Three Essays on the Causal Impacts of Child Labour Laws in Brazil

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Declaration

I hereby declare that this thesis has not been and will not be, submitted in whole or in part to another university for the award of any other degree.

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SUMMARY

This thesis focuses on different impacts of an important change in Brazil's child labour legislation. In December 1998, Brazil raised the minimum employment age from 14 to 16 banning from the labour force children who turned 14 just after the law passed. Two year later, in December 2000, Brazil institutionalised an apprenticeship programme aimed at children aged 14 to 17. In chapter one of this thesis I investigate the short run effects of both laws on children's time allocation using a regression discontinuity design technique. I look at the impact of both laws on schooling and labour market outcomes for two cohorts: children just under age 14 and teenagers just under age 16.

The second chapter turns attention to the long-term effects of the 1998 ban, comparing the labour market and schooling outcomes of the cohorts who turned 14 before and after the law came into effect. The analysis is conducted for white and non-white males to check how the ban affected individuals from different socioeconomic backgrounds. This is the first study that looks at the long-term effects of a child labour ban.

The third chapter investigates whether the ban had spillover effects on time allocation of younger siblings and parents. This is chapter covers a broad set of outcomes, exploring family composition and potential liquidity constraints to shed light on potential underlying mechanisms.

This thesis contributes to the understanding of the consequences of child labour legislation, looking at immediate impacts on children themselves, long-term effects, and spillover effects on other household members. Its main results show that such legislation may have unintended consequences, long-lasting effects, and affect time allocation of other household members in ways policy makers might not be able to foresee.

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CONCLUSION

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ATE – Average Treatment Effect
ATT – Average Treatment Effect on the Treated
CCT – Conditional Cash Transfer
DD – Difference-in-differences
CSL – Compulsory Schooling Law
IBGE – Instituto Brasileiro de Geografia e Estatística
ILO – International Labour Organisation
ITT – Intent to treat
IV – Instrumental Variable
LATE – Local Average Treatment Effect
LATT – Local Average Treatment Effect on the Treated
LFPR – Labour Force Participation Rate
MLA – Minimum Legal Age (of entry into the labour force)
MW – Minimum Wage
OLS – Ordinary Least Squares
PNAD – Pesquisa Nacional por Amostra de Domicílios
QTE – Quantile Treatment Effect
R$ - Brazilian Currency Real
RAIS – Relação Anual de Informações Sociais
RDD – Regression Discontinuity Design
2SLS – Two Stage Least Squares
UK – United Kingdom
US – United States
WB – World Bank

INTRODUCTION

Should child labour be banned? From a normative perspective, there is likely wide agreement that child labour is bad and should be condemned. Child labour is often associated with an image of very young children engaged in different forms of hazardous work, including unpaid household services.[1] In fact, the reality is far more complex. The definition of 'child labour' depends on who is considered a child – the age range – and the types of work/activities in which a 'child' is involved. Thus, before fighting against child labour one should question *who* is actually working and on *what*. Child labour could also be criticized on empirical grounds, as engagement in the world work early in life might harm children's human capital – e.g., missed school days and low academic achievement.

The International Labour Organization (ILO) considers children to be between the ages of 5 and 17 and use two criteria to define child labour: minimum employment age and form of labour. The two criteria are not mutually exclusive. If a child below the minimum employment age is working, she is immediately considered to be in child labour unless she is participating in an apprenticeship programme. In this case, an arbitrary age threshold determines whether work performed by two children who are close in age is child labour. But the ILO's definition also encompasses the type of work performed by children. Children older than the minimum employment age but younger than 18 found performing "the worst forms of work" will be included in child labour statistics as well.[2]

ILO (2013) estimates for 2012 show that 264 million children were in employment, 168 million in child labour, and 85 million in hazardous work,[3] with most employed in the agricultural and service sectors (58.6 percent and 32.3 percent respectively). The incidence of child labour was 9.9 percent (120.5 million in absolute terms) among 5 to 14 year olds and 13.1 percent (47.4 million) among those aged 12 to

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[1] This category of child labour includes long hours of work in an unhealthy and unsafe environment.

[2] ILO (2013) lists the following as the worst forms of child labour: child prostitution; compulsory labour; compulsory recruitment in armed conflict; work underground, in confined spaces, with dangerous machinery, carrying heavy loads; and so on. Note that household chores and sporadic work are excluded from this definition.

[3] ILO uses 'Children in Employment' as a broader definition that encompasses illegal (child labour) and legal work activities carried out by 5- to 17-year-old children. Work is considered legal if it is in accordance with the ILO Conventions No. 138 and 182. Convention No. 138 sets the definition of minimum employment ages, whereas the Convention No. 182 prohibits the worst (most hazardous) forms of child labour. See ILO (2013).

14, whereas the incidence of hazardous work was 3.1 percent and 5.3 percent for these two age groups respectively.

Although the incidence of child labour increases with age, it could be argued that a higher incidence among 12- to 14-year-olds might be less worrying than a lower incidence among those aged 5 to 11,[4] in particular in cases in which children are approaching the minimum employment age and they are not performing any type of condemned work. There is increasing evidence in early childhood development literature showing that the earlier vulnerable children are exposed to interventions aimed at remedying deficiencies in cognitive and non-cognitive skills, the higher the return per dollar invested in terms of adult outcomes.[5] Because early exposure to child labour may come at the expense of human capital accumulation through formal education as well as health outcomes, this literature suggests that policies aimed at reducing incidences of child labour should perhaps pay closer attention to the youngest groups—those more likely to being harmed in the long run from early exposure to work activities.

For children approaching the minimum legal working age, it might be more difficult to know upfront whether they should accumulate human capital through formal education or labour market experience, especially in contexts in which formal education is unlikely to add much to children's productivity.[6] Identifying (i) the forms of work that can actually contribute to children's human capital accumulation and (ii) the appropriate age at which they can be exposed to the labour market environment is particularly relevant for developing countries that still have a high number of children working illegally and with public schools of questionable quality.[7] The consequences of policies developed to help children accumulate human capital through training/employment are still to be better understood, but the evidence available suggests their effects can be broad and persistent (see Edmonds, 2008).

[4] Evidence for Brazil, for instance, shows that the earlier an individual is exposed to work the higher the probability of their offspring entering the labour market early (Emerson and Souza, 2003), the higher the probability of developing health problems in adult life (Kassouf et al. 2001), the higher the probability of performing poorly in school (Bezerra et al. 2009), and the lower the wages earned in adult life (Emerson and Souza, 2011).

[5] Interventions include the provision of daycare centres. This literature is evolving quickly, and there is evidence covering an array of outcomes such as employability and wages, probability of committing crime and being incarcerated, and probability of pregnancy during adolescence. See Heckman (2008).

[6] Although formal education can affect labour market outcomes through signaling effect, it is unlikely that employers are unaware of the average quality of public education children receive. In this case, the signaling effect of education might be dominated by the productivity effect.

[7] A new wave of studies looking at long-term effects of conditional cash transfer programmes has found small or nil impact on labour market outcomes.

This debate is very relevant in face of the high proportion of young people in the labour force worldwide. Concerns over youth unemployment have increased around the world in the aftermath of the financial crises of 2008 (see WB 2013 and ILO 2014). Studies show that youth unemployment may have permanent negative consequences for individuals' lives through lower accumulation of work experience. Individuals who do not accumulate experience during their youth may face difficulty finding a job in the long run and consequently see lower returns to an additional year of work experience. In fact, the perception that postponing entrance into the labour force might have long-term negative consequences for their labour market prospects may be one of the drivers in parents' decisions to send their children to work relatively young. To help youth transition from school to work, many countries have created apprenticeship programmes[8]. These vocational training programmes work as an entry point to the labour market, which in some countries the youth are highly encouraged to take-up.[9]

Countries that have ratified ILO Convention No. 138 and set minimum ages for admission to employment have had to deal with a dilemma: identifying a minimum employment age that discourages child labour without harming opportunities for disadvantaged youth to accumulate human capital through labour market experience. It is important to mention that in ratifying the ILO Convention No. 138 countries agree on setting the minimum employment age above the school leaving age to reduce children's incentive to abandon school before graduating. The success of this endeavour is ultimately an empirical matter that this thesis explores. Theoretical models suggest that bans on child labour such as those based on specifying minimum employment ages may not deliver as expected (Basu and Van 1998; Basu 2005). Perhaps one of the assumptions of advocates of a minimum legal age is that parents can actually afford children's leisure activities and decide not to make such investments. Parents' decisions to send their children to work would therefore be due to some misperception regarding the returns to schooling (underestimated) and the returns to labour market experience (overestimated). Thus, with a ban in place, it is hoped that parents would reallocate children's time towards school and leisure. On theoretical grounds, Basu (2005) has shown that ban policies can actually harm poor households by limiting the sources of

[8] Apprenticeship programmes are usually seen as a form of vocational training for youth.
[9] See Brodaty et al. (1999) and Caliendro et al. (2011).

household income.[10] Since the ban will lead to a reallocation of children's time in the short run, it may affect their schooling – such as school attendance and learning – and short-term and long-term labour market outcomes – such as participation in the informal sector, occupation, and wage prospects. These effects on children's human capital have to be assessed empirically.

A few studies have investigated the impact of minimum legal age laws on the incidence of child labour in the US (Moheling 1999; Fargo and Finegan 1996; Lleras-Muney 2002; Golding and Katz 2008). These studies look at the effect of child labour laws on participation rate, and the evidence suggests that the impact of this type of intervention reduces child labour but not much.[11]

Much less is known about the impact of these policies in developing countries. The available evidence casts doubts on its efficacy in reducing child labour (see Edmonds 2008; Edmonds and Shrestha 2013; and Braradwaj et al. 2013). Questionable efficacy is not unexpected, as the large size of the informal sector and the number of children still engaged in paid work activities in developing countries demonstrates the imperfect enforceability of the rule of law in such contexts. This leads some to conclude that bans may have limited, if any, impact on reducing child labour (Kassouf 2001). Perhaps the impact of ban policies, particularly in places where the law is not fully enforced, should be tested in urban areas given the lower monitoring costs incurred by local authorities and the higher concentration of children engaged in paid work activities.[12]

This thesis focuses on the case of Brazil and uses regression discontinuity design to shed light on the impact of two recent law changes: a ban policy on child labour and an apprenticeship programme.

Context

[10] With imperfect enforcement, ban policies could harm children's human development, as they end up working illegally and for longer hours.

[11] This is particularly evident when the ban is broadly defined as a combination of minimum admission age and compulsory schooling law, as in Margo and Finegan (2006). It is interesting to note that these studies looked at participation rate in the labour force as whole, that is, without distinguishing between participation in the formal and informal sectors.

[12] Child labour that is due to unpaid family farm work might be much more difficult to reduce with this type of intervention.

In December 1998, Brazil raised the minimum employment age from 14 to 16. Interventions increasing the minimum employment age are usually considered a ban policy on child labour (Edmonds and Shrestha, 2013).

The change was made at a critical moment for the Brazilian economy. In 1998 the Brazilian GDP grew in real terms by less than a half percentage point. The unemployment rate reached about 10 percent and the informality rate approached 60 percent.[13] The statistics for younger age groups are not very different: for those aged 16 to 24, the participation rate approached 48.2 percent with participation in the formal sector accounting for 55 percent of the total employed. Among children aged 10 to 14 in urban areas, the participation rate was much lower and predominantly informal (6.9 and 6.8 percentage points respectively).

The increase in the minimum legal age in such a context may have aggravated the struggle of many poor households that used to rely on work performed by their 14 year-old children. Using the instruction level of the household head as a proxy for the socioeconomic background of households, one can see that children aged 10 to 14 with the head holding fewer than 8 years of education (incomplete primary education) are almost twice as likely to work than children with a household head with 8 or more years of education (3.2 percent against 1.7 percent). In that case, the change in the law would have contributed to increasing children's human capital accumulation in households that could afford to move forward without relying on child labour, but it would exacerbate earnings inequality between these households and those that have to send their children to work informally.

It is difficult to determine the extent to which this ban contributed to the observed decrease in child labour in Brazil. Official statistics show that child labour has steadily declined in Brazil in the last couple of decades, even before the ban of 1998 (Ferro and Kassouf 2005). In the last 12 years or so, the decrease has been accentuated. According to the Brazilian Bureau of Statistics, the number of boys and girls aged 10 to 14 in child labour in urban areas more than halved between 2001 and 2013. In 2001, 1.3 million boys and 655,000 girls were working, whereas in 2013 those numbers dropped to 537,000 and 242,000 respectively. The school attendance rate, on the other hand,

[13] See ipeadata.gov.br for a compilation of official statistics in Brazil.

increased over the years and now approaches 100 percent for the same age group.[14] The numbers suggest a reallocation of children's time towards education.

The decrease in child labour rates has been attributed to many causes, such as better employment opportunities for parents (Neri et al. 2000; Duryea et al. 2001), social protection policies such as conditional cash transfer programmes (Cardoso and Souza, 2004), and demographic changes in the Brazilian population that imply a lower number of children per household (Lam and Duryea 1999).

Another intervention that might have contributed to reducing child labour in Brazil but that has not received much attention is the apprenticeship programme introduced in December 2000. Two years after increasing the minimum legal working age to 16, Brazil put in place an apprenticeship programme targeted to children between the ages of 14 and 17. Where the programme attracted children working in the informal sector it helped to reduce the incidence of child labour.

This programme is similar to successful European experiences, such as the German, French, and British apprenticeship programmes, but it attracted very few participants in the beginning.[15] Evaluations of similar vocational training programmes for youth show positive short- and long-term effects on labour market outcomes, with more educated youths having higher returns, though the evidence for developing countries remains scarce and mixed.[16]

The 1998 and 2000 laws in Brazil permit an assessment of the impact of child labour legislation in several outcomes of interest using a regression discontinuity design.

In chapter one I investigate the short-run effects of both laws on children's time allocation. I look at the impact of both laws on schooling and labour market outcomes of boys and girls. Estimates are provided for two groups of children, those around age 14 at the time the ban of 1998 passed, and those around age 16 – the new cutoff – after the ban. To check whether the apprenticeship programme counterbalanced the effect of the ban, difference-in-differences estimates are provided comparing cohorts of 14 and 16 year-olds before 1998 and after 2000.

[14] Primary school is basically universalised in Brazil. The challenge now is to reduce high school dropout rates. The data is available at www.ibge.org.br.

[15] In 2002, for instance, only 582 14-year-olds were participating in the programme. See Courseuil et al. (2011).

[16] See Clark and Fahr (2001) and Caliendro et al. (2011) for the case of Germany; Blundell et al. (2004) and De Giorgi (2005) for UK; Attanasio et al. (2015) for Colombia; Card et al. (2011) for Dominican Republic; Courseuil et al. (2012) for Brazil; and Cho et al. (2013) for Malawi.

The second chapter turns to the long-term effects of the 1998 ban, comparing the outcomes of the cohorts who turned 14 before (comparison group) and after (treatment group) the law was passed. This is the first empirical analysis for the long-term causal effects of a child labour ban.[17] The analysis is conducted for white and non-white males to check how the ban affected individuals from different socioeconomic backgrounds. Theoretical models predict that ban policies will have a welfare enhancing effect on not-so-poor households, and a welfare reducing effect on poor households.

The third chapter investigates whether the 1998 law had spillover effects on the time allocation of younger siblings and parents of children affected by the ban. As far as I am aware, Manacorda (2006) is the only paper to look at the spillover effects of a child labour ban on other household members. Most papers that consider the links between child labour and time allocation of other household members examine the impact of parents' inputs on their children's outcomes.[18] This thesis goes beyond Manacorda (2006), as it covers a broader set of outcomes and explores family composition and liquidity constraints to outline the potential mechanisms of underlying results.

Potential Caveats for the Interpretation of the Results as Causal Effects

It will be claimed throughout the thesis that the point estimates capture an exclusive effect of the child labour laws on the outcomes of interest. A set of validity checks is carried out to verify the plausibility of the identification strategy used in the thesis. Robustness checks and placebo tests are also undertaken to test the validity of the results and to rule out the role of potential confounders. Although some estimates seem more stable and precisely estimated than others, the main results are robust. The estimates are then given a causal interpretation.

This section outlines potential caveats and provides an explanation of why they should not be seen as real threats to the main findings of the thesis, in particular those related to the short-term impacts of the 1998 ban.

By the time Brazil passed the two child labour laws, different programmes targeting similar age groups studied in this thesis had been put in place and could

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[17] Efforts have been made to estimate the long-term effects of child labour. See e.g., Emerson and Souza (2011), Lee and Orazem (2010), and Beegle et al. (2009). Chapter two provides a more detailed discussion of these studies.

