in the baseline surveys. The treatment group is the subgroup of covered workers indicated in each column. The comparison group is the same as that for Tables 5: all private covered workers who were earning above the NMW. Control variables are: gender, ethnicity, age, years of schooling, and industry. The marginal effect of the interaction terms are derived from the probit coefficient estimates and are evaluated at the sample means of the explanatory variables.
*10%, **5%, and ***1% significance level.

2.5.2 Dynamic Effects: Employment Transitions of Covered Workers

In the previous section, I found significant evidence that the Campaign led to some labourers moving out of the covered sector. In this part, I examine where labourers move after they lost their jobs because of the Campaign. Labourers who leave the private covered sector can migrate to another sector of the labour market (self-employment, business owner, the public sector) or they can change employment status (unemployment, unpaid family work, out of the workforce, or part-time employment in the same covered sector).

To track the movement of covered workers, I identify the sector or employment status where he/she is working in the follow-up surveys. The options for labourers are: stay in the private covered sector, move to self-employment or become a business owner, move to the public sector, become unemployed or an unpaid family worker, or leave the labour force. Additionally, full-time labourers in the private covered sector can also move within the same sector from full-time to part-time employment.

I use a multinomial logit model to determine the difference-in-differences estimates of the effect of the Campaign on employment transitions. The dependent variable $Trans_{icz,t}$ identifies the movement of labourer *i* from sector *c* (the private covered sector) into one of the other sectors/status *z* of the labour market. The base category for the analysis is that worker *i* stays in the private covered sector. Hence, the probability that workers *i* leaves the private covered sector *c* for sector/status *z*, conditional on reporting being in sector *c* in the baseline survey, is characterized by:

$$Prob\big(Trans_{icz,t} = 1\big) = exp\big(\theta_{icz,t}\big) \,/\, \Big(1 - exp\big(\theta_{icz,t}\big)\Big) \tag{2.3}$$

where

$$\theta_{icz,t} = \alpha_{cz} + \gamma_{1cz} Below_{i,t} + \gamma_{2cz} At_{i,t} + \lambda_{cz} C_t + \beta_{1cz}\big(C_t * Below_{i,t}\big) + \beta_{2cz}\big(C_t * At_{i,t}\big) + \boldsymbol{X'}_{i,t}\delta_{cz} + \varepsilon_{icz,t}$$

The explanatory variables are the same as those used in the wage and employment equations; the vector of control variables, $\boldsymbol{X'}$, in this case includes age, years of education, and a dummy variable for ethnicity. The effects of the Campaign on the probability of moving from the private covered sector *c* into sector/status *z* are the "marginal effects" of the interactions of $\big(C_t * Below_{i,t}\big)$ and $\big(C_t * At_{i,t}\big)$. These marginal effects derived from logit coefficients estimates and are evaluated at the sample means of the explanatory variables. A positive number indicates that the Campaign increased the probability that a labourer who was earning below or at the NMW leaves his/her job and moves to sector/status z.

Table 2.7 reports the difference-in-differences estimates of the effect of the Campaign on employment transitions for labourers. Panel A shows the estimates for the sample of labourers who were working full-time in the baseline surveys. Panel B reports the estimates for both full-time and part-time labourers in the baseline surveys. None of the estimates of the

interaction term $(C_t * Below_{i,t})$ are statistically different from zero for this group of workers. For comparison, I also ran the transition analysis for the other sub-groups of covered workers and, as was expected, I found that the estimates of the interaction $(C_t * Below_{i,t})$ are not statistically different from zero (see Table 2.21 in the Appendix).

Table 2.7 Difference-in-Difference Estimates of the Effect of the Campaign on the Probability of Leaving the Private Covered Sector for Another Sector/Status, for Labourers

	C*Below		C*At	
	Marginal Effect	Std. Err.	*Marginal Effect*	Std. Err.
Panel A: Origin is Full-time Labourer				
Self-employed	0.078	(0.055)	0.048	(0.058)
Public Sector	-0.005	(0.009)	-0.005	(0.009)
Unemployed	0.049	(0.055)	-0.028	(0.034)
Not in Labour Force	0.014	(0.014)	0.01	(0.012)
Part-time Private Covered	0.019	(0.056)	0.001	(0.055)
N	1,034			
Panel B: Origin is Part-time or Full-time Labourer				
Self-employed	0.066	(0.049)	0.06	(0.057)
Public Sector	-0.002	(0.009)	-0.002	(0.009)
Unemployed	0.059	(0.052)	-0.041	(0.037)
Not in Labour Force	0.006	(0.011)	-0.003	(0.012)
N	1,034			

Source: Author's calculations from the ENEMDU panel data sets: June 2008-June 2009 and June2010-June2011.
Note: Robust standard errors are shown in parentheses. The sample is restricted to (full-time) private covered workers in the baseline surveys. The treatment group is the sub-group of covered workers indicated in each Panel. The comparison group is the same as that for Tables 5: private covered workers who were earning above the NMW. Control variables are: ethnicity, age and years of schooling. The marginal effect of interactions terms derived from the mlogit coefficient estimates and are evaluated at the sample means of the explanatory variables. Hausman-McFadden test of IIA assumption: prob > Chi-square =1 when omitting the categories of Self-employed, Unemployed, Not in Labour Force, and Part-time Private Covered; did not meet asymptotic assumptions of the test when omitting Public Sector.
*10%, **5%, and ***1% significance level.

The significant disemployment effects for labourer (shown in Table 2.6) and the magnitude of the estimates in Table 2.7 suggest that the labourers who lost their jobs mainly moved to the uncovered self-employment sector, or they became unemployed. The transition of some labourers from the covered sector into the uncovered sector goes in line with the

prediction of the Two-Sector Model, and calls for the analysis of the effects of the Campaign on the earnings of (uncovered) self-employed workers. I perform this analysis in the following section.

2.6 Effects on the Uncovered Sector

In Section 2.5, I found some evidence that the Campaign reduced the likelihood of remaining employed in the covered sector of labourers, and that some of these workers migrated mostly to self-employment, or they became unemployed. In this section, I analysed the effects of the Campaign on the change in earnings of uncovered self-employed workers and tested the predictions of the Two-Sector Model. According to this model, one should expect that the earnings of self-employed workers will fall as result of the displacement of workers from the covered sector into the uncovered sector.

Table 2.8 OLS Difference-in-Difference Estimates of the Effect of the Campaign on the Change in Earnings of Uncovered Self-employed Workers

	Full-time Workers Only			Both Full-time and Part-time Workers		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Raw OLS difference-in-differences estimates						
C*Below	0.018	-0.03	0.17	-0.026	-0.043	0.089
	(0.129)	(0.146)	(0.301)	(0.118)	(0.136)	(0.250)
C*At	-0.280**	-0.304**	-0.177	-0.356***	-0.338**	-0.287
	(0.127)	(0.141)	(0.301)	(0.120)	(0.136)	(0.251)
OLS difference-in-differences estimates with controls						
C*Below	0.01	-0.042	0.136	-0.035	-0.056	0.066
	(0.130)	(0.148)	(0.301)	(0.118)	(0.135)	(0.250)
C*At	-0.249**	-0.294**	0.007	-0.344***	-0.338**	-0.192
	(0.125)	(0.138)	(0.293)	(0.118)	(0.135)	(0.249)
N	686	504	182	882	604	278

Source: Author's calculations from the ENEMDU panel data sets: June 2008-June 2009 and June2010-June2011.
Note: Robust standard errors are shown in parentheses. The sample is restricted to (full-time) uncovered self-employed workers in the follow-up surveys. The comparison group includes self-employed workers with earnings above the NMW. Control variables are: age, years of schooling, and industry.
*10%, **5%, and ***1% significance level.

Table 2.8 reports the difference-in-difference estimates of the Campaign on the change in earnings of individuals who report being self-employed workers in the follow-up surveys. This group includes those workers who were employed in other sectors in the baseline surveys and report being self-employed workers in the follow-up surveys. Table 2.8 shows the OLS estimates of the coefficients on the interaction terms, β_1 and β_2, from Eq. (1) applied to the earnings of self-employed workers. The comparison group, in this case, are self-employed workers with earnings above the NMW. Table 2.8 shows the results of the earnings models for men, women, and for both groups. The top panel shows the raw OLS estimates (i.e. without controls) and the bottom panel shows the OLS estimates with controls.

The results in Table 2.8 reveal a significant fall in the *relative* earnings growth of male self-employed workers who were earning at the NMW during the Campaign period. This finding provides evidence that is in line with the predictions of the Two-Sector Model: the earnings of uncovered workers will fall as a consequence of the migration of workers from the covered to the uncovered sector. As I mentioned above, some labourers in the covered sector move to the uncovered self-employment sector and this might be the cause of the fall in the earnings growth of workers in this sector.

2.7 Effects on Household Income and Poverty

In Section 2.4, I found that the Campaign increased the wages of male salaried workers, labourers, and domestic workers. I also found (in Section 2.6) that the Campaign led to a reduction in labour earnings of male self-employed workers. In this section, I study the impact that these changes, in wages and earnings, had on household income and monetary poverty. Specifically, I try to answer two questions: 1) Do covered workers that were earning below the NMW, and their families, are less likely to be poor during the Campaign period? 2) Are uncovered workers, who saw their earnings decrease, and their families more likely to be poor during the Campaign?

To answer these two questions, I first compute the per-capita household income for each worker based on the official methodology of the INEC. Additionally, I use the official monthly poverty line, which equals 66.29 USD in 2009 and 71.33 USD in 2011. Second, I define workers in each follow-up survey that have a monthly per-capita household income below the poverty line as poor. Finally, I implement my difference-in-differences identification strategy to estimate the impact of the Campaign on poverty.

The effect of the Campaign on household income and poverty may be different from its impact on wages of covered workers and on earnings of self-employed individuals; it will depend on the pattern of employment, the magnitude of the income sources, and the number of people within the household. High income earners may be the only workers in relatively low-income households, while low-pay workers may be secondary family workers in relatively high-income households (Alaniz et al. 2011). Additionally, in households with comparable patterns of employment and numbers of individuals, the incidence of poverty among families of workers earning below the NMW will be larger than the incidence among the families of workers earning at or above the NMW.

2.7.1 Impact on Covered Workers and their Families

To obtain the difference-in-differences estimates of the effect of the Campaign on poverty, I first compute the incidence of poverty in the follow-up surveys during the pre-Campaign and Campaign periods for covered workers earning below, at, and above the NMW in the baseline surveys. Table 2.9 shows these statistics. The Table shows that while the incidence of poverty among covered workers who were earning above the NMW remains constant (at around 7-8%), during the pre-Campaign and Campaign periods, the incidence of poverty among covered workers who were earning below the NMW reduces in around 8 p.p., from 17-19% in the pre-Campaign period to 9-11% during the Campaign period. At the bottom of the Table, I report the difference-in-difference estimates. The estimates are positive and statistically significant (at the 5% level). They suggest that the increase in wages of those earning below the NMW, due to the Campaign, reduced the probability that their households are poor by around 7-8 p.p.

In Table 2.10, I report the difference-in-difference estimates of the Campaign implemented via OLS regression and via a probit model with controls. The probit model is described as:

$$Prob(Poor_{i,t} = 1) = \Phi\big[\alpha + \gamma_1 Below_{i,t} + \gamma_2 At_{i,t} + \lambda C_t + \beta_1\big(C_t * Below_{i,t}\big) + \beta_2\big(C_t * At_{i,t}\big) + \boldsymbol{X}'_{i,t}\delta + \varepsilon_{i,t}\big] \quad (2.4)$$

where the binary response variable $Poor_{i,t}$ = 1 if the worker's household is poor in the follow-up survey and 0 if non poor in the follow-up survey, for each period *t* (pre-Campaign and Campaign).

Table 2.9 Difference-in-Difference Estimates of the Effect of the Campaign on the Probability that a Covered Worker's Household is Poor in t+1

	Full-time Workers Only		Both Full-time and Part-time Workers	
A. Pre-Campaign Period				
	Obs.	*Percentage*	*Obs.*	*Percentage*
Covered Workers in June 2008:				
Below the NMW	281	17.1	352	18.8
		(2.2)		(2.1)
At the NMW	368	8.2	418	9.8
		(1.4)		(1.5)
Above the NMW	286	7.7	316	7.6
		(1.6)		(1.5)
Difference: Below – Above		9.4***		11.2***
		(2.7)		(2.6)
Difference: At – Above		0.5		2.2
		(2.1)		(2.1)
B. Campaign Period				
	Obs.	*Percentage*	*Obs.*	*Percentage*
Covered Workers in June 2010:				
Below the NMW	247	9.3	321	11.2
		(1.9)		(1.8)
At the NMW	450	6.9	493	7.3
		(1.2)		(1.2)
Above the NMW	295	7.1	314	7.6
		(1.5)		(1.5)
Difference: Below – Above		2.2		3.6
		(2.4)		(2.3)
Difference: At – Above		-0.2		-0.3
		(1.9)		(1.9)
Difference-in-Difference (Below-Above)		-7.2**		-7.6**
		(3.4)		(3.3)
Difference-in-Difference (At-Above)		-0.7		-2.6
		(3.1)		(3.1)

Source: Author's calculations from the ENEMDU panel data sets: June 2008-June 2009 and June2010-June2011.
Note: Standard errors are shown in parentheses. The sample is restricted to (full-time) private covered workers in the baseline surveys who remain employed as (full-time) private covered workers in the follow-up surveys.
*10%, **5%, and ***1% significance level.

The explanatory variables are the same as those in the wage equation and Φ is the probit transformation. As in the wage equation, workers with wages above the NMW in the baseline surveys are considered the comparison group. The vector of control variables, $\boldsymbol{X'}_{i,t}$, of each worker *i* include gender, ethnicity, age, years of schooling, and industry, reported in the baseline survey for each period *t*. The coefficient on the interaction terms captures the effect of the Campaign. Specifically, the probability difference-in-differences estimates of the effect of the Campaign are the *marginal effects* of the interactions $(C_t * Below_{i,t})$ and $(C_t * At_{i,t})$ derived from the probit coefficient estimates and evaluated at the sample means of the explanatory variables.

Table 2.10 OLS and Probit Difference-in-Difference Estimates of the Effect of the Campaign on the Probability that a Covered Worker's Household is Poor in t+1

	Full-time Workers Only		Both Full-time and Part-time	
	OLS	Probit	OLS	Probit
	(1)	(2)	(3)	(4)
Campaign*Below	-0.072**	-0.059*	-0.076**	-0.068**
	(0.036)	(0.031)	(0.035)	(0.030)
Campaign*At	-0.007	0.002	-0.026	-0.013
	(0.029)	(0.025)	(0.028)	(0.025)
Individual controls		Yes		Yes
N	1,927	1,927	2,214	2,214

Source: Author's calculations from the ENEMDU panel data sets: June 2008-June 2009 and June2010-June2011.
Note: Robust standard errors are shown in parentheses. The sample is restricted to (full-time) private covered workers in the baseline surveys who remain employed as (full-time) private covered workers in the follow-up surveys. Control variables are: gender, ethnicity, age, years of schooling, and industry. The marginal effect of interaction terms derived from the probit coefficient estimate and are evaluated at the sample means of the explanatory variables.
*10%, **5%, and ***1% significance level.

On Table 2.10, columns two and four show the difference-in-differences estimates of the probit model with controls. For comparison, Columns 1 and 3 report the raw (i.e., without control variables) OLS difference-in-differences estimates. Table 2.10 shows the results for full-time workers only, and for both full-time and part-time workers. All *marginal effects* of the interactions $(C_t * Below_{i,t})$ are positive and significantly different from zero. These results support the findings in Table 2.9.

Table 2.11 reports estimates of the impact of the Campaign for covered workers classified into heads and non-heads of households by gender. In the Appendix, I also show the

estimates for workers classified the same way as in Table 2.4: salaried men, salaried women, labourers, and domestic workers (see Table A11). In both tables, the comparison group is the same as that in Table 2.10: private covered workers who were earning above the NMW in the baseline surveys. The first block of Table 2.11 shows the raw OLS difference-in-differences estimates. The second block presents the probit difference-in-differences estimates, which are the *marginal effects* of the interactions $(C_t * Below_{i,t})$ and $(C_t * At_{i,t})$. These estimates are probability difference-in-differences and can be interpreted as the effect of the Campaign on the probability that a worker's household is poor.

Table 2.11 OLS and Probit Difference-in-Difference Estimates of the Effect of the Campaign on Poverty for Different Groups of Private Covered Workers

	Full-time Workers Only				Both Full-time and Part-time Workers			
	Heads of Household		Non-Heads of Household		Heads of Household		Non-Heads of Household	
	Male	Female	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Raw OLS difference-in-differences estimates								
C*Below	-0.066	-0.041	-0.108*	-0.034	-0.079	-0.031	-0.107**	-0.047
	(0.068)	(0.095)	(0.057)	(0.039)	(0.063)	(0.082)	(0.054)	(0.040)
C*At	-0.007	0.046	-0.027	0.008	-0.037	0.002	-0.036	0.000
	(0.051)	(0.067)	(0.034)	(0.029)	(0.051)	(0.065)	(0.034)	(0.029)
Probit difference-in-differences estimates with controls								
C*Below	-0.04	-0.07	-0.095**	-0.071	-0.063	-0.063	-0.098**	-0.077
	(0.053)	(0.137)	(0.047)	(0.051)	(0.053)	(0.112)	(0.046)	(0.049)
C*At	0.000	0.018	-0.014	0.01	-0.026	-0.03	-0.018	0.002
	(0.043)	(0.129)	(0.029)	(0.031)	(0.045)	(0.106)	(0.029)	(0.033)
N	1,001	694	1,003	972	1,111	770	1,127	1,096

Source: Author's calculations from ENEMDU panel data sets: June 2008-June 2009 and June2010-June2011.
Note: Robust standard errors are shown in parentheses. The sample is restricted to (full-time) private covered workers in the baseline surveys who remain employed as (full-time) private covered workers in the follow-up surveys. The treatment group is the subgroup of covered workers indicated in each column. The comparison group is the same as that in Table 10 (covered workers with wages above the NMW). Control variables are: gender, ethnicity, age, years of schooling, and industry. The marginal effect of interaction terms derived from the probit coefficient estimate and are evaluated at the sample means of the explanatory variables.
*10%, **5%, and ***1% significance level.

The results in Table 2.11, and in Table 2.23 (in the Appendix), show that the Campaign reduced the incidence of poverty among male covered workers (salaried and labourers) and their families. Table 2.11 reveals that this effect takes place among male non-head of households. For this group, the effect of the Campaign on poverty is around 10-11 p.p. Most men within this group are the sons of working heads of household. This result suggests that the increase in wages of male covered workers (due to the Campaign) helped to reduce the incidence of poverty among families in which the low-pay covered worker is the secondary family worker. It is also interesting to note that while I found an increase in wages among domestic workers, this increase does not have a significant effect on reducing the incidence of poverty among female heads (and non-heads) of household.

There is a reduction in the incidence of poverty among households with a male secondary wage earner. However, does this mean that poor households transitioned out of poverty? Unfortunately, there is not a high enough number of observations to estimate the impact of the Campaign on the probability that a poor household in the baseline survey will become non-poor in the follow-up survey. The sample size for the estimation of the impact on poverty for male non-heads of households is 1003 workers (see Column 4 of Table 2.11). Out of this total, 76 workers are poor in the follow-up surveys and 72 are poor in the baseline surveys. For the analysis of the transition out of poverty, our sample will restrict to the 72 poor workers in the baseline surveys. With this small sample size, the number of observations in each cell for the implementation of my difference-in-difference strategy is far less than 25, with some cells with zero observations.

2.7.2 Impact on Uncovered Workers and their Families

In Section 2.6, I found that the Campaign led to a reduction in labour earnings of male self-employed workers that were earning at the NMW. The questions that follows are: Do these workers and their families became more likely to be poor as consequence of the Campaign? Table 2.12 show the OLS and Probit difference-in-difference estimates of the Campaign for self-employed workers. I show the estimates for all self-employed workers, and for self-employed workers classified by gender. None of the estimates are significantly different from zero at conventional levels. Although there is a reduction in wages among male self-employed workers, the fact that this effect takes place among those that were earning at the NMW makes the impact on poverty not significant because these workers and their families are less likely to be below the poverty line.

Table 2.12 OLS and Probit Difference-in-Difference Estimates of the Effect of the Campaign on the Probability that an Uncovered Worker's Household is Poor in t+1

	Full-time Workers Only			Both Full-time and Part-time Workers		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Raw OLS difference-in-differences estimates						
C*Below	-0.079	-0.137	0.017	-0.029	-0.129	0.133
	(0.072)	(0.088)	(0.126)	(0.063)	(0.081)	(0.097)
C*At	0.07	0.117	-0.053	0.083	0.09	0.075
	(0.075)	(0.092)	(0.128)	(0.066)	(0.084)	(0.103)
Probit difference-in-differences estimates with controls						
C*Below	-0.066	-0.125	-0.01	-0.022	-0.121	0.119
	(0.071)	(0.088)	(0.115)	(0.062)	(0.080)	(0.090)
C*At	0.062	0.109	-0.091	0.076	0.084	0.049
	(0.073)	(0.088)	(0.127)	(0.064)	(0.081)	(0.099)
N	686	504	182	882	604	278

Source: Author's calculations from the ENEMDU panel data sets: June 2008-June 2009 and June2010-June2011.
Note: Robust standard errors are shown in parentheses. The sample is restricted to (full-time) uncovered self-employed workers in the follow-up surveys. The treatment group is the subgroup of covered workers indicated in each column. The comparison group are self-employed workers with earnings above the NMW. Control variables are: age, years of schooling, and industry.
*10%, **5%, and ***1% significance level.

2.8 Conclusions

During the years of 2010 and 2011, the Ecuadorian government campaigned to increase compliance with the NMW legislation. To evaluate the effects of the Campaign period, I used a difference-in-difference identification strategy. With this approach, I compared what happened to the wage growth, the change in employment, and the incidence of poverty of covered workers who were earning below the NMW on the one hand, and covered workers who were earning above the NMW on the other hand, during the pre-Campaign and Campaign periods. I use two panel data sets of workers. The first panel has information of workers for the pre-Campaign period, June of 2008 and June of 2009, and the second panel has information from workers for the Campaign period, June months of 2010 and 2011.

The evidence for men falls into line with the theoretical predictions of the Two-Sector Model. I found that the Campaign increased by approximately 12 p.p. to 16 p.p. the wages of

male covered workers (salaried and labourers) who were earning below the NMW and remain employed in the covered sector. This increase in wages lead to a reduction in the probability of being poor for this group of workers and their families. This impact on monetary poverty takes places particularly in families where the low-paid covered workers are the secondary workers in the household.

In terms of employment, the results suggest that the Campaign reduced the probability that labourers remain employed in the private covered sector. Specifically, I found that the relative probability of remaining employed in the covered sector for labourers who were earning below the NMW was approximately 15 p.p. lower during the Campaign period, compared to the pre-Campaign period. Additionally, the analyses of employment transitions suggest that most of the labourers who lost their jobs in the covered sector moved to the uncovered self-employed sector, and others became unemployed.

As predicted by the Two-sector Model, I also found that the Campaign led to a fall in the earnings of uncovered self-employed workers. My results show that the *relative* earnings growth of male self-employed workers who were earning at the NMW decreased by approximately 27 p.p. during the Campaign period, compared to the *relative* earnings growth during the pre-Campaign period. Some labourers moved to the uncovered self-employment sector, which increased the labour supply in this sector and may have caused the fall in the earnings of male self-employed workers who were earning around the NMW. Despite this fall in earnings, I did not find that the Campaign led to a significant increase in the probability of being poor for this group of workers and their families. This may be explained by the fact that the increase in labour supply reduced the earnings of self-employed workers who were earning at the NMW and these individuals are less likely to be poor than those with earnings that are far below the NMW.

The results regarding domestic workers indicate that the Campaign increased by approximately 16 p.p. the wages of workers who remained in full-time employment. This effect implies that the *Decent Domestic Work Campaign* has effectively increased compliance with the NMW. However, in terms of poverty reduction, this increase in wages has not reduced significantly the incidence of poverty among domestic workers and among female covered workers in general. The results also show (weak) evidence that the Campaign led to some domestic workers moving from full-time to part-time employment. This transition is a novel result and suggests that some domestic workers' employers started to comply with the NMW law on a part-time basis.

Finally, I do not find a significant effect on wages of salaried women who were earning sub-minimum wages. This implies that the Campaign has not increased compliance with the NMW among this group of workers. Considering that almost half of salaried women with sub-minimum wages work in commerce, the Ministry of Labour can design a special campaign targeting women in this industry if its objective is to increase compliance with the NMW.

Appendix

Table 2.13 Compliance with the Affiliation to the IESS and Noncompliance with the NMW for Full-time Private Covered Workers, in Urban and Rural Areas, December 2007 – December 2014

	Urban Areas					Urban and Rural Areas				
	All	Male Salaried	Female Salaried	Labourer	Domestic Worker[b]	All	Male Salaried	Female Salaried	Labourer	Domestic Worker[b]
A Compliance with the IESS: % of employees affiliated to the IESS by their employers										
Jun-07[a]	42.8	37.8	43.7	0.5	10.4					
Dec-07	41.4	36.1	41.8	1.8	12.5	34.0	33.5	39.5	2.1	11.7
Jun-08	43.8	40.6	44.1	2.0	11.4	36.5	37.7	41.2	2.1	10.6
Dec-08	43.7	40.5	44.7	3.6	13.1	36.0	38.3	42.3	3.9	11.5
Jun-09[a]	47.8	45.7	49.0	1.3	17.3					
Dec-09	48.0	46.9	50.0	2.1	16.6	39.9	44.1	46.5	2.1	14.4
Jun-10	52.7	49.8	54.8	3.7	23.2	43.7	47.8	52.6	2.4	20.4
Dec-10	54.3	53.3	57.8	1.9	22.3	45.2	50.6	54.5	2.1	19.7
Jun-11	58.4	56.0	61.7	2.3	34.3	49.6	54.2	59.0	3.1	31.0
Dec-11	62.2	61.8	66.3	5.4	33.9	53.3	59.8	63.5	4.4	31.6
Jun-12	62.6	62.8	67.5	4.0	37.8	53.7	61.3	65.0	3.8	35.9
Dec-12	64.6	64.9	70.0	5.8	27.8	54.7	62.9	66.7	4.1	27.7
Jun-13	66.2	66.5	70.8	2.0	42.8	56.4	64.7	67.9	2.3	39.4
Dec-13	62.8	64.4	68.0	2.6	34.9	54.5	63.2	65.6	2.3	32.5
Jun-14	67.3	68.4	72.8	3.6	47.8	57.9	66.5	69.9	3.4	43.7
Dec-14	64.3	67.1	67.7	2.9	41.3	58.2	67.2	66.0	2.9	44.3
B Noncompliance with the NMW: % of (full-time) employees earning less than the 95% of the NMW										
Jun-07[a]	27.0	21.7	33.0	50.3	84.5					
Dec-07	26.4	22.3	32.1	47.0	72.1	33.5	24.8	36.0	61.7	74.0
Jun-08	27.5	24.0	35.0	47.3	79.5	33.7	26.2	39.9	59.3	81.3
Dec-08	23.4	18.6	30.7	40.7	71.4	31.0	21.2	34.2	58.1	73.2
Jun-09[a]	31.5	29.1	39.2	54.7	85.3					
Dec-09	29.2	25.7	32.9	52.5	80.7	37.0	28.6	37.1	67.5	83.1
Jun-10	22.8	21.4	25.8	43.7	66.1	30.6	23.2	29.0	61.3	69.7
Dec-10	22.0	19.5	26.2	45.5	61.9	28.7	21.7	29.6	58.0	64.0
Jun-11	25.6	25.2	30.7	52.6	63.2	33.3	27.6	34.2	67.7	66.1
Dec-11	22.3	21.1	25.9	51.4	65.4	29.6	23.1	28.8	66.7	68.0
Jun-12	21.1	18.9	24.4	48.2	54.0	27.4	21.1	26.1	61.0	56.0
Dec-12	16.7	13.9	19.7	39.0	56.7	24.0	15.8	22.1	57.5	60.0
Jun-13	28.2	28.4	34.7	57.6	75.9	34.9	30.3	37.5	69.7	76.3
Dec-13	25.0	24.0	29.4	49.4	68.0	30.6	25.6	31.9	61.9	69.1
Jun-14	25.9	26.1	31.0	55.8	59.3	31.4	27.8	32.4	66.9	60.6
Dec-14	26.9	25.2	35.1	52.1	66.0	30.9	26.7	36.8	60.6	63.1

Source: Author's calculations from the ENEMDU 2007-2014.

