Master Degree in Specialized Economic Analysis

Putting Child Labour Laws to Work: Evidence from Colombia

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ABSTRACT IN ENGLISH:

This paper uses household-level survey data from the USAID and the Colombian government to analyse the e˙ect of the Code of Childhood and Adolescence of 2006, which raised the minimum working age from 12 to 15, on the socioeconomic outcomes of the a˙ected cohort. We utilise OLS, probit, di˙erence-in-di˙erence, and triple di˙erence techniques. Our results show statistically significant but marginally impactful changes in outcomes due to the law. Education results are mixed, with an increase in secondary school completion but a decrease in tertiary school enrollment. We o˙er a “Gap Year Hypothesis” to explain this outcome. Rural areas and areas with lower incidences of violence seem to experience more of an e˙ect from the law change. Our results for probability of being employed are inconclusive, but there is a decrease in wages which asymmetrically harms females. The paper includes a variety of policy proposals to decrease incidences of child labour and create more positive social outcomes.

ABSTRACT IN CATALAN:

Aquest article fa servir dades de les enquestes a nivell domiciliari de la USAID i el govern colombià per analitzar el Codi de la infància i l'adolescència de 2006, que va elevar l'edat mínima de treball de 12 a 15 anys, sobre els resultats socioeconòmics de la AH cohort ected. Utilitzem tècniques OLS, probit, diherence-in-tenència i triple indicació. Els nostres resultats mostren canvis estadísticament significatius però impactants en els resultats derivats de la llei. Els resultats de l'educació es combinen, amb un augment de la formació de secundària, però una disminució de la matrícula escolar. Es tracta d'una "hipòtesi de bretxa" per explicar aquest resultat. Les zones rurals i les zones amb menor incidència de violència semblen experimentar més d'un eix del canvi de llei. Els nostres resultats per a la probabilitat de ser emprats no són concloents, però hi ha una disminució dels salaris que perjudiquen asimètricament les femelles. El document inclou una varietat de propostes polítiques per reduir les incidències del treball infantil i crear resultats socials més positius.

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Abstract

This paper uses household-level survey data from the USAID and the Colombian government to analyse the effect of the Code of Childhood and Adolescence of 2006, which raised the minimum working age from 12 to 15, on the socioeconomic outcomes of the affected cohort. We utilise OLS, probit, difference-in-difference, and triple difference techniques. Our results show statistically significant but marginally impactful changes in outcomes due to the law. Education results are mixed, with an increase in secondary school completion but a decrease in tertiary school enrollment. We offer a *"Gap Year Hypothesis"* to explain this outcome. Rural areas and areas with lower incidences of violence seem to experience more of an effect from the law change. Our results for probability of being employed are inconclusive, but there is a decrease in wages which asymmetrically harms females. The paper includes a variety of policy proposals to decrease incidences of child labour and create more positive social outcomes.

Child labour has existed for centuries, but the issue has only come to the forefront as an international topic in recent years. With globalisation, the international community has raised concerns as they become increasingly informed on the extent and reality of child labour. As a result, almost all countries have been attempting to prohibit or regulate child labour to ensure that all children have the opportunity to develop to their full potential physically and mentally (Basu 1999). Several strategies have emerged to strengthen efforts to prevent child labour. These include compulsory schooling, trade sanctions, and mandating labels for products that involve child labour as a deterrent method.

The ILO has set international standards that define 15 as the minimum age required to engage in economic activity. These were officially formulated in Convention No. 138 on the Minimum Age of Employment (C138) in 1973 and Convention No. 182 on the Worst Forms of Child labour (C182) in 1999. At the national level, labour laws and standards have been the primary tools to combat child labour. The international pressure on low and middle income countries for the ratification of ILO conventions to ban child labour has been rising in the past two decades. Studies have shown that laws governing the minimum ages of schooling and employment have had limited success historically (Brown 2002). This paper considers the effectiveness of such a reform and its impact on socioeconomic outcomes in the context of Colombia in 2006. Specifically, this paper looks at the impact of the Code of Childhood and Adolescence of 2006 that raised the minimum working age from 12 to 15, replacing the Minor's Code of 1989.[1] We further decompose our analysis by gender, wealth index, violence index, and type of residence (urban vs. rural) to estimate the most affected subpopulation groups.

Using census survey data covering years before and after the ban, and further applying age restrictions that determined the cohorts affected by the ban, we show that the law has had minimal impacts on the education outcomes for the entire population and negative labour market outcomes for those affected by the law. However, we see that individuals in rural areas, as well as relatively peaceful areas benefit from the law in terms of educational outcomes.

In Section 1, we build the general framework for the analysis of the child labour law in Colombia by means of a literature review, a qualitative analysis of the potential mechanisms through which the child labour reform may work, and a detailed examination of the context in Colombia to form our hypothesis on the impact of the law. In Section 2, we present our methodology, data and the limitations of our analysis. Section 3 presents the empirical results. Section 4 follows with a discussion on the policy implication of our results and a proposition of alternatives. Section 5 concludes.

1 Framework

1.1 Literature Review

As high income countries began to implement child labour bans and compulsory schooling laws at the national level, a body of literature developed on the effectiveness of these policies. A vast number of theoretical models were developed in an attempt to explain the complexities of the economics of child labour. Baland and Robinson (2000) argued that child labour is Pareto inefficient by incorporating a child's future earnings into the model. However when the capital market is imperfect or the household is so poor that bequests are impossible, parents fail to internalise the trade-off between child labour and earning ability, leading to socially inefficient outcomes. They conclude that a ban on child labour will increase Pareto outcomes. Ranjan (1999) counterclaims with an overlapping model in which child labour bans lead to inefficient outcomes as they can reduce the household utility due to the foregone wages. He argues that when the cost of foregone wages is too high, the government should not simply ban child labour but rather fill the gap by relaxing borrowing constraints, directly raising parental income or providing scholarships for children. Many other models have focused on the different causes and consequences of child labour.[2] The implementation of these laws across the world enabled researchers and policymakers to test the validity of these theoretical models. Many studies have looked at the multi-faceted impact of compulsory schooling laws and have led to mixed results, despite being focused on the same countries. A recent study by Clay et al. (2012) finds significant effects on enrollment,

[1] Note that the Code of Childhood and Adolescence was enforced in May 2007. The Minor's Code of 1989 authorised children to work with permission from Work Inspectors or local authorities starting at the age of 12, with some hour and condition restrictions before the age of 14. It also established that labour was to be paid at a rate equivalent to the minimum salary.

[2] Other types of models have also tried to pin down the determinants of child labour. Gupta (2000) highlights the bargaining decision faced by parents who are constrained by credit. Similarly, Basu and Hoang Van (1998) build a model with multiple equilibria, incorporates government interventions and defines leisure of children as a luxury good, which parents cannot afford at low income levels. They use this to show that a ban on child labour can move the economy from a low wage equilibrium with child labour to a high wage one without child labour.

attendance, educational outcomes and wage income in US children from 1880 to 1927. Other studies on these same reforms have found modest effects on educational attainment and economic outcomes when compared to the secular trends (Moehling 1999, Lleras-Muney 2001, Goldin and Katz 2003). In fact, they find that, at best, only 5 percent of the increase in high school enrollments can be attributed to the reform for the affected cohorts. These authors claim that reforms are implemented as a result of the trends in better educational outcomes, rather than the other way around. Other studies have focused on health outcomes, although there is relatively less literature on this outcome. Bellés-Obrero et al. (2015) find that the implementation of a minimum age requirement for work in Spain decreased marriage and fertility. However, it also had detrimental effects on the health of the offsprings at the moment of delivery.[3]

Following the international pressure that has built up in the past two decades, there has been further research done on similar policies for middle and low income countries. Indeed, policies may not have the same results due to the inherent contextual differences in terms of politics, economics and society as a whole. Krueger (1996) finds a very strong negative correlation between child labour force participation and per capita GDP, as child labour practices clearly depend on the level of economic development. For many families, the income earned by their children is necessary for the family's survival. Edmonds (2005) brings further insight on the economics of child labour as he investigates the relationship with improvements in per capita expenditure. He finds that improvements in per capita expenditure can explain 80 percent of the decline in child labour that occurs in households whose expenditures improve enough to move out of poverty. Jensen and Nielsen (1997) and Bharadwaj et. al. (2013) both confirm this finding with evidence from Zambia and India, respectively. They argue that there is very little evidence to validate the effectiveness of bans against child labour, despite the fact that they are a common policy tool. In fact, Bharadwaj et. al. find that child wages decrease and child labour increases (i.e reduced school enrollment) after the ban, consistent with some theoretical models.

Another set of literature that focuses on the determinants of child labour. As a baseline, there is ample evidence to suggest that child labour affects the poorest families, as these household often critically rely on the children's income. Some models have been developed to illustrate the rise of inefficient child labour as a result of household credit constraints (Ranjan 2001). This usually leads to worse educational outcomes, with adverse effects on earnings as adults (Gunnarsson et. al. 2004, Jacoby and Skoufias 1997). Other determinants include the characteristics of the parents and further which parent - mother or father - holds these characteristics (Grootaert 1998, Ersado 2004). In addition, child labour laws have been shown to have asymmetric outcomes. Krueger and Donohue (2005) look at the impact of child labour legislation on human capital accumulation and the distribution of wealth and welfare in 19th century United States. They find that high wage workers benefit most from child labour bans and that households with significant financial assets unambiguously lose from government interventions.

To our knowledge, no study has combined the analysis of the future outcomes of cohorts affected by child labour laws and that of the impact of violence on the effectiveness of such a reform. Using the available data, we try to estimate the impact of the law on future outcomes, taking into account that some regions are more plagued by violence than others. For our analysis, we use the more liberal approach of defining a child labourer in addition to the definition set by the Conven-

[3]The authors hypothesise three channels through which this could have occurred : change in marital status at the time of delivery, increased engagement in unhealthy behavior due to better education and economic outcomes, and postponement in age of delivery (Bellés-Obrero et al. 2015).

tions. As such, a child labourer is considered to be any person between the ages of 5 to 14 that is economically active or "gainfully employed".[4] According to the ILO, there exists a distinction between "child labourers" and "child workers". The former is more restrictive as it excludes full-time household work and chores (Basu 1999). This paper addresses "child work" as a broader measure of child labour in order to evaluate the full impact of the law change on educational outcomes. It is also reasonable to assume that the numbers in our analysis are also underreported due to the illegal nature of this type of employment (see Limitations subsection). Thus we will henceforth use both terms interchangeably, but always refer to "child work".

1.2 Context in Colombia

To formulate our hypothesis, we turn to the context in Colombia in the early 2000s and more specifically in 2006. Estimates ranged from 10,000 to 200,000 children working in illegal mining operations alone in that year depending on the organisation that measured it.[5] Many of these children participate in the labour force without attending school, an issue that is most prevalent amongst boys.[6] Girls tend to be more active in the service sector while boys are mostly found in the agricultural sector. Children between the ages of 12 and 14, the cohort that would have been primarily affected by the law change, worked on average 25 hours per week in 2001 (IPEC 2001). As statistics emerged on the extent of child labour, policies to abolish and regulate child labour became an urgent matter for the Colombian government.

The Code of Childhood and Adolescence of 2006 was stimulated by the two fundamental ILO Conventions, C138 and C182, which were ratified by Colombia in the early 2000s. This minimum age reform passed in November 8, 2006 and was adopted in May 2007. In brief, Article 35 of the code prohibits all types of economic activity for children under 15. Children between 15 and 17 need an authorisation from the Work Inspector or, in special cases, from the local authorities. Further, this reform ensured protection and rights for working minors under the Colombian Constitution, labour laws and international treaties.

There are multiple law enforcement entities in Colombia who have varying degrees of jurisdiction over issues of child labour. These include the Ministry of Labour's Inspection, Monitoring, Control and Territorial Management Department; the Ministry of the Interior; the National Police; the Colombian Institute for Family Well-Being; the Attorney General's Office; the Office of the Ombudsman; the Ministry of Health and Social Protection; and the National Training Service. The coordination between these entities has had a mixed track record. While it has been improving recently, problems still exist as law enforcement organisations struggle to make an impact in the most remote regions of Colombia especially those plagued by conflict.

In terms of education, the country still has some significant flaws in its system. In their report on the Colombian educational system in the 2008, the World Bank noted that while the government had increased education spending, challenges remained in improving the quality of learning. A key piece of evidence they cited was that a high proportion (nearly half) of all students could

[4] Note that the ILO formally defines a child labourer between the ages of 5 to 17 but for in the context of Colombia and for the purpose of our analysis, we must restrict this to the age of 15.

[5] DANE estimated between 10,000 to 15,000 child labourers in Colombia in 2006; the ILO estimated it at approximately 100,000.

[6] Among children ages 10-14, those who are economically active are less likely to attend school than those who are not (75.1 vs. 94.5 per cent). (IPEC 2001)

not answer basic mathematics questions. In sum, despite the government's efforts, there has only been moderate advancement both in terms of education quality and child labour according to the U.S. Department of Labour (2015) and the OECD (2016).