[18] See, for instance, Duncan (1990 and 1994), Bhalotra (2004), Oreopoulos et al. (2006c), Emerson and Souza (2003 and 2007), and McCrary and Royer (2011).

therefore be seen as potential threats to the internal validity of the impact of the two laws.

For instance, the Brazilian conditional cash transfer (CCT) scheme *Bolsa Escola* (renamed *Bolsa Familia* in 2003) started in 1995 and targeted 'poor families with children aged 6 to 15 enrolled in school and attending at least 85 percent of school days' (Glewwe and Kassouf 2012). However, the programme started as a pilot in only two municipalities, Brasilia and Campinas, and by 1998 only 1 percent of Brazil's municipalities were participating. The programme was federalised in April 2001. Since this programme targeted 13-, 14-, and 15-year-olds from poor families, it can be thought of as having a common effect on children pertaining to these age groups.[19] However, the comparison of 15- and 16-year-olds presented in chapter one may be partially contaminated by this programme, as 16-year-olds were ineligible for the CCT at that time.[20]

Although the PNAD provides information on variables that account for the eligibility of a household for the CCT that could be controlled for in the regression models, the survey does not have an identifier variable for the municipalities. Thus, even though the programme covered only slightly more than 50 municipalities by 2001, one cannot exclude those municipalities from the sample. Since the main results in the thesis come from the comparison between 14-year-olds who were equally eligible for the CCT, I do not think this would be a real threat to the internal validity of the estimates.

Another potential source of concern is related to changes in the national minimum wage by the time the child labour laws were approved. Because minimum wage applies only to the formal sector, one could think of it as a potential confounder once it affects the participation rate of the control children only. In fact, according to the Brazilian Ministry of Labour and Employment, between 1995 and 1999 the Brazilian minimum wage was annually adjusted on 1 May 1 of each year according to the accumulated inflation of the previous calendar year. It is thus unexpected that the

[19] See Glewwe and Kassouf (2012) for the causal impact of *Bolsa Escola/Familia* on schooling outcome as well as for a programme description. The authors estimate the average treatment effect on school enrollment, dropout rates, and grade promotion and find that the programme increased the first by 6 percent, reduced the second by half a percentage point, and increased the third by 0.6 percentage points. The few empirical papers considering the impact of the Brazilian CCT on child labour find relatively small effects, even though they are statistically significant (see e.g., Cardoso and Souza 2004).

[20] The literature has shown that CCT affects the same outcomes considered in the empirical section. For an overview of the impact of CCT, see Fiszbein and Schady (2009).

increase in the minimum wage would affect participation rate in the labour force of children in the control group.[21]

One last source of concern is the Compulsory Schooling Law (CSL) in Brazil. Law No. 9394 of 20 December 1996, which defines the guidelines and foundation of education in Brazil (*Lei de Diretrizes e Bases da Educação*), is clear in stating that what is mandatory is the school cycle, not the age range; that is, individuals should complete at least a primary education degree regardless their age.[22] By the time the law passed, primary school was mandatory in Brazil. Children aged 7 to 14 should thus not be lagging behind. At the time of the change in the child labour laws, about 40 percent of 14-year-olds were delayed in school according to official estimates.

Brazil does not have an official date (deadline) at which children should be enrolled in school. Usually, children who turn seven in the first semester of the calendar year – in Brazil the school year coincides with the calendar year – would be allowed to enroll in school in that year.[23] Those who turn seven after 30 June would have to wait until the next school year to start school. Thus, regression discontinuity design estimates obtained with a bandwidth of six months or narrower will compare the 'treatment' and 'control' children who are supposed to have started school in the same year.[24]

Despite official statistics from 1999 showing that 13 percent of children had abandoned school before graduating primary school and that about 40 percent of 14-year-olds were delayed in school, meaning that many children would graduate primary school older than age 14, a set of robustness checks was carried out seeking to rule out the risk of bias. In all chapters a placebo test comparing children before and after December 1997, one year before the ban, was carried out. In chapter 2, the last week of June 1999 is used as a threshold to check whether my results could be confounded by the age at school entry (Bedard and Dhuey 2006; Black et al. 201). In chapter 3 a few more robustness checks are performed to rule out a potential confounding factor accruing from the CSL and sample composition.

Taking the above into account, this thesis identifies the impacts of the two child labor laws. In so doing, it contributes to the understanding of the consequences of child

[21] See www.mte.gov.br.
[22] See Article 4 paragraph 1st of the Law.
[23] There is a current debate in Brazil trying to make 30 March the official cutoff. Children who turn 6 by 30 March will be able to start schooling in that calendar year, while those who turn 6 after that cutoff will be able to enroll in school in the following year.
[24] The 'control' group turned 7 in the second semester of 1991, while the 'treatment' group turned 7 in the first semester of 1992.

labour laws in various ways. First, by looking at their immediate impacts on children, second by shedding light on the long-term effects of the ban, and third by exploring the spillover effects of the ban on time allocation of other household members. It provides a broader analysis of the impact of child labour laws aimed at reducing child labour and increasing youth employment. Its main results show that these types of laws may have unintended consequences, long-lasting effects, and affect time allocation of other household members in ways policy makers might not be able to foresee.

CHAPTER 1: CAUSAL IMPACTS OF CHILD LABOUR LAWS IN BRAZIL

1 INTRODUCTION

The literature on child labour has grown considerably over the last 15 years, and this is not only because of increasing data availability. Child labour rates have fallen over the years, but the worldwide figure is still alarming. According to the ILO (2013), in 2012 264 million children aged 5 to 17 were participating in the labour market with 168 million in child labour.[25]

Due to the negative externalities associated with children's participation in the labour force, it is argued that the public sector could intervene in the labour market by changing the incentives that lead parents to send 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' (Emerson and Souza 2003; see Edmonds 2008 for a survey).

Basu and Van (1998), for instance, argue that parents' decisions to send a child into the labour force might be seen as a rational choice for poor households facing a varied set of constraints. Based on a set of assumptions, it is shown that the labour market may have multiple stable equilibria: an equilibrium characterised by children's labour force participation and depressed adult wages and an equilibrium in which children do not participate in the labour force and adult wages are higher. Because these equilibria are Pareto efficient, the authors argue that if children are observed participating in the labour force, the government could put a ban policy in place to shift the economy from this equilibrium to one without child labour.[26]

Although many policies have been implemented to fight against child labour, too little is known about the causal impact of such interventions, particularly in developing countries where empirical evidence remains almost inexistent. This chapter

[25] ILO uses 'Children in Employment' as a broader definition that encompasses illegal (child labour) and legal work activities carried out by 5- to 17-year-old children. Work is considered legal if it is in accordance with ILO Conventions No. 138 and 182. Convention No. 138 defends the definition of minimum employment ages whereas Convention No. 182 defends the prohibition of the worst (most hazardous) forms of child labour. See ILO (2013).

[26] The two main assumptions stemming from this result are that the increase in adult wages that results from a ban must be high enough to compensate for the children's 'forgone' income and to allow parents to consume children's leisure. The two assumptions set are (i) child and adult labour inputs are perfect substitutes, and (ii) children's leisure is a normal good for parents. This theoretical framework suggests that households' net benefit from a child labour ban is ultimately an empirical question.

helps fill this gap by delving into the consequences of two recent pieces of federal legislation in Brazil aimed at children.

The available evidence of the impact of such policies comes almost exclusively from the US and investigates the effect of minimum legal age laws at the beginning of the last century. 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 focuses her analysis on the first three decades of the century, taking advantage of the fact that different states set different minimum legal ages. She exploits variations across states and time to estimate the impact of legislation on the incidence of child labour. According to her results, child labour laws affected only marginally the time trend prevalent in that period.[27]

On the other hand, Margo and Finegan (1996) conclude that in combination with compulsory schooling laws passed more or less in the same period, minimum age legislations were effective in reducing the proportion of children in the labour force. More recent evidence for the combination of these two laws during the early 20th century in the US confirms Margo and Finegan's findings (see Lleras-Muney 2002).

Tyler (2003) 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 12th 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 percent and 5.1 percent respectively.[28]

In December 1998, Brazil increased the minimum employment age from 14 to 16, and this policy change gave rise to a natural experiment, the consequences of which are investigated in this chapter.

The first attempt to understand the consequences of this law is seen in Ferro and Kassouf (2005). The authors look exclusively at the effect of the change in the law on labour force participation rates. They pool eight years of the Brazilian household survey (PNAD), from 1995 to 2003[29] to assess the effect of the law on work incidence among children aged 14 and 15 before and after the law passed. Unfortunately, their empirical

[27] Using double and triple difference estimations, Moehling (1999) compares the magnitude of treatment effect with the magnitude of time dummy and notices that the latter is larger in absolute terms. She concludes that child labour would decrease without any intervention, and that the legislation made a minor contribution to the observed trend.
[28] Evidence for Brazil shows that the impact of child labour on standardised exams in maths and reading is strong and negative (Bezerra et al. 2009).
[29] The year 2000 was excluded from their analysis, because in census years the PNAD is not surveyed.

strategy does not include a control group; therefore, their work does not indicate what would have happened for children affected by the law had the law not been enacted. Since they select only individuals at age 14 and 15, they compare different cohorts before and after the change in the law. Apart from that, Ferro and Kassouf (2005) do not distinguish among the variety work activities in which children engage, such as household chores and informal work. When one looks at the relatively large participation rate in paid work activities among children aged 10 to 14, one may conclude that the law was unsuccessful in reducing child labour.[30] However, this chapter shows that the law contributed to some reduction in child labour.

In December 2000, two years after increasing the minimum employment age to 16, an apprenticeship programme aimed at 14- to 17-year-olds was implemented in Brazil. Children who participate in the programme are registered with the Ministry of Labour and Employment, can work up to 6 hours per day, and are paid the hourly minimum wage. The programme also requires formal training (at school and on the job) and has the objective of helping children transition from school to work. This chapter questions whether the programme incentivised 14-year-old children to enter the labour market and, if so, how this affected their time allocation.

In a recent paper, Courseuil et al. (2012) use data from *Relação Anual de Informações Sociais* (RAIS), a census of firms in the formal sector in Brazil, to estimate the effect of this programme on several labour market outcomes. The authors use as their identification strategy the fact that before 2005 only youth ages 14 to 17 were eligible for the programme. They use the discontinuity at age 18 to identify the effect of the programme on employment and wage outcomes. The findings point to positive effects on the likelihood of being employed after two years of programme participation, but only marginal effects on wages. The authors conclude that the programme has had a positive medium run local average treatment effect on 17-year-olds who participate in the programme. Unfortunately, due to data restrictions, the authors are unable to shed light on the impact of the programme on participation rates in the informal labour force. In addition, since they do not use the exact birthdate in days, weeks, or months to define the forcing variable, one could argue that their identification strategy is problematic;

[30] Kassouf (2001) provides an overview of the child labour situation in Brazil in 1999, arguing along these lines.

they are comparing individuals far from the cut-off point. They did not check the balance around the cut-off point or check robustness using different bandwidth sizes.

This chapter contributes to the literature by providing evidence of the impact of the law passed in December 1998 and the apprenticeship programme of December 2000. For the 1998 law, I combine the exact birthdate with the date the law was enacted and use a regression discontinuity design (RDD) to identify the impact of the law on those banned from the labour market. Unlike most of the papers that turn to the impact of changes in the legal minimum age, this thesis provides the first evidence of the causal effects of the ban for both work and school related outcomes of recent law change in a developing country.

For the apprenticeship programme, I use age at the survey date and an RDD. The estimates for the apprenticeship programme complement Corseuil et al. (2012), as my results inform short-term impacts of the intervention looking at different sets of outcomes and age groups.

Given that the ban was imperfectly enforced, the participation rate among those banned from the labour force did not fully shrink. In addition, not all who are eligible to work actually work. This problem of imperfect two-sided compliance explains the fuzzy design in the case of the ban. For the apprenticeship program one observes a similar problem, as some below age 14 are observed working informally.

Imperfect compliance with the law ultimately means that participation rate remains endogenous. However, the law could be used as an instrumental variable to inform the effect on those who actually dropped out of the labour force as a consequence of the ban. The estimates discussed throughout the thesis refer to the effect of the ban on the eligible group. Given the relatively small sample size and the imperfect enforceability of the law, the instrument would very likely be weak and the estimates very imprecise.

The main results of the chapter suggest that the 1998 law reduced the labour force participation rate among 14-year-old boys but not for girls, who seem to have shifted to the informal sector. The results for boys are interesting, as they indicate that a law can be a powerful instrument to affect individuals who are not supposed to be affected since by definition the law is weakly enforced in the informal sector. It seems that girls became more likely to attend school. The results are robust to different bandwidth sizes and functional form specifications. Difference-in-differences estimates are qualitatively similar to most of the RDD estimates.

Since the law should not affect the eligibility status of those who turned 14 before the law passed, a comparison between children close to 16 years of age is performed to check whether this is really the case, as 15-year-olds should have similar participation rates to children just above age 16. The RDD estimates comparing individuals close to age 16 are statistically insignificant in most cases.

With regard to the apprenticeship programme, the RDD estimates suggest that children aged 14 became slightly more likely to participate in the formal labour force and to be in a formal paid occupation. However, the decrease in informality was strong enough to reduce the participation rate as a whole. As a result, children at age 14 are working more hours per week in formal activities and fewer hours in the informal sector, though points estimates are only statistically significant against a one-sided alternative. There is also an indication of higher school attendance among boys. These results could contribute to the design of public interventions to boost the programme's take-up rate.[31] If these impacts last over time is an open question.

For those around age 16, the RDD estimates show a decrease in the difference in participation rates between 15- and 16-year-olds, although there is some suggestive evidence that 15-year-old girls are more likely to participate in the formal labour force and have a formal occupation. On the other hand, there is some indication that girls worked fewer hours in the formal sector. The results for participation rate in the labour force as a whole suggests that the apprenticeship programme made the minimum legal age an unbinding constraint for youth aged 15. With regard to school outcomes, there is evidence that boys became less likely to attend school.

2. THE INSTITUTIONAL SETTING

2.1 Minimum Age of Entry to the Labour Market

The Brazilian Constitution of 1988 set the minimum legal age of entry into the labour market at 14, and in 1990 a federal rule called 'The Statute of Children and

[31] The low take-up rate may be related to information constraints – the target group is unaware of the programme and how to enroll – or misperceptions regarding the returns of the programme. The findings of this thesis can be used to design public interventions aimed at increasing programme take-up, similar to Jensen's (2010) successful experiment in the Dominican Republic.

Adolescents'[32] established children's and youth rights beyond regulating the conditions of formal labour market entry. Complementing the Constitution of 1988, the statute is considered the legal framework for children and youth in the labour market.[33] 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.

As a consequence of comprehensive modifications approved for the pension system on 15 December 1998, Constitutional Amendment No. 20 also increased the minimum legal age for entry into the labour market from 14 to 16.[34] According to the law, individuals under 16 could work only as apprentices, and individuals younger than 18 were prohibited from hazardous and night work. The reasons for increasing the minimum employment age are not spelled out in the law, but the main two reasons seem to be (i) the change in the retirement age based on time of contribution to the pension system that increased by two years with the Constitutional Amendment,[35] and (ii) the ratification of ILO Convention No. 138, by which Brazil agreed to set the minimum employment age above the school leaving age, which was 14 at the time the law passed.

The Constitutional Amendment of 15 December 1998 itself is mute about the penalties to be applied to those who decide to employ children below the minimum employment age. However, a recent report commissioned by the Brazilian Public Prosecutor's Office (PPO or *Ministério Público Federal* in Portuguese), the institution responsible for monitoring the practice of child labour in Brazil, employers (including parents in case the child works for a family firm) can face several forms of penalties, ranging from paying a fine and other administrative costs to being prosecuted criminally depending on the type of work performed by the child. The report is also clear in stating that children below the minimum employment age are not allowed to work as self-employed. It is important to emphasise that in neither case is the child herself penalised. The ultimate goal of the law is to protect the child (see Medeiros Neto, 2013).

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[32] *Lei do Estatuto e do Adolescente,* Law No. 8069 from 13 July 1990. Complementary to the Constitution of 1988, the statute is considered the legal framework for children and youth in the labour market.

[33] Although ILO considers a child to be an individual 17 years old or younger, in this chapter the terms 'children,' 'teenagers,' and 'youth' are used interchangeably.

[34] The law was passed on 15 December and was made effective the following day.

[35] In Brazil there are two retirement mechanisms, an age cut-off and the time contributed to the pension system. Because many Brazilians start working early in life, they end up retiring relatively early as well. With the increase in the minimum employment age, people had to postpone their entrance into the formal labour force by two years. Consequently, by the time they are eligible to retire based on the time contributed to the pension system they would be two years older.