[a] The ENEMDU survey of June 2009 covers urban areas only.

[b] Although the minimum wage for domestic workers before 2010 was lower than the NMW, I use the value of the NMW in all years to compute noncompliance with the NMW for this group of workers.

Table 2.14 Labour Force in Ecuador, by sector June 2008 - June 2011

Sector	Urban Areas				Urban and Rural Areas			
	Jun-08	Jun-09	Jun-10	Jun-11	Jun-08	Jun-09[a]	Jun-10	Jun-11
Private Covered Sector	46.0	44.8	44.7	44.6	42.9		42.5	41.8
Uncovered Self-employed Sector	24.8	25.7	27.4	28.4	27.7		29.5	31.0
Business Owners	5.5	4.7	4.4	3.7	5.0		4.1	3.5
Covered Public Sector	10.3	10.3	11.4	11.9	8.0		8.8	9.3
Unpaid Family Workers	7.0	6.0	4.5	5.0	11.3		8.9	9.4
Unemployment	6.4	8.4	7.7	6.4	5.2		6.2	5.0
Total (%)	100.0	100.0	100.0	100.0	100.0		100.0	100.0
Total Labour Force (in million)	4.43	4.52	4.41	4.38	6.59		6.58	6.55

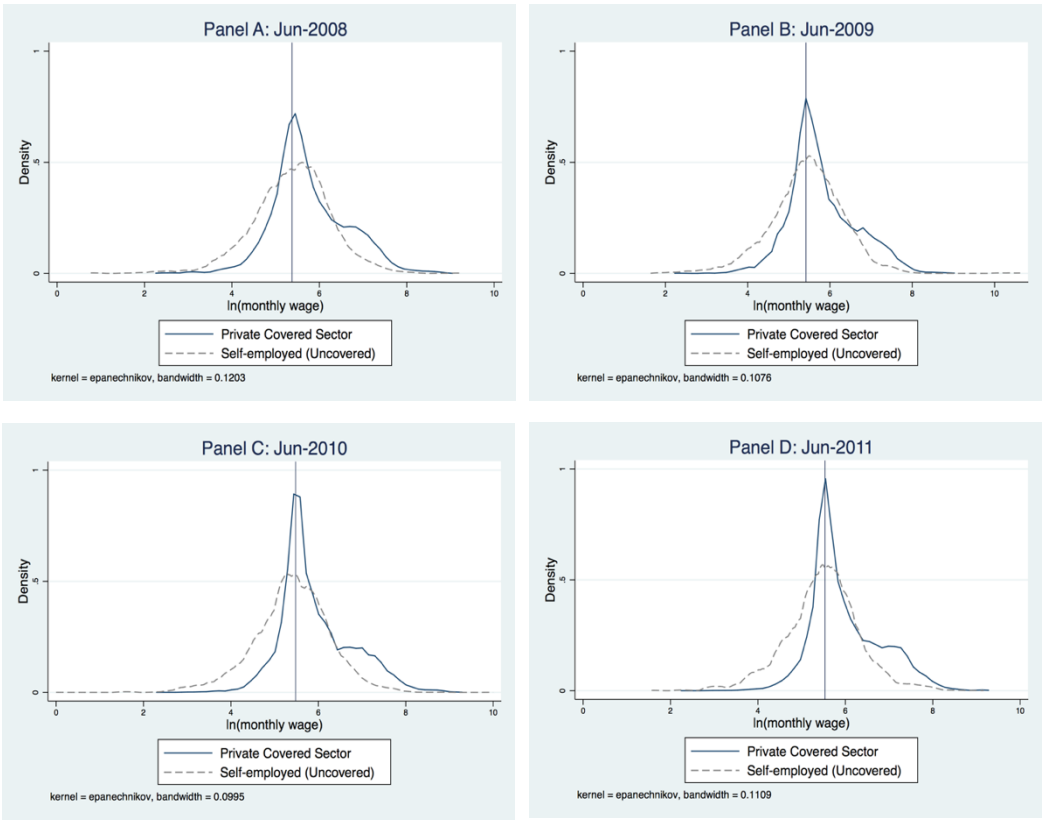
Source: Author's calculations from the ENEMDU June 2008- June 2011.
[a] The ENEMDU survey of June 2009 covers urban areas only

Table 2.15 Workers in the Private Covered Sector, by subgroup June 2008 - June 2011

Subgroup	Urban Areas				Urban and Rural Areas			
	Jun-08	Jun-09	Jun-10	Jun-11	Jun-08	Jun-09[a]	Jun-10	Jun-11
Salaried Men	50.5	50.2	48.4	50.1	43.4		40.8	42.6
Salaried Women	27.6	27.9	29.0	28.8	23.2		23.4	23.5
Labourers	13.7	13.8	13.9	13.2	26.0		28.3	27.1
Domestic Workers	8.2	8.1	8.7	7.9	7.3		7.6	6.8
Total (%)	100.0	100.0	100.0	100.0	100.0		100.0	100.0
Total Workers (in million)	2.04	2.03	1.97	1.95	2.82		2.80	2.74

Source: Author's calculations from the ENEMDU June 2008- June 2011.
[a] The ENEMDU survey of June 2009 covers urban areas only.

Figure 2.4 Kernel Density Estimates of the Distribution of Wages (Earnings) of Full-time Private Sector Employees, and of Full-time Self-employed Workers, June 2008 - June 2011



Source: Author's calculations from the ENEMDU June 2008-June 2011
Note: for the density estimation the Epanechnikov kernel is used as the kernel function and the bandwidth is given by the "rule-of-thumb" proposed by Silverman (1986). The vertical line corresponding to the log of the NMW in each year.

Figure 2.5 Components of the Campaign illustrated in a Ministry of Labour brochure

Source: Ministry of Labour 2011.
Note: a) A labour inpection taking place. b) Cars of the Ministry of Labour. C) The branch of the Ministry of Labour in the City of Manta before the Campaign (left) and after the Campaign (right).

Table 2.16 Number of Labour Inspectors in each provincial branch of the Ministry of Labour, by year

	2006	2007	2008	2009	2010	2011	2012	2013	2014
Azuay	5	8	8	12	16	26	27	30	20
Bolivar	0	0	0	0	0	0	2	1	2
Cañar	4	4	4	5	5	5	5	6	6
Carchi	0	0	0	0	0	3	2	1	2
Cotopaxi	0	0	0	0	0	3	4	3	4
Chimborazo	0	0	0	0	0	4	6	6	5
El Oro	0	0	0	0	0	3	14	14	12
Esmeraldas	0	0	0	0	0	5	6	3	4
Guayas	0	0	0	0	12	59	51	46	35
Imbabura	0	0	0	0	0	3	7	8	5
Loja	0	0	0	0	0	5	12	9	8
Los Rios	0	0	0	0	0	3	4	7	3
Manabí	0	0	0	0	0	14	25	20	18
Morona Santiago	1	1	1	1	1	2	3	3	2
Napo	0	0	0	0	0	3	3	3	3
Pastaza	0	0	0	0	0	3	2	2	2
Pichincha	0	0	14	14	7	57	70	70	54
Tungurahua	0	0	4	4	3	13	10	9	9
Zamora Chinchipe	0	0	0	0	0	2	2	2	2
Galapagos	0	0	0	0	0	1	2	1	2
Sucumbíos	0	0	0	0	0	7	2	3	2
Orellana	0	0	0	0	0	6	6	4	5
Santo Domingo	0	0	0	0	0	10	3	6	7
Santa Elena	0	0	0	0	0	3	1	3	1
Regional Secretariat	0	0	15	17	0	0	0	0	0
Regional Secretariat - Coast	0	0	2	2	0	0	0	0	0
Not specified	0	0	0	0	2	0	0	0	0
Total	10	13	48	55	46	240	269	260	213

Source: Administrative records of the Ecuadorian Ministry of Labour, 2015.

Table 2.17 National Survey of Employment and Unemployment (ENEMDU), 2-2-2 rotation process from June 2007 to September 2015.

ENEMDU Survey Round								
16	**17**	**18**	**19**	**20**	**21**	**22**	**23**	**24**
Jun-07	Sep-07	Dic-07	Mar-08	Jun-08	Sep-08	Dic-08	Mar-09	Jun-09
O0	S0	S0	W0	W0	S0	S0	W0	W0
P0	T0	T0	X0	X0	T0	T0	X0	X0
Q0	Q0	U0	U0	Y0	Q0	U0	U0	Y0
R0	R0	V0	V0	Z0	R0	V0	V0	Z0

25	**26**	**27**	**28**	**29**	**30**	**31**	**32**
Sep-09	Dic-09	Mar-10	Jun-10	Sep-10	Dic-10	Mar-11	Jun-11
S2	S2	W2	W2	S2	S2	W2	W2
T2	T2	X2	X2	T2	T2	X2	X2
Y0	U2	U2	Y2	**Y0**	U2	U2	Y2
Z0	V2	V2	Z2	**Z0**	V2	V2	Z2

33	**34**	**35**	**36**	**37**	**38**	**39**	**40**
M39	M42	M45	M48	M51	M54	M57	M60
Sep-11	Dic-11	Mar-12	Jun-12	Sep-12	Dic-12	Mar-13	Jun-13
S4	S4	W5	W5	S4	S4	W5	W5
T4	T4	X4	X4	T4	T4	X4	X4
Y2	U4	U4	Y4	Y2	U4	U4	Y4
Z2	V4	V4	Z4	Z2	V4	V4	Z4

Source: National Institute of Statistics and Census (INEC).

Table 2.18 Comparison of Distribution of Working-Age Population across Sectors between the Complete ENEMDU and the Baseline Surveys of the Panel Data Sets

Labour Sector (or employment status)	Jun-08		Jun-10	
	Complete ENENDU	Baseline Survey of Panel 1	Complete ENENDU	Baseline Survey of Panel 2
Private Covered Sector	32.2	30.2	27.6	27.9
Uncovered Self-employed Sector	17.4	18.6	18.6	19.5
Business Owners (Patronos)	4.3	4.3	2.9	3.2
Public Sector	7.8	7.8	9.0	8.2
Unpaid Family Workers	5.3	5.6	3.6	3.4
Unemployment	4.4	4.1	5.0	4.7
Not in Labour Force	28.6	29.4	33.4	33.1
Total (%)	100.0	100.0	100.0	100.0
Total individuals aged 15 to 70	17,398	11,144	29,325	10,974

Source: Author's calculations from the ENEMDU-June 2010 and its sub-sample panel data set in June 2010. Sample includes all individuals aged 15 to 70 years living in urban areas.

Table 2.19 Descriptive Statistics for Uncovered Self-employed Workers with Earnings Below, At, and Above the NMW in June 2010

	Full-time Workers Only			Both Full-time and Part-time workers		
Characteristics (% in each category)	Below the NMW	At the NMW	Above the NMW	Below the NMW	At the NMW	Above the NMW
Gender						
Male	71.5	62.9	79.1	63.5	58.1	74.1
Female	28.5	37.1	20.9	36.5	41.9	25.9
Ethnicity						
Mestizo / White / others	93.9	93.3	89.9	94.4	94.0	91.4
Indigenous / Afro	6.1	6.7	10.1	5.6	6.0	8.6
Age						
15-24	3.9	2.2	2.2	4.8	2.6	2.9
25-34	16.2	16.9	18.7	15.3	16.2	20.1
35-44	29.6	20.2	28.1	26.5	25.6	27.0
45-54	25.7	28.1	23.0	25.7	26.5	25.9
54-64	19.6	23.6	19.4	22.5	22.2	17.2
65+	5.0	9.0	8.6	5.2	6.8	6.9
Education						
Primary (1–6years)	37.4	43.8	41.7	39.4	40.2	36.2
Secondary (7–12years)	44.7	38.2	47.5	43.8	38.5	48.3
University (13–21 years)	17.9	18.0	10.8	16.9	21.4	15.5
Industry						
Agriculture	2.8	3.4	4.3	2.8	3.4	3.4
Manufacturing	14.0	11.2	11.5	14.1	10.3	12.1
Service	3.9	9.0	2.2	6.8	9.4	6.3
Construction	7.8	5.6	9.4	8.0	6.0	8.6
Commerce	47.5	44.9	45.3	49.0	47.0	43.7
Transportation	16.8	21.3	23.0	12.9	18.8	21.3
Finance	7.3	4.5	4.3	6.4	5.1	4.6
Number of Observations	179	89	139	249	117	174

Source: Author's calculations from the ENEMDU panel data set June2010-June2011. The sample is restricted to individuals aged 15-70 who reported being self-employed workers in June 2010.

Table 2.20 OLS Difference-in-Difference Estimates of the Effect of the Campaign on Wage Growth of Fulltime Covered Workers in Baseline Surveys Who Report Being Full-time or Part-time Covered Workers in Follow-up Surveys, by groups

	For Fulltime Workers in Baseline Surveys Who Report Being Full-time or Part-time Workers in Follow-up Surveys			
	Salaried Men	Salaried Women	Labourer	Domestic Worker
	(1)	(2)	(3)	(4)
Raw OLS difference-in-differences estimates				
C*Below	0.092*	-0.038	0.094	0.009
	(0.053)	(0.053)	(0.076)	(0.082)
C*At	0.059	0.07	0.118*	0.146*
	(0.037)	(0.047)	(0.064)	(0.077)
OLS difference-in-differences estimates with controls				
C*Below	0.092*	-0.024	0.115	0.018
	(0.052)	(0.053)	(0.078)	(0.080)
C*At	0.048	0.068	0.126**	0.140*
	(0.036)	(0.046)	(0.062)	(0.076)
N	1,288	1,041	825	732

Source: Author's calculations from the ENEMDU panel data sets: June 2008-June 2009 and June2010-June2011.
Note: Robust standard errors are shown in parentheses. The sample is restricted to full-time private covered workers in the baseline surveys who report being full-time or part-time private covered workers in the follow-up surveys. The treatment group is the subgroup of covered workers indicated in each column. The comparison group is the same as that for Table 3 (covered workers with wages above the NMW).
*10%, **5%, and ***1% significance level.

Table 2.21 Difference-in-Difference Estimates of the Effect of the Campaign on the Probability of Leaving the Private Covered Sector for Another Sector/Status, for Different Groups of Private Covered Workers

ç	Full-time Workers Only			
	C*Below		C*At	
	Marginal Effect	Std. Err.	Marginal Effect	Std. Err.
Panel A: Origin is Salaried Men				
Self-employed	-0.033	(0.039)	-0.03	(0.031)
Public	0.014	(0.018)	-0.023*	(0.014)
Unemployed	0.033	(0.031)	0.006	(0.022)
Not in Labour Force	0.011	(0.022)	0.025	(0.018)
Part-time Private Covered	0.026	(0.028)	0.003	(0.018)
N	1,635			
Panel B: Origin is Salaried Women				
Self-employed	0.008	(0.037)	-0.002	(0.028)
Public	-0.012	(0.012)	-0.014	(0.017)
Unemployed	-0.045	(0.031)	-0.01	(0.029)
Not in Labour Force	0	(0.038)	0.054	(0.033)
Part-time Private Covered	0.038	(0.040)	0.037**	(0.019)
N	1,331			
Panel C: Origin is Labourer				
Self-employed	0.078	(0.055)	0.048	(0.058)
Public	-0.005	(0.009)	-0.005	(0.009)
Unemployed	0.049	(0.055)	-0.028	(0.034)
Not in Labour Force	0.014	(0.014)	0.01	(0.012)
Part-time Private Covered	0.019	(0.056)	0.001	(0.055)
N	1,034			
Panel D: Origin is Domestic Worker				
Self-employed	-0.024	(0.061)	0.007	(0.040)
Public	-0.005	(0.010)	-0.005	(0.010)
Unemployed	-0.008	(0.040)	-0.031	(0.032)
Not in Labour Force	0.075	(0.062)	-0.07	(0.079)
Part-time Private Covered	0.1	(0.085)	-0.002	(0.072)
N	915			

Source: Author's calculations from the ENEMDU panel data sets: June 2008-June 2009 and June2010-June2011.
Note: Robust standard errors are shown in parentheses. The sample is restricted to full-time private covered workers in the baseline surveys. The treatment group is the sub-group of covered workers indicated in each Panel. The comparison group is the same as that for Tables 5: private covered workers who were earning above the NMW. The marginal effect of interactions terms derived from the mlogit coefficient estimates and are evaluated at the sample means of the explanatory variables.
*10%, **5%, and ***1% significance level.

Table 2.22 Difference-in-Difference Estimates of the Effect of the Campaign on the Probability of Subsequent Employment for Self-employed Uncovered Workers

	Full-time Workers Only			Both Full-time and Part-time Workers		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Raw OLS difference-in-differences estimates						
C*Below	0.065	0.012	0.207	-0.061	-0.072	-0.014
	(0.087)	(0.105)	(0.166)	(0.071)	(0.089)	(0.123)
C*At	0.002	-0.012	0.092	-0.028	-0.01	-0.034
	(0.096)	(0.112)	(0.177)	(0.079)	(0.099)	(0.135)
Probit difference-in-differences estimates with controls						
C*Below	0.057	-0.01	0.233	-0.084	-0.108	-0.02
	(0.088)	(0.106)	(0.168)	(0.072)	(0.090)	(0.124)
C*At	0.002	-0.024	0.151	-0.035	-0.038	0.006
	(0.096)	(0.113)	(0.182)	(0.081)	(0.101)	(0.135)
N	734	511	223	1,009	650	359

Source: Author's calculations from the ENEMDU panel data sets: June 2008-June 2009 and June2010-June2011.
Note: Robust standard errors are shown in parentheses. The sample is restricted to (full-time) self-employed workers in the baseline surveys. The treatment group is the group of self-employed workers indicated in each column. The comparison group are self-employed workers who were earning above the NMW. Control variables are: age, years of schooling, and industry. The marginal effect of interactions terms derived from the probit coefficient estimates and are evaluated at the sample means of the explanatory variables.
*10%, **5%, and ***1% significance level.

Table 2.23 OLS and Probit Difference-in-Difference Estimates of the Effect of the Campaign on Poverty for Different Groups of Private Covered Workers

	Full-time Workers Only				Both Full-time and Part-time Workers			
	Salaried Men	Salaried Women	Labourer	Domestic Worker	Salaried Men	Salaried Women	Labourer	Domestic Worker
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Raw OLS difference-in-differences estimates								
C*Below	-0.088*	-0.042	-0.078	-0.009	-0.078	-0.042	-0.119	-0.036
	(0.049)	(0.045)	(0.099)	(0.064)	(0.049)	(0.043)	(0.083)	(0.072)
C*At	0.014	0.031	-0.101	-0.074	0	0.011	-0.144*	-0.065
	(0.034)	(0.032)	(0.077)	(0.059)	(0.034)	(0.032)	(0.076)	(0.049)
Probit difference-in-differences estimates with controls								
C*Below	-0.071*	-0.075	-0.066	-0.03	-0.063	-0.069	-0.097*	-0.049
	(0.041)	(0.057)	(0.064)	(0.113)	(0.042)	(0.052)	(0.058)	(0.091)
C*At	0.018	0.041	-0.051	0	0.01	0.015	-0.088*	0
	(0.030)	(0.038)	(0.046)	(.)	(0.030)	(0.040)	(0.052)	(.)
N	1,228	981	772	689	1,364	1,104	874	762

Source: Author's calculations from the ENEMDU panel data sets: June 2008-June 2009 and June2010-June2011.
Note: Robust standard errors are shown in parentheses. The sample is restricted to (full-time) private covered workers in the baseline surveys who remain employed as (full-time) private covered workers in the follow-up surveys. The treatment group is the subgroup of covered workers indicated in each column. The comparison group is the same as that for Table 10 (covered workers with wages above the NMW). Control variables are: gender, ethnicity, age, years of schooling, and industry.
*10%, **5%, and ***1% significance level.

3 The Impact of the National Minimum Wage Policy on Wage Inequality in Ecuador

3.1 Introduction

Minimum wage laws have been an effective policy tool in shaping the distribution of wages in developed countries. Research into the cases in the US, UK and Canada shows that the introduction, or increase (in real terms) in the value, of a binding minimum wage significantly reduced wage dispersion (see (Card and Krueger 1994b) for the US; Dickens et al. 1999 for the UK; Fortin and Lemieux 2000 for Canada) or, alternatively, that the fall in the real value of the minimum wage increased wage inequality (see DiNardo et al. 1996 and Lee 1999 for the US; Machin and Manning 1994 for the UK). In developing countries, however, such a direct relationship between changes in the minimum wage and wage dispersion has not been evident. For example, in the case of Honduras (Gindling and Terrell 2009), Uruguay (Borraz and Pampillon 2011) and Thailand (Leckcivilize 2015), researchers have found that increases in the real value of the minimum wage have not compressed the wage distribution among covered workers because of a lack of compliance with the law.

In this paper, I analyse the distributional effects of increases in both of these components of the national minimum wage (NMW) policy in the case of Ecuador: the level of the minimum wage and the intensity of enforcement of the law. To the best of my knowledge, this paper is the first attempt to evaluate the compression effect of the minimum wage policy when there is an increase not only in the level of the minimum wage but also in the intensity of enforcement of the labour law. The contribution of this paper is relevant considering that the increase in the other component of the NMW policy of Ecuador resulted in a significant improvement in compliance with the law. This case is important since the problem of noncompliance with the minimum wage is widespread in developing countries (Marshall 2007; Rani et al. 2013).

To estimate the effect that the changes in both components of the NMW policy had on wage dispersion during the period 2000 – 2016, I use the methodology proposed by Lee (1999). Lee (1999) exploits the state-variation in the effective minimum wage, which is defined as the difference between the statutory minimum wage and the median wage in each state, to identify the impact of changes in the level of the minimum wage in the US. In order to account for the changes in noncompliance with the law, and to capture the improvements that took place in Ecuador in 2011, under an enforcement campaign, I adjust Lee's (1999) measure of the effective

minimum wage of each province by the level of noncompliance in that province. I implement this adjustment based on the definition of the Kaitz index (Kaitz 1970). This adjustment is a contribution to the empirical literature for the future implementation of Lee's (1999) method to countries with high noncompliance.

The results suggest that the NMW policy has indeed compressed the wage distribution in Ecuador. Specifically, I found that the steady increase in the real value of NMW through the period 2000 to 2016, and the increase in the enforcement of the NMW law since 2011, increased the wages of workers located up through the 60th percentile, although the magnitude of the impact decreases as one moves upwards in the wage distribution. Based on a counterfactual analysis, I also find that these changes in the NMW policy explain around a half of the decline in wage inequality that took place between 2000 and 2016.

One limitation of the methodology used, and hence the results obtained, is that it ignores the potential negative effects that changes in NMW policies can have on employment levels. This prediction is exactly what I found in a previous study where the results show that the increases in both the value of the NMW and in the intensity of enforcement of the law lead to employment losses in the covered sector (Guzman 2017). By restricting the analysis to all salaried workers with nonzero (or negative) wages, I do not include unemployed workers and those who migrated from the covered sector to the uncovered self-employment sector, as consequence of the changes in the NMW policy.

This study focused exclusively on the direct effects that changes in the NMW policy have on the distribution of wages among covered workers. In Ecuador, the NMW law covers all employees in the economy, regardless of the sector in which they work, e.g. the formal or informal sector. Hence, both employees in small establishments (with less than five or ten workers) and in unregistered firms, regarded as informal enterprises by the International Labour Organization (ILO 2002), are covered by the NMW law. The workers that are not covered by the NMW law are the self-employed and business owners, because they cannot be forced to pay themselves the NMW.

The reminder of this paper proceeds as follows: Section 3.2 presents a brief review of the theoretical approaches and empirical literature concerning the impact of minimum wage policies on wage inequality. Section 3.3 provides a background to the NMW policy in Ecuador. Section 3.4 describes and discusses the data. Section 3.5 describes the empirical strategy, while the regression results are presented in Section 3.6. In section 3.7 a counterfactual analysis is performed. Section 3.8 concludes.

3.2 Related literature

The effects of the minimum wage policy in developing countries is usually analysed using two-sector models where workers are classified into the covered and uncovered sector. For covered workers, the models predict that a binding minimum wage will compress the wage distribution, albeit with the risk of employment losses under a competitive market setting, and with no disemployment effects under an imperfectly competitive labour market (Freeman 1996). In both scenarios, the models assume perfect enforcement, and by implication, complete compliance with the minimum wage in the covered sector. This assumption is at odds with a growing body of empirical evidence, especially in developing countries (see Marshall 2007; Rani et al. 2013, for a review). Despite this problem, most empirical studies have focused solely on measuring the effect on the labour market of changes in the value of the minimum wage (see Maloney and Mendez 2004; Neumark and Wascher 2008, for a literature review).

Some studies that focus on the distributional effects, in particular, suggest that the problem of noncompliance is the reason why the minimum wage policy has not been an effective redistribution tool. For example, Gindling and Terrell (2009), for the case of Honduras, and Leckcivilize (2015), for Thailand, find that the minimum wage affects only the wage distribution of workers in large firms, and that this policy has no effect in the small scale private sector because of noncompliance. Similarly, Borraz and Pampillon (2011), for the case of Uruguay, find that the increase in the real value of the minimum wage after 2004 had no impact on wage inequality because of a lack of compliance with the law.

Basu et al. (2010) observe that the problem of noncompliance is the reason why the theoretical predictions of the standard models have not been replicated in practice, especially in developing countries. The authors relax the assumption of perfect enforcement and develop a theoretical model of the minimum wage policy, under an imperfectly competitive labour market. They predict that an increase in the intensity of enforcement will increase the wages of workers whose employers start to comply with the minimum wage law.

If there are increases in both components of the minimum wage policy: in the level of the minimum wage and in the intensity of its enforcement, one will expect that these changes will increase wages of workers in the lower tail of the wage distribution to exactly the new wage floor. Assuming that workers earning above the minimum wage are not affected by the changes in the minimum wage, i.e. no spillover effects, the increase in wages for those who were earning below the minimum will result in a compression of the wage distribution (the "censoring" effect hypothesis). Alternatively, the compression effect of the changes in the

minimum wage policy will be weaker if the value of the minimum acts as an index and wages above the minimum are set as multiples of the minimum wage. Even under this scenario, however, as long as the spillover effects gradually diminish as one moves upwards in the wage distribution, the minimum wage policy will still reduce wage inequality.

In this study, I aim to contribute to the literature by testing these predictions. Specifically, I exploit the fact that the Ecuadorian government, apart from increasing the value of the NMW (in real terms), implemented an enforcement programme to improve compliance with the NMW. These policy changes allow me to estimate the distributional effect of changes in both components of the minimum wage. To the best of my knowledge, this is the first paper to do so. I perform the analysis with three specific objectives in mind. The first is to evaluate if the changes in the NMW policy have influenced the wage distribution. The second is to observe if the NMW policy has increased the wages of workers located at the lower percentiles by more than those located in higher percentiles, thus reducing wage dispersion. Finally, a third objective is to estimate how much of the decline in wage inequality can be attributed to the changes in the NMW policy.

3.3 The Changes in Both Components of the National Minimum Wage Policy in Ecuador

In Ecuador, the NMW law covers all employees in the economy, regardless of the sector in which they work, e.g. the formal or informal sector. Employees have to be affiliated by their employer to the Ecuador Social Security Institute (IESS is its Spanish acronym), which implies, in theory, compliance with the NMW. The workers that are not covered by the NMW law are self-employed individuals and business owners because these workers cannot be forced to affiliate themselves IESS and pay themselves the NMW.

The value of the NMW is fixed by the Consejo Nacional de Salarios (CONADES is the Spanish acronym). This is a tripartite committee composed by one representative from the Ministry of Labour, one from private employers, and one from workers' bodies. The members of the CONADES have to set the NMW by consensus. If they cannot agree, the Ministry of Labour sets the NMW, which is generally the case. Additionally, the NMW is used by Sectorial Councils as a floor in order to fix the sectoral minimum wages. This approach to setting the minimum wages implies, in theory, that all employees in the economy should earn at least the NMW.

Since the dollarization of the economy in 2000, the NMW is set annually, and is published by the Ministry of Labour in December of the preceding year. From 2000 to 2016, the real value of the NMW has increased steadily (see Figure 3.1), with the largest increases taking place in the years 2001, 2002 and 2008 (See Figure 3.1 and Table 3.6 in the Appendix). The increases set in 2000 and 2001, applicable for the year 2001 and 2002, respectively, were mainly aimed at recovering the real value of the NMW after the high inflation of the late 1990s, during which the nominal value of the NMW was kept constant (at 100.000 Sucres). The other large increase in the NMW, applicable for the year 2008, was enacted in 2007 when a new government took office.