1.3 Hypothesis

Following the implementation of the law restricting the use of child labour, there are three plausible outcomes for the children who were the correct age to be affected by the law. Our outcomes are based on assumptions of law enforcement and the quality of the educational system. Below, we present the three potential hypotheses :

- *Hypothesis I*: The law is implemented and enforced. Students attend school instead of working, gain quality knowledge and skills, and are thus able to work better jobs and earn higher wages upon completion of their education. If this is the case, there should be an increase in educational and economic outcomes for the affected cohort to reflect the increase in skills and abilities.
- *Hypothesis II*: As above, the law is enforced but the additional years of education gained by the children add very little value in terms of skills and knowledge due to the poor quality of the educational system. If this is the case, we should observe a decrease in the socioeconomic outcomes of interest because the affected cohort fails to accumulate human capital through schooling, and the foregone experience and wages that they would have gained from working instead of going to school leaves them worse off.
- *Hypothesis III*: In this scenario, the law goes unenforced. If this is the case, there would be no significant effect on socioeconomic outcomes.

Based on the literature and context in Colombia, we speculate that the implementation of the Code of Childhood and Adolescence of 2006 will follow *Hypothesis III* leading to unchanged outcomes due to the imperfect educational and enforcement systems. In our paper, we will decompose our analysis to aid in understanding how different subgroups are affected. In terms of wealth decomposition, we hypothesize that there will be a stronger impact on the second poorest subgroup of the population because child labour issues are more prevalent in the poorer segments of the population. However, due to the lack of enforcement, we also believe that the poorest subgroup will not be impacted as the income from child labour is usually a matter of household subsistence. For our decomposition based on the type of residence, we believe that the rural sector will be most affected by the law change because, based on our dataset, 15.4 percent of rural children between the ages of 12 and 14 are economically active compared to 7.5 percent in urban areas.[7] In terms of gender, 11.8 percent of boys aged between 12 and 14 are economically active compared to 12.0 percent of girls. This gender difference is fairly small and we therefore do not expect to see a significantly different impact based on the gender. Lastly, we expect that violence-prone areas have a smaller impact than the safer area and that therefore our results are driven by the safer areas. We propose a measure of violence as a proxy for weak law enforcement since these areas are usually those plagued with paramilitary presence in the long-lasting civil conflict of Colombia.

[7] These estimates are from our 2005 DHS datasets. See Apendix Table

2 Data and Methodology

2.1 Data

This paper utilizes two data sets to construct an analysis of the socioeconomic outcomes of individuals in Colombia both before and after the Code of Childhood and Adolescence of 2006 was implemented. The first is a dataset constructed by the United States Agency for International Development's (USAID) Demographic and Health Surveys (DHS) Program in collaboration with Profamilia, a Colombian NGO. The second was constructed by the National Administrative Department of Statistics of Colombia (DANE). Additionally, we merge the DHS datasets with municipal panel data from the Center for Economic Development Studies (CEDE), which contains general information on municipality characteristics as well as violence data at the municipality level.

The DHS data provided by USAID consists of nationally-representative household level survey data on a variety of socioeconomic indicators. The information is collected through a personal interview, for which men and women aged between 15-49 are eligible. In addition, the head of each household is asked about each member within their household, so the dataset provides large amounts of data on children in Colombia. To conduct our analysis, we used surveys conducted in 2005 and 2015. Both surveys have approximately 160,000 individual observations. From the DHS dataset, we gathered a variety of educational outcome variables. We create dummies for completion of secondary education and start of tertiary education. Additionally, we look at years of education and create a potential education[8] variable which is the number of schooling years attained divided by the possible total years of education assuming that an individual starts primary school at age six and does not interrupt the educational cycle until they have reached 16 years of education, which is (generally) the number of years needed to obtain a bachelor's degree. For controls, we use the age and gender of the individuals and the household heads as well as the household size and structure. The relationship structure variable describes the number of adults in a given household and their relationship to each other, for example if the household is composed by two adults of the opposite sex or by three or more related adults. On top of this, we exploit a household wealth index created by the DHS, which is based on the assets a given household owns. Finally, every household in the DHS data is matched to the municipality in which it resides, so we control for municipality fixed effects. On top of this, we cluster at the original sampling cluster level of the DHS survey.

We further exploited two surveys compiled by DANE. The first dataset is the 2005 Continuous Household Survey, whose Spanish acronym is ECH. The ECH consists of a monthly survey for the major cities and a quarterly survey for the rest of the country, which consists of general socioeconomic and employment outcomes. The survey is representative at the national level and is collected through one-on-one in-person interviews. Any member of the household older than 12 is eligible, but the household head is prioritized for the responses. We compiled the observations for each month of the year, obtaining approximately 1,000,000 observations. The second dataset is the Large Integrated Household Survey (GEIH) of 2015. The GEIH uses the same methodology of the ECH, but it includes data from two related surveys that were conducted separately until 2006. Thus, we perform the same data merging procedure as with the ECH and end up with approximately 780,000 observations. For this database, we are able to use the same educational

[8] More information on the potential education variable is available in the appendix

outcome variables as in the DHS Surveys and additionally exploit two labour outcomes, namely the probability of being currently employed and the log of real monthly income, which we adjust for inflation. We use the log of real wages as generally done in economics to allows us to measure the growth rates (percent changes in wages) as the result of the law implementation rather than absolute wages. For the DANE datasets, we also control for individual gender and age as well as household size and gender and age of the household head. We do not have the household structure and wealth index variables of the DHS datasets, but we control for family earnings. The smallest geographical unit for the DANE surveys is the department, so we cluster at this level.

Table 1 shows the summary statistics for the samples of the individual datasets. On average, the age and gender distribution of the sampled individuals is similar across datasets for a given year. However, a salient difference in the samples is that the DANE database jumps from a 68 percent proportion of households located in urban areas in 2005 to a 93 percent proportion in 2015, whereas this proportion only varies from 76 percent in 2015 to 77 percent in 2015 for the DHS datasets. The average years of schooling are higher in both DANE samples. Household specific characteristics, even though differing in their average value across datasets in 2005, show the same trends in 2015. Individuals in the same age group live in smaller households with younger and more educated household heads in 2015. Additionally, the proportion of male household heads decreases from 2005 to 2015 in both datasets.

Table 1: Summary Statistics Sample

	2005 DANE	2005 DHS	2015 DANE	2015 DHS
Age	22.51	22.50	22.45	22.47
	(1.71)	(1.71)	(1.71)	[1.71]
Male	46.43%	47.80%	47.47%	47.74%
	(49.87)	(49.95)	(49.94)	(50.43)
Years of School	10.37	9.33	11.32	10.53
	(3.48)	(3.77)	(3.20)	(3.36)
Completed Secondary	68.87%	56.80%	78.26%	69.65%
	(46.30)	(49.54)	(41.25)	(45.98)
Started Tertiary	30.11%	23.33%	44.64%	39.98%
	(45.87)	(42.30)	(49.71)	(48.99)
Potential Education	67.02%	65.76%	73.26%	70.84%
	(22.50)	(60.60)	(20.72)	(44.53)
Urban	68.08%	75.88%	92.09%	77.43%
	(46.62)	(42.78)	(27.00)	(41.81)
HH Size	4.98	5.35	4.40	4.77
	(2.30)	(2.61)	(2.07)	(2.26)
HH Head Age	45.77	44.68	44.33	44.53
	(15.04)	(15.75)	(15.59)	(15.51)
Male HH Head	67.04%	69.85%	58.17%	63.40%
	(47.01)	(45.89)	(48.65)	[48.36]
HHH Years of Schooling	7.83	8.41	8.90	8.56
	(4.66)	(13.63)	(4.69)	(10.09)
Working	62.44%		62.30%	
	(48.42)		(48.46)	
Wages	313.21		774.24	
	(242.31)		(730.00)	
Observations	116,669	16,689	78,851	16,357

Sources: DANE ECH 2005, DANE GEIH 2015, DHS 2005 & DHS2015
Note: Standard errors in parentheses. Monthly real wages are presented in 2008 thousands of Colombian Pesos, are adjusted for inflation and are conditional on the individual currently working. (1,686 Colombian Pesos (2008) = 1 U.S. Dollar (2015)).

Lastly, the Municipal Panel is an initiative of the CEDE at the University of the Andes in Bogotá, Colombia. Their goal is to consolidate information at the municipal level in a single database. This panel presents information on the general characteristics of municipalities as well as fiscal, violence, agricultural and education variables from 1993 to 2013. For our violence variable, we use the average of per capita violence over the four years that a student should be in high school (ages 15 to 18). We do this because we expect that violence experienced during these four years will a determining factors for both secondary completion and tertiary enrollment.

2.2 Methodology

Our identification strategy is based on a cohort analysis. First, we compare inter-cohort outcomes using the 2015 datasets. For this analysis, we categorise as the treated group those individuals that were between 12 and 14 years old at the time the law came into force in May 2007 and as the control group those that were between 15 and 17 at the time, and thus unaffected by the law change. The rationale behind this categorisation is that the cohorts should be fairly similar because they are similar in age, but only the treatment group is affected by the law. These individuals are between 20 and 25 years old by 2015. We end up with approximately 16,000 observations from the DHS dataset and 79,000 observations from DANE's GEIH for this comparison.

We compare the same treatment and control cohorts from the 2015 datasets with individuals in the same 20 to 25 age range in 2005. For this analysis we have approximately 33,000 observations for DHS after appending both datasets and 196,000 observations after appending DANE's datasets.

We use the following baseline regression:

$$(i) \quad Y_{ic} = \beta_0 + \beta_1 \lambda_c + \beta_2 X_{ic} + \epsilon_{ic}$$

where Y_{ic} can be one of five outcome variables (completed secondary education dummy, started tertiary education dummy, potential years of education, probability of working, or logarithm of wages) for individual i in cohort c. λ_c is a cohort dummy, which takes the value of 1 for children between the ages of 12 and 14 in 2007 and the value of 0 for those aged 15 to 17, thus representing the treatment dummy for this regression. X_{ic} is the vector of socioeconomic control variables described in the data section. This again includes individual and household head age and gender as well as household size. For the DHS sample we include household structure, wealth index and municipality fixed effects and for the DANE sample we include the log of the household income and department fixed effects. The last term ϵ_{ic} is the error term. The coefficient of interest is β_1,which shows the average difference between the treatment and control cohorts. We exploit four further variations of this regression by interacting the treatment variable with our urban dummy and our gender dummy in order to analyse the possible heterogeneous impacts of the laws on urban and rural areas as well as males and females. We use a probit model with marginal effects to determine the impact of the treatment. We then subdivide our results by wealth and exposure to violence. The last two specifications only apply to the DHS datasets. The wealth index categorises households into five wealth quintiles. This allows us to observe how the outcomes vary from the poorest to the richest households. Similarly, we divide the violence index into quintiles to measure the difference outcomes from the most violent to least violent municipalities.

Our second approach consists of a difference-in-difference method, which allows us to contrast the same treatment and control groups of the 2015 sample with comparable cohorts in 2005. Thus, we compare the difference in outcomes between 20 to 22 year olds in 2005 and 23 to 25 year olds in 2005 to the difference in outcomes between 20 to 22 year olds in 2005 and 23 to 25 year olds in 2015. This method allows us to compare the average change in outcomes of the younger cohort across time with the same average change of the older cohort across time. As in the past analysis, we use probit regressions with marginal effects for the outcomes expressed as dummies and linear regressions otherwise.

We use the following baseline differences-in-differences regression for our estimation:

$$(ii) \quad Y_{ict} = \beta_0 + \beta_1 \tau_t + \beta_2 \lambda_c + \beta_3 (\tau_t * \lambda_c) + \beta_4 X_{ict} + \epsilon_{ict}$$

where the variables Y_{ict} , λ_c, X_{ict} and ϵ_{ict} hold the same definitions as in the OLS regression specification described above, and t is time. However, we add years of education as an outcome variable, as this is a comparable measure for the same cohort at a different point in time. Additionally, we include τ_t as a time dummy that takes the value of 1 for any year after 2007 and 0 for years prior to 2007. The coefficient of interest is β_3, which represents the difference-in-difference coefficient, i.e. the difference between the differences of the cohorts in each year. Note that fixed effects are differenced out using this regression design. We add the same individual and household controls as in the OLS/probit regression.

Finally, we use a triple difference approach to further break down our analysis. For this purpose, we exploit the following regression:

$$\begin{aligned}(iii) \quad Y_{ict} = &\beta_0 + \beta_1 \tau_t + \beta_2 \lambda_c + \beta_3 Z_{ict} + \beta_4 (\tau_t * \lambda_c) + \beta_5 (\tau_t * Z_{ict}) \\ &+ \beta_6 (\lambda_c * Z_{ict}) + \beta_7 (\tau_t * \lambda_c * Z_{ict}) + \beta_8 X_{ict} + \epsilon_{ict}\end{aligned}$$

where in addition to the previous model, we add a third term Z_{ict}. This can represent one of two variables: gender or urban-rural. For gender, Z_{ict} takes the value of 1 for males and 0 for females; in this case, the urban-rural variable is found in the X_{ict} variable. For urban-rural, Z_{ict} takes the value of 1 for urban and 0 for rural, and gender is included as a control. The coefficient of interest, β_7, either shows the outcome for males or urban individuals in the treatment cohort in 2015 depending on our interaction terms. ϵ_{ict} is the error term. We can thus measure the average effect of the policy change for sub groups within our treatment cohort in the difference in difference setting.