One could question the enforceability of such a law in a country where informal activities are widespread. In fact, as will be shown later, the law is almost perfectly enforced in the formal sector, but less so in the informal sector. The good enforcement of the law in the formal sector is explained by the fact that the Ministry of Labour and Employment is the institution responsible for issuing the working permits. With the change in the law, it stopped issuing work permits for individuals who turned 14 after the law passed. Thus, the enforceability of the law in the formal sector is almost deterministic.[36]

The ban policy divided similar children into two groups: those banned from the formal labour force (‘treatment group’) and those unaffected by the ban (control group). It is interesting to note that banned children who shifted to the informal sector automatically entered the child labour statistics, whereas those of a similar age (and plausibly other characteristics) who were in the labour force but unaffected by the law did not.[37]

If the law had no bite in the informal sector, its effect on child labour would have been very small, around 1 to 2 percentage points (see Table 3). If some children participating in the formal sector simply shifted to informality after the ban, the effect of the law on child labour would have been negligible or even negative if the increase in the informality overcame the fall in the participation in the formal sector.[38] As will be discussed later, the ban seems to have affected the child labour demand as well, at least for boys. It could be that some employers decided to no longer employ children under age 16 to avoid legal consequences, such as paying fines. If that was the case, even if for a relatively small sample of firms, one should expect the law reducing participation rate in the informal sector as well.

2.2 The Apprenticeship Programme

[36] The official statistics of participation rate and weekly hours worked for children at age 15 in 1999 show a high participation rate for those with full-time jobs that year (more than 35 hours per week), suggesting that those who turned 14 before the ban were unaffected.

[37] Looking at the statistics of participation rate in the formal labour force of children aged 14, it seems that the first interpretation prevails. As discussed below, the participation rate in the formal labour force dropped to almost zero among those who turned 14 after the change in the law and remained small, though positive, among those who turned 14 before the law passed (see figure 3 below).

[38] This result is predicted in the theoretical models of Ranjan (1999) and Basu (2005). Braradwaj et al. (2013) suggest that the Indian child labour ban of 1986 led to an increase in informality and in child labour as a whole.

Two years after increasing the legal minimum working age, Brazil passed Law No. 10.097 on 19 December 2000. This law formalised the apprenticeship programme initially conceived in the 1940s. The programme was originally designed for individuals aged 14 to 17 and aimed at providing skills for youth making the transition from school to work.[39] Interestingly, the Constitutional Amendment of December 1998 refers to an apprenticeship status in the labour force despite the fact that the programme was formally institutionalised only in December 2000. In fact, the number of apprentices was very low before that year.[40]

According to the rules of the programme, an apprentice is permitted to work up to six hours per day as long as she is enrolled in secondary school and in a technical course designed to provide the skills employers require.[41] Apprentices should earn at least the hourly minimum wage.[42] The law also created a constraint for firms, as at least 5 percent of employees must be apprentices. The apprentice's contract cannot last more than two years, and firms cannot fire the apprentice except under very special circumstances.[43] To counterbalance the extra cost imposed on firms compelled to hire apprentices,[44] firms are subsidised, as they deposit only 2 percent of the worker's salary in the worker's pension fund (*Fundo de Garantia por Tempo de Serviço*), around six percentage points less than it deposits for a regular worker.

A question arising is whether the apprenticeship programme fully counterbalances the employment constraint that the 1998 law created. Depending on the take-up rate into the apprenticeship programme, the programme might have partially undermined the objective of the increase in the minimum legal age. Although the set of

[39] In Brazil this law is called *Lei do Menor Aprendiz.* In 2005 the law was amended by the *Decreto Lei* No. 5.598, which enlarged the scope of beneficiaries to individuals under age 25. According to the amendment, firms should give priority to youth aged 14 to 18 (see *Manual da Aprendizagem* 2011).
[40] According to Corseuil et al. (2011), who use the Brazilian Census of formal enterprises (*Relação Anual de Informações Sociais - RAIS*) to assess the impact of the Brazilian Apprenticeship Programme of 2000, the number of apprentices at age 14 in 1999 and 2000 was 82 and 99 respectively. On the other hand, the number of apprentices increases sharply from 2001 onwards. In 2002, for instance, the number of apprentices aged 14 reached 582.
[41] Officially qualified agencies that comprise Brazl's S-System normally provide skills training. This programme has a lot in common with European youth employment programmes (see Brodaty et al. 1999; Caliendro et al. 2011).
[42] The monthly wage is estimated as follows: [hourly (minimum) wage*weekly hours worked*number of weeks in a month*7]/6. The monthly wage is multiplied by a factor (7/6) to take the remunerated weekly rest into account (see *Manual da Aprendizagem* 2011).
[43] The apprenticeship programme does not apply to small firms (i.e., those with seven or fewer employees) (see *Manual da Aprendizagem* 2011).
[44] It is quite plausible that apprentices are less productive than their counterparts. Even paying the hourly minimum wage, firms would surely be at least as good as without such a constraint.

incentives embedded in the apprenticeship programme is very different from those that prevail in full-time occupations in the formal labour market, this is an empirical question this chapter aims to address.

It is worth pointing out that I will approach this question comparing outcomes of children who turned 14 before and after September 2002. These cohorts are different from those used to estimate the impact of the ban of 1998. Note that with the apprenticeship programme, children who turned 14 are thus exposed to both child labour laws. Because they are still under 16 in September 2002, they are banned from the formal labour force, but those just above age 14 are eligible to enter the formal labour force as apprentices.

To check whether the apprenticeship program counterbalanced the effect of the ban for those turning 14 after September 2002, I compare the participation rate of children aged 14 before and after September 1998 and 2002. Because both laws provide antagonist incentives to children entering the labour force, I will interpret the coefficients of this exercise as the composite (or net) effect of the two laws.

2.3 Theoretical Implications of the Change in the Law

Although changes to child labour laws do not mandate a direct income transfer, it is expected that they will affect the household budget constraint, either because households cannot count on children's earnings any longer or because they will get paid less in the informal labour market.[45] Children who turned 14 just before the law passed were not affected by the ban, whereas those who turned 14 just after December 1998 were affected. This peculiarity of the law results in children with similar characteristics facing different wage rates in the labour market. For children who either dropped out of the labour force or shifted to the informal sector, the lower wage rate paid in the informal sector work as a reduction in the household budget. A more limited choice set

[45] According to the Brazilian household survey (*Pesquisa Nacional por Amostra de Domicílios*) of 1998, the monthly wage paid in the formal sector was, on average, about 46 percent higher than in the informal sector (R$ 187.5 vs. R$ 128.5). The difference is statistically significant at the 1 percent level. Using the PNAD of 1999 it is possible to compare the wage rates of children affected (eligible group) and non-affected (ineligible) by the change in the law. The average wage rate of the ineligible group was 15.7 reais, whereas the eligible group faced a wage rate of 14.15 reais. The difference in means is not statistically significant, but the Kolmogorov-Smirnov test rejects the null of equal distributions at the 5 percent level (p-value of 0.049) using 6 months as bandwidth.

might lead parents to reallocate the household's income towards goods other than children's leisure.

Since the interventions might affect households differently depending on socioeconomic background and preferences and perceptions regarding returns to education, households can be divided into two groups: those that can afford children's leisure or schooling – 'not so poor' – and those that send their children to work in the informal sector – 'poor'.

Using a very simple child labour supply model with a perfect credit market (Ranjan, 1999), one could argue that if the fall in household income caused by the ban is relatively high, children would be expected to shift to the informal labour force. This would be the group of children who do not comply with the law. To use the terminology of Angrist et al. (1996), this would be equivalent to the *never-takers*.

On the other hand, if the drop in household income is relatively small for a typical household, one could hypothesise that households would compensate children's forgone income by increasing marginally adults' labour supply or cutting consumption of luxury goods. Consequently, one could expect either an increase in unpaid (domestic) work, as children would spend more time at home, or an increase in school attendance. The results can also be affected by other constraints, such as access to credit markets and the household production function.[46]

Note that the apprenticeship programme can be seen as a relaxation in the ban, as children aged 14 are once again eligible to participate in the formal labour force, although part-time. The impact of the programme on the outcomes of interest can be theoretically hypothesised along the same lines.

The next section discusses the identification strategy based on the laws of December 1998 and 2000. The objective is to check first whether the law was enforced, the consequences of the apprenticeship program, and the implications for children affected by both policies.

3. IDENTIFICATION STRATEGY

Identification of the impact of the laws under study on the outcomes of interest depends, to some degree, on compliance with both laws so that an RDD can be used to

[46] See Ranjan (1999 and 2001) and Baland and Robinson (2000).

estimate the local treatment effects of the laws. It is standard in the impact evaluation literature to use of the term 'causal' when the ultimate goal of the econometrician is to estimate the average treatment effect of a policy or intervention, that is, to uncover what would have happened to those units exposed to an intervention/programme had the programme not existed. This is known as *the fundamental problem of causal inference* (Holland, 1986). This terminology has become standard in the program evaluation literature (see e.g. Imbens and Rubin 2015), applied labor economics literature (see e.g. Angrist and Pischke 2009), and applied development economics literature (see e.g. Duflo et al. 2008).

According to Lee and Lemieux (2010), when the assumptions necessary to make the RDD a credible identification strategy hold, RDD can be seen as a local randomised trial. Thus, 'causal effects' in the context of this thesis refer to any observed difference in means in the outcomes of interest that can, under plausible assumptions, be attributed to the an intervention ('treatment'). In this thesis, the focus is on the effect of the assignment rule rather than the treatment itself for reasons discussed below.

Since household surveys are annual, the difference-in-differences (DD) approach is also used. The identification strategy of the DD draws directly on the discontinuities generated by the two laws and uses information about different cohorts of teenagers before and after the intervention. The DD can be seen as a way of checking robustness (see Lemieux and Milligan 2008). The subsection below describes the RD design in more detail and shows how it will be used to identify the effect of the laws. It is then followed by a brief description of how the DD approach is used to inform the impact of the policies comparing different cohorts in two periods of time.

3.1 Regression Discontinuity Design

The identification strategy is based on the discontinuities that can result from a rule that makes a group of individuals eligible to participate in a programme based on an arbitrary threshold of some observed variable. The rule therefore exogenously assigns groups of individuals to the eligible and ineligible groups, making the identification of the impact of the programme possible to estimate by comparing units just on the right and the left of the threshold.

For that reason, RDD is widely seen as a quasi-experimental technique, as the existence of an assignment variable (also called the forcing variable) gives rise to a natural experiment for those units close to the cut-off point. The identification strategy requires that, on average, the units close to the cut-off point be similar in observed and unobserved characteristics so that the only difference between them is that one can take up a treatment due to some exogenous reason and the other cannot (Lee and Lemieux 2010 and Imbens and Lemieux 2008).

The validity of the RDD depends on (1) the similarity in observed and unobserved characteristics around the threshold, and (2) the lack of perfect manipulation of the assignment variable. The first test consists of checking whether children close to the cut-off point have, on average, similar observed characteristics. One cannot check whether children are similar in unobserved characteristics, but being similar enough in observed characteristics suggests that the two groups are, on average, similar in unobserved characteristics as well[47]. The second test consists of checking if the assignment variable is smooth around the threshold. A high concentration of eligible units close to the cutoff point would indicate that children (or their parents) could be sorting, that is, misreporting their age in order to enter work. This condition can be formally tested as shown below.

In cases in which all eligibles take up the treatment through a deterministic process, the discontinuity design is called ‘sharp.’ In most of the cases, the forcing variable defines an eligibility status and the take-up decision is up to the individual. In the case where only a subsample of the eligible group takes up the treatment, the design is called ‘fuzzy.’ Unlike the sharp design, the fuzzy design gives space for individuals to self-select into the programme. Thus, the estimates of the causal effect under the fuzzy design require more assumptions than under the sharp design, but are weaker than any IV approach.[48]

The key assumption of a fuzzy design is that without the assignment rule some of those who take up the treatment would not participate in the programme (for similarities between IV and RDD approaches, see Imbens and Lemieux 2008 and van der Klaauw 2008). The forcing variable acts as a nudge. The subgroup that participates in a programme due to the selection rule is called *compliers* (see e.g. Angrist and

[47] Exactly the same assumption has to be made in randomised controlled trials.
[48] It requires the monotonicity assumption, that is, that the participation rate among the eligible participants is higher (or lower, as in the present case) than the participation rate of those who are ineligible. Unlike the IV, it does not require exclusion restriction, as the forcing variable is allowed to have a direct effect on the outcome. For this point see Lee and Lemieux (2009).

Imbens 1994, and Imbens, Angrist, and Rubin 1996). Thus, under the RDD the treatment effects are estimated only for the group of compliers.

When the group of compliers is identified, and assuming a binary treatment variable, the Wald estimate is obtained by dividing the impact of the eligibility rule for the outcome of interest (the *intent-to-treat* estimator) by the proportion of the eligible group that took up the treatment. The Wald estimator can be seen as an IV estimator and can be estimated in two steps. The first step consists of a regression of the treatment variable (*X*) on the assignment to the treatment variable (*Z*). For the sake of illustration, 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*, with $\hat{\beta}_{itt}$ being the estimate of the effect of *Z* on *Y*. The Wald estimator is then 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 (Hahn, Todd, and Van der Klaauw, 2001).[49]

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 arbitrarily 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^-.

[49] Hahn et al. (2001) show that the RDD estimators can be seen as Local Wald versions of the aforementioned IV. As in 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 compliers.

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 latter $X^+ \neq 1$ 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, i.e., the treatment effect for the individuals close to the cut-off, Hahn et al. (2001) note that this method has many advantages compared to other quasi-experimental approaches in that it does not depend on functional form assumptions when estimates can be obtained with narrow bandwidths 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 chapter affect the eligibility of individuals aged 14 and 15 to participate in the formal labour market. Thus, the laws give rise to two fuzzy designs.[50] Before December 1998, one expects to observe a jump from zero to a positive number in the participation rate in the formal labour force around age 14, as 13-year-olds were not permitted to participate in the formal labour market. Thus, since the estimation of the effect of the minimum legal age on participation in the formal labour market after December 1998 will concentrate only on those who are observed working after age 14, the parameter of interest can be interpreted as the local average treatment effect on the treated (LATT) (Battistin and Rittore 2008). After the increase in the legal minimum age, the data should no longer detect a discontinuity.[51] For all other outcomes than participation in the formal labour market, the parameter of interest can be seen as a

[50] 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 RDD is not an issue of concern in the present case.

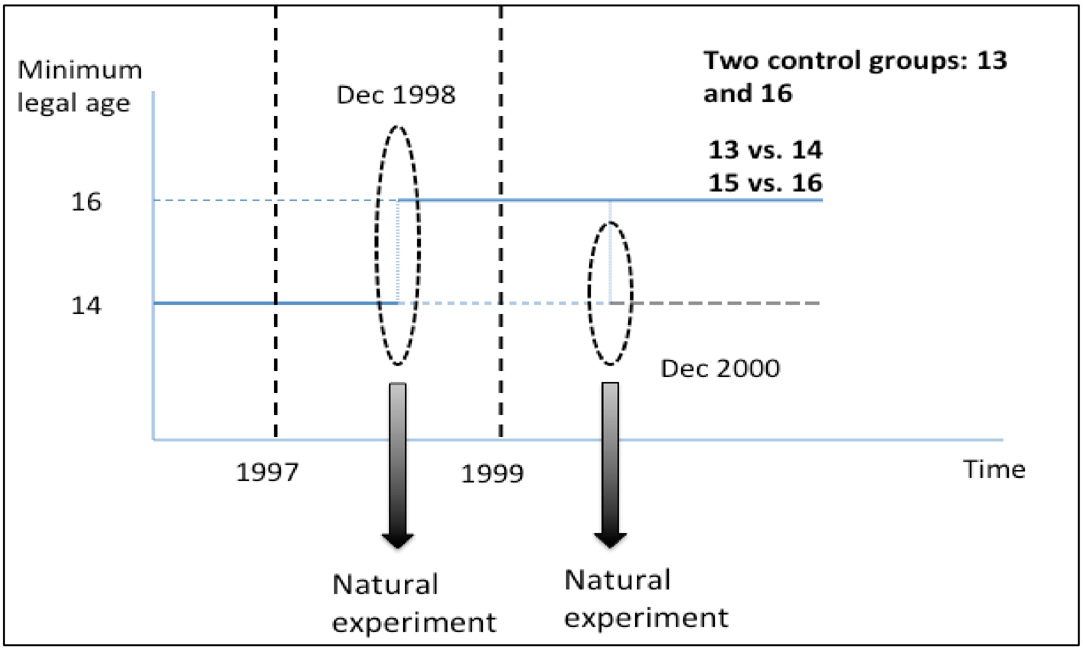
[51] In fact, before the increase in the minimum age in December 1998, X^- equals zero. Corseuil et al. (2012) also employ this estimator.

local intent to treat, since the law likely affected the other outcomes directly and an instrumental variable strategy would not provide credible estimates.[52]

A complementary exercise is undertaken comparing teenagers just under age 16 with teenagers just over age 16. No discontinuity is expected in participation rate in the formal labour force before and after the law passed, because 15-year-olds did not have their eligibility status changed by the ban. For this reason, the impact of the ban on those close to age 16 can be seen as a placebo test, as no effect should be observed for this age group.