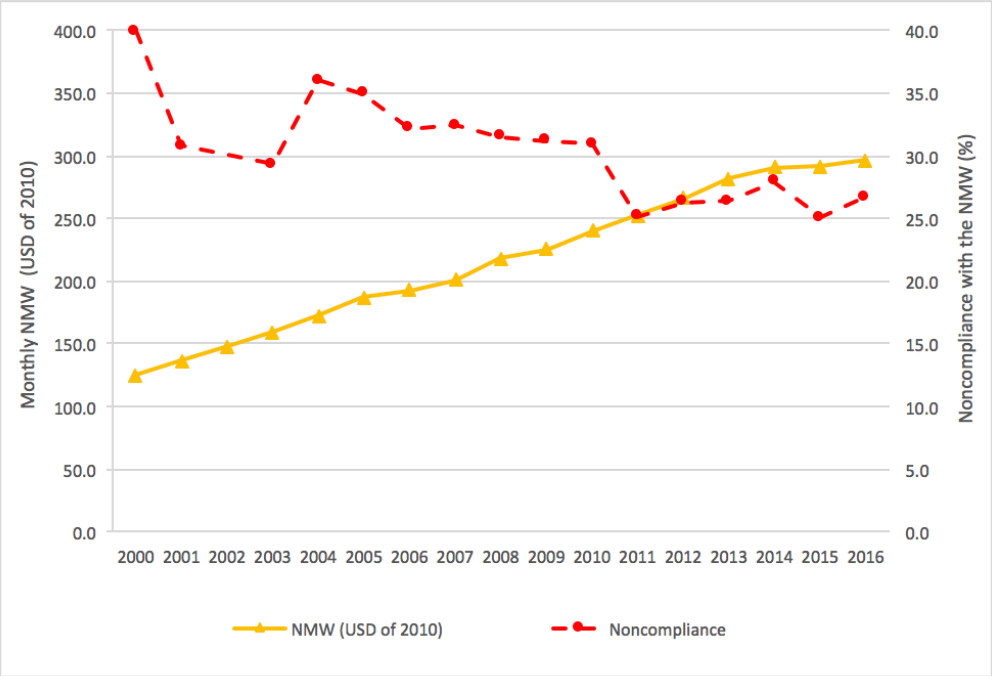
Regarding the other component of the NMW policy, in 2010 the Ministry of Labour launched a campaign programme (hereafter the Campaign) to increase the intensity of enforcement of the law, which resulted in a significant reduction in noncompliance in 2011 (see Figure 3.1). I capture noncompliance with the NMW law using the percentage of full-time employees who are paid less than 90% of the value of the monthly NMW, in November-December each year.

In this study, I exploit the exogenous variation in noncompliance rates resulted from the Campaign. Originally launched in March 2010, the Campaign was targeted at domestic workers and their employers. In January 2011, the government extended the programme to cover all salaried workers in the economy. Under this broader Campaign, the number of labour inspectors increased from 46 in 2010 to 240 in 2011, distributed across all provinces in Ecuador so as to provide staff to the new branches of the Ministry of Labour (see Table 3.5 in the Appendix). Furthermore, to signal the credible enforcement of the NMW, in a referendum in May 2011, the government asked Ecuadorians whether employers that did not affiliate their employees to the Ecuador Social Security Institute (IESS is its Spanish acronym) should go to prison. This question was approved with 55.02% of valid votes and created awareness in respect to this issue within the Ecuadorian society.

The changes resulting from the Campaign can be regarded as an exogenous variation in the intensity of enforcement of the NMW law. The government's decision to start the Campaign programme in 2010 was not driven by an increase in noncompliance, rather it started as a strategy of the government to protect the most vulnerable employees in the economy, the domestic workers. Before the Campaign started, domestic workers used to have a minimum wage set at a lower level than the NMW, and less than 15% of them were affiliated to the IESS (see Panel A of Table 2.13 in Chapter 2). By the end of 2010, however, the government realised

that the Campaign for domestic workers not only increased compliance with the affiliation to the IESS among domestic workers, from 14% to around 20%, but also among all private employees, from 40 to 45% (see Panel A of Table 2.13 in Chapter 2). This motivated the government to extend the scope of the programme by including all private employees.

Figure 3.1 The National Minimum Wage (monthly value in USD as of 2010) and Noncompliance



Source: The real value of the NMW is computed using the nominal value published by the Ministry of Labour and the CPI Historical Series of the INEC. Noncompliance with the NMW is estimated from the ENEMDUR 2000-2016, excluding 2002 which covers urban areas only. The ratio of noncompliance corresponds to the percentage of full-time employees who are paid less than 90% of the value of the monthly NMW, in each year.

Both the steady increase in the real value of the NMW and the improvement in compliance with the NMW law suggest the potential role of the NMW policy in compressing the wage distribution in Ecuador. In the next section, I discuss the data used this study and present some basic trends in wage inequality.

3.4 Data and Basic Trends in Wage Inequality

3.4.1 Data

I use microdata from the ENEMDUR (Encuesta Nacional de Empleo y Desempleo Urbana y Rural) between 2000 and 2016. The ENEMDUR has been carried out around November each year since 2000, by the National Institute of Statistics and Census (INEC is its Spanish

acronym). It covers both urban and rural areas, with the only exception being in 2002 when the INEC surveyed urban areas only. These surveys are representative at the national, regional, provincial and large cities levels, and are the official data set used in Ecuador to compute and monitor labour market indicators, and measures of income poverty and income inequality.

The ENEMDUR collects data on individual characteristics for every member of the household including age, working status and labour sector, the wages of salaried employees, the earnings of self-employed workers, and the hours worked for all employed individuals. Salaried employees are covered by the NMW law and this is the main group of analysis in this study. This group of workers includes public sector employees, private salaried workers, labourers and domestic workers. Additionally, I use the group of uncovered self-employed individuals as a placebo group; I test my identification strategy by analysing the effects of the NMW on their earnings distribution.

The wages of covered workers, and labour earnings of self-employed individuals, as reported in the ENEMDUR, are gross of taxes. The questionnaire asks each worker for up to three sources of labour income: main job, secondary job and other job; and also asks for different sources of non-labour income. The wage (earnings) variable used in this study refers to the hourly wages (earnings) of workers (both full-time and part-time) in their main job. I compute this variable from the reported monthly wage (earnings) of workers, without subtracting any deduction or adding any payment in kind, divided by the number of usual hours worked. Additionally, as a robustness check, I also present the results using the reported monthly wages (earnings) of full-time workers only. This exercise helps to reduce any measurement error that might come from combining monthly wages (earnings) and usual hours worked (per week) to generate the hourly wage (earnings) variable.

In order to maintain comparability with other studies for Ecuador that analyse the effects of minimum wages in the labour market (see Canelas 2014; Wong 2017), I restrict the sample to workers aged between 15 and 70 years. I also exclude workers with wages (earnings) below the bottom, or above the top, percentile, in each province and year. Using this individual data, I calculate all percentiles of provincial wage distributions of covered workers for the period 2000-2016. I also compute all percentiles of provincial labour earnings distribution of uncovered self-employed workers. Following Autor et al. (2016), I calculate the percentiles weighting individual observations by their ENEMDUR sampling weights multiplied by their weekly hours worker. Finally, I aggregate the data across provinces and years in order to construct my cross-sectional/time series panel, where the number of provinces $N = 20$ and the number of years $T = 16$.

The analysis is performed using this province-year level data. The average number of workers per province (per year) is 847 for covered workers and 547 for uncovered workers (see Tables 3.7 and 3.8 in the Appendix for the sample size by year and province for covered and uncovered workers, respectively). I use provincial wage (earnings) distributions for the original 20 provinces of Ecuador because the ENEMDUR, since it was first collected in 2000, is representative at this level.[6] Finally, there is heterogeneity in the level of wages between provinces and also in the rates of compliance with the NMW law. This heterogeneity generates variation across provinces in our measures of both components of the NMW policy, which helps to capture the effects of the policy (to check the data for each province see Figures 3.6 in the Appendix).

3.4.2 Basic Trends in Wage Inequality

Using the restricted individual-level data, I first show the evolution of the wage distribution for the period 2000-2016. Specifically, in Figure 3.2, I display kernel density estimates of the log hourly (real) wage distribution for the years 2000, 2005, 2010 and 2016. The density estimates are based on the Epanechnikov kernel with optimal bandwidth given by the "rule-of-thumb" proposed by (Silverman 1986). Additionally, Figure 3.2 shows three vertical lines which refer to the (log) of the real value of the NMW in the years 2000, 2005 and 2016.

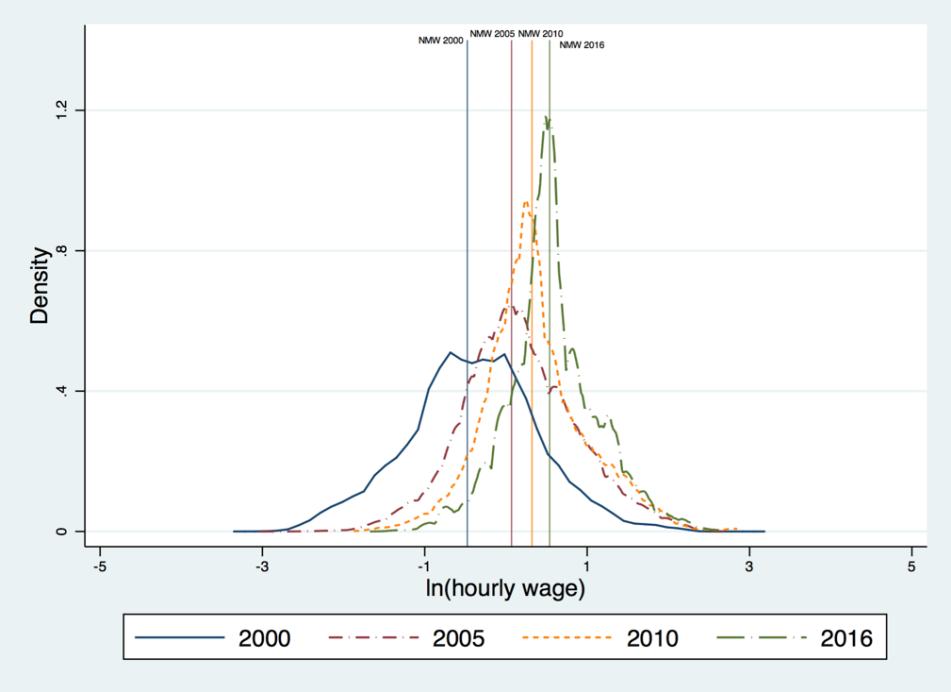
The kernel density estimates for 2000 and 2005, in Figure 3.2, show that the value of the NMW is located around the median, meaning therefore that around half of salaried workers have wages that are located below the NMW. This suggests a clear problem of noncompliance with the minimum wage law. In 2010 and 2016, however, the wage distributions show evidence of a spike at the NMW and a compression of the distribution. From the cumulative distribution function shown in the Appendix (see Figure 3.4), we can also note that around 50-60 percent of hourly wages are located at or below the value of the NMW, which suggests that hourly wages at or above the 70th percentile are not affected directly by the NMW.

Using the province-level data, next, I describe the evolution of the of the 10th, 30th, 50th and 90th percentiles of the hourly wage distribution relative to the 70th percentile. I define this difference as percentile gaps, which are I compute by subtracting from each percentile *(p)*, by province and year, the corresponding 70th percentile $\left(w_{s,t}^{(p)} - w_{s,t}^{70}\right)$. The data shown in Figure 3.3 refers to the yearly change in the average of each percentile gap, across all 20 provinces,

6 The 20 provinces are part of mainland Ecuador. The Galapagos Islands, which is also a province of Ecuador, is not included in this analysis.

standardised to its value in 2000 (to check the data for each province see Figure 3.7 in the Appendix). This data derives from a weighted regression of each percentile gap on year and provincial dummies, with regression weights given by the total of the ENEMDUR sampling weights multiplied by salaried workers' weekly hours worked for each province and year.

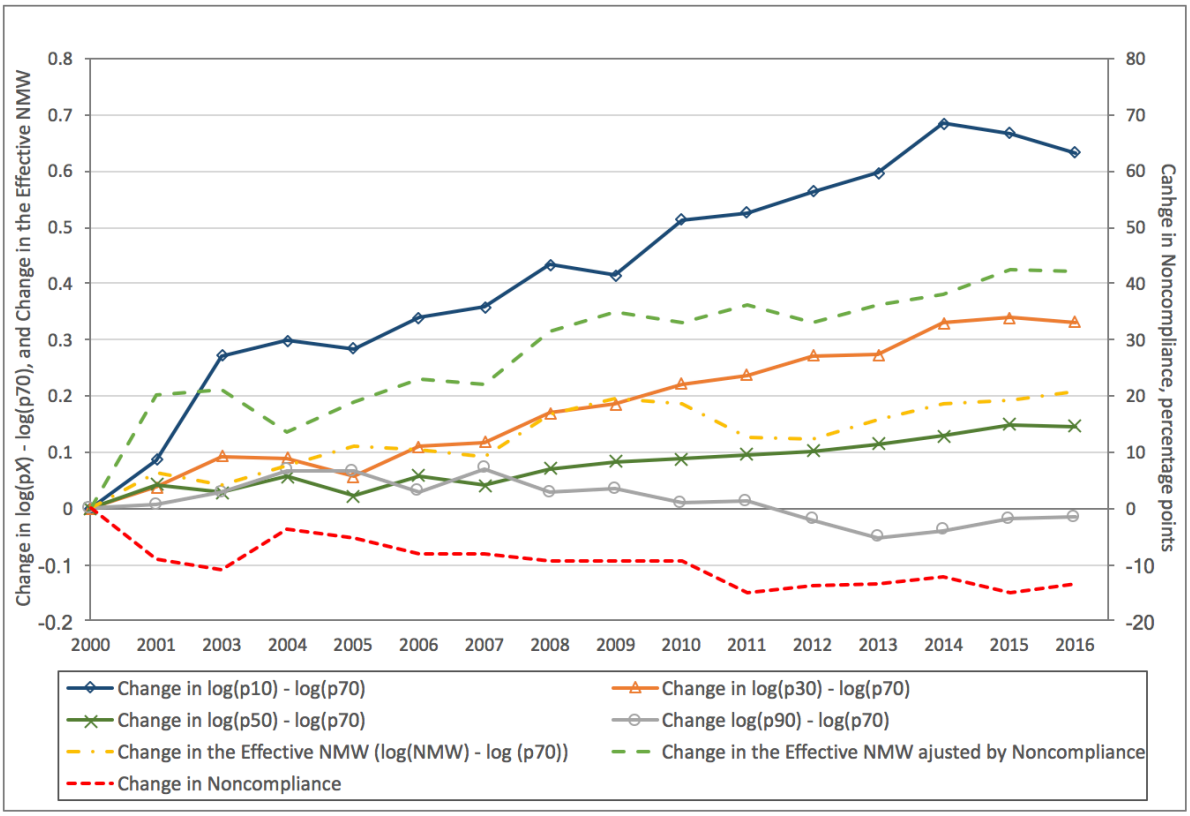
Figure 3.2 Kernel Density Estimates of the Log Hourly Wage Distribution of Covered Workers, 2000 - 2016



Source: Author's calculations from the ENEMDUR 2000-2016.
Note: for the density estimation, the Epanechnikov kernel is used as the kernel function and the bandwidth is given by the "rule-of-thumb" proposed by (Silverman 1986). The vertical line corresponding to the log of the hourly NMW in each year.

Overall, the data in Figure 3.3 shows a decline in wage inequality, especially since 2006. The 10th percentile, relative to the 70th percentile, increases by around 0.60 log points between 2000 and 2016. Or, equivalently, wage inequality at the bottom of the distribution measured by the 70-10 percentile gap reduces by approximately 0.60 log points. Wage inequality at the lower-middle and middle of the distribution remains relatively constant until 2005 and then it decreases from 2006 onwards. Between 2005 and 2016, the 70-30 percentile gap falls by approximately 0.28 log points and the 70-50 percentile gap decreases by around 0.12 log points. Finally, wage inequality at the top of the distribution remains relatively constant throughout the period of analysis.

Figure 3.3 Trends in Wage Inequality and the National Minimum Wage (NMW) Policy: Ecuador 2000-2016



Source: Author's calculations from the ENEMDUR 2000-2016.
Notes: Figure 3.3 depicts the evolution of the difference between the log of percentiles 10th, 30th, 50th, 90th and the log of the 70th percentile, computed for each province. The effective NMW results from the differential between the value of the NMW and the 70th percentile of each province. Noncompliance is measured by the percentage of full-time employees who are paid less than 90% of the value of the monthly NMW, in each province. The NMW adjusted for noncompliance refers to an index of the NMW policy which adjusts the effective NMW by the rate of noncompliance. All series refer to the weighted average of the estimates across the 20 provinces of continental Ecuador and are standardised to their value in 2000.

The falling trend in wage inequality during the 2000s goes in line with the findings of Ponce and Vos (2014), who conclude, based on the evolution of the Gini index of the labour income distribution, estimated from the ENEMDUR, that most of the reduction in earnings inequality occurred during the first half of the decade. My estimates of the Gini indexes of the labour income distribution and wage distribution confirm this conclusion (see Figure A2 in the Appendix). When I analyse the evolution of the percentile gaps, however, as shown in Figure 3, I find that this statement is true only for the reduction of inequality at the bottom of the distribution. The 70-10 percentile gap decreases by 0.28 log points during the first half of the 2000s and then by 0.23 log points during the second half. On the other hand, inequality at the lower-middle (measured by the 70-30 percentile gap) and middle of the distribution (measured by the 70-50 percentile gap) remains relatively constant during the period 2000-2005.

Ponce and Vos (2014) argue that the significant drop in earnings inequality during the first half of the decade is explained by the recovery in real wages and employment after the 1999 financial crisis. Based on the results in Figure 3.3, this may be the case for inequality at the bottom of the distribution, but not for inequality at the middle of the distribution. From 2006 onwards, wage inequality decreases markedly, not only at the bottom but also at the lower-middle and middle of the wage distribution. The question that arises is what force, or policy, explains these reductions in wage inequality.

Figure 3.3 reveals that the changes in wage dispersion in the lower half of the wage distribution move in close tandem with the changes in the NMW policy. Wages at the 10th, 30th and 50th percentiles, relative to wages at 70th percentile, and the effective NMW adjusted for noncompliance, show an upward trend from 2006 onwards. The upward trend in the effective NMW adjusted for noncompliance results from the combined changes in the two components of the NMW policy. For example, between 2007 and 2008, the impact of the value of the NMW measured by the (unadjusted) effective NMW increases significantly, but noncompliance remains relatively constant. In contrast, between 2010 and 2011, the (unadjusted) effective NMW decreases but noncompliance reduces significantly by around 6 p.p.

3.5 Methodology

To identify the effects of the NMW on the wage distribution, I follow the method proposed by Lee (1999) and the modifications to it suggested by Autor et al. (2016). In both papers, the authors assess the effects of the changes in the value of the legislated minimum wage in the United States, exploiting the state-variation in the relative level of the minimum wage with respect to the median wage of each state. Lee (1999) defines the effective minimum wage, $\widetilde{mw}_{s,t}$, for each state s in year t as $\left(max\left[\log\left(\mathrm{MW}_{s,t}\right), \log\left(\mathrm{NMW}_{,t}\right)\right] - \log\left(w_{s,t}^{50}\right)\right)$, i.e. the maximum between the log of the state and federal minimums minus the log median wage. Identification is based on the fact that the states where the minimum wages bite more, i.e. those with a higher effective minimum wage, will experience a greater compression in wages than the states with a lower effective minimum wage.

Lee (1999) assumes that the level of wage inequality in state s at time t will be determined by the level of latent wage inequality (i.e. wage inequality in the absence of a minimum wage policy) captured by time effects, and the effective minimum wage. The key identification assumption, in this case, is that the state-variation in latent wage inequality is uncorrelated with states' median wage levels. Autor et al.(2016) argue that this is not the case

for the US; they find a positive correlation between states' median wage levels and a measure of states' wage inequality that is unlikely to be affected by the level of the minimum wage (the p60/p40 ratio). They propose using state fixed effects and state trends to correct the bias in Lee's estimates.

Lee's (1999) model, with Author's et al. (2016) modifications to estimate the effect of changes in the value of the minimum wage on inequality at any point of the wage distribution, for (in this paper) province *s* in year *t*, which includes province fixed effects and provincial linear trends is of the form:

$$\begin{aligned} log\left(w_{s,t}^{p}\right) - log\left(w_{s,t}^{q}\right) = \beta_1^p\left(\log\left(NMW_t\right) - \log\left(w_{s,t}^{q}\right)\right) \\ + \beta_2^p\left(\log\left(NMW_t\right) - log\left(w_{s,t}^{q}\right)\right)^2 \\ + \delta_t^p + \gamma_{s0}^p + \gamma_{s1}^p * T + \boldsymbol{X'}_{s,t}\delta^p + \epsilon_{s,t}^p \end{aligned} \tag{3.5}$$

where $w_{s,t}^{p}$ is the percentile *p* of the real hourly wage distribution of province *s* at time *t* and $w_{s,t}^{q}$ denotes a level of local wages that may be unaffected by the NMW. The percentile gaps, the outcome variables, are the difference between each percentile $w_{s,t}^{p}$ and the comparison percentile $w_{s,t}^{q}$. Lee (1999) and Author et al. (2016), for the case of the US, use $w_{s,t}^{q=50}$, the median, as the wage level that is unaffected by the minimum wage law. As shown in Figure A1 (in the Appendix), the value of NMW in Ecuador locates at or below the 60th percentile of the wage distribution, for this reason I use as comparison the 70th percentile, $w_{s,t}^{q=70}$, and assume that wages at or above this level will be unaffected by the NMW. Other studies for developing countries also use the 70th percentile of wage distribution (see Bosch and Manacorda 2010, for Mexico; Borraz and Pampillon 2011, for Uruguay).

In Eq. 3.1, year fixed effects are represented by δ_t^p; province fixed effects are denoted by γ_{s0}^p; and provincial linear trends are captured by $\gamma_{s1}^p * T$. $\boldsymbol{X'}_{s,t}$ is a vector of the provincial control variables, which include the share of workers in each age group (15-20, 21-30, ... ,61-70), the share of workers classified by educational level (primary, secondary and tertiary), the share of females, the fraction of workers living in urban areas, and the proportion of workers by one-digit industry. Finally, $\epsilon_{s,t}^p$ is an error term, which I assume to be uncorrelated with: the province and year effects, provincial trends, and provincial control variables.

The differential $\left(log(NMW_t) - log(w_{s,t}^{q=70})\right)$ is the effective NMW, $\widetilde{mw}_{s,t}$, for each

province *s* at time *t*. In Ecuador, the value of the NMW is set yearly nationwide but wage levels vary greatly across provinces. My identification strategy therefore exploits this variation of the effective NMW across the 20 provinces, and over 16 years from 2000 – 2016, to identify the distributional effects of the NMW. The coefficients on the effective NMW, β_1^p, and on its quadratic term, β_2^p, capture the effect of the changes in the NMW. Specifically, the impact of the changes in the effective NMW on the p to q=70 percentile gap is given by $\beta_1^p + 2\beta_2^p\left(log\ (NMW_t) - log(w_{s,t}^{q=70})\right)$.

For example, if p=10 the coefficients β_1^{10} and β_2^{10}measure the effect of the effective NMW on the wage gap between the 10th percentile and the 70th percentile of the wage distribution. In the results section, I estimate Eq. 1 for percentiles p=10, 20, 30, 40, 50, 60, 80, 90. For the cases in which p=80 and 90, the coefficients on the effective NMW must not be significantly different from zero, as I am assuming that the NMW has no effect on wages at the 70th percentile or above. This assumption is testable, and its rejection would undermine the reliability of my estimates of the effect of the NMW.

One limitation of the implementation of Lee's (1999) and Author's et al. (2016) strategy in the context of developing countries is that the effective minimum wage does not take into account the changes in the intensity of enforcement of the law and the heterogeneity in the levels of noncompliance across provinces. In developed countries, this may be not a problem, but, in developing countries, where there is widespread noncompliance (Marshall 2007; Rani et al. 2013), it is a major issue. There can be a high legislated level of the NMW and a low level of local earnings, which generates a high effective NMW, but this is meaningless if there is no institution that enforces compliance with the law. In this paper, therefore, I also contribute to the literature by modifying Lee's (1999) measure of the effective minimum wage to take account of noncompliance.

The original Kaitz index (Kaitz 1970) adjusts the value of the minimum wage by the fraction of workers most likely to be affected the law. For the US, therefore, it adjusts the minimum wage by the fraction of teenagers in each state. Analogously, I adjust the relative value of the NMW by multiplying it for one minus the rate of noncompliance in each province *s* at time *t*, in this way:

$$\widetilde{mw}\boldsymbol{adjusted}_{s,t} = log\left(\frac{NMW_t}{w_{s,t}^{70}} * \left(1 - NC_{s,t}\right)\right)$$

where NMW_t denotes the real hourly NMW, in year *t*, and $w_{s,t}^{70}$ is the 70th percentile of the real hourly wage distribution, in province *s* in year *t*. $NC_{s,t}$ is the rate of noncompliance with the NMW in province *s* in year *t*; it is the percentage of full-time employees who are paid less than 90% of the value of the monthly NMW. The effective NMW adjusted for noncompliance captures changes in both components of the minimum wage policy: 1) changes in the real value NMW across time, and 2) changes in noncompliance with the law across time for each province.

In Eq. 3.2, I replace the effective NMW, $\widetilde{mw}_{s,t}$, with the effective NMW adjusted for noncompliance, $\widetilde{mw}\boldsymbol{adjusted}_{s,t}$, in this way:

$$log\left(w_{s,t}^{p}\right) - log\left(w_{s,t}^{q}\right) = \beta_1^p \left(log\left(\frac{NMW_t}{w_{s,t}^{70}} * \left(1 - NC_{s,t}\right) \right) \right) + \beta_2^p \left(log\left(\frac{NMW_t}{w_{s,t}^{70}} * \left(1 - NC_{s,t}\right) \right) \right)^2 + \delta_t^p + \gamma_{s0}^p + \gamma_{s1}^p * T \; + \boldsymbol{X'}_{s,t}\delta^p + \epsilon_{s,t}^p \tag{3.6}$$

The adjustment helps to identify the effects of the NMW policy in three ways. First, $NC_{s,t}$ introduces an additional source of variation across provinces and over time, Second, it helps to deal with the problem of division bias, which results from a using a variable potentially measured with error in both sides of the regression equation (Borjas 1980). In other words, Eq. 1 uses the 70th percentile of the wage distribution directly on both sides of the equation and, because of measurement error, the effective NMW may be spuriously correlated with the outcome variable. The adjustment by noncompliance mitigates this problem. Third, it generates a more comprehensive measure of the changes in NMW policy, because $\widetilde{mw}\boldsymbol{adjusted}_{s,t}$ captures both the increases in the NMW and the reduction in noncompliance (see Figure 3.2).

The coefficients on the effective NMW and on the rate of compliance are restricted to be the same in Eq. 3.2. In Eq. 3.3, I write the unrestricted version of Eq. 3.2, where the coefficients on both components of the NMW policy are allowed to be different. In the results sections, I present the regression estimates of the restricted model, Eq. 3.2, and the unrestricted model, Eq. 3.3, for each percentile gap (difference between percentiles *p*=10, 20, 30, 40, 50, 60, 80, 90 and the 70th percentile). With Eq. 3.3, I also test in the results section if the restriction of equality of marginal effects of both components of the NMW policy is valid.

$$log\left(w_{s,t}^{p}\right) - log\left(w_{s,t}^{q}\right) = \beta_1^p\left(\log\left(NMW_t\right) - \log\left(w_{s,t}^{q}\right)\right) + \alpha_1^p \log\left(1 - NC_{s,t}\right)$$
$$+ \beta_2^p\left(\log\left(NMW_t\right) - \log\left(w_{s,t}^{q}\right)\right)^2 + \alpha_2^p log\left(1 - NC_{s,t}\right)^2$$
$$+ \delta_t^p + \gamma_{s0}^p + \gamma_{s1}^p * T + \boldsymbol{X'}_{s,t}\delta^p + \epsilon_{s,t}^p \qquad (3.7)$$

3.6 Results

I begin by estimating the distributional impact of the effective NMW without accounting for compliance with the law. Table 3.1 presents the OLS estimates of Eq. 3.1 using the effective NMW, $\widetilde{mw}_{s,t}$. Each entry refers to a separate regression, where each row refers to the effect of the effective NMW on the wage gap between selected percentiles (10, 20, 30, … ,90) and the 70th percentile of the wage distribution. I report the first derivative for each dependent variable with respect to the effective NMW evaluated at its hours-weighted average over all provinces and all years between 2000 and 2016, in this way $\beta_1^p + \beta_2^p(\widetilde{mw})$. OLS regressions are weighted by the total of the ENEMDUR sampling weights multiplied by the weekly hours worked in each province and year, and standard errors in parentheses are clustered by province.