3 Empirical Results

In this section, we first look at the educational outcomes with a primary focus on the probablity of completing education, and the probability of starting tertiary. After looking at the general models, we decompose the analysis of each outcome by gender, type of residence and wealth index. In our last section, we look at the impact of violence on educational outcomes. We then turn to economic outcomes - the probability of working and the impact on wages - to present the general model, followed by the decomposition by type of residence and gender.

3.1 Educational Outcomes

For our educational outcomes, we focus on the DHS survey as it is more representative of the population as a whole. Further, the municipal-level panel data is only compatible with the DHS dataset. Using this information therefore allows for an analysis of the impact of violence on policy effectiveness.[9] As mentioned in our Methodology, we use two other variables - potential education and years of schooling - and find results consistent with the ones presented. We fully report these results in Appendix Tables 11 and 12. Because of space limitations, we report the complete results of our estimations for secondary education, but only the coefficients of interest for the rest of the outcome variables.The complete tables are available in the Appendix.

3.1.1 Secondary Completion

In our analysis of the impact of the law on secondary school completion, we first run model (i) with a probit specification using our 2015 cross-sectional data.[10] Our results show that the probability of secondary school completion increases by 2.1 percent at the 90 percent confidence level. This finding supports both *Hypothesis I* and *Hypotheses II*, but it does not indicate whether the quality of schooling positively impacts individuals, allowing them to have better labour market outcomes.

Next, we suspect that there may be heterogeneous effects among different subsets of the population due to the differing incidence rates of child labour (see Table 28 in the Appendix). Specifically, since there are more child labourers in rural regions, we expect that the law had a larger impact in rural areas than in urban areas. When we include an interaction between the dummy for our treatment and the dummy for type of resident (equal to one if urban and 0 if rural), we find that the effect of the law is stronger in rural areas, where your probability of completing secondary school has increased on average by 3.09 percentage points when compared to the urban counterparts. This result is consistent with our expectations.

We also want to test if there are heterogeneous effects between males and females. Since boys and girls between ages 12 and 14 have roughly the same employment participation rate (see Appendix Table 28) in 2005, we do not expect a significant difference in the impact of the law based on gender. Our probit model using 2015 cross-sectional data bolsters this claim, as the interaction term between the treatment and gender[11] dummies has a coefficient is close to 0 (-0.0098) and is insignificant. In Appendix Table 13, we see that running the same model with data from DANE leads to similar results.

Again, we consider that there may be heterogeneous results among different household wealth percentiles. Using the DHS wealth index, we find that there are insignificant results for all quintiles except for the second-poorest quintile, indicating that this group may be driving the increase in secondary school completion. The second-poorest group has a 4.6 percentage-point increase in secondary school completion as a result of the law, significant at the 90 percent level. This is

[9] Recall that the urban population surveyed by DANE increased by 20 percent between 2005 and 2015, reaching approximately 85 percent of the total survey sample, whereas the proportion in urban and rural in DHS was kept relatively constant over time. According to the World Bank, this proportion changed from 73.6 percent in 2005 to 76.2 percent in 2014.

[10] As explained in the data section, the treatment group is the cohort affected by the law (aged 12 to 14) and the control group is the cohort of slightly older individuals unaffected by the law (aged 15 to 18).

[11] Recall that our gender dummy is equal to 1 if male and 0 if female.

Table 2: Probability of Completing Secondary School (DHS)

	Cross-Sectional			Differences in Differences		
VARIABLES	Entire Sample	Urban v. Rural	Male v. Female	Entire Sample	Urban v. Rural	Male v. Female
Treatment	.0209 *	0.160***	0.0946*	0.0559	0.100*	0.0693*
	(0.0126)	(0.0593)	(0.0513)	(0.0359)	(0.0528)	(0.0413)
Treatment x Urban		-.0309**			-0.0533	
		(0.0142)			(0.0508)	
Treatment x Gender			-.0098			-0.0232
			(0.0123)			(0.0426)
Treatment x Post07				-0.00712	0.0514	0.0118
				(0.0325)	(0.0621)	(0.0443)
Treatment x Post07 x Urban					-0.0841	
					(0.0723)	
Treatment x Post07 x Gender						-0.0365
						(0.0622)
Post07				0.505***	0.557***	0.579***
				(0.0281)	(0.0570)	(0.0346)
Urban x Post07					-0.0544	
					(0.0612)	
Gender x Post07						-0.149***
						(0.0442)
Urban		0.189***			0.205***	
		(0.0547)			(0.0465)	
Gender	-0.281***	-0.281***	-0.262***	-0.212***	-0.212***	-0.122***
	(0.0234)	(0.0234)	(0.0326)	(0.0161)	(0.0161)	(0.0302)
Age	0.0262*	0.0266*	0.0262*	0.0208**	0.0215**	0.0209**
	(0.0138)	(0.0137)	(0.0138)	(0.00956)	(0.00956)	(0.00957)
Household Head Age	0.00332***	0.00336***	0.00333***	0.00513***	0.00518***	0.00516***
	(0.00105)	(0.00105)	(0.00105)	(0.000697)	(0.000697)	(0.000697)
Household Size	-0.102***	-0.102***	-0.102***	-0.0923***	-0.0926***	-0.0924***
	(0.00776)	(0.00776)	(0.00776)	(0.00468)	(0.00468)	(0.00469)
Poor Households	0.655***	0.584***	0.656***	0.662***	0.605***	0.664***
	(0.0393)	(0.0463)	(0.0393)	(0.0279)	(0.0319)	(0.0279)
Middle Class Households	1.047***	0.951***	1.047***	1.103***	1.024***	1.106***
	(0.0461)	(0.0573)	(0.0461)	(0.0316)	(0.0377)	(0.0317)
Rich Households	1.616***	1.519***	1.617***	1.578***	1.494***	1.583***
	(0.0575)	(0.0667)	(0.0576)	(0.0370)	(0.0429)	(0.0370)
Richest Households	1.952***	1.857***	1.953***	1.976***	1.891***	1.981***
	(0.0694)	(0.0773)	(0.0695)	(0.0441)	(0.0493)	(0.0441)
One Adult hh	-0.269***	-0.276***	-0.267***	-0.246***	-0.256***	-0.248***
	(0.0832)	(0.0834)	(0.0832)	(0.0565)	(0.0566)	(0.0566)
Two Adults Opp Sex hh	-0.321***	-0.324***	-0.320***	-0.363***	-0.365***	-0.361***
	(0.0650)	(0.0651)	(0.0650)	(0.0393)	(0.0394)	(0.0393)
Two Adults Same Sex hh	0.0344	0.0310	0.0342	-0.0298	-0.0359	-0.0333
	(0.0873)	(0.0874)	(0.0873)	(0.0569)	(0.0569)	(0.0570)
Three Plus Related Adults hh	0.0890	0.0861	0.0890	0.0493	0.0486	0.0502
	(0.0602)	(0.0604)	(0.0602)	(0.0347)	(0.0347)	(0.0347)
Unrelated Adults hh	-	-	-	-	-	-
Fixed Effects	yes	yes	yes	yes	yes	yes
Observations	16,211	16,211	16,211	33,019	33,019	33,017

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Treatment coefficients are marginal effects. Individual and household level controls as well as municipality fixed effects are included. Standard errors clustered at the original sampling level.
Sources: DHS 2005 & DHS 2015

consistent with our hypothesis that the three richest quintiles and the poorest quintile should not be affected as much by the law because the richest are less likely to be child labourers in the first place and the poorest need to work to maintain subsistence-level income[12], respectively. Meanwhile, the children in the second poorest quintile are poor enough to be child labourers, but rich enough to not be plagued by the constraints of the poorest people. It therefore seems that the second poorest group is driving our results for the increase in secondary school completion.

3.1.2 Tertiary Enrollment

Table 3: Probability of Starting Tertiary School (DHS)

COEFFICIENTS	Cross-Sectional			Differences in Differences		
OF INTEREST	Entire Sample	Urban v. Rural	Male v. Female	Entire Sample	Urban v. Rural	Male v. Female
Treatment	-.0128 (0.0137)					
Treatment x Urban		-.0407** (0.0184)				
Treatment x Gender			-.0066 (.0132)			
Treatment x Post07				0.013 (0.009)		
Treatment x Post07 x Urban					-0.045* (0.025)	
Treatment x Post07 x Gender						-0.019 (0.018)
Controls	yes	yes	yes	yes	yes	yes
Fixed Effects	yes	yes	yes	yes	yes	yes
Observations	16,169	16,169	16,169	32,670	32,670	32,665

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Treatment coefficients are marginal effects. Individual and household level controls as well as municipality fixed effects are included. Standard errors clustered at the original sampling level.
Sources: DHS 2005 & DHS 2015

While we may expect tertiary enrollment to increase as a result of the law, in reality we find evidence that points to the contrary when we use our model (i) with only 2015 cross section data. For the entire sample, we see that enrollment in tertiary education decreases as a result of the law, as the marginal effects of our treatment coefficient is -0.13, albeit insignificant. Running a similar model with the DANE dataset, we find that the marginal effects of the treatment coefficient is -0.011 and significant at the 95 percent confidence level, raising our concerns that the law may have negative impacts on tertiary enrollment. However, we develop the *"Gap Year Hypothesis"* to explain the negative sign of our coefficient, whereby individuals may wait one or more years after completing their secondary education to enroll in tertiary education in order to work or simply take time off their education. Since our control group is younger than our treatment group, they have not had as much time to enroll in tertiary education after completing their secondary education. In fact, we see that overall school enrollment for the treatment group is decreasing (from 37.91 percent in 2014 to 30.63 in 2015), while overall school enrollment for the control group is increasing (from 17.52 percent to 20.06 percent in 2015), supporting our *"Gap Year Hypothesis"*. For this reason, the impact on starting tertiary education appears to be negative, when in reality this may not be the case.

[12]Children from the poorest households may either attend school until the age of 15 and drop out without having completed their education or simply not attend school to work and to bolster their family income at the risk of being caught for engaging in child labour.

Due to differing trends for enrolling in tertiary education for our treatment and control group in the model (i), we argue that model (ii) may be more appropriate for this analysis as there are no differences in the treatment and control members' ages. When we run the difference-in-difference model with a probit specification, we see that the marginal effect of the treatment coefficient on starting tertiary education is 0.013 but insignificant. We obtain similar results using the DANE dataset (Appendix Table 16). Thus, we do not have conclusive results to confirm that there exists a significant impact on the probability of starting tertiary education. Perhaps, waiting to conduct the analysis when both treatment and control groups are older - and so much less likely to re-enroll in schooling - would yield more accurate results.

Again, we are concerned with the heterogeneous effects of the law on educational outcomes between different subsets of the population. First, we analyse the differences in outcomes between urban and rural populations and find that individuals from rural areas are less negatively affected by the law than individuals from urban areas. Indeed, the marginal effects of the interaction term coefficient is -0.041 and significant at the 95 percent level, meaning that there is a stronger impact in tertiary school enrollment for the rural treatment when compared to urban.

With respect to heterogeneous effects between males and females, we again see that there are no significant differences in outcomes, also captured with our different specifications and datasets (see Appendix Tables 15 and 16). We hold the same explanation as with secondary schooling outcomes.

Like our results for secondary completion, we find that the second-poorest group is driving the results for beginning tertiary education. We see that the results for the three richest quintiles and the poorest quintile are insignificant. However, the marginal effects of the treatment coefficient for the treatment is -0.172 and significant at the 95 percent confidence level. As with secondary completion, we hypothesize that this result is due to the fact that the second-poorest quintile is rich enough to not face the pressure to continue employing their children, but poor enough for the law to have an effect.

3.1.3 Violence

Our final analysis looks at heterogeneity of educational outcomes as a result of the prevalence of violence in each individual's secondary schooling years as described in the Data section. We run a model (i) with a probit specification for each violence quintile to further understand these effects because the CEDE panel data does not permit us to look into the difference-in-difference model, because the data does not go back far enough. We find evidence - albeit weak - that the presence of violence leads to lower probabilities of completing secondary school and starting tertiary education.