The figure below illustrates how the law is used to identify the local average treatment effect for both age groups.

Figure 1 – Timeline of the laws of 1998 and 2000



The estimation of the effect of the laws can be performed parametrically using linear probability models (OLS) to fit the following reduced form regression model:

$$y_{ic} = \alpha + \delta T_{ic} + h(Z_{ic}) + \varepsilon_{ic} \qquad (1)$$

where y_{ic} is the outcome of interest of individual i in cohort c, T_{ic} is an indicator function that takes the value of 1 for individuals in the cohort affected by the child labour laws, and 0 for individuals in the cohort unaffected by the laws. Z is the

[52] When there is perfect compliance of the control group (i.e., nobody from the control group participates in the programme), the Wald estimate will be equivalent to the average treatment effect on the treated. For this point, see Duflo et al. (2008).

assignment variable that defines an observable clear cut-off point, and *h(Z)* is a polynomial function in *Z*. The identification strategy therefore relies on the exogeneity of *Z* at the threshold. With that assumption, the parameter of interest, δ, provides the estimate of the intent-to-treat – the effect of the eligibility rather than the treatment itself on the outcomes of interest. In the present case, the ITT estimates are obtained comparing the average of the outcome variables of children who turned 14 after December 1998 ('eligible') with children who turned 14 in December 1998 or before (comparison group).

To estimate the impact of the 1998 ban on 14-year-olds, *Z* is defined in accordance with the individual's exact date of birth. Since the household survey reports the date of birth and the date the survey was collected, one can define age in days, weeks, or months at the date the law passed and/or at the date of the survey.[53]

Age is defined in weeks in the empirical analysis age.[54] The variable *Z* takes the value of 0 for individuals who turned 14 in the last week of December 1998, 1 for those who turned 14 in the first week of January 1999, 2 for those who turned 14 in the second week of January 1999, and so on.

For the comparison between children close to age 16, *Z* is defined slightly differently. According to the Brazilian bureau of statistics, IBGE, the reference date for the Brazilian household survey is the last week of September.[55] Thus, *Z* takes the value of 0 if an individual turned 16 in the last week of September 1999, 1 for individuals aged 16 in the first week of October 1999, 2 in the second week of October 1999 and so on. The dummy *T* takes the value of 1 for teenagers aged 16 or above and 0 otherwise. Note that for this age group the use of the exact date of birth when the law passed would make no difference, because individuals aged 15 before December 1998 did not have their eligibility status affected by the ban.

The estimation of the impact of the apprenticeship programme uses exactly the same definition of the forcing variable *Z* for both age groups. For individuals aged 14 (16), Z takes the value of 0 for children who turned 14 in the last week of September 2002, 1 for those aged 14 (16) in the first week of October 2002, and so on. The dummy T takes the value of 1 for $Z > 0$ and zero for $Z \leq 0$.

[53] The survey is usually collected in the last week of September. The exact date suggested by the Brazilian bureau of statistics is the last week of September, or 26 September 1998.
[54] Age in days could also be provided but would add too much noise to the estimates.
[55] IBGE uses individuals' date of birth to define the exact age in years in that reference date. The Brazilian Bureau of Statistics collects the annual household survey between October and December of each year except in census years.

Since no discontinuity is expected around age 16, the comparison of the labour force participation rate of children just under and just above age 16 can be seen as a way of checking whether children who turned 14 before the ban were, in fact, unaffected by the ban. That said, the effects of major interest of both laws are those that come from the comparison of 14-year-old children around the date the ban passed with 14-year-old children around the survey's reference date in the case of the apprenticeship programme.

As long as the law affected individuals' eligibility to participate in the labour force and there is no indication of sorting or perfect manipulation of the forcing variable, the law would work as a local experiment, as children around the cutoff point are expected to have similar observed and unobserved characteristics. In this case, because Z is orthogonal to the error term, the covariates are expected to be uncorrelated with eligibility status. The inclusion of covariates in the estimates would be justified only with the purpose of improving efficiency of local estimates (Imbens and Lemieux, 2008; Lee and Lemieux, 2010).[56] To explore sensitivity of results, the specification of eq. (1) is extended, accommodating interaction terms between T and $h(Z)$ to allow for different slopes on both sides of the cut-off point.

Table 1 summarises how the treatment and control groups are defined for different age groups in different years.

Table 1 – Definition of the Treatment and Control Groups for the RDD Estimates

	1997	1999	2002	1997	1999	2002
	13 vs. 14			15 vs. 16		
Eligible Group	>=14 on the date of the survey (26 Sept)	14 after 31December 1998	>=14 on the date of the survey (26 Sept)	>=16 on the date of the survey (26 Sept)	>=16 on the date of the survey (26 Sept)	>=16 on the date of the survey (26 Sept)
Control Group	< 14 on the date of the survey (26 Sept)	14 up to 31 December 1998	< 14 on the date of the survey (26 Sept)	< 16 on the date of the survey (26 Sept)	< 16 on the date of the survey (26 Sept)	< 16 on the date of the survey (26 Sept)

[56] Lee and Lemieux (2010) argue that the covariates have to be continuous around the cut-off, that is, have equal distributions, so as the RDD can be interpretable as a local experiment. As stated by the authors (2010, p. 297), a consequence of a local random assignment 'is that the assignment to treatment is, by construction, independent of the baseline covariates. As such, it is not necessary to include them [in the regression model] to obtain consistent estimates of the treatment effect.'

Equation (1) is fit parametrically with several functional forms.[57] The smooth function is specified as polynomials of orders zero to three, and as linear and quadratic splines.[58]

The caveat that underlies the specification of the *h(Z)* function is directly related to bandwidth size. The narrower the bandwidth size, the less the need for a flexible *h(Z)* function. As Lee and Lemieux (2010) suggest, for a very small bandwidth size the LATE could be estimated through a simple difference of means. However, the feasibility of this strategy depends on a sufficient number of observations just on the right and the left of the cut-off point. A small bandwidth minimises the bias and frees up the researcher from looking for the optimal specification; however, it increases the sampling variance and harms the precision of local estimates. Estimates are therefore provided with bandwidths of 20 weeks (5 months) and 16 weeks (4 months).[59] These bandwidth sizes are actually smaller than what seems to be the optimal choice based on the Imbens and Kalyanaraman (2012) algorithm.[60] To select the functional form of the smooth function, Lee and Lemieux (2009) recommend the generalised cross-validation technique, but Gelman and Imbens (2014) show that this technique is inadequate since it is a global fit measure.[61]

To test whether there is a manipulation of the forcing variable, I use the McCrary density test. This test plots density functions on each side of the cut-off point to check how smoothly (or continuously) they behave around the threshold. Under the null of absence of perfect control over the assignment variable, one should observe no jump around the threshold. The presence of a statistically significant discontinuity of the assignment variable around the cut-off point would characterise either perfect manipulation of the eligibility status or some measurement error due to data heap. The latter seems to be very common when the assignment variable is self-reported. In either

[57] 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 RDD.
[58] Higher order polynomials are avoided, because I use parametric regressions. For this point, see Gelman and Imbens (2014). The higher the order of the polynomial the higher the weight assigned to observations far from the cut-off point.
[59] I have also estimated the regressions with 12 weeks bandwidth. The results are almost identical to those obtained with 16 weeks bandwidth.
[60] Imbens and Kalyanaraman (2012) recommend a bandwidth that minimises the mean squared error, which is equivalent to minimising the sum of bias and variance.
[61] Lee and Lemiux (2009) recommend the Akaike criterion information for model selection, but Gelman and Imbens (2014) argue that such global fit tests are not very informative, particularly when one uses non-parametric regression (local linear and local polynomial).

case, the rejection of the null hypothesis would put in check the identification strategy of the RDD (see Barreca et al. 2015).

3.2 Difference-in-Differences

The empirical approach can be extended so as to take into account pre-treatment information on different cohorts of children aged 14 and 15. To estimate the effect of the 1998 law, children who turned 14 around December 1998 are compared with those who turned 14 around December 1997. Note that the second difference, that is, the difference in outcomes between treatment and control groups around December 1998, is equivalent to the RDD estimates as explained above, whereas the first difference will be given by the difference in outcomes of 14-year-olds before and after December 1997.[62] For the apprenticeship programme, comparison is made between children around age 14 on the surveys' reference dates – September 1998 and September 1999 respectively.

DD estimates are also provided for the 'net' effect of the both laws. To do so, the PNADs of 1998 and 2002 are used. Note that because the apprenticeship programme allowed 15-year-olds to participate in the formal labour force, at least part-time, the coefficient of the joint effect of the two laws will pick up the lower-bound effect of the ban. Table 2 shows how the 'treatment' and comparison groups are defined for the difference-in-differences estimation.

[62] The DD estimate will be given by: $DD = \left(y_{Dec98}^{14after} - y_{Dec98}^{14before}\right) - \left(y_{Dec97}^{14after} - y_{Dec97}^{14before}\right)$

Table 2 – Definition of the Eligible and Comparison Groups for the DD Estimates

	1998	1999	2002	1998	1999	2002
		13 vs. 14			*15 vs. 16*	
		Minimum Legal Age of 1998			***Minimum Legal Age of 1998***	
Eligible Group	14 after 31 December 1997	14 after 31 December 1998		>=16 on the date of the survey (26 Sept)		>=16 on the date of the survey (26 Sept)
Comparison Group	14 before 31 December 1997	14 before 31 December 1998		< 16 on the date of the survey (26 Sept)		< 16 on the date of the survey (26 Sept)
		Apprenticeship Programme of 2000			***Apprenticeship Programme of 2000***	
Eligible Group		>= 14 at 26 September 1999	>= 14 at 26 September 2002		>=16 at the date of the survey (26 Sept)	>=16 on the date of the survey (26 Sept)
Comparison Group		< 14 at 26 September 1999	< 14 at 26 September 2002		< 16 at the date of the survey (26 Sept)	< 16 at the date of the survey (26 Sept)
		Net Effect			***Net Effect***	
Eligible Group	>= 14 at 26 September 1999		>= 14 at 26 September 2002	>=16 at the date of the survey (26 Sept)		>=16 at the date of the survey (26 Sept)
Comparison Group	< 14 at 26 September 1999		< 14 at 26 September 2002	< 16 at the date of the survey (26 Sept)		< 16 at the date of the survey (26 Sept)

The identification strategy for the DD depends on two assumptions: (1) the difference in labour force participation between the treatment and control groups exists in level but not in difference, i.e., the groups would show a common trend in the absence of the laws. This is a key assumption in the DD framework, and in the present case it might be even stronger, since the comparison is between different cohorts;[63] and (2) all unobserved characteristics that can be correlated with the eligibility status of the individual or other covariates are additive and time-invariant.[64] In fact, it is impossible to control for individual fixed effects in the present case, since the analysis compares cohorts. Instead, it is assumed that individuals of the same age group have characteristics that are common and invariant across time. In other words, the difference

[63] 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 in time 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-differences matching estimator see also Heckman et al. (1997) and Blundell and Dias (2002).

[64] 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 unobserved characteristics.

in participation rate that would prevail in the absence of the ban would be the same as that observed between similar age groups the year before.

The estimation of the impact of the 1998 law on the outcomes of interest is conducted through the regression model:

$$y_{ict} = \beta_0 + X'_{ict}\beta_1 + \beta_2 T_{ic} + \beta_3 D_{99} + \delta T_{ic} D_{99} + u_{ict} \quad (2)$$

where y_{ict} is the outcome variable of individual *i* in cohort *c* in time *t*, X_{ict} is the vector of observed characteristics of individual *i* in cohort *c* in time *t*. The vector includes dummies of individuals' gender and ethnicity, years of schooling of the household head, age of the household head, gender of the household head, and dummy variables for regions and the metropolitan region. T_{ic} is a dummy variable that equals 1 if individual *i* in cohort *c* turned 14 after the date of the ban and zero if (s)he was 14 before the date of the ban, D_{99} is a year dummy that takes value 1 in 1999 and zero for 1998, and u_{ict} denotes a composite error term, $\mu_{ic} + \varepsilon_{ict}$, which is allowed to be correlated with *X* and *T* through effects that are common to cohorts but fixed in time, μ_{ct} (see Meyer 1995).[65] The parameter of interest, δ, provides the average treatment effect on the cohort affected by the ban. Since we do not observe the same individuals before and after the ban, we cannot be sure who dropped out of the labour force after the ban. The parameter δ therefore captures the intent-to-treat, that is, the impact of the ban on the cohort that was hindered from participating in the formal labour force at age 14 after December 1998. The estimation of the impact of the apprenticeship programme is performed similarly, with age defined in accordance with the date of the survey as discussed above. For both laws, DD estimates will be provided with 16 weeks bandwidth only.

For the binary outcomes, linear probability models are used to estimate the marginal effects.[66] For weekly hours worked, I estimate a Tobit model, as many children in the dataset work zero hours per week. Unfortunately, the Tobit regressions did not converge in the RDD regressions. I therefore provide DD estimates for the intensive margin of labour supply only.

[65] For individuals aged 15, age is defined according to the date of the survey. See table 2.
[66] The coefficients of linear probability model have a straightforward interpretation, but the there are two drawbacks associated with linear probability models. First, the probability distribution does not necessarily lie within the [0, 1] interval. In some cases it can even be negative. The second issue has to do with the linearity assumption. The marginal effect is constant for all values of the covariates.

4 DATA

The sample used in this chapter is drawn from the Brazilian household surveys (*Pesquisa Nacional por Amostra de Domicilios* – PNAD) of 1997, 1998, 1999, 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.[67] The year 2001 was dropped, because take-up into the apprenticeship programme was close to zero.

The PNAD 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, and household income and labour force status.

According to IBGE, the survey is collected between October and December each year, although the institute provides three different dates as reference: the month, the week, and the day the survey was collected. The month of reference is always September and the week is always the last of that month. The day changes from year to year, but it is usually 26 or 27 of September. The sub-samples of interest are the urban cohorts of children aged 13, 14, 15, and 16.

4.1 Definition of the Outcomes

The extensive margin of child labour relies on the following measures: labour force participation rate(s), participation in the formal labour force, participation rate in the informal labour force, and household chores.[68]

The first definition of labour force participation rate is based on whether the child is employed in the week the survey took place.[69] Thus, LFPR is a dummy that

[67] The last year in which the household survey was collected annually was 2013. In 2014, the IBGE merged PNAD and the Monthly Employment Survey (PME) into a new survey called *PNAD Continua*. *PNAD Continua* is a rotating quarterly panel covering fewer households than the original PNAD, but unlike the original labour survey, it is still nationally representative.
[68] Although *child labour* is generally associated with hazardous activity and authors such as Edmonds (2008) suggest the term *child work* to refer to non-hazardous work, in this chapter both terms will be used interchangeably.
[69] This includes market work and housework. The PNAD differentiates housework such as food production for personal consumption and construction for personal 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.

takes the value of 1 if the individual is employed in the week of reference and zero otherwise.

A child is regarded as formal worker if (s)he has a formal labour contract with her/his employer. All formal labour contract of a worker is registered in her/his work permit (in Brazil called *carteira de trabalho*) issued by the Brazilian Ministry of Labour.[70] The work permit is akin to a notebook where the employee keeps records of all her/his labour contracts in the formal sector. A registered (formal) worker has to have her/his work permit dully signed by the employer. According to Brazilian labour legislation, a formal worker should begin working only after receiving the registration of the employer in her/his work permit. In addition to the wages, formal workers are endowed with a set of legal rights that include unemployment insurance benefits and compulsory savings accounts that can be accessed in the case of dismissal.[71]

The Brazilian household survey asks whether the individual has a job in the week of reference. For those who answer yes, surveyors then ask whether or not (s)he is registered in that work. Thus, the variable “formal” is a dummy that equals 1 if the individual was an active registered worker in the week of reference and zero otherwise (working unregistered or not working at all). I also look at occupations in the formal sector. This variable is referred to as “formal paid work” or “occupation in the formal sector” and is defined as a dummy that takes on the value of 1 if the individual was an active registered worker and zero if (s)he was working unregistered. The formal occupation dummy is an alternative measure to capture the effect of the laws on formality rate, as this variable has more variability than the “formal” dummy.