In Column 1, I present the OLS estimate including year and province fixed effects. Column 2 includes provincial trends, and Column 3 shows the results including provincial controls. The estimates of the marginal effect of the effective NMW, without accounting for compliance, are all positive and statistically significantly different from zero, including the estimates for percentiles above the 70th. The significant relationship between the effective NMW and the upper tail of the wage distribution violates my identification assumption and, as suggested by Autor et al. (2016), is a signal of bias in the estimates. A possible candidate is division bias (Borjas 1980), since the 70th percentile, potentially measured with error, is included directly on the left- and right-hand side of Eq. 3.1.

Autor et al. (2016) show that the OLS estimates of Eq. 3.1 lead to an upward bias in the estimates of the impact of the minimum wage in both the lower and the upper tail. The authors address this problem using a two-stage least squares (2SLS) strategy, with the effective minimum wage instrumented by the statutory minimum wage in each state. Since there are no provincially set minimum wages in Ecuador it is not possible to use this strategy in this case. In the country, all provinces are subject to the same change in the NMW from one year to the

next, so I cannot identify this aggregate effect from the year fixed effects that are also included in Eq. 3.1.

Table 3.1 OLS Relationship Between the Log(p) – Log(p70) and the Effective NMW, Log(NMW) – log(p70), for Selected Percentiles of the Wage Distribution of Covered Workers, 2000-2016

	Effective NMW		
	(1)	(2)	(3)
p10-p70	0.366***	0.328**	0.425***
	(0.103)	(0.133)	(0.158)
p20-p70	0.386***	0.392***	0.476***
	(0.073)	(0.088)	(0.097)
p30-p70	0.356***	0.408***	0.402***
	(0.065)	(0.076)	(0.066)
p40-p70	0.309***	0.424***	0.450***
	(0.070)	(0.090)	(0.078)
p50-p70	0.261***	0.397***	0.475***
	(0.067)	(0.073)	(0.081)
p60-p70	0.185***	0.254***	0.264***
	(0.045)	(0.042)	(0.052)
p80-p70	0.147***	0.165***	0.258***
	(0.052)	(0.051)	(0.073)
p90-p70	0.159*	0.284***	0.480***
	(0.083)	(0.062)	(0.091)
Province FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Provincial trends		Yes	Yes
Provincial controls			Yes

Source: Author's calculations from the ENEMDUR 2000-2016.
Notes: $N = 320$, with number of provinces = 20 and number of years =16. Sample period is 2000-2016. Each entry in the table refers to the marginal effect of the effective NMW, evaluated at its hours-weighted average across provinces and years. Standard errors are clustered at the provincial level and shown in parentheses. Provincial controls include the share of workers in each age group (15-20, 21-30, ... ,61-70), the share of workers classified by educational level (primary, secondary and tertiary), the share of females, the fraction of workers living in urban areas, and the proportion of workers by one-digit industry.
*10%, **5%, and ***1% significance level.

In Table 3.2, I estimate the marginal effect of the NMW by including in the right-hand side of the equation both components of the NMW policy. Columns 1-3 present the estimates of the restricted model (see Eq. 3.2) using the effective NMW adjusted for compliance, $\widetilde{mwadjusted}_{s,t}$. Columns 4-12 show the results of the unrestricted model (see Eq. 3.3). Specifically, columns 4, 7 and 10 show the marginal effects of the (unadjusted) effective NMW and columns 5, 8 and 11 shows the marginal effects of the rate of compliance. Columns 6, 9 and 12 report the *p*-value of the test of equality of marginal effects of the (unadjusted) effective NMW and the rate of compliance, $\beta_1^p + \beta_2^p(\widetilde{mw}) = \alpha_1^p + \alpha_2^p\left(1 - \widetilde{NC}\right)$.

Using $\widetilde{mwadjusted}_{s,t}$ instead of $\widetilde{mw}_{s,t}$ seems to address the problem of division bias: Columns 1-3 of Table 3.2 show no significant effect of the NMW policy to percentiles above the 70th. It is also interesting to highlight that while the estimates for the upper tail of the distribution decrease in magnitude, and became not significant, the estimates for the lower part of the distributions increase in magnitude. Additionally, for the middle of the distribution, percentile 40th and 50th, the magnitude of the estimates with and without the adjustment is roughly the same (see Column 2 of Table 3.1 vs Column 2 of Table 3.2).

Comparing the results of the restricted and unrestricted model in Table 3.2, we can see that the restriction of equality of marginal effects for both components of the NMW policy is valid for regressions that have the p10-p70, p20-p70, and p30-p70 gaps as dependent variables. For these regressions, we do not reject the null hypothesis of equality of marginal effects of the effective NMW and the rate of compliance (see *p*-value in Column 9).

For percentile 40th and 50th, the estimates of the effect of both components of the NMW policy are positive and significant differently from zero, but they differ in magnitude. For these percentiles, the effect of the changes in compliance are far lower than the effect of changes in the value of the NMW. One would expect these results since the increase in the intensity of enforcement of the NMW law will have a larger effect on wages of workers earning far below the NMW than on workers with wages close to the NMW. For percentiles 60th and above the effect of changes in compliance are not significantly different from zero, while effects of changes in the value of the NMW are positive and significant differently from zero. This suggests that, even accounting for compliance, in the unrestricted model, the fact that the 70th percentile, potentially measured with error, is included directly on the left- and right-hand of the regression continues to generate bias in the estimates.

My preferred estimates are therefore, the ones from the restricted model (se Eq.3.2). The results in Columns 1-3 of Table 3.2 suggest that the changes in NMW policy had a

significant impact on reducing wage inequality in Ecuador. The effects are significantly different from zero at the 1% level for percentiles up to the 60th, and the magnitude of the impact decreases as one moves upwards in the wage distribution. The estimates are robust to model specification; the inclusion of provincial trends and provincial controls do not change the magnitude of the estimates significantly, and they remain statistically significant at the 1% level. Column 2 shows the results of my preferred specification of Eq. 3.2, using province and year fixed effects, and adding provincial trends. Based on these estimates, a 10 log point increase in the effective NMW policy reduces the 70-10 percentile gap by approximately 8.2 log points, the 70-30 percentile gap by roughly 4.5 log points, and the 70-50 percentile gap by 3.8 log points.

Table 3.3 shows additional robustness checks, but the conclusions do not change. In columns 1 and 2, I report the results of regressions that use the unweighted data (as opposed to that weighted by total of the ENEMDUR sampling weights multiplied by the weekly hours worked in each province and year). Columns 3 and 4 show the estimates of the effects of the regression using the percentiles of the monthly wage distribution of full-time workers only (as opposed to the hourly wage distribution of both full-time and part-time workers) and weighting by workers rather than by worker hours. In both cases, I show the estimates for the specification with province and year fixed effects (see Columns 1 and 3) and, of my preferred specification, adding provincial trends (see Columns 2 and 4). None of the alternative specifications makes any substantial difference to the estimates of the impact of the effective NMW adjusted for noncompliance.

Columns 5 and 6 of Table 3.3 show the results for workers in the uncovered sector. In this case, I still use the same measures of the effective NMW adjusted for noncompliance as in Table 3. 2 – computed based on the 70th percentile of the wage distribution of covered workers and, for the adjustment, the rate of noncompliance among covered workers. The results for uncovered workers show that when I do not add provincial trends the estimates for the p20-p70, p30-p70, p40-p70 and p50-p70 percentile gaps are positive and significant at the 5% level (see Column 5). When provincial trends are added, however, the estimates decrease in magnitude and none of them remain statistically significant at conventional levels (see Columns 6).

Table 3.2 OLS Relationship Between the Log(p) – Log(p70), and the Effective NMW accounting for Noncompliance, Restricted and Unrestricted Models, for Selected Percentiles of the Wage Distribution of Covered Workers, 2000-2016

	Restricted			Unrestricted								
	Effective NMW adjusted for Compliance			Effective NMW	Compli-ance	*p*-value	Effective NMW	Compli-ance	*p*-value	Effective NMW	Compli-ance	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
p10-p70	0.872***	0.827***	0.800***	0.804***	0.954***	0.217	0.742***	0.889***	0.158	0.732***	0.836***	0.973
	(0.073)	(0.095)	(0.094)	(0.083)	(0.072)		(0.111)	(0.098)		(0.101)	(0.094)	
p20-p70	0.716***	0.684***	0.681***	0.776***	0.743***	0.236	0.727***	0.671***	0.314	0.742***	0.698***	0.563
	(0.039)	(0.067)	(0.081)	(0.045)	(0.053)		(0.063)	(0.083)		(0.058)	(0.100)	
p30-p70	0.496***	0.453***	0.437***	0.636***	0.474***	0.923	0.611***	0.362***	0.436	0.571***	0.392***	0.070
	(0.050)	(0.067)	(0.069)	(0.048)	(0.070)		(0.062)	(0.083)		(0.067)	(0.098)	
p40-p70	0.407***	0.428***	0.428***	0.542***	0.325***	0.305	0.584***	0.264***	0.081	0.582***	0.268***	0.002
	(0.040)	(0.072)	(0.070)	(0.055)	(0.058)		(0.076)	(0.050)		(0.061)	(0.071)	
p50-p70	0.316***	0.383***	0.362***	0.444***	0.230***	0.003	0.526***	0.243***	0.003	0.561***	0.196***	0.001
	(0.049)	(0.077)	(0.085)	(0.056)	(0.050)		(0.071)	(0.059)		(0.079)	(0.058)	
p60-p70	0.134***	0.170***	0.166***	0.266***	0.058	0.012	0.302***	0.078	0.013	0.301***	0.063	0.033
	(0.042)	(0.049)	(0.056)	(0.038)	(0.042)		(0.040)	(0.051)		(0.056)	(0.056)	
p80-p70	0.062	0.015	0.068	0.153**	-0.001	0.008	0.138*	-0.063	0.004	0.245***	-0.035	0.004
	(0.081)	(0.089)	(0.085)	(0.066)	(0.057)		(0.072)	(0.061)		(0.085)	(0.050)	
p90-p70	0.024	0.054	0.135	0.189**	-0.045	0.000	0.257***	-0.087	0.001	0.457***	-0.105	0.000
	(0.061)	(0.094)	(0.107)	(0.083)	(0.061)		(0.081)	(0.095)		(0.108)	(0.114)	
Prov. FE	Yes	Yes	Yes		Yes			Yes			Yes	
Year FE	Yes	Yes	Yes		Yes			Yes			Yes	
Provincial trends		Yes	Yes					Yes			Yes	
Provincial controls			Yes								Yes	

Source: Author's calculations from the ENEMDUR 2000-2016.

Notes: *N* = 320, with number of provinces = 20 and number of years =16. Sample period is 2000-2016. Each entry in the table refers to the marginal effect of the explanatory variable, evaluated at its hours-weighted average across provinces and years. Standard errors are clustered at the provincial level and shown in parentheses. Provincial controls include the share of workers in each age group (15-20, 21-30, ... ,61-70), the share of workers classified by educational level (primary, secondary and tertiary), the share of females, the fraction of workers living in urban areas, and the proportion of workers by one-digit industry. Columns 6, 9 and 12 report the p-value of the test of equality of coefficients on the Effective NMW and on the rate of compliance (see unrestricted model, Eq. 3.3.) *10%, **5%, and ***1% significance level.

Table 3.3 Robustness Checks for the OLS Relationship Between the Log(p) – Log(p70) and the Effective NMW adjusted for Noncompliance, Restricted Model, for Selected Percentiles, 2000-2016.

	Effective NMW adjusted for noncompliance (Restricted Model)					
	Covered Workers				Uncovered Workers	
	Unweighted		Full-time			
	(1)	(2)	(3)	(4)	(5)	(6)
p10-p70	0.809***	0.793***	0.993***	1.011***	0.207	0.036
	(0.087)	(0.094)	(0.082)	(0.096)	(0.135)	(0.136)
p20-p70	0.713***	0.688***	0.839***	0.830***	0.227**	0.062
	(0.058)	(0.064)	(0.053)	(0.046)	(0.112)	(0.099)
p30-p70	0.521***	0.473***	0.667***	0.717***	0.246***	0.098
	(0.048)	(0.047)	(0.086)	(0.080)	(0.087)	(0.092)
p40-p70	0.419***	0.382***	0.470***	0.509***	0.243***	0.144
	(0.038)	(0.036)	(0.077)	(0.068)	(0.088)	(0.094)
p50-p70	0.300***	0.280***	0.390***	0.433***	0.172**	0.103
	(0.036)	(0.044)	(0.055)	(0.047)	(0.073)	(0.087)
p60-p70	0.132***	0.113**	0.365***	0.426***	0.102*	0.036
	(0.042)	(0.049)	(0.061)	(0.066)	(0.055)	(0.065)
p80-p70	0.047	0.032	0.1	0.138*	-0.028	0.005
	(0.050)	(0.061)	(0.065)	(0.081)	(0.049)	(0.056)
p90-p70	0.174**	0.200*	0.024	0.123*	-0.137	-0.151
	(0.081)	(0.104)	(0.067)	(0.072)	(0.108)	(0.113)
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Provincial trends		Yes		Yes		Yes

Source: Author's calculations from the ENEMDUR 2000-2016.

Notes: $N = 320$, with number of provinces = 20 and number of years =16. Sample period is 2000-2016. Each entry in the table refers to the marginal effect of the effective NMW, evaluated at its hours-weighted average across provinces and years. Standard errors are clustered at the provincial level and shown in parentheses. Provincial controls include the share of workers in each age group (15-20, 21-30, ... ,61-70), the share of workers classified by educational level (primary, secondary and tertiary), the share of females, the fraction of workers living in urban areas, and the proportion of workers by one-digit industry.

*10%, **5%, and ***1% significance level.

Uncovered workers, such as self-employed workers and business owners, cannot be forced to pay themselves the NMW. Hence, one might expect to find no effect among these workers. The results in Table 3.3 for this group of workers, once I add provincial trends, are in line with this proposition. This exercise also helps to test my identification strategy of using this group of workers as a placebo group. Assuming that covered and uncovered workers are affected in the same way by other factors that might influence the changes in the distribution of labour earnings, the fact that I do not find a significant effect for uncovered workers suggests that the results for covered workers, under the preferred specification, are not capturing changes in wage dispersion generated by reasons other than the changes in the NMW policy.

3.7 Counterfactual Estimates of the Changes in Wage Inequality

Until now I have focused on measuring the relationship between the changes in the NMW policy and the changes in wage dispersion. I estimate that the increase in the level of NMW and the reduction in noncompliance have reduced lower tail inequality, but how much of the decline observed since 2000 can be attributed to the changes in the NMW policy? Based on Lee (1999) and Autor et al. (2016), I present reduced form counterfactual estimates of the change in *latent* wage inequality in a scenario without the changes in the NMW policy —that is, the change in wage inequality that would have been observed had the value of NMW been held constant (in real terms) and had there not been any improvement in noncompliance with the law.

I consider three time periods for the counterfactual analysis: 2000-2016, 2000-2010, and 2000-2005. The period 2000-2016 covers both changes in the NMW policy: the yearly increase in the real value of the NMW and the significant reduction in noncompliance that took place in 2011. The time frame 2000-2010 excludes the significant drop in noncompliance, but includes the largest increase in the real value of the NMW (in absolute terms) that took place between 2007 and 2008. This time period also allows my results to be compared with the findings of Ponce and Vos (2014). Finally, the period 2000-2005 excludes the two major changes in the NMW policy.

Following Autor et al.'s (2016) procedure to estimates changes in latent wage gaps, I first generate the rank for each worker in its respective province-year wage distribution, using the individual-level data. Then, I adjust each worker’s wage by the quantity:

$$\Delta \log\left(w_{s,t1}^{p}\right) = \widehat{\beta_1^p}\left(mw\widetilde{\boldsymbol{adjusted}}_{s,t0} - mw\widetilde{\boldsymbol{adjusted}}_{s,t1}\right) + \widehat{\beta_2^p}\left(mw\widetilde{\boldsymbol{adjusted}}^{2}_{s,t0} - mw\widetilde{\boldsymbol{adjusted}}^{2}_{s,t1}\right) \quad (3.8)$$

where $mw\widetilde{\boldsymbol{adjusted}}_{s,t1}$ is the observed end-of-period effective NMW adjusted for noncompliance in province *s* in some year $t1$, and $mw\widetilde{\boldsymbol{adjusted}}_{s,t0}$ is the corresponding beginning-of-period effective NMW adjusted for noncompliance in $t0$. $\widehat{\beta_1^p}$ and $\widehat{\beta_2^p}$ are estimated coefficients from the OLS estimates of Eq. 3.2, shown in Table 3.2 for selected percentiles.

Finally, I pool the adjusted wage observations to form a counterfactual national wage distribution and compare the changes in wage inequality in the simulated distribution to those in the observed distribution. As in Autor et al. (2016), I compute the standard errors by bootstrapping the estimates within the province-year panel.

The bootstrap procedure takes provinces as the sampling unit, and thus I start by drawing 20 provinces with replacement. I next estimate the restricted model, Eq. 3.2, for the selected provinces using the percentile estimates. I run the OLS regressions of Eq. 3.2 with regression weights given by the total of the ENEMDUR sampling weights multiplied by salaried workers' weekly hours worked for the selected provinces. I next apply the coefficients $\widehat{\beta_1^p}$ and $\widehat{\beta_2^p}$ of Eq. 3.2 to get adjustments, $\Delta w_{s,t1}^{p}$, in Eq. 3.4 and calculate the counterfactual wage distribution by applying them to the individual-level ENEMDURs. Table 3.4 reports the mean and the standard deviation of 200 replications of this counterfactual exercise.

Panel A of Table 3.4 shows the results of the counterfactual analysis for the period 2000 – 2016. In this case, $t1$=2016 and $t0$=2000. Adding $\Delta log(w_{s,t1}^{p})$ to the corresponding (log) wage of each worker in 2016 would adjust the 2016 distribution to its counterfactual under the realised effective NMW adjusted for noncompliance in 2000. Similarly, Panel B reports the results for the period 2000-2010. In this case, the counterfactual estimates are computed using the coefficients on the effective NMW from analogous regressions for the shorter sample period. Adding $\Delta log(w_{s,t1}^{p})$, when $t1$=2010 and $t0$=2000, to the corresponding (log) wage of each worker in 2009 would adjust the 2010 distribution to its counterfactual had the NMW policy in 2010 equalled the NMW policy in 2000. In the same way, Panel C reports the counterfactual results for the 2000 – 2005 period.

Table 3.4 Actual and Counterfactual Changes in Wage Gap Ratios, for Selected Percentiles, Between Selected Years: Changes in Log Points (100 * Log Change)

	Observed change	Latent Change		
		Effective NMW adjusted for Noncompliance		
		FE	FE, Trend	FE, Trend, Controls
	(1)	(2)	(3)	(4)
Panel A. 2000-2016				
p70/p10	-66.6	-34.8***	-35.5***	-36.9***
		(2.3)	(3.3)	(2.7)
p70/p30	-39.8	-22.1***	-22.5***	-22.9***
		(1.4)	(2)	(2)
p70/p50	-18.6	-9.3***	-7.3***	-8***
		(1.7)	(2.3)	(2.3)
p90/p70	-0.7	0.4	0.6	-1.6
		(2.8)	(2.6)	(3.4)
Panel B. 2000-2010				
p70/p10	-60.4	-34.3***	-35.1***	-35.8***
		(3.6)	(4.1)	(3.9)
p70/p30	-35.5	-17.1***	-18***	-18.5***
		(2.6)	(3.9)	(2.6)
p70/p50	-21.9	-5.9**	-7.7***	-7.7***
		(2.5)	(2.4)	(2.5)
p90/p70	15.4	17.7***	13.8***	13***
		(2.3)	(3.4)	(4)
Panel C. 2000-2005				
p70/p10	-28.5	-17.6***	-16.8**	-18.4***
		(6.1)	(6.6)	(6.6)
p70/p30	-12.3	-3.3	-3.3	-5.2
		(3.7)	(4)	(3.9)
p70/p50	-5.9	-2.6	-2.9	-3.6
		(2.7)	(2.7)	(3)
p90/p70	5.9	8.9***	8.6***	7.3*
		(3)	(3.2)	(3.8)

Source: Author's calculations from the ENEMDUR 2000-2016.

Note: Estimates represent changes in actual and counterfactual wage gaps (for selected percentiles), between 2000 and 2016, between 2000 and 2010, and between 2000 and 2005, measured in log points (100 * log change). Counterfactual wage changes in Panel A represent counterfactual changes in the selected percentiles gaps had the effective minimum wage adjusted for noncompliance in 2016 equalled the effective minimum wage adjusted for noncompliance in 2000 for each province. Counterfactual wage changes in Panel B represent changes had the effective minimum wage adjusted for noncompliance in 2010 equalled the effective minimum wage adjusted for noncompliance in 2000 for each province. The counterfactuals (using point estimates from the 2000 - 2016 period) are formed using coefficients from the OLS estimations of Eq. 3.2, reported in columns 1 to 3 of Tables 3.2, shown for selected percentiles. Counterfactuals using point estimates from the 2000 - 2010 period are formed using coefficients from analogous regressions for the shorter sample period. In the same way, Panel C reports the counterfactual results for the 2000 - 2005 period. Marginal effects are bootstrapped as described in the text; the standard deviation associated with the estimates are reported in parentheses.

*10%, **5%, and ***1% significance level.

Table 3.4 also shows that while the magnitude of the observed drop in the 70/10 ratio is around the same during the first half and second half of the 2000s, most of the fall in the 30/70 and 50/70 ratios took place during the second half of the 2000s (see Column 1, Panels B and C). These findings suggest that the argument of Ponce and Vos (2014) may apply for the drop in lower-tail inequality, but not for the reduction in inequality at the low-middle and middle of the distribution. As illustrated in Figure 3.3, the 70/30 and 70/50 ratios remain relatively constant between 2000 and 2005, and show a falling trend from 2006 onwards. Additionally, the counterfactual estimates in Panel B of Table 3.4 suggest that the changes in the NMW explain between 49 and 65 percent of the reduction in wage inequality in the lower-middle and middle of the distribution between 2000 and 2010 (see Column 3).

Finally, it is worth noting that the counterfactual estimates of the change in the 70/10 ratio for the 2000 – 2016 period and for the 2000 – 2010 period are the same, at around 0.35 log points (see Column 3 Panels A and B of Table 3.3). This implies that the compression in the lower-tail of the wage distribution that took place from 2011 onwards is entirely driven by the changes in the NMW policy. Especially by the increase in the intensity of enforcement of the NMW law.

3.8 Conclusions

Although noncompliance with minimum wage policies is known to be high in developing countries, policymakers usually focus their attention solely on changes to the value of the minimum wage. For example, in Latin America, compliance with the minimum wage ranges from a best-case level of 70% to 80%, as in Costa Rica, Chile, Uruguay and Panama, to a worst-case level of 25% to 50%, as in Peru, Ecuador, Nicaragua, Bolivia and Paraguay (Marshall 2007). In this context, increasing the level of the minimum wage in order to compress the wage distribution might not be effective without a parallel increase in the intensity of enforcement of the labour law. This study suggests that increases in both components of the NMW policy served to compress the wage distribution in Ecuador between 2000 - 2016, and that they account for around half of the observed reduction in wage inequality.

The compression effect of the Ecuadorian minimum wage policy can be divided into two time periods. In the period 2005-2010 there was a significant increase in the real value of the NMW, especially in 2008 when the new government increased it by 17.12 USD (in real terms), from 200.68 USD per month in 2007 to 217.80 USD in 2008. The steady increase in the real value of the NMW compressed the wage distribution, by increasing wages at the lower

half of the wage distribution. Before this period, between 2000 and 2005, there was a significant reduction in wage inequality, but most of this was due to the recovery in wages at the lowest decile of the distribution after the deep financial crisis of the late 1990s. Wages at the low-middle and middle of the distribution (relative to the 70th percentile) remained relatively constant during this period (see Figure 3.3).

Since 2010, there was also an increase in enforcement of the NMW law, generated through an awareness campaign programme. This increase in enforcement resulted in a significant reduction in noncompliance with the NMW law in 2011. The increased enforcement of the NMW law, along with the increase in the real value of the NMW through the period 2010-2016, compressed the wage distribution by pushing wages at the lower half of the distribution towards the value of the NMW, which is located at around the 50th to 60th percentiles (see Figure 3.4 in the Appendix).

The wages of workers in the lower half of the distribution (relative to the 70th percentile) increased by more during the Campaign period, years 2010-2016, than in the pre-campaign period, years 2005-2009 (see Figure 3.3). Among these workers, those at the lowest decile experienced the largest increase during the Campaign period. Additionally, during the Campaign period, the NMW policy accounted for all the observed reduction in the 70/10 percentile ratio. This implies that, if there were no change in the NMW policy after 2010, wage inequality at the lower part of the distribution (as measured by the 70/10 percentile ratio) would not have reduced further as we observed.

The further reduction in wage inequality from 2010 onwards occurred in a context of increased enforcement of labour regulations. This highlights the role that the other component of the minimum wage policy can have on reducing further wage inequality, especially in the context of high noncompliance. Policymakers who seek to use a minimum wage as a policy tool to compress the wage distribution should therefore not forget the intensity of enforcement of the law as an additional policy instrument. That said, policymakers also have to consider that, as I found in a previous study (see Guzman 2017), better enforcement and higher compliance with the NMW might also lead to a migration of low-paid workers from the covered sector into the uncovered self-employment sector or to unemployment.

3.9 Appendix

Table 3.5 Number of Labour Inspectors in each provincial branch of the Ministry of Labour, by year

	2006	2007	2008	2009	2010	2011	2012	2013	2014
Azuay	5	8	8	12	16	26	27	30	20
Bolivar	0	0	0	0	0	0	2	1	2
Cañar	4	4	4	5	5	5	5	6	6
Carchi	0	0	0	0	0	3	2	1	2
Cotopaxi	0	0	0	0	0	3	4	3	4
Chimborazo	0	0	0	0	0	4	6	6	5
El Oro	0	0	0	0	0	3	14	14	12
Esmeraldas	0	0	0	0	0	5	6	3	4
Guayas	0	0	0	0	12	59	51	46	35
Imbabura	0	0	0	0	0	3	7	8	5
Loja	0	0	0	0	0	5	12	9	8
Los Rios	0	0	0	0	0	3	4	7	3
Manabí	0	0	0	0	0	14	25	20	18
Morona Santiago	1	1	1	1	1	2	3	3	2
Napo	0	0	0	0	0	3	3	3	3
Pastaza	0	0	0	0	0	3	2	2	2
Pichincha	0	0	14	14	7	57	70	70	54
Tungurahua	0	0	4	4	3	13	10	9	9
Zamora Chinchipe	0	0	0	0	0	2	2	2	2
Galapagos	0	0	0	0	0	1	2	1	2
Sucumbíos	0	0	0	0	0	7	2	3	2
Orellana	0	0	0	0	0	6	6	4	5
Santo Domingo	0	0	0	0	0	10	3	6	7
Santa Elena	0	0	0	0	0	3	1	3	1
Regional Secretariat	0	0	15	17	0	0	0	0	0
Regional Secretariat -	0	0	2	2	0	0	0	0	0
Not specified	0	0	0	0	2	0	0	0	0
Total	10	13	48	55	46	240	269	260	213

Source: Administrative records of the Ecuadorian Ministry of Labour, 2015.