Only one of the quintiles yields significant results for the probability of completing secondary education. Being a child in the second safest quintile increases your probability of completing secondary schooling by 6.1 percentage points as a result of the policy change, significant at the 90 percent confidence level. For tertiary education, we find insignificant results for the four lowest violence quintiles - the most peaceful - but a negative and significant effect for individuals in the most violent quintile. Being in the most violent quintile decreases your probability of attending tertiary schooling by 7 percentage points. This disparity is likely due to the negative effect of

Table 4: Educational Outcomes by Violence (DHS)

COEFFICIENTS OF INTEREST	Least Violent Q1	Q2	Q3	Most Violent Q4	Q5
Secondary Completion	0.031	0.061*	0.026	0.017	0.002
	(0.031)	(0.036)	(0.031)	(0.035)	(0.038)
Tertiary Start	0.0580	0.203	-0.0876	-0.147	-0.379***
	(0.112)	(0.142)	(0.123)	(0.127)	(0.128)
Potential Education	0.0126	0.0480	-0.0227	-0.0684*	-0.00600
	(0.0312)	(0.0414)	(0.0354)	(0.0385)	(0.0389)
Controls	yes	yes	yes	yes	yes
Fixed Effects	yes	yes	yes	yes	yes
Observations	3,099	1,428	2,564	2,463	2,223

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Treatment coefficients are marginal effects. Individual and household level controls as well as municipality fixed effects are included. Standard errors clustered at the original sampling level.
Sources: DHS 2015

violence on psychological, educational, safety, and many other different types of outcomes (Akbulut-Yuksel and Yuksel, 2015).

3.2 Labour Outcomes

Analyzing just education outcomes is not enough to determine whether *Hypothesis I* or *Hypothesis II* predominates. We turn to economic outcomes, using the DANE dataset as it includes variables of interests not found in the DHS dataset. We find that our results for economic outcomes support *Hypothesis II* - the law is successfully implemented but further education does not lead to better labour market outcomes for individuals in our treatment group. Thus, we analyse the impact on the probability of being employed and the log of real wages conditional on being employed.

3.3 Probability of Employment

In order to analyse the impact of the law on employment, we only look at the subset of individuals in the sample that are not enrolled in school. We first use model (i) with a probit specification and find no significant impact of the law on probability of having a job. When we run the difference-in-difference model, we find that the law decreases your probability of being employed by 1.7 percentage points. Although this is weak evidence, it is suggestive that the law harms labour market outcomes for individuals, lending credibility to *Hypothesis II.* Further, as seen from the summary statistics, the proportion of the survey sample interviewed in urban areas jumps from 68 percent in 2005 to 92 percent in 2015. Since the proportion of child labourers in rural areas is higher that of urban areas, we expect an attenuation bias in our results for model (ii) because the impact of the law should be lower in urban areas and we have an overrepresentation of the urban areas for 2015. This oversampling is a result of sample design rather than urbanization as the urban populace only made up 73.5 percent to 76.4 percent of the population between 2005 and 2015 (World Bank 2014).

Table 5: Probability of Employment (DANE)

COEFFICIENTS OF INTEREST	Cross-Sectional Entire Sample	Urban v. Rural	Male v. Female	Differences in Differences Entire Sample	Urban v. Rural	Male v. Female
Treatment	0.007 (0.008)					
Treatment x Urban		-0.016 (0.011)				
Treatment x Gender			-0.023*** (0.008)			
Treatment x Post07				-0.017** (0.008)		
Treatment x Post07 x Urban					-0.021* (0.011)	
Treatment x Post07 x Gender						-0.00002 (0.0102)
Controls	yes	yes	yes	yes	yes	yes
Fixed Effects	no	no	no	no	no	no
Observations	62,190	62,190	62,190	147,428	147,428	147,428

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Treatment coefficients are marginal effects. Individual and household level controls are included. Standard errors clustered at the department level.
Sources: ECH 2005 & GEIH 2015

Again, we test for whether there are heterogenous effects among subsets of the population. We find that individuals in rural areas have seen an increase in employment opportunities as a result of the law, as the marginal effects of the interaction of treatment and urban coefficient is -0.113 and significant at the 95 percent confidence level. This result holds under model (iii) as seen with the negative and significant coefficient of -0.021 (see Appendix Table 18). This indicates that we have evidence that rural workers are better off as a result of the law compared to urban workers, consistent with our expectations.

Similar to the comparison of rural and urban individuals, we see a significant gender difference under model (i). Being a male on average increases your probability of being employed by 2.3 percentage points as a result of the law when compared to being a female, significant at the 99 percent confidence level. This result vanishes in model (iii), again suggesting only weak evidence for heterogenous effects between males and females.

3.3.1 Log of Real Wages

In order to assess the impact of the law on real wages, we employ models (i) and (ii) using the DANE dataset. Using the probit specification in model (i), we find that there is a negative impact of the law on log of real wages, further supporting *Hypothesis II.* Indeed, the law lowers the real wages of the treatment group by nearly 7 percent - with 90 percent confidence - when compared to the control group. It is possible that the 2015 cohort has lower wages because they fail to accumulate human capital in the educational system and simultaneously fail to gain work experience. Another possible explanation is that freshly graduated students find jobs with lower wages but much higher growth potential. A final explanation found in the literature is that the law may have simply altered the time allocation between school and work hours for the affected cohorts in order to avoid getting caught, leading to worse educational and economic outcomes (Ilahi et. al. 2004).

Table 6: Log Wage (DANE)

COEFFICIENTS OF INTEREST	Cross-Sectional Entire Sample	Cross-Sectional Urban v. Rural	Cross-Sectional Male v. Female	Differences in Differences Entire Sample	Differences in Differences Urban v. Rural	Differences in Differences Male v. Female
Treatment	-0.0683*					
	(0.0395)					
Treatment x Urban		-0.186***				
		(0.0376)				
Treatment x Gender			0.133***			
			(0.0418)			
Treatment x Post07				-0.132***		
				(0.0415)		
Treatment x Post07 x Urban					-0.164***	
					(0.0595)	
Treatment x Post07 x Gender						0.158**
						(0.0738)
Controls	yes	yes	yes	yes	yes	yes
Fixed Effects	no	no	no	no	no	no
Observations	7,100	11,027	11,027	49,721	49,721	49,721

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
We take the natural logarithm of monthly wages in 2008 thousands of Colombian Pesos. Individual and household level controls are included. Standard errors clustered at the department level.
Sources: ECH 2005 & GEIH 2015

Again, we find that the impact of the law affects individuals in rural areas more than individuals in urban areas, this time for the worse. Individuals in rural areas see an 18.6 percent larger decrease in wages than their urban counterparts. As with our other urban-rural analyses, we believe that this impact is driven by the fact that child labour is more prevalent in rural areas. This results is supported by evidence from our triple differences model which shows that rural individuals earn a monthly wage that is on average 16.4 percent than those of urban individuals, giving robustness to our result.

When comparing males and females using the OLS version of model (i), we find that males see smaller decreases in their incomes as a result of the law, gaining 13.3 percent more than females. Since child labour is more prevalent in rural areas, and males are more likely to be employed as child labourers in rural areas, effect is consistent with our expectations. Again, this result is supported by our triple differences model which indicates that men gain 15.8 percent more relative to woman as a result of the law with 95 percent confidence. Thus, we see that this outcome is robust.

3.4 Limitations

Before proceeding to potential policy proposals, we address some limitations in our analysis. The most important is the lack of available panel data, which does not allow to track individuals over the 10-year time lapse. However, we attempt to account for this by merging a representative cross-sectional census of the population with municipal-level panel data. By employing a difference-in-difference strategy and clustering at the municipal level, we are able to better isolate the impact of the law. Second, and still related to data availability, we omit variables that were not found in the data. Some potential variables typically found in the literature are cognitive abilities, parental characteristics, and years of total experience. [13] Based on our dataset, we carefully selected the

[13] Grooteart (1998) finds that parental characteristics matter, and which parent holds these characteristic also matters.

variables most appropriate to account for these. Again, panel data would allow us to difference out this effect. Third, our analysis focuses on the cohort that was first affected by the law change - not later cohorts - due to our design and the lack of time lapsed since the law implementation. Hence, we miss any effects that are gradual. It could be that those already involved in child labour are not affected but rather that the regulation impacts future household decision making for the other children. Our design does not allow for gradual effects to be captured. Once again, despite this limitation, the short time frame analysed allows us to isolate our desired measured effect of the initial implementation of the law.

In addition, our data omits information on occupation by sector which would enable us to determine whether the lower wages are maybe associated with positions that have more growth potential. Hence, it would be interesting to perform a similar analysis on the same cohorts in future research to observe career growth potential changes. Furthermore, performing a similar analysis in a couple of years to see the effect of the peace deal between the FARC and the government would allow to capture the impact of civil conflict on child labour.

One opportunity for further analysis of the effects of the law is to conduct complexity analysis and examine the outcomes for various types of child labour before and after the law. This involves ranking the various occupations of children based upon their relative harmfulness. For example, child prostitution is significantly more harmful for children than working as a shopkeeper. By breaking the child labour statistics out into its component parts, it could be possible to see if the law reduced the most harmful types of child labour, or if it had a larger effect on more visible occupations (such as manual labour or shopkeeping), than on more discrete or unregulated occupations (like housekeeping or prostitution).

4 Discussion: Policy Implications

We find overall suggestive evidence that child labour laws have an effect on future educational and economic outcomes for the affected cohort. Indeed, our data suggests that the law increases the total years of education (in rural areas where child labour is most prevalent) but also decreases future wages for the affected cohorts. Our findings are consistent with theoretical models found in the literature (Ranjan 1999). This analysis raises the question of whether a case can be made for promoting minimum age of employment regulations. It is first important to note that this paper focuses on the impact of the Code of Childhood and Adolescence of 2006 on the first affected cohort due to the long-term nature of our analysis. It is plausible that as the law progressively consolidated implementation had a stronger impact on later cohorts, as there may be a 'learning by doing' effect at play for the enforcement of the law. Our decomposition results on violence suggest that minimum employment age laws are most effective in countries that are not plagued by conflict and that have strong law enforcement institutions. It would be interesting to see the effect of the recent FARC peace treaty on the incidence of child labour for future research. To reinforce the effectiveness of such laws, we propose that lawmakers must clearly lay out the law enforcement mechanisms before the law comes into force to prevent miscoordination and neglect. Further, random checks could be performed in factories and fields to deter employers from circumventing the law.

In any case, child labour regulations and standards are a necessary first step to establish international norms on the issue. But, implementing regulations is simply not enough. These reforms must be brought forth with a social package in order to have the desired impact on society. As seen in our results, passing a child labour regulation law as a means to increase human capital may affect only subgroups of the population, but not necessarily the most vulnerable subgroups such as the poorest individuals. Based on our results, we propose the following alternatives to improve outcomes for the population as a whole and to target the most vulnerable subgroups (i.e. the poorest wealth quintile): education and easing of household financial pressures.

A better educational system would help students attending school accumulate human capital for better socioeconomic outcomes. In fact there exists a broad set of literature that supports this type of reform as a solution to reduce incidences of child labour. Grootaert and Kanbur (1995) highlight the benefits of education whereby social returns exceed private returns. They propose that bolstering returns to education may be a better policy than a child labour ban to reduce child labour and can encourage children to complete schooling instead.

The context in Colombia further supports our policy proposal for an improvement of the education system as a necessary step. As part of their comprehensive analysis, the World Bank (2008) looked into the 2006 results of 57 countries that participated in PISA test scores.[14] Colombia scored below its predicted score line for all subjects, performing quite poorly when compared to other participating Latin American counterparts. [15] The report also showed that Colombia had a relatively low dispersion of test scores between top and bottom performers and that scores presented more variance within school than between schools. These results suggest that Colombia's education system achieves fairly high equity but delivers low quality. [16] There has been some limited progress with education reform in the country. The OECD (2016) report noted that while Colombia's PISA scores had improved slightly according to the 2012 tests, they remained well below the OECD average across all three subjects and still had the highest proportion of "grade repeaters". The World Bank (2008) also found that the capacity for sub-national governments to allocate resources effectively towards education varied greatly, with Bogota, Barranquilla and Cali being the only municipalities on the quality frontier. Furthermore, although compulsory education has been mandatory since 1999 in Colombia,[17] guaranteed free schooling has only been ensured since 2010. The Colombian government launched the National Development Plan for 2014-2018 which focuses on both quality and quantity of education.

For educational reforms, we propose that the government intervene to improve the quality of schooling and therefore incentivise children to receive an education. Possible interventions include building a stronger support system within schools for those who need more attention to succeed (for example girls) or providing free school meals. The World Bank (2008) also highlighted a number of policies that could be implemented to raise the standard of education. Such measures included continued participation in standardised assessments, enabling disadvantaged populations to succeed in the education system, strengthening the system of accountability, and using resources more efficiently. In addition, limiting or subsidising costs of school supplies, uniforms, and similar education related expenses could increase attendance (Tenor 2015). Further, vocational training

[14] PISA is an OECD initiative that provides a measure of reading, mathematics and science achievement among 15 year old students for a nationally representative sample of schools.

[15] In their report the Bank compared mean test scores to assess Colombia's performance relative other countries and relative to expectations based on the country's GDP and socioeconomic characteristics.

[16] Further evidence of the need for Colombia to improve its educational system is provided. Nearly half of test takers in Colombia performed at the 'Below Level 1' proficiency segment in mathematics, i.e. students could not answer the most basic questions. In addition, Colombia had the largest share of "grade repeaters" amongst PISA participating countries.