Participation rate in the informal labour market mirrors the definition of “formal,” i.e., a child is considered to be an informal worker if (s)he works unregistered and zero otherwise (working registered or not participating in the labour market at all). Participation rate in household chores is also considered in the analysis so that we can get a better picture of time-reallocation caused by the laws. Household chores are defined separately from the other three measures of child labour, as they are very different from work performed outside home. Thus, the variables could also be categorised as a “paid work” and “unpaid work.”

[70] This definition does not include domestic servants, because in Brazil domestic servants are covered by separate legislation.
[71] A similar definition is given by Fernandes and Felício (2005).

The empirical analysis is performed considering five measures of work incidence: (1) LFPR; (2) LFPR in the formal labour market in the week of reference; (3) LFPR in the informal sector in the week of reference; (4) occupation in formal paid activities; and (5) participation rate in household chores (unpaid work).[72]
The PNAD also reports the weekly hours worked. Thus, it is possible to estimate the impact of the law on the intensive margin of child labour supply. For the latter, two measures are considered: weekly hours worked in the formal labour market and weekly hours worked in the informal labour market.[73]

For school related outcomes one looks at school attendance, as this is the most commonly used indicator 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. School attendance is defined as a dummy that equals 1 if the individual attended school in the week of reference and zero otherwise.

4.2 Descriptive Statistics

Table 3 shows how the outcome variables evolved between 1997 and 2002 among children aged 13 to 16. The tables also show the statistics for boys and girls separately. The data is restricted to children living in urban areas, because (1) rural areas are underrepresented in the Brazilian PNADs; (2) the laws might be easier to enforce in urban areas; (3) there is a lower incidence of formal workers in rural areas; and (4) the potential for bias due to cash transfer programs designed for rural children in particular.[74]

The tables show interesting patterns. First, although the labour force participation rate increases monotonically with age, one can observe a fall in the incidence of child labour. The proportion of children aged 14 in the formal labour force was small in 1997, but dropped to almost zero with the increase in the minimum legal age. Although the occupation rate in the formal sector stayed almost stable for boys

[72] The definitions of participation rate in the formal and informal labour forces do not include active workers and those who had a job in the last 12 months. Consequently, the participation rates in the formal and informal labour forces will be equal to LFPR but not to LFPR2.
[73] Emerson and Souza (2003, 2007, 2008, and 2011) use this information to define the indicator measure for child labour. Based on their definition, a child is defined as participating in the labour force if (s)he worked any positive number of hours per week.
[74] 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).

between 1997 and 1998, it halved by September 1999. A similar pattern is observed for 15-year-olds, but for this age group participation in the formal labour force does not shrink completely in 1999, because some of these children turned 14 before the ban.

Girls are much less likely to participate in the labour force than boys. Participation rates in both formal and informal sectors increases with age, but remains well below those registered for boys. In 1998, for instance, only 2.1 percent of 14-year-old girls were working compared to 7.7 percent of boys in the same age group.

Since children who turned 14 before December 1998 could carry on working in the formal sector, by September 1999 some of those children would be 15 years old. This peculiarity of the law of 1998 implied a higher proportion of 15-year-olds participating in the formal labour force in 1999 – after the ban – than in 2002 – after the apprenticeship program. Also interesting is the high participation rate in the informal sector, even among 16-year-old children. This suggests that the minimum legal age is not the only constraint affecting children's (or parents') decisions to work in the formal sector.

When attention is turned to household chores, it seems that this type of work is much less sensitive to macroeconomic shocks and perhaps to demographic changes in Brazilian society.[75] Household chores are much more prevalent among girls and are quite stable across age groups and time.

Children's school and work outcomes have to do with how their time is allocated, and as such time allocation decisions need to be thought of as simultaneous decisions taken by children themselves or their parents.[76] Thus, it is worth checking how school attendance evolved over this period.

Children working informally used to work about 30 hours per week or approximately 5 hours per weekday as shown in table 1. The number of weekly hours in the formal sector was much lower for boys and girls aged 14 or more. The small proportion of 14-year-olds participating in the formal labour force suggests that estimates for the intensive margin of labour supply will likely be noisy and imprecise.

The table shows that the attendance rate is slightly higher among girls, regardless of age, and that it increased over time for both boys and girls in all age groups. The literature has reported a trade-off between school attendance and child

[75] The introduction of the thesis lists some of the potential factors underlying the decrease in child labour over the years in Brazil.
[76] Although we assume that parents are responsible for children's time allocation, this does not necessarily mean that an altruistic household model is assumed.

labour, but suggests by descriptive statistics that the two outcomes are far from perfect substitutes.[77]

The figure below shows the time allocation between working and studying of 14-year-olds at the time of the ban of December 1998. The statistics are for the group of 'control' children only, that is, those who turned 14 before the ban, and used a window of 5 months. Thus, the distributions can be seen as counterfactual distributions for the cohort of children affected by the ban. The great majority 14-year-old children in the comparison group were only studying, whereas among the working children (11.1 percent in total), the majority were also studying.

[77] In fact, 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 12th grade performed worse on math and reading exams. Obviously this might not hold in countries with poor school quality.

Table 3 – Descriptive Statistics for the Outcome Variables Between 1997 and 2002

	13				14				15				16			
	Girls		Boys		Girls		Boys		Girls		Boys		Girls		Boys	
	N	Mean	N	Mean	N	Mean	N	Mean	N	Mean	N	Mean	N	Mean	N	Mean
1997																
LFPR	1916	0.029	1879	0.04	1687	0.056	1625	0.098	2141	0.115	2015	0.178	2152	0.171	1825	0.262
Formal	1916	0	1879	0	1687	0.004	1625	0.016	2141	0.027	2015	0.049	2152	0.043	1825	0.084
Informal	1916	0.029	1879	0.04	1687	0.052	1625	0.082	2141	0.088	2015	0.129	2152	0.127	1825	0.178
Formal occupation	55	0	75	0	94	0.064	160	0.163	248	0.238	358	0.274	370	0.254	481	0.322
Weekly hours worked formal	60	0	92	0	97	2.454	172	5.692	250	9.216	369	10.425	375	10.008	495	12.453
Weekly hours worked informal	60	28.217	92	25.38	97	36.856	172	26.174	250	29.872	369	25.694	375	29.203	495	25.147
Household chores	1779	0.836	1704	0.499	1474	0.843	1311	0.5	1678	0.865	1399	0.482	1461	0.849	1034	0.477
School attendance	1916	0.951	1879	0.949	1687	0.902	1625	0.919	2141	0.858	2015	0.864	2152	0.812	1825	0.789
1998																
LFPR	1876	0.007	1768	0.037	1853	0.021	1812	0.077	1944	0.056	1935	0.143	1963	0.104	1836	0.241
Formal	1876	0.001	1768	0.002	1853	0.002	1812	0.011	1944	0.015	1935	0.034	1963	0.037	1836	0.076
Informal	1876	0.007	1768	0.036	1853	0.019	1812	0.066	1944	0.041	1935	0.11	1963	0.067	1836	0.165
Formal occupation	14	0.071	66	0.045	39	0.103	140	0.143	108	0.269	279	0.233	204	0.358	445	0.315
Weekly hours worked formal	14	2.5	67	1.403	39	2.615	141	4.787	108	9.676	279	8.72	204	13.216	445	12.166
Weekly hours worked informal	14	24.571	67	26.657	39	26	141	28.759	108	25.426	279	27.358	204	22.931	445	26.926
Household chores	1821	0.82	1653	0.506	1756	0.846	1606	0.507	1726	0.838	1548	0.506	1623	0.842	1235	0.498
School attendance	1879	0.959	1778	0.954	1863	0.939	1829	0.928	1944	0.905	1935	0.896	1963	0.842	1836	0.839
1999																
LFPR	1854	0.009	1794	0.038	1846	0.02	1784	0.062	1800	0.039	1755	0.13	1804	0.075	1833	0.223
Formal	1854	0	1794	0	1846	0.002	1784	0.003	1800	0.007	1755	0.018	1804	0.032	1833	0.077
Informal	1854	0.009	1794	0.038	1846	0.018	1784	0.059	1800	0.032	1755	0.112	1804	0.043	1833	0.146
Formal occupation	17	0	68	0	37	0.108	111	0.045	71	0.183	228	0.136	135	0.43	410	0.344
Weekly hours worked formal	17	0	68	0	37	4.973	112	1.214	71	6.845	228	5.101	135	15.867	410	13.576
Weekly hours worked informal	17	22.235	68	26.75	37	25.649	112	30.741	71	28.394	228	30.307	135	20.119	410	24.544
Household chores	1797	0.845	1678	0.537	1769	0.858	1618	0.54	1643	0.872	1433	0.513	1556	0.875	1274	0.509
School attendance	1854	0.967	1794	0.969	1846	0.943	1784	0.942	1800	0.908	1755	0.906	1804	0.86	1833	0.852
2002																
LFPR	1960	0.022	1978	0.039	2059	0.042	2032	0.058	2034	0.076	1976	0.127	2111	0.135	1977	0.213
Formal	1960	0.001	1978	0.001	2059	0.001	2032	0.003	2034	0.004	1976	0.009	2111	0.021	1977	0.042
Informal	1960	0.021	1978	0.038	2059	0.041	2032	0.054	2034	0.071	1976	0.118	2111	0.114	1977	0.171
Formal occupation	44	0.045	77	0.026	88	0.034	117	0.06	155	0.058	254	0.067	286	0.157	424	0.196
Weekly hours worked formal	47	1.872	87	0.92	90	1.422	132	2.01	161	2.335	267	2.629	292	5.795	438	7.438
Weekly hours worked informal	47	22.128	87	24.218	90	30.711	132	27.583	161	30.832	267	28.607	292	29.884	438	27.623
Household chores	1896	0.782	1847	0.485	1904	0.831	1823	0.491	1710	0.848	1542	0.473	1557	0.824	1276	0.456
School attendance	1960	0.973	1978	0.965	2059	0.96	2032	0.949	2034	0.91	1976	0.917	2111	0.85	1977	0.861

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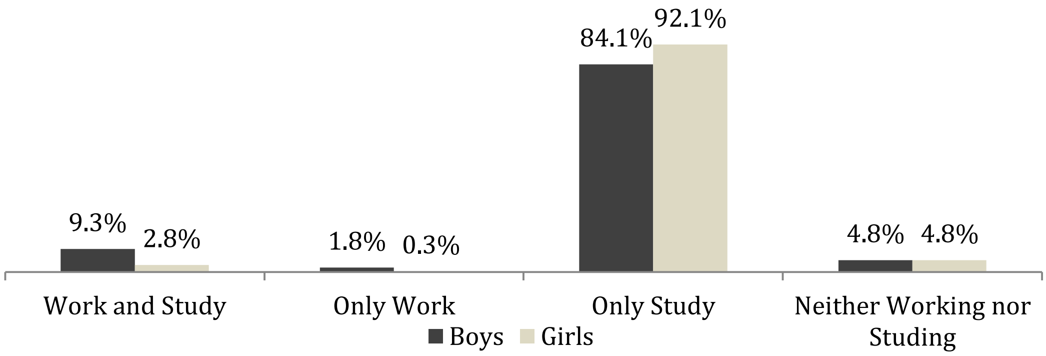
Very few were only working. It is clear in figure that girls are much more likely than boys to be studying full-time, while boys are more likely to conciliate working and schooling activities.

Figure 2 – Time Allocation of 14-year-old ‘Control’ Children Between Schooling and Working Activities



Source: PNAD1999.

4.3 Visual Check of the Discontinuities

According to Imbens and Lemieux (2008), the regression discontinuity analysis should start with a visual check. The figures below inspect whether the laws created discontinuity in the participation rate in the labour force among children close to age 14 and 16 respectively. I follow Lee and Lemieux (2010) and compute the unconditional mean of the outcome variable for each month bin and fit local linear regression lines with a triangle kernel on each side of the threshold. For all figures, the 95 per cent confidence interval is also showed. I used monthly bins to visualise the potential effect of the laws instead of weekly or daily to better smooth the regression lines.[78] The local regressions were estimated with a one-month bandwidth over a year interval, that is, between -12 and 12 months.

The identification of the causal impact of the law of 1998 in the RDD framework depends on the discontinuity in the participation rate of teenagers who turned 14 just after December 1998. Similarly, for the apprenticeship programme, the

[78] With regard to the bandwidth choice, they say (p. 308 and 309), ‘In practice, this is typically done informally by trying to pick a bandwidth that makes the graphs look informative in the sense that bins are wide enough to reduce the amount of noise, but narrow enough to compare observations “close enough” on both sides of the cutoff point.’

effect is identified through a jump in the participation rate distribution around age 14 by the time the household survey is collected.

Figures 3 to 7 check the effect of the ban. In these figures the threshold is defined at December 1998, and the comparison is made between children who turned 14 in December 1998 or before 1998 ($Z \leq 0$) and children who turned 14 after ($Z > 0$) December 1998.

Figure 2 suggests that ban was well enforced in the formal sector, as the participation rate among those who turned 14 after the ban dropped to almost zero. Figures 4 and 5 indicate that the ban was relatively well enforced in the informal sector, at least for boys, given the sharp reduction in their participation rate. There is no equivalent fall in girls' participation rate, and it might because the participation rate of girls was relatively low even before the ban, as shown in table 3 above. Interestingly, figure 6 points to a small increase in girls' school attendance rate after the ban.

Figure 3 – Boys' Participation Rate in the Formal Labour Force – 1999
14 Before and After Dec 1998

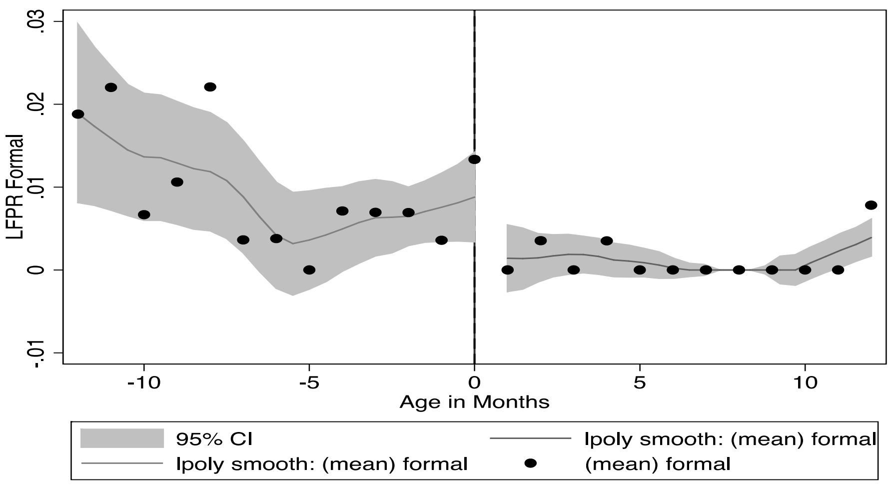


Figure 4 – Boys' Participation Rate in the Labour Force –1999
14 Before and After Dec 1998

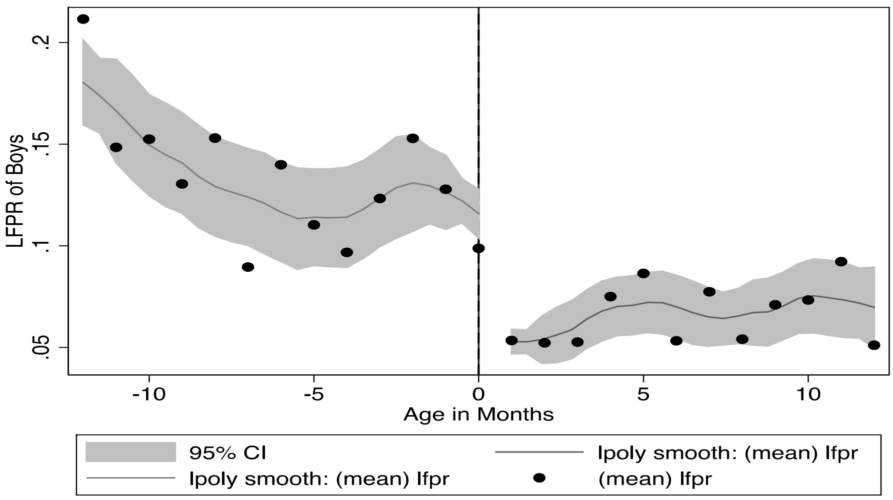


Figure 5 – Girls' Participation Rate in the Labour Force –1999
14 Before and After Dec 1998

