THE UNINTENDED CONSEQUENCES OF CHILD LABOUR LEGISLATION: EVIDENCE FROM BRAZIL

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Abstract

This paper looks at the impact of two Brazilian laws that changed the minimum legal age of entry into the labour market. Whereas the law of December 1998 can be seen as a ban, as it increased the minimum legal age from 14 to 16, the law of December 2000 was the opposite, permitting youth aged 14 and 15 to work as apprentices. Since these two laws set two clear cut–off points it is possible to estimate the local average treatment effect of these laws using regression discontinuity design (RD) and difference-in-differences (DD) techniques. This study uses four age groups, comparing children aged 13 and 14, and those aged 15 and 16. It looks at the impact on both work and school outcomes in order to see whether the laws had unintended consequences. Comparing individuals aged 14 and 15 I found that the 1998 ban led to a fall of 13 pp. in boys' participation rate in formal paid activities and of 7 pp. in boys' labour force participation. With regard to school outcomes, the ban reduced attendance among boys and increased their schooling delay of boys and girls. The DD estimates support most of the RD estimates. With respect to the law of 2000, the estimates show an increase in children's participation rate in formal paid work activities, a rise in boys' school attendance, and a negative effect on grade transition. I also looked at the impact of the laws on the gender gap and found that after 1998 the gap between boys' and girls' participation rates in domestic work widened by 5pp.

Keywords: Child labour, human capital, minimum legal age legislation, causal impact.

JEL: J08, J22, J23.

INTRODUCTION

The literature on child labour has grown considerably over the last decade. Although the labour force participation of children has fallen over the years, the global figure is still alarming. According to the ILO (2010), in 2010 more than 200 million children were participating in the labour market. In fact, there are many nuances behind this estimate as child labour estimates vary between and within countries. Developing countries tend to show a higher incidence of child labour when compared with developed countries. Even among the group of developing countries, the heterogeneity is by no means irrelevant (ILO, 2010). A similar pattern emerges within countries as child labour incidence tends to be higher amongst the poorest, in rural areas and in larger households (Edmonds, 2008).

Due to the negative externalities associated with children's labour force participation, it is argued that the public sector should intervene in the labour market by changing the incentives that make parents sending their children to work (see Basu and Van, 1998). In fact, many countries have adopted bans or other mechanisms aiming to break down the 'intergenerational child labour trap' (see Edmonds 2008 for a survey).

Basu and Van (1998) argue that parents' decision to send a child into the labour market might be seen as a rational choice for poor households facing a varied set of constraints. Under two assumptions it is shown that there may be multiple stable equilibria in the labour market, one of them characterised by children's labour force participation and depressed adult wages, and another in which children do not participate in the labour market and adult wages are higher. Because these two equilibria are Pareto efficient, the authors argue that whenever children are observed participating in the labour force, the government could pass a ban to make the economy move from an equilibrium with child labour to one without it. The main assumption stemming from this result is that the rise in adult wages that results from a ban has to be high enough to compensate for the income 'forgone' by children and to permit parents to consume children's leisure. This suggests that households' net benefit from a ban is ultimately an empirical question.

The available evidence for this sort of intervention are not conclusive. One of the most cited papers is Moehling (1999), who analyses state legislation on the minimum legal age for labour market entry, looking at the experience of the US at the beginning of the 20th century. The author takes advantage of the fact that different states set different minimum legal ages and exploits the variations between them over the first three decades of the century to estimate the impact of legislation on the incidence of child labour. She found no evidence for the effectiveness of these laws as they do not seem to have contributed to the reduction of child labour incidence.

Looking at the effects of compulsory school attendance laws on the incidence of child labour for the period covered by Moehling (1999), Margo and Finegan (1996) conclude that in combination with a compulsory schooling law, minimum age legislation was effective in reducing the proportion of children in the labour force. More recent evidence for the combination of these two laws during the early twentieth century in the US confirms Margo and Finegan’s findings (see Lleras-Muney 2002).

Manacorda (2006) uses US census data from a similar period to investigate whether the minimum age legislation affected time allocation between household members. Unlike Moheling he looks at the 1920's rather than the first decade of that century. Among his main findings is a positive spillover effect of the law on younger siblings, measured as a reduction in the probability of those siblings entering the labour market, and an increase in the likelihood that they would attend school instead. With respect to parents’ labour supply, the study found no effect.

These findings are interesting because they show that laws intended to reduce labour force participation rates among children of a specific age group may have unintended effects. Tyler (2003), for instance, uses the US child labour laws of the 1980s to identify the causal effect of child labour on the academic performance of students in the twelfth grade in 1992. The author finds that 10 hours of weekly work during high school reduced academic performance in maths and reading by 3.6% and 5.1% respectively. The evidence from Brazil shows that the impact of child labour on standardised exams in maths and reading is strong and negative (Bezerra et al 2009).

This paper contributes to the literature in several ways. First, it is the first evidence for the impact of two Brazilian laws approved in December 1998 and December 2000 respectively. Second, it provides causal estimates for both work and schooling outcomes. Finally, unlike the evidence available in the literature so far, it covers a recent period in a developing country.

According to some preliminary estimates, the law of December 1998 reduced children's participation in the labour market among those aged 14 but increased informality among those of 15. Most of the impact seems to be borne by the boys. There is therefore some (weak) evidence that the law increased labour intensity in informal activities among 15-year-olds. As far as school outcomes are concerned, there is some indication of an increase in school attendance but, on the other hand, the law apparently negatively affected the successful grade transition of boys. In this regard, the impacts on children aged 14 and 15 are fairly similar.

Although most of the 15-year-olds who participated in the labour force in 2002 were engaged in informal activities, the law of December 2000 seems to have mitigated the effect of the law of 1998 as its impact on work intensity in formal paid activities was positive and marginally significant. For this age group there is a strong indication of a higher incidence of work in domestic activities. Therefore, most of 15-year-olds prohibited from working in December 1998 apparently divided their time between school, informal and domestic activities.

Finally, the law of 1998 apparently widened the gap in domestic activities between boys and girls aged 14. After being prohibited from participating in the (formal) labour force, girls seem to have had their time allocated to domestic (unpaid) activities more than boys.

The paper is organised as follows. The next section discusses the Brazilian institutional setting and provides the rationale as to how these two laws might affect the children's time allocation. The third section describes the data while the fourth presents the identification strategy and results. Section five analyses whether the laws affected gender gaps, and the final section highlights the main findings and briefly discusses next steps for this research.

2. THE BRAZILIAN LABOUR MARKET: INSTITUTIONAL SETTING

2.1 MINIMUM AGE OF ENTRY TO THE LABOUR MARKET

The Brazilian Constitution of 1988 set the minimum legal age of entry to the labour market at 14, and in 1990 a federal rule named 'The Statute of Children and Adolescents'[1] established children's and youth rights beyond regulating the conditions of entry to the formal labour market. Complementary to the Constitution of 1988, the statute is considered the legal framework for entry to the labour market.[2] From 1988 to November 1998, the minimum legal working age in Brazil was 14 and individuals under 17 were prohibited from working in hazardous activities.

Motivated by modifications to the pension system, the Brazilian Congress then passed Constitutional Amendment No. 20 on 12/16/1998 which increased the minimum legal age for entry to labour market from 14 to 16. Individuals under 17 could work only as apprentices, whereas individuals younger than 18 were prohibited from hazardous and night work.

The law was approved in December 1998 and affected mostly those individuals who turned 14 years old from January 1999 onwards. The law required a transition period up to January 2001 because children aged 14 or 15 before December 1998 who already held a working permit were allowed to keep it.

Since the Ministry of Labour is responsible for issuing working permits, it had to stop issuing them for individuals younger than 16 from December 16th 1998 onwards. For that reason, the statistics for the formal labour force show a significant reduction of formal paid work incidence among youth aged 14 and 15 from January 1999 onwards. From 2001, when the transition period finished, the incidence of formal workers aged 14 and 15 should be zero.

In fact this was not the case. Two years after increasing the legal minimum age the Brazilian President signed law 10,097 on 12/19/2000. This law set up an

[1] Law No.8069 from 07/13/1990.

[2] Although ILO considers as child an individual 15 years old or younger, in Brazil a child is someone aged 12 or less and a youth someone aged 13-18. In this paper, children, teenagers and youth are used interchangeably.

apprenticeship programme that allows individuals aged 14 to 18 to participate in the formal labour market as apprentices.[3] An apprentice is permitted to work part-time and earn half the Brazilian minimum wage. In order to avoid an increase in school dropouts, the law also states that individuals who have not yet finished secondary school must be enrolled in school in order to be able to work as apprentices.[4]

Although the laws do not mandate a direct income transfer, it is expected that they will affect household budget constraint given that to many households could not count on children's earnings any longer. This shift in household budget might lead parents to reallocate the household's consumption bundle of goods and children's leisure. If the income reduction due to the ban is relatively high for a typical household, one could expect an increase in informal work and an ambiguous impact on school attendance and domestic (unpaid) work. If, on the other hand, the household income reduction was relatively small one would expect an increase in unpaid (domestic) work and/or in school attendance. It is impossible to predict which of these two effects will prevail, so the effect of the laws on children's time allocation is an empirical question.

During the period in which the laws were passed another Brazilian programme took place: the conditional cash transfer scheme *Bolsa Escola* (renamed *Bolsa Familia* in 2003). The programme started in 1995 in only two municipalities, Brasilia and Campinas, and by 1998 only 1% of Brazil's municipalities were participating. By April 2001 when the programme was federalised it was reaching about 5 million households. The programme focused on 'poor families with children age 6 to 15 enrolled in school and attending at least 85% of school days' (Glewwe and Kassouf, 2012). Since adolescents of 13, 14 and 15 from poor families were targeted by this programme it might be seen as having a common effect on eligible and non-eligible groups[5].

The empirical analysis starts by looking at the short-run impact of the programme on outcome variables for those directly affected by the laws. The outcomes of interest are the incidence and intensity of child work (general, formal, informal and

[3] In Brazil this law is called *Lei do Menor Aprendiz.*
[4] This Brazilian programme has a lot in common with some European youth employment programmes (see e.g. Brodaty et al., 1999; Caliendro et al., 2011).
[5] See Glewwe and Kassouf (2012) for the causal impact of *Bolsa Escola/Familia* on school outcomes as well as for a description of it. The authors estimated the average treatment effect on school enrollment, dropout rates, and grade promotion and found that the programme increased the first by 6%, reduced the second by half percentage point, and increased the third by 0.6 percentage point. The few empirical papers that looked at the impact of the Brazilian CCT on child labour found relatively small effects even though statistically significant (see e.g. Cardoso and Souza 2004).

domestic); school attendance, and schooling delay. The analysis is conducted for boys and girls separately because the literature demonstrated the need to take into account gender bias within households as well as in the labour market. It also concentrates only on urban areas, motivated by the relatively weak level of enforcement of the law in rural areas (see Basu 1999, Edmonds 2008).

3. DATA

The sample was drawn from the Brazilian household surveys (*Pesquisa Nacional por Amostra de Domicilios* – PNAD) of 1997, 1999, 2001, and 2002. The year 2000 is not included because the Brazilian Census Bureau (Brazilian Institute of Geography and Statistics, IBGE) does not run this survey in census years. The PNAD is an annual household survey that covers around 100,000 households and about 320,000 individuals. It constitutes one of the main sources of microdata in Brazil, and is a nationally representative survey that contains detailed information on each household's socioeconomic characteristics, demographic data, as well as household income and labour force status.

The year 1997 is included as a baseline for the difference-in-differences analysis. The selection of 1997 rather than 1998 is because the household survey data is collected in September of each year by IBGE so that if there were any anticipation bias derived from a pre-announcement of the law of December 1998 the data collected in 1998 would likely be contaminated. Seeking to avoid this problem, most double-difference regressions use the year 1997 as the baseline, though we still use the data for 1998 in a model that controls for pre-treatment difference in trends between eligible and non-eligible groups.

The sub-samples of interest, as already mentioned, are the cohorts of 13, 14, 15 and 16 respectively. The 14-15 cohorts constitute the eligible group in both cases since these individuals had their status changed by the two laws. The other two cohorts are used as control groups because they were not affected by the laws.

This paper defines *child work incidence* as follows.[6] A child is considered a worker if either (s)he worked in the week the survey took place,[7] or (s)he worked in the

[6] Although *child labour* is generally associated with hazardous activity and authors like Edmonds (2008) suggest the term *child work* to refer to non-hazardous work, in this paper both terms will be used interchangeably.

last 12 months, or if (s)he was as an active worker in the week of reference but was prevented from working due to external causes.

The inclusion of work done over the last 12 months follows Kruger and Berthelon (2007) who argue that child work is seasonal and therefore will be underreported if defined exclusively according to work done in the last week. Finally we follow Kassouf (2001) in including in the definition of 'child work' whether the individual was an active worker in the week of reference but could not work for external reasons.[8]

A worker is regarded as formal if (s)he was working with a permit issued by the Brazilian Ministry of Labour.[9] Schooling attendance is denoted by a dummy that equals 1 if the individual stated that (s)he attended school in the last seven days. Schooling delay is a dummy that equals 1 if the distortion age-grade is at least equal to two years. For instance, a child should enter school at age 6 and a kid aged seven should have coursed one year of school. A child is considered delayed in school if (s)he is aged eight and has studied less than one year. A teenager who has never failed a grade should have 11 years of schooling at age 17. Therefore, a youth aged 17 is delayed in school if (s)he studied less than nine years[10].

Table 1 shows work incidence and intensity for boys and girls aged 10-17.

[Table 1 Here]

The table illustrates some interesting patterns. First, that the older the child, the higher the labour force participation rate. Despite this, there is a sharp fall in the incidence of child labour over the period for all cohorts. The greatest reduction was observed for the group of children aged 10, about 38%. There was no difference between boys and girls. For individuals aged 14 and 15, the participation rate in the labour force fell by 31% for boys and 34% for girls.

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7 This includes market work and housework. The PNAD differentiates housework such as food production for own consumption and construction for own use from domestic work. For the first (housework) there is data for the week of reference as well as for the previous 12 months, whereas for domestic work there is data only for the week the survey took place.

8 The PNADs also reports the weekly hours worked. Emerson and Souza (2003, 2007, 2008 and 2011) define a child as worker if (s)he worked any positive number of hours per week.

9 This definition does not include domestic service because in Brazil this job is covered by separate legislation.

10 This definition is based on one of the official definitions used by the Ministry of Education of Brazil (MEC).

As expected, the law of December 1998 substantially reduced the incidence of 14 and 15-year-olds participating in the formal labour market. In 1999, the incidence dropped about 8 percentage points for boys and about 3 points for girls. Due to the transition period however, the incidence of formal work for the group aged 14-15 in 1999 was higher than in 2001 and 2002.

The table also shows that girls work less in paid work activities than boys but do more domestic work. This is in line with the empirical literature which shows that the incidence of work is higher among girls once domestic activities are taken into account (see Edmonds 2008). The high incidence of children aged 10 engaged in domestic work dropped over the period, but was still over 50% in 2002, and about 76% among the subsample of girls.

It is noteworthy the amount of hours worked per week by children aged 10 to 14. Whereas children aged 10 to 13 worked more than 20 hours per week, the intensity of work among children aged 14 is similar to a full-time worker. Most of this time may have been spent in domestic work as the participation rate in domestic work is remarkably high across time and between all cohorts. Interestingly, work intensity in formal paid activities increased only slightly after 2000. This is suggesting that some apprentices are working more than 20 hours per week, the limit set up by the apprenticeship programme.

Given that children's school outcomes and work incidence have to do with their time allocation and therefore have to be thought of as simultaneous decisions taken by their parents,[11] it is worth assessing how school outcomes responded to these two laws.

This analysis is undertaken for two outcomes: school attendance and schooling delay. The first is more commonly used in the literature on the determinants of child labour (e.g. Patrinos and Psacharopolous 1997, Psacharopolous 1997, Jensen and Nielsen 1997) and children's time allocation. The second is suggested by Orazem and Gunnarsson (2003) who argue that it is more appropriate when school attendance is high. As shown by table 2 below, this applies to the Brazilian context.

The literature has reported a trade-off between school attendance and child labour, but these two outcomes are far from perfect substitutes, as has been shown by many studies.[12]

[11] Although we assume that parents are responsible for children's time allocation, this does not necessarily mean that we are assuming an altruistic household model.

[Table 2 Here]

With regard to school outcomes, there is an increase in the already high incidence of school attendance for both boys and girls, with a slight advantage for the latter. Another issue that stands out is that school attendance tends to fall with age, particularly for individuals aged 14 or older. This might be influenced by the Brazilian Compulsory Schooling Law that states that children aged 7 to 14 must be enrolled in school.

Despite high school enrolment, the proportion of children who fail at least one grade is significant even though this has been dropping over the years. This could have been, for instance, either because (1) the higher the proportion of children attending school the more likely they are to have failed a grade, or (2) because the quality of Brazilian schools improved over this period, or (3) because children face difficulties balancing school with work activities. In this case, although work does not seem to displace schooling, it still might affect a successful grade transition as it does academic performance.

One of the contributions of this paper consists of showing whether and how the 1998 and 2000 laws affected these trends. Given that Moehling (1999) showed that the reduction of children's participation in the US labour force during the 1920s was more due to a time trend than to the legislation itself, this analysis aims to verify whether this was the case in Brazil.

4. METHODOLOGY: LOOKING FOR NATURAL EXPERIMENTS

Two methodologies are used to identify the impact of the laws on the outcomes of interest. First, the regression discontinuity design (RDD) is applied to the identification of the local average treatment effect (LATE) on the compliers – the subsample of teenagers aged 14-15 who decided to participate in the labour market exclusively as formal workers (see Angrist and Imbens 1994, and Imbens, Angrist and Rubin 1996).

[12] Ravallion and Wodon (2000) show that an exogenous reduction in the price of school in Bangladesh increased school attendance and reduced child labour, but only marginally. This finding leads them to argue that child labour does not displace schooling. However, Tyler (2003) shows that in the US students who worked during the twelfth grade performed worse in maths and reading exams. Obviously this might not hold in countries with poor school quality.

Second, we use the difference-in-differences approach (DD) around the cutoff to check whether the law affected boys and girls differently. In this case, the treatment dummy will be equal to one for boys just over 14 and zero for girls. The difference between boys and girls on the right of the cutoff provides the first difference. The counterfactual is given by the difference in outcomes of boys and girls just under 14 and therefore corresponds to the second difference.

The identification of the causal impact of the law in the RDD framework depends on a discontinuity in the probability of teenagers working formally while aged 14 and 15. According to Imbens and Lemieux (2008) the regression discontinuity analysis should start with a visual check. The figures below show whether the laws created any discontinuity in the incidence of formal labour force among individuals aged 14-15.

Starting with the law of December 1998, figures A.1 to A.3 use data from 1997 to illustrate the incidence of formal paid work among individuals 14 and 15 years old before the law passed. This analysis is therefore based on the PNADs of 1997. Figures A.4 to A.6 replicate the exercise using data from 1999 to capture what happened after the law, whereas figures A.7 to A.9 do the same for 2002, almost two years after the law was passed.

[Figures A.1 to A.9 Here]

All the empirical analysis is done in urban areas of metropolitan zones only. This is to avoid contamination bias from (i) lower enforcement in rural areas, (ii) lower incidence of formal workers in rural areas, and (iii) any sort of bias due to cash transfer programs designed for rural children in particular[13]. Apart from that, all regressions use the sample weighting due to the relatively small number of observations close to the cutoff points.

As can be seen, there is small but positive and significant jump in formal work incidence around the cutoff in 1997, but this result seems to hold only for boys. In 1999 the discontinuity disappears, suggesting that the law of 1998 was effectively enforced.

[13] In 1996 Brazil implemented an unconditional cash transfer programme aimed at eradicating child labour in rural areas. The progamme was called *Programa de Erradicação do Trabalho Infantil* (PETI), and in 2003 it was integrated to the Brazilian conditional cash transfer programme *Bolsa Familia* (Yap et al. 2002).

Looking at the figures from 2002, one can see a very small discontinuity around the cutoff, although zero is inside the confidence interval of 95%. This small effect of the 2000 law could be due to (1) the relatively recent new legislation allowing 14-15-year-olds to participate in the labour market as apprentices, (2) the relatively weak incentives generated by the law, since individuals aged 14-15 could work only part-time and earn half the minimum wage. Some households, even given credit constraints, may not wish send their children to informal work and therefore reallocate their time to domestic activities in order to allow the adults to dedicate more time to paid activities.

The laws appear to have affected work incidence as a whole. In 1997 boys aged 14-15 used to work more than younger ones and the difference was statistically significant at 5%. In 1999 though, the difference disappeared. This suggests that by prohibiting work in the formal labour market, the law of 1998 also led to a drop in the participation of 14-15-year-olds in the labour force.

On the other hand, the law of 2000 apparently brought a small proportion of this cohort of boys back into the labour force. Figures A.10 to A.18 show how these laws might have affected children work incidence with ages close to the cutoff.

[Figures A.10 to A.18 Here]

Looking at school attendance over the period, it seems that boys tend to trade off additional (formal) work with less schooling. This could be either (1) because parents over-weight returns to experience versus returns to education, or (2) that parents are credit constrained and therefore send boys into the labour market so that they can pay back in the future by supporting them when they get old, or even (3) that parents are myopic and hence do not internalise all the benefits of investing in their children's human capital (these motives are thoroughly discussed in Edmonds 2008).

The analysis of school outcomes is consistent with the empirical literature as well. Thus the descriptive analysis points to some gender effects, i.e., the laws apparently affected boys and girls differently.

[Figures A.19 to A.27 Here]

Figures A.19 to A.27 show that a small but probably significant reduction in school attendance for boys followed the ban of 1998. No effect, though, was observed for girls. The question is: if the girls stopped formal work due to the ban, how did they (or their parents) re-allocate their time? At first sight, boys seem to attend school less than girls and to enter the labour force sooner. The incidence of domestic activities does not seem to respond to this time re-allocation given that no discontinuity regarding this activity occurred. Thus, one could ask how those children reacted to the ban policy, and how the 1998 law might have altered their time allocation. The next sections address these questions empirically.

4.1 IDENTIFICATION STRATEGY

The identification strategy is based on the discontinuities illustrated in the figures A.1 to A.27. Based on those, the impact of the laws can be estimated through the regression discontinuity design technique (RD).

This approach depends on an assignment to the treatment variable that breaks the sample into two groups: one eligible to take up the treatment and one non-eligible (the control group). The RD is a quasi-experimental approach that mimics a random experimental design. In the RD context, the sample of treated and control groups is naturally split by a (supposedly) exogenous intervention, such as a rule that allows a group of individuals to participate in a programme due to, for instance, their age. The identification assumption is that, on average, these two groups are very similar in unobservable characteristics and the only difference between them is that one can access treatment while the other cannot.[14]

In cases where all those eligible for the treatment access it, the discontinuity designed is known as 'sharp'. When only a subsample of the eligible group decides to take up the treatment it is called 'fuzzy'. The subgroup that participates in a programme due to the selection rule is named *compliers* (see e.g. Angrist and Imbens 1994, and Imbens, Angrist and Rubin 1996). The usual assumption is that without the selection rule, the group would not be interested in participating in the programme (for the similarities between IV and RD approaches, see Imbens and Lemieux 2008 and van der Klaauw 2008).