Table 3.6 Changes in the Value of the National Minimum Wage

Year	Nominal NMW (USD)	Real NMW (USD of 2010)	Increase in real terms (USD)	Increase in real terms (%)
2000	57.00	124.72		
2001	85.70	136.21	11.48	9.21
2002	104.90	148.22	12.01	8.82
2003	121.90	159.58	11.37	7.67
2004	135.60	172.78	13.20	8.27
2005	150.00	187.07	14.29	8.27
2006	160.00	193.17	6.10	3.26
2007	170.00	200.68	7.51	3.89
2008	200.00	217.80	17.12	8.53
2009	218.00	225.75	7.95	3.65
2010	240.00	240.00	14.25	6.31
2011	264.00	252.69	12.69	5.29
2012	292.00	265.93	13.23	5.24
2013	318.00	281.93	16.00	6.02
2014	340.00	290.99	9.06	3.21
2015	354.00	291.41	0.42	0.15
2016	366.00	296.17	4.76	1.63

Source: Central Bank of Ecuador (Monthly Statistical Reports) and National Institute of Statistics and Census (CPI Historical Series)

Table 3.7 Sample Size by Province and Year of the Data Used to Generate the Percentiles of Provincial Wage Distributions of Covered Workers

	2000	2001	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
Azuay	1062	1036	895	890	703	831	1017	1044	971	1018	928	917	1047	1294	1318	1287
Bolivar	197	148	608	598	649	687	606	619	589	558	468	528	615	568	641	606
Cañar	190	266	697	697	561	652	733	763	683	677	490	548	683	697	697	661
Carchi	121	143	797	793	774	867	769	744	764	822	585	661	792	918	892	905
Cotopaxi	268	226	697	705	722	785	750	822	625	670	517	569	776	1421	1417	1313
Chimborazo	370	275	571	598	588	596	602	650	535	561	410	492	547	645	653	615
El Oro	750	731	1361	1445	1324	1289	1491	1533	1457	1598	1235	1442	1445	1558	1520	1543
Esmeraldas	224	262	1000	975	1031	1087	1095	1068	935	1051	719	750	1006	1207	1147	1081
Guayas	2645	2146	2167	2276	2234	2303	2092	2203	2734	2790	2498	2524	2705	2964	2791	2757
Imbabura	333	327	763	746	766	754	741	732	746	801	682	625	779	1669	1714	1670
Loja	280	403	578	646	616	696	706	670	628	652	543	649	655	690	650	644
Los Rios	727	570	1310	1351	1200	1353	1337	1325	1143	1241	1079	1104	1226	1160	1247	1184
Manabí	832	734	1077	1108	1149	1115	1111	1119	1024	1092	977	1065	1199	1223	1136	1143
Morona Santiago	129	142	117	111	79	107	111	142	93	117	81	102	146	585	583	561
Napo	154	178	215	193	239	228	197	211	163	214	148	164	215	1504	1258	1262
Pastaza	206	175	112	99	117	118	111	138	103	108	77	113	105	649	620	561
Pichincha	1662	1719	1742	1763	1827	1860	1632	1735	2170	2342	2017	1961	2380	2943	2715	2658
Tungurahua	454	356	763	766	824	763	890	1034	952	996	833	885	948	1425	1420	1332
Zamora Chinchipe	138	130	99	74	56	92	98	93	84	84	73	97	97	647	636	605
Sucumbíos	196	207	181	174	161	160	190	198	179	186	125	144	183	728	734	744

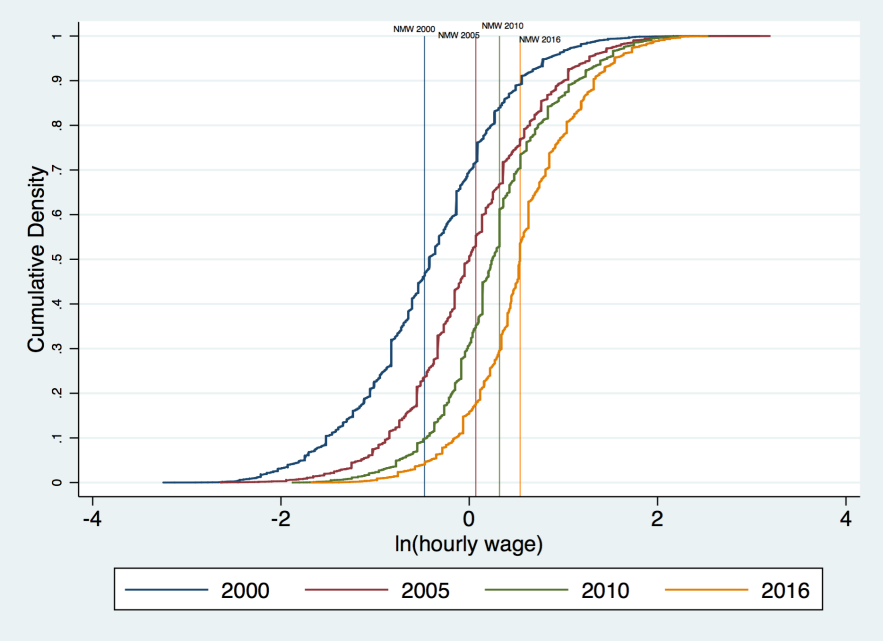
Source: Author's calculations from ENEMDUR 2000-2016.

Table 3.8 Sample Size by Province and Year of the Data Used to Generate the Percentiles of Provincial Earnings Distributions of Uncovered Self-employed Workers

	2000	2001	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
Azuay	692	736	615	603	562	598	533	632	623	634	625	540	631	882	816	879
Bolivar	234	225	544	604	562	561	461	501	493	496	375	427	527	495	411	545
Cañar	283	270	575	583	504	528	473	481	394	456	427	490	579	504	467	490
Carchi	102	121	590	591	483	492	406	394	364	369	322	328	412	484	379	447
Cotopaxi	275	257	590	614	621	595	515	541	487	508	432	468	510	1037	929	1013
Chimborazo	437	382	582	598	611	600	493	520	501	478	395	383	591	664	555	587
El Oro	557	474	687	777	739	699	763	775	717	711	711	742	732	895	793	818
Esmeraldas	242	256	867	870	809	845	686	570	569	624	490	550	583	684	624	670
Guayas	1703	1401	1068	1325	1299	1179	1207	1179	1542	1545	1425	1547	1439	1695	1712	1832
Imbabura	261	297	494	542	518	601	438	381	420	396	426	360	433	1000	809	975
Loja	414	378	568	595	574	560	450	551	428	503	487	518	556	574	566	627
Los Rios	453	423	692	817	777	701	572	553	583	641	595	637	585	657	544	719
Manabí	518	541	752	1084	869	851	727	626	625	730	687	650	593	722	661	846
Morona Santiago	140	136	119	128	91	101	129	105	89	122	98	108	122	582	579	611
Napo	88	100	165	179	145	182	104	132	104	159	102	90	123	928	913	1099
Pastaza	147	143	69	85	67	69	56	57	65	77	48	64	63	516	490	492
Pichincha	818	840	796	844	845	841	731	700	1014	1062	1125	999	978	1243	1180	1413
Tungurahua	341	321	731	679	638	616	699	791	689	700	694	657	772	1131	982	1055
Zamora Chinchipe	63	48	67	99	70	96	61	67	58	75	63	64	70	590	569	583
Sucumbíos	179	161	127	153	128	121	95	100	76	91	128	88	77	393	392	432

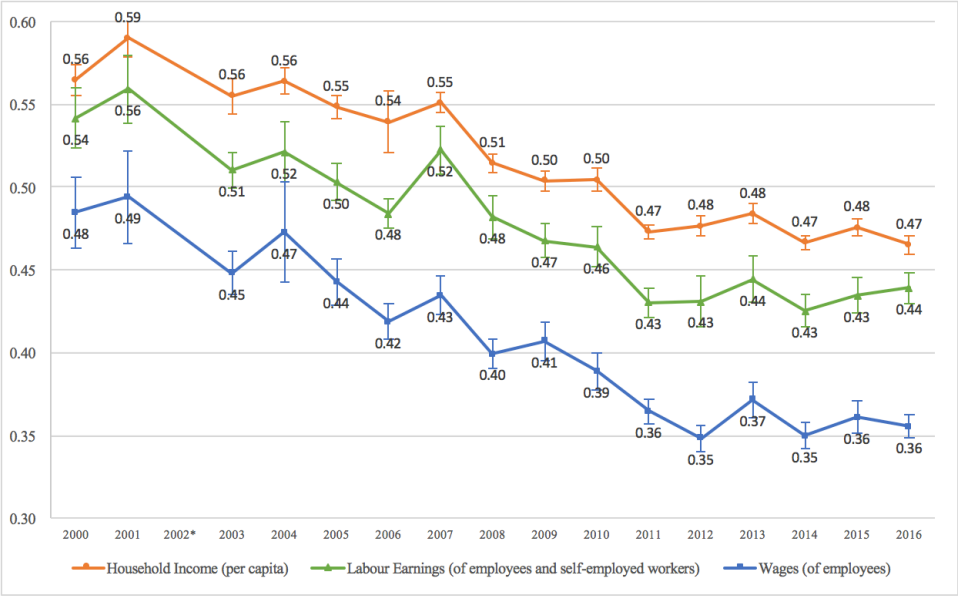
Source: Author's calculations from ENEMDUR 2000-2016

Figure 3.4 CDF Estimates of the Log Hourly Wage Distribution of Covered Workers, 2000 - 2016



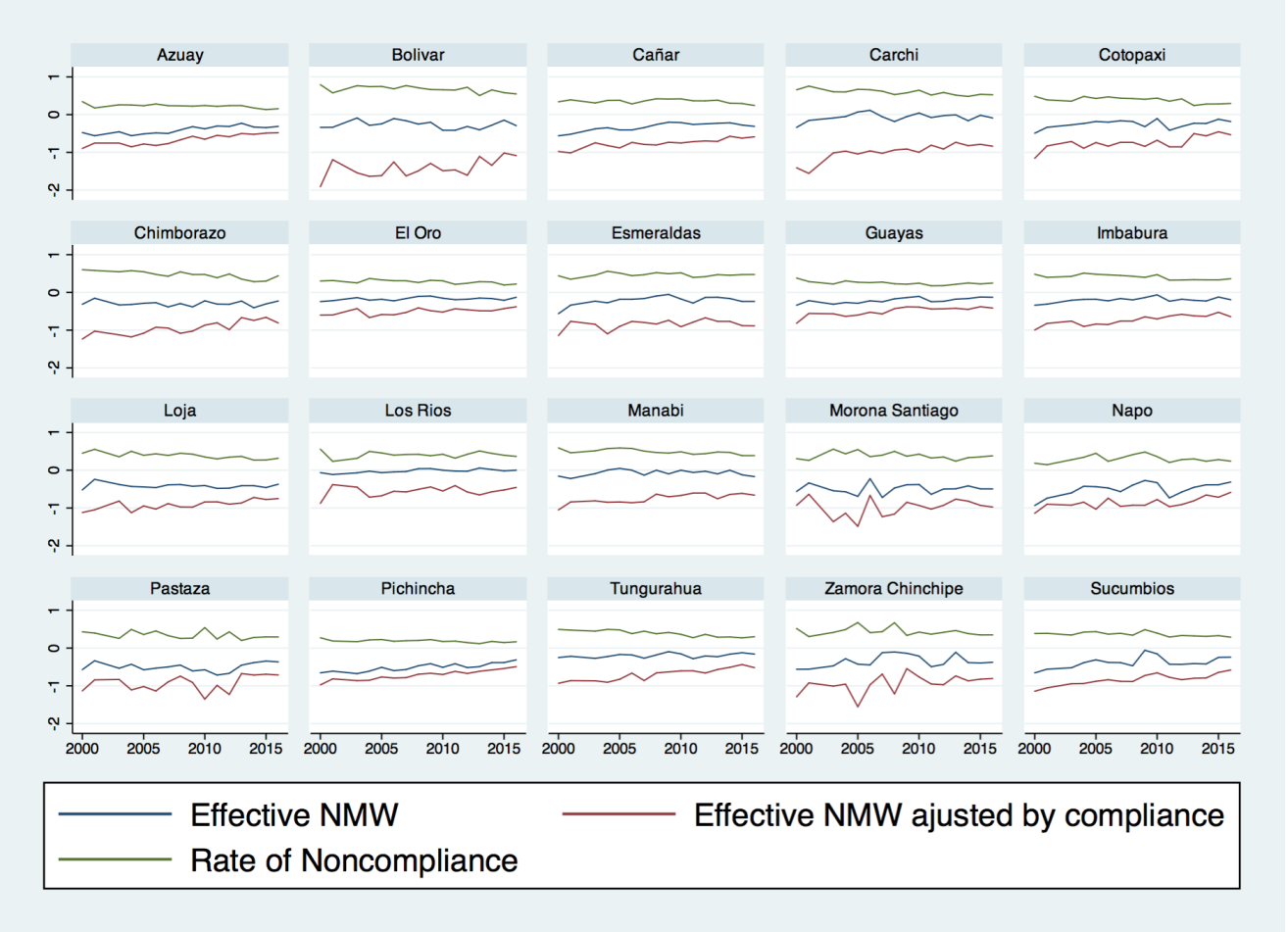
Source: Author's calculations from the ENEMDUR 2000-2016.
Note: The vertical line corresponding to the log of the hourly NMW in each year.

Figure 3.5 Trends in Income Inequality by Income Source, Gini Coefficient, 2000-2016



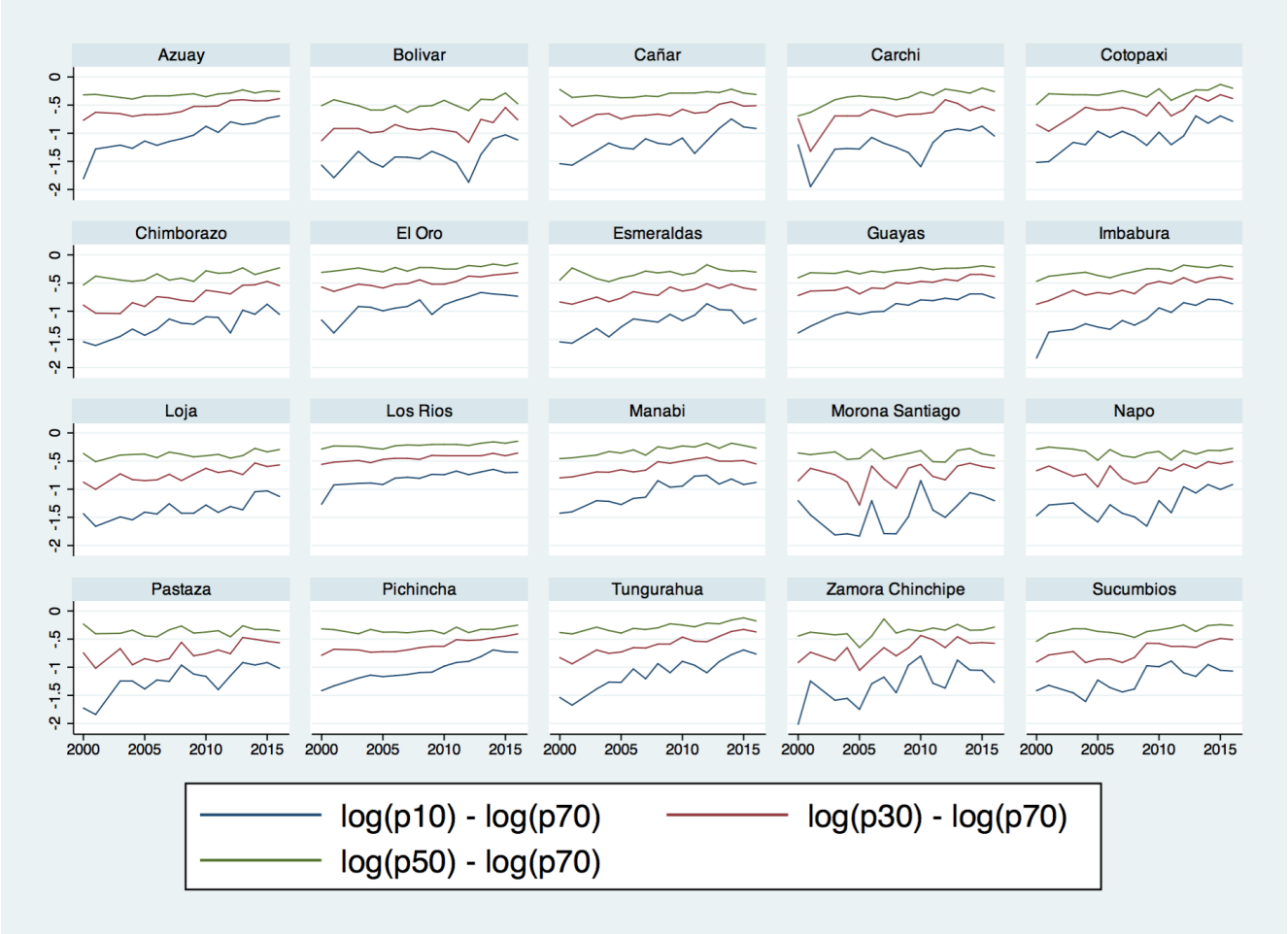
Source: Author's calculations from the ENEMDUR 2000-2016, excluding 2002* which covered urban areas only.
Error bars show point-wise 95% confidence intervals, calculated using bootstrap standard errors (50 replications).

Figure 3.6 The Effective NMW, The Effective NMW Adjusted for Noncompliance, and the Rate of Noncompliance by Province



Source: Author's calculations from the ENEMDUR 2000-2016.
Notes: The effective NMW results from the differential between the value of the NMW and the 70th percentile of each province. Noncompliance is measured by the percentage of full-time employees who are paid less than 90% of the value of the monthly NMW, in each province. The NMW adjusted for noncompliance refers to an index of the NMW policy which adjusts the effective NMW by the rate of noncompliance. All series are computed using the ENEMDUR sample weights.

Figure 3.7 Trends in Lower-Tail Inequality by Province



Source: Author's calculations from the ENEMDUR 2000-2016.
Notes: The Figure depicts the evolution of the difference between the log of percentiles 10th, 30th, 50th and the log of the 70th percentile, computed for each province. All series computed using the ENEMDUR sample weights.

Part II

4 The Impact of a Cash Transfer Programme on Food Expenditure: The Bono de Desarrollo Humano (BDH) of Ecuador

4.1 Introduction

Cash transfer (CT) programmes are widely used in developing countries as a policy tool to break the intergenerational transmission of poverty. In Latin America, CTs gained popularity at the beginning of the 2000s and the majority of countries implemented conditional cash transfer (CCT) programmes (i.e., tied to certain behaviours such as: school enrolment, health check-ups, and attendance at health information sessions). CTs were established with the objective of fostering the accumulation of human capital among children in poor families, focusing mainly on access to food, health and education.

In Ecuador, the first means-test CT programme was launched in 2003 and was called the Bono de Desarrollo Humano (BDH). Mothers of children aged 0-18 years qualify to receive a monthly transfer if they are in the bottom two quintiles of the "Selben" index. In contrast to most CT programmes in the region, the BDH was implemented as an unconditional cash transfer (UCT) programme. The objectives of the programme, as defined by the Ministry of Economic and Social Inclusion, are: (i) to ensure a minimum consumption of food and access to basic services, (ii) to reduce child malnourishment, and (iii) to incentivise school attendance.[7]

The impact of this unconditional cash transfer programme on access to food is a relevant question considering that in the last decade the fraction of children with chronic malnutrition in the country has stagnated at around 25% (SIISE 2015). There are two papers that analyse the effects of the BDH on food expenditure. The first one finds that rural households that were randomly assigned to receive the transfer have a significantly higher food share, after the programme, compared to households in the control group (Schady and Rosero 2008). The second one finds that the programme increased the total amount of food expenditure for beneficiary household in urban areas (Buser et al. 2017). However, this prior research has

[7] These goals are outlined in the description of the BDH programme available online at: http://www.inclusion.gob.ec/objetivos-bdh/.

been mostly silent on the question of which of the components of food expenditure has increased the most.

In this paper, I assess the effects of the BDH on food expenditure and its components, using a dataset that covers both urban and rural areas. The objective is to identify if the BDH improves the quality of the diet of the beneficiary households. For example, the Ecuadorian government has a goal that at least 260 Kcal /day of food intake should consist of proteins; based on a diet of 2000 Kcal /day this goal implies that at least 13% of food energy should derive from proteins.[8] In this study, I reveal in which of the components of food expenditure the BDH has an effect. To do so, I classify the expenditure on food into eight groups, based on their nutritional intake: 1) proteins (meat, chicken, milk, etc.); 2) potatoes, yucca and other tubers; 3) cereals; 4) fruit and vegetables; 5) pulses; 6) fats and oils; 7) sugar and sweets; and 8) miscellaneous food.

Additionally, I evaluate the effects of the BDH on the consumption of unintended items. While most of the studies in this regard focus on the effects of CTs on alcohol and tobacco consumption (see Evans and Popova 2014, for a review of the literature), I include in the analysis expenditure on mobile top-ups. I do so also with the objective of trying to deal with the problem of underreporting of expenditure on unintended items. This issue may be more present when asking individuals about their expenditure on alcohol and tobacco than when asking them about their expenditure on mobile top-ups. Finally, I also evaluate if the BDH has an impact on the consumption of durable goods.

The monthly CT increases poor household's disposable income, shifting to the right the budget constraint of the household. This increase in total household income allows members of the household to consume more. If the family uses the CT to purchase more and better-quality food, or invest in food production or productive assets, household food security and household diet diversity are improved. Additionally, the improvement in access to food may help to improve children's nutritional intake if food resources are shared in a child-sensitive way within the household (see Groot et al. 2017, for a comprehensive synthesis of the link between CTs and child nutrition).

Another mechanism by which the monthly CT can improve the availability of food, and its quality, is by affecting intra-household dynamics of bargaining power. If the transfer is received by women, as in the case of the BDH, then they are better able to advocate for their preferences as a result of controlling more resources. There is some evidence that rejects the

[8] See goal 2.1.2. in the Ecuadorian National Plan 2009-2013.

hypothesis of income pooling resources within the family and shows that transfers made to women have substantial and significant effects on family expenditure patterns increasing the expenditure on food and on items for children (Thomas 1990; Hoddinott and Haddad 1995; Lundberg et al. 1997; Phipps and Burton 1998; Doss 2006). Non-unitary economic models of household bargaining predict that transfers made to women will increase their bargaining power within the household and result in expenditure patterns that reflects better their preferences (see for example, Chiappori 1988; Bourguignon et al. 1993).

There are several studies that evaluate the effects of BDH. The studies show the programme increases school enrolment (Araujo and Schady 2006), but it does not have an effect on cognitive achievement (Ponce and Bedi 2010). With regard to child development for under 6s, a study finds that the programme has modest positive effects on development outcomes (including growth, cognition and language), but only for rural children (aged 36-72 months) from the lowest income quartile (Paxson and Schady 2010). Similarly, for younger children (aged 12-35 months) in rural areas, another study finds that the BDH programme improves language development but it does not have an effect on child malnourishment (Fernald and Hidrobo 2011).

In terms of food expenditure, Schady and Rosero (2008) evaluate the effect of the BDH transfer received by women in rural areas on food share. The authors find that the BDH programme increases the expenditure on food as a fraction of total expenditure. They suggest that the BDH is used differently from other sources of income. This goes in line with the theory of non-unitary household's model. An important limitation in respect to the external validity of their study, however, is that it covered rural areas only, while approximately 60% of BDH recipients live in urban areas. On the other hand, in a study for urban areas only, Buser et al. (2017) find that two years after beneficiary families lost the BDH transfer their young children weighted less and were shorter and more likely to be stunted than young children in families that kept the BDH transfer. The authors identified that the mechanism behind this result is a reduction in total household food expenditure by households that lost the transfer.

The studies that evaluate the impact of CCTs on food consumption patterns, for the region, find that the programmes increase expenditure on fruits, vegetables and products rich in proteins, such as meat, chicken, eggs, etc. (see Hoddinott and Skoufias 2004, for *Oportunidades* in Mexico; Maluccio and Flores 2005 for *Red de Protección Social* (RPS) in Nicaragua; Attanasio and Mesnard 2006, for *Familias en Accion* in Colombia; Martins and Monteiro 2016, for *Bolsa Família* in Brazil). In the case of the Mexican programme, *Oportunidades*, a study also shows that the programme increases the expenditure on durable

goods, including TVs, DVDs, phones, white goods, etc. (Angelucci et al. 2012). In contrast, in the case of the Nicaraguan programme, *Red de Protección Social,* Maluccio (2010) does not find evidence that the programme led to an increase in the consumption of this kind of items.

A distinguishing feature of the BDH compared to other CT programmes in Latin America, is that it was implemented as an unconditional cash transfer programme. This characteristic raises the question of whether UCT programmes generate similar effects in terms of changes in food consumption patterns or whether these kinds of transfers are spent on other unintended items such as mobile top-ups, alcohol and tobacco. Contributing to the answering this question, this study I evaluate the effects of the unconditional BDH transfer on expenditure, focusing primarily on food expenditure and its components.

UCTs allow recipients freedom of choice and assume efficient allocation of resources based on the specific needs of each household. On the other hand, from a paternalistic view, in-kind transfers can be useful to correct distortions in the use of the cash transfer, although these kinds of transfers may not provide the specific items the family needs. A middle approach, between these two options, are CCTs, with which the recipients get the cash, but on the condition that the money is used to achieve specific objectives.

In the case of the BDH, the marketing campaign when launching the programme stressed that beneficiaries were responsible of enrolling their children to school and taking the under 5's to health centres for growth and preventive check-ups. In addition, the beneficiaries were encouraged to participate in nutrition and family planning talks. However, in practice, the government never enforced and monitored compliance with these conditions, which makes the BDH an UCT programme.[9]

After using a regression discontinuity identification strategy, the results suggest that the BDH has an impact on food consumption by increasing the expenditure on protein-rich food such as meat, chicken, cheese, eggs, etc. for beneficiary household living in urban areas. The BDH increases the amount expended in this kind of products in both absolute terms (USD) and as a fraction of total food expenditure. This effect takes place mainly for beneficiary households with children under 5 years. For this group, the findings suggest that the programme also generates a change in the pattern of food consumption by reducing the spending on ready meals outside the house and substituting it with an increase in the expenditure on protein-rich products.

[9] In 2012, after the dataset used in this study was collected, the government started to randomly check the conditionalities covering a small fraction of beneficiary households.

The remaining of this study is organized as follows. Section 4.2 provides a background to the BDH cash transfer programme. In Section 4.3, I discuss and present my identification strategy. I describe the data used for the analysis in Section 4.4. Section 4.5 presents the results of the analysis of the effects of the BDH on total expenditure and on food expenditure. Finally, in Section 4.7, I discuss the results and conclude.

4.2 Programme Background

The implementation of a cash transfer programme in Ecuador began in 1998 with the launching of the *Bono Solidario.* This unconditional cash transfer programme was designed to compensate poor household for the removal of gas and electricity subsidies. In 2003, the government merged the *Bono Solidario* with *Beca Escolar,* a conditional cash transfer programme designed to increase school enrolment. The *Bono Solidario* became an UCT programme and was renamed as *Bono de Desarrollo Humano* (BDH). The main goal of the BDH is to improve the accumulation of human capital among poor Ecuadorian families.

The BDH is the first cash transfer programme that uses a proxy means test to target the poorest families and select the beneficiaries. The score is computed using principal components analysis and it is called the Selben index. The information to compute the Selben score comes from the *Registro Social* (RS), which is a dataset of a census of poor households updated every five years. From 2003 to 2008 the BDH was targeted based on the Selben I index. All household in the lowest 40% of the Selben I distribution (those with a score less than 50.65) were defined as "poor" and were eligible to participate in the programme. The amount of the monthly transfer was initially $15 USD for households in the lowest 20% of the distribution and $11.50 USD for those located above the 20% and below the 40% levels. In 2007, the transfer was increased to $30 USD for all households in the bottom 40% of the Selben I distribution.

From 2009 to 2013, the BDH was targeted based on the new Selben (Selben II) score using information from the second wave of the RS, mostly collected in 2008. The Selben II index is computed using 59 variables covering areas such as household asset, characteristics of the house (access to water, toilet, shower, type of floor, walls and roof, and household appliances), characteristics of the head of household (level of education, employment), characteristics of children, and household size (see Fabara 2009, for the complete list of variables).

During the time period from 2009 to 2013, all households with a score less than 35.6 points in the Selben II were defined as "poor" and were eligible to get the monthly transfer of

$35 USD per month. Finally, in March 2013, the government reduced the cut-off and increased the amount of the transfer so that all households with a Selben II score lower than 32.5 were eligible to get a monthly transfer of $50 USD.

The target population of BDH are mothers in households defined as "poor" with children aged 0 to 18 years. The monthly transfer is received by women, who can be the head of the household or the wife of the head of the household. It can also be the case that the transfer recipient is the daughter of the head of the household or another woman, within the household, who has children under 18 years. The BDH transfer is collected by mothers through local banks and, since 2008, mothers also have the option of collect the transfer in food stores, pharmacies, and other establishments, that have a payment terminal machine.

The non-enforced conditions, called "co-responsibilities" by the administrators of the programme, are that school-aged children (5-18 years of age) have to regularly attend school and under-5s have to go to health checkouts. The administrators of the programme use the term "co-responsibilities" to reflect the responsibility that parents have in complying with the conditions of the programme. However, this approach implied, in practice, that there were no substantive efforts to control and enforce the conditionalities impose by the programme. This fact makes the BDH to be regarded as a UCT programme.