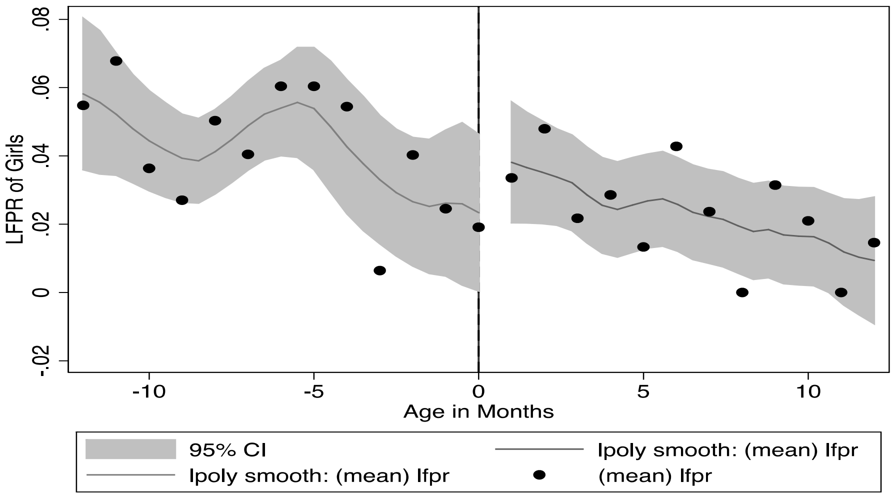
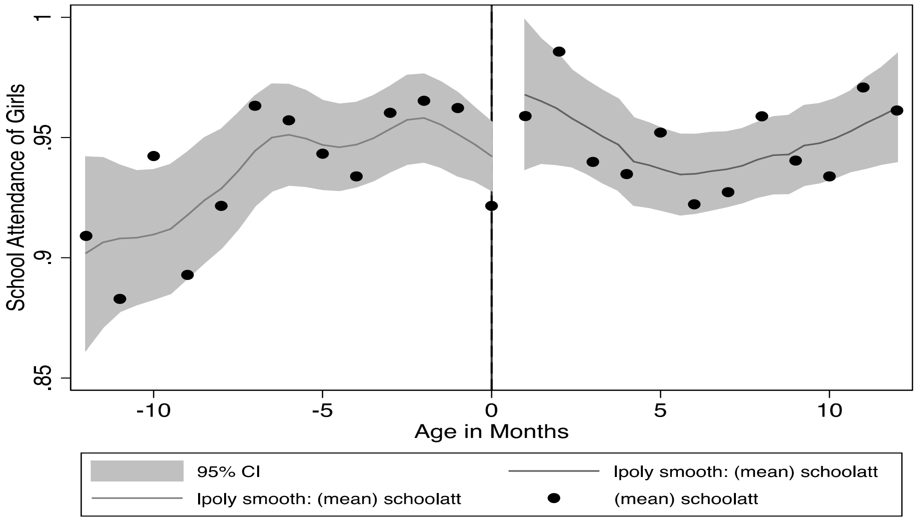


Figure 6 – Girls' School Attendance –1999
14 Before and After Dec 1998



Figures 7 and 8 show the participation rate and the participation rate in the formal labour force for children around age 16. While figure 6 suggests a small jump in participation rate, figure 8 indicates that the participation rate in the formal labour force of children around the new threshold was not affected. This is expected, since children who were 14 by the time of the increase in the minimum legal age where unaffected by this law change.

Figure 7 – Labour Force Participation –1999
15 vs.16 – September 1999

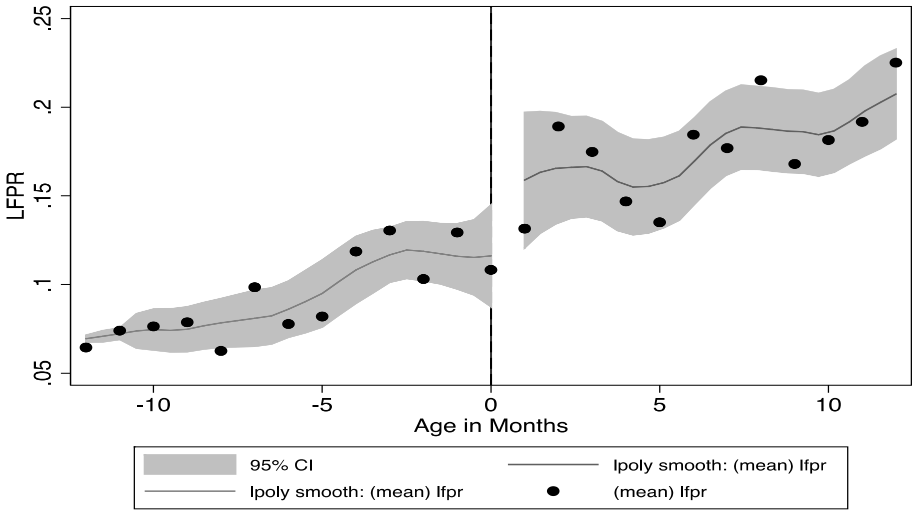


Figure 8 – Participation Rate in the Formal Labour Force –1999
15 vs.16 – September 1999

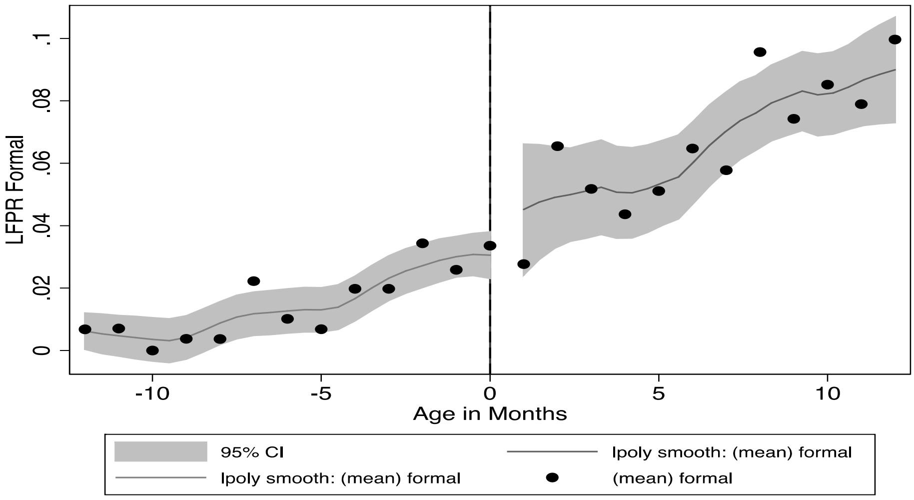
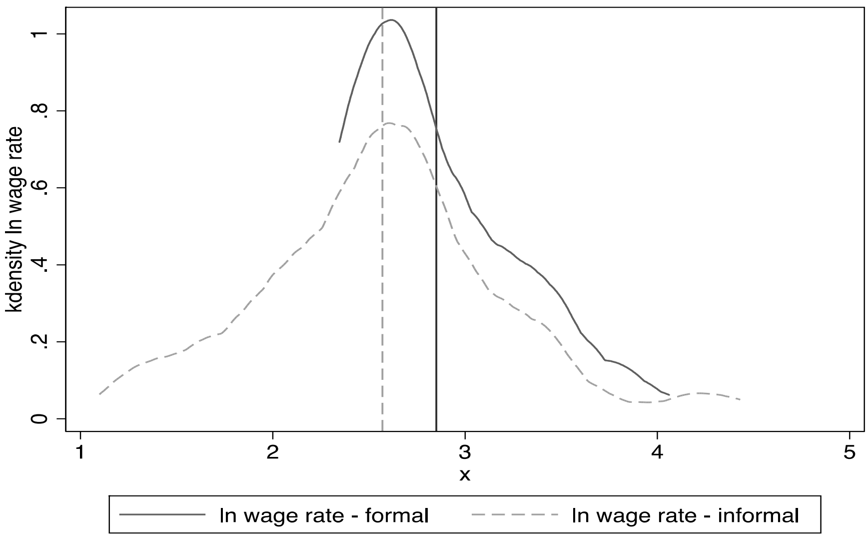


Figure A.1 in the appendix (Appendix 1, page 179) shows local linear regression lines for participation rate in the labour force for children close to age 14 in December 1997, one year before the ban. Figure A.2 (Appendix 1, page 179) presents local linear estimates for boys only. These figures can be seen as placebo checks. As can be seen, there is no indication of discontinuity around December 1997. Figures A.3 and A.4 (Appendix 1, page 180) show local linear regression lines for participation rate in the labour force and participation in the formal sector for children around age 16 in September 1998. As expected, no discontinuity is observed around age 16.

One could expect that children banned from the labour force would receive a smaller wage rate in the informal sector. This fall is represented in figure 9. There is clear indication that banned boys received a wage rate about two-thirds of that received by the comparison boys. This is an interesting result and could suggest a couple of things. On one hand, this could be simply show that participation rate is lower among the treatment group due to a substitution effect, since the wage rate in the informal sector is about 28 percent lower than in the formal sector (figure 10). On the other hand, this could show some equilibrium effect in the case in which the law was well enforced. If boys in the eligible and comparison groups are in fact similar in observed and unobserved characteristics, they should be taken as perfect substitute inputs in the labour market. As such, with the reduction in participation rate among eligible boys, the child labour supply would shift to the left, increasing wage rates of the control children. One could argue that this is less likely the case, since most children participating in the labour force were in the informal sector, and these 14 year-olds in the comparison group should not be much different from 15-year-olds also allowed to work. In other words, the fall in participation rate of boys aged 14 should not have such a large effect on the labour supply.

Figure 9 – Hourly Wage of Boys 1999

Figure 10 – Density Distributions of Log of Hourly Wage 1999
Comparison group



Note: Dashed line refers to the mean of log hourly wage of informal workers in the comparison group (2.57), whereas the solid line crosses the horizontal axis at the mean of log hourly wage of formal workers in the comparison group (2.85).

In figures 11 and 12, I look for a jump in participation rate in the formal labour force that might be attributed to the apprenticeship programme. In these figures the cut-off point is September 2002, the month the survey was collected. Figure 11 indicates that the apprenticeship program may have marginally increased participation rate in the formal labour force of children aged 14.

Figure 11 – Participation rate in the formal labour force in 2002
13 vs. 14 in Sept 2002

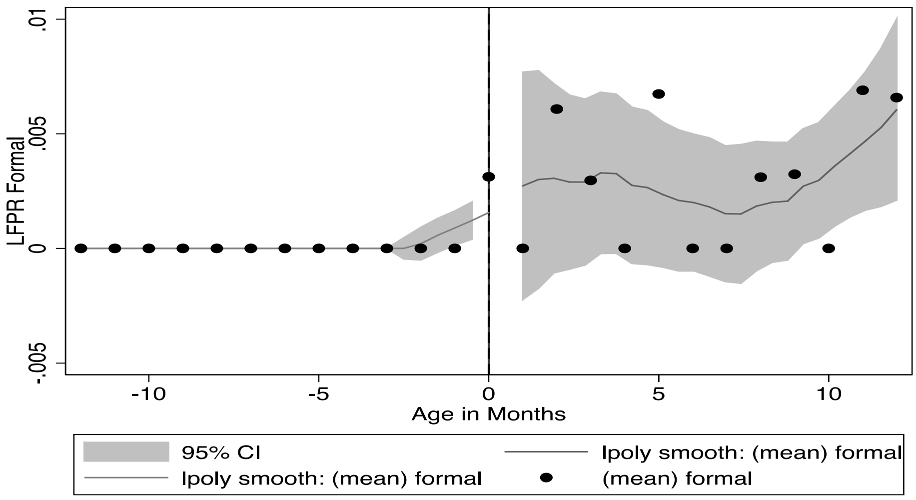
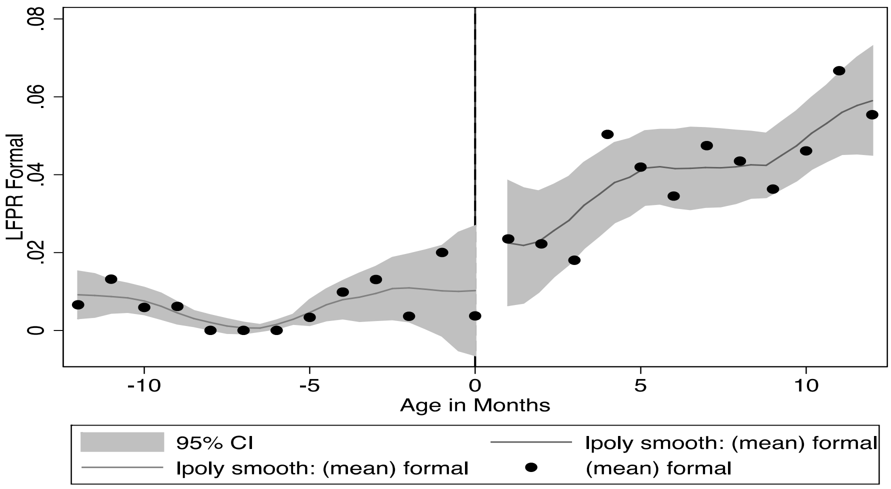


Figure 12 points to some small discontinuities in participation rate in the formal sector around age 16. The lack of a sharper discontinuity around the new threshold suggests that the apprenticeship programme might have counterbalanced the effect of the ban for 15-year-olds given the similarity in participation rate between children just under and just above age 16.

Figure 12 – Participation Rate in the Formal Labour Force –2002
15 vs. 16 in Sept 2002



As mentioned above, the validity of the RDD goes beyond the visual check. It requires a balanced sample of individuals close to the cut-off point—a necessary condition that characterises discontinuity as a (local) natural experiment (see Lee and Lemieux 2009 and Imbens and Lemieux 2008), and an imperfect control over the assignment variable. Table 4 presents the difference in means for a set of covariates for two bandwidth sizes – 16 weeks and 20 weeks. The vector of covariates includes parents' years of schooling, child's ethnicity, household size, land title ownership, and household non-labour income. All these variables are commonly used as important predictors in the labour supply literature, as they capture household socioeconomic background. The table reports the coefficient of regressions of each covariate on a constant and a dummy indicating whether the teenager was 14 years of age after December 1998. Standard errors are clustered at the forcing variable level.

Table 4 – T-test for Difference in Means – Urban Area Only
14 before Dec 1998 vs. 14 after Dec 1998

	Comparison Group		Eligible Group		T-statistic
Outcomes	Mean	SD	Mean	SD	
LFPR	0.12	0.32	0.08	0.28	(3.06)
LFPR - Formal	0.01	0.09	0.00	0.04	(2.37)
LFPR – Informal	0.11	0.31	0.08	0.27	(2.58)
Domestic work	0.72	0.45	0.70	0.46	(1.46)
School attendance	0.93	0.25	0.94	0.23	(-1.38)
Covariates					
White	0.49	0.50	0.50	0.50	(-0.24)
Father's years of schooling	5.33	4.48	5.28	4.58	(0.24)
Mother's years of schooling	5.02	4.50	5.10	4.69	(-0.44)
Father's age	37.32	20.25	36.85	20.21	(0.63)
Mother's age	32.78	20.26	31.85	20.36	(1.22)
Household size	4.73	1.59	4.65	1.57	(1.32)
Land title	0.92	0.28	0.91	0.28	(0.13)
Non-labor income	3.33	23.94	3.62	28.73	(-0.29)
Observations	*1387*		*1287*		*2674*

Source: PNAD of 1999.

The table shows the mean for the vector of outcomes and for vector covariates. The last column shows the t-statistic for the difference in mean. For all covariates, the null of equal means cannot be rejected, suggesting that the sample of individuals around the discontinuity is similar, even for a 20-week bandwidth. As consequence, I report the coefficients of the impact of the ban with 20 weeks bandwidth and use a shorter window to check robustness.[79]

Table A.1 in Appendix 1 (page 181) presents the same set of estimates for youth aged 15 and 16. The null of equal means is rejected in only one case.

Table 5 shows the balance check analysis for 2002 to check whether children aged 14 who were eligible to participate in the apprenticeship programme are similar in observed characteristics to those 13-year-olds who were a few weeks away from turning 14.