When the group of compliers is identified, and assuming a binary treatment variable, the so-called Wald estimate is obtained by dividing the impact of the eligibility rule on the outcome of interest (the *intent-to-treat* estimator) by the proportion of the eligible group who took up the treatment. The Wald estimator can be seen as an IV estimator and thus can be estimated in two steps. The first step consists of a regression

[14] The special issue of Journal of Econometrics (2008) on RD design contains applications of this framework on a diverse set of subjects.

of the treatment variable (*X*) on the assignment to the treatment variable (*Z*). Let $\hat{\beta}_z$ be the effect of *Z* on *X*. The second step is given by a regression of the outcome *Y* on the *Z*. Let $\hat{\beta}_{itt}$ be the estimate of the effect of *Z* on *Y*. The Wald estimator is given the ratio $\hat{\beta}_{itt} \big/ \hat{\beta}_z$. In the IV framework, the identification of the Wald estimator depends on a non-zero correlation between *Z* and *X*, and a zero correlation between *Z* and the error term of the outcome equation.

Unlike the standard IV, the identification of the treatment effect via RDD does not require zero correlation between Z and the error term of the outcome equation. All that is required is that the assignment variable be continuous at the cutoff (for instance, in $Z = Z_0$, with Z_0 defining the cutoff) (see Hahn, Todd and Van der Klaauw, 2001).

For example, in the present context, the assumption is that those individuals aged 14-15 who stopped working formally after December 1998 did so exclusively because of the law approved that month. For the same token, those who entered the formal labour market after December 2000 did so because they were allowed to by the law.

Hahn et al. (2001) were the first to theoretically systematise the RDD estimators as Local Wald versions of the aforementioned IV. Like Imbens and Angrist (1994), they refer to the Wald estimator as a local average treatment effect since this framework identifies the impact only for the subgroup of the compliers. The authors show that under sharp design the treatment variable *X* is a deterministic function of *Z*, and $X = f(Z)$ is discontinuous in some observable values of *Z*, i.e. Z_0. Defining the observed outcome model as $Y_i = \alpha_i + X_i \beta_i$, and assuming that:

(1) The limits $X^+ \equiv \lim_{z \to z_0^+} E[X_i \mid Z_i = Z]$ and $X^- \equiv \lim_{z \to z_0^-} E[X_i \mid Z_i = Z]$ exist, with $X^+ \neq X^-$; and

(2) $E[\alpha_i \mid Z_i = Z]$ is continuous in Z at Z_0 such that for an arbitrary small $e > 0$, $E[\alpha_i \mid Z_i = Z_0 + e] \cong E[\alpha_i \mid Z_i = Z_0 - e]$

Then the (local) treatment effect in a sharp design is given by:

$\beta_{sharp} = \dfrac{Y^+ - Y^-}{X^+ - X^-} = Y^+ - Y^-$, since $X^+ = 1$ and $X^- = 0$. Y^+ and Y^- are defined similarly to X^+ and X^-.

In the fuzzy design, X_i is a random variable given Z_i and the conditional probability $X = f(Z) = \Pr[X_i = 1 | Z_i = Z]$ is known to be discontinuous in Z_0. Thus the only difference between the sharp and fuzzy estimators is that for the later $X^+ \neq 1$ and $X^- \neq 0$, i.e., 'there are additional variables unobserved by the econometrician that determine assignment to the treatment' (Hahn et al. 2001, p.202). So, the treatment effect in a fuzzy design is given by:

$$\beta_{fuzzy} = \frac{Y^+ - Y^-}{X^+ - X^-}.$$

Although the sharp and fuzzy estimators identify only the local average treatment effect – the treatment effect for the individuals close to the cutoff – Hahn et al. (2001) note that this method has many advantages when compared to other quasi-experimental approaches in that it does not depend on functional form assumptions and does not require identifying instruments, or the set of variables that affect the selection rule for a particular programme (or treatment).

The laws investigated in this paper affected the eligibility of individuals aged 14 and 15 to participate in the formal labour market. Thus the laws gave rise two fuzzy designs.[15] Note that even under a sharp design the law of 1998 would have to be treated as a fuzzy design due to the transition period. The aforementioned accommodation period created some leakage in that some individuals who turned 14 shortly before the law passed could ask for a work permit and hence participate in the formal labour market in 1999.[16]

A complementary exercise is undertaken comparing teenagers just under 16 with teenagers just over. We do not expect to find a discontinuity before the law passed but expect some discontinuity after it passed. The problem with this exercise is that the accommodation period lasted two years. Thus the discontinuity may not be convincing or statistically significant using the data of 1999, less than one year after the law's implementation. The ideal scenario would be to look for discontinuity using data from 2000 or later. However, while 2000 was a census year, the analysis in 2001 could be jeopardised by the allowance law of December 2000. Unless the work participation or

[15] Since the assignment to the treatment is exclusively based on the age variable, any manipulation that could compromise the internal validity of the Wald estimate via RD is not an issue of concern in the present case.

[16] As long as the law of 1998 set the cutoff in 16, the value of X^- would be different of zero.

work intensity is different enough between the ages of 15 and 16, we do not expect to find a statistically significant LATE when comparing these two age groups.

The effect of the laws can be estimated parametrically using OLS to fit the following reduced form regression model:

$$y_i = \alpha + \delta T_i + \tau T_i Z + \sum_{i=1}^{6} \beta_i Z^i + \varepsilon_i \qquad (1)$$

where *y* is the outcome of interest of individual *i*, *T* is the treatment variable that takes value 1 for individuals aged 14 or older, and *Z* is the assignment variable that defines an observable cutoff, in this case when an individual is aged 14 (or 16).[17] Z is defined such that it takes the value of 0 when the age is lower than 14 and is higher or equal to 0 when it is above 14. Since the law makes Z orthogonal to the error term, there is no need for control variables. Estimates are provided for three outcomes related to work incidence – incidence of child work, incidence of formal child work, and incidence of domestic work – and two related to school outcomes– attendance and delay. The parameter of interest is the coefficient of the dummy *T*, δ .

In this example the functional form is specified as a polynomial of order six.[18] The advantage of having a high-order polynomial is that it improves the local adjustment, thus reducing the bias. However, this strategy increases the variance. The caveat that underlies this approach has to do with the choice of the bandwidth as well as with the specification of the model itself – whether or not include interaction terms, the order of the polynomial, etc.

Some *ad hoc* preliminary attempts suggested that the results were very sensitive to the model specification and to the interval of the bandwidth. Given the difficulty of dealing with both of these issues, one opted to run the model using the non-parametric procedure.[19] This approach estimates the LATE by fitting a local linear regression on both sides of the cutoff. A triangle kernel is used as the weighted function and the

[17] The variable Z is equal to (age-14), and it is defined in a way that it takes value 0 (14) in the month the survey took place, i.e., in September of each year. The week the survey takes place is the last of September. When comparing the 15 and 16-year-old age groups, Z is given by age16.
[18] van der Klaauw (2002) proposes a semi-parametric procedure for the selection of the polynomial order. The author suggests the cross-validation technique to select the optimal polynomial order.
[19] van der Klaauw (2008) provides a comprehensive discussion about the critical role the functional form specification in a parametric framework plays for the consistency of the LATE estimate in RD design.

bandwidth is optimally selected to minimise the mean squared error (MSE) in accordance with the Imbens and Kalyanaraman's (2009) algorithm.[20]

Given that the point estimates are sensitive to the bandwidth choice, the model is fitted with three bandwidth options: the preferred option (the optimal), twice the preferred option (lower variance), and half of the preferred option (lower bias).

4.2 DIFFERENCE-IN-DIFFERENCES FRAMEWORK

Both the RDD and DD estimations are performed by comparing individuals just below and just above the ages of 14 and 16 respectively. In a first set of estimates, children aged 13 and 14 are compared whereas in a second set the comparison is between children aged 15 and 16.

Due to the transition period required by the law of 1998 individuals under 16 can still be found in formal work in 1999. For this reason, the estimates for this period can be considered the lower-bound effect of the law due to the attenuation bias generated by the transition period.

It is important to remember that the Brazilian compulsory schooling law requires individuals aged 7 to 14 be enrolled in school. Since individuals who are at least 15 are allowed to drop out of school without any sanction, the ATT and ITT estimates try to avoid the contamination coming from this rule by comparing age 13 to 14, and age 15 to 16. Splitting the eligible group in two helps mitigate the risk of both contamination and attenuation bias.

The impact of the 2000 law will be estimated only for the cohorts aged 15 and 16 because the number of formal workers (apprentices) aged 14 is very small in 2001 compared to individuals belonging to the same age group in 1999. This mismatch results from the transition period that followed the law of 1998. Thus the analysis for labour force participation in formal activities among youths aged 14 over the period 1999 and 2001 would render a negative coefficient for the impact of the apprenticeship programme. This result could sound counterintuitive as one is comparing individuals aged 14 with individuals who could never hold a work permit (those 13 years old). To

[20] The triangle kernel is the optimal choice for boundary estimation. The procedure is done using the command *rd* in STATA. See Nichols 2007.

avoid this problem, the DD analysis for the impact of the law of 2000 is performed only for individuals aged 15 and 16.

The identification strategy for the DD depends on two assumptions: (1) the difference in labour force participation between the eligible and control groups exists in level but not in difference, i.e., that the groups would evolve in parallel in the absence of the law. This is a key assumption in the DD framework, and in the present case might be even stronger since one is not comparing individuals in the same age-groups;[21] and (2) all unobservables that could be correlated with the eligibility or other covariates are additive and time-invariant.[22]

The estimation of the impact of the law of 1998 on the outcomes of interest is conducted through the following linear probability model:

$$Y_{it} = \beta_0 + X_{it}^{'}\beta_1 + \beta_2 Z_{ti} + \beta_3 D_t + \alpha_{DD}\left(Z_{it} D_t\right) + u_{it} \qquad (1)$$

where Y_{it} is a dummy variable equal to 1 if the *i-th* individual participates in the labour market and 0 otherwise. X_{it} is the vector of observable characteristics which change through time and includes ethnicity, parents' educational level and age, family composition, a dummy indicating if the household owns a land title, the monthly non-labour income, and dummy variables for regions and the metropolitan region. Z_{it} is a dummy variable that equals 1 if the *i-th* individual is aged 14 (16) and zero if (s)he is aged 13 (15), D_t is a *dummy* variable equal 0 before the law was passed and equal to 1 after that, and u_{it} denotes the error term, which is assumed to be independent of *X* and *T* (see Meyer 1995, Blundell and Dias 2002, and Ravallion 2005). [23]

The inclusion of the land title ownership is to control for household credit constraint. The ownership of land title might be a proxy for wealth (collateral) and therefore to allow a household to access the credit market. The rationale for the

[21] Abadie (2005), for instance, argues that one could match the groups in the baseline (in our case 1997) when there is reason to believe that the group trends would not be parallel in the absence of the law. Although this approach cannot be implemented in this study because the estimation is performed with cohorts in two different periods rather than with the same individuals, figures 1 to 3 show that the compared cohorts evolved in parallel before the law passed. For the difference-in-difference matching estimator see also Heckman et al. (1997) and Blundell and Dias (2002).

[22] This second assumption is relevant in the present context only if it is assumed that individuals from different cohorts have, on average, the same distribution of time invariant unobservables characteristics.

[23] To check robustness the double difference regression is run with 1998 and 1999 as the law passed in December of 1998 and the survey takes place in Sept of each year. This is done to check whether there is any indication of anticipation bias.

inclusion of non-labour income rather than household income is that the latter is more likely to be endogenous since it depends on the labour supply allocation of household members.[24]

The parameter of interest is the coefficient of the interaction term $Z_{it} * D_t$, α_{DD}, which identifies the average treatment effect on the treated (ATT).[25] The analysis is performed in urban areas only, for a pooled sample of boys and girls, and separately by gender. Table 3 contains the descriptive statistics for the groups used in the DD analysis and allows us to compare ages 13 to 14 and 15 to 16 in terms of outcome variables and a set of covariates. This simple comparison of means is informative mainly regarding the outcome variables, as most of the DD estimates more or less confirm these differences. Starting with the outcome variables, it can be seen that while labour force participation increases with age, school attendance and successful school progress drop as individuals get older. This distinct pattern in time allocation between children aged 13 and 14 may reflect not only the age effect but the effect of the minimum age legislation. As can be inferred from the table, the higher labour force participation of children of 14 is negatively correlated with school outcomes. Thus although work may not fully crowd out education there seems to be some trade-off between these two activities.

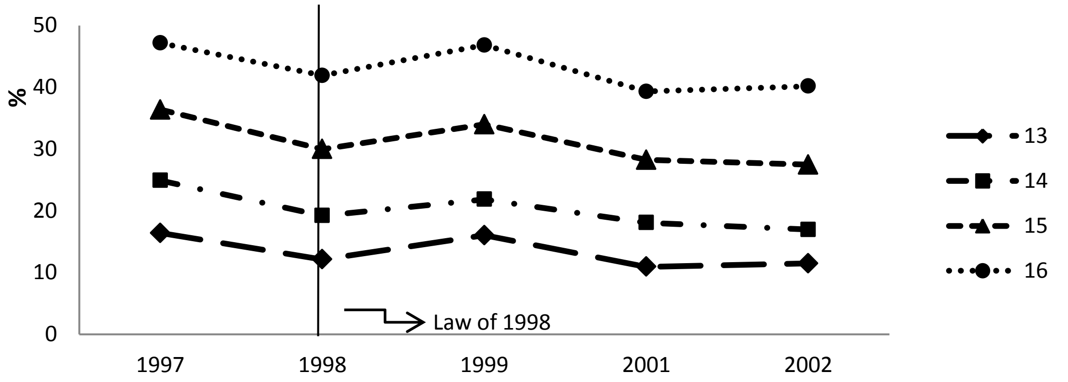
[Table 3 Here]

Despite the differences detected in some outcomes, the similarity in the covariates suggests that the two sub-samples of eligible and non-eligible groups are very well-balanced in observable characteristics. An untestable assumption that guarantees the consistency of RDD estimates is that children close enough to the cutoffs have similar distribution of unobservable characteristics. Note that the law was a random event and by controlling for pre-treatment trends the DD approach minimises even further any potential bias coming from unobservables.

[24] Orazem and Gunnarsson (2003) provide the rationale for the list of control variables that should be included in the child labour and schooling outcome regressions.

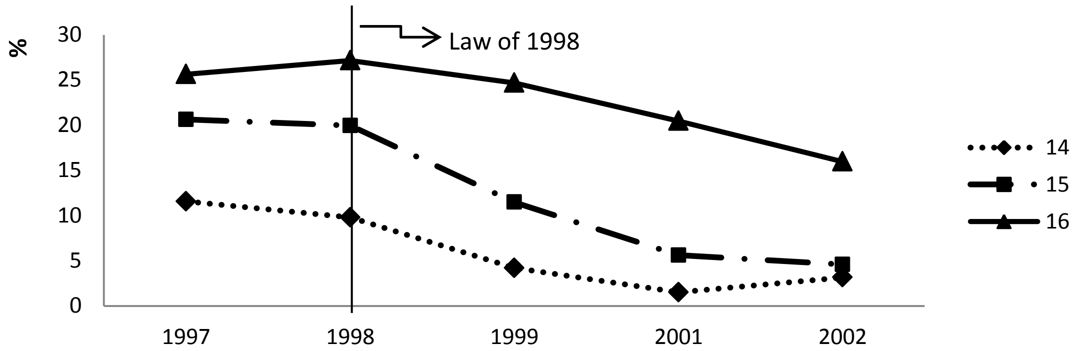
[25] For the outcomes other than formal paid work the parameter of interest is the intent-to-treat (ITT) since the analysis is performed for all individuals 14-15 years old (the eligible to take the treatment), not necessarily for those working formally (treated). For a detailed discussion on this issue, see Heckman, Lalonde and Smith (1999) and Duflo et al. (2007).

Figure 1 – Trends in Work Incidence, Different Cohorts



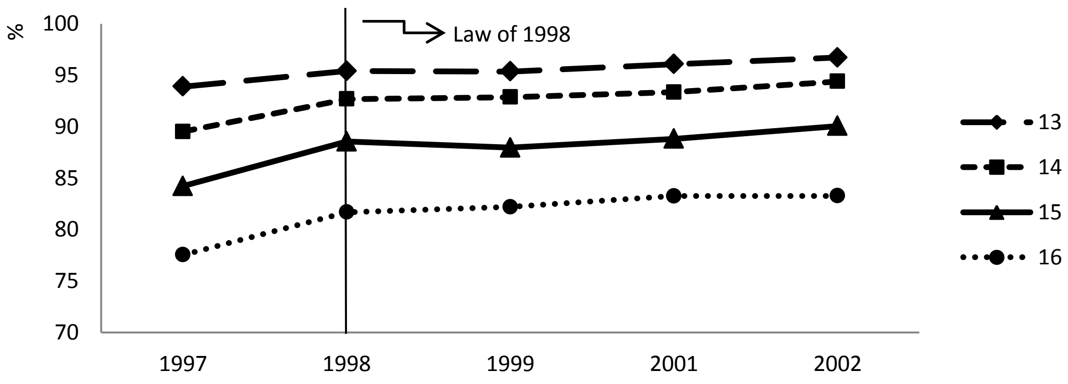
Source: PNADs of 1997, 1998, 1999, 2001, and 2002.

Figure 2 – Trends in Formal Paid Work Incidence, Different Cohorts



Source: PNADs of 1997, 1998, 1999, 2001, and 2002.

Figure 3 – Trends in School Attendance, Different Cohorts



Source: PNADs of 1997, 1998, 1999, 2001, and 2002

Except for the trends concerning the participation rate in the formal labour market, all groups share very similar trends. This suggests that the control groups are good counterfactuals for what would have happened to individuals aged 14 and 15 in the absence of the law.

The difference in trends in formal labour market participation raises concerns about the reliability of the DD approach. To take these different trends into account we control for pre-law difference trends between the groups in one of the models. We can anticipate that this term was not statistically significant, suggesting that the pre-law difference level was not different in statistical terms.

4.3 (Preliminary) RDD Results

Work Incidence

Tables 4 and 5 report the non-parametric reduced form estimates for labour force participation, and work incidence in formal, informal and domestic activities comparing children aged 13 and 14, and children aged 15 and 16. All estimates are performed for the subsample of children from 13 to 16 years old who live in urban areas of metropolitan zones.

[Tables 4 and 5 Here]

According to table 4, in 1997 children aged 13 and 14 had very similar labour force participation rates. In 1999 though, the ban led to a reduction in labour force participation rate of about 5 percentage points (pp.) for the pooled sample of boys and girls. The effect is about 7 pp. for boys, and about 4 pp. for girls, though the coefficients for boys are more precisely estimated. Interestingly, the magnitude of the point estimates does not seem to be very sensitive to the bandwidth choices. The law of 2002 does not seem to have had any impact on work incidence, although the coefficients were positive, particularly for boys, in most of the cases.

Since both laws were designed to affect work participation in formal paid activities, the effectiveness of these laws has to be assessed in that regard. In 1997 children aged 14 were about 13 pp. more likely to participate in formal paid work activities than children about to turn 14. The impact is even higher among boys, though not statistically significant. Consequently, teenagers in this age group were less likely to work informally and in domestic activities. In fact, the coefficients for work participation in informal activities are exactly the inverse of those for participation in formal paid activities.

The point estimates for domestic work were about -6 pp. and statistically significant for the pooled sample and for girls. In 1999 all differences in work participation shrank. It is worth mentioning though the magnitude of the coefficients for domestic work, mainly among girls. Although statistically insignificant, the point estimates approached -13 pp.

With the apprenticeship programme of December 2000, children's participation rate in formal paid work increased remarkably by 19 pp. among children aged 14 or older. Given the relatively small sample size of boys and girls engaged in this programme in 2002, decomposed estimates by gender are not reported. None of the coefficients for work incidence in domestic activities are statistically significant.

With regard to participation rate in informal work, the coefficients are negative and statistically significant particularly for girls, suggesting that the law of December 2000 reduced girls' participation in informal work by 5 percentage points. These estimates are quite insensitive to the bandwidth size.

Table 5 shows some similar patterns. Before the law of December 1998, children aged 15 and 16 could work, which explains why there is no difference in participation rates in formal paid activities in 1997, although there is a weak and imprecise indication that individuals aged 16 were less likely to participate in both labour force and informal activities than children close to turning 16.

Although not very precisely estimated, the coefficients for domestic work are interesting as they suggest that boys aged 16 used to be less likely to do domestic work than boys of 15, whereas for girls the coefficients show the opposite, i.e., a higher incidence in domestic work among girls of 16.

After December 1998 children of 16 became more likely to participate in the formal labour market than those aged 15, particularly girls, however the coefficients are statistically insignificant in almost all regressions. This lack of significance may have been influenced by the transition period discussed above. That accommodation period explains why individuals of 15 are observed working in September of 1999, when the survey was collected. Thus, with the point estimated biased downward, the T-statistics are in fact lower than they would be in the absence of the transition period.

One could expect that by allowing 15-year–olds to participate in formal work as apprentices, the apprenticeship programme of 2000 would substantially shrink the difference in labour force participation rates between teenagers just below and just over the age of 16. The estimates using data from 2002 show mixed evidence that children just over 16 were more likely to work in the formal sector. It is interesting to note that the coefficients were only negative for girls, suggesting that the programme did not attract many boys.

Work Intensity

The last two columns of tables 4 and 5 show the impact of the laws on the intensity of children labour supply. According to table 4, none of the point estimates, for formal or for informal work, are statistically significant. On the other hand, after the ban of 1998 children aged 14, particularly girls, started working more intensely in informal activities. The point estimates suggest that the law increased children's work intensity in informal activities by about 10 hours per week, and for girls the effect approached 17 hours per week. This is consistent with the apparent fall in participation rate in domestic work after December 1998.

In 2002 the coefficients suggest a decrease in weekly hours worked in informal activities, but they are very imprecisely estimated. The coefficients for hours worked in the formal sector have the expected sign and are statistically significant for the pooled sample of boys and girls. The point estimates are very sensitive to the bandwidth size, and point to an increase of about 8 weekly hours worked.

The comparison between children of 15 and 16 does not indicate any impact of the apprenticeship programme on the work intensity of those aged 15. Overall, these reduced form estimates suggest that the law of 2000 impacted more children just under 16 than just over 14 (see table 5).

School Outcomes

Tables 6 and 7 present the non-parametric reduced form estimates regarding school attendance and schooling delay for the two eligible and control groups.

[Tables 6 and 7 Here]

In 1997 there was no statistically significant difference in school attendance between individuals just under and just over the age of 14. In 1999 however, the ban appears to have increased the school cost faced by the boys, although the coefficient is not statistically significant.

It is worth noting the sharp fall in schooling delay among boys and girls between 1997 and 2002 where the point estimates more than halved. This might have been because the ban permitted girls to spend more time studying for their exams. Interestingly, this drop in schooling delay is not linked to an increase in school attendance. In 1997 and 1999 girls apparently suffered more than boys with schooling delay. Considering the impact of the law of 1998 on girls' time allocation, this result is consistent with the fact that girls started working more intensely in informal sector (see table 4).

The estimates for schooling delay in 2002 are even lower in absolute terms than in 1999, suggesting that boys and girls are performing better in school. Unlike the previous years though, schooling delay was higher among boys than girls. Children aged 14, both boys and girls, were about 11 pp. more likely to fail a grade than those close to turning 14.

It is not easy to identify the reasons for these huge jumps in schooling delays after age 14. On one hand, one could argue that the 14-year-old generation were working more in 2002 than before, which is consistent with the growth in work incidence and intensity in formal paid activities verified in 2002. Another possibility could be that the quality of the Brazilian schools increased over the period, which is unlikely, or even that the returns to extra years of schooling were not considered worthwhile for many children from poor backgrounds. The latter argument is widespread used in the CCT literature as part of the rationale of why conditionality is a key component in these social protection programmes[26]. Whatever is the explanation, the evidences point to some trade-off between child labour and grade transition, even though schooling delay fell remarkably over the period.