[17] As found in the Minor's Code of 1989 of Colombia.

could be a viable alternative for children that may require an intermediate point between education and child labour. Technical degrees exist in Colombia but are thought to hold much less esteem within the population. A combination of these reforms should increase the incentives to obtain a secondary education, increase the probability of starting tertiary education, and improve labour market outcomes.

Assuming that the educational system is improved upon, we next propose conditional cash transfers (CCTs) as an alternative policy to target child labourers, specifically within the poorest quintile of the population. A body of literature shows that it is necessary to ease financial pressures on households. Jacoby and Skoufias (1997) find evidence that availability of credit on reasonable terms can rescue many from the perils of child labour. A study by Filho (2008) in Brazil finds that increased benefits from social security reforms are associated with increases in school enrollment and decreases in labour participation for girls aged 10 to 14. Further, Grootaert (1998), finds that very poor households often critically rely on the children's income. Therefore, a CCT would allow families to maintain their level of income foregone by the child going to school and it would allow children to accumulate human capital. We propose a cash transfer be conditional on on the children's school attendance and refrainment from participating in the labour market. This is similar to an initiative implemented in Colombia from 1991 to 2004, the *Programa de Ampliación de Cobertura de la Educación Secundaria*, which increased educational attainment and test scores (Angrist et. al. 2002). Such a program would allow us to target the poorest quintile by providing them with an alternative to reach subsistence level while accumulating human capital through increased schooling. However, it is key to note that the transfer must be high enough to be effective as past literature has found that a CCT in Brazil did not have an effect on child labour rates (Cardoso and Souza 2004).

5 Conclusion

This paper uses household level survey data from DHS and DANE to examine the effects of The Code of Childhood and Adolescence of 2006, which raised the minimum working age from 12 to 15, on socioeconomic outcomes of the affected cohort. Using OLS, probit, and difference-in-difference models, we find mixed results for the effectiveness of the law on educational outcomes of the overall cohort. While there is an increase in secondary school completion, there is also a decrease in tertiary enrollment which can be explained with the *"Gap Year Hypothesis."* Breaking down the analysis into different subgroups allows for more conclusive results. Indeed rural areas, which have a higher prevalence of child labour, seem to be more strongly affected by the law change. We also see that cohorts living in safer areas experience a bigger impact from the law, suggesting that law enforcement is a necessary tool for the effectiveness of such a law.

The results support a vast body of literature that suggests that the implementation of minimum age working requirements must be accompanied by a social reform package to have any significant effect on socioeconomic outcomes. We propose policies that may be implemented simultaneously to the child labour ban in order to maximise the effect on the most vulnerable members of society. First, the government should focus efforts on increasing the returns to education by improving the quality of schooling. In addition, providing financial relief to the poorest quintile of the population through relaxed credit constraints, social security reforms, or conditional cash transfers could negate the need for families to send their children to work during times of low household income.

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Appendix

Table 7: OLS Probability of Completing Secondary School (DHS)

VARIABLES	Entire Sample	Urban v. Rural	Male v. Female
Treatment	.0209 *	0.160***	0.0946*
	(0.0126)	(0.0593)	(0.0513)
Treatment x Urban		-.0309**	
		(0.0142)	
Treatment x Gender			-.0098
			(0.0123)
Urban		0.189***	
		(0.0547)	
Gender	-0.281***	-0.281***	-0.262***
	(0.0234)	(0.0234)	(0.0326)
Age	0.0262*	0.0266*	0.0262*
	(0.0138)	(0.0137)	(0.0138)
Household Head Age	0.00332***	0.00336***	0.00333***
	(0.00105)	(0.00105)	(0.00105)
Household Size	-0.102***	-0.102***	-0.102***
	(0.00776)	(0.00776)	(0.00776)
Poor Households	0.655***	0.584***	0.656***
	(0.0393)	(0.0463)	(0.0393)
Middle Class Households	1.047***	0.951***	1.047***
	(0.0461)	(0.0573)	(0.0461)
Rich Households	1.616***	1.519***	1.617***
	(0.0575)	(0.0667)	(0.0576)
Richest Households	1.952***	1.857***	1.953***
	(0.0694)	(0.0773)	(0.0695)
One Adult hh	-0.269***	-0.276***	-0.267***
	(0.0832)	(0.0834)	(0.0832)
Two Adults Opp Sex hh	-0.321***	-0.324***	-0.320***
	(0.0650)	(0.0651)	(0.0650)
Two Adults Same Sex hh	0.0344	0.0310	0.0342
	(0.0873)	(0.0874)	(0.0873)
Three Plus Related Adults hh	0.0890	0.0861	0.0890
	(0.0602)	(0.0604)	(0.0602)
Unrelated Adults hh	-	-	-
Observations	16,211	16,211	16,211

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Treatment coefficients are marginal effects
Sources: DHS 2015

Table 8: Diff in Diff Probability of Completing Secondary School (DHS)

VARIABLES	Entire Sample	Rural v. Urban	Male v. Female
Treatment x Post07	-0.00712	0.0514	0.0118
	(0.0325)	(0.0621)	(0.0443)
Treatment x Post07 x Urban		-0.0841	
		(0.0723)	
Treatment x Post07 x Gender			-0.0365
			(0.0622)
Treatment	0.0559	0.100*	0.0693*
	(0.0359)	(0.0528)	(0.0413)
Post07	0.505***	0.557***	0.579***
	(0.0281)	(0.0570)	(0.0346)
Urban		0.205***	
		(0.0465)	
Urban x Post07		-0.0544	
		(0.0612)	
Treatment x Urban		-0.0533	
		(0.0508)	
Gender x Post07			-0.149***
			(0.0442)
Treatment x Gender			-0.0232
			(0.0426)
Gender	-0.212***	-0.212***	-0.122***
	(0.0161)	(0.0161)	(0.0302)
Age	0.0208**	0.0215**	0.0209**
	(0.00956)	(0.00956)	(0.00957)
Household Head Age	0.00513***	0.00518***	0.00516***
	(0.000697)	(0.000697)	(0.000697)
Household Size	-0.0923***	-0.0926***	-0.0924***
	(0.00468)	(0.00468)	(0.00469)
Poor Households	0.662***	0.605***	0.664***
	(0.0279)	(0.0319)	(0.0279)
Middle Class Households	1.103***	1.024***	1.106***
	(0.0316)	(0.0377)	(0.0317)
Rich Households	1.578***	1.494***	1.583***
	(0.0370)	(0.0429)	(0.0370)
Richest Households	1.976***	1.891***	1.981***
	(0.0441)	(0.0493)	(0.0441)
One Adult hh	-0.246***	-0.256***	-0.248***
	(0.0565)	(0.0566)	(0.0566)
Two Adults Opp Sex hh	-0.363***	-0.365***	-0.361***
	(0.0393)	(0.0394)	(0.0393)
Two Adults Same Sex hh	-0.0298	-0.0359	-0.0333
	(0.0569)	(0.0569)	(0.0570)
Three Plus Related Adults hh	0.0493	0.0486	0.0502
	(0.0347)	(0.0347)	(0.0347)
Unrelated Adults hh	-	-	-
Observations	33,019	33,019	33,017

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Triple diff and whole sample treatment coefficients are marginal effects
Sources: DHS 2005 & DHS 2015

Table 9: OLS Probability of Starting Tertiary School (DHS)

VARIABLES	Entire Sample	Urban v. Rural	Male v. Female
Treatment	-.0128	0.0706	-0.0322
	(0.0137)	(0.0683)	(0.0496)
Treatment x Urban		-.0407**	
		(0.0184)	
Treatment x Gender			-.0066
			(.0132)
Urban		0.0870	
		(0.0591)	
Gender	-0.376***	-0.376***	-0.364***
	(0.0233)	(0.0233)	(0.0326)
Age	0.00932	0.00949	0.00937
	(0.0135)	(0.0135)	(0.0135)
Household Head Age	0.00700***	0.00699***	0.00700***
	(0.000941)	(0.000941)	(0.000941)
Household Size	-0.122***	-0.122***	-0.122***
	(0.00880)	(0.00880)	(0.00880)
Poor Households	0.642***	0.634***	0.642***
	(0.0418)	(0.0489)	(0.0418)
Middle Class Households	1.062***	1.049***	1.062***
	(0.0475)	(0.0589)	(0.0475)
Rich Households	1.644***	1.632***	1.644***
	(0.0530)	(0.0633)	(0.0530)
Richest Households	2.079***	2.068***	2.079***
	(0.0627)	(0.0714)	(0.0627)
One Adult hh	-0.280***	-0.280***	-0.279***
	(0.0807)	(0.0807)	(0.0807)
Two Adults Opp Sex hh	-0.404***	-0.405***	-0.404***
	(0.0635)	(0.0635)	(0.0635)
Two Adults Same Sex hh	-0.0623	-0.0626	-0.0624
	(0.0820)	(0.0820)	(0.0820)
Three Plus Related Adults hh	-0.0334	-0.0334	-0.0334
	(0.0583)	(0.0583)	(0.0583)
Unrelated Adults hh	-	-	-
Observations	16,169	16,169	16,169

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: Treatment coefficients are marginal effects

Sources: DHS 2015

Table 10: Diff in Diff Probability of Starting Tertiary Education (DHS)

VARIABLES	Entire Sample	Urban v. Rural	Male v. Female
Treatment x Post07	0.013	0.184**	0.0785*
	(0.009)	(0.0880)	(0.0443)
Treatment x Post07 x Urban		-0.045*	
		(0.025)	
Treatment x Post07 x Gender			-0.019
			(0.018)
Treatment	0.0195	0.0235	0.0122
	(0.0382)	(0.0758)	(0.0440)
Post07	0.701***	0.703***	0.776***
	(0.0278)	(0.0737)	(0.0343)
Urban		0.114*	
		(0.0602)	
Urban x Post07		0.00461	
		(0.0762)	
Treatment x Urban		-0.00283	
		(0.0739)	
Gender x Post07			-0.162***
			(0.0457)
Treatment x Gender			0.0212
			(0.0484)
Gender	-0.296***	-0.296***	-0.198***
	(0.0172)	(0.0172)	(0.0339)
Age	0.0463***	0.0467***	0.0463***
	(0.0100)	(0.0100)	(0.0100)
Household Head Age	0.00636***	0.00637***	0.00636***
	(0.000691)	(0.000690)	(0.000692)
Household Size	-0.112***	-0.112***	-0.112***
	(0.00568)	(0.00568)	(0.00570)
Poor Households	0.579***	0.552***	0.580***
	(0.0328)	(0.0379)	(0.0328)
Middle Class Households	0.973***	0.937***	0.976***
	(0.0360)	(0.0439)	(0.0361)
Rich Households	1.510***	1.470***	1.513***
	(0.0389)	(0.0466)	(0.0389)
Richest Households	2.094***	2.054***	2.097***
	(0.0433)	(0.0502)	(0.0433)
Observations	32,670	32,670	32,665

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Triple diff and whole sample treatment coefficients are marginal effects
Sources: DHS 2005 & DHS 2015

Table 11: OLS Potential education (DHS)

VARIABLES	Entire Sample	Rural v. Urban	Male v. Female
Treatment	-0.0196	0.0148	-0.0154
	(0.0136)	(0.0209)	(0.0145)
Treatment x Urban		-0.0438**	
		(0.0185)	
Treatment x Gender			-0.00870
			(0.0133)
Urban		0.0325*	
		(0.0168)	
Gender	-0.0222***	-0.0223***	-0.0178*
	(0.00749)	(0.00749)	(0.0106)
Age	-0.0144***	-0.0143***	-0.0144***
	(0.00417)	(0.00417)	(0.00417)
Household Head Age	0.000663*	0.000661*	0.000663*
	(0.000387)	(0.000387)	(0.000387)
Household Size	-0.00793**	-0.00794**	-0.00792**
	(0.00358)	(0.00358)	(0.00357)
Poor Households	0.0989***	0.0932***	0.0990***
	(0.0119)	(0.0132)	(0.0119)
Middle Class Households	0.174***	0.167***	0.174***
	(0.0134)	(0.0152)	(0.0134)
Rich Households	0.233***	0.225***	0.233***
	(0.0152)	(0.0171)	(0.0152)
Richest Households	0.289***	0.282***	0.289***
	(0.0178)	(0.0199)	(0.0178)
One Adult hh	-0.0526**	-0.0450*	-0.0500*
	(0.0255)	(0.0254)	(0.0260)
Two Adults Opp Sex hh	-0.103***	-0.0948***	-0.101***
	(0.0142)	(0.0146)	(0.0150)
Two Adults Same Sex hh	-0.0395	-0.0315	-0.0373
	(0.0240)	(0.0241)	(0.0244)
Three Plus Related Adults hh	-0.0581***	-0.0502***	-0.0559***
	(0.0151)	(0.0157)	(0.0160)
Unrelated Adults hh	-0.0333	-0.0252	-0.0311
	(0.0268)	(0.0268)	(0.0274)
Observations	16,264	16,264	16,264
R-squared	0.082	0.082	0.082

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Treatment coefficients are marginal effects