Table 5 – T-test for Difference in Means – Urban Area Only
13 vs. 14 – Sept 2002

	Comparison Group		Eligible Group		T-statistic
Outcomes	Mean	SD	Mean	SD	
LFPR	0.11	0.31	0.09	0.28	(2.24)
LFPR - Formal	0.00	0.05	0.00	0.00	(1.90)
LFPR – Informal	0.11	0.31	0.09	0.28	(2.03)
Domestic work	0.66	0.48	0.65	0.48	(0.50)
School attendance	0.96	0.21	0.96	0.20	(-0.27)
Covariates					
White	0.47	0.50	0.44	0.50	(1.77)
Father’s years of schooling	5.29	4.79	5.41	4.67	(-0.73)
Mother’s years of schooling	6.83	4.36	6.87	4.31	(-0.23)
Father’s age	33.55	20.47	34.58	19.94	(-1.44)
Mother’s age	40.92	12.04	40.23	12.16	(1.62)
Household size	4.63	1.60	4.60	1.55	(0.44)
Land title	2.16	0.54	2.17	0.59	(-0.57)
Non-labor income	3.06	19.09	4.44	26.29	(-0.47)
Observations	*1621*		*1424*		*3045*

Source: PNAD of 2002.

As before, the children on each side of the cutoff point look similar in observed characteristics. The null hypothesis of equal means is rejected in only one case at 10 percent.

For the comparison between 15- and 16-year-olds, one must bear in mind that 16-year-olds were also eligible for the programme. The objective of this analysis is to

[79] Angrist and Rokkanen (2012) develop a combination of RDD and propensity score matching techniques to extrapolate the local estimates a little farther from the cut-off point.

see whether the programme fully counterbalanced the effect of the ban. The balance check for children around age 16 is shown in table A.2 (Appendix 1, page 182).

The similarity in the covariates suggests that the two sub-samples of eligible and ineligible groups are very well balanced in observed characteristics. Figures 13 and 14 show the McCrary density test around ages 14 and 16. The test checks for perfect manipulation of the assignment variable and is therefore a key test to validate the RD design as a credible identification strategy. The rejection of the null hypothesis that the assignment variable is smooth around the threshold would cast some doubt on the validity of RDD regressions, because it would indicate that the groups on each side of the cut-off point are systematically different (Lee and Lemieux, 2010).

Because the assignment variable used in the thesis is self-reported, one could expect to observe some measurement error or data heaping if the head of the household misreported the age of children who turned 14 after the ban passed. As long as the measurement error is random and not associated with heaps in the outcome variable, the RD estimates would still be internally valid. However, if the measurement error in fact is caused by a deliberate decision of the household head in misreporting the age of the 14 year-old as an attempt to avoid complying with the ban then the RD estimates could be biased (see Barreca et al. 2015).[80] Since this is a non-parametric test that compares two local linear regressions on each side the cut-off point, it does not perform well with a discrete assignment variable (Card and Lee, 2007; McCrary, 2008). The test is thus performed with age defined in days over a 180-day interval. The figures show no statistically significant difference between density distributions around the thresholds.

[80] According to the Brazilian Bureau of Statistics, the head of the household is the one who reports to the surveyor the age of the household members. From 2005 onwards, the annual household survey began asking for individuals' birth certificate.

Figure 13 – McCrary Density Test for the Manipulation of the Assignment Variable
14 Before and After Dec 1998 – Age in days

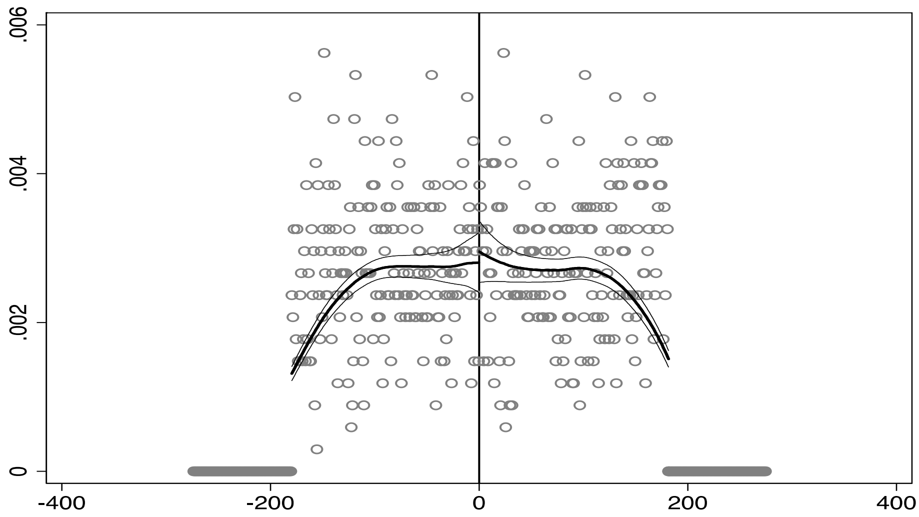
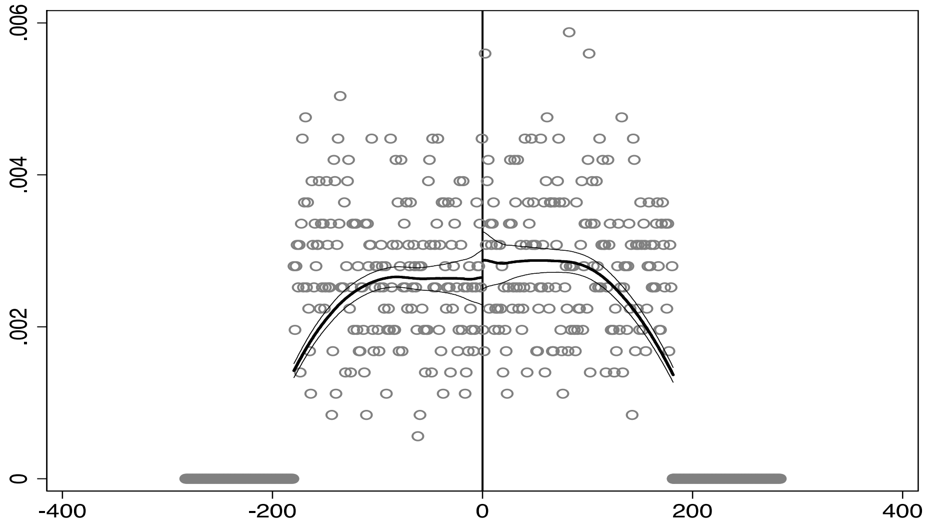


Figure 14 – McCrary Density Test for the Manipulation of the Assignment Variable
15 vs. 16 in Sept 1999 – Age in days



An untestable assumption that assures internal validity for the RDD estimates and that will be critical to guaranteeing credibility for the DD estimates is that children close enough to the cut-off points have similar distributions of unobserved characteristics. This assumption will be important for the identification strategy of the DD model, because there is no way to control for individual fixed effects once the regressions are estimated with different cohorts.

Although such a combination of approaches has been used recently (Lemieux and Milligan 2008), Lemieux and Milligan (2008) argue that the RDD and DD estimates might differ if the key the assumption of parallel trends in the DD framework is violated. The authors apply both approaches to investigate the effect of a social assistance programme focused on individuals over age 30 on labour supply. Although both approaches rendered estimates that are qualitatively similar, the magnitudes differed substantially in some cases. Lemieux and Milligan (2008) interpret this as an indication that the DD approach may provide misleading results. One could argue that the difference in point estimates could result from a misspecification of the RDD model.[81]

5 RESULTS: IMPACT OF THE LAW OF DECEMBER 1998

This section is organised as follows. The first set of results report the RDD estimates for children around age 14, the old threshold, and for children around age 16, the new threshold. The key focus is on the comparison between the cohorts of 14-year-olds given that no discontinuity in labour force participation rate is expected for children around age 16. The section is complemented with a discussion on the DD estimates. Though the main reason for reporting DD estimates is to check robustness of RDD results, the DD estimates provide a way to estimate the impact of the laws on the intensive margin of labour supply. Joining two waves of PNADs increases the number of observations (and statistical power) so that Tobit regressions can be estimated to account for censored data.

5.1 RDD Estimates

Age 13 vs. age 14

The results are reported for the pooled sample of boys and girls and then separately for boys and girls to see whether the law had heterogeneous effects.

Tables 6 and 7 show the results for a 20 weeks bandwidth, that is, 20 weeks on each side of the cut-off point.

[81] Fajnzylber et al. (2012) combine a polynomial specification with a weighting scheme to assign higher weights to observations close to the cutoff. The authors use a normal kernel function with one month of bandwidth to weight the data. Although the RDD and DD estimates differ substantially in some cases, the authors did not provide any justification for the differences.

Table 6 – Parametric ITT Estimates for the Impact of the Laws of 1998 on Extensive Margin of Labour Supply
14 before Dec 1998 vs. 14 after Dec 1998
Bandwidth of 20 weeks

Polynomial degree	Labour Force Participation			Formal			Informal			Domestic work		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
0	-0.035***	-0.065***	-0.0070	-0.0069***	-0.011***	-0.0029	-0.028***	-0.055***	-0.0042	-0.011	0.00038	-0.0036
	(-3.45)	(-3.55)	(-0.54)	(-3.02)	(-2.84)	(-1.06)	(-2.97)	(-3.05)	(-0.32)	(-0.51)	(0.011)	(-0.22)
1	-0.030	-0.086*	0.025	-0.0074	-0.0037	-0.011*	-0.023	-0.083*	0.036	0.018	0.036	0.010
	(-1.24)	(-1.93)	(0.96)	(-1.54)	(-0.50)	(-1.91)	(-1.00)	(-1.88)	(1.42)	(0.40)	(0.56)	(0.27)
2	-0.029	-0.085*	0.026	-0.0074	-0.0033	-0.011*	-0.022	-0.082*	0.037	0.021	0.046	0.010
	(-1.21)	(-1.93)	(1.01)	(-1.47)	(-0.45)	(-1.92)	(-0.97)	(-1.87)	(1.50)	(0.47)	(0.67)	(0.28)
3	-0.018	-0.032	-0.0022	-0.012**	-0.0090	-0.014	-0.0071	-0.024	0.011	0.0089	0.019	-0.0081
	(-0.58)	(-0.58)	(-0.072)	(-1.98)	(-1.05)	(-1.61)	(-0.25)	(-0.43)	(0.41)	(0.13)	(0.20)	(-0.14)
Spline linear	-0.029	-0.084*	0.026	-0.0074	-0.0032	-0.012*	-0.022	-0.081*	0.037	0.021	0.046	0.011
	(-1.18)	(-1.90)	(1.00)	(-1.44)	(-0.43)	(-1.92)	(-0.94)	(-1.85)	(1.50)	(0.48)	(0.65)	(0.29)
Spline quadratic	-0.013	-0.0073	-0.018	-0.013**	-0.011	-0.016	-0.00052	0.0023	-0.0024	-0.0058	-0.021	-0.0051
	(-0.38)	(-0.12)	(-0.52)	(-2.00)	(-1.02)	(-1.49)	(-0.016)	(0.038)	(-0.074)	(-0.085)	(-0.24)	(-0.081)
Observations	2674	1306	1368	2674	1306	1368	2674	1306	1368	2570	1230	1340

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

The estimates in table 6 point to a decrease in the participation rate of boys at about 8.5 percentage points, or 40.5 percent over the mean of the control group (21 percent). The point estimates vary with the specification of the smooth function, but most of them are qualitatively similar, except for higher order polynomials.[82] The coefficients for boys are statistically significant at the 10 percent level (and at 5 percent against a one-sided alternative) in most of the cases. It is interesting to note that labour force participation rate for boys dropped in the formal and informal sectors, but mostly in the latter.

In the case of girls, the decrease in participation rate in the formal labour force was similar for boys and girls, but the effect on girls was apparently counterbalanced by an increase in participation in the informal labour force, although none of the coefficients for participation rate in the informal sector are statistically significant. No effect is observed for participation in household chores among boys or girls.

Table 7 – Parametric ITT Estimates for the Impact of the Laws of 1998 on Schooling outcome
14 before Dec 1998 vs. 14 after Dec 1998
Bandwidth of 20 weeks

Polynomial degree	School Attendance		
	All	Boys	Girls
0	0.0095	0.012	0.0070
	(1.32)	(0.94)	(0.74)
1	0.021	0.0020	0.040**
	(1.38)	(0.067)	(2.03)
2	0.021	0.0020	0.039*
	(1.35)	(0.067)	(1.89)
3	0.013	-0.031	0.056**
	(0.60)	(-0.77)	(2.10)
Spline linear	0.021	0.0015	0.039*
	(1.34)	(0.052)	(1.88)
Spline quadratic	0.011	-0.051	0.073***
	(0.46)	(-1.21)	(3.50)
Observations	2822	1409	1413

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

[82] The sensitivity of results to high order polynomials is consistent with a point recently made by Gelman and Imbens (2014). They argue against the use of parametric estimation of LATE with high order polynomials. They use a Monte Carlo simulation to show that high order polynomials give too much weight to the tails of the outcome variable distribution, and this can bias the results. Instead, they argue for the use of local linear and local polynomials.

Table 7 indicates that the law of 1998 increased school attendance among girls. Most of the coefficients are statistically significant at standard levels and point to an increase of 4 percentage points or 4.2 percent over the mean of the control group (95 percent). Robustness check with 16 weeks bandwidth are shown in tables A.3 and A.4 (Appendix 1, page 183-184). The results are qualitatively similar, though less precisely estimated.

Age 15 vs. age 16

Tables 8 and 9 present the estimates comparing outcomes of 15- and 16-year-olds with the same bandwidth size.

Table 8 shows that, except for a couple of point estimates, the ban did not have an impact on participation rate. This is what one could expect had the ban of 1998 not affected those who turned 14 before the change in the law. In other words, children just under age 16 kept working as much as children just above the new threshold.

Table 9 indicates that boys aged 15 became more likely to attend school after the ban, but most of the coefficients are imprecise. Tables A.5 and A.6 (Appendix 1, pages 185-186) present the results with a narrower bandwidth of 16 weeks. Results are very similar to those discussed above, but less precisely estimated.

Table 8 – Parametric ITT Estimates for the Impact of the Laws of 1998 on Extensive Margin of Labour Supply
15 vs. 16
Bandwidth of 20 weeks

Polynomial degree	Labour Force Participation			Formal			Informal			Domestic work		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
0	0.039**	0.080***	0.00052	0.023***	0.039***	0.0098*	0.018	0.048*	-0.0087	0.031	0.041	-0.0013
	(2.10)	(3.05)	(0.032)	(3.74)	(3.68)	(1.75)	(1.01)	(1.88)	(-0.52)	(1.57)	(1.21)	(-0.066)
1	0.015	0.044	-0.023	0.0074	0.016	-0.0013	0.0099	0.034	-0.022	-0.0053	0.023	-0.021
	(0.48)	(0.90)	(-0.82)	(0.74)	(0.89)	(-0.12)	(0.28)	(0.66)	(-0.71)	(-0.13)	(0.38)	(-0.63)
2	0.014	0.043	-0.025	0.0072	0.016	-0.0021	0.0085	0.033	-0.023	-0.0054	0.023	-0.021
	(0.44)	(0.90)	(-0.86)	(0.73)	(0.91)	(-0.20)	(0.24)	(0.66)	(-0.72)	(-0.13)	(0.38)	(-0.62)
3	-0.011	0.052	-0.052	-0.0027	0.0047	-0.0035	-0.0069	0.054	-0.049	0.040	0.022	0.021
	(-0.28)	(0.79)	(-1.54)	(-0.26)	(0.23)	(-0.31)	(-0.15)	(0.75)	(-1.25)	(0.66)	(0.29)	(0.42)
Spline linear	0.013	0.042	-0.024	0.0074	0.017	-0.0025	0.0075	0.031	-0.022	-0.0063	0.023	-0.023
	(0.42)	(0.90)	(-0.86)	(0.75)	(0.94)	(-0.24)	(0.22)	(0.64)	(-0.71)	(-0.15)	(0.39)	(-0.65)
Spline quadratic	-0.026	0.041	-0.061*	0.00024	0.011	-0.0042	-0.025	0.038	-0.057	0.043	0.021	0.015
	(-0.63)	(0.65)	(-1.69)	(0.024)	(0.52)	(-0.37)	(-0.54)	(0.56)	(-1.36)	(0.60)	(0.24)	(0.25)
Observations	2536	1217	1319	2536	1217	1319	2536	1217	1319	2269	1018	1251

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

Table 9 – Parametric ITT Estimates for the Impact of the Laws of 1998 on Schooling outcome
15 vs. 16
Bandwidth of 20 weeks

Polynomial degree	School Attendance		
	All	Boys	Girls
0	-0.031***	-0.058***	-0.0052
	(-2.75)	(-3.53)	(-0.33)
1	0.00028	-0.024	0.026
	(0.012)	(-0.71)	(0.89)
2	0.00041	-0.026	0.029
	(0.018)	(-0.75)	(1.15)
3	-0.044	-0.064	-0.026
	(-1.53)	(-1.31)	(-0.75)
Spline linear	0.0021	-0.026	0.033
	(0.10)	(-0.74)	(1.30)
Spline quadratic	-0.037	-0.060*	-0.021
	(-1.62)	(-1.85)	(-0.52)
Observations	2837	1414	1423

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

5.2 Difference-in-Differences Estimates

Age 13 vs. age 14

Tables 10 to 12 report the DD estimates on work outcomes of children aged 14. Due to the gain in power of using two waves of the Brazilian household survey, regressions are estimated only with a 16 weeks bandwidth. The decision to use a 16 weeks bandwidth instead of 20 weeks has to do with gain in power in pooling two waves of the household survey. Thus, with DD it is possible to explore the estimates more locally with more precision than the RDD estimates. One could also see the DD estimates as a way of cleaning up the RDD estimates from any cohort effect that might be idiosyncratic of the cohorts who turned 14 by the time the law change took effect.[83]

Results shown in table 10 are qualitatively similar to the RDD estimates. There is an indication that the ban reduced participation rate in the labour force only for boys by 3.3 percentage points. The estimate is statistically significant at the 10 percent level

[83] Lemieux and Milligan (2008) interpret the differences between RDD and DD estimates as an indication that the necessary conditions to validate the DD estimates, specifically the common trend assumption, were violated.