Table 7 shows the school outcomes for children aged 15 and 16. The ban seems to have increased school attendance among boys close to turning 16 by about 7 pp. compared to boys of 16. The point estimates are very insensitive to the bandwidth size. Although all coefficients for schooling delay are high are statistically significant, they are very similar over the period under study. Therefore for this age group the schooling delay may be better explained by an age effect than by the laws themselves. It is important to bear in mind that these coefficients might be upwards biased because children aged 16 are no longer constrained by the compulsory schooling law. The difference-in-difference estimates will shed extra light on the impact of the laws as they take into consideration both *pre* and *post* treatment periods.

[26] See Fizbein and Schady (2009).

4.4 (Preliminary) Difference-in-Differences Results

The Impact of the 1998Law: 13 vs. 14

Table 8 reports the DD estimates for work outcomes for children aged 13 and 14. It is worth bearing in mind that these estimates represent the lower bound effect of the 1998 law as long as there is attenuation bias implied by the transition period.

[Table 8 Here]

As shown in the table, the ITT estimate for work incidence is negative and statistically significant at 5% for the pooled sample. It suggests that, on average, the law reduced work incidence among children aged 14 by 2.5 pp. When estimated for boys and girls separately the results suggest that the law was effective in reducing boys' work participation only. The impact on boys was 4.3 pp. and significant at 5% whereas for girls it was statistically insignificant.

The effect of the law on the incidence of formal paid work is quite high and significant for boys only. On average, the law caused a fall of 10 pp. in the participation rate. The effect on girls was indistinguishably different from zero. This might be explained by the lower labour force participation among girls in the baseline. These results suggest that the law was effectively enforced. No effect was observed for participation in informal activities. It is also worth noting the similarity between these point estimates and the reduced form RDD estimates.

Table 9 contains the estimates for work intensity. The dependent variable is weekly hours worked. The work intensity in formal activities reduced by 3.5 hours per week among boys aged 14 after December 1998. Although the coefficients for work intensity in informal work activities are positive, none of them are statistically significant.

[Table 9 Here]

The next table shows the ITT estimates for school outcomes.

[Table 10 Here]

The first column of this table shows the results regarding school attendance for the pooled sample of boys and girls. As can be seen, the law seems to have caused an increase in school attendance of about 1.8 pp. in comparison to the control group. Boys seem to be slightly more likely to attend school than girls, although the difference between the coefficients is not statistically significant. This is consistent with the finding that the law reduced boys' work incidence only. As this result suggests there seems to be some trade-off between work and school activities for the boys.

Apparently boys paid a price for attending school more as they became more likely to fail a grade. This might be because boys tend to enter the labour force sooner than girls, but it could also be that boys (or parents) tend to give more importance to the returns to experience than to the returns to school. The coefficients of the other covariates have the expected sign.[27]

When the sample is split between boys and girls the pattern remains very much the same. The point estimates remain very similar even when the composition of the groups are changed to try to isolate the effect of the law from other possible confounders. The main difference lies in the coefficient of girls' school outcomes, which is not statistically significant. Tables 11, 12 and 13 show the results for work incidence, work intensity and school outcomes of children aged 13 and 14. The difference though is that this time the data are from 1997 and 1998 in order to check whether there is any evidence of anticipation bias before the law was approved. Since the coefficients of interest are not significant, apart from schooling delay, there does not seem to be any evidence of anticipation bias.

[Tables 11-13 Here]

The Impact of the 1998 Law: 15 vs. 16

When the analysis turns to children 15 and 16 years old, it is possible to control for pre-treatment differences in the eligible and non-eligible groups' trends by estimating the following regression model:

$$Y_{it} = \beta_0 + X_{it}^{'}\beta_1 + \beta_2 Z_{ti} + \beta_3 D_{98} + \beta_4 D_{99} + \delta Z_{it} D_{98} + \alpha_{DD}\left(Z_{it} D_{99}\right) + u_{it} \qquad (2)$$

where D_{98} is a year dummy that equals to 1 in 1998, and zero in 1997 and 1999, D_{99} is a year dummy that takes value 1 in 1999 and zero otherwise, and the coefficient δ captures any pre-treatment difference in groups' trends. This coefficient should be insignificant in the absence of pre-treatment trend difference between the groups. Table 14 reports the DD estimates for work outcomes. The results are very close to those discussed previously for children aged 13 and 14, though this time a substantial and statistically significant effect is observed among girls, along with some effect regarding work participation in informal activities. Individuals aged 15 are 8.5 pp. less likely to

[27] Not discussed here to save space.

work as formal because of the law of 1998. The coefficient for informal work is large and significant at 5% against a one-sided alternative. Apparently the law of 1998 led children aged 15 who were prohibited to work in the formal labour market into informality. The crowding out effect of the law is very clear among boys and quite clear among girls. This is a very interesting finding as it points to the unintended consequences of the legislation, i.e. potentially negative effects among a sub-group of children aged 15 – those who wanted to work formally but could not do so due to the ban– and for the whole economy as it stimulated informality among a group of teenagers. Moreover, given the almost perfect crowding out effect, the ITT estimates for work incidence in informal activities is very similar to the local average treatment effect. If this is the case, the LATE becomes close to the ATE.

[Table 14 Here]

The coefficient capturing the pre-treatment trend difference is insignificant, suggesting that both groups were following similar trends before the law was passed. This new set of results supports the argument that the law was effectively enforced and that the group of individuals aged 16 is showing us what would have happened with the eligible group prohibited from participating in the formal labour market in the absence of the law.

Table 15 contains the estimates for work intensity. As with the previous analysis for 13 and 14-year-olds, the effect on boys seems to be driving the results for the pooled sample. Work intensity in formal activities reduced by 4 hours per week among boys aged 15, very similarly to children aged 14. Although the coefficients for work intensity in informal work activities are positive, they are statistically significant. When the sample of boys and girls are pooled, the coefficient of 2.2 weekly hours worked is barely significant at 10% against a one-sided alternative.

[Table 15 Here]

Table 16 shows the ITT estimates regarding schooling outcomes.

[Table 16 Here]

The estimates for school attendance are relatively similar to those observed for children aged 14, but slightly higher. The DD coefficient suggests that the 1998 law caused a reduction of 3.5 pp. in school attendance among boys aged 15. The coefficient for pre-treatment effect has exactly the same magnitude and is statistically significant at 5%. Although this may be consistent with the previous finding that there seem to be some anticipation bias going on, this result is puzzling as one could expect children of

15 to attend more school than those of 16 due to the age effect and because of a time reallocation motivated by the ban. Consistent with this finding, there is no statistically significant effect on schooling delay.

The Impact of the 2000 Law: 16 vs. 15

Table 17 presents the estimates for the effect of the law of December 2000 on work incidence outcomes. The DD coefficients point to a positive but insignificant effect of the law on formality. Only the coefficient for the pooled sample is statistically significant at 10% against a one-sided alternative.

[Table 17 Here]

On the other hand, the point estimates for the other outcomes are very precisely estimated. The 2000 law seems to have boosted work participation among both boys and girls, particularly in informal activities which might appear puzzling at first sight. However, when it is taken into consideration that apprentices could work only part time and should attend school, it is not so surprising that the “take-up” of the treatment was not high among teenagers who could not afford to work for only half the Brazilian minimum wage.

In other words, the apprenticeship programme may not pay off for many children who cannot reconcile work and schooling. In this case, the ban policy that increased the legal minimum age of full time work in formal labour market from 14 to 16 overrides the 2000 law, which explains the high percentage of individuals aged 15 participating in informal work activities.

Maybe as consequence of the law of 1998, many children aged 15 opted to help at home by doing some domestic work instead of engaging in the apprenticeship programme. The point estimates show an increase of about 9 pp. in domestic work incidence for both boys and girls.

Interestingly, the ITT estimates show an impact on work intensity in formal activities despite the weak indication of impact on work incidence. According to table 15, the 2000 law caused an increase of about 1.7 hours per week in formal activities among youth aged 15 and the coefficient is statistically significant at 5% against a one-sided alternative (or 10% against a two-sided alternative). The impact is higher among boys, reaching 2.1 hours per week, and is statistically significant at 10% against a one-sided alternative.

[Table 18 Here]

It is interesting to observe the almost perfect shift away from weekly hours worked in informal activities. The law of 2000 appears to have contributed to reducing work intensity in the informal labour market by 2.9 hours per week among boys, marginally counterbalancing the huge effect on informality of the 1998 law as shown in table 14. Although the DD coefficient has a similar magnitude for boys and girls, it is estimated precisely only for boys.

Almost no effect is detected for school outcomes among children aged 15 as shown in table 10. In fact, the only coefficient that is statistically significant is the impact on schooling delay of boys, which apparently increased by 5 pp. due to the 2000law. This could sound surprising since the apprenticeship programme conditions children's school enrolment in cases where they have not finished secondary school. However, programme eligibility rule conditions enrolment but is unclear with regard to grade repetition. Thus it is possible that children, and boys in particular, are enrolled but do not prioritise grade progression.

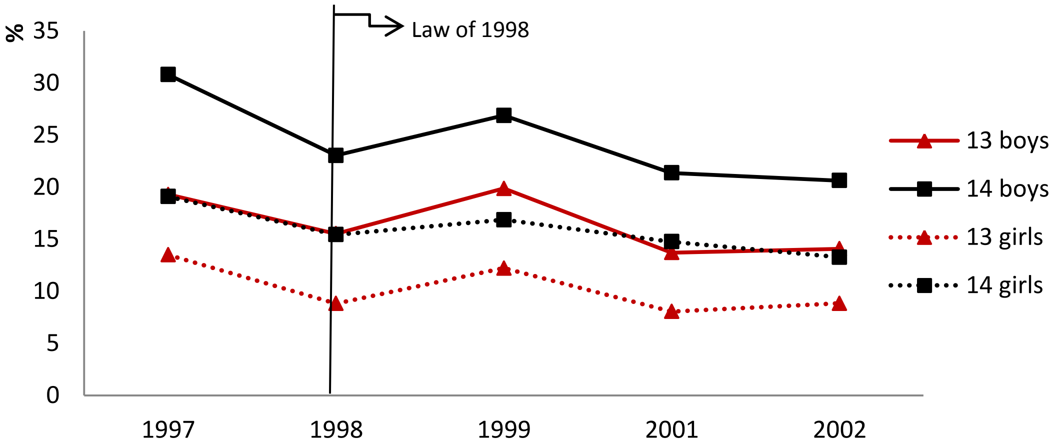
[Table 19 Here]

5. EFFECT OF THE LAWS ON THE GENDER GAP

This section aims to verify whether the law affected boys and girls differently. Although as the non-parametric Wald for the DD analysis provides point estimates for boys and girls separately, the point estimates have not been compared to each other.

Figure 4 shows how the law might have affected boys and girls aged 13 and 14.

Figure 4 – Work Incidence for Boys and Girls, Different Cohorts

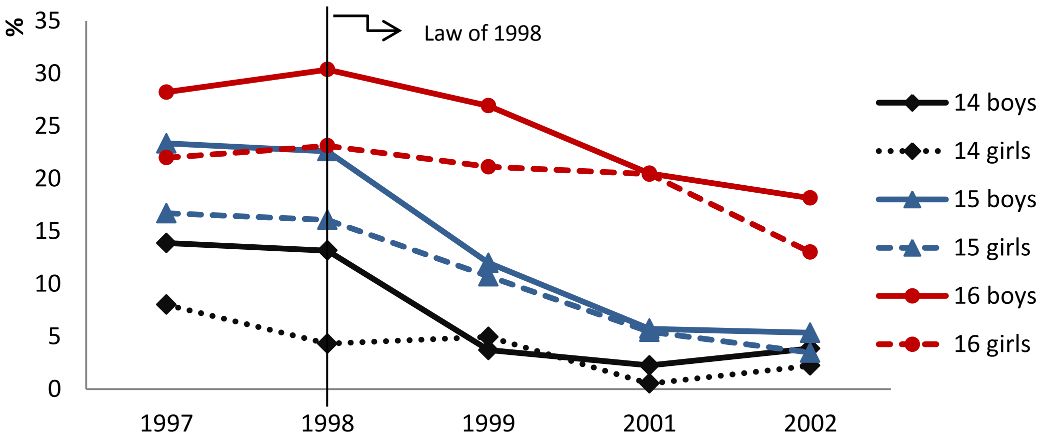


Source: PNADs of 1997, 1998, 1999, 2001, and 2002.

The figure suggests that the law did not affect the gap in work participation between boys and girls aged 13 since the work participation of both groups evolved in parallel. However, the law does seem to have increased the gender gap relative to labour force participation for youth aged 14. While girls' participation rate basically flattened between 1998 and 1999, the boys' increased more than 3 pp. The figure suggests that the law may have increased the gender gap in the work participation rate, even though it narrowed somewhat from 2001 onwards. Whether this affected the earnings gender gap in the long run is a question that will not be addressed in this paper.

Figure 5 illustrates the trends for the participation rates in the formal labour market of boys and girls aged 14, 15 and 16.

Figure 5 – Formal Paid Work Incidence for Boys and Girls, Different Cohorts



Source: PNADs of 1997, 1998, 1999, 2001, and 2002.

One can see that irrespective of the age group considered, boys and girls display parallel trends between 1997 and 1998, but from 1998 to 2001 the gender gaps narrowed considerably. In 2002 the difference in participation rates in formal paid activities between boys and girls remained constant for boys and girls aged 14, slightly increased for boys and girls 15 years old, but grew by about 5.1 pp. between boys and girls aged 16, a difference only 1 pp. lower than the pre-law level difference.

In order to investigate any possible effect of the law on the gender gap, the RDD approach is used as identification strategy for the local average treatment effect of the law. In the regression model below, the parameter of interest is the coefficient of the interaction term between gender and *Z*. This time, *Z* is defined as in the RDD estimates, i.e., it is normalised to zero for age 14. A variable *T* is defined as an indicator function that takes the value of 1 for *Z* equal or higher than zero. Thus, the gender variable plays a similar role to the treatment variable in the DD approach, whereas the variable *Z* plays the role of the time effect.

The main flaw of this approach is that it is fully parametric in that the bandwidth and the polynomial degree have to be defined in an *ad hoc* way. I follow Green et al. (2009) who showed that the best fit seems to be given by a specification that minimises the mean squared error. Therefore, although parametric, the best model is very much guided by the approach recommended by Imbens and Kalyanaraman (2009).[28]

$$y_i = \alpha + \gamma_1 boys_i + \gamma_2 T + \gamma_3 boys * T + \sum_{i=1}^{2} \beta_i Z^i + \varepsilon_i , \qquad (3)$$

The model is defined with a smooth *Z* function to make the functional form more flexible. If the law affected boys and girls equally, the coefficient of the interaction term, γ_3 , should be statistically insignificant. Note that this identification approach is the same as the difference-in-differences one.

Table 20 shows the reduced form estimates for work outcomes in 1997, 1999 and 2002.[29]

[Table 20 Here]

Starting with age groups 13 and 14, the estimates for 1997 are statistically significant for participation rates in formal paid activities, domestic work, and weekly hours worked in formal activities. Before the law of 1998, boys used to be 9 pp. more

[28] To save space, the results of only one specification will be presented.
[29] These are very preliminary findings and do not include the estimates for work intensity as well as for teenagers aged 15 and 16.

likely to do formal paid work than girls, and used to work almost 5 hours per week more than girls in formal paid activities. In the following years these gaps disappear. With the ban of 1998, boys became about 4 pp. less likely to do domestic work than girls. The laws do not appear to have affected the gender gap relative to school outcomes.

Regarding children aged 15 and 16, there is some indication that the gap in labour force participation widened after 2000. This widening seems to be due to the greater participation rate of boys in informal activities. With regard to school outcomes, the coefficient for the interaction term is statistically significant in 1999, suggesting that after the 1998 ban boys became about 5 pp. less likely to attend school than girls.

In a nutshell, the ban reduced the gap in formal paid work between boys and girls aged 14, and appears also to have enlarged the difference in participation rates in domestic work. The comparison between children 15 and 16 years old suggests that the ban affected boys negatively with respect to school attendance whereas the 2000 law seems to have widened the gap in labour force participation, though the participation rate seems to be concentrated in informal activities.

FINAL REMARKS

This paper has looked at the impact of two Brazilian laws that aimed to affect children's participation in the formal labour force, one from December 1998 and other from December 2000. These laws can be seen as a random event since the eligibility criteria were based on age, which is plausibly an exogenous variable in this context. Regression discontinuity design and difference-in-difference techniques are used to estimate the local average treatment effect of the laws. The impacts of the laws on child labour and school outcomes were estimated in order to show whether the laws had unintended consequences. The results suggest that the 1998 ban led to a fall in boys' participation rates in labour force, particularly in formal paid activities. These effects were found almost exclusively among children aged 14.

With regard to school outcomes, the LATE suggested an impact of the 1998 ban on boys' school attendance as well as an increase in schooling delay. This was verified for children aged 14 and 15. For the latter, the point estimate was larger in absolute terms.

The 2000 law increased children's participation rate in formal paid work activities and boys' school attendance and schooling delay. It is argued that this might have happened because the 2000 law conditioned children's participation in youth programmes to depend on school enrolment. Thus, even if attending school to fulfil the legal requirement, some children may not have managed to reconcile both activities. Thus a price was paid in terms of more grade repetition.

Finally, this paper has investigated whether the laws had any gender effect, decreasing or increasing the pre-existing gender gap regarding the outcomes covered here. The estimates show that after the 1998 law the gap between boys and girls aged 14 regarding participation rates in domestic work increased. For children aged 15, the 2000 law seems to have enlarged the gap in labour force participation between boys and girls, though most of the effect is observed in informal work. Boys aged 16 were also less likely to attend school than girls after the 1998 increase in the legal minimum age.

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Appendix

Table 1 – Descriptive Statistics for Children's Work Outcomes, Different Years

	Age								Age						
	10	11	12	13	14	15	16		10	11	12	13	14	15	16
				1997								***1999***			
Participation	8.6	12.3	16.8	22.1	30.7	41.2	50.5	*Participation*	9.0	12.7	16.0	22.4	29.1	39.5	50.9
Boys	12.4	16.6	21.8	27.4	38.7	50.2	61.2	Boys	12.2	17.2	20.7	28.1	36.2	47.4	59.9
Girls	4.8	8.1	11.6	16.9	23.0	32.3	40.7	Girls	5.8	8.1	11.3	16.7	21.9	31.7	41.6
Formal	0	0	0	0	10.0	19.1	24.5	*Formal*	0	0	0	0	4.2	10.7	22.4
Boys	0	0	0	0	11.8	20.9	26.5	Boys	0	0	0	0	4.0	11.6	24.0
Girls	0	0	0	0	7.2	16.3	21.5	Girls	0	0	0	0	4.6	9.2	19.8
Informal	1.0	1.0	1.0	1.0	90.0	80.9	75.5	*Informal*	1.0	1.0	1.0	1.0	95.8	89.3	77.6
Boys	1.0	1.0	1.0	1.0	88.3	79.1	73.5	Boys	1.0	1.0	1.0	1.0	96.0	88.4	76.1
Girls	1.0	1.0	1.0	1.0	92.8	83.7	78.5	Girls	1.0	1.0	1.0	1.0	95.5	90.8	80.3
Domestic	60.1	65.3	67.4	69.4	70.5	69.4	69.3	*Domestic*	61.0	64.9	69.1	71.2	71.9	71.1	70.7
Boys	44.2	44.9	50.1	51.5	52.2	49.7	48.4	Boys	46.4	49.1	53.9	54.3	54.5	52.1	51.8
Girls	76.9	81.4	85.2	87.5	88.6	89.2	89.2	Girls	76.1	81.1	84.6	88.2	89.5	90.0	90.3
Weekly hours worked - formal	0.0	0.0	0.0	0.0	37.4	38.6	40.8	*Hours worked - formal*	0.0	0.0	0.0	0.0	36	37	39
Boys	0.0	0.0	0.0	0.0	39.8	37.8	40.1	Boys	0.0	0.0	0.0	0.0	27	34.5	38.8
Girls	0.0	0.0	0.0	0.0	30	38.8	39.1	Girls	0.0	0.0	0.0	0.0	44.6	38.9	37.5
Weekly hours worked - informal	18.3	27.7	32.4	33	38	37.7	39.2	*Hours worked - informal*	28.8	21.1	28.5	26.9	34.8	37	39.5
Boys	19	29	28	26.9	35.4	35.2	38.2	Boys	20.6	18.6	23.8	26.2	35.2	34.5	39.6
Girls	15	25.4	35.7	30.3	41.8	38.8	37.6	Girls	Na	23.2	45.6	22.7	31.2	38	35.8

Cont. Table 1

	2001								2002						
Participation	6.3	7.9	11.5	15.8	22.6	32.7	42.6	*Participation*	5.5	8.2	11.1	15.9	21.9	31.8	43.2
Boys	8.7	10.6	15.6	19.9	28.2	40.5	51.0	Boys	7.9	11.1	14.4	20.5	27.4	38.2	51.8
Girls	3.9	5.2	7.4	11.5	16.9	24.8	34.2	Girls	3.1	5.2	7.8	11.2	16.4	25.4	34.6
Formal	0.0	0.0	0.0	0.0	1.4	5.1	18.6	*Formal*	0.0	0.0	0.0	0.0	2.8	4.6	14.6
Boys	0.0	0.0	0.0	0.0	1.8	5.1	18.4	Boys	0.0	0.0	0.0	0.0	3.4	5.1	15.9
Girls	0.0	0.0	0.0	0.0	0.9	5.2	18.8	Girls	0.0	0.0	0.0	0.0	1.9	3.9	12.6
Informal	1.0	1.0	1.0	1.0	98.6	94.9	81.5	*Informal*	1.0	1.0	1.0	1.0	97.2	95.4	85.4
Boys	1.0	1.0	1.0	1.0	98.2	94.9	81.6	Boys	1.0	1.0	1.0	1.0	96.6	94.9	84.1
Girls	1.0	1.0	1.0	1.0	99.1	94.8	81.2	Girls	1.0	1.0	1.0	1.0	98.1	96.1	87.4
Domestic	47.4	55.1	58.1	61.6	64.0	65.2	64.1	*Domestic*	50.2	56.6	60.7	64.7	67.1	67.3	65.1
Boys	30.5	36.7	39.7	41.6	43.8	44.7	42.5	Boys	34.6	40.3	42.0	47.0	48.6	46.7	44.3
Girls	64.9	73.3	76.6	82.2	84.7	85.9	86.1	Girls	65.8	73.6	79.2	82.9	86.1	87.9	85.9
Weekly hours worked - formal	0.0	0.0	0.0	0.0	0	37.4	38.2	*Hours worked - formal*	0.0	0.0	0.0	0.0	40	43.6	37.4
Boys	0.0	0.0	0.0	0.0	0	33.2	37.1	Boys	0.0	0.0	0.0	0.0	28	42.4	37.9
Girls	0.0	0.0	0.0	0.0	0	48	39.5	Girls	0.0	0.0	0.0	0.0	40	42.3	35.6
Weekly hours worked - informal	29.7	19.8	22.6	23.8	31.4	32.7	34.2	*Hours worked - informal*	21	17.9	27.8	26.3	31.7	32.3	34
Boys	29.7	14.5	23.3	23.9	30.4	31.7	33.9	Boys	27.8	17.7	26.3	29.6	30.9	30.3	33.7
Girls	0	0	14.8	19.1	32.3	30.7	32.9	Girls	16.3	17.5	32.8	21	29.7	29.2	32.4

Source: PNADs of 1997, 1999, 2001 and 2002.
Note: *Child labour* refers to the second definition provided in the text.