In this paper, I assess the effects of the BDH with data collected by the end of 2011. The amount of the unconditional monthly transfer was therefore $35 USD, which represents around 13% of the monthly national minimum wage (NMW, which equals $264 USD in 2011). In 2011, the annual budget of the programme was $722 million USD (around 1 percent of GDP) and the programme covered 1.2 million households (around 32% of the total number of households in Ecuador). Approximately 60% of the beneficiaries lived in urban areas and 40% lived in rural areas.

4.3 Data

I use the Survey of Socioeconomic Situation of Households (ESSHO for its Spanish acronym) collected between October and December 2011 by the Ministry of Social Development of Ecuador. The ESSHO survey covered both urban and rural areas. It collected information from a variety of households and individual characteristics, including an extensive and detailed module on household expenditure. I use this expenditure module to compute total expenditure and total food expenditure. I also distinguish between the different components of these two aggregates to analyse the effect of the BDH on expenditure components.

The ESSHO 2011 has information for 1,422 households with children aged under 18 years. One important characteristic of this survey is that it samples all the households registered in the Registro Social, with a sample designed such that the Selben II index of the selected households is normally distributed around the threshold of 36.5. This sample design gives us the opportunity to compare households with a Selben II index just below the threshold (treated group) with those with a Selben II index just above the threshold (control group) and have enough observations to make the comparison.

The ESSHO questionnaire asks about a wide range of household expenditures, including expenditure on durables, on household services, on food (to prepare and serve in the house, and ready meals consumed outside), on school-related items for under 5s, on school-related items for children older than 5 years, on alcohol and tobacco, on mobile credit, and on other services such as entertainment and travel tickets. I calculate expenditure in each of these items and compute total expenditure. All expenditure items were converted into weekly flows and expressed in US dollars (the official currency of Ecuador since 2000). Additionally, I compute the share of expenditure in each of these items as fraction of total expenditure

For the analysis of the effect of the BDH on the components of food consumption, I first classify the expenditure on food into eight groups, based on their nutritional intake: 1) proteins (meat, chicken, milk, etc.); 2) potatoes, yucca and other tubers; 3) cereals; 4) fruit and vegetables; 5) pulses; 6) fats and oils; 7) sugar and sweets; and 8) miscellaneous food (which includes expenditure on food outside the house). For each group of food, I compute weekly expenditure in US dollars and the share of expenditure as fraction of total food expenditure.

In Panel A of Table 4.1, I show the average household weekly expenditure for the components of total expenditure, as well as the average of their shares as fractions of total expenditure. In Panel A of the table, I present the average of the weekly expenditure in each of the components of food expenditure and the average shares, as fractions of total food expenditure. I present the results for BDH recipients and non-recipients using the complete sample of the ESSHO (N=1422 households), with SELBEN II scores ranging from 1.5 to 77.3.

The results in Table 4.1 show that there are substantial differences in expenditure in USD, and in their shares, between recipients and non-recipients in almost all expenditure items. As expected, total expenditure for BDH recipients is about 61 USD lower than non-recipients. In line with the food Engel curve, poorer households that are beneficiaries of the BDH have a significantly higher food share, 44%, compared to richer households that score above the threshold, and do not receive the transfer, 38%. In terms of shares of expenditure for the components of total food expenditure, BDH recipients have a lower share on products rich in

proteins and have a larger share on cereals, pulses, fat and oils, and sugar and sweets, compared to non-recipients.

Table 4.1 Household weekly expenditure in USD, BDH recipients and non-recipients

	Mean (weekly, USD)			Share (as fraction of the total)		
	Recipi-ents	Non-recipi-ents	p-value, diff	Recipi-ents	Non-recipi-ents	p-value, diff
Panel A: Total expenditure and its components						
Total expenditure	110.79	172.14	0.00			
Food expenditure	45.65	55.64	0.00	0.44	0.38	0.00
Food expenditure outside the house	10.10	17.99	0.00	0.08	0.10	0.00
Alcohol and tobacco	3.17	2.28	0.03	0.02	0.01	0.00
Mobile credit	1.70	3.35	0.00	0.01	0.02	0.00
Household services	5.23	13.66	0.00	0.05	0.09	0.00
Durables	30.47	56.19	0.00	0.26	0.26	0.36
School-related for under 5s	0.34	1.03	0.00	0.00	0.01	0.00
School-related (older than 5)	6.06	11.03	0.00	0.05	0.07	0.00
Other expenses	8.06	10.97	0.00	0.07	0.07	0.63
Panel B: Total food expenditure and its components						
Total food expenditure	55.75	73.64	0.00			
Proteins (meat, chicken, milk etc.)	16.06	21.81	0.00	0.28	0.30	0.04
Potatoes, yucca and other tubers	2.23	2.68	0.00	0.04	0.04	0.13
Cereals	9.97	10.63	0.06	0.19	0.16	0.00
Fruit and vegetables	8.93	11.66	0.00	0.16	0.16	0.55
Pulses	0.81	0.86	0.29	0.02	0.01	0.00
Fats and oils	2.50	2.24	0.01	0.05	0.03	0.00
Sugar and sweets	3.04	3.20	0.31	0.06	0.05	0.00
Miscellaneous food	12.22	20.56	0.00	0.20	0.25	0.00
Number of Obs.	774	648		774	648	

Source: Author's calculations from ESSHO 2011 dataset.

In order to identify the effects of the BDH on food consumption, I need to work with a comparable sample of households. As stated above, households scoring below 36.5 in the Selben II index received the BDH, and households scoring above this threshold did not receive the BDH. This creates the conditions to use a regression discontinuity design (RD) to try to identify the effects of the programme.

Restricting the sample to households within a very small interval around the threshold will provide a potentially unbiased estimate of the impact of the programme. However, the selection of the bandwidth around the threshold presents a clear bias–variance trade-off, where wider bandwidths will be more efficient (will have lower variance) at the expense of increased risk of bias in the estimates, and narrower bandwidths will be less efficient (will have larger variance) but are likely to be less biased. To implement the comparison, following Buser et al. (2017), I restrict the sample to household scoring within 0.3 standard deviations of the cut-off on Selben II index.

Table 4.2 Descriptive statistics for selected variables of BDH recipients and non-recipients close to the SELBEN II threshold

	Recipients	Non-recipients	p=value, diff
Household characteristics			
Urban	0.78	0.75	0.51
Proper roof	0.81	0.75	0.16
Proper floor	0.84	0.83	0.86
Proper walls	0.84	0.85	0.74
Pipeline water access	0.65	0.76	0.03
Proper toilet	0.90	0.95	0.05
Proper shower	0.64	0.68	0.43
Household size	4.93	4.68	0.21
Crowding	2.17	2.01	0.25
No. children aged 0-4	0.68	0.60	0.38
No. children aged 5-18	1.75	1.73	0.92
No. persons aged 19-44	1.85	1.79	0.61
No. persons aged 45-64	0.65	0.55	0.22
No. persons age 65 or older	0.02	0.01	0.54
Head of household (HH) characteristics			
Age	41.68	41.25	0.71
Years of education	6.86	7.75	0.02
Female	0.25	0.20	0.30
Indigenous or Afro-Ecuadorian	0.13	0.15	0.61
N	150	194	

Source: Author's calculations from ESSHO 2011 dataset.
Note: Proper floor, roof, and walls are dummy variables indicating the state of the building where 1 denotes a "good" or "normal" state. Water access is a dummy indicator for whether the household is connected to the water network. Proper toilet and shower are dummy variables for the availability of toilet facility and shower in the household, respectively. Crowding denotes number of household members per room.

In the ESSHO dataset, the interval of 0.3 standard deviations around the cut-off corresponds to a bandwidth of +/- 5 points in the Selben II index, from 31.5 to 41.5. In my main analysis of the effects of the BDH, I restrict the ESSHO sample to households scoring within this interval of the Selben II index; the number of observations in this restricted sample is 344, with 150 BDH recipients and 194 non-recipients. In Table 4.2, I present descriptive statistics for the restricted sample. The table shows that there are not significant differences in most of the households and head of household's characteristics between BDH recipient and non-recipients. Additionally, as robustness checks, I also present the estimates of the effect of the BDH using a data driven method for the selection of the bandwidth.

Table 4.3 Means of the outcome variables of BDH recipients and non-recipients close to the SELBEN II threshold

	Mean (weekly, USD)			Share (as fraction of the total)		
	Recipi-ents	Non-recipi-ents	p-value, diff	Recipi-ents	Non-recipi-ents	p-value, diff
Total expenditure and its components						
Total expenditure	126.20	131.63	0.54			
Food expenditure	48.86	51.25	0.41	0.43	0.42	0.93
Food expenditure outside the house	11.11	14.35	0.04	0.08	0.10	0.07
Alcohol and tobacco	2.71	2.47	0.73	0.02	0.02	0.67
Mobile credit	2.43	2.05	0.21	0.02	0.02	0.10
Household services	6.26	8.83	0.00	0.06	0.07	0.01
Durables	38.09	34.45	0.50	0.26	0.23	0.02
School-related for under 5s	0.47	0.52	0.76	0.00	0.00	0.76
School-related (older than 5)	7.20	8.15	0.35	0.06	0.06	0.55
Other expenses	9.04	9.55	0.70	0.07	0.07	0.76
Total food expenditure and its components						
Total food expenditure	59.97	65.60	0.13			
Proteins (meat, chicken, milk etc.)	17.23	18.16	0.48	0.29	0.28	0.54
Potatoes, yucca and other tubers	2.58	2.56	0.96	0.04	0.04	0.79
Cereals	10.27	11.11	0.24	0.18	0.18	0.91
Fruit and vegetables	10.50	10.70	0.82	0.17	0.16	0.25
Pulses	0.75	0.87	0.30	0.01	0.01	0.77
Fats and oils	2.24	2.27	0.87	0.04	0.04	0.56
Sugar and sweets	2.89	3.12	0.35	0.05	0.05	0.89
Miscellaneous food	13.51	16.80	0.05	0.21	0.24	0.17
Number of Obs.	150	194		150	194	

Source: Author's calculations from ESSHO 2011 dataset.

Finally, in Table 4.3, I also present the average of the outcome variables for recipient and non-recipients for my restricted sample. There are not significant differences in most of the components of total expenditure and of total food expenditure. The only exceptions are expenditure on household services and on food outside the household, for which non-recipient have a higher expenditure in USD and a higher share, as a fraction of total expenditure.

4.4 Empirical Strategy

The idea of the RD identification strategy is that assignment to the programme for each household, i, depends on the value of an observed continuous variable, running variable, r_i, relative to a given threshold, denoted by c. For example, if $r_i > c$ the household does not receive the treatment (control group), and if $r_i < c$ the household gets the treatment (treatment group). In the RD design one compares the outcome variables between treated and control households within a small interval around the threshold. By restricting the analysis to observations close to the threshold the RD method try to make the treatment and control group as alike as possible. In a small interval around the threshold, controlling by the assignment and other covariates, any difference in the outcome variables between the treatment and control group can be attributed as the effect of the programme.

In Table 4.4, I present the relationship between the assignment rule and the self-reported treatment status of households around the threshold. The table shows that eligibility and programme status match in about 83 percent of cases in the sample (287/344), but there are 36 households who are eligible for the BDH but report in the survey that they do not receive the programme. There are also 21 households who are not eligible for the programme but report in the survey that they get the BDH cash transfer.

Table 4.4 Assignment rule and treatment status

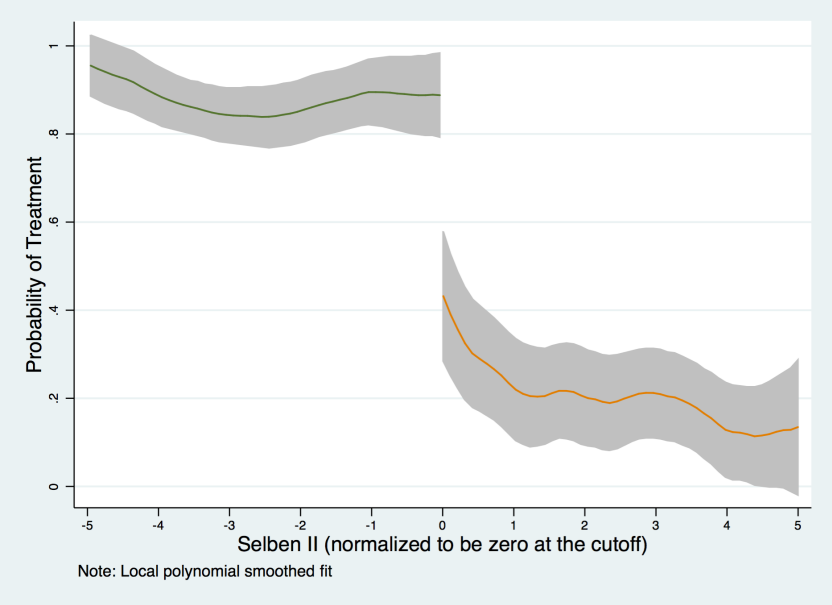
Treatment status	Selben II score		
	Less than 36.5	More than 36.5	Total
Recipients	129	21	150
Non-recipients	36	158	194
Total	165	179	344

Source: Author's calculations from ESSHO 2011 dataset.

The results in Table 4.4 imply that treatment status in my restricted sample is not a deterministic function of the running variable. In this case, I cannot implement a sharp RD

design. However, the probability of receiving the BDH is a discontinuous function of the Selben II score at the threshold. As shown in Figure 4.1, there is an increase in the probability of being treated, with a sharp spike at the cut-off. Households with a Selben II index of less than 36.5 are about 50 percentage points more likely to receive the BDH compared with households that have a Selben II index of just above the 36.5.

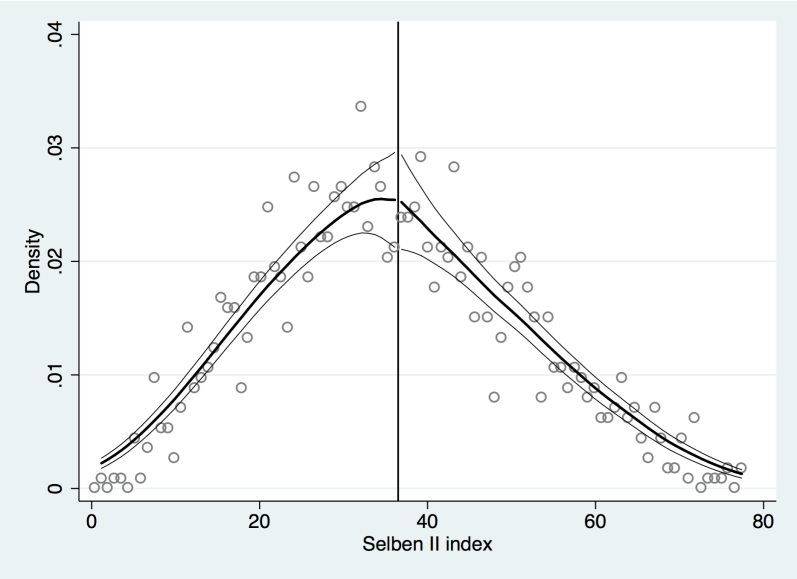
Figure 4.1 Relation between treatment and RS index



Source: Author's calculations from ESSHO 2011 dataset.
Note: Following a suggestion by Imbens and Lemieux (2008), two separate smoothed regression lines based on local polynomials are estimated on either side of the RS cut-off point of 36.5 and the predicted probabilities of treatment from these two regressions are plotted versus the RS index. For the local polynomial estimates the Epanechnikov kernel is used as the kernel function and the bandwidth is given by the "rule-of-thumb" proposed by Silverman (1986).

The main assumption behind the RD design is that unobserved characteristics vary continuously with the observed characteristics used to determine the treatment, for households around the threshold. This assumption may not be true if households can manipulate their position relative to the threshold. If there is not manipulation, then there should be no break in the distribution of the household score as the cut-off is approached from above or below. Figure 4.2 shows the density of the Selben II index. In the figure, we can see that there is not a sharp increase in the number of observations either right above or right below the cut-off point of 36.5. The results of the formal McCrary (2008) manipulation test confirm this finding; we fail to reject the null hypothesis of no sorting around the threshold (the p-value of the t-test is larger than 0.10).

Figure 4.2 McCrary manipulation test



Note: with a bin size of 0.79 and a bandwidth of 17 the density of the assignment variable, the SELBEN II index, does not show significant differences around the cut-off of 36.5. Using the formal sorting test proposed by McCrary (2008), we do not reject the null hypothesis of no sorting around the threshold (p-value=0.43).

Once I confirm that there is not the problem of manipulation, or sorting, around the cut-off, I exploit the discontinuity in the probability of treatment around the cut-off (see Figure 4.1) to identify the causal effect of the BDH. Specifically, I employ a fuzzy regression discontinuity design and estimate the effects of the BDH using a two-stage least squares (2SLS) framework (Angrist and Lavy 1999; Hahn et al. 2001; Battistin et al. 2009). In the first stage, I instrument the treatment status, T_i, with the assignment rule, $1\{r_i \leq 36.5\}$, as follows:

$$T_i = \delta 1\{r_i \leq 36.5\} + f(r_i) + \boldsymbol{X}_i\tau + w_i \qquad (4.1)$$

where $1\{r_i \leq 36.5\}$ is an indicator function that takes the value of one when the Selben II score of household *i* is lower that the cut-off (36.5) and zero when the score is above the cut-off.

$f(r_i)$ is a smooth function of the Selben II index, r_i. In the empirical literature it is common to use a high-order (third, fourth or higher) polynomial of the running variable, nevertheless, following the recommendations of Gelman and Imbens (2014), I present the results using a linear and a quadratic function. Finally, $\boldsymbol{X}_i$ is a vector of control variables that includes a dummy variable for households located in urban areas, a set of variables for the head of the household (female dummy, indigenous or Afro-Ecuadorian dummy, years of schooling), and the number of individuals in the household.

The second stage is then:

$$Y_i = \alpha + \beta T_i + f(r_i) + \boldsymbol{X_i}\theta + u_i \quad (4.2)$$

where Y_i is the outcome variable, T_i is a binary indicator for receiving the BDH transfer, instrumented by the assignment rule (see Eq. 4.1). Other notation is as in the first stage equation.

I run this model for total expenditure and for each component of total expenditure, both expressed in weekly USD. I also run the model for the expenditure in each component of total expenditure expressed in shares as fractions of total expenditure. Similarly, I run the model for each component of total food expenditure in weekly USD and in shares as fractions of total food expenditure.

In the parametric regression models, in the form of Eq. 4.2, I use the restricted sample of household scoring ± 5 around the discontinuity threshold and assume that households below and above the cut-off are essentially identical, prior to the treatment. Thus, any differences in expenditure patterns between these two groups, once controlled for the running variable, are regarded as the causal effect of the BDH.

In the fuzzy regression discontinuity case, the estimate of β in Eq. 4.2 is the treatment effect for those whose participation in the BDH is influenced by the assignment rule. This effect is usually called the local average treatment effect and is estimated only for the compliers, i.e.; household with a score less than the thresholds that received the BDH transfer and household with a score higher than the threshold that do not receive the transfer.

Finally, as a robustness check, I estimate the effects using nonparametric local polynomial estimators (Calonico et al. 2014a), with bias-corrected point estimators and robust standard errors (Calonico et al. 2014b). For the selection of the bandwidth around the cut-off, I use a mean-squared error (MSE) optimal bandwidth selection procedure (Calonico et al. 2017). These nonparametric estimators are the results of weighted polynomial regressions estimated separately to each side of the cut-off within the chosen bandwidth.

4.5 Results

4.5.1 BDH Programme Participation

In Table 4.5, I show the estimate of the first stage equation (see Eq. 4.1), confirming the statistical significance of the discontinuity seen in Figure 4.1. The coefficient on the assignment

rule, my instrument, is around 0.51 and is statistically significant at the 1% level in all three specifications. Additionally, the partial R^2 of the instrument is around 0.10 and the F-statistic is larger than 10, which suggest that our IV estimates of the effect of the BDH implemented via 2SLS are reliable (Stock et al. 2002)

Table 4.5 Participating in BDH (first stage)

	Participating in BDH		
	(1)	(2)	(3)
Below cutoff point of 36.5 ($Z=1$)	0.511***	0.509***	0.503***
	(0.088)	(0.088)	(0.089)
Quadratic term	N	Y	Y
Covariates	N	N	Y
R2	0.456	0.457	0.477
Partial *R*2 of excluded instrument	0.103	0.103	0.103
F-Statistic on excluded instrument	38.872	37.048	36.485

Source: Author's calculations from the ESSHO 2011 dataset.
Notes: N = 344. Sample limited to household with a Selben II score of ± 5 around the cut-off. Robust standard errors clustered at the province level appear in parentheses. Controls: dummy for urban areas, a set of variables for the head of household (female dummy, indigenous or Afro-Ecuadorian dummy, years of schooling), and the number of individuals in the household.
*10%, **5%, and ***1% significance level.

4.5.2 Effect on Total Expenditure and its Components

Before I present the effect of the BDH on the patterns of food expenditure, I show the results of estimates of the impact of the BDH on total expenditure and its components, including total food expenditure in and outside of the house. Table 4.6 contains the results. In Column 1, I use a linear function of the running variable and do not add the set of the control variable, $\boldsymbol{X_i}$. In Columns 2 and 3, I show the results using a quadratic function of the running variable without and with the set of the control variable, respectively. Column 3 shows the result of my preferred specification. In this model I control for characteristics of the head of the household (female dummy, indigenous or Afro-Ecuadorian dummy, years of schooling), a binary variable for urban areas, and the number of individuals in the household.

All three specifications show similar results. The IV estimates of the coefficient on the BDH treatment variable for the total expenditure model are large, positive and statistically significant at the 5% level. Based on the estimate of the more comprehensive specification (Column 3), my preferred specification, the results suggest that effect of the BDH on total

expenditure is about $79.6 USD per week. Analysing each component of total expenditure, we can see that this effect is generated by higher expenditure on food (inside the house), on mobile credit and on household durables. However, when I analyse the effects on shares, as a fraction of total expenditure, there is not significant effect of the programme on the share of expending in these items. Additionally, the results of the programme effects on shares also show a reduction in the share of expending on meals outside the house.

Table 4.6 IV estimates of the effect of the BDH on total expenditure and its components

	Programme Effects on Expenditure (USD)			Programme Effects on Expenditure Shares (as fraction of total expenditure)		
	(1)	(2)	(3)			
Total expenditure	73.585**	73.495**	79.626**			
	(36.862)	(36.589)	(32.133)			
Food expenditure	16.339*	15.994*	16.870**	0.017	0.016	0.011
	(9.158)	(8.949)	(8.355)	(0.070)	(0.071)	(0.073)
Food expenditure (outside the house)	-1.122	-1.142	-0.882	-0.049**	-0.049**	-0.047*
	(6.635)	(6.571)	(5.714)	(0.024)	(0.023)	(0.024)
Alcohol and tobacco	1.712	1.692	2.126	-0.002	-0.002	0.001
	(3.068)	(3.102)	(3.503)	(0.022)	(0.022)	(0.024)
Mobile credit	2.380**	2.411**	2.539**	0.004	0.004	0.005
	(1.134)	(1.177)	(1.160)	(0.005)	(0.005)	(0.006)
Household services	-1.966	-1.959	-1.552	-0.024	-0.024	-0.023
	(3.924)	(3.945)	(3.919)	(0.018)	(0.018)	(0.020)
Durables	44.424**	44.748**	49.159**	0.043	0.044	0.048
	(19.514)	(19.874)	(20.202)	(0.037)	(0.038)	(0.038)
School-related for under-5s	0.549	0.546	0.686	0.002	0.002	0.004
	(0.684)	(0.693)	(0.774)	(0.008)	(0.008)	(0.009)
School-related for children >5 years old	5.472	5.419	4.891	0.02	0.02	0.013
	(4.354)	(4.345)	(4.254)	(0.032)	(0.032)	(0.034)
Other services	5.796	5.787	5.791	-0.011	-0.012	-0.013
	(4.393)	(4.444)	(3.770)	(0.019)	(0.019)	(0.020)
Quadratic term	N	Y	Y	N	Y	Y
Covariates	N	N	Y	N	N	Y

Source: Author's calculations from ESSHO 2011 dataset.

Notes: Sample limited to household with a Selben II score of ± 5 around the cut-off. Robust standard errors clustered at the province level appear in parentheses. Controls: dummy for urban areas, a set of variables for the head of household (female dummy, indigenous or Afro-Ecuadorian dummy, years of schooling), and the number of individuals in the household.

*10%, **5%, and ***1% significance level.

From the non-parametric analysis, see Table 4.12 in the Appendix, the fuzzy RD estimates of the effect of the BDH for total expenditure and expenditure on durable goods are significant only at the 10% level. The expenditure on mobile top-up remind significant at the 5% level. The effect on food expenditure is not significantly different from zero. In terms of shares, none of the non-parametric estimates are significantly different from zero, under my preferred specification with controls. In this analysis, the bandwidths range from 6.2 to 15.2 and are computed using the mean-squared error (MSE) optimal bandwidth selection procedure (Calonico et al. 2017).

On Table 4.7, I present the IV parametric estimates of the BDH on total expenditure and its components for households classified into different groups: urban areas only, rural areas only, households with children under-5 years of age, and household without an under-5. The results for expenditure on food (to be consumed inside house) reveal that the increase, in absolute terms, is mainly driven by the effect on BDH recipients living in urban areas. This result goes in line with the findings Buser et al. (2017). However, while the authors find that the magnitude of the effect of the programme is quite close to the monthly transfer of 35 USD, the estimates on Table 4.7 suggest that the magnitude of the effect is around the double; 14 USD per-week or equivalently 60 USD per-month.

The results in Table 4.7 suggest that the BDH programme increases expenditure on school related items and services (books and notebooks, school uniforms, transport to school, tuition fees, etc.) for school-aged children living in urban areas, both in absolute terms (weekly USD) and as a fraction of total expenditure. On the other hand, the findings also suggest that the BDH programme reduces the expenditure on ready meals outside the house for household with under-5s, in both absolute terms (weekly USD) and as the fraction total expending

Before concluding this section, it is important to note that the IV estimates of the effect of the BDH are the ratio of the difference in average expenditure between those below and those above the cut-off (intention to treatment effect, ITT) to the first stage (treatment discontinuity). The IV estimates are therefore the effect of the BDH for the compliers within the chosen bandwidth. In Table 4.10 in the Appendix, I show the ITT estimates. For all of the outcome variables where the IV estimates are statistically significant, in Table 4.6, the ITT effects are positive and significant, but they differ in magnitude. As expected, the IV estimates are around the double of the ITT estimates, since the first stage coefficient is around 0.51.

In the next section, I examine if the increase in expenditure on food is driven by expenditure in nutrient rich products or in other products.

Table 4.7 IV estimates of the effect of the BDH on total expenditure and its components expenditure, for groups

	Programme Effects on Food Expenditure (USD)				Programme Effects on Shares (as fraction of total exp.)			
	Urban	Rural	Under-5 in H.	No under-5 in H.	Urban	Rural	Under-5 in H.	No under-5 in H.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total expenditure	82.627**	20.824	22.702	99.797***				
	(39.185)	(55.606)	(29.064)	(37.314)				
Food expenditure	14.440**	15.881	10.572	18.931	-0.004	0.117	0.065	0.006
	(6.728)	(32.576)	(9.992)	(14.601)	(0.071)	(0.134)	(0.073)	(0.095)
Food expenditure (outside)	-2.286	-4.416	-15.811***	9.309	-0.059**	-0.052	-0.115***	0.008
	(7.379)	(10.091)	(6.001)	(7.713)	(0.024)	(0.073)	(0.037)	(0.033)
Alcohol and tobacco	2.878	-3.824	-0.535	2.44	0.002	-0.018	-0.026	0.016
	(3.618)	(7.561)	(2.507)	(4.948)	(0.024)	(0.064)	(0.021)	(0.030)
Mobile credit	2.338*	2.315	3.031**	0.794	0.001	0.017	0.01	-0.005
	(1.238)	(2.741)	(1.274)	(1.502)	(0.006)	(0.017)	(0.010)	(0.008)
Household services	-2.001	-1.933	1.185	-4.582	-0.021	-0.031	-0.009	-0.03
	(4.608)	(4.673)	(3.935)	(6.094)	(0.024)	(0.023)	(0.020)	(0.033)
Durables	51.666**	17.000	12.777	65.759**	0.029	0.131	0.043	0.048
	(23.241)	(21.809)	(18.947)	(26.684)	(0.039)	(0.114)	(0.067)	(0.067)
School-related for under-5s	0.64	-0.072	1.86		0.003	0	0.013	
	(0.834)	(1.325)	(1.760)		(0.010)	(0.012)	(0.019)	
School-related for children >5	9.665***	-12.625	7.257**	-0.041	0.058***	-0.136	0.031	-0.023
	(3.305)	(14.574)	(3.649)	(6.037)	(0.020)	(0.120)	(0.025)	(0.058)
Other services	5.287	8.499	2.365	7.187	-0.009	-0.029	-0.011	-0.02
	(3.666)	(11.165)	(4.572)	(5.477)	(0.020)	(0.048)	(0.026)	(0.036)
N	265	79	168	176	265	79	168	176

Source: Author's calculations from ESSHO 2011 dataset.