Sources: DHS 2015

Table 12: Diff in Diff Years of Schooling (DHS)

VARIABLES	Entire Sample	Urban v. Rural	Male v. Female
Treatment x Post07	-0.107	0.0174	-0.141
	(0.0668)	(0.154)	(0.0894)
Treatment x Post07 x Urban		-0.153	
		(0.171)	
Treatment x Post07 x Gender			0.0742
			(0.129)
Treatment Diff in Diff	0.0645	0.444***	0.0877
	(0.0766)	(0.119)	(0.0879)
Post07	1.363***	1.804***	1.542***
	(0.0610)	(0.146)	(0.0734)
Urban	0.445***	0.951***	
	(0.0813)	(0.117)	
Urban x Post07		-0.490***	
		(0.153)	
Treatment x Urban		-0.485***	
		(0.116)	
Gender x Post07			-0.358***
			(0.0953)
Treatment x Gender			-0.0513
			(0.0941)
Gender	-0.612***	-0.611***	-0.431***
	(0.0329)	(0.0329)	(0.0690)
Age	0.0417**	0.0435**	0.0404**
	(0.0199)	(0.0199)	(0.0199)
Household Head Age	0.0101***	0.0101***	0.00991***
	(0.00151)	(0.00151)	(0.00152)
Household Size	-0.232***	-0.232***	-0.231***
	(0.0104)	(0.0104)	(0.0104)
Poor Households	1.820***	1.855***	2.041***
	(0.0793)	(0.0792)	(0.0716)
Middle Class Households	2.984***	3.020***	3.280***
	(0.0897)	(0.0899)	(0.0767)
Rich Households	4.064***	4.092***	4.374***
	(0.0974)	(0.0974)	(0.0834)
Richest Households	5.148***	5.175***	5.461***
	(0.103)	(0.103)	(0.0897)
One Adult hh	-1.492***	-1.452***	-1.660***
	(0.175)	(0.176)	(0.181)
Two Adults Opp Sex hh	-1.939***	-1.897***	-2.124***
	(0.151)	(0.152)	(0.157)
Two Adults Same Sex hh	-1.236***	-1.197***	-1.418***
	(0.174)	(0.175)	(0.179)
Three Plus Related Adults hh	-0.909***	-0.870***	-1.099***
	(0.147)	(0.149)	(0.154)
Unrelated Adults hh	-1.149***	-1.109***	-1.342***
	(0.162)	(0.164)	(0.167)
Observations	32,808	32,808	32,808
R-squared	0.359	0.361	0.358

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Triple diff and whole sample treatment coefficients are marginal effects
Sources: DHS 2005 & DHS 2015

Table 13: OLS Probability of Secondary Completion (DANE)

VARIABLES	Entire Sample	Urban v. Rural	Male v. Female
Treatment	0.003	0.0868**	0.0122
	(0.004)	(0.0346)	(0.0218)
Treatment x Urban		-0.019**	
		(0.009)	
Treatment x Gender			0.001
			(0.007)
Urban		0.680***	
		(0.0345)	
Gender	-0.251***	-0.256***	-0.252***
	(0.0157)	(0.0156)	(0.0213)
Household Head Age	0.0315***	0.0301***	0.0315***
	(0.000578)	(0.000568)	(0.000579)
Household Head Gender	-0.0255	0.0293*	-0.0255
	(0.0160)	(0.0150)	(0.0160)
Household Head Education	0.151***	0.139***	0.151***
	(0.00309)	(0.00269)	(0.00309)
Household Size	-0.0754***	-0.0780***	-0.0754***
	(0.00420)	(0.00388)	(0.00419)
Family Earnings	-5.14e-08***	-6.31e-08***	-5.13e-08***
	(1.86e-08)	(1.90e-08)	(1.84e-08)
Observations	78,410	78,410	78,410

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Treatment coefficients are marginal effects Sources: DANE 2015

Table 14: Diff in Diff Probability of Secondary Completion (DANE)

VARIABLES	Entire Sample	Urban v. Rural	Male v. Female
Treatment x Post07	-0.007	0.0713	-0.0589**
	(0.005)	(0.0436)	(0.0232)
Treatment x Post07 x Urban		-0.033***	
		(0.012)	
Treatment x Post07 x Gender			0.018**
			(0.008)
Treatment	0.0197	-0.0153	0.0489***
	(0.0165)	(0.0166)	(0.0171)
Post07	0.218***	-0.287***	0.271***
	(0.0215)	(0.0372)	(0.0248)
Urban		0.0977***	0.219***
		(0.0199)	(0.0288)
Urban x Post07		0.546***	
		(0.0326)	
Treatment x Urban		0.0525***	
		(0.0176)	
Gender x Post07			-0.203***
			(0.0247)
Treatment x Gender			-0.0598***
			(0.0157)
Gender	-0.171***	-0.172***	-0.0781***
	(0.0165)	(0.0164)	(0.0217)
Age	-0.00521	-0.00536	-0.00505
	(0.00534)	(0.00531)	(0.00530)
Household Head Age	0.0316***	0.0311***	0.0314***
	(0.000638)	(0.000628)	(0.000623)
Household Head Gender	-0.0382**	-0.0144	-0.0236
	(0.0161)	(0.0147)	(0.0151)
Household Head Education	0.149***	0.144***	0.146***
	(0.00321)	(0.00293)	(0.00295)
Household Size	-0.0733***	-0.0743***	-0.0738***
	(0.00507)	(0.00488)	(0.00490)
Family Earnings	2.53e-07***	2.48e-07***	2.47e-07***
	(4.47e-08)	(4.58e-08)	(4.53e-08)
Observations	194,428	194,428	194,428

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sources: Triple diff and whole sample treatment coefficients are marginal effects
Sources: DANE 2005 & DANE 2015

Table 15: OLS Probability of Tertiary Start (DANE)

	(1)	(2)	(3)
VARIABLES	Entire Sample	Urban v. Rural	Male v. Female
Treatment	-0.011**	-0.0115	-0.0260
	(0.005)	(0.0509)	(0.0184)
Treatment x Urban		-0.008	
		(0.017)	
Treatment x Gender			-0.007
			(0.005)
Urban		0.788***	
		(0.0503)	
Gender	-0.293***	-0.297***	-0.283***
	(0.0148)	(0.0148)	(0.0165)
Household Head Age	0.0307***	0.0298***	0.0307***
	(0.000635)	(0.000637)	(0.000635)
Household Head Gender	-0.0535***	-0.0197	-0.0536***
	(0.0169)	(0.0169)	(0.0169)
Household Head Education	0.134***	0.126***	0.134***
	(0.00245)	(0.00222)	(0.00246)
Household Size	-0.0974***	-0.0990***	-0.0974***
	(0.00475)	(0.00490)	(0.00474)
Family Earnings	-2.29e-08	-2.85e-08	-2.32e-08
	(2.52e-08)	(2.55e-08)	(2.52e-08)
Observations	78,410	78,410	78,410

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Treatment coefficients are marginal effects
Sources: DANE 2015

Table 16: Diff in Diff Probability of Tertiary Start (DANE)

VARIABLES	(1) Entire Sample	(2) Urban v. Rural	(3) Male v. Female
Treatment x Post07	0.003	0.0269	0.0293
	(0.007)	(0.0570)	(0.0270)
Treatment x Post07 x Urban		-0.007	
		(0.015)	
Treatment x Post07 x Gender			-0.012
			(0.009)
Treatment	0.0366*	0.0209	0.0382
	(0.0193)	(0.0226)	(0.0250)
Post07	0.332***	-0.305***	0.389***
	(0.0306)	(0.0496)	(0.0380)
Urban		0.0770***	0.186***
		(0.0205)	(0.0314)
Urban x Post07		0.656***	
		(0.0529)	
Treatment x Urban		0.0228*	
		(0.0133)	
Gender x Post07			-0.216***
			(0.0282)
Treatment x Gender			-0.00197
			(0.0286)
Gender	-0.163***	-0.164***	-0.0585**
	(0.0152)	(0.0154)	(0.0259)
Age	0.0283***	0.0283***	0.0284***
	(0.00558)	(0.00555)	(0.00552)
Household Head Age	0.0314***	0.0311***	0.0314***
	(0.000527)	(0.000558)	(0.000544)
Household Head Gender	-0.0432***	-0.0275**	-0.0344***
	(0.0136)	(0.0132)	(0.0131)
Household Head Education	0.140***	0.137***	0.139***
	(0.00242)	(0.00263)	(0.00257)
Household Size	-0.0904***	-0.0910***	-0.0908***
	(0.00434)	(0.00442)	(0.00445)
Family Earnings	3.12e-08	2.93e-08	3.08e-08
	(2.79e-08)	(2.83e-08)	(2.82e-08)
Observations	194,428	194,428	194,428

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Sources: Triple diff and whole sample treatment coefficients are marginal effects

Sources: DANE 2005 & DANE 2015

Table 17: OLS Potential Education (DANE)

VARIABLES	Sample	Urban v. Rural	Male v. Female
Treatment	-0.0139***	0.0409***	0.00266
	(0.00254)	(0.00490)	(0.00194)
Treatment x Urban		-0.0404***	
		(0.00545)	
Treatment x Gender			0.00216
			(0.00256)
Age	-0.00589***		
	(0.000900)		
Urban		0.146***	
		(0.00752)	
Gender	-0.0409***	-0.0409***	-0.0420***
	(0.00177)	(0.00168)	(0.00254)
Household Head Age	0.00435***	0.00410***	0.00436***
	(9.63e-05)	(8.10e-05)	(9.62e-05)
Household Head Gender	-0.00733***	0.000417	-0.00744***
	(0.00210)	(0.00190)	(0.00208)
Household Head Education	0.0215***	0.0195***	0.0215***
	(0.000449)	(0.000335)	(0.000450)
Household Size	-0.0118***	-0.0120***	-0.0118***
	(0.000563)	(0.000538)	(0.000562)
Family Earnings	6.03e-09**	4.03e-09	5.53e-09**
	(2.49e-09)	(2.54e-09)	(2.47e-09)
Observations	78,400	78,400	78,400
R-squared	0.278	0.303	0.277

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Sources: Treatment coefficients are marginal effects
Sources: DANE 2015

Table 18: Diff in Diff Years of Education (DANE)

VARIABLES	(1) Sample	(2) Urban v. Rural	(3) Male v. Female
Treatment x Post07	-0.0518	0.514***	-0.102**
	(0.0419)	(0.105)	(0.0482)
Treatment x Post07 x Urban		-0.600***	
		(0.104)	
Treatment x Post07 x Gender			0.103*
			(0.0555)
Treatment	0.0608*	0.0940**	-0.0975**
	(0.0342)	(0.0410)	(0.0421)
Post07	0.618***	-0.960***	0.653***
	(0.0593)	(0.118)	(0.0559)
Urban x Post07		1.608***	
		(0.104)	
Treatment x Urban		-0.0492	
		(0.0372)	
Gender x Post07			-0.396***
			(0.0543)
Treatment x Gender			-0.0622
			(0.0436)
Age	0.0633***	0.0629***	
	(0.0123)	(0.0122)	
Urban		0.463***	0.672***
		(0.0664)	(0.0865)
Gender	-0.464***	-0.462***	-0.290***
	(0.0338)	(0.0334)	(0.0503)
Household Head Age	0.0730***	0.0714***	0.0721***
	(0.00123)	(0.00114)	(0.00116)
Household Head Gender	-0.150***	-0.0932***	-0.114***
	(0.0354)	(0.0330)	(0.0333)
Household Head Education	0.368***	0.354***	0.359***
	(0.00803)	(0.00699)	(0.00712)
Household Size	-0.191***	-0.192***	-0.192***
	(0.0102)	(0.00965)	(0.00971)
Family Earnings	5.39e-07***	5.27e-07***	5.32e-07***
	(4.48e-08)	(4.72e-08)	(4.70e-08)
Observations	193,096	193,096	193,096
R-squared	0.314	0.324	0.321

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Triple diff and whole sample treatment coefficients are marginal effects
Sources: DANE 2005 & DANE 2015

Table 19: OLS Probability of Employment (DANE)

VARIABLES	Entire Sample	Urban v. Rural	Male v. Female
Treatment	0.007	-0.113***	-0.129***
	(0.008)	(0.0313)	(0.0150)
Treatment x Urban		-0.016	
		(0.011)	
Treatment x Gender			-0.023***
			(0.008)
Age	0.0746***		
	(0.00762)		
Urban		0.210***	
		(0.0347)	
Household Head Gender		-0.0717***	-0.0854***
		(0.0157)	(0.0157)
Family Earnings		5.49e-07***	5.50e-07***
		(5.05e-08)	(5.05e-08)
Gender	0.748***	0.846***	0.878***
	(0.0360)	(0.0294)	(0.0332)
Household Head Age	-0.00293***	0.00199***	0.00242***
	(0.000640)	(0.000524)	(0.000530)
Household Head Education	0.0154***	0.0164***	0.0198***
	(0.00249)	(0.00225)	(0.00236)
Household Size	-0.0450***	-0.0489***	-0.0484***
	(0.00518)	(0.00443)	(0.00448)
Observations	62,190	62,190	62,190