Table 2 – Descriptive Statistics for Schooling Outcomes, Different Years

	Age								Age						
	10	11	12	13	14	15	16		10	11	12	13	14	15	16
				1997								***1999***			
School Attendance	95.5	95.2	94.4	92.5	87.3	81.2	73.7	School Attendance	97.4	97.7	96.6	94.3	91.4	86.1	79.1
Boys	94.8	94.4	93.4	92.1	86.6	80.6	71.1	Boys	97.0	97.0	96.4	94.0	91.1	86.4	79.3
Girls	96.3	96.1	95.4	92.8	88.1	81.8	76.2	Girls	97.8	98.4	96.8	94.5	91.8	85.8	78.9
Schooling Delay	0.27	0.36	0.40	0.50	0.55	0.61	0.65	Schooling Delay	0.20	0.26	0.32	0.41	0.47	0.52	0.56
Boys	0.31	0.39	0.45	0.55	0.61	0.67	0.71	Boys	0.22	0.30	0.36	0.46	0.53	0.57	0.62
Girls	0.23	0.32	0.34	0.44	0.49	0.55	0.59	Girls	0.17	0.23	0.28	0.36	0.41	0.47	0.50
				2001								***2002***			
School Attendance	97.9	97.8	96.9	95.6	92.6	87.5	81.4	School Attendance	98.3	98.3	97.7	96.4	93.6	88.9	81.8
Boys	97.7	97.3	96.6	95.4	92.8	88.4	82.7	Boys	97.9	98.1	97.1	95.8	93.1	89.3	81.5
Girls	98.2	98.3	97.2	95.9	92.4	86.6	80.0	Girls	98.6	98.5	98.2	97.0	94.0	88.5	82.1
Schooling Delay	0.18	0.23	0.29	0.37	0.42	0.47	0.53	Schooling Delay	0.05	0.09	0.12	0.17	0.23	0.29	0.34
Boys	0.21	0.27	0.32	0.42	0.47	0.53	0.58	Boys	0.06	0.11	0.15	0.21	0.28	0.34	0.40
Girls	0.14	0.19	0.25	0.31	0.37	0.41	0.48	Girls	0.04	0.07	0.09	0.13	0.19	0.25	0.29

Source: PNADs of 1997, 1999, 2001 and 2002.

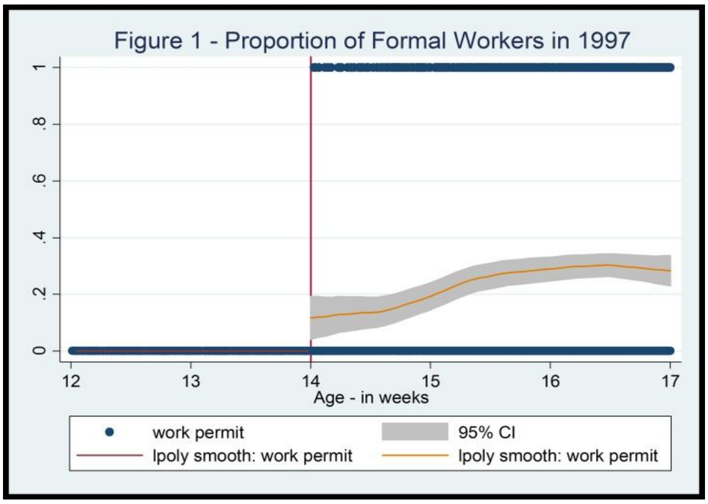
Figure 1 - Proportion of Formal Workers in 1997
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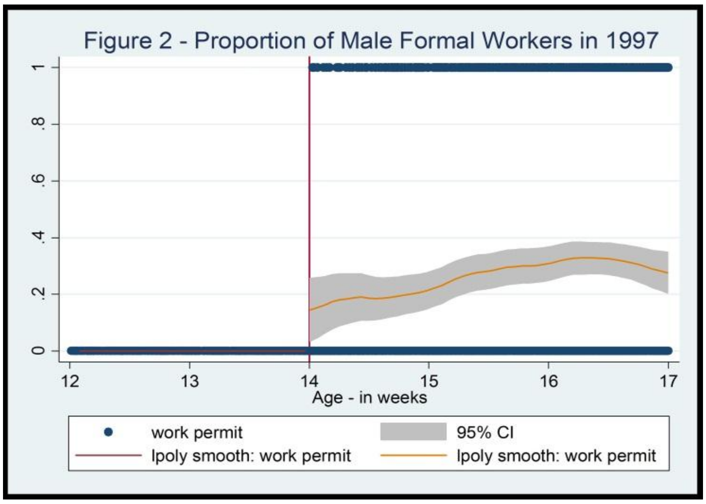
Figure 2 - Proportion of Male Formal Workers in 1997
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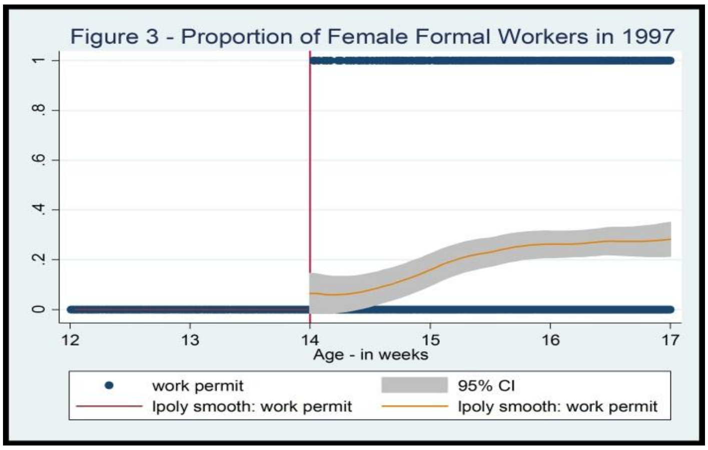
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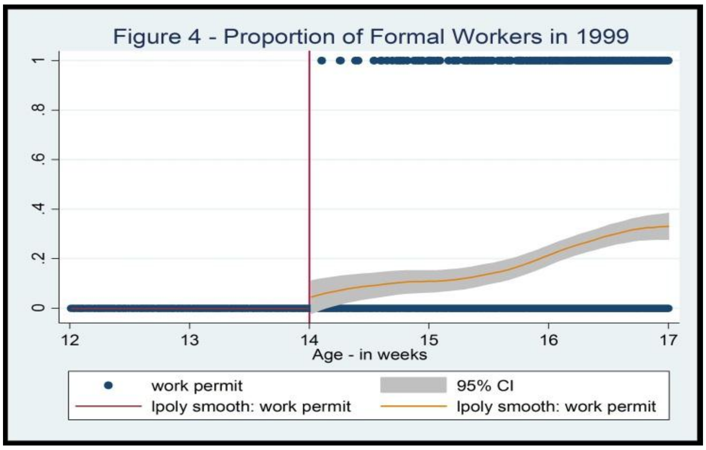
Figure 4 - Proportion of Formal Workers in 1999
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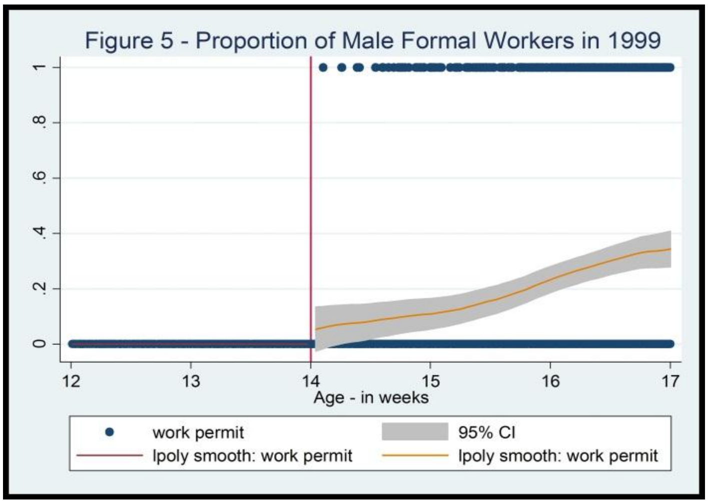
Figure 5 - Proportion of Male Formal Workers in 1999
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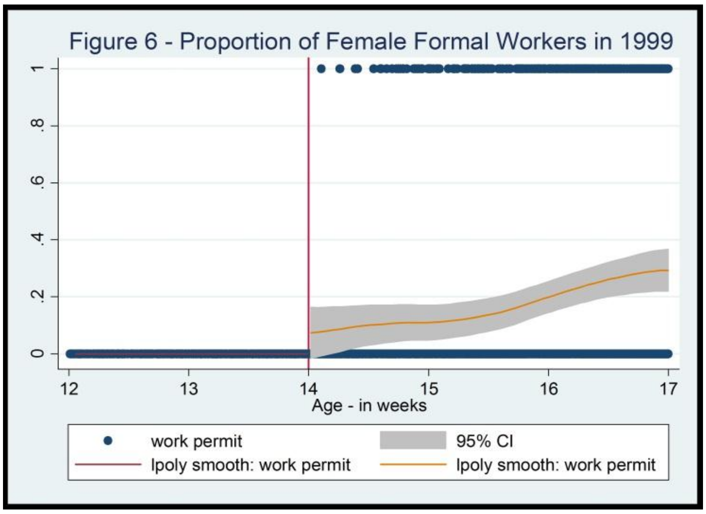
Figure 6 - Proportion of Female Formal Workers in 1999
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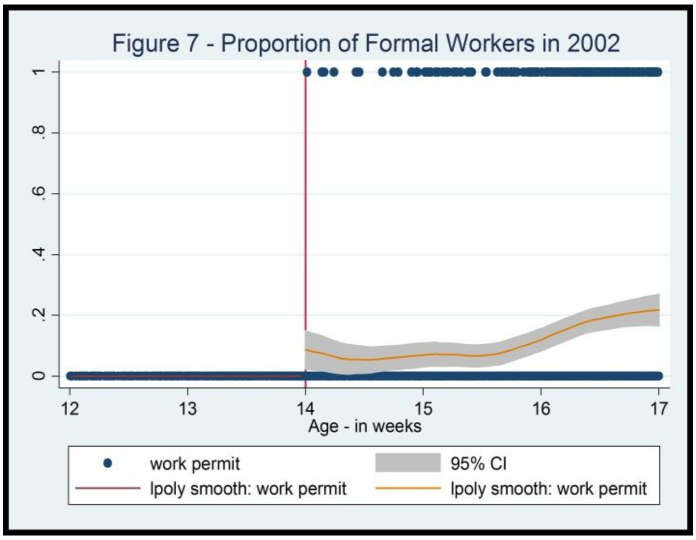