Notes: Sample limited to household with a Selben II score of ± 5 around the cut-off. IV estimates using a second order polynomial of the Selben II index. Control variables: the number of individuals in the household; and for models shown in columns 3,4,7, and 8, I also include a dummy variable for urban areas. Robust standard errors clustered at the province level appear in parentheses. *10%, **5%, and ***1% significance level.

4.5.3 Effect on the Components of Food Expenditure

I now focus on the effect of the BDH on components of food expenditure. In Table 4.6, I showed that the BDH has a positive and significant effect on food expenditure, of about $17 USD per week. The results in Table 4.8 help us to identify if the impact on food expenditure is generated by expenditure in a particular group of food items. The table shows that the effect on food expenditure is generated mainly by the increase in expenditure on items rich in proteins such as meat, chicken, milk, cheese, etc. The results in Table 4.8 suggest that the BDH programme has a large, statistically significant, positive effect of around $14 USD per week on expenditure in items rich in proteins.

The results for expenditure shares (as a fraction of total food expenditure), see columns 4-6 of Table 4.8, suggest that the BDH generates a change in the pattern of expenditure on food. The programme increases the share of expending on protein-rich products in around 16 percent and reduces the share of expending on sugars and sweets in about 3 percent. The results from the non-parametric analysis confirm these findings (see Table 4.13 in the Appendix).

Comparing the magnitude of the estimates of the effect on total food expenditure, about $17 USD per week, and on expenditure in items rich in proteins, around $14 USD per week, one can conclude that the impact on food expenditure is mainly driven by a higher expenditure on these items among beneficiary households compared to non-beneficiary households. This finding is similar to the results of other studies of the impact of CCT programmes in Latin America such as *Progresa* in Mexico (Hoddinott and Skoufias 2004) and *Familias en Acción* in Colombia (Attanasio and Mesnard 2006), where the increase in consumption is largely driven by higher expenditure on animal products. However, in contrast to the research on the *Progresa* and the *Familias en Acción* programmes, I do not find that the BDH has a significant effect on expenditure on either fruit and vegetables or cereals.

In Table 4.9, I present the estimates of the effect of the BDH on the components of food expenditure for different groups of households. As found in the previous subsection, the increase on food expenditure take place mainly in beneficiary households living in urban areas and, as expected, the effect on expenditure on product rich in proteins is statistically significant only for urban areas (see Columns 1 and 5). The results in Columns 3, 4, 7 and 8, of Table 4.9, reveal that the increase in expenditure, in absolute value (weekly USD) and in share, on protein-rich products is more than the double for households with children under 5 compared to households where there is not an under-5.

Table 4.8 IV estimates of the effect of the BDH on food expenditure components

	Programme Effects on Food Expenditure (USD)			Programme Effects on Expenditure Shares (as fraction of total food expenditure		
	(1)	(2)	(3)	(4)	(5)	(6)
Proteins (meat, chicken, milk)	13.522**	13.401***	14.452***	0.164***	0.164***	0.174***
	(5.311)	(5.184)	(4.991)	(0.056)	(0.056)	(0.052)
Potatoes, yucca and other tubers	0.046	0.028	-0.111	-0.017	-0.017	-0.019
	(0.816)	(0.809)	(0.727)	(0.015)	(0.015)	(0.014)
Cereals	0.274	0.221	-0.155	-0.014	-0.013	-0.02
	(2.303)	(2.342)	(2.501)	(0.040)	(0.039)	(0.038)
Fruit and vegetables	2.978	2.86	3.275	-0.03	-0.031	-0.03
	(4.558)	(4.485)	(4.340)	(0.041)	(0.041)	(0.040)
Pulses	0.318	0.313	0.316	0.00	0.00	0.00
	(0.510)	(0.506)	(0.480)	(0.006)	(0.006)	(0.006)
Fats and oils	-0.281	-0.289	-0.372	-0.014	-0.014	-0.016
	(0.609)	(0.627)	(0.697)	(0.012)	(0.012)	(0.012)
Sugar and sweets	-1.25	-1.262	-1.353	-0.029*	-0.029*	-0.030*
	(0.931)	(0.951)	(0.905)	(0.016)	(0.017)	(0.016)
Miscellaneous food	-0.391	-0.419	-0.064	-0.062	-0.061	-0.059
	(6.529)	(6.462)	(5.449)	(0.062)	(0.063)	(0.063)
Quadratic term	N	Y	Y	N	Y	Y
Covariates	N	N	Y	N	N	Y

Source: Author's calculations from ESSHO 2011 dataset.
Notes: Sample limited to household with a Selben II score of ± 5 around the cut-off. Robust standard errors clustered at the province level appear in parentheses. Controls: dummy for urban areas, a set of variables for the head of household (female dummy, indigenous or Afro-Ecuadorian dummy, years of schooling), and the number of individuals in the household.
*10%, **5%, and ***1% significance level.

Additionally, for households with under-5s the BDH programme also generates a reduction in expending on miscellaneous food, which is mainly composed by expending on ready meals outside the house, of about 15 USD per week, or 15 percent point as a fraction of total food expenditure. This effect implies that the BDH generates a change in the pattern of food consumption by reducing the expenditure on ready meals outside the house and substituting this expenditure with an increase in the expenditure on protein-rich products. The finding of this substitution effect allows me to answer the question of why the impact of the BDH on expenditure on protein-rich products, in absolute terms, is larger than the monthly transfer of $35 USD.

Table 4.9 IV estimates of the effect of the BDH on food expenditure components, for groups

	Programme Effects on Food Expenditure (USD)				Programme Effects on Shares (as fraction of total food expenditure)			
	Urban	Rural	Under-5 in H.	No under-5 in H.	Urban	Rural	Under-5 in H.	No under-5 in H.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Proteins (meat, chicken, milk)	13.855***	11.166	18.177**	7.751*	0.186***	0.132	0.237**	0.091*
	(4.566)	(13.195)	(8.409)	(4.445)	(0.050)	(0.161)	(0.093)	(0.048)
Potatoes, yucca and other tubers	0.398	-2.838	-1.089	0.502	-0.012	-0.049	-0.023	-0.017
	(0.737)	(3.401)	(1.179)	(1.244)	(0.016)	(0.035)	(0.028)	(0.014)
Cereals	0.553	-3.548	-1.429	1.449	-0.021	0.031	0.015	-0.031
	(2.713)	(4.529)	(2.653)	(2.974)	(0.039)	(0.120)	(0.040)	(0.048)
Fruit and vegetables	0.428	12.382	-2.504	7.483	-0.044	0.038	-0.033	-0.031
	(2.862)	(17.868)	(4.255)	(7.584)	(0.037)	(0.121)	(0.052)	(0.057)
Pulses	0.091	1.202	0.699	-0.006	-0.003	0.018*	0.011	-0.009
	(0.599)	(1.099)	(0.841)	(0.787)	(0.007)	(0.010)	(0.010)	(0.008)
Fats and oils	-0.435	-0.332	-0.754	-0.018	-0.017	-0.005	-0.013	-0.013
	(0.790)	(1.650)	(0.849)	(1.215)	(0.015)	(0.030)	(0.016)	(0.020)
Sugar and sweets	-1.269	-2.448	-3.450**	1.212	-0.027*	-0.043	-0.047*	0.001
	(0.956)	(3.012)	(1.676)	(0.916)	(0.016)	(0.053)	(0.025)	(0.017)
Miscellaneous food	-1.467	-4.12	-14.888**	9.868	-0.062	-0.121	-0.147**	0.01
	(7.273)	(10.269)	(6.319)	(7.515)	(0.063)	(0.160)	(0.059)	(0.094)
Quadratic term	Y	Y	Y	Y	Y	Y	Y	Y
Household size control	Y	Y	Y	Y	Y	Y	Y	Y
N	265	79	168	176	265	79	168	176

Source: Author's calculations from ESSHO 2011 dataset.

Notes: Sample limited to household with a Selben II score of ± 5 around the cut-off. IV estimates using a second order polynomial of the Selben II index. Control variables: the number of individuals in the household; and for models shown in columns 3,4,7, and 8, I also include a dummy variable for urban areas. Robust standard errors clustered at the province level appear in parentheses.

*10%, **5%, and ***1% significance level.

4.6 Conclusions

CT programmes are a core social policy instrument in Latin America. These programmes were set with the goal of breaking the cycle of poverty by promoting the accumulation of human capital. With regard to their objective of improving the access to food, several papers have shown that CCTs in the region boost the consumption of fruits, vegetables and products rich in proteins. While there is a considerable empirical evidence that suggests that CCTs boost consumption of these products, there is no evidence of the effects of CTs when there is no enforced conditionality attached to them.

This study contributes to the empirical literature on the impact of CTs on consumption by evaluating the effects of an UCT programme implemented in Ecuador, the *Bono de Desarrollo Humano* (BDH). I take advantage of the programme's design and implement a regression discontinuity analysis. Using data around the threshold that is representative of both beneficiary and non-beneficiary households, I estimate the effects of the BDH on total expenditure and its components, focusing on food expenditure.

My findings suggest that households that receive the BDH, with a Selben II score just below the cut-off, spend more on products rich in proteins such as meat, chicken, eggs, etc. compared to households with a Selben II score just above the cut-off that do not get the transfer. This effect takes place only for beneficiary household living in urban areas. The difference is around $14 USD per week and in terms of share as a fraction of total food expenditure the difference is about 19 percentage points (see Table 4.9). These findings are in line with the impact evaluation of other CT programmes with conditionality attached to them (Attanasio and Mesnard 2006; Maluccio and Flores 2005; Hoddinott and Skoufias 2004). However, unlike the results of the effects of these CCT programmes, I do not find a significant effect on expenditure on fruit and vegetables.

There are two pathways, and their interaction, that might explain this increase in expenditure on protein-rich food. First is the increase in the household budget, which in turn increases the capability to buy products as meat, chicken, milk, etc. that otherwise would be difficult to afford. Second, is the change in the intra-household dynamics. As argued by Schady and Rosero (2008), the BDH appears to increase the bargaining power of women within the household and, as a result, the BDH cash transfer is used differently from other sources of income. Considering that the BDH is received by mothers who have children, the increase in

expenditure on more nutritious food may be the result of a pattern of expenditure that better reflects their preferences.

Additionally, the BDH generates a substitution effect by increasing the expenditure on meat, chicken, eggs, etc. and reducing the expenditure on ready meals outside the house, for household with children under 5. This finding reinforces our conclusion that the BDH not only increases consumption but also generates changes in the pattern of consumption that reflect the preferences of the mother in the household.

With regard to the effects of the BDH on the components of total expenditure, the results suggest increases, in absolute terms, in the expenditure on mobile credit, mainly for household with under-5s, and on durable good, mainly for households where there is not an under-5. However, the effects in terms of shares as a fraction of total expenditure for these items are not significantly different from zero, at conventional levels. Additionally, in line with most of the literature, I do not find any effect of the BDH on expenditure on alcohol and tobacco (see Evans and Popova 2014, for a review of the literature).

The BDH increases expenditure on school related items and services (books and notebooks, school uniforms, transport to school, tuition fees, etc.), both in absolute terms and as a fraction of total expenditure, for beneficiary families living in urban areas. This increase and the increase in expenditure on products rich in proteins go in line with the objectives of the programme. Based on these results, one can conclude that the programme is achieving its objectives of increasing the access to more and better-quality food and facilitating the access to education for school-aged children.

From a policy perspective, the fact that the BDH increases the consumption of protein-rich food for urban household with children under 5 highlights the importance that this policy has on reducing child malnourishment in the country. However, the fact that there is not a significant effect for households in rural areas should call the attention of policy makers. It can be the case that households in rural areas, which are more likely to produce their own food, are less prone to change their patter of food consumption. The reason may be the lack of access to other food products, apart from the ones they produce, and to information regarding good nutrition.

The fact that the BDH has no effect on expenditure on fruits and vegetables, in both urban and rural areas, unlike other CCT programmes in the region, should also call the attention of policy makers. One possible alternative to incentive the consumption of fruits and vegetables is to inform beneficiary families about the benefits of their intake. A channel to do so could be through media campaigns, as it was implemented when launching the program.

Finally, an important limitation of this study is that the results show the local average treatment effect and the conclusions, therefore, apply only for beneficiary households with a Selben II index located below the cut-off around the threshold. The findings cannot be generalised to all BDH recipients and it is possible that the effects of the programme are different for households in the poorest quintile of the Selben II index.

4.7 Appendix

Table 4.10 Intention to treatment (ITT) estimates of the effect of the BDH on total expenditure and its components

	Programme Effects on Expenditure (USD)			Programme Effects on Shares (as fraction of total expenditure)		
	(1)	(2)	(3)			
Total expenditure	37.573**	37.438**	38.940**			
	(17.261)	(16.850)	(13.959)			
Food expenditure	8.343*	8.147*	6.761	0.009	0.008	0
	(4.524)	(4.437)	(4.590)	(0.037)	(0.037)	(0.039)
Food expenditure (outside the house)	-0.573	-0.582	-0.174	-0.025*	-0.025*	-0.021
	(3.530)	(3.498)	(3.326)	(0.014)	(0.014)	(0.015)
Alcohol and tobacco	0.874	0.862	1.246	-0.001	-0.001	0.003
	(1.628)	(1.647)	(1.830)	(0.011)	(0.011)	(0.012)
Mobile credit	1.215**	1.228**	1.307**	0.002	0.002	0.003
	(0.568)	(0.582)	(0.511)	(0.003)	(0.003)	(0.003)
Household services	-1.004	-0.998	-0.642	-0.012	-0.012	-0.01
	(2.091)	(2.104)	(2.082)	(0.010)	(0.010)	(0.012)
Durables	22.683**	22.794**	25.895**	0.022	0.022	0.031
	(9.376)	(9.377)	(9.426)	(0.019)	(0.019)	(0.023)
School-related for under-5s	0.281	0.278	0.515	0.001	0.001	0.003
	(0.349)	(0.355)	(0.418)	(0.004)	(0.004)	(0.005)
School-related for children >5 years old	2.794	2.76	1.429	0.01	0.01	-0.001
	(2.228)	(2.219)	(1.804)	(0.017)	(0.017)	(0.014)
Other services	2.96	2.948	2.601	-0.006	-0.006	-0.007
	(2.198)	(2.205)	(1.520)	(0.010)	(0.010)	(0.009)
Quadratic term	N	Y	Y	N	Y	Y
Covariates	N	N	Y	N	N	Y

Source: Author's calculations from ESSHO 2011 dataset.

Notes: Sample limited to household with a Selben II score of ± 5 around the cut-off. Robust standard errors clustered at the province level appear in parentheses. Controls: dummy for urban areas, a set of variables for the head of household (female dummy, indigenous or Afro-Ecuadorian dummy, years of schooling), and the number of individuals in the household.

*10%, **5%, and ***1% significance level.

Table 4.11 Intention to treatment (ITT) estimates of the effect of the BDH on the components of food expenditure

	Programme Effects on Food Expenditure (USD)			Programme Effects on Food Shares		
	(1)	(2)	(3)	(4)	(5)	(6)
Proteins (meat, chicken, milk)	6.905***	6.826***	6.883***	0.084***	0.083***	0.088***
	(2.424)	(2.350)	(2.298)	(0.028)	(0.028)	(0.027)
Potatoes, yucca and other tubers	0.023	0.014	-0.296	-0.009	-0.009	-0.012
	(0.429)	(0.424)	(0.422)	(0.008)	(0.008)	(0.008)
Cereals	0.14	0.112	-0.658	-0.007	-0.007	-0.015
	(1.211)	(1.230)	(1.349)	(0.021)	(0.020)	(0.020)
Fruit and vegetables	1.521	1.457	1.245	-0.015	-0.016	-0.017
	(2.350)	(2.313)	(2.279)	(0.021)	(0.021)	(0.022)
Pulses	0.162	0.159	0.109	0	0	0
	(0.253)	(0.250)	(0.228)	(0.003)	(0.003)	(0.003)
Fats and oils	-0.143	-0.147	-0.223	-0.007	-0.007	-0.008
	(0.308)	(0.315)	(0.420)	(0.006)	(0.006)	(0.006)
Sugar and sweets	-0.638	-0.643	-0.668	-0.015*	-0.015*	-0.014*
	(0.466)	(0.474)	(0.467)	(0.008)	(0.008)	(0.008)
Miscellaneous food	-0.2	-0.213	0.195	-0.032	-0.031	-0.022
	(3.443)	(3.408)	(3.137)	(0.035)	(0.035)	(0.039)
Quadratic term	N	Y	Y	N	Y	Y
Covariates	N	N	Y	N	N	Y

Source: Author's calculations from ESSHO 2011 dataset.

Notes: Sample limited to household with a Selben II score of ± 5 around the cut-off. Robust standard errors clustered at the province level appear in parentheses. Controls: dummy for urban areas, a set of variables for the head of household (female dummy, indigenous or Afro-Ecuadorian dummy, years of schooling), and the number of individuals in the household.

*10%, **5%, and ***1% significance level.

Table 4.12 Non-parametric fuzzy RDD estimates of the effect of the BDH on total expenditure and its components

	Programme Effects on Expenditure (USD)			Programme Effects on Shares (as fraction of total expenditure)		
	(1)	(2)	(3)			
Total expenditure	82.805*	87.353*	93.914*			
	(45.703)	(51.218)	(52.065)			
Food expenditure	18.476	21.502	22.838	0.016	0.017	0.007
	(12.761)	(14.145)	(14.514)	(0.069)	(0.079)	(0.082)
Food expenditure (outside the house)	-1.494	-0.403	2.739	-0.033	-0.053	-0.046
	(7.573)	(8.308)	(7.482)	(0.030)	(0.038)	(0.038)
Alcohol and tobacco	1.233	1.21	1.245	-0.007	-0.002	0.001
	(2.413)	(2.926)	(2.858)	(0.014)	(0.018)	(0.018)
Mobile credit	2.491*	2.752*	2.817**	0.007	0.006	0.006
	(1.281)	(1.424)	(1.383)	(0.006)	(0.007)	(0.007)
Household services	-1.295	-2.519	-2.423	-0.038*	-0.03	-0.031
	(3.093)	(4.501)	(4.488)	(0.021)	(0.032)	(0.031)
Durables	50.045*	52.908*	55.489*	0.052	0.045	0.049
	(26.431)	(30.151)	(31.746)	(0.066)	(0.078)	(0.081)
School-related for under-5s	0.692	0.647	0.57	0.005	0.005	0.006
	(0.811)	(0.983)	(1.021)	(0.008)	(0.009)	(0.009)
School-related for children >5 years old	4.438	6.608	6.318	-0.002	0.01	0.011
	(3.970)	(5.195)	(5.350)	(0.024)	(0.032)	(0.035)
Other services	4.989	3.801	4.103	-0.017	-0.014	-0.015
	(4.162)	(4.873)	(4.993)	(0.020)	(0.026)	(0.026)
Quadratic term	N	Y	Y	N	Y	Y
Covariates	N	N	Y	N	N	Y

Note: The table shows robust estimates, and robust standard errors in parentheses. The number of observations varies for all expenditure models and specifications, since the bandwidths are data-driven and selected based on the MSE-optimal bandwidth method (Calonico et al. 2017). The bandwidths range from 6.2 to 15.2. A triangular kernel was used for all cases.

*10%, **5%, and ***1% significance level.

Table 4.13 Non-parametric fuzzy RDD estimates of the effect of the BDH on food expenditure components

	Programme Effects on Food Expenditure (USD)			Programme Effects on Shares (as fraction of total food expenditure)		
	(1)	(2)	(3)	(4)	(5)	(6)
Proteins (meat, chicken, milk)	13.014**	14.684**	15.223***	0.149**	0.183**	0.192**
	(5.641)	(6.234)	(5.881)	(0.060)	(0.077)	(0.075)
Potatoes, yucca and other tubers	0.267	0.211	0.02	-0.014	-0.025	-0.028
	(0.950)	(1.116)	(1.273)	(0.015)	(0.024)	(0.025)
Cereals	0.672	0.487	0.509	-0.023	-0.032	-0.035
	(3.047)	(3.434)	(3.408)	(0.034)	(0.049)	(0.048)
Fruit and vegetables	3.736	5.718	5.799	-0.012	-0.006	-0.008
	(3.807)	(5.054)	(5.232)	(0.041)	(0.056)	(0.056)
Pulses	0.254	0.681	0.602	-0.001	0.004	0.002
	(0.403)	(0.529)	(0.507)	(0.004)	(0.006)	(0.006)
Fats and oils	-0.313	-0.312	-0.372	-0.016*	-0.018	-0.021*
	(0.637)	(0.715)	(0.722)	(0.010)	(0.011)	(0.012)
Sugar and sweets	-1.485	-1.652	-1.619	-0.033*	-0.040**	-0.041*
	(1.153)	(1.304)	(1.273)	(0.017)	(0.020)	(0.021)
Miscellaneous food	-0.532	0.809	2.102	-0.029	-0.062	-0.064
	(7.902)	(8.606)	(8.207)	(0.057)	(0.079)	(0.084)
Quadratic term	N	Y	Y	N	Y	Y
Covariates	N	N	Y	N	N	Y

Note: (1), (2) and (3) present non-parametric estimates of the effect of BDH on food expenditure and its components. The table shows robust estimates, and robust standard errors in parentheses. The number of observations varies for all expenditure models and specifications since the bandwidths are data-driven and selected based on the MSE-optimal bandwidth method (Calonico et al. 2017). The bandwidths range from 6.0 to 14.0. A triangular kernel was used for all cases.
*10%, **5%, and ***1% significance level.

5 Conclusions and Discussion

5.1 Summary of Findings and Link with Theory

The objective of this thesis has been to analyse the impact of two major forms of social protection in the region: minimum wage policies and cash transfer programmes. The experience of the Ecuadorian national minimum wage (NMW) policy and the cash transfer (CT) programme, the *Bono de Desarrollo Humano* (BDH), show the critical role of accounting for the intensity of enforcement when evaluating the effects of these policies.

In the case of the NMW, I evaluated the effects of this policy when there are increases in its two main components: the level of the statutory minimum wage and the intensity of its enforcement. In a region where noncompliance is widespread, the results of this thesis can encompass lessons for similar proposals elsewhere. The case of BDH is also relevant because unlike other programmes in the region it does not have an enforced conditionality attached to it.

In Part I of the thesis, I analysed the effects of the changes in the two main components of the NMW of Ecuador on wages, employment, and monetary poverty (Chapter 2) and on wage inequality (Chapter 3). Theoretically, one can expect that these changes will increase wages of low-paid workers whose employers start to comply with the labour law; additionally, the increases in the level of the minimum will also increase wages of workers whose employers already comply. These increases in wages will compress the wage distribution. In terms of employment, in a competitive market, employers will lay off less-skilled workers whose productivity (marginal revenue product) is less than the new enforced minimum wage.

The findings in Chapters 2 and 3 provide evidence that confirms the theoretical predictions. Specifically, in Chapter 2, I found that the increases in the two components of the NMW law increased wages of male covered workers and reduced the probability of remaining employed in the covered sector for the less-skilled workers, the labourers. Additionally, the increase in wages for male covered workers -both salaried and labourers-, who remained employed in the covered sector, reduced the probability of being poor for this group of workers and their families.

In the analysis of employment transitions, I found that most of the labourers who lost their jobs in the covered sector migrated to the uncovered sector and others became unemployed. This finding goes in line with the predictions of competitive dual-sector models for developing countries, originally proposed by Harris and Todaro (1970), where labour

markets are segmented into covered (formal) and uncovered (informal) sectors. Additionally, in line with dual-sector models, I found that the increase in labour supply in the uncovered sector reduced the earnings of uncovered self-employed individuals, who were earning at the NMW.

Unlike the case of male covered workers, the results for female covered workers, in Chapter 2, suggest that the changes in the two components of the NMW law did not have an effect on their wages and employment status. The only exception is the group of female domestic workers for whom I found a significant increase in wages and for whom the government implemented a special enforcement campaign.

In terms of wage compression, in Chapter 3, I found that the increases in the two components of the NMW law reduced wage inequality by increasing the wages of workers located up through the 60th percentile of the wage distribution. Additionally, in my counterfactual analysis, I estimated that the increase in the real value of the NMW, throughout the period 2000-2016, and the increase in the intensity of enforcement of the NMW, since 2011, account for approximately 50% of the reduction in wage inequality we observe for the period 2000-2016.

In Part II, Chapter 4 of the thesis, I analysed the effect of the unconditional cash transfer programme of Ecuador, the BDH, on the components of food expenditure. Previous evidence of the impact of the BDH on food expenditure report that the programme increases the food share (as a fraction of total expenditure, Schady and Rosero 2008) and the total amount of expenditure on food (in absolute values, Buser et al. 2017). However, these studies do not analyse the effects of the BDH on the components of food expenditure. The evidence for other CTs implemented in the region shows that the transfer increases consumption of fruits and vegetables, and of items rich in protein such as meat, chicken, milk, eggs, etc. (see Hoddinott and Skoufias 2004, for *Oportunidades* in Mexico; Maluccio and Flores 2005 for *Red de Protección Social* (RPS) in Nicaragua; Attanasio and Mesnard 2006, for *Familias en Accion* in Colombia; Martins and Monteiro 2016, for *Bolsa Família* in Brazil).

In theory, we will expect that CTs will allow beneficiary families to purchase more food and other items and services, since the transfer increases poor household's disposable income. If we consider that in most of the cases the transfer is received by the mother of the household, then we can expect that the transfer will also alter the intra-household dynamics of bargaining power. Mothers are paid the transfer with the main objective of investing in human capital of their children. In order to achieve this objective, they will spend on more and better-quality food to improve the nutrition of their children. In this scenario, CT programmes will

also affect the pattern of food expenditure by reflecting better the preferences of the mother in the household and by going in line with the objective of the program of enhancing child nutritional status.

The findings in Chapter 4 go in line with these predictions, and with the empirical evidence of the effects of CCTs for other countries in the region. I found that the BDH increased food expenditure on protein-rich products such as meat, chicken, milk, eggs, etc. Additionally, I found that beneficiary families with under-5s spent significantly more on this kind of products and significantly less on sugar and sweets, and on meals outside the house. However, unlike other CCT programmes in the region, I did not find that the BDH increased expenditure on fruits and vegetables.

5.2 Limitations and Further Research

The findings of this thesis are not without important limitations. In this section, I discuss those that I consider imperative and suggest directions for further research.

In the empirical analysis of Chapter 2 the main limitation is that my identification strategy relies in two separate panel data sets of workers. One panel for the period before the increase in enforcement of the NMW law and one panel during the campaign. With this data, I do not follow the same worker before, and during the campaign period. Instead, I observe one group of workers twice before the campaign, and another group of workers twice during the campaign. My results rely on the assumption that workers in both of my panel data sets are comparable. It is not a big assumption since both samples are drawn from the same master sample.

Future research can omit this assumption if authors have access to a panel data set that follows the same workers before, during, and after the enforcement campaign. In the case of Ecuador, to the best of my knowledge, there is not such a dataset. On other countries, it can be feasible to plan the data collection process ahead of the changes in the minimum wage law. A panel data set that follows the same worker will provide more detailed evidence of the effects of the changes in the minimum wage policy, especially when analysing the dynamics of employment transitions.

Other limitation in Chapter 2, is that when I analyse the effects of the NMW law on poverty, I focused on monetary poverty. Further studies should examine the impact of the changes in the NMW policy in other measures of poverty that include other dimensions of people's experience of deprivation. With appropriate data, the analysis on multidimensional

poverty can also directly capture the effects that the changes in the minimum wage generate on dimensions such as health, education, empowerment, household adequacy and others. The data available for Ecuador does not allow me to implement such analysis.

Data that allows to compute multidimensional poverty indexes, such as surveys on living conditions, is not as periodically collected as surveys on employment. These surveys are also less likely to follow the same individual over time. This implies that if researches want to analyse the effects of changes in the minimum wage policy on multidimensional poverty indexes, they will have to plan the collection process of the data ahead to achieve this objective.

In Chapter 3, I showed that the NMW policy reduces wage inequality by increasing the wages of low-paid covered workers. However, by restricting the analysis to covered workers with nonzero (or negative) wages, I am losing some information regarding the number of people who lost their jobs or migrated to the uncovered sectors and, as a consequence, report zero or missing in their wage. Further analysis can include the number of zeros generated due to job losses to account for the potential adverse effects of the NMW law. This further research can also analyse the effect of the NMW policy on the distribution of labour earnings, including workers in all sectors of the economy (covered sector, uncovered self-employment, business owners, public sector, unpaid family workers). The data used in Chapter 3 will be enough to implement this analysis.