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Treatment coefficients are marginal effects
Sources: DANE 2005 & DANE 2015

Table 20: Diff in Diff Probability of Employment (DANE)

VARIABLES	(1) Entire Sample	(2) Urban v. Rural	(3) Male v. Female
Treatment x Post07	-0.017**	0.0110	-0.0430*
	(0.008)	(0.0353)	(0.0250)
Treatment x Post07 x Urban		-0.021*	
		(0.011)	
Treatment x Post07 x Gender			-0.00002
			(0.0102)
Treatment	0.0718***	0.0774***	0.0961***
	(0.0205)	(0.0217)	(0.0256)
Post07	-0.0771*	-0.264***	-0.0199
	(0.0456)	(0.0473)	(0.0439)
Urban	0.0241*	-0.00972	0.0243*
	(0.0145)	(0.0171)	(0.0146)
Treatment x Urban		-0.00996	
		(0.0137)	
Gender x Post07			-0.152***
			(0.0438)
Treatment x Gender			-0.0698***
			(0.0250)
Gender	0.953***	0.953***	1.052***
	(0.0292)	(0.0293)	(0.0438)
Gender x Post07			-0.152***
			(0.0438)
Treatment x Gender			-0.0698***
			(0.0250)
Household Head Age	0.00220***	0.00209***	0.00225***
	(0.000788)	(0.000792)	(0.000795)
Household Head Education	0.0238***	0.0229***	0.0239***
	(0.00254)	(0.00256)	(0.00255)
Household Head Gender	-0.131***	-0.127***	-0.130***
	(0.0141)	(0.0140)	(0.0141)
Household Size	-0.0292***	-0.0294***	-0.0294***
	(0.00470)	(0.00468)	(0.00473)
Observations	147,428	147,428	147,428

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Triple diff and whole sample treatment coefficients are marginal effects
Sources: DANE 2005 & DANE 2015

Table 21: OLS Log of Income (DANE)

VARIABLES	(1) Entire Sample	(2) Urban v. Rural	(3) Male v. Female
Treatment	-0.0683*	-0.0207	-0.277***
	(0.0395)	(0.0317)	(0.0346)
Treatment x Urban		-0.186***	
		(0.0376)	
Treatment x Gender			0.133***
			(0.0418)
Age	0.0341**		
	(0.0127)		
Years of School	0.0638***		
	(0.00511)		
Previous Job Length	0.00166***		
	(0.000573)		
Job Length	0.00148**		
	(0.000659)		
Urban		0.538***	
		(0.0541)	
Household Head Gender		0.0254	-0.0119
		(0.0185)	(0.0171)
Gender	0.337***	0.289***	0.226***
	(0.0237)	(0.0256)	(0.0301)
Household Head Age	-0.00164**	0.00222***	0.00322***
	(0.000743)	(0.000644)	(0.000784)
Household Head Education	0.0256***	0.0436***	0.0542***
	(0.00330)	(0.00259)	(0.00328)
Household Size	-0.0134**	-0.0301***	-0.0281***
	(0.00509)	(0.00449)	(0.00489)
Observations	7,100	11,027	11,027
R-squared	0.161	0.153	0.122

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Sources: DANE 2015

Table 22: Diff in Diff Log of Income (DANE)

VARIABLES	(1) Entire Sample	(2) Urban v. Rural	(3) Male v. Female
Treatment x Post07	-0.132***	0.0157	-0.252***
	(0.0415)	(0.0662)	(0.0685)
Treatment x Post07 x Urban		-0.164***	
		(0.0595)	
Treatment x Post07 x Gender			0.158**
			(0.0738)
Treatment	-0.0484	-0.0521	-0.0465
	(0.0404)	(0.0562)	(0.0554)
Post07	1.237***	0.871***	1.095***
	(0.132)	(0.137)	(0.113)
Urban	0.155***	0.108***	0.157***
	(0.0332)	(0.0226)	(0.0330)
Urban x Post07		0.428***	
		(0.0562)	
Treatment x Urban		0.00396	
		(0.0412)	
Gender x Post07			0.218***
			(0.0543)
Treatment x Gender			-0.00445
			(0.0549)
Gender	0.0418	0.0422	-0.0106
	(0.0493)	(0.0490)	(0.0571)
Household Head Age	-0.00554***	-0.00563***	-0.00568***
	(0.00207)	(0.00205)	(0.00208)
Household Head Education	0.0310***	0.0296***	0.0311***
	(0.00353)	(0.00344)	(0.00352)
Household Head Gender	0.0187	0.0245	0.0175
	(0.0290)	(0.0289)	(0.0286)
Household Size	-0.0252***	-0.0257***	-0.0255***
	(0.00780)	(0.00780)	(0.00776)
Observations	49,721	49,721	49,721
R-squared	0.086	0.087	0.087

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sources: DANE 2005 & DANE 2015

Table 23: OLS Secondary Completion by Wealth Index (DHS)

VARIABLES	Poorest	Poor	Middle	Rich	Richest
Treatment	-0.0305	0.143*	0.103	0.178	0.0874
	(0.0914)	(0.0817)	(0.103)	(0.155)	(0.230)
Household Head Age	0.00477**	0.00274	0.00314	0.00110	0.00571
	(0.00211)	(0.00187)	(0.00222)	(0.00354)	(0.00517)
Gender	-0.168***	-0.299***	-0.419***	-0.341***	-0.563***
	(0.0465)	(0.0425)	(0.0488)	(0.0733)	(0.132)
Age	-0.000731	0.0259	0.0511*	0.0778	0.0382
	(0.0282)	(0.0238)	(0.0310)	(0.0475)	(0.0684)
Household Size	-0.0629***	-0.138***	-0.122***	-0.165***	-0.147***
	(0.0128)	(0.0128)	(0.0218)	(0.0228)	(0.0347)
Oriental Region	-0.425	-0.470	-0.413	0.804	0.0911
	(0.710)	(0.615)	(0.460)	(0.681)	(0.718)
Central Region	-0.946**	-0.954*	-0.632	0.415	0.0566
	(0.477)	(0.537)	(0.416)	(0.533)	(0.596)
Pacifica Region	-1.473**	-1.059**	-0.621	0.241	-0.868
	(0.591)	(0.536)	(0.544)	(0.830)	(0.752)
Bogota Region		-0.988*	-0.693*	0.586	0.384
		(0.522)	(0.392)	(0.519)	(0.588)
Orinoquia/Amazonia Region	-0.831**	-0.483	-0.544	0.471	
	(0.366)	(0.552)	(0.766)	(0.617)	
No Adults hh	-0.294*				
	(0.170)				
One Adult hh	-0.493***	0.0408	0.0112	-0.401	0.238
	(0.127)	(0.0879)	(0.144)	(0.336)	(0.458)
Two Adults Opp Sex hh	-0.235	0.378***	0.244	0.221	0.0238
	(0.203)	(0.128)	(0.193)	(0.459)	(0.554)
Two Adults Same Sex hh	0.0211	0.509***	0.243	-0.0202	0.702*
	(0.118)	(0.104)	(0.162)	(0.342)	(0.425)
Three Plus Related Adults hh		0.344**	0.111	-0.118	0.494
		(0.142)	(0.189)	(0.377)	(0.468)
Observations	3,620	4,724	3,278	2,070	1,248

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sources: DHS 2015

Table 24: OLS Tertiary Start By Wealth Index (DHS)

VARIABLES	Poorest	Poor	Middle	Rich	Richest
Treatment	-0.0438	-0.172**	0.0621	-0.0213	0.0872
	(0.119)	(0.0839)	(0.0953)	(0.113)	(0.142)
Household Head Age	0.00664***	0.00602***	0.00918***	0.00462*	0.0126***
	(0.00233)	(0.00172)	(0.00201)	(0.00254)	(0.00348)
Gender	-0.227***	-0.390***	-0.504***	-0.384***	-0.382***
	(0.0596)	(0.0426)	(0.0483)	(0.0562)	(0.0741)
Age	-0.00352	-0.0179	0.0374	0.0144	0.0474
	(0.0349)	(0.0245)	(0.0289)	(0.0337)	(0.0435)
Household Size	-0.0701***	-0.137***	-0.130***	-0.189***	-0.144***
	(0.0161)	(0.0139)	(0.0231)	(0.0205)	(0.0249)
Oriental Region	-0.737*	-0.136	-0.238	1.114*	-0.670
	(0.420)	(0.524)	(0.462)	(0.591)	(0.475)
Central Region	-1.170**	-1.182***	-0.878***	0.980**	-0.760*
	(0.514)	(0.302)	(0.322)	(0.498)	(0.451)
Pacifica Region	-1.819***	-1.086***	-1.508***	0.578	-0.176
	(0.583)	(0.311)	(0.520)	(0.767)	(0.770)
Bogota Region		-1.194***	-1.212***	0.953*	-0.444
		(0.273)	(0.303)	(0.487)	(0.445)
Orinoquia/Amazonia Region	-0.881*	-0.457	0.129	1.448**	-0.570
	(0.489)	(0.299)	(0.727)	(0.630)	(0.651)
No Adults hh	-0.543***				
	(0.203)				
One Adult hh	-0.681***	-0.118	-0.267**	-0.0372	0.163
	(0.150)	(0.0882)	(0.130)	(0.206)	(0.333)
Two Adults Opp Sex hh	-0.143	0.250**	-0.0854	0.436*	-0.136
	(0.235)	(0.125)	(0.167)	(0.260)	(0.379)
Two Adults Same Sex hh	-0.174	0.385***	-0.0870	0.339	0.378
	(0.132)	(0.0988)	(0.145)	(0.213)	(0.316)
Three Plus Related Adults hh		0.303**	0.102	0.0863	0.346
		(0.142)	(0.175)	(0.250)	(0.339)
Observations	3,189	4,668	3,391	2,329	1,634

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Sources: DHS 2015

Table 25: OLS Secondary Completion by Violence (DHS)

VARIABLES	Q1 (Least Violent)	Q2	Q3	Q4	Q5 (Most Violent)
Treatment	0.031	0.061*	0.026	0.017	0.002
	(0.031)	(0.036)	(0.031)	(0.035)	(0.038)
Household Head Age	0.00519**	0.00628	0.00255	0.00772***	0.000998
	(0.00227)	(0.00429)	(0.00266)	(0.00263)	(0.00259)
Gender	-0.325***	-0.363***	-0.298***	-0.288***	-0.260***
	(0.0555)	(0.0828)	(0.0604)	(0.0576)	(0.0631)
Age	0.0671**	0.0672	0.0446	0.0132	-0.0358
	(0.0332)	(0.0624)	(0.0416)	(0.0385)	(0.0374)
Household Size	-0.111***	-0.170***	-0.122***	-0.0796***	-0.120***
	(0.0153)	(0.0277)	(0.0203)	(0.0181)	(0.0184)
Poor Households	0.606***	1.040***	0.821***	0.717***	0.726***
	(0.0769)	(0.258)	(0.162)	(0.0864)	(0.0856)
Middle Class Households	0.870***	1.361***	1.221***	0.974***	1.259***
	(0.0966)	(0.272)	(0.164)	(0.0997)	(0.109)
Rich Households	1.584***	1.885***	1.867***	1.699***	1.712***
	(0.143)	(0.275)	(0.174)	(0.133)	(0.178)
Richest Households	1.727***	2.480***	2.152***	2.233***	1.699***
	(0.190)	(0.294)	(0.192)	(0.229)	(0.315)
Oriental Region	0.136	-0.0123	-0.346	0.919**	0.843
	(0.604)	(0.800)	(0.403)	(0.444)	(1.110)
Central Region	-0.0720	-0.447	-1.411**	0.651	-0.763
	(0.180)	(0.430)	(0.656)	(0.623)	(1.102)
Pacifica Region	0.229	-0.0642	-1.162***	0.445	-0.185
	(0.290)	(0.553)	(0.430)	(0.770)	(1.027)
Bogota Region		-0.491	-0.741**		
		(0.356)	(0.339)		
Orinoquia/Amazonia Region	-0.00549		0.375	0.965***	1.032
	(0.188)		(0.576)	(0.270)	(1.136)
No Adults hh				0.199	
				(0.225)	
One Adult hh	0.0233	-0.289	-0.0254	-0.0228	0.0354
	(0.130)	(0.245)	(0.160)	(0.165)	(0.120)
Two Adults Opp Sex hh	0.515***	-0.286	0.545**	0.0770	0.403**
	(0.182)	(0.347)	(0.246)	(0.202)	(0.183)
Two Adults Same Sex hh	0.399***	0.117	0.459***	0.242	0.530***
	(0.144)	(0.271)	(0.173)	(0.150)	(0.147)
Three Plus Related Adults hh	0.432**	0.234	0.201		0.305
	(0.178)	(0.305)	(0.209)		(0.210)
Observations	3,099	1,428	2,564	2,463	2,223

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sources: DHS 2015

Table 26: OLS Tertiary Start by Violence (DHS)