Figure 7 - Proportion of Formal Workers in 2002

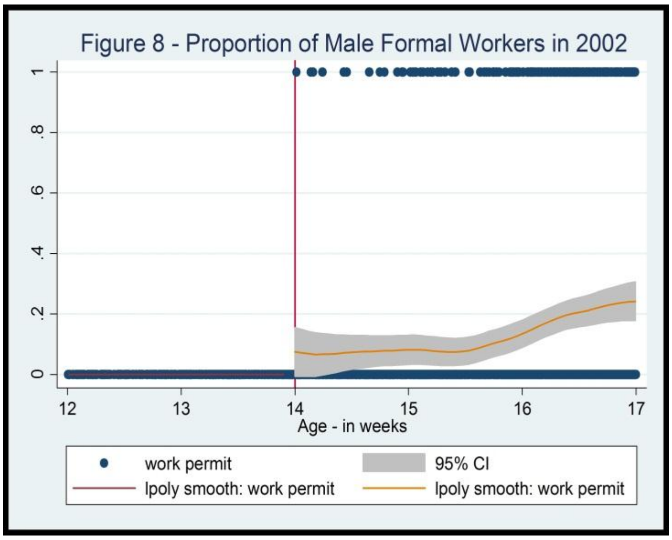


Figure 8 - Proportion of Male Formal Workers in 2002

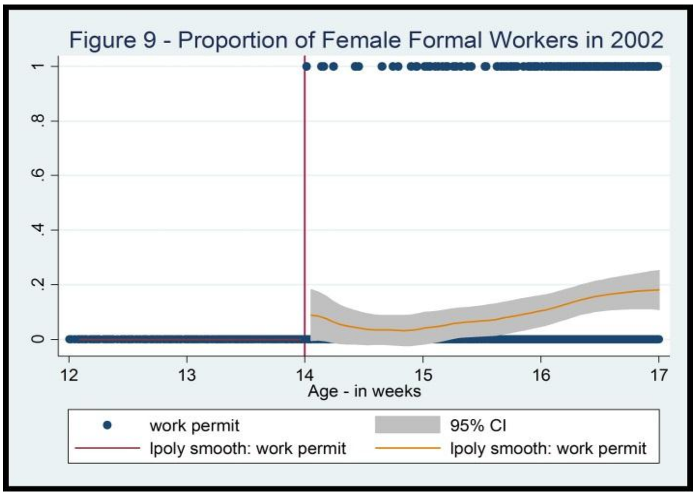


Figure 9 - Proportion of Female Formal Workers in 2002

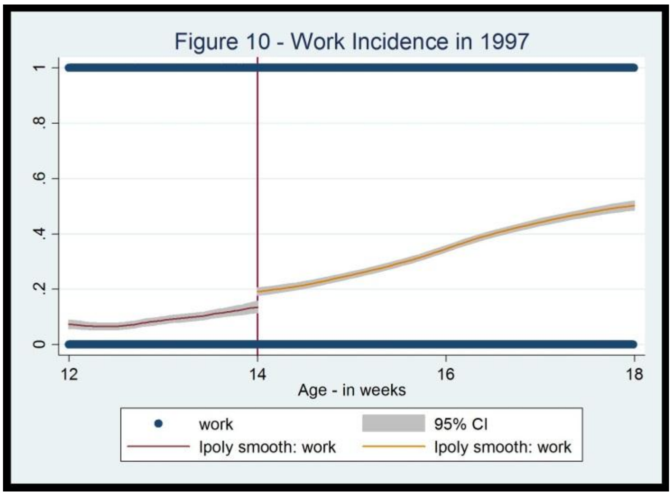


Figure 10 - Work Incidence in 1997

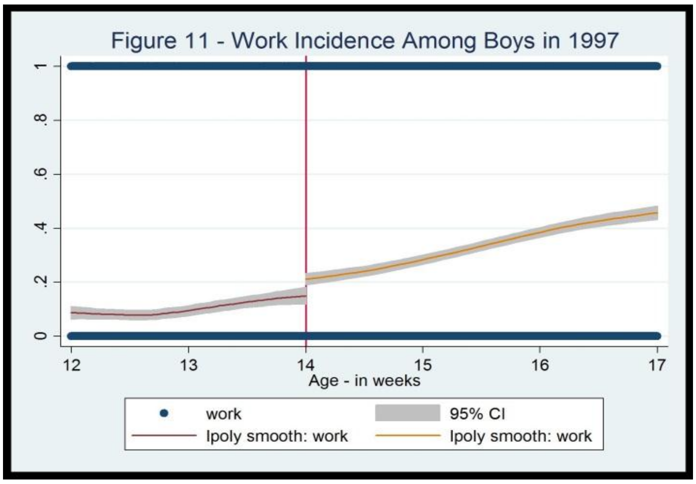


Figure 11 - Work Incidence Among Boys in 1997

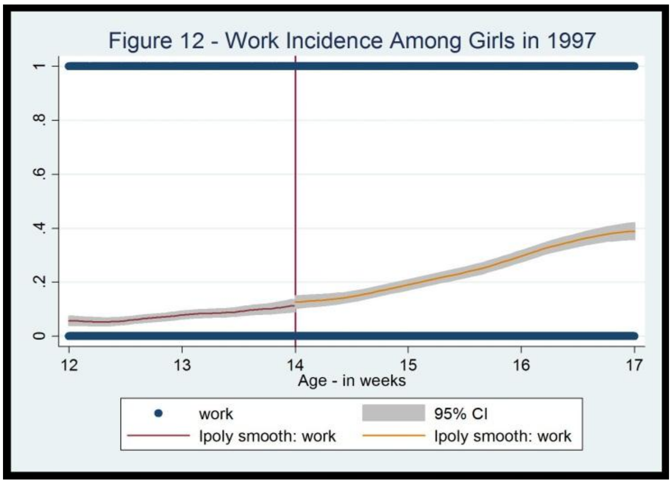


Figure 12 - Work Incidence Among Girls in 1997

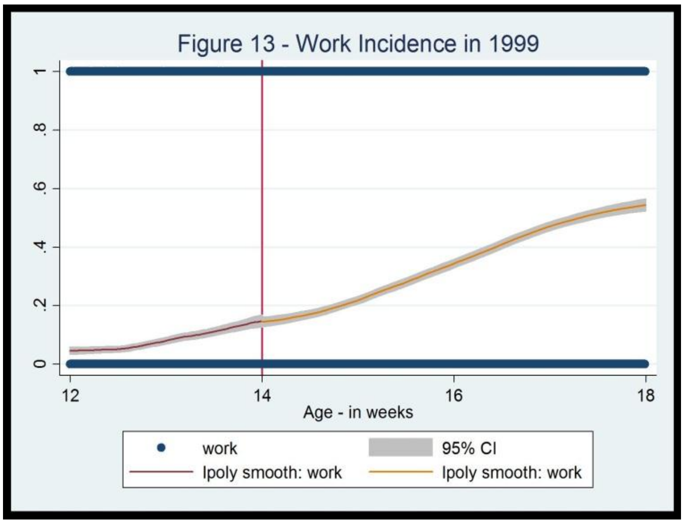


Figure 13 - Work Incidence in 1999

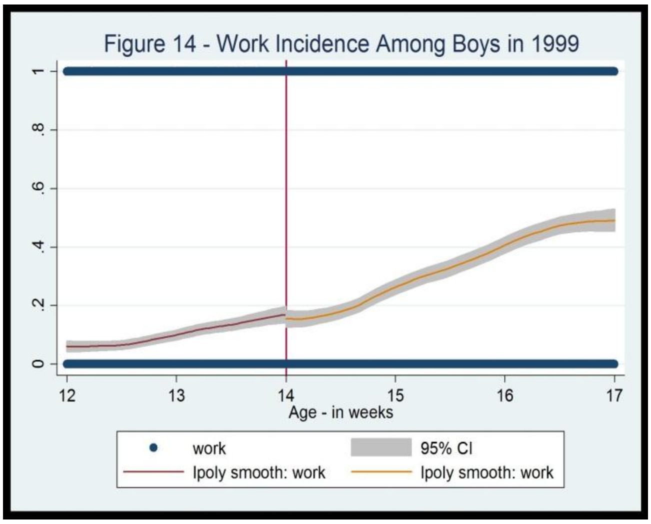


Figure 14 - Work Incidence Among Boys in 1999

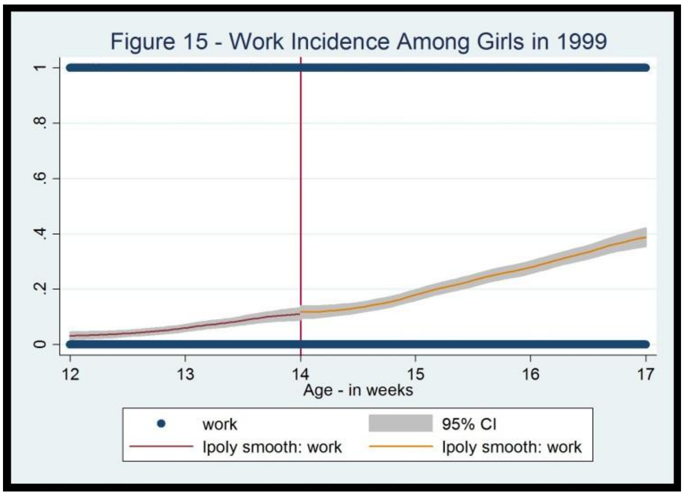


Figure 15 - Work Incidence Among Girls in 1999

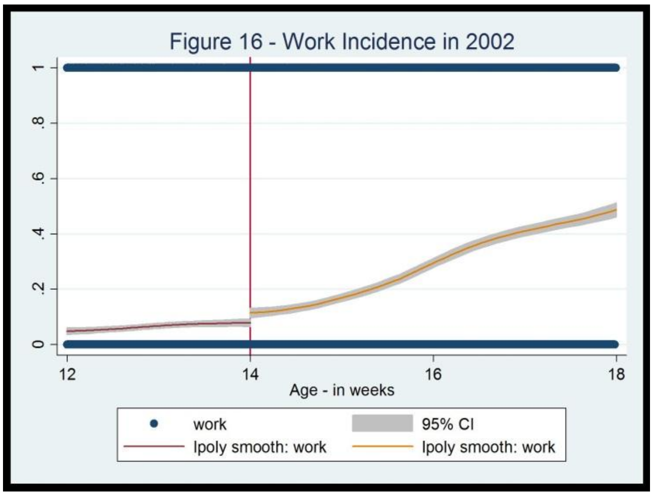


Figure 16 - Work Incidence in 2002

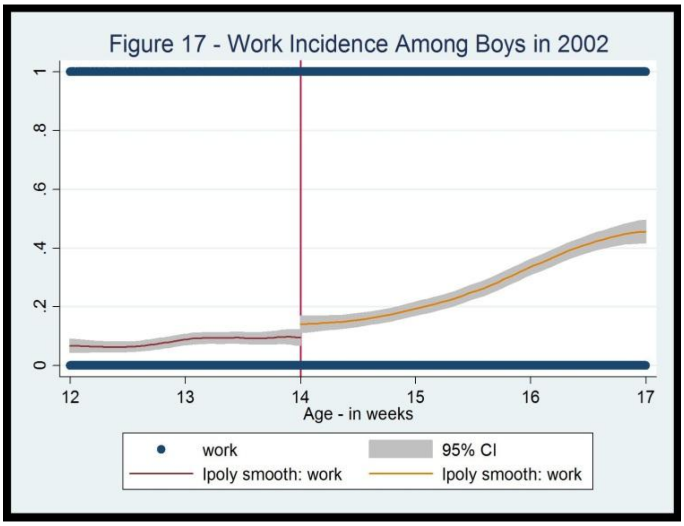


Figure 17 - Work Incidence Among Boys in 2002

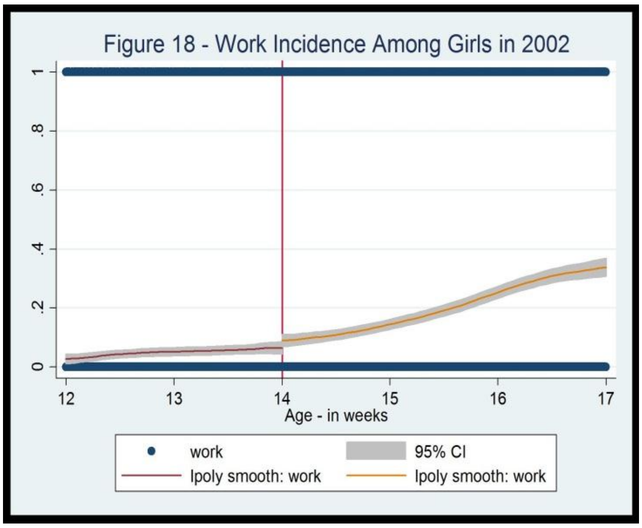


Figure 18 - Work Incidence Among Girls in 2002

Figure 19 - School Attendance in 1997

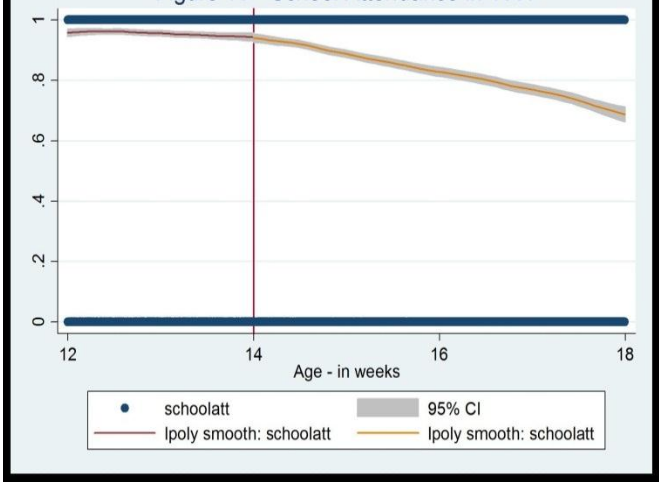


Figure 20 - School Attendance Among Boys in 1997

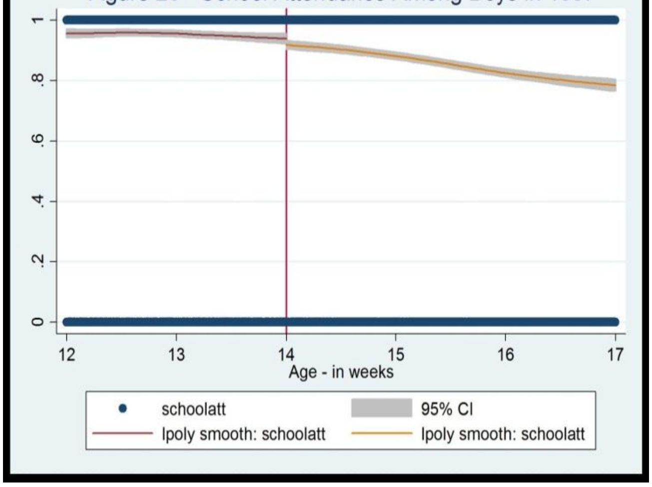


Figure 21 - School Attendance Among Girls in 1997

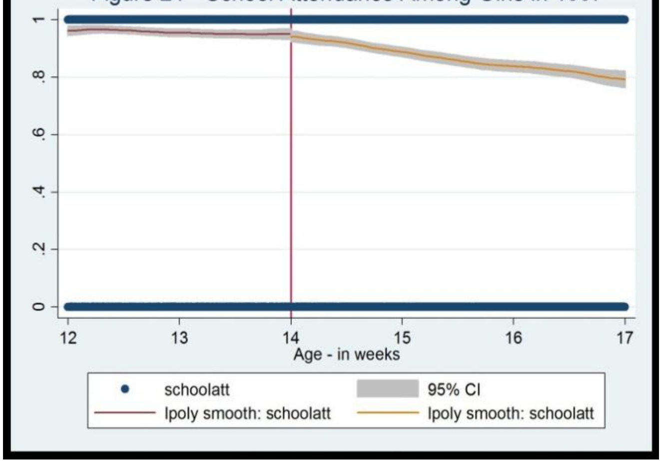


Figure 22 - School Attendance in 1999

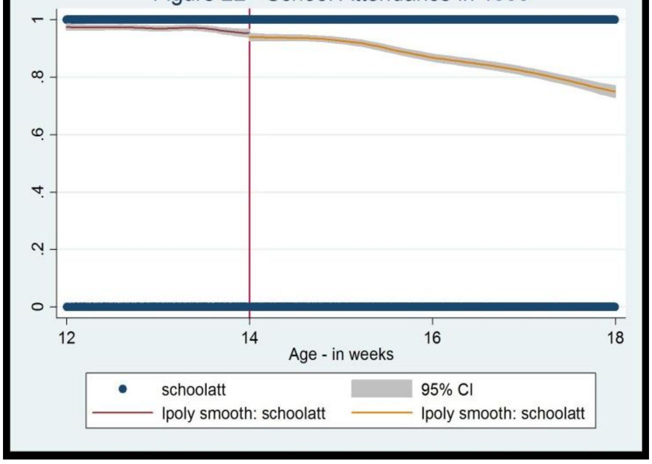


Figure 23 - School Attendance Among Boys in 1999

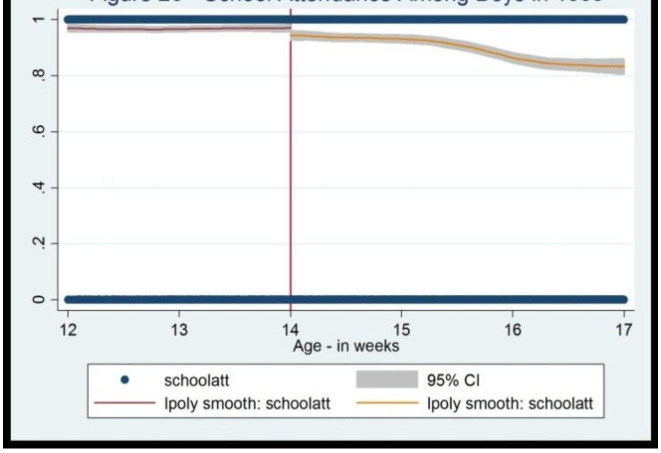
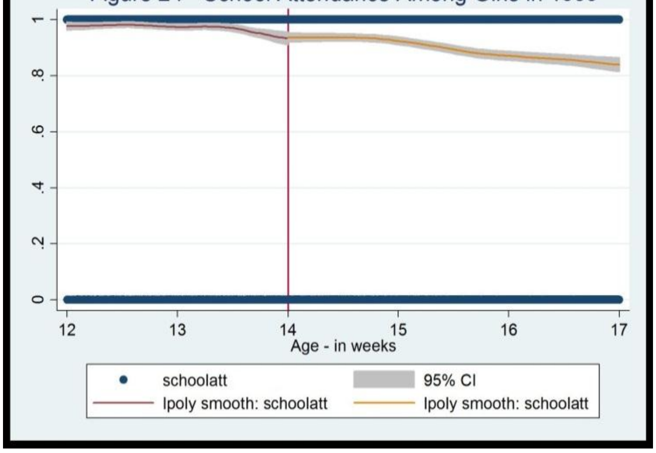


Figure 24 - School Attendance Among Girls in 1999



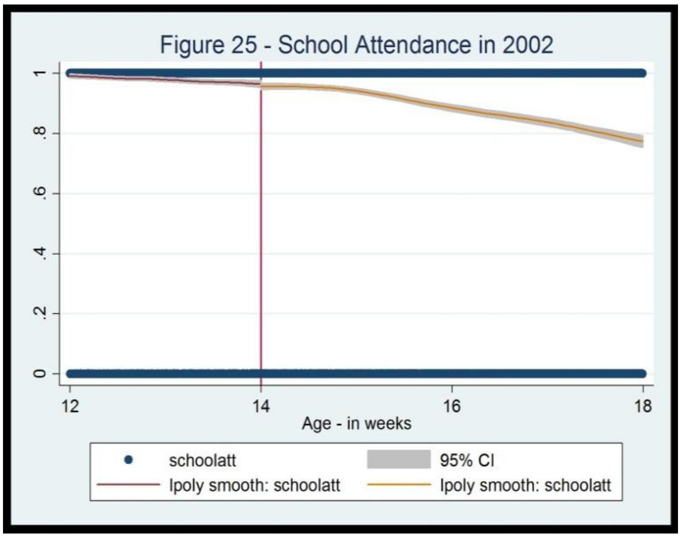


Figure 25 - School Attendance in 2002

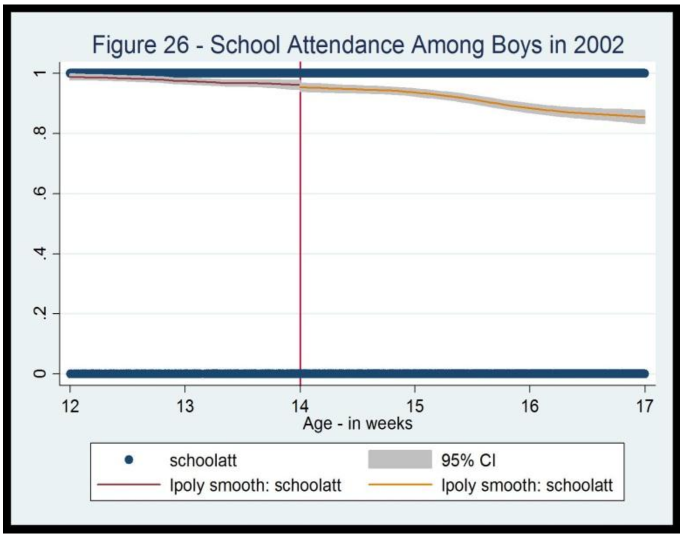


Figure 26 - School Attendance Among Boys in 2002

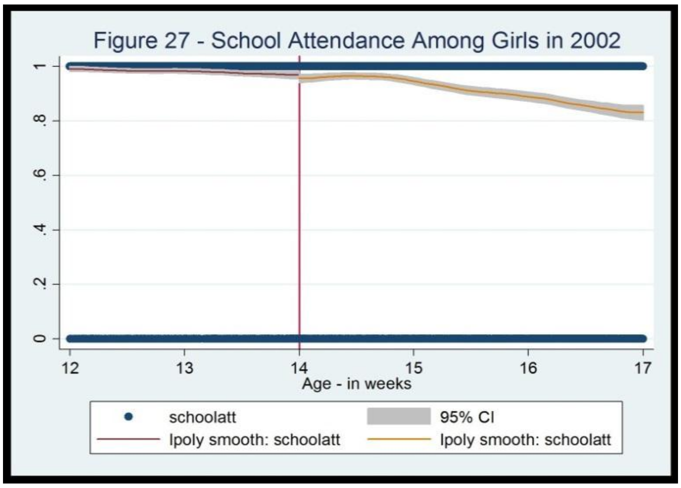


Figure 27 - School Attendance Among Girls in 2002

Table 3 – Descriptive Statistics for the Difference-in-Differences Analysis – Baseline (1997)
Bandwidth of 6 Months

	14 vs. 13				*15 vs. 16*			
	13		14		15		16	
Outcomes	Obs	Mean	Obs	Mean	Obs	Mean	Obs	Mean
Formal Paid Work	206	0.00	484	0.12	768	0.22	1171	0.26
Labour Force Participation	4043	0.17	5332	0.24	4364	0.37	5191	0.46
Informal Work	3553	0.06	4529	0.09	3504	0.17	3964	0.22
Weekly Hours Worked - Formal	206	0.00	484	4.76	768	8.87	1171	10.56
Weekly Hours Worked - Informal	187	31.92	441	29.24	713	27.7	1095	27.31
Domestic Work	4038	0.70	5326	0.70	4362	0.7	5190	0.70
School Attendance	4042	0.93	5331	0.90	4363	0.84	5188	0.79
Schooling Delay	4043	0.47	5332	0.54	4364	0.58	5191	0.64
Covariates								
Male	4043	0.47	5332	0.48	4364	0.47	5191	0.45
White	4043	0.49	5332	0.48	4364	0.49	5191	0.51
Mother's Years of Schooling	4043	8.02	5332	7.93	4364	7.98	5191	8.07
Father's Years of Schooling	4043	8.02	5332	8.03	4364	8.1	5191	8.17
Children 0-5	4043	0.55	5332	0.53	4364	0.54	5191	0.53
Children 6-11	4043	0.71	5332	0.67	4364	0.63	5191	0.64
Children 12-13	**4043**	**0.98**	**5332**	**0.43**	**4364**	**0.44**	**5191**	**0.43**
Children 14-15	**4043**	**0.48**	**5332**	**1.00**	**4364**	**0.99**	**5191**	**0.45**
Children 16-17	**4043**	**0.45**	**5332**	**0.47**	**4364**	**0.46**	**5191**	**1.00**
Children 18-30	4043	0.78	5332	0.79	4364	0.82	5191	0.83
Land Title	3239	0.92	4334	0.93	3568	0.92	4221	0.93
Non-labour Household Income	4039	1.96	5329	2.60	4360	2.28	5188	3.55
Metropolitan Region	4043	0.44	5332	0.44	4364	0.44	5191	0.46

Source: PNAD of 1997.

Table 4 – LATE Estimates for the Impact of the Laws of 1998 and 2000 on Work Outcomes

14 vs. 13

	Work			Formal Paid Work			Informal Work			Domestic Work			Weekly Hours Worked - Formal			Weekly Hours Worked - Informal		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
									1997									
LATE	-0.001	-0.01	-0.001	0.13*	0.15	0.09	-0.13*	-0.15	-0.09	-0.07*	-0.05	-0.05	4.7	7.27	2.9	-8.36	-7.87	-3.18
	(0.02)	(0.04)	(0.04)	(0.07)	(0.10)	(0.13)	(0.07)	(0.10)	(0.13)	(0.03)	(0.05)	(0.04)	(2.90)	(5.21)	(3.80)	(5.10)	(6.15)	(6.99)
LATE	0.02	0.004	0.03	0.12	0.04	0.19	-0.12	-0.04	-0.19	-0.08*	-0.06	-0.05	3.6	3.05	5.8	-8.34	-3.03	-4.652
	(0.03)	(0.01)	(0.05)	(0.08)	(0.07)	(0.18)	(0.08)	(0.07)	(0.18)	(0.04)	(0.05)	(0.05)	(2.96)	(4.13)	(5.30)	(5.21)	(6.92)	(8.15)
LATE	-0.001	-0.01	-0.01	0.13*	0.18*	0.07	-0.13*	-0.18*	-0.07	-0.06**	-0.05	-0.05	4.97	8.25	2.3	-8.37*	-8.73	-2.897
	(0.02)	(0.04)	(0.03)	(0.07)	(0.10)	(0.12)	(0.07)	(0.10)	(0.12)	(0.03)	(0.05)	(0.04)	(2.97)	(5.30)	(3.65)	(5.09)	(6.14)	(6.84)
									1999									
LATE	-0.05**	-0.07*	-0.017	Na	Na	Na	0.019	Na	Na	0.052	0.074	0.016	Na	Na	Na	10.3*	7.77	16.85*
	(0.024)	(0.04)	(0.03)	Na	Na	Na	(0.06)	Na	Na	(0.03)	(0.05)	(0.04)	Na	Na	Na	(6.26)	(6.56)	(8.97)
LATE	-0.067**	-0.10*	-0.028	Na	Na	Na	0.03	Na	Na	0.048	0.068	0.024	Na	Na	Na	17.4**	13.24	14.94*
	(0.03)	(0.053)	(0.033)	Na	Na	Na	(0.04)	Na	Na	(0.04)	(0.06)	(0.05)	Na	Na	Na	(7.71)	(8.64)	(8.97)
LATE	-0.045*	-0.06	-0.015	Na	Na	Na	0.018	Na	Na	0.053*	0.077	0.014	Na	Na	Na	8.86	6.69	17.16*
	(0.023)	(0.038)	(0.03)	Na	Na	Na	(0.07)	Na	Na	(0.03)	(0.05)	(0.04)	Na	Na	Na	(6.30)	(6.60)	(9.18)
									2002									
LATE	0.01	0.036	-0.03	0.19**	Na	Na	-0.028	0.003	-0.05**	-0.027	-0.037	-0.048	7.94**	Na	Na	-4.27	-11.3	7.8
	(0.02)	(0.03)	(0.02)	(0.09)	Na	Na	(0.02)	(0.03)	(0.03)	(0.03)	(0.05)	(0.04)	(3.70)	Na	Na	(7.80)	(7.47)	(13.00)
LATE	0.005	0.023	-0.03	0.23**	Na	Na	-0.066**	-0.06	-0.06*	-0.012	-0.017	-0.049	8.9**	Na	Na	-0.86	-5.89	12.6
	(0.02)	(0.04)	(0.02)	(0.10)	Na	Na	(0.03)	(0.05)	(0.03)	(0.04)	(0.06)	(0.05)	(4.07)	Na	Na	(9.50)	(7.30)	(23.33)
LATE	0.01	0.038	-0.03	0.18**	Na	Na	-0.02	0.013	-0.05**	-0.028	-0.039	-0.047	7.7*	Na	Na	-4.41	-12.3	4.44
	(0.02)	(0.03)	(0.02)	(0.09)	Na	Na	(0.02)	(0.03)	(0.03)	(0.03)	(0.05)	(0.04)	(3.61)	Na	Na	(7.70)	(7.65)	(10.63)

Source: PNADs of 1997, 1999, and 2002.

Note: Bootstrapped standard errors with 50 repetitions in parentheses. ***, **, * Statistically significant at 1%, 5% and 10% respectively.

Table 5 – LATE Estimates for the Impact of the Laws of 1998 and 2000 on Work Outcomes

16 vs. 15

	Labour Force Participation			Formal Paid Work			Informal Work			Domestic Work			Weekly Hours Worked - Formal			Weekly Hours Worked - Informal		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
1997																		
LATE	-0.0169	0.0370	-0.0592	-0.0226	0.0865	-0.137	-0.0281	-0.0279	-0.0305	-0.000881	-0.0758	0.0643*	0.258	4.339	-4.457	-0.785	-4.646	3.857
	(0.0338)	(0.0496)	(0.0455)	(0.0824)	(0.114)	(0.125)	(0.0224)	(0.0337)	(0.0292)	(0.0334)	(0.0491)	(0.0344)	(3.545)	(5.229)	(5.137)	(3.451)	(4.336)	(5.610)
LATE	-0.0820*	-0.0160	-0.106*	-0.0254	0.167	-0.196	-0.0653**	-0.0892**	-0.0360	0.000725	-0.111*	0.0878*	0.979	8.228	-5.022	-1.675	-10.21*	6.140
	(0.0472)	(0.0661)	(0.0635)	(0.0995)	(0.139)	(0.151)	(0.0319)	(0.0452)	(0.0411)	(0.0450)	(0.0626)	(0.0449)	(4.321)	(6.689)	(5.907)	(4.719)	(5.523)	(6.945)
LATE	-0.00528	0.0419	-0.0513	-0.0265	0.0649	-0.132	-0.0162	-0.00821	-0.0280	0.00664	-0.0637	0.0658**	-0.138	3.029	-4.522	-0.102	-2.105	3.510
	(0.0312)	(0.0477)	(0.0425)	(0.0811)	(0.113)	(0.122)	(0.0207)	(0.0322)	(0.0277)	(0.0321)	(0.0480)	(0.0333)	(3.505)	(5.146)	(5.084)	(3.320)	(4.248)	(5.477)
	5,478	2,414	2,842	861	446	364	5,478	2,414	2,842	5,471	2,412	2,837	861	446	364	1,086	585	435
1999																		
LATE	0.0262	0.0219	0.0246	-0.0854	-0.0448	-0.142	0.0854	0.0448	0.142	0.0313	0.0742	-0.0101	-1.811	1.317	-5.928	1.844	3.825	-2.845
	(0.0342)	(0.0517)	(0.0452)	(0.0878)	(0.126)	(0.127)	(0.0878)	(0.126)	(0.127)	(0.0331)	(0.0530)	(0.0341)	(3.430)	(4.598)	(5.358)	(3.976)	(5.222)	(6.648)
LATE	0.0317	0.00903	0.0372	-0.107	0.0663	-0.306*	0.107	-0.0663	0.306*	0.00746	0.0539	-0.00921	-1.554	6.740	-11.08	2.092	0.749	-1.485
	(0.0450)	(0.0624)	(0.0588)	(0.117)	(0.172)	(0.169)	(0.117)	(0.172)	(0.169)	(0.0397)	(0.0663)	(0.0425)	(4.607)	(6.341)	(7.194)	(4.379)	(6.857)	(7.239)
LATE	0.0270	0.0254	0.0218	-0.0809	-0.0564	-0.121	0.0809	0.0564	0.121	0.0365	0.0810	-0.0104	-1.720	0.721	-5.287	1.783	4.460	-3.198
	(0.0331)	(0.0507)	(0.0439)	(0.0846)	(0.120)	(0.122)	(0.0846)	(0.120)	(0.122)	(0.0327)	(0.0520)	(0.0334)	(3.303)	(4.437)	(5.126)	(3.919)	(5.045)	(6.550)
	5,076	2,306	2,564	617	341	233	617	341	233	5,072	2,304	2,562	617	341	233	617	341	233
2002																		
LATE	0.0130	-0.0246	0.0510	-0.00422	0.101	-0.166	0.0113	-0.0330	0.0621	-0.0105	0.00518	-0.0325	-0.598	3.495	-6.839	1.270	1.540	-0.179
	(0.0320)	(0.0482)	(0.0431)	(0.0719)	(0.0901)	(0.108)	(0.0324)	(0.0497)	(0.0415)	(0.0339)	(0.0498)	(0.0362)	(3.118)	(3.938)	(4.592)	(3.743)	(4.861)	(6.164)
LATE	-0.00973	-0.0255	0.0294	-0.0823	0.0562	-0.240*	-0.00313	-0.0293	0.0546	0.0408	0.00687	-0.0318	-2.417	2.446	-8.484	1.331	1.627	0.278
	(0.0397)	(0.0490)	(0.0557)	(0.0866)	(0.103)	(0.124)	(0.0447)	(0.0667)	(0.0482)	(0.0446)	(0.0579)	(0.0367)	(3.598)	(4.304)	(5.179)	(4.502)	(5.484)	(6.241)
LATE	0.0186	-0.0242	0.0581	0.00699	0.107	-0.151	0.0182	-0.0291	0.0634	-0.0135	0.00667	-0.0328	-0.295	3.686	-6.481	1.227	1.509	-0.339
	(0.0314)	(0.0480)	(0.0420)	(0.0706)	(0.0897)	(0.106)	(0.0309)	(0.0479)	(0.0408)	(0.0329)	(0.0491)	(0.0362)	(3.079)	(3.922)	(4.552)	(3.706)	(4.840)	(6.164)
	5,037	2,278	2,506	568	302	221	5,108	2,294	2,563	5,188	2,341	2,590	568	302	221	568	302	221

Source: PNADs of 1997, 1999, and 2002.

Note: Bootstrapped standard errors with 50 repetitions in parentheses. ***, **, * Statistically significant at 1%, 5% and 10% respectively.

Table 6 – LATE Estimates for the Impact of the Laws of 1998 and 2000 on School Outcomes
14 vs. 13

	School Attendance			Failed Grade		
	All	Boys	Girls	All	Boys	Girls
			1997			
LATE	0.018	0.02	0.008	0.237***	0.227***	0.277***
	(0.014)	(0.020)	(0.015)	(0.030)	(0.049)	(0.048)
LATE	0.025	0.018	0.005	0.219***	0.197***	0.281***
	(0.017)	(0.024)	(0.015)	(0.048)	(0.067)	(0.066)
LATE	0.020	0.02	0.008	0.24***	0.23***	0.275***
	(0.013)	(0.020)	(0.015)	(0.029)	(0.048)	(0.047)
			1999			
LATE	-0.008	-0.02	-0.004	0.13***	0.1**	0.17***
	(0.013)	(0.02)	(0.017)	(0.03)	(0.05)	(0.05)
LATE	-0.006	-0.013	-0.001	0.02	0.01	0.05
	(0.014)	(0.02)	(0.017)	(0.04)	(0.03)	(0.06)
LATE	-0.08	-0.02	-0.005	0.16***	0.13**	0.2***
	(0.013)	(0.02)	(0.017)	(0.03)	(0.05)	(0.05)
			2002			
LATE	-0.011	-0.002	-0.016	0.11***	0.14***	0.06***
	(0.012)	(0.019)	(0.014)	(0.022)	(0.035)	(0.029)
LATE	-0.006	0.03	-0.032	0.09***	0.10**	0.07**
	(0.014)	(0.024)	(0.018)	(0.030)	(0.044)	(0.039)
LATE	-0.011	-0.005	-0.013	0.11***	0.138***	0.06***
	(0.012)	(0.019)	(0.014)	(0.020)	(0.034)	(0.028)

Source: PNADs of 1997, 1999, and 2002.
Note: Bootstrapped standard errors with 50 repetitions in parentheses. *, **, *** Statistically significant at 1%, 5% and 10% respectively.