In Chapter 4, I showed that the unconditional cash transfer programme of Ecuador, the BDH, increased the consumption of protein-rich products such as meat, chicken, eggs, etc. The central assumption in order to conclude that this effect might improve the quantity and the quality of food intake for children within the house is that the resources are shared in a child-sensitive way. Further research can overtake this limitation and collect data at the individual level to quantify the amount of food that children are actually eating. The analysis can also include the conversion of food consumption into per capita daily energy (in calories). Using information at the individual level, this analysis will provide more detailed information regarding food intake for children. The implications for the data collection process, however, are more demanding since interviewers will need to collect the information on food consumption for each member of the household.

Other limitation is that the analysis in Chapter 4 does not allow me to identify how much of the effect on the pattern of food consumption derives from the social marketing campaign around the BDH, how much derives from the fact that the BDH is transferred to women, and how much responds to the increase in household's income per se. This can be a topic for future research. Answering this question is relevant since one can be tented to

conclude that a cash transfer with nonenforced conditions will generate the same results as a CCT programme that spend a significant amount of time and economic resources to promote and impose compliance with the conditions. It is necessary to identify what mechanism is driving more of the change in the pattern of food consumption generated by the BDH.

Finally, as I mentioned in Chapter 4, my fuzzy regression discontinuity approach provides an estimate of the local average treatment effect (ATE) for compliers at the threshold. This means that the results of this study cannot be generalized and, more importantly, it implies that the beneficiary households included in the analysis are the well-off between the poor. The results presented in this chapter should be interpreted in light of these limitations.

5.3 Policy Implications and Recommendations

Based on the results of Chapters 2 and 3, I can conclude that a well enforced minimum wage serves as a policy tool to increase wages of low-paid salaried workers. This policy also helps to reduce monetary poverty and wage inequality. The policy implications of these findings are important. They suggest that in countries with high noncompliance of the labour regulation it may be more relevant for policy makers to spend their efforts on increasing the intensity of enforcement of the existing minimum wage laws rather than only focusing on increasing the level of the minimum.

However, this policy also reduces employment in the covered sector. Covered workers who lost their jobs as a consequence of the increased enforcement of the NMW law are the ones who pay for the implementation of this policy. A well-designed contributory social security system will prevent them, and their families, from falling into poverty by providing access to an effective unemployment insurance. In Ecuador, as in many countries in the region, the labour law states this *de jure* social protection, but it is usually unclaimed.

In the case of Ecuador, the workers who lost their jobs are mainly labourers. These workers have low education and do manual work mostly in the agriculture and construction industries. Other way to help these workers is to implement training programmes to increase their productivity. These programmes can be on the form of on-the-job training to prevent them from losing their jobs, or training programmes directly implemented by the government for those who already lost their jobs.

Policy makers who want to use the minimum wage law as a social policy tool should consider that a higher and well-enforced minimum wage might be socially desirable, but it has a cost. Although they may value redistribution towards low-paid workers more than labour

market efficiency, the migration of low-skilled covered workers to (uncovered) self-employment or to unemployment is a potential involuntary outcome that they also need to consider. These workers and their families, if not protected, can become poor.

In developing countries there is also a high proportion of individuals that are not covered by the contributory social security system. These individuals can be categorized into three groups: 1) self-employed workers, 2) economically inactive individuals, and 3) unremunerated workers. Non-contributory social assistance programmes such as cash transfers are the main protection schemes for families that depend on an economically inactive individual, or on an unremunerated worker. These individuals do not have earned income. This fact makes it difficult to think in a sustainable mechanism to include them in the contributory system. In the case of the Ecuadorian CT programme, the fraction of beneficiary families with an economically inactive head of household is around 22 percent, and the fraction beneficiary households with an unpaid household head is less than 1 percent (see Table 1.3 in Chapter 1). In the absence of a non-contributory protection scheme as the BDH these families will not have access to a social protection system.

On the other hand, self-employed workers (including farmers) and their families can be covered by contributory or non-contributory social protection. In the region, poor households that depend on a self-employed worker are covered by CT programmes. In the cases of countries as Colombia, Ecuador, Guatemala, Mexico, and Panama, around half of beneficiary families have a head of household that is a self-employed worker or farmer (see Table 1.3 in Chapter 1). In countries as Peru and Paraguay this fraction is even higher.

It is also feasible to think that self-employed workers, and their families, can be covered by contributory social security systems. However, there are at least three factors that need to be considered in advance. First, self-employed workers do not have an employer, and this implies that these workers have to finance their affiliation to the social security systems all by themselves. This represents a barrier for extending the coverage to independent workers, especially for unskilled self-employed workers who have irregular earned income and limited payment capacity.

Second, self-employed workers usually have volatile employment conditions. These workers jump between working as independent workers to be salaried workers, they can also leave the labour force and enter again after a period of time. This unstable employment status will generate interruptions to their contributions and this can cut their rights to the benefits of the contributory system. Third, the high level of heterogeneity among independent workers makes it difficult to design a simple incorporation system. This heterogeneous group includes

independent professionals, unipersonal businesses struggling for economic sustainability, and low-skilled self-employed workers with limited access to information regarding the benefits of contributing to the social security systems. These three factors illustrate that there is not a simple solution to the extension of the contributory system to self-employed individuals.

Finally, an informal worker is typically defined as one that does not contribute to the social security system (see Hussmanns 2004, for a review of definitions of informality proposed by the ILO). Enforcing the affiliation of salaried workers to the social security and extending the coverage to self-employed workers is an option to reduce labour informality in the region. The increase in compliance with the affiliation of salaried workers to the social security system, and the possible expansion of coverage to self-employed workers, not only encourages labour formality but can also reduce the burden assigned to CT programmes by securing a pension for these workers when they reach retirement age.

The results in Chapter 4 suggest that the BDH, as other CT programmes in the region, serves as a protection tool for beneficiary families by helping them to increase the expenditure on products rich in proteins (chicken, meat, milk, eggs, etc.) and on school related items. The increase in expenditure on protein-rich products not only comes from the increase in household disposable income due to the transfer, but also derivates from a change in food expenditure patterns. BDH recipients, in urban households with children under 5, spend significantly less on sugar and sweets and on ready meals outside the house and significantly more on protein-rich products.

The change in the pattern of food consumption, generated by the BDH, aligns to a behaviour that will benefit the nutritional status of children, if resources are shared in a child-sensitive way. This result should not be minimized because it is a powerful proof that CTs can be used as a social policy tool to reduce child malnourishment. In the case of Ecuador, this fact is of paramount importance since a fourth of under-5s suffer from chronic malnourishment and this rate has stagnated during the last decade (SIISE 2015).

Additionally, policy makers should consider that CTs given to the mother of the beneficiary families serves as a protection tool not only by providing additional resources to a poor household, but also by increasing the bargaining power of women within the household. This change in the intra household dynamics can potentially benefit children, since mothers are usually the ones in charge of taking care of them. However, policy makers should not forget that assigning this role to women can decrease their participation in the labour market, making them more dependent on the monthly transfer. As highlighted by Mideros and O'Donoghue (2015), for households that received the BDH transfer, childcare and gender equity policies

can be used as complementary policy tools if paid-labour participation is to be promoted among the mothers of beneficiary households.

In the case of the BDH, the changes in the pattern of food expenditure are even more relevant for policy makers since this programme was implemented as an unconditional CT programme. One may be tented to suggest that UCTs serve as a protection tool, in terms of their effects on food consumption, in the same way as CCTs do. However, the BDH does not generate a significant effect on expenditure on fruits and vegetables, unlike other CCTs in the region. This fact suggests that the programme administrators need to further promote the consumption of this kind of products with initiatives such as nutrition educational workshops, or media campaigns.

Finally, the main objective of CTs is to ensure that children of beneficiary households achieve a sufficient level of human capital. This would allow them to break the intergenerational transmission of poverty and to remove the need for non-contributory social assistance to the next generation. The change in the pattern of food consumption towards expending more on protein rich products has the potential to minimize the problem on protein–calorie malnutrition, and more specifically to combat the deficiency in the intake of iron, iodine and vitamin A. This is particularly important for young children in developing countries as there is consistent evidence that protein consumption has lasting beneficial effects on their cognitive development; especially when it takes place during the first 2 years of life, the period during which the brain is rapidly growing (Benton 2010).

While there is sufficient evidence that CTs increase school attendance, the fact that there is not a conclusive link between CTs and learning outcomes of school-aged children should call the attention of policy makers. One factor that can partially explain this issue is the fact that if there was a protein/calorie malnutrition when these children where under 2, it will be difficult to have a positive effect on school performance. Malnutrition in the first 2 years of life has a lasting impact on both behaviour and cognition (Benton 2010). Consequently, policy makers should put attention on improving the nutritional intake of children in their earlier life as this will have a lasting positive impact on their capacity of accumulating human capital in terms of cognitive achievement later in life.

References

Acemoglu, Daron, and James A. Robinson. 2012. *Why Nations Fail: The Origins of Power, Prosperity, and Poverty*. London: Profile.

Alaimo, Verónica, Mariano Bosch, David S. Kaplan, Carmen Pagés, and Laura Ripani. 2015. *Jobs for Growth*. Inter-American Development Bank.

Alaniz, Enrique, T. H. Gindling, and Katherine Terrell. 2011. "The Impact of Minimum Wages on Wages, Work and Poverty in Nicaragua." *Labour Economics* 18, Supplement 1 (December): S45–59.

Almeida, Rita, and Pedro Carneiro. 2009. "Enforcement of Labor Regulation and Firm Size." *Journal of Comparative Economics* 37 (1): 28–46.

———. 2012. "Enforcement of Labor Regulation and Informality." *American Economic Journal: Applied Economics* 4 (3): 64–89.

Angelucci, Manuela, Orazio Attanasio, and Vincenzo Di Maro. 2012. "The Impact of Oportunidades on Consumption, Savings and Transfers." *Fiscal Studies* 33 (3): 305–34.

Angrist, Joshua D., and Victor Lavy. 1999. "Using Maimonides' Rule to Estimate the Effect of Class Size on Scholastic Achievement." *The Quarterly Journal of Economics* 114 (2): 533–75.

Araujo, Maria, Mariano Bosch, and Norbert Schady. 2017. "Can Cash Transfers Help Households Escape an Inter-Generational Poverty Trap?" IDB Working Paper Series No. 767.

Araujo, Maria, and Norbert Schady. 2006. "Cash Transfers, Conditions, School Enrollment, and Child Work: Evidence from a Randomized Experiment in Ecuador." World Bank Policy Research Working Paper No. 3930.

Attanasio, Orazio, and Alice Mesnard. 2006. "The Impact of a Conditional Cash Transfer Programme on Consumption in Colombia." *Fiscal Studies* 27 (4): 421–442.

Auerbach, Paula, Maria Eugenia Genoni, and Carmen Pages. 2007. "Social Security Coverage and the Labor Market in Developing Countries." IZA Discussion Paper No. 2979.

Autor, David H., Alan Manning, and Christopher L. Smith. 2016. "The Contribution of the Minimum Wage to US Wage Inequality over Three Decades: A Reassessment." *American Economic Journal: Applied Economics* 8 (1): 58–99.

Baird, Sarah, Francisco HG Ferreira, Berk Özler, and Michael Woolcock. 2013. "Relative Effectiveness of Conditional and Unconditional Cash Transfers for Schooling Outcomes in Developing Countries: A Systematic Review." *Campbell Systematic Reviews* 9 (8).

Bastagli, Francesca, Jessica Hagen-Zanker, Luke Harman, Valentina Barca, Georgina Sturge, Tanja Schmidt, and Lucca Pellerano. 2016. "Cash Transfers: What Does the Evidence Say." Technical report, Overseas Development Institute.

Basu, Arnab K., Nancy H. Chau, and Ravi Kanbur. 2010. "Turning a Blind Eye: Costly Enforcement, Credible Commitment and Minimum Wage Laws." *The Economic Journal* 120 (543): 244–69.

Battistin, Erich, Agar Brugiavini, Enrico Rettore, and Guglielmo Weber. 2009. "The Retirement Consumption Puzzle: Evidence from a Regression Discontinuity Approach." *The American Economic Review* 99 (5): 2209–26.

Benton, David. 2010. "The Influence of Dietary Status on the Cognitive Performance of Children." *Molecular Nutrition & Food Research* 54 (4): 457–70.

Bird, Kelly, and Chris Manning. 2008. "Minimum Wages and Poverty in a Developing Country: Simulations from Indonesia's Household Survey." *World Development* 36 (5): 916–33.

Borjas, George J. 1980. "The Relationship between Wages and Weekly Hours of Work: The Role of Division Bias." *The Journal of Human Resources* 15 (3): 409–23.

Borraz, Fernando, and Nicolas Gonzalez Pampillon. 2011. "Assessing the Distributive Impact of More than Doubling the Minimum Wage: The Case of Uruguay." SSRN Scholarly Paper ID 2213031. Rochester, NY: Social Science Research Network.

Bosch, Mariano, and Marco Manacorda. 2010. "Minimum Wages and Earnings Inequality in Urban Mexico." *American Economic Journal: Applied Economics* 2 (4): 128–49.

Bourguignon, François, Martin Browning, Pierre-André Chiappori, and Valérie Lechene. 1993. "Intra Household Allocation of Consumption: A Model and Some Evidence from French Data." *Annales d'Économie et de Statistique*, no. 29: 137–56.

Buser, Thomas, Hessel Oosterbeek, Erik Plug, Juan Ponce, and José Rosero. 2017. "The Impact of Positive and Negative Income Changes on the Height and Weight of Young Children." *The World Bank Economic Review* 31 (3): 786–808.

Calonico, Sebastian, Matias D. Cattaneo, and Max H. Farrell. 2017. "On the Effect of Bias Estimation on Coverage Accuracy in Nonparametric Inference." *Journal of the American Statistical Association* 0 (0): 1–13.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik. 2014a. "Robust Data-Driven Inference in the Regression-Discontinuity Design." *Stata Journal* 14 (4): 909–946.

———. 2014b. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica* 82 (6): 2295–2326.

Canelas, Carla. 2014. "Minimum Wage and Informality in Ecuador." UNU-WIDER Working Paper 6. UNU-WIDER.

Card, David, and Alan B. Krueger. 1994a. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania." *The American Economic Review* 84 (4): 772–93.

———. 1994b. "A Living Wage? The Effects of the Minimum Wage on the Distribution of Wages, the Distribution of Family Earnings, and Poverty." 712. Working Papers. Princeton University, Department of Economics, Industrial Relations Section.

Cecchini, Simone, and Bernardo Atuesta. 2017. "Conditional Cash Transfer Programmes in Latin America and the Caribbean: Coverage and Investment Trends." ECLAC – Social Policy Series N° 224 ID 3037640.

Chiappori, Pierre-André. 1988. "Rational Household Labor Supply." *Econometrica* 56 (1): 63–90.

Cornia, Giovanni Andrea. 2014. *Falling Inequality in Latin America: Policy Changes and Lessons*. OUP Oxford.

Cunningham, Wendy V. 2007. *Minimum Wages and Social Policy: Lessons from Developing Countries*. Washington, DC: World Bank.

Currie, Janet, and Bruce C. Fallick. 1996. "The Minimum Wage and the Employment of Youth Evidence from the NLSY." *The Journal of Human Resources* 31 (2): 404–28.

Dickens, Richard, Stephen Machin, and Alan Manning. 1999. "The Effects of Minimum Wages on Employment: Theory and Evidence from Britain." *Journal of Labor Economics* 17 (1): 1–22.

DiNardo, John, Nicole M. Fortin, and Thomas Lemieux. 1996. "Labor Market Institutions and the Distribution of Wages, 1973-1992: A Semiparametric Approach." *Econometrica* 64 (5): 1001–44.

Doss, Cheryl. 2006. "The Effects of Intrahousehold Property Ownership on Expenditure Patterns in Ghana." *Journal of African Economies* 15 (1): 149–80.

ECLAC. 2012. *Social Panorama of Latin America, 2012*. Santiago: United Nations - Economic Commission for Latin America and the Caribbean.

Evans, David, and Anna Popova. 2014. "Cash Transfers and Temptation Goods: A Review of Global Evidence," World Bank Policy Research Working Paper, 6886.

Fabara, Cristina. 2009. "Reformulación Del Índice de Clasificación Socioeconomica Del Registro Social." Ministerio de Coordinación de Desarrollo Social.

Fajnzylber, Pablo. 2001. "Minimum Wage Effects Throughout the Wage Distribution: Evidence from Brazil's Formal and Informal Sectors." CEDEPLAR Working Paper No. 151.

Fernald, Lia C. H., and Melissa Hidrobo. 2011. "Effect of Ecuador's Cash Transfer Program (Bono de Desarrollo Humano) on Child Development in Infants and Toddlers: A Randomized Effectiveness Trial." *Social Science & Medicine* 72 (9): 1437–46.

Fiszbein, Ariel, and Norbert Schady. 2009. *Conditional Cash Transfers: Reducing Present and Future Poverty*. World Bank Publications.

Fortin, Nicole M., and Thomas Lemieux. 2000. "Income Redistribution in Canada: Minimum Wages versus Other Policy Instruments." *Adapting Public Policy to a Labour Market in Transition*, 211–244.

Freeman, Richard B. 1996. "The Minimum Wage as a Redistributive Tool." *The Economic Journal* 106 (436): 639–49.

Gelman, Andrew, and Guido Imbens. 2014. "Why High-Order Polynomials Should Not Be Used in Regression Discontinuity Designs." NBER Working Paper 20405. National Bureau of Economic Research.

Gindling, T. H., Nadwa Mossaad, and Juan Diego Trejos. 2015. "The Consequences of Increased Enforcement of Legal Minimum Wages in a Developing Country An Evaluation of the Impact of the Campaña Nacional de Salarios Mínimos in Costa Rica." *Industrial and Labor Relations Review*, March, 1–42.

Gindling, T. H., and Katherine Terrell. 2010. "Minimum Wages, Globalization, and Poverty in Honduras." *World Development*, Globalization, Poverty, and Inequality in Latin America, 38 (6): 908–18.

Gindling, T. H., and Juan Diego Trejos. 2010. *Reforzar El Cumplimiento de Los Salarios Mínimos En Costa Rica*. FUSADES.

Gindling, T.H., and Katherine Terrell. 2009. "Minimum Wages, Wages and Employment in Various Sectors in Honduras." *Labour Economics* 16 (3): 291–303.

Gramlich, Edward M. 1976. "Impact of Minimum Wages on Other Wages, Employment, and Family Incomes." *Brookings Papers on Economic Activity* 1976 (2): 409–51.

Greif, Avner. 2005. "Commitment, Coercion and Markets: The Nature and Dynamics of Institutions Supporting Exchange." In *Handbook of New Institutional Economics*, 727–88. Rochester, NY: Social Science Research Network.

Groot, Richard de, Tia Palermo, Sudhanshu Handa, Luigi Peter Ragno, and Amber Peterman. 2017. "Cash Transfers and Child Nutrition: Pathways and Impacts." *Development Policy Review* 35 (5): 621–43.

Guzman, Wilson. 2017. "The Effects of Increased Enforcement of the National Minimum Wage Law in a Developing Country: The Case of Ecuador." LSE, Department of Social Policy.

Hahn, Jinyong, Petra Todd, and Wilbert Van der Klaauw. 2001. "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design." *Econometrica* 69 (1): 201–9.

Harris, John R., and Michael P. Todaro. 1970. "Migration, Unemployment and Development: A Two-Sector Analysis." *The American Economic Review* 60 (1): 126–142.

Harrison, Ann, and Jason Scorse. 2010. "Multinationals and Anti-Sweatshop Activism." *American Economic Review* 100 (1): 247–73.

Hoddinott, John, and Lawrence Haddad. 1995. "Does Female Income Share Influence Household Expenditures? Evidence from Côte d'Ivoire." *Oxford Bulletin of Economics and Statistics* 57 (1): 77–96.

Hoddinott, John, and Emmanuel Skoufias. 2004. "The Impact of PROGRESA on Food Consumption." *Economic Development and Cultural Change* 53 (1): 37–61.

Hussmanns, Ralf. 2004. "Defining and Measuring Informal Employment." Bureau of Statistics, ILO.

Imbens, Guido, and Thomas Lemieux. 2008. "Regression Discontinuity Designs: A Guide to Practice." *Journal of Econometrics*, The regression discontinuity design: Theory and applications, 142 (2): 615–35.

Kaitz, H. 1970. "Experience of the Past: The National Minimum, Youth Unemployment and Minimum Wages." In *US Bureau of Labor Statistics Bulletin*, 1657:30–54.

Kaufman, Bruce E. 2010. "Institutional Economics and the Minimum Wage: Broadening the Theoretical and Policy Debate." *ILR Review* 63 (3): 427–53.

Lakner, Christoph, and Branko Milanovic. 2013. "Global Income Distribution : From the Fall of the Berlin Wall to the Great Recession." Policy Research Working Paper Series 6719. The World Bank.

Leckcivilize, Attakrit. 2015. "Does the Minimum Wage Reduce Wage Inequality? Evidence from Thailand." *IZA Journal of Labor & Development* 4 (December): 21.

Lee, David S. 1999. "Wage Inequality in the United States during the 1980s: Rising Dispersion or Falling Minimum Wage?" *The Quarterly Journal of Economics* 114 (3): 977–1023.

Lee, Sangheon, Mariya Aleksynska, Uma Amara Rani, Florence Bonnet, Colin Fenwick, Mark Lansky, Mario Macis, and Paola Monti. 2014. "Labour and Social Protection

Institutions: Recent Trends and Impact on Development." *World of Work Report* 2014 (1): 85–108.

Lemos, Sara. 2009. "Minimum Wage Effects in a Developing Country." *Labour Economics* 16 (2): 224–37.

Lundberg, Shelly J., Robert A. Pollak, and Terence J. Wales. 1997. "Do Husbands and Wives Pool Their Resources? Evidence from the United Kingdom Child Benefit." *The Journal of Human Resources* 32 (3): 463–80.

Machin, Stephen, and Alan Manning. 1994. "The Effects of Minimum Wages on Wage Dispersion and Employment: Evidence from the U.K. Wages Councils." *Industrial and Labor Relations Review* 47 (2): 319–29.

MacIsaac, Donna, and Martin Rama. 1997. "Determinants of Hourly Earnings in Ecuador: The Role of Labor Market Regulations." *Journal of Labor Economics* 15 (3): S136-65.

Maloney, William, and Jairo Nuñez. 2004. "Measuring the Impact of Minimum Wages. Evidence from Latin America." In *Law and Employment: Lessons from Latin America and the Caribbean*, 109–130. University of Chicago Press.

Maluccio, John. 2010. "The Impact of Conditional Cash Transfers on Consumption and Investment in Nicaragua." *The Journal of Development Studies* 46 (1): 14–38.

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

Manley, James, Seth Gitter, and Vanya Slavchevska. 2012. "How Effective Are Cash Transfer Programmes at Improving Nutritional Status? A Rapid Evidence Assessment of Programmes' Effects on Anthropometric Outcomes." EPPI-Centre, Social Science Research Unit, Institute of Education, University of London.

Marinakis, Andrés. 2016. "Non-Compliance with Minimum Wage Laws in Latin America: The Importance of Institutional Factors." *International Labour Review* 155 (1): 133–56.

Marshall, Adriana. 2007. *Explaining Non-Compliance with Labour Legislation in Latin America: A Cross-Country Analysis*. International Labour Organization (ILO).

Martins, Ana Paula Bortoletto, and Carlos Augusto Monteiro. 2016. "Impact of the Bolsa Família Program on Food Availability of Low-Income Brazilian Families: A Quasi Experimental Study." *BMC Public Health* 16 (August).

McCrary, Justin. 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics* 142 (2): 698–714.

Mincer, Jacob. 1976. "Unemployment Effects of Minimum Wages." *Journal of Political Economy* 84 (4): S87–104.

Ministry of Labour. 2011. "Rendición de Cuentas 2011." Quito, Ecuador.

Neumark, David, Mark Schweitzer, and William Wascher. 2004. "Minimum Wage Effects throughout the Wage Distribution." *The Journal of Human Resources* 39 (2): 425–50.

Neumark, David, and William Wascher. 2007. "Minimum Wages and Employment." IZA Discussion Paper No. 2570.

Neumark, David, and William L. Wascher. 2008. *Minimum Wages*. Book, Whole. Cambridge, Mass: MIT Press.

Özler, Berk. 2013. “Defining Conditional Cash Transfer Programs: An Unconditional Mess.” *Development Impact*.

Paxson, Christina, and Norbert Schady. 2010. “Does Money Matter? The Effects of Cash Transfers on Child Development in Rural Ecuador.” *Economic Development and Cultural Change* 59 (1): 187–229.

Phipps, Shelley A., and Peter S. Burton. 1998. “What’s Mine Is Yours? The Influence of Male and Female Incomes on Patterns of Household Expenditure.” *Economica* 65 (260): 599–613.

Ponce, Juan, and Arjun S. Bedi. 2010. “The Impact of a Cash Transfer Program on Cognitive Achievement: The Bono de Desarrollo Humano of Ecuador.” *Economics of Education Review* 29 (1): 116–25.

Ponce, Juan, and Rob Vos. 2014. “Redistribution without Structural Change in Ecuador: Rising and Falling Income Inequality in the 1990s and 2000.” *Falling Inequality in Latin America. Policy Changes and Lesson, Oxford, United Kingdom*.

Rani, Uma, Patrick Belser, Martin Oelz, and Setareh Ranjbar. 2013. “Minimum Wage Coverage and Compliance in Developing Countries.” *International Labour Review* 152 (3–4): 381–410.

Ribe, Helena, David A. Robalino, and Ian Walker. 2010. *Achieving Effective Social Protection for All in Latin America and the Caribbean: From Right to Reality*. World Bank Publications.

Ronconi, Lucas. 2010. “Enforcement and Compliance with Labor Regulations in Argentina.” *Industrial and Labor Relations Review* 63 (4): 719–36.

———. 2015. “Enforcement and the Effective Regulation of Labor.” IDB Working Paper Series No. 622.

Saavedra, Juan Esteban, and Sandra García. 2012. “Impacts of Conditional Cash Transfer Programs on Educational Outcomes in Developing Countries: A Meta-Analysis.” *RAND Labor and Population Working Paper WR-921-1. Santa Monica, Calif.: Rand*.

Schady, Norbert, and José Rosero. 2008. “Are Cash Transfers Made to Women Spent like Other Sources of Income?” *Economics Letters* 101 (3): 246–48.

SIISE. 2015. “Indicadores Sociales Del Ecuador.”

Silverman, Bernard W. 1986. *Density Estimation for Statistics and Data Analysis*. Chapman and Hall/CRC.

Soundararajan, Vidhya. 2014. “Minimum Wage Effects at Different Enforcement Levels: Evidence from Employment Surveys in India.” Working Paper 12. Cornell University, Department of Applied Economics and Management.

Stampini, Marco, and Leopoldo Tornarolli. 2012. “The Growth of Conditional Cash Transfers in Latin America and the Caribbean: Did They Go Too Far?” IZA Working Paper No. 49.

Stewart, Mark B. 2004. “The Impact of the Introduction of the UK Minimum Wage on the Employment Probabilities of Low-Wage Workers.” *Journal of the European Economic Association* 2 (1): 67–97.

Stewart, Mark B., and Joanna K. Swaffield. 2008. “The Other Margin: Do Minimum Wages Cause Working Hours Adjustments for Low-Wage Workers?” *Economica* 75 (297): 148–67.

Stigler, George J. 1946. “The Economics of Minimum Wage Legislation.” *The American Economic Review* 36 (3): 358–65.

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

Thomas, Duncan. 1990. “Intra-Household Resource Allocation: An Inferential Approach.” *The Journal of Human Resources* 25 (4): 635–64.

Torre, Augusto de la, and Rudolph Heinz. 2018. “The Troubled State of Pension Systems in Latin America.” Working Paper 112. Brookings Papers on Global Economy & Development.

Welch, Finis. 1974. “Minimum Wage Legislation in the United States.” *Economic Inquiry* 12 (3): 285–318.

Wong, Sara. 2017. “Minimum Wage Impacts on Wages and Hours Worked of Low-Income Workers in Ecuador.” Working Papers PMMA. PEP-PMMA.

Wong, Sara, Yael Negrete, and Danny Véliz. 2016. “Minimum Wage Policies: Wage, Employment, and Distributional Impacts in Ecuador.” Conference report PEP-PMMA No. 12808. PEP.