VARIABLES	Q1 (Least Violent)	Q2	Q3	Q4	Q5 (Most Violent)
Treatment	0.0580	0.203	-0.0876	-0.147	-0.379***
	(0.112)	(0.142)	(0.123)	(0.127)	(0.128)
Household Head Age	0.00673***	0.00581*	0.0113***	0.0113***	0.000196
	(0.00226)	(0.00340)	(0.00229)	(0.00234)	(0.00235)
Gender	-0.439***	-0.345***	-0.394***	-0.406***	-0.338***
	(0.0541)	(0.0749)	(0.0597)	(0.0587)	(0.0657)
Household Size	-0.130***	-0.144***	-0.162***	-0.0937***	-0.124***
	(0.0177)	(0.0259)	(0.0229)	(0.0187)	(0.0212)
Poor Households	0.645***	0.637**	0.746***	0.605***	0.626***
	(0.0823)	(0.304)	(0.188)	(0.0908)	(0.0945)
Middle Income Households	1.088***	0.867***	1.021***	0.958***	1.245***
	(0.0989)	(0.304)	(0.193)	(0.106)	(0.108)
Rich Households	1.687***	1.501***	1.657***	1.654***	1.759***
	(0.113)	(0.309)	(0.198)	(0.121)	(0.158)
Richest Households	2.013***	2.008***	2.226***	1.977***	2.198***
	(0.162)	(0.316)	(0.207)	(0.160)	(0.283)
Oriental Region	-0.760	0.671	-0.220	0.134	-0.785
	(0.502)	(0.439)	(0.444)	(0.564)	(1.035)
Central Region	0.180	-0.269	0.411	0.997*	-1.998*
	(0.414)	(0.409)	(0.563)	(0.583)	(1.061)
Pacifica Region	-0.820***	-0.493	-1.283**	0.911	-1.011
	(0.169)	(0.505)	(0.571)	(0.748)	(1.048)
Orinoquia/Amazonia Region	0.201		0.0101	0.647***	0.228
	(0.172)		(0.612)	(0.229)	(1.314)
One Adult hh	0.123	-0.300	-0.156	-0.293*	-0.208
	(0.131)	(0.203)	(0.154)	(0.164)	(0.127)
Two Adults Opp Sex hh	0.697***	-0.197	-0.0592	-0.196	0.271
	(0.176)	(0.269)	(0.201)	(0.199)	(0.176)
Two Adults Same Sex hh	0.490***	0.144	0.178	0.0195	0.222
	(0.142)	(0.212)	(0.166)	(0.149)	(0.141)
Three Plus Related Adults hh	0.593***	-0.189	0.261		0.170
	(0.183)	(0.262)	(0.205)		(0.210)
Observations	3,078	1,428	2,554	2,457	2,213

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sources: DHS 2015

Table 27: OLS Potential Education by Violence (DHS)

VARIABLES	Q1 (Least Violent)	Q2	Q3	Q4	Q5 (Most Violen
Treatment	0.0126	0.0480	-0.0227	-0.0684*	-0.00600
	(0.0312)	(0.0414)	(0.0354)	(0.0385)	(0.0389)
Household Head Age	0.00130	0.00271	0.00136	0.00112	-0.000748
	(0.000872)	(0.00182)	(0.000969)	(0.000857)	(0.000769)
Gender	-0.0268*	-0.0208	-0.0260	-0.0342	-0.0134
	(0.0154)	(0.0251)	(0.0181)	(0.0229)	(0.0176)
Household Size	-0.00983**	0.00902	-0.00951	-0.0106	-0.0205***
	(0.00417)	(0.0165)	(0.0132)	(0.00677)	(0.00690)
Poor Households	0.0830***	0.0351	0.118***	0.0968***	0.127***
	(0.0232)	(0.121)	(0.0443)	(0.0281)	(0.0231)
Middle Class Households	0.153***	0.0844	0.187***	0.168***	0.188***
	(0.0278)	(0.115)	(0.0472)	(0.0312)	(0.0225)
Rich Households	0.208***	0.143	0.265***	0.213***	0.258***
	(0.0259)	(0.120)	(0.0474)	(0.0361)	(0.0267)
Richest Households	0.281***	0.223*	0.323***	0.236***	0.295***
	(0.0421)	(0.125)	(0.0535)	(0.0383)	(0.0333)
Oriental Region	-0.0713	0.0892	-0.141	0.131	-2.082
	(0.0750)	(0.107)	(0.179)	(0.0906)	(2.096)
Central Region	-0.0572	-0.0946	-0.421**	0.0791	-2.441
	(0.0427)	(0.0895)	(0.208)	(0.116)	(2.097)
Pacifica Region	-0.0770**	0.0504	-0.323*	0.113	-2.156
	(0.0381)	(0.0762)	(0.180)	(0.175)	(2.097)
Orinoquia/Amazonia Region Region	-0.0294		-0.118	0.113	-2.015
	(0.0438)		(0.201)	(0.0764)	(2.099)
One Adult Household	-0.0383	-0.305*	-0.0931	-0.141***	-0.0230
	(0.0511)	(0.163)	(0.0797)	(0.0280)	(0.0212)
Two Adults Opp Sex hh	0.0136	-0.367**	-0.0721	-0.0372	0.0591*
	(0.0610)	(0.183)	(0.0898)	(0.0803)	(0.0317)
Two Adults Same Sex hh	-0.0183	-0.343*	-0.0609	-0.0720**	0.0987***
	(0.0638)	(0.207)	(0.105)	(0.0292)	(0.0342)
Three Plus Related Adults hh	0.0226	-0.357*	-0.0531	0.00336	0.136*
	(0.0742)	(0.193)	(0.115)	(0.0928)	(0.0781)
Observations	3,144	1,428	2,594	2,494	2,248
R-squared	0.099	0.054	0.085	0.071	0.119

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sources: DHS 2015

Table 28: Labour Cohorts of Interest

	DHS 2005 (12-14)			DHS 2015 (12-14)			
	Total	Male	Female	Total	Male	Female	Difference
Total	11.92%	11.80%	12.04%	5.73%	6.01%	5.42%	6.19***
	[32.40]	[32.27]	[32.54]	[23.24]	[23.79]	[22.65]	[0.00]
Rural	22.50%	23.03%	21.89%	10.56%	11.29%	9.74%	11.94***
	[41.76]	[42.12]	[41.36]	[30.73]	[31.65]	[29.75]	[0.01]
Urban	7.50%	6.84%	8.16%	3.52%	3.49%	3.55%	3.98***
	[26.34]	[25.25]	[27.38]	[18.42]	[18.35]	[18.50]	[0.00]
Obs	10,547			9,496			20,043
	DHS 2005 (15-17)			DHS 2015 (15-17)			
	Total	Male	Female	Total	Male	Female	Difference
Total	31.58%	30.01%	33.11%	20.87%	20.49%	21.26%	10.71***
	[29.75]	[45.84]	[47.07]	[24.34]	[40.37]	[40.92]	[40.92]
Rural	48.75%	50.86%	46.38%	30.48%	32.65%	28.06%	18.27***
	[49.99]	[50.01]	[49.89]	[46.04]	[46.91]	[44.94]	[0.01]
Urban	24.94%	21.14%	28.45%	17.10%	15.44%	18.74%	7.83***
	[43.27]	[40.84]	[45.12]	[37.65]	[36.14]	[39.03]	[0.01]
Obs	9,772			9,522			19.294

Sources: DHS 2005 & DHS 2015 Note: Child Labor is defined as the proportion of children whose main occupation is working or doing household chores. Standard errors in square brackets. The symbols ***, (**) and [*] stand for significance at the 1%, (5%) and [10%] levels, respectively

Table 29: Summary Statistics: Complete Dataset

	2005 DANE	2005 DHS	2015 DANE	2015 DHS
Age	30.16	28.20	32.19	31.41
	[20.45]	[20.55]	[21.36]	[21.55]
Male	47.09%	48.12%	47.15%	48.14%
	[49.92]	[49.96]	[49.92]	[50.14]
Years of School	7.43	5.59	7.86	6.49
	[4.69]	[4.67]	[5.09]	[4.85]
Completed Secondary	40.32%	23.15%	46.38%	31.80%
	[49.05]	[42.18]	[49.87]	[56.57]
Started Tertiary	22.23%	10.32%	28.50%	16.91%
	[41.58]	[30.42]	[45.14]	[37.49]
Potential Education	60.95%	56.27%	62.33%	57.67%
	[31.14]	[90.78]	[35.76]	[89.00]
Urban	67.13%	73.07%	85.73%	72.54%
	[46.96]	[44.36]	[34.97]	[44.63]
HH Size	4.92	5.40	4.33	4.65
	[2.24]	[2.55]	[2.04]	[2.21]
HH Head Age	48.41	47.73	48.70	48.70
	[14.75]	[14.92]	[15.15]	[15.16]
Male HH Head	70.23%	72.12%	61.25%	65.87%
	[45.72]	[44.84]	[48.18]	[47.49]
HHH Years of Schooling	7.53	7.99	8.48	7.95
	[4.78]	[13.68]	[4.98]	[9.96]
Working	44.25%		45.34%	
	[49.67]		[49.78]	
Wages	441.70		986.75	
	[557.69]		[1537.2]	
Observations	1,033,292	157,840	780,951	162,459

Sources: DANE ECH 2005, DANE GEIH 2015, DHS 2005 & DHS2015
Note: Standard errors in square brackets. Monthly real wages are presented in 2008 thousands of Colombian Pesos, are adjusted for inflation and are conditional on the individual currently working.

Table 30: DANE Summary Statistics by Treatment

	Baseline 2005	Control 2005	Treatment 2015	Control 2015
Age	21.01	23.99	21.00	23.99
	[0.84]	[0.82]	[0.82]	[0.82]
Male	46.08%	46.77%	47.34%	47.60%
	[49.85]	[49.90]	[49.93]	[49.94]
Years of School	10.30	10.45	11.21	11.44
	[3.21]	[3.72]	[2.92]	[3.47]
Completed Secondary	69.33%	68.42%	78.23%	78.29%
	[46.11]	[46.48]	[41.27]	[41.23]
Started Tertiary	29.42%	30.79%	43.88%	45.46%
	[45.57]	[46.17]	[49.62]	[49.79]
Potential Education	68.79%	65.28%	74.89%	71.53%
	[21.56]	[23.26]	[19.62]	[21.69]
Urban	68.08%	68.01%	92.00%	92.17%
	[46.62]	[46.61]	[27.12]	[26.86]
HH Size	5.06	4.89	4.47	4.33
	[2.30]	[2.30]	[2.05]	[2.08]
HH Head Age	46.38	45.17	45.01	43.60
	[14.51]	[15.52]	[15.31]	[15.85]
Male HH Head	65.90%	68.16%	56.92%	59.47%
	[47.41]	[46.59]	[48.82]	[48.44]
HHH Years of Schooling	7.76	7.90	8.75	9.05
	[4.66]	[4.66]	[4.67]	[4.70]
Working	59.58%	64.72%	59.03%	65.17%
	[49.07]	[47.79]	[49.18]	[47.64]
Wages	276.74	340.52	650.27	866.84
	[165.23]	[283.74]	[541.35]	[831.89]
Observations	57,770	58,899	40,556	38,295

Sources: DANE ECH 2005 & DANE GEIH 2015

Note: Standard errors in square brackets. Monthly real wages are presented in 2008 thousands of Colombian Pesos, are adjusted for inflation and are conditional on the individual currently working.

Table 31: DHS Summary Statistics by Treatment

	Baseline 2005	Control 2005	Treatment 2015	Control 2015
Age	21.00	24.00	20.98	23.99
	[0.83]	[0.81]	[0.83]	[0.82]
Male	47.25%	48.36%	48.34%	47.13%
	[49.93]	[49.98]	[50.48]	[50.37]
Years of School	9.28	9.37	10.52	10.54
	[3.55]	[3.99]	[3.12]	[3.60]
Completed Secondary	56.64%	56.96%	70.36%	68.91%
	[49.56]	[49.52]	[45.67]	[46.29]
Started Tertiary	21.23%	24.73%	39.61%	40.35%
	[41.38]	[43.15]	[48.91]	[49.06]
Potential Education	66.79%	64.74%	72.58%	69.05%
	[57.93]	[63.12]	[41.62]	[47.26]
Urban	75.05%	76.70%	77.60%	77.25%
	[43.27]	[42.27]	[41.70]	[41.92]
HH Size	5.49	5.21	4.84	4.70
	[2.63]	[2.59]	[2.26]	[2.25]
HH Head Age	45.54	43.83	45.31	43.72
	[15.33]	[16.10]	[15.03]	[15.94]
Male HH Head	68.08%	71.61%	62.52%	64.30%
	[46.62]	[45.09]	[48.56]	[48.15]
HHH Years of Schooling	8.10	8.71	8.42	8.71
	[13.25]	[13.99]	[9.98]	[10.21]
Observations	8,314	8,375	8,290	8,067

Sources: DHS 2005 & DHS 2015

Note: Standard errors in square brackets.