Table 7 – LATE Estimates for the Impact of the Laws of 1998 and 2000 on School Outcomes

16 vs. 15

	School Attendance				Schooling Delay		
	All	Boys	Girls		All	Boys	Girls
				1997			
LATE	0.0352	0.0465	0.0191		0.221***	0.194***	0.254***
	(0.0242)	(0.0373)	(0.0330)		(0.0301)	(0.0439)	(0.0430)
LATE	0.0339	0.0837*	-0.0186		0.232***	0.133**	0.335***
	(0.0311)	(0.0498)	(0.0425)		(0.0397)	(0.0538)	(0.0585)
LATE	0.0372	0.0438	0.0270		0.224***	0.208***	0.243***
	(0.0236)	(0.0357)	(0.0321)		(0.0292)	(0.0431)	(0.0415)
	5,474	2,412	2,840		5,478	2,414	2,842
				1999			
LATE	-0.0219	-0.0737**	0.0245		0.215***	0.187***	0.230***
	(0.0228)	(0.0318)	(0.0325)		(0.0326)	(0.0481)	(0.0463)
LATE	-0.0416	-0.0777**	-0.000568		0.195***	0.160***	0.212***
	(0.0308)	(0.0383)	(0.0448)		(0.0378)	(0.0582)	(0.0538)
LATE	-0.0204	-0.0727**	0.0255		0.220***	0.194***	0.234***
	(0.0218)	(0.0312)	(0.0308)		(0.0322)	(0.0473)	(0.0459)
	5,076	2,306	2,564		5,076	2,306	2,564
				2002			
LATE	0.0428*	0.0389	0.0529*		0.225***	0.232***	0.221***
	(0.0223)	(0.0320)	(0.0307)		(0.0335)	(0.0492)	(0.0474)
LATE	0.0749**	0.0418	0.0851*		0.218***	0.240***	0.230***
	(0.0312)	(0.0336)	(0.0439)		(0.0423)	(0.0622)	(0.0497)
LATE	0.0367*	0.0379	0.0458		0.219***	0.224***	0.219***
	(0.0210)	(0.0315)	(0.0291)		(0.0329)	(0.0483)	(0.0472)
	5,188	2,341	2,590		5,189	2,342	2,590

Source: PNADs of 1997, 1999, and 2002.

Note: Bootstrapped standard errors with 50 repetitions in parentheses. *, **, *** Statistically significant at 1%, 5% and 10% respectively.

Table 8 – Difference-in-Differences Estimates for the Impact of the Law of 1998 on Work Incidence

Eligible Group = 14; Control Group = 13
Bandwidth of 6 Months

	Labour Force Participation			Formal Paid Work			Informal Work			Domestic Work		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Eligible*Time (DD)	-0.025*	-0.043*	-0.0055	-0.070***	-0.10***	-0.021	0.013	0.0089	0.014	0.0042	-0.0024	-0.0019
	(-2.23)	(-2.46)	(-0.39)	(-3.62)	(-3.78)	(-0.80)	(1.14)	(0.50)	(1.03)	(0.34)	(-0.11)	(-0.14)
Eligible (14=1, 13=0)	0.087***	0.12***	0.061***	0.12***	0.15***	0.076***	-0.12***	-0.14***	-0.099***	0.0067	0.0079	0.0093
	(11.1)	(9.47)	(5.98)	(7.44)	(6.42)	(3.67)	(-11.6)	(-8.50)	(-7.75)	(0.77)	(0.53)	(0.91)
Time (1999=1, 1997=0)	-0.0035	0.0041	-0.012	0.00067	-0.0029	-0.000029	-0.029**	-0.033*	-0.026*	0.013	0.025	0.0082
	(-0.48)	(0.36)	(-1.26)	(0.16)	(-0.36)	(-0.0051)	(-3.15)	(-2.25)	(-2.22)	(1.50)	(1.68)	(0.80)
Male	0.090***	*Na*	*Na*	0.019	*Na*	*Na*	0.065***			-0.34***	*Na*	*Na*
	(16.2)	*Na*	*Na*	(1.45)	*Na*	*Na*	(11.8)			(-55.9)	*Na*	*Na*
White	-0.032***	-0.033***	-0.032***	0.033*	0.035	0.023	-0.015**	-0.014	-0.015*	-0.016*	-0.013	-0.011
	(-5.51)	(-3.53)	(-4.43)	(2.31)	(1.64)	(1.29)	(-2.63)	(-1.50)	(-2.11)	(-2.49)	(-1.19)	(-1.46)
Mother's years of schooling	-0.0080***	-0.0098***	-0.0070***	-0.0024	0.000063	-0.0042	0.010***	0.011**	0.0095**	-0.0041***	-0.0034	-0.0031*
	(-7.70)	(-5.89)	(-5.34)	(-0.81)	(0.016)	(-0.83)	(3.78)	(2.59)	(2.91)	(-3.49)	(-1.67)	(-2.31)
Father's years of schooling	-0.0055***	-0.0082***	-0.0034**	0.0031	0.00069	0.0053	-0.0070***	-0.010***	-0.0043***	-0.0047***	-0.0027	-0.0060***
	(-5.38)	(-4.97)	(-2.64)	(1.06)	(0.19)	(1.07)	(-6.74)	(-6.08)	(-3.34)	(-4.12)	(-1.34)	(-4.60)
# of Siblings 0-5	0.017***	0.020***	0.014***	-0.0042	-0.0052	-0.0035	-0.0052***	-0.0077***	-0.0031*	0.015***	0.022***	0.011***
	(6.59)	(4.84)	(4.31)	(-0.83)	(-0.62)	(-0.67)	(-5.11)	(-4.65)	(-2.42)	(5.87)	(4.83)	(3.69)
# of Siblings 6-11	0.0083**	0.010*	0.0056	0.00053	-0.0036	0.0032	0.00086	0.0026	-0.0012	0.0096***	0.013**	0.0095**
	(3.18)	(2.51)	(1.71)	(0.10)	(-0.43)	(0.53)	(0.32)	(0.59)	(-0.37)	(3.52)	(2.72)	(3.11)
# of Siblings 12-13	0.011**	0.012	0.010	-0.015	-0.018	-0.010	0.0088*	0.012	0.0062	-0.0010	-0.0021	0.0011
	(2.66)	(1.69)	(1.90)	(-1.85)	(-1.37)	(-1.22)	(2.19)	(1.85)	(1.23)	(-0.22)	(-0.26)	(0.22)
# of Siblings 14-15	0.00083	0.0017	-0.00022	-0.0044	-0.0097	0.0022	0.012**	0.017**	0.0072	0.0090*	0.012	0.0075
	(0.21)	(0.27)	(-0.042)	(-0.51)	(-0.84)	(0.16)	(2.78)	(2.61)	(1.37)	(2.02)	(1.55)	(1.48)
# of Siblings 16-17	0.0067	0.0076	0.0038	-0.013	-0.025*	0.00020	0.0066	0.014*	0.00081	-0.0028	-0.014*	0.014**
	(1.77)	(1.28)	(0.79)	(-1.55)	(-2.05)	(0.017)	(1.72)	(2.20)	(0.17)	(-0.66)	(-1.96)	(2.86)
# of Siblings >=18	-0.0065***	-0.0096***	-0.0035*	-0.0018	-0.0024	-0.0023	-0.0022	-0.0044	-0.00037	-0.0030*	-0.0061*	-0.0024
	(-5.44)	(-5.19)	(-2.25)	(-0.65)	(-0.51)	(-0.85)	(-1.33)	(-1.74)	(-0.18)	(-2.10)	(-2.50)	(-1.43)
Land Title	-0.011	-0.0058	-0.014	0.0057	-0.040	0.043**	-0.0078	0.0045	-0.016	-0.012	-0.010	-0.013
	(-1.00)	(-0.34)	(-1.02)	(0.25)	(-0.94)	(3.29)	(-0.72)	(0.27)	(-1.16)	(-1.02)	(-0.52)	(-1.02)
Non-labour Household Income	0.0000099	-0.000056	0.000074	-0.00062***	-0.00055*	-0.00062	-0.000100*	-0.00010	-0.00011*	-0.00011	-0.00026	-0.00019
	(0.087)	(-0.61)	(0.51)	(-3.49)	(-2.43)	(-1.50)	(-2.37)	(-1.38)	(-2.17)	(-0.82)	(-1.34)	(-1.29)

Cont.

Metropolitan Region	-0.079***	-0.10***	-0.058***	0.026	0.043	0.011	-0.071***	-0.098***	-0.049***	-0.011	0.019	-0.037***
	(-13.8)	(-11.4)	(-7.94)	(1.52)	(1.65)	(0.55)	(-12.9)	(-11.1)	(-7.04)	(-1.64)	(1.67)	(-4.86)
Constant	0.28***	0.42***	0.26***	-0.0089	0.061	-0.040	0.32***	0.45***	0.27***	0.94***	0.57***	0.92***
	(19.0)	(18.0)	(13.7)	(-0.31)	(1.19)	(-1.45)	(20.9)	(19.0)	(13.4)	(60.2)	(20.6)	(51.7)
N	19107	8864	10243	1173	653	520	14992	6839	8153	19091	8854	10237
Adjusted R2	0.07	0.08	0.04	0.05	0.07	0.01	0.08	0.10	0.04	0.15	0.01	0.02

Note: Robust T statistics in parentheses. *, **,*** Statistically significant at 10%, 5% and 1% respectively.

Table 9 – Difference-in-Differences Estimates for the Impact of the Law of 1998 on Work Intensity

Eligible Group = 14; Control Group = 13

Bandwidth of 6 Months

	Weekly Hours Worked - Formal			Weekly Hours Worked - Informal		
	All	Boys	Girls	All	Boys	Girls
Eligible*Time (DD)	-2.45*	-3.46*	-1.06	1.84	1.46	2.77
	(-2.10)	(-2.24)	(-0.61)	(1.21)	(0.74)	(1.14)
Eligible (14=1, 13=0)	-4.65***	-5.09**	-4.21*	-0.40	-0.25	-0.46
	(-3.80)	(-3.14)	(-2.24)	(-0.28)	(-0.13)	(-0.20)
Time (1999=1, 1997=0)	-0.25	-0.79	0.36	-0.61	0.14	-1.63
	(-0.30)	(-0.70)	(0.30)	(-0.68)	(0.12)	(-1.16)
Male	2.58***	*Na*	*Na*	-2.02**	*Na*	*Na*
	(3.99)	*Na*	*Na*	(-2.71)	*Na*	*Na*
White	5.06***	5.24***	4.69***	-5.63***	-5.25***	-6.16***
	(7.46)	(5.61)	(4.78)	(-7.34)	(-5.23)	(-5.17)
Mother's years of schooling	-0.63*	-0.93**	-0.20	1.20***	1.71***	0.45
	(-2.47)	(-2.72)	(-0.53)	(3.73)	(4.10)	(0.91)
Father's years of schooling	-0.026	-0.039	0.018	-0.090	-0.27	0.16
	(-0.21)	(-0.23)	(0.10)	(-0.63)	(-1.42)	(0.72)
# of Siblings 0-5	0.31*	0.41*	0.19	-0.63***	-0.62***	-0.68**
	(2.53)	(2.36)	(1.13)	(-4.44)	(-3.31)	(-3.16)
# of Siblings 6-11	-0.048	0.18	-0.41	0.22	-0.25	0.95
	(-0.16)	(0.44)	(-1.06)	(0.62)	(-0.57)	(1.68)
# of Siblings 12-13	-0.73	-0.23	-1.33*	0.11	-0.23	0.60
	(-1.72)	(-0.39)	(-2.21)	(0.22)	(-0.35)	(0.75)
# of Siblings 14-15	-0.46	-1.05	0.23	0.090	0.78	-0.71
	(-0.97)	(-1.64)	(0.33)	(0.17)	(1.16)	(-0.82)
# of Siblings 16-17	-0.98*	-1.67**	-0.060	1.10*	1.88**	-0.071
	(-2.24)	(-2.89)	(-0.089)	(2.10)	(2.87)	(-0.085)
# of Siblings >=18	0.0014	0.0037	-0.081	0.030	-0.032	0.17
	(0.0071)	(0.014)	(-0.28)	(0.13)	(-0.10)	(0.48)
Land Title	-0.18	-2.06	2.66	-1.20	0.53	-3.93
	(-0.14)	(-1.11)	(1.83)	(-0.85)	(0.29)	(-1.81)
Non-labour Household Income	0.0012	0.014	-0.020	-0.030*	-0.041*	-0.010
	(0.087)	(1.10)	(-0.54)	(-2.26)	(-2.37)	(-0.33)
Metropolitan Region	2.20**	1.93	2.58*	-2.89***	-2.48*	-3.69**
	(2.99)	(1.91)	(2.41)	(-3.49)	(-2.27)	(-2.89)
Constant	4.96**	8.93***	2.86	37.5***	34.6***	38.9***
	(3.14)	(3.99)	(1.45)	(20.5)	(14.8)	(14.5)
N	2670	1566	1104	2492	1491	1001
Adjusted R2	0.08	0.09	0.06	0.07	0.08	0.06

Note: Robust T statistics in parentheses. *, **,*** Statistically significant at 10%, 5% and 1% respectively.

Table 10 – Difference-in-Differences Estimates for the Impact of the Law of 1998 on School Outcomes
Eligible Group = 14; Control Group = 13
Bandwidth of 6 Months

	School Attendance			Schooling Delay		
	All	Boys	Girls	All	Boys	Girls
Eligible*Time (DD)	0.018**	0.019*	0.017*	0.028**	0.039**	0.020
	(2.51)	(1.85)	(1.79)	(2.30)	(2.29)	(1.20)
Eligible (14=1, 13=0)	-0.043***	-0.046***	-0.041***	0.028***	0.015	0.040***
	(-7.99)	(-5.91)	(-5.44)	(3.41)	(1.28)	(3.33)
Time (1999=1, 1997=0)	0.017***	0.017***	0.017***	-0.073***	-0.081***	-0.069***
	(3.84)	(2.62)	(2.74)	(-8.35)	(-6.47)	(-5.52)
Male	-0.0071**	*Na*	*Na*	0.087***	*Na*	*Na*
	(-2.00)	*Na*	*Na*	(14.4)	*Na*	*Na*
White	0.021***	0.012**	0.029***	-0.14***	-0.14***	-0.15***
	(5.73)	(2.24)	(5.66)	(-21.8)	(-14.8)	(-16.3)
Mother's years of schooling	0.0053***	0.0053***	0.0053***	-0.019***	-0.019***	-0.019***
	(8.50)	(5.87)	(6.25)	(-15.8)	(-10.9)	(-11.6)
Father's years of schooling	0.0017***	0.0028***	0.00079	-0.015***	-0.016***	-0.015***
	(2.82)	(3.00)	(0.95)	(-12.8)	(-9.27)	(-8.99)
Children 0-5	-0.015***	-0.015***	-0.015***	0.035***	0.031***	0.039***
	(-8.24)	(-5.24)	(-6.27)	(14.8)	(9.27)	(11.4)
Children 6-11	-0.0035**	-0.0068***	-0.00059	0.019***	0.018***	0.020***
	(-1.97)	(-2.59)	(-0.25)	(7.62)	(4.95)	(5.59)
Children 12-13	0.0017	-0.0013	0.0044	0.0024	-0.0074	0.011*
	(0.62)	(-0.30)	(1.16)	(0.57)	(-1.20)	(1.77)
Children 14-15	0.0022	0.00042	0.0039	0.032***	0.027***	0.037***
	(0.87)	(0.12)	(1.14)	(7.66)	(4.68)	(6.13)
Children 16-17	-0.0016	-0.0069*	0.0030	0.017***	0.018***	0.015***
	(-0.63)	(-1.83)	(0.86)	(4.45)	(3.42)	(2.66)
Children 18-30	0.0012	-0.00040	0.0025**	0.0032**	0.0037*	0.0031
	(1.45)	(-0.34)	(2.16)	(2.35)	(1.93)	(1.64)
Land Title	0.027***	0.027**	0.028**	-0.047***	-0.054***	-0.041***
	(3.40)	(2.27)	(2.52)	(-4.64)	(-3.88)	(-2.79)
Non-labour Household Income	-0.00011**	-0.000084	-0.00012*	-0.00021**	-0.00019	-0.00018
	(-2.18)	(-1.23)	(-1.72)	(-2.16)	(-1.08)	(-1.56)
Metropolitan Region	0.013***	0.019***	0.0078	0.016**	0.016*	0.016*
	(3.53)	(3.69)	(1.50)	(2.42)	(1.75)	(1.74)
Constant	0.86***	0.87***	0.85***	0.93***	1.04***	0.92***
	(79.5)	(56.5)	(57.2)	(63.8)	(52.7)	(46.0)
N	19104	8864	10240	19107	8864	10243
Adjusted R2	0.03	0.04	0.03	0.17	0.17	0.16

Note: Robust T statistics in parentheses. *, **,*** Statistically significant at 10%, 5% and 1% respectively.

Table 11 – Difference-in-Differences Estimates for the Impact of the Law of 1998 on Work Incidence

Checking the Antecipation Bias

Eligible Group = 14; Control Group = 13

Bandwidth of 6 Months

	Labour Force Participation			Formal Paid Work			Informal Work			Domestic Work		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Eligible*Time (DD)	-0.010	0.0044	-0.021	-0.0063	-0.070	0.073	0.00052	-0.013	0.013	-0.0054	0.0075	-0.022
	(-0.85)	(0.23)	(-1.42)	(-0.19)	(-1.44)	(1.54)	(0.045)	(-0.71)	(0.86)	(-0.38)	(0.31)	(-1.30)
Eligible (14=1, 13=0)	0.073***	0.071***	0.075***	0.047	0.11*	-0.026	-0.11***	-0.13***	-0.100***	0.015	-0.0024	0.027*
	(8.16)	(4.94)	(6.76)	(1.49)	(2.34)	(-0.59)	(-10.2)	(-7.23)	(-7.10)	(1.40)	(-0.13)	(2.03)
Time (1999=1, 1997=0)	0.019*	0.0090	0.026**	-0.028	-0.014	-0.044	0.00072	0.011	-0.0095	0.021*	0.010	0.031*
	(2.45)	(0.71)	(2.77)	(-1.19)	(-0.45)	(-1.13)	(0.074)	(0.71)	(-0.76)	(2.02)	(0.60)	(2.55)
Male	0.082***	*Na*	*Na*	0.013	*Na*	*Na*	0.051***	*Na*	*Na*	-0.33***	*Na*	*Na*
	(13.8)	*Na*	*Na*	(0.86)	*Na*	*Na*	(8.73)	*Na*	*Na*	(-48.4)	*Na*	*Na*
White	-0.029***	-0.032**	-0.028***	0.040*	0.042	0.037	-0.010	-0.011	-0.0088	-0.015*	0.0057	-0.022**
	(-4.74)	(-3.21)	(-3.67)	(2.51)	(1.85)	(1.65)	(-1.69)	(-1.13)	(-1.15)	(-2.12)	(0.46)	(-2.65)
Mother's years of schooling	-0.0071***	-0.011***	-0.0041**	-0.0030	-0.0036	-0.00025	0.0088**	0.012**	0.0066	-0.0044***	-0.0027	-0.0046**
	(-6.47)	(-6.19)	(-3.00)	(-1.14)	(-0.92)	(-0.087)	(3.23)	(2.67)	(1.95)	(-3.39)	(-1.20)	(-3.09)
Father's years of schooling	-0.0058***	-0.0084***	-0.0038**	0.0050	0.0051	0.0036	-0.0060***	-0.0090***	-0.0034*	-0.0062***	-0.0058**	-0.0060***
	(-5.27)	(-4.69)	(-2.85)	(1.89)	(1.31)	(1.03)	(-5.43)	(-4.99)	(-2.50)	(-4.84)	(-2.59)	(-4.05)
# of Siblings 0-5	0.016***	0.015***	0.017***	-0.010	-0.014	-0.0051	-0.0050***	-0.0073***	-0.0029*	0.015***	0.020***	0.010**
	(5.91)	(3.48)	(4.98)	(-1.80)	(-1.62)	(-0.70)	(-4.50)	(-4.03)	(-2.11)	(5.34)	(4.17)	(3.05)
# of Siblings 6-11	0.0061*	0.010*	0.0022	-0.0069	-0.0058	-0.0095	0.0058*	0.0013	0.0089**	0.0068*	0.0088	0.0071*
	(2.26)	(2.29)	(0.66)	(-1.43)	(-0.75)	(-1.63)	(2.18)	(0.31)	(2.64)	(2.32)	(1.66)	(2.15)
# of Siblings 12-13	0.0032	-0.0022	0.0080	0.0043	0.0043	0.0044	0.0013	-0.00071	0.0026	0.0048	-0.0019	0.011
	(0.73)	(-0.32)	(1.42)	(0.46)	(0.28)	(0.49)	(0.32)	(-0.12)	(0.49)	(0.99)	(-0.23)	(1.95)
# of Siblings 14-15	-0.0035	-0.0033	-0.0033	-0.015	-0.014	-0.015	0.000075	0.00078	0.00090	0.011*	0.016	0.0065
	(-0.83)	(-0.50)	(-0.59)	(-1.86)	(-0.96)	(-1.90)	(0.018)	(0.12)	(0.17)	(2.25)	(1.91)	(1.14)
# of Siblings 16-17	0.0078	0.00068	0.012*	0.00080	-0.0025	0.0049	-0.00014	0.0030	-0.0034	-0.0032	-0.015	0.013*
	(1.93)	(0.11)	(2.32)	(0.091)	(-0.21)	(0.40)	(-0.035)	(0.47)	(-0.70)	(-0.72)	(-1.88)	(2.46)
# of Siblings >=18	-0.0038**	-0.0049*	-0.0030	-0.0015	-0.00019	-0.0029	0.0013	-0.0026	0.0044*	-0.0025	-0.0041	-0.0023
	(-3.04)	(-2.38)	(-1.92)	(-0.47)	(-0.037)	(-0.73)	(0.77)	(-1.04)	(2.03)	(-1.65)	(-1.58)	(-1.23)
Land Title	-0.019	-0.020	-0.016	0.011	0.025	-0.0012	0.00050	-0.014	0.013	-0.017	-0.045*	0.0027
	(-1.62)	(-1.09)	(-1.09)	(0.49)	(0.76)	(-0.034)	(0.046)	(-0.77)	(1.00)	(-1.35)	(-2.03)	(0.19)
Non-labour Household Income	0.00010	0.00035	0.000049	-0.00043***	-0.00050***	-0.00014	-0.000058	0.000049	-0.00013*	0.000049	0.00027	-0.00017

	(0.79)	(1.33)	(0.34)	(-4.63)	(-3.70)	(-0.57)	(-0.81)	(0.32)	(-2.11)	(0.32)	(0.94)	(-1.17)
Metropolitan Region	-0.053***	-0.070***	-0.038***	0.052**	0.070*	0.031	-0.059***	-0.083***	-0.038***	-0.014	0.0056	-0.030***
	(-8.58)	(-7.06)	(-4.97)	(2.85)	(2.55)	(1.26)	(-10.00)	(-8.82)	(-5.14)	(-1.94)	(0.45)	(-3.50)
Constant	0.25***	0.41***	0.19***	0.0052	-0.019	0.043	0.27***	0.41***	0.19***	0.94***	0.62***	0.89***
	(15.2)	(15.5)	(9.54)	(0.12)	(-0.35)	(0.69)	(16.5)	(15.3)	(9.39)	(50.9)	(19.2)	(41.2)
N	15617	7169	8448	787	438	349	12411	5690	6721	15612	7166	8446
Adjusted R2	0.06	0.06	0.03	0.05	0.07	0.01	0.06	0.09	0.04	0.14	0.01	0.02

Note: Robust T statistics in parentheses. *, **,*** Statistically significant at 10%, 5% and 1% respectively.

Table 12 – Difference-in-Differences Estimates for the Impact of the Law of 1998 on Work Intensity

Checking the Antecipation Bias
Eligible Group = 14; Control Group = 13
Bandwidth of 6 Months

	Weekly Hours Worked - Formal			Weekly Hours Worked - Informal		
	All	Boys	Girls	All	Boys	Girls
Eligible*Time (DD)	-0.100	-1.81	2.63	-0.13	0.11	-0.25
	(-0.074)	(-0.96)	(1.43)	(-0.068)	(0.045)	(-0.087)
Eligible (14=1, 13=0)	-7.42***	-6.18**	-9.13***	2.41	1.23	4.11
	(-5.15)	(-3.04)	(-4.66)	(1.29)	(0.49)	(1.45)
Time (1999=1, 1997=0)	0.029	-0.31	0.17	-0.27	0.79	-1.69
	(0.028)	(-0.23)	(0.12)	(-0.24)	(0.54)	(-0.96)
Male	2.38**	*Na*	*Na*	-2.39**	*Na*	*Na*
	(3.23)	*Na*	*Na*	(-2.72)	*Na*	*Na*
White	5.35***	6.30***	3.90***	-5.80***	-5.98***	-5.40***
	(6.97)	(5.98)	(3.51)	(-6.41)	(-4.97)	(-3.90)
Mother's years of schooling	-0.42	-0.59	-0.19	0.71	0.92	0.36
	(-1.42)	(-1.47)	(-0.44)	(1.89)	(1.90)	(0.61)
Father's years of schooling	0.10	0.048	0.18	-0.10	-0.37	0.26
	(0.68)	(0.23)	(0.88)	(-0.59)	(-1.65)	(0.96)
# of Siblings 0-5	0.20	0.34	0.0067	-0.63***	-0.65**	-0.61*
	(1.41)	(1.68)	(0.035)	(-3.74)	(-2.91)	(-2.40)
# of Siblings 6-11	-0.50	0.15	-1.36**	1.05**	0.42	1.83**
	(-1.64)	(0.36)	(-3.28)	(2.62)	(0.80)	(2.85)
# of Siblings 12-13	0.024	0.39	-0.48	-0.44	-0.80	0.0090
	(0.050)	(0.58)	(-0.72)	(-0.74)	(-1.03)	(0.0100)
# of Siblings 14-15	0.11	0.59	-0.51	-0.53	-0.85	0.044
	(0.20)	(0.78)	(-0.73)	(-0.88)	(-1.07)	(0.048)
# of Siblings 16-17	0.049	-0.29	0.50	-0.34	-0.16	-0.64
	(0.097)	(-0.43)	(0.66)	(-0.56)	(-0.21)	(-0.69)
# of Siblings >=18	-0.020	-0.14	0.091	0.29	0.32	0.27
	(-0.088)	(-0.43)	(0.29)	(1.08)	(0.86)	(0.71)
Land Title	0.67	0.21	2.15	-2.27	-2.34	-2.82
	(0.45)	(0.10)	(1.11)	(-1.36)	(-1.09)	(-1.06)
Non-labour Household Income	0.0016	-0.0097	0.020	-0.0047	0.00098	-0.015
	(0.15)	(-0.86)	(1.15)	(-0.39)	(0.060)	(-0.73)
Metropolitan Region	1.82*	1.82	1.67	-2.41*	-2.01	-3.06*
	(2.21)	(1.64)	(1.34)	(-2.53)	(-1.60)	(-2.08)
Constant	3.95	5.11	5.06	37.3***	37.3***	34.7***
	(1.95)	(1.85)	(1.85)	(16.1)	(12.6)	(10.0)
N	1951	1118	833	1803	1051	752
Adjusted R2	0.08	0.10	0.06	0.07	0.08	0.05

Note: Robust T statistics in parentheses. *, **,*** Statistically significant at 10%, 5% and 1% respectively.

Table 13 – Difference-in-Differences Estimates for the Impact of the Law of 1998 on School Outcomes

Checking the Antecipation Bias
Eligible Group = 14; Control Group = 13
Bandwidth of 6 Months

	School Attendance			Schooling Delay		
	All	Boys	Girls	All	Boys	Girls
Eligible*Time (DD)	0.0036	0.0045	0.0025	0.050***	0.042***	0.056***
	(0.49)	(0.42)	(0.26)	(5.19)	(3.04)	(4.24)
Eligible (14=1, 13=0)	-0.028***	-0.031***	-0.026***	0.0038	0.0091**	0.00027
	(-5.07)	(-3.75)	(-3.38)	(1.18)	(2.04)	(0.060)
Time (1999=1, 1998=0)	0.0034	0.0047	0.0023	-0.37***	-0.33***	-0.40***
	(0.74)	(0.71)	(0.37)	(-51.6)	(-32.5)	(-40.1)
Male	-0.0056	*Na*	*Na*	0.051***	*Na*	*Na*
	(-1.56)	*Na*	*Na*	(9.25)	*Na*	*Na*
White	0.017***	0.0082	0.024***	-0.090***	-0.082***	-0.098***
	(4.56)	(1.50)	(4.82)	(-15.3)	(-9.65)	(-12.1)
Mother's years of schooling	0.0033***	0.0038***	0.0028***	-0.011***	-0.010***	-0.012***
	(5.16)	(4.21)	(3.20)	(-10.1)	(-6.37)	(-7.93)
Father's years of schooling	0.0027***	0.0031***	0.0025***	-0.011***	-0.012***	-0.0098***
	(4.39)	(3.33)	(2.94)	(-9.76)	(-7.29)	(-6.65)
Children 0-5	-0.012***	-0.012***	-0.011***	0.016***	0.013***	0.019***
	(-6.43)	(-4.42)	(-4.62)	(7.50)	(4.06)	(6.41)
Children 6-11	-0.0039**	-0.0036	-0.0040*	0.012***	0.013***	0.011***
	(-2.21)	(-1.38)	(-1.71)	(5.39)	(3.94)	(3.59)
Children 12-13	0.0012	-0.0023	0.0044	-0.0029	-0.0091	0.0034
	(0.48)	(-0.58)	(1.24)	(-0.77)	(-1.72)	(0.66)
Children 14-15	0.0022	0.0021	0.0022	0.024***	0.023***	0.025***
	(0.90)	(0.58)	(0.66)	(6.50)	(4.27)	(4.80)
Children 16-17	-0.00080	-0.0017	-0.000016	0.011***	0.014***	0.0072
	(-0.33)	(-0.47)	(-0.0049)	(3.14)	(2.88)	(1.50)
Children 18-30	0.00072	0.00076	0.00067	0.0012	0.0017	0.00090
	(0.90)	(0.66)	(0.60)	(0.97)	(0.97)	(0.54)
Land Title	0.023***	0.011	0.032***	-0.027***	-0.032**	-0.021
	(2.82)	(0.96)	(2.85)	(-2.85)	(-2.40)	(-1.62)
Non-labour Household Income	-0.00010*	-0.00017	-0.000081	-0.000043	0.000056	-0.000049
	(-1.96)	(-1.40)	(-1.42)	(-0.43)	(0.29)	(-0.44)
Metropolitan Region	0.0078**	0.0041	0.011**	0.0094	0.00090	0.017**
	(2.08)	(0.76)	(2.09)	(1.59)	(0.11)	(2.05)
Constant	0.89***	0.90***	0.88***	1.16***	1.20***	1.17***
	(80.3)	(57.9)	(57.3)	(83.1)	(61.0)	(62.3)
N	15616	7169	8447	15617	7169	8448
Adjusted R2	0.02	0.03	0.02	0.25	0.23	0.26

Note: Robust T statistics in parenthesis. ***, **, * Statistically Significant at 1%, 5%, and 10% respectively.

Table 14 – Difference-in-Differences Estimates for the Impact of the Law of 1998 on Work Incidence

Controlling for Pre-Treatment Differences in Trends
Eligible Group = 15; Control Group = 16
Bandwidth of 6 Months

	Labour Force Participation			Formal Paid Work			Informal Work			Domestic Work		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Eligible*D98 (DD pre-intervention)	-0.0094	-0.012	-0.0092	-0.016	0.0088	-0.049	0.016	-0.0089	0.023	0.00012	-0.0085	0.0070
	(-0.64)	(-0.55)	(-0.47)	(-0.50)	(0.19)	(-1.12)	(0.49)	(-0.19)	(0.48)	(0.0092)	(-0.36)	(0.45)
Eligible*D99 (DD pos-intervention)	-0.021	-0.025	-0.022	-0.098***	-0.10**	-0.085*	0.098***	0.10**	0.066	-0.0071	-0.0094	0.0017
	(-1.60)	(-1.25)	(-1.21)	(-3.71)	(-2.84)	(-2.27)	(3.71)	(2.82)	(1.65)	(-0.60)	(-0.45)	(0.12)
Eligible (15=1, 16=0)	-0.11***	-0.11***	-0.10***	-0.044*	-0.060*	-0.031	0.030	0.025	0.062	0.010	0.022	0.00058
	(-11.6)	(-8.13)	(-8.11)	(-2.35)	(-2.28)	(-1.20)	(1.23)	(0.75)	(1.71)	(1.20)	(1.47)	(0.061)
Time (1998=1, 1997=0)	-0.022*	-0.045**	-0.00022	0.0022	-0.0050	0.015	-0.0023	0.0052	-0.011	-0.0017	0.029	-0.029**
	(-2.02)	(-2.78)	(-0.015)	(0.099)	(-0.16)	(0.50)	(-0.11)	(0.16)	(-0.36)	(-0.18)	(1.70)	(-2.64)
Time (1999=1, 1998=0)	-0.0048	-0.016	0.0068	0.0069	-0.010	0.028	-0.0071	0.010	-0.021	0.021*	0.035*	0.0044
	(-0.50)	(-1.12)	(0.52)	(0.37)	(-0.39)	(1.06)	(-0.38)	(0.40)	(-0.72)	(2.55)	(2.33)	(0.46)
Male	0.13***	*Na*	*Na*	0.064***	*Na*	*Na*	-0.064***	*Na*	*Na*	-0.38***	*Na*	*Na*
	(23.0)	*Na*	*Na*	(5.29)	*Na*	*Na*	(-5.29)	*Na*	*Na*	(-72.5)	*Na*	*Na*
White	-0.0050	0.0058	-0.016*	0.13***	0.12***	0.13***	-0.13***	-0.12***	-0.13***	-0.016**	0.0041	-0.022***
	(-0.83)	(0.64)	(-2.03)	(10.2)	(6.82)	(7.39)	(-10.2)	(-6.80)	(-6.76)	(-3.02)	(0.43)	(-3.64)
Mother's years of schooling	-0.013***	-0.017***	-0.011***	0.0026	0.0017	0.0045	-0.0029	-0.0020	-0.0030	-0.0056***	-0.0060***	-0.0041***
	(-11.9)	(-9.72)	(-7.48)	(1.05)	(0.51)	(1.27)	(-1.21)	(-0.58)	(-0.82)	(-5.62)	(-3.35)	(-3.68)
Father's years of schooling	-0.0084***	-0.012***	-0.0056***	0.0097***	0.014***	0.0047	-0.0092***	-0.014***	-0.0045	-0.0024*	0.0020	-0.0061***
	(-7.59)	(-6.99)	(-3.82)	(4.03)	(4.11)	(1.36)	(-3.84)	(-4.05)	(-1.27)	(-2.45)	(1.14)	(-5.58)
# of Siblings 0-5	0.015***	0.014***	0.016***	-0.018***	-0.022***	-0.011	0.018***	0.022**	0.010	0.014***	0.017***	0.012***
	(5.83)	(3.52)	(4.75)	(-3.78)	(-3.31)	(-1.65)	(3.75)	(3.29)	(1.38)	(6.44)	(4.12)	(5.01)
# of Siblings 6-11	0.011***	0.0097*	0.011**	-0.0050	0.0068	-0.022**	0.0049	-0.0068	0.022**	0.0081***	0.011*	0.0072**
	(4.04)	(2.34)	(3.10)	(-0.94)	(0.87)	(-3.05)	(0.91)	(-0.88)	(2.90)	(3.40)	(2.51)	(2.71)
# of Siblings 12-13	0.0083*	0.0074	0.0067	-0.018*	-0.016	-0.020	0.018*	0.016	0.020	0.0011	0.0020	0.0057
	(2.06)	(1.25)	(1.23)	(-2.22)	(-1.43)	(-1.84)	(2.22)	(1.42)	(1.81)	(0.30)	(0.32)	(1.40)
# of Siblings 14-15	0.016***	0.023***	0.0089	-0.0057	-0.0058	-0.0075	0.0056	0.0057	0.013	0.00015	0.0021	0.0013
	(3.82)	(3.70)	(1.61)	(-0.67)	(-0.49)	(-0.64)	(0.66)	(0.48)	(1.04)	(0.040)	(0.33)	(0.31)
# of Siblings 16-17	0.0068	0.0036	0.0087	-0.020*	-0.040***	0.0048	0.020*	0.041***	-0.0024	-0.010**	-0.016*	-0.0034
	(1.69)	(0.60)	(1.62)	(-2.35)	(-3.59)	(0.37)	(2.34)	(3.60)	(-0.18)	(-2.84)	(-2.52)	(-0.81)
# of Siblings >=18	-0.0062***	-0.0066***	-0.0062***	0.0064*	0.0052	0.0079*	-0.0065*	-0.0051	-0.0067	-0.0021	-0.0024	-0.0016

	(-5.02)	(-3.51)	(-3.75)	(2.38)	(1.38)	(2.03)	(-2.40)	(-1.35)	(-1.60)	(-1.90)	(-1.18)	(-1.27)
Land Title	0.0042	-0.014	0.022	0.038	0.026	0.060*	-0.038	-0.026	-0.063*	0.010	0.0056	0.011
	(0.38)	(-0.87)	(1.46)	(1.81)	(0.86)	(2.11)	(-1.80)	(-0.86)	(-2.09)	(1.06)	(0.33)	(0.96)
Non-labour Household Income	-0.000075	-0.000017	-0.000076	-0.00016	-0.000036	-0.00026	0.00016	0.000036	0.00016	-0.0000055	0.00022	-0.00020*
	(-1.05)	(-0.12)	(-0.98)	(-0.62)	(-0.097)	(-0.66)	(0.61)	(0.096)	(0.39)	(-0.065)	(1.45)	(-2.03)
Metropolitan Region	-0.065***	-0.096***	-0.039***	0.041**	0.048*	0.034	-0.041**	-0.048*	-0.049*	0.0050	0.025**	-0.013*
	(-10.8)	(-10.5)	(-4.83)	(3.01)	(2.55)	(1.76)	(-3.01)	(-2.55)	(-2.35)	(0.92)	(2.64)	(-2.02)
Constant	0.59***	0.82***	0.53***	0.024	0.11*	0.0044	1.00***	0.94***	0.97***	0.92***	0.48***	0.93***
	(37.6)	(35.9)	(25.2)	(0.82)	(2.52)	(0.12)	(32.3)	(21.6)	(22.5)	(67.7)	(19.8)	(60.1)
N	26527	12054	14473	4491	2458	2033	4491	2458	1776	26515	12048	14467
Adjusted R2	0.08	0.09	0.04	0.07	0.08	0.06	0.07	0.08	0.06	0.17	0.00	0.02

Note: T statistics in parentheses. *, **,*** Statistically significant at 10%, 5% and 1% respectively.

Table 15 – Difference-in-Differences Estimates for the Impact of the Law of 1998 on Work Intensity

Controlling for Pre-Treatment Differences in Trends
Eligible Group = 15; Control Group = 16
Bandwidth of 6 Months

	Weekly Hours Worked - Formal			Weekly Hours Worked - Informal		
	All	Boys	Girls	All	Boys	Girls
Eligible*D98 (DD pre-intervention)	-0.56	0.37	-1.16	-1.94	-1.66	-2.52
	(-0.42)	(0.19)	(-0.60)	(-1.20)	(-0.76)	(-0.95)
Eligible*D99 (DD pos-intervention)	-3.70***	-3.95*	-2.34	2.17	1.98	1.01
	(-3.35)	(-2.57)	(-1.38)	(1.60)	(1.11)	(0.45)
Eligible (15=1, 16=0)	-1.72	-2.02	-2.43	1.12	0.13	3.73
	(-1.68)	(-1.44)	(-1.53)	(0.93)	(0.083)	(1.92)
Time (1998=1, 1997=0)	-0.46	-0.60	-0.064	1.22	0.76	1.38
	(-0.50)	(-0.45)	(-0.048)	(1.13)	(0.51)	(0.82)
Time (1999=1, 1998=0)	-0.051	-0.65	0.37	-0.76	0.074	-1.16
	(-0.064)	(-0.58)	(0.31)	(-0.83)	(0.060)	(-0.78)
Male	2.46***	*Na*	*Na*	-3.74***	*Na*	*Na*
	(4.86)	*Na*	*Na*	(-6.02)	*Na*	*Na*
White	5.48***	5.24***	5.46***	-6.81***	-5.62***	-8.19***
	(10.3)	(7.01)	(6.88)	(-10.8)	(-6.64)	(-8.05)
Mother's years of schooling	0.050	-0.034	0.097	-0.15	-0.20	-0.038
	(0.49)	(-0.24)	(0.64)	(-1.23)	(-1.29)	(-0.20)
Father's years of schooling	0.29**	0.48***	0.11	-0.53***	-0.75***	-0.23
	(2.92)	(3.35)	(0.75)	(-4.38)	(-4.73)	(-1.21)
# of Siblings 0-5	-0.67***	-0.92**	-0.28	1.41***	1.61***	1.02*
	(-3.29)	(-3.28)	(-0.89)	(5.38)	(4.72)	(2.33)
# of Siblings 6-11	-0.15	0.39	-0.90**	0.46	-0.34	1.86***
	(-0.70)	(1.21)	(-2.91)	(1.65)	(-0.92)	(4.17)
# of Siblings 12-13	-0.77*	-0.61	-0.91	0.70	0.97	0.47
	(-2.35)	(-1.30)	(-1.94)	(1.69)	(1.73)	(0.72)
# of Siblings 14-15	-0.23	-0.42	-0.27	0.59	0.62	1.07
	(-0.64)	(-0.82)	(-0.49)	(1.38)	(1.11)	(1.48)
# of Siblings 16-17	-0.70*	-1.47**	0.16	0.46	1.22*	-0.52
	(-1.98)	(-3.20)	(0.28)	(1.08)	(2.20)	(-0.75)
# of Siblings >=18	0.31**	0.22	0.34	-0.49***	-0.46**	-0.47*
	(2.76)	(1.43)	(1.96)	(-3.61)	(-2.58)	(-2.10)
Land Title	1.45	1.06	2.37	0.042	0.98	-1.30
	(1.61)	(0.81)	(1.88)	(0.038)	(0.69)	(-0.71)
Non-labour Household Income	-0.011	-0.0095	-0.0073	-0.0046	-0.0022	-0.0091
	(-1.29)	(-1.02)	(-0.50)	(-0.39)	(-0.11)	(-0.61)
Metropolitan Region	1.18*	1.43	1.46	-2.38***	-2.66**	-2.72*
	(2.11)	(1.84)	(1.66)	(-3.55)	(-2.98)	(-2.48)
Constant	1.39	4.17*	1.70	41.6***	38.7***	38.7***
	(1.06)	(2.25)	(0.92)	(26.4)	(18.9)	(15.5)
N	4491	2458	1776	4491	2458	1776
Adjusted R2	0.06	0.07	0.05	0.07	0.08	0.07

Table 16 – Difference-in-Differences Estimates for the Impact of the Law of 1998 on School Outcomes

Controlling for Pre-Treatment Differences in Trends
Eligible Group = 15; Control Group = 16
Bandwidth of 6 Months

	School Attendance			Schooling Delay		
	All	Boys	Girls	All	Boys	Girls
Eligible*D98 (DD pre-intervention)	-0.010	-0.035*	0.010	0.028***	0.033**	0.026*
	(-0.95)	(-2.19)	(0.68)	(3.40)	(3.00)	(2.20)
Eligible*D99 (DD pos-intervention)	-0.019	-0.035*	-0.0049	-0.0064	-0.018	0.0027
	(-1.88)	(-2.34)	(-0.37)	(-0.57)	(-1.12)	(0.17)
Eligible (15=1, 16=0)	0.070***	0.087***	0.055***	-0.028***	-0.031**	-0.026*
	(9.48)	(7.86)	(5.57)	(-3.73)	(-3.00)	(-2.40)
Time (1998=1, 1997=0)	0.038***	0.062***	0.019*	0.20***	0.17***	0.23***
	(4.56)	(4.93)	(1.67)	(35.0)	(21.3)	(27.7)
Time (1999=1, 1998=0)	0.059***	0.072***	0.046***	-0.052***	-0.052***	-0.052***
	(7.78)	(6.36)	(4.62)	(-6.75)	(-4.83)	(-4.71)
Male	-0.016***	*Na*	*Na*	0.060***	*Na*	*Na*
	(-3.78)	*Na*	*Na*	(14.1)	*Na*	*Na*
White	0.023***	0.027***	0.020***	-0.096***	-0.086***	-0.11***
	(5.26)	(4.14)	(3.35)	(-21.6)	(-13.7)	(-17.0)
Mother's years of schooling	0.0082***	0.0082***	0.0080***	-0.012***	-0.011***	-0.013***
	(10.3)	(6.78)	(7.74)	(-13.6)	(-8.76)	(-10.5)
Father's years of schooling	0.0072***	0.0096***	0.0051***	-0.011***	-0.012***	-0.010***
	(9.28)	(8.12)	(5.00)	(-13.1)	(-10.5)	(-8.54)
Children 0-5	-0.041***	-0.030***	-0.050***	0.022***	0.014***	0.027***
	(-19.2)	(-9.29)	(-17.3)	(12.9)	(5.70)	(12.0)
Children 6-11	-0.0065**	-0.011***	-0.0031	0.013***	0.0083**	0.017***
	(-3.00)	(-3.30)	(-1.07)	(7.08)	(3.26)	(6.56)
Children 12-13	0.0061*	-0.0021	0.013**	0.0060*	0.010**	0.0016
	(2.05)	(-0.48)	(3.26)	(2.15)	(2.68)	(0.40)
Children 14-15	-0.00028	-0.0026	0.0019	0.00081	-0.00022	0.00099
	(-0.091)	(-0.60)	(0.44)	(0.28)	(-0.054)	(0.24)
Children 16-17	-0.0026	-0.0030	-0.0018	0.017***	0.012**	0.021***
	(-0.85)	(-0.66)	(-0.44)	(6.10)	(2.95)	(5.33)
Children 18-30	0.0033***	0.00073	0.0053***	0.0018*	0.0033**	0.00074
	(3.58)	(0.53)	(4.21)	(2.03)	(2.61)	(0.58)
Land Title	0.029**	0.018	0.039**	-0.026***	-0.0095	-0.039***
	(3.19)	(1.39)	(3.09)	(-3.70)	(-0.95)	(-4.07)
Non-labour Household Income	-0.000098*	-0.000032	-0.00012*	-0.00013	0.000084	-0.00022**
	(-2.20)	(-0.56)	(-1.98)	(-1.91)	(0.72)	(-2.67)
Metropolitan Region	0.0083	0.013*	0.0045	-0.0015	-0.0077	0.0030
	(1.89)	(2.01)	(0.75)	(-0.33)	(-1.19)	(0.48)
Constant	0.65***	0.63***	0.66***	0.97***	1.02***	0.98***
	(50.7)	(33.7)	(38.1)	(90.2)	(68.6)	(67.5)
N	26523	12052	14471	26527	12054	14473
Adjusted R2	0.07	0.07	0.07	0.18	0.16	0.18

Table 17 – Difference-in-Differences Estimates for the Impact of the Law of 2000 on Work Incidence
Controlling for Pre-Treatment Differences in Trends
Eligible Group = 15; Control Group = 16
Bandwidth of 6 Months

	Labour Force Participation			Formal Paid Work			Informal Work			Domestic Work		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Eligible*Time (DD)	0.035	0.043	0.014	0.12***	0.14***	0.10***	0.16***	0.17***	0.16***	0.089***	0.089**	0.084**
	(1.46)	(1.30)	(0.41)	(10.9)	(8.74)	(7.06)	(9.43)	(6.77)	(6.99)	(4.17)	(2.82)	(3.08)
Eligible (15=1, 16=0)	-0.16***	-0.18***	-0.11**	-0.12***	-0.12***	-0.13***	-0.027*	-0.014	-0.036*	0.0066	0.017	-0.0034
	(-6.32)	(-5.68)	(-2.82)	(-8.63)	(-5.87)	(-6.28)	(-2.06)	(-0.70)	(-2.25)	(0.50)	(0.74)	(-0.25)
Time (2002=1, 1999=0)	-0.12***	-0.13***	-0.10***	0.53***	0.45***	0.63***	0.64***	0.58***	0.71***	-0.13***	-0.14***	-0.11***
	(-6.28)	(-4.94)	(-3.70)	(66.6)	(40.9)	(58.4)	(45.4)	(28.6)	(36.6)	(-8.91)	(-6.53)	(-5.76)
Male	0.037**	*Na*	*Na*	0.11***	*Na*	*Na*	0.061***	*Na*	*Na*	-0.39***	*Na*	*Na*
	(2.86)	*Na*	*Na*	(13.3)	*Na*	*Na*	(7.88)	*Na*	*Na*	(-48.6)	*Na*	*Na*
White	0.076***	0.080***	0.069***	-0.00089	0.0078	-0.0092	-0.016*	-0.012	-0.019	-0.0067	0.0093	-0.023**
	(5.68)	(4.33)	(3.56)	(-0.11)	(0.66)	(-0.79)	(-1.97)	(-0.92)	(-1.87)	(-0.83)	(0.67)	(-2.67)
Mother's years of schooling	0.0052*	0.0068*	0.0037	-0.013***	-0.015***	-0.012***	-0.0086***	-0.011***	-0.0067***	-0.0026	-0.00081	-0.0045**
	(2.22)	(2.17)	(1.04)	(-8.98)	(-7.23)	(-5.61)	(-5.88)	(-4.60)	(-3.75)	(-1.72)	(-0.32)	(-2.82)
Father's years of schooling	0.0052*	0.0066*	0.0037	-0.0054***	-0.0082***	-0.0029	-0.0049***	-0.0084***	-0.0019	-0.0049***	-0.0036	-0.0062***
	(2.18)	(2.03)	(1.08)	(-3.68)	(-3.94)	(-1.37)	(-3.43)	(-3.64)	(-1.10)	(-3.30)	(-1.43)	(-3.94)
# of Siblings 0-5	-0.016**	-0.0090	-0.029***	0.0051	0.0081	0.0018	0.0087*	0.011	0.0065	0.011**	0.014*	0.0081*
	(-3.06)	(-1.27)	(-3.35)	(1.35)	(1.51)	(0.35)	(2.40)	(1.80)	(1.51)	(3.21)	(2.40)	(2.17)
# of Siblings 6-11	-0.0043	0.0020	-0.015	0.0052	0.0015	0.0083	0.0020	-0.0015	0.0035	0.011**	0.018**	0.0042
	(-0.72)	(0.23)	(-1.84)	(1.30)	(0.27)	(1.47)	(0.53)	(-0.25)	(0.73)	(2.86)	(2.88)	(1.03)
# of Siblings 12-13	-0.019*	-0.0097	-0.036**	0.0092	0.0074	0.010	0.013*	0.013	0.012	0.0066	0.010	0.0024
	(-2.12)	(-0.78)	(-2.86)	(1.58)	(0.90)	(1.23)	(2.36)	(1.51)	(1.76)	(1.18)	(1.08)	(0.41)
# of Siblings 14-15	-0.015	-0.019	-0.0098	0.0096	0.021*	-0.00054	0.015**	0.026**	0.0072	-0.0054	-0.0052	-0.0059
	(-1.58)	(-1.47)	(-0.71)	(1.61)	(2.50)	(-0.063)	(2.72)	(2.95)	(1.02)	(-0.94)	(-0.55)	(-0.99)

Cont.

# of Siblings 16-17	-0.0060	-0.0050	-0.012	0.0070	0.0027	0.012	-0.0053	-0.0071	-0.0025	-0.0089	-0.012	-0.0063
	(-0.64)	(-0.40)	(-0.83)	(1.13)	(0.31)	(1.34)	(-0.93)	(-0.82)	(-0.34)	(-1.50)	(-1.22)	(-0.98)
# of Siblings >=18	-0.0018	-0.0081	0.0061	0.0047	0.0041	0.0058	0.0024	0.0025	0.0024	-0.00043	-0.0038	0.0032
	(-0.40)	(-1.31)	(0.94)	(1.88)	(1.14)	(1.65)	(1.04)	(0.71)	(0.83)	(-0.18)	(-0.92)	(1.24)
Land Title	-0.0045	-0.0082	0.0023	0.012	0.0033	0.021	0.011	0.0026	0.019	0.017	0.038	-0.0023
	(-0.17)	(-0.22)	(0.062)	(0.75)	(0.14)	(0.92)	(0.72)	(0.11)	(0.98)	(1.10)	(1.43)	(-0.14)
Non-labour Household Income	0.00075*	0.0010***	-0.000030	-0.000086	0.000087	-0.00014	-0.00018*	-0.00035	-0.000077	0.000046	0.00024	-0.000049
	(2.52)	(3.36)	(-0.041)	(-0.72)	(0.37)	(-1.04)	(-2.36)	(-1.93)	(-1.09)	(0.35)	(0.86)	(-0.34)
Metropolitan Region	0.037*	0.026	0.048*	-0.069***	-0.089***	-0.051***	-0.072***	-0.088***	-0.057***	0.023**	0.051***	-0.0050
	(2.40)	(1.28)	(2.14)	(-8.22)	(-7.36)	(-4.29)	(-9.14)	(-7.16)	(-5.75)	(2.75)	(3.65)	(-0.55)
Constant	0.16***	0.19***	0.17***	0.54***	0.71***	0.48***	0.27***	0.39***	0.21***	0.90***	0.45***	0.98***
	(4.75)	(4.11)	(3.61)	(25.3)	(24.2)	(16.2)	(13.1)	(12.4)	(8.35)	(45.9)	(13.4)	(47.5)
N	2900	1674	1226	11311	5700	5611	8474	4048	4426	11305	5697	5608
Adjusted R2	0.09	0.09	0.09	0.28	0.27	0.27	0.40	0.37	0.43	0.19	0.01	0.03

Table 18 – Difference-in-Differences Estimates for the Impact of the Law of 2000 on Work Intensity

Controlling for Pre-Treatment Differences in Trends
Eligible Group = 15; Control Group = 16
Bandwidth of 6 Months

	Weekly Hours Worked - Formal			Weekly Hours Worked - Informal		
	All	Boys	Girls	All	Boys	Girls
Eligible*Time (DD)	1.71	2.14	0.60	-2.81*	-2.93	-2.28
	(1.71)	(1.55)	(0.41)	(-2.16)	(-1.74)	(-1.11)
Eligible (15=1, 16=0)	-6.29***	-7.56***	-3.96*	4.25***	4.39**	3.73
	(-6.14)	(-5.67)	(-2.49)	(3.33)	(2.76)	(1.75)
Time (2002=1, 1999=0)	-4.79***	-5.10***	-4.09***	3.68***	3.81**	3.34*
	(-6.08)	(-4.69)	(-3.62)	(4.12)	(3.24)	(2.43)
Male	1.56**	*Na*	*Na*	-1.21	*Na*	*Na*
	(2.93)	*Na*	*Na*	(-1.83)	*Na*	*Na*
White	3.21***	3.57***	2.67***	-3.33***	-3.26***	-3.27**
	(5.72)	(4.61)	(3.32)	(-4.88)	(-3.66)	(-3.09)
Mother's years of schooling	0.15	0.19	0.12	-0.41***	-0.59***	-0.18
	(1.57)	(1.49)	(0.82)	(-3.39)	(-3.73)	(-0.98)
Father's years of schooling	0.21*	0.26	0.16	-0.33**	-0.40*	-0.27
	(2.12)	(1.93)	(1.07)	(-2.72)	(-2.50)	(-1.44)
# of Siblings 0-5	-0.60**	-0.24	-1.19***	1.02***	0.65	1.64***
	(-2.66)	(-0.80)	(-3.32)	(3.46)	(1.72)	(3.37)
# of Siblings 6-11	-0.25	-0.052	-0.62	0.26	-0.18	0.90
	(-1.06)	(-0.16)	(-1.96)	(0.78)	(-0.41)	(1.74)
# of Siblings 12-13	-0.94**	-0.64	-1.48**	0.84	0.75	1.06
	(-2.61)	(-1.29)	(-2.97)	(1.74)	(1.19)	(1.44)
# of Siblings 14-15	-0.51	-0.60	-0.38	0.78	0.81	0.84
	(-1.21)	(-1.02)	(-0.67)	(1.60)	(1.29)	(1.08)
# of Siblings 16-17	-0.29	-0.17	-0.68	0.47	0.38	0.88
	(-0.78)	(-0.33)	(-1.25)	(0.96)	(0.60)	(1.12)
# of Siblings >=18	-0.018	-0.35	0.40	0.33	0.58	-0.022
	(-0.100)	(-1.40)	(1.48)	(1.48)	(1.92)	(-0.065)
Land Title	-0.57	-0.85	-0.15	0.21	0.79	-0.91
	(-0.50)	(-0.54)	(-0.097)	(0.15)	(0.44)	(-0.43)
Non-labour Household Income	0.017	0.025	-0.0066	-0.033***	-0.047***	0.011
	(1.32)	(1.55)	(-0.23)	(-3.33)	(-3.35)	(0.40)
Metropolitan Region	1.00	0.49	1.57	-2.82***	-2.71**	-2.94*
	(1.61)	(0.57)	(1.74)	(-3.79)	(-2.79)	(-2.55)
Constant	7.19***	8.94***	6.74***	32.3***	32.4***	30.9***
	(5.02)	(4.55)	(3.49)	(18.1)	(14.2)	(11.4)
N	2900	1674	1226	2740	1606	1134
Adjusted R2	0.08	0.08	0.08	0.07	0.08	0.06

Table 19 – Difference-in-Differences Estimates for the Impact of the Law of 2000 on School Outcomes

Controlling for Pre-Treatment Differences in Trends
Eligible Group = 15; Control Group = 16
Bandwidth of 6 Months

	School Attendance			Schooling Delay		
	All	Boys	Girls	All	Boys	Girls
Eligible*Time (DD)	-0.016	-0.0098	-0.030	0.018	0.051	-0.016
	(-0.89)	(-0.40)	(-1.06)	(0.86)	(1.76)	(-0.49)
Eligible (15=1, 16=0)	0.046***	0.049**	0.045**	-0.071***	-0.080***	-0.065**
	(4.33)	(3.21)	(3.00)	(-4.84)	(-3.88)	(-3.14)
Time (2002=1, 1999=0)	-0.052***	-0.054**	-0.047*	-0.18***	-0.18***	-0.19***
	(-4.21)	(-3.26)	(-2.50)	(-12.7)	(-9.20)	(-9.21)
Male	-0.0069	*Na*	*Na*	0.099***	*Na*	*Na*
	(-1.08)	*Na*	*Na*	(11.7)	*Na*	*Na*
White	0.0078	0.0074	0.0077	-0.16***	-0.17***	-0.15***
	(1.16)	(0.77)	(0.83)	(-17.7)	(-13.3)	(-11.7)
Mother's years of schooling	0.0086***	0.0080***	0.0090***	-0.020***	-0.022***	-0.017***
	(7.50)	(4.76)	(5.83)	(-12.1)	(-9.68)	(-7.41)
Father's years of schooling	0.0068***	0.010***	0.0034*	-0.018***	-0.019***	-0.017***
	(5.98)	(6.15)	(2.22)	(-11.2)	(-8.16)	(-7.54)
# of Siblings 0-5	-0.035***	-0.023***	-0.048***	0.032***	0.021***	0.044***
	(-10.4)	(-4.83)	(-9.88)	(8.20)	(3.70)	(8.18)
# of Siblings 6-11	-0.0031	-0.0031	-0.0027	0.029***	0.023***	0.035***
	(-0.96)	(-0.68)	(-0.60)	(7.00)	(3.95)	(5.98)
# of Siblings 12-13	0.0072	-0.0014	0.015*	0.010	0.0038	0.017*
	(1.56)	(-0.21)	(2.37)	(1.74)	(0.45)	(2.02)
# of Siblings 14-15	0.0030	-0.0030	0.0092	0.0079	0.0094	0.0062
	(0.65)	(-0.47)	(1.39)	(1.27)	(1.11)	(0.68)
# of Siblings 16-17	0.0043	-0.00085	0.0083	0.030***	0.023**	0.038***
	(0.92)	(-0.12)	(1.31)	(4.64)	(2.59)	(4.16)
# of Siblings >=18	-0.0051**	-0.0062*	-0.0042	0.018***	0.020***	0.016***
	(-2.61)	(-2.18)	(-1.54)	(6.83)	(5.28)	(4.23)
Land Title	0.029*	0.043*	0.015	-0.078***	-0.048*	-0.11***
	(2.02)	(2.05)	(0.75)	(-4.70)	(-2.05)	(-4.63)
Non-labour Household Income	-0.00012	0.0000082	-0.00018	0.000033	0.00015	-0.000026
	(-1.59)	(0.086)	(-1.72)	(0.27)	(0.60)	(-0.19)
Metropolitan Region	0.014*	0.011	0.018	-0.027**	-0.023	-0.032*
	(2.19)	(1.22)	(1.94)	(-3.01)	(-1.83)	(-2.50)
Constant	0.74***	0.69***	0.78***	0.87***	0.99***	0.85***
	(40.3)	(26.4)	(31.2)	(40.3)	(34.2)	(27.8)
N	11310	5699	5611	11311	5700	5611
Adjusted R2	0.06	0.06	0.06	0.20	0.20	0.19

Table 20 – Gender Gap Analysis: Parametric Estimates for the Impact of the Laws of 1998 and 2000 on Work Outcomes

	Labour Force Participation			Formal Paid Activities			Informal Work			Domestic Work			Weekly Hours Worked - Formal			Weekly Hours Worked - Informal		
	1997	1999	2002	1997	1999	2002	1997	1999	2002	1997	1999	2002	1997	1999	2002	1997	1999	2002
										14 vs. 13								
Eligible*Boys	0.023	0.011	0.0061	0.092*	-0.006	-0.022	0.016	0.006	0.0068	0.023	-0.042*	-0.041	4.59**	-1.79	-1.09	1.94	1.3	-5.94
	(0.02)	(0.02)	(0.017)	(0.05)	(0.06)	(0.035)	(0.01)	(0.06)	(0.017)	(0.03)	(0.03)	(0.026)	(1.93)	(2.39)	(1.27)	(4.22)	(5.20)	(6.41)
Boys	0.032**	0.046***	0.041***	0.00038	0.0019	-0.00068	-0.00068	-0.0019	0.041***	-0.35***	-0.28***	-0.28***	0.042	0.058	-0.029	-9.38***	-1.84	4.00
	(0.01)	(0.01)	(0.011)	(0.00)	(0.00)	(0.0015)	(0.01)	(0.00)	(0.011)	(0.02)	(0.02)	(0.019)	(0.08)	(0.08)	(0.051)	(3.02)	(3.73)	(5.24)
Eligible	-0.038*	-0.043**	-0.0084	0.067	-0.052	0.055	-0.0077	0.052	-0.010	-0.041	0.039	-0.029	1.37	-0.65	2.48	1.81	7.28	-1.35
	(0.02)	(0.02)	(0.018)	(0.06)	(0.05)	(0.060)	(0.01)	(0.05)	(0.018)	(0.03)	(0.03)	(0.028)	(1.68)	(1.61)	(2.31)	(5.11)	(5.86)	(8.50)
Z	0.088***	0.074***	0.048***	0.0092	0.13***	-0.014	0.027***	-0.13***	0.048***	0.048**	-0.016	0.074***	0.74	4.39***	-0.84	5.06	-4.92	7.77
	(0.02)	(0.02)	(0.015)	(0.04)	(0.05)	(0.034)	(0.01)	(0.05)	(0.015)	(0.02)	(0.02)	(0.023)	(1.51)	(1.66)	(1.21)	(3.30)	(4.55)	(5.52)
Z2	0.035**	0.0057	0.026*	-0.0002	0.15**	0.0012	0.012	-0.15**	0.026*	-0.011	-0.040*	-0.024	-0.31	4.57**	-0.37	-2.66	1.96	-8.80*
	(0.02)	(0.02)	(0.014)	(0.05)	(0.06)	(0.042)	(0.01)	(0.06)	(0.014)	(0.02)	(0.02)	(0.022)	(1.76)	(2.08)	(1.36)	(3.17)	(4.66)	(5.28)
Constant	0.13***	0.13***	0.073***	0.004	0.018***	-0.0066	0.037***	0.98***	0.073***	0.86***	0.84***	0.81***	0.4	0.66**	-0.26	21.2***	25.6***	33.6***
	(0.01)	(0.01)	(0.011)	(0.01)	(0.01)	(0.0048)	(0.01)	(0.01)	(0.011)	(0.02)	(0.02)	(0.018)	(0.39)	(0.25)	(0.19)	(3.52)	(3.98)	(6.28)
Obs	5041	4945	4861	227	159	157	5041	159	4858	5030	4943	4859	227	159	157	431	159	157
										16 vs. 15								
Eligible*Boys	0.0047	0.036	0.050**	0.030	0.044	0.069	-0.00076	-0.044	0.043*	-0.015	0.0040	0.029	1.61	3.04	3.36	-0.77	2.45	1.50
	(0.026)	(0.026)	(0.025)	(0.064)	(0.066)	(0.057)	(0.017)	(0.066)	(0.025)	(0.024)	(0.024)	(0.025)	(2.66)	(2.62)	(2.46)	(2.92)	(3.59)	(3.56)
Boys	0.094***	0.087***	0.050***	0.036	0.0098	-0.0024	0.036***	-0.0098	0.047***	-0.37***	-0.35***	-0.38***	1.04	-0.85	-0.41	-5.38**	-4.88*	-4.39
	(0.017)	(0.018)	(0.016)	(0.049)	(0.045)	(0.040)	(0.011)	(0.045)	(0.016)	(0.017)	(0.017)	(0.018)	(1.98)	(1.78)	(1.83)	(2.18)	(2.63)	(2.85)
Eligible	-0.0040	-0.014	0.00077	-0.054	-0.051	-0.019	-0.0072	0.051	0.0051	0.020	0.020	-0.017	-2.64	-1.66	-1.57	1.80	-3.63	0.79
	(0.028)	(0.029)	(0.028)	(0.070)	(0.073)	(0.064)	(0.019)	(0.073)	(0.028)	(0.024)	(0.024)	(0.026)	(3.03)	(2.84)	(2.70)	(3.36)	(4.17)	(3.85)
Z	0.13***	0.12***	0.12***	0.076	0.21***	0.072	0.046***	-0.21***	0.11***	-0.026	-0.023	0.0060	3.68	7.08***	2.79	-0.51	-2.01	-3.72
	(0.022)	(0.023)	(0.022)	(0.054)	(0.055)	(0.050)	(0.015)	(0.055)	(0.022)	(0.021)	(0.021)	(0.022)	(2.30)	(2.16)	(2.11)	(2.56)	(3.00)	(2.97)
Z2	0.0030	0.012	0.011	-0.067	0.044	0.11**	0.016	-0.044	-0.00039	-0.027	0.022	0.014	-2.34	2.52	4.23**	3.55	-4.44	-3.08
	(0.022)	(0.022)	(0.021)	(0.052)	(0.055)	(0.051)	(0.015)	(0.055)	(0.021)	(0.020)	(0.020)	(0.021)	(2.23)	(2.16)	(2.11)	(2.51)	(2.90)	(2.90)
Constant	0.30***	0.28***	0.24***	0.30***	0.19***	0.083*	0.093***	0.81***	0.23***	0.85***	0.86***	0.85***	12.1***	7.26***	3.77**	23.1***	35.5***	32.5***
	(0.018)	(0.018)	(0.017)	(0.048)	(0.045)	(0.043)	(0.012)	(0.045)	(0.017)	(0.015)	(0.015)	(0.017)	(2.05)	(1.78)	(1.86)	(2.26)	(2.84)	(2.75)
Obs	5287	5007	4969	817	581	534	5287	581	4891	5280	5003	4968	817	581	534	1031	581	534

Note: Robust standard errors in parentheses. *, **,*** Statistically significant at 10%, 5% and 1% respectively.

Table 21 – Gender Gap Analysis: Parametric Estimates for the Impact of the Laws of 1998 and 2000 on School Outcomes

	School Attendance			Schooling Delay		
	1997	1999	2002	1997	1999	2002
			14 vs. 13			
Eligible*Boys	0.012	-0.00027	0.00065	-0.0003	0.0095	0.0011
	(0.01)	(0.01)	(0.0100)	(0.03)	(0.03)	(0.028)
Boys	-0.0039	0.0048	-0.0043	0.092***	0.067***	0.10***
	(0.01)	(0.01)	(0.0063)	(0.02)	(0.02)	(0.020)
Eligible	0.016	-0.0056	-0.017	0.26***	0.22***	0.32***
	(0.01)	(0.01)	(0.012)	(0.03)	(0.03)	(0.031)
Z	-0.052***	-0.018*	0.0028	-0.24***	-0.20***	-0.29***
	(0.01)	(0.01)	(0.0087)	(0.02)	(0.02)	(0.025)
Z2	-0.024**	-0.00071	0.010	0.054***	-0.02	0.0047
	(0.01)	(0.01)	(0.0081)	(0.02)	(0.02)	(0.024)
Constant	0.93***	0.95***	0.98***	0.50***	0.51***	0.35***
	(0.01)	(0.01)	(0.0067)	(0.02)	(0.02)	(0.020)
Obs	5039	4944	4861	5041	4945	4861
			16 vs. 15			
Eligible*Boys	-0.0033	-0.047***	0.017	0.018	0.027	-0.026
	(0.020)	(0.018)	(0.017)	(0.023)	(0.025)	(0.027)
Boys	-0.0041	0.033***	0.0055	0.080***	0.046**	0.094***
	(0.013)	(0.012)	(0.011)	(0.016)	(0.018)	(0.019)
Eligible	0.034	0.017	0.0017	0.18***	0.18***	0.18***
	(0.022)	(0.021)	(0.020)	(0.025)	(0.029)	(0.031)
Z	-0.082***	-0.049***	-0.057***	-0.17***	-0.16***	-0.13***
	(0.018)	(0.016)	(0.015)	(0.020)	(0.022)	(0.024)
Z2	-0.0017	0.019	0.0077	0.017	-0.017	-0.0084
	(0.017)	(0.015)	(0.014)	(0.019)	(0.022)	(0.023)
Constant	0.82***	0.86***	0.88***	0.63***	0.61***	0.52***
	(0.014)	(0.013)	(0.012)	(0.017)	(0.019)	(0.020)
Obs	5283	5007	4968	5287	5007	4969

Note: Robust standard errors in parentheses. *, **,*** Statistically significant at 10%, 5% and 1% respectively.