(or at the 5 percent level against a one-sided alternative), and as with the RDD estimates the total effect is mostly explained by the decrease in participation in the informal sector. Note that the gain in power due to a larger sample size is counterbalanced by the reduction in the magnitude of the effect size that is less than half of that found with the RDD. The table also presents estimates for occupation in the formal sector. Since this outcome is conditioned on individuals participating in the labour force, the sample size is adjusted accordingly.

Table 11 shows the estimates for participation in household chores as well as weekly hours worked. Consistent with the RDD estimates, the DD regressions show no impact on household chores. The estimates for the intensive margin of labour supply suggest that the ban increased weekly hours worked in the formal labour force among boys but that the ban decreased the same among girls. The results for girls are consistent with the RDD estimates, but the increase in weekly hours of work among boys is difficult to explain, particularly because of the decrease in participation rate. The coefficient for the dummy 'eligible' in table 11 shows a pre-treatment difference in hours worked between the 'treatment' and control groups. Prior to the ban, eligible boys worked 26 fewer hours per week than boys unaffected by the ban. This might indicate that a decrease in weekly hours worked in the formal sector, as denoted by the coefficient of the time dummy (D_{99}), was stronger among the control group, because boys affected by the ban were already working fewer hours before the ban. Put differently, the DD coefficient, though more precisely estimated, might be picking up a disproportionate reduction in weekly hours worked among the control group.

Table 12 presents the estimates for school attendance. The DD estimates are small in magnitude and very imprecisely estimated. The ban seems to have reduced the participation rate of boys, and this result is very much driven by the effect on the informal sector. The effect of the ban on girls points to a reduction in the participation rate in the formal labour force, but there is no detectable impact on labour force participation in the informal sector.

Table 10 – Difference-in-Differences Estimates for the Impact of the Law of 1998 on Extensive Margin of Labour Supply

14 before Dec 1998 vs. 14 after Dec 1998

Bandwidth of 16 weeks

	Labour Force Participation Rate			Participation Rate – Formal Labour Force			Occupation in Formal Sector			Participation Rate – Informal Labour Force		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Eligible*D_{99} (*DD*)	-0.016	-0.033*	0.0020	-0.0052	-0.0026	-0.0068*	-0.017	0.049	-0.24	-0.011	-0.030*	0.0088
	(-1.64)	(-1.93)	(0.22)	(-1.29)	(-0.37)	(-1.77)	(-0.26)	(0.68)	(-1.46)	(-1.20)	(-1.91)	(1.06)
Eligible	-0.0065	-0.013	-0.00075	-0.0044*	-0.010**	0.00092	-0.077	-0.11**	-0.0026	-0.0021	-0.0028	-0.0017
	(-1.09)	(-1.25)	(-0.13)	(-1.82)	(-2.31)	(0.50)	(-1.57)	(-2.03)	(-0.021)	(-0.39)	(-0.29)	(-0.31)
D_{99} (1998=0, 1999=1)	0.037***	0.064***	0.010	0.0078**	0.0073	0.0080**	0.0037	-0.061	0.23*	0.029***	0.057***	0.0022
	(5.11)	(4.91)	(1.57)	(2.38)	(1.24)	(2.57)	(0.079)	(-1.18)	(1.98)	(4.42)	(4.76)	(0.38)
Controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Dummies for states?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Constant	0.11**	0.21***	0.064	-0.00027	0.0065	-0.0016	-0.021	0.014	-0.079	0.11***	0.20***	0.065*
	(2.55)	(2.67)	(1.59)	(-0.023)	(0.29)	(-0.23)	(-0.15)	(0.087)	(-0.32)	(2.62)	(2.66)	(1.66)
Observations	9368	4634	4734	9368	4634	4734	545	428	117	9368	4634	4734
Adjusted R2	0.033	0.021	0.005	0.009	0.009	0.001	0.045	0.063	-0.032	0.027	0.017	0.004

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 the Intensive Margin of Labour Supply and Household Chores

14 before Dec 1998 vs. 14 after Dec 1998

Bandwidth of 16 weeks

	Household Chores			Weekly Hours Worked - Formal			Weekly Hours Worked - Informal		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Eligible*D_{99} (*DD*)	0.0043	0.014	0.0081	-1.34	12.3***	-43.7***	-0.13	-0.20	0.52
	(0.23)	(0.46)	(0.38)	(-0.54)	(4.47)	(-9.03)	(-0.035)	(-0.049)	(0.065)
Eligible	-0.0080	0.0048	-0.023	-19.4***	-26.1***	2.53	4.47	4.49	6.30
	(-0.61)	(0.23)	(-1.46)	(-8.36)	(-10.2)	(0.54)	(1.58)	(1.35)	(1.07)
D_{99} (1998=0, 1999=1)	0.024*	0.0074	0.029*	1.93	-12.6***	47.9***	3.21	4.61	-2.75
	(1.85)	(0.33)	(1.91)	(0.88)	(-5.23)	(11.0)	(1.27)	(1.63)	(-0.51)
Controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Dummies for states?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Constant	0.82***	0.43***	0.88***	-345.5***	-330.9***	-548.9***	35.1***	36.8***	32.3*
	(10.7)	(3.33)	(9.84)	(-131.4)	(-116.1)	(-108.7)	(3.56)	(3.06)	(1.85)
Sigma				52.1***	49.9***	45.1***	20.0***	19.8***	18.0***
				(38.3)	(34.3)	(19.7)	(28.9)	(26.0)	(12.8)
Observations	8474	4020	4454	545	428	117	545	428	117
Adjusted R2/Pseudo-R2	0.16	0.05	0.02	0.05	0.07	0.10	0.01	0.01	0.04

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 Schooling Outcome
14 before Dec 1998 vs. 14 after Dec 1998
Bandwidth of 16 weeks

	School Attendance		
	All	Boys	Girls
Eligible*D_{99} (*DD*)	-0.0042	-0.0090	0.00072
	(-0.39)	(-0.57)	(0.049)
Eligible	0.017**	0.015	0.019**
	(2.45)	(1.40)	(2.04)
D_{99} (1998=0, 1999=1)	-0.023***	-0.016	-0.031***
	(-2.90)	(-1.38)	(-2.81)
Controls?	*Yes*	*Yes*	*Yes*
Dummies for states?	*Yes*	*Yes*	*Yes*
Constant	0.87***	0.82***	0.91***
	(18.5)	(10.9)	(16.0)
Observations	9368	4634	4734
Adjusted R2	0.011	0.009	0.012

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

Age 15 vs. age 16

The DD estimates of the impact of the ban on 15-year-olds are shown in tables A.7 to A.9 (Appendix 1, pages 187-189). The treatment dummy is defined as 1 for individuals above the threshold—age 16 on the survey date—and 0 otherwise. Thus, the impact of the ban of December 1998 on 15-year-olds will have the opposite sign.

None of the estimates for participation rate are statistically significant. There is an indication that 15-year-old boys are more likely to do household chores, but the estimate is significant only against a one-sided alternative. Table A.8 suggests, on the other hand, that 15-year-old girls worked fewer hours per week in formal sector due to the ban. This result would be consistent with the shift of some girls to the apprenticeship programme, which is a part-time programme. But this could also be due to the fact that school is no longer mandatory for teenagers aged 16. This is also consistent with the results for school attendance. A premature dropout of school at age 16 would explain the lower school attendance as shown in table A.9.

6 RESULTS: IMPACT OF THE APPRENTICESHIP PROGRAMME

This section presents the results of the causal effects of the apprenticeship programme. As in the previous section, the discussion starts with the RDD estimates and is followed by the DD regressions. As before, DD estimates are provided for 16 weeks bandwidth only. The low take-up of the programme suggests a small effect on participation rate in the formal labour market and, consequently, on time allocation of children aged 14.

6.1 RDD Estimates

Age 13 vs. age 14

Tables 13 and 14 show estimates of the effect of the apprenticeship programme approved in December 2000 for a 20 weeks bandwidth.

Most of the estimates point to a reduction in participation rate in the labour force between 3.7 percentage points and 7 percentage points; however, the result is almost fully explained by the decrease in informality among girls. There is an indication that the programme decreased the participation rate of girls in the informal sector by about 5.5 percentage points to 9.5 percentage points. The effect participation rate in formal labour is almost indistinguishable from zero in magnitude.

Given that such a decrease is not followed by a sharp increase in participation in the formal sector, it is worth looking at the occupations of those already in the labour force. The impact on occupation in formal paid work is positive and relatively high in most of the regressions for the pooled sample of boys and girls. This suggests that those who did not drop out of the labour force became more likely to take up the apprenticeship programme. In other words, the programme seems to have changed the composition of 14-year-olds from informal to formal occupations.

Robustness checks with a bandwidth of 16 weeks are presented in tables A.10 and A.11 in Appendix 1 (pages 190-191). Results are qualitatively the same. This is an interesting result. The programme might not have nudged children to enter the labour force, but seems to have incentivised those already in the labour force to shift to the formal sector. No effect on household chores was detected.

Table 13 – Parametric ITT Estimates for the Impact of the Apprenticeship Programme on the Extensive Margin of Labour Supply
13 vs. 14
Bandwidth of 20 weeks

Polynomial degree	Labour Force Participation			Participation Rate in Formal Sector			Formal Paid Work – occupation			Informal			Domestic Work		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
0	0.024*	0.036**	0.012	0.0025**	0.0026	0.0024	0.053**	0.047	0.061	0.021*	0.034**	0.0095	0.0089	-0.013	0.017
	(1.88)	(2.23)	(0.77)	(2.18)	(1.45)	(1.47)	(2.13)	(1.56)	(1.43)	(1.67)	(2.10)	(0.60)	(0.52)	(-0.54)	(0.94)
1	-0.037**	-0.020	-0.052**	0.0039*	0.0043	0.0035	0.10*	0.083	0.14	-0.040**	-0.024	-0.055**	0.0071	-0.0091	0.011
	(-2.08)	(-0.72)	(-2.10)	(1.65)	(1.01)	(1.35)	(1.68)	(1.06)	(1.43)	(-2.33)	(-0.88)	(-2.22)	(0.23)	(-0.20)	(0.41)
2	-0.038**	-0.022	-0.053**	0.0038*	0.0042	0.0034	0.098*	0.077	0.15	-0.042**	-0.026	-0.056**	0.0019	-0.012	0.0059
	(-2.24)	(-0.80)	(-2.21)	(1.65)	(1.03)	(1.30)	(1.66)	(1.08)	(1.42)	(-2.49)	(-0.95)	(-2.34)	(0.063)	(-0.27)	(0.21)
3	-0.070***	-0.041	-0.096***	0.0016	0.0042	-0.00083	0.058	0.064	0.060	-0.072***	-0.045	-0.095***	0.023	-0.016	0.043
	(-2.94)	(-1.24)	(-3.12)	(0.53)	(0.66)	(-0.45)	(0.79)	(0.63)	(0.80)	(-3.08)	(-1.37)	(-3.06)	(0.57)	(-0.26)	(1.11)
Spline linear	-0.038**	-0.022	-0.053**	0.0038*	0.0042	0.0034	0.099*	0.077	0.15	-0.042**	-0.026	-0.056**	0.00057	-0.013	0.0052
	(-2.22)	(-0.81)	(-2.20)	(1.68)	(1.04)	(1.34)	(1.67)	(1.11)	(1.39)	(-2.46)	(-0.95)	(-2.32)	(0.018)	(-0.29)	(0.17)
Spline quadratic	-0.065**	-0.030	-0.095***	0.0014	0.0047	-0.0013	0.047	0.066	-0.022	-0.066***	-0.034	-0.094**	0.042	-0.0034	0.064*
	(-2.49)	(-0.87)	(-2.63)	(0.49)	(0.72)	(-0.94)	(0.66)	(0.73)	(-0.31)	(-2.58)	(-0.99)	(-2.57)	(1.07)	(-0.050)	(1.85)
Observations	3045	1503	1542	3045	1503	1542	127	71	56	3045	1503	1542	2918	1431	1487

Source: PNAD 2002.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table 14 shows the estimates for school attendance. Consistent with the conditionality embedded in the programme, school attendance seems to have increased with the programme, but only for boys.

Table 14 – Parametric ITT Estimates for the Impact of the Apprenticeship Programme on Schooling Outcome
13 vs. 14
Bandwidth of 20 weeks

Polynomial degree	School Attendance		
	All	Boys	Girls
0	-0.0019	0.0075	-0.012
	(-0.25)	(0.69)	(-1.02)
1	0.029**	0.052**	0.0075
	(2.01)	(2.38)	(0.31)
2	0.029**	0.051**	0.0088
	(2.01)	(2.32)	(0.37)
3	0.039**	0.049*	0.031
	(2.23)	(1.78)	(1.13)
Spline linear	0.030**	0.051**	0.0093
	(2.04)	(2.32)	(0.39)
Spline quadratic	0.049***	0.052*	0.045
	(2.63)	(1.89)	(1.64)
Observations	3241	1632	1609

Source: PNAD 2002.
Note: Clustered T-statistics in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Age 15 vs. age 16

The objective of this exercise is to check whether the apprenticeship programme annulated the effect of the ban among children aged 15. Table 15 shows that the coefficients on participation rate in the formal labour force are positive and statistically significant only for 15-year-old girls. They indicate that girls became 2.3 percentage points more likely to participate in the formal labour force, and for girls in the labour force already, the estimates indicate that they became 18 percentage points more likely to work in a formal occupation. For 15-year-old boys, the results suggest that the programme fully counterbalanced the effect of the ban.

Table 15 – Parametric ITT Estimates for the Impact of the Apprenticeship Programme on the Extensive Margin of Labour Supply
15 vs. 16
Bandwidth of 20 weeks

Polynomial degree	Labour Force Participation			Participation Rate in Formal Sector			Formal Paid Work – occupation			Informal			Domestic Work		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
0	0.043***	0.054***	0.030	0.013**	0.021**	0.0051	0.072**	0.085**	0.048	0.033**	0.038*	0.026	-0.016	-0.021	-0.017
	(2.68)	(2.66)	(1.20)	(1.99)	(2.11)	(0.99)	(1.96)	(1.96)	(1.08)	(2.01)	(1.76)	(1.03)	(-0.89)	(-0.72)	(-0.83)
1	0.027	0.020	0.050	-0.015	-0.0046	-0.023***	-0.076	-0.019	-0.17***	0.040	0.024	0.072	-0.0092	-0.031	-0.033
	(0.91)	(0.52)	(0.95)	(-1.31)	(-0.26)	(-3.13)	(-1.20)	(-0.23)	(-2.79)	(1.37)	(0.55)	(1.36)	(-0.29)	(-0.61)	(-0.76)
2	0.026	0.022	0.046	-0.013	-0.0026	-0.023***	-0.068	-0.0026	-0.18***	0.038	0.024	0.066	-0.0070	-0.029	-0.032
	(0.89)	(0.54)	(0.88)	(-1.37)	(-0.17)	(-3.12)	(-1.19)	(-0.037)	(-2.85)	(1.36)	(0.54)	(1.29)	(-0.23)	(-0.60)	(-0.74)
3	0.025	0.030	0.032	-0.0011	0.014	-0.014*	-0.017	0.027	-0.087	0.026	0.019	0.045	0.0042	0.021	-0.050
	(0.68)	(0.62)	(0.48)	(-0.098)	(0.69)	(-1.79)	(-0.26)	(0.30)	(-1.42)	(0.75)	(0.35)	(0.67)	(0.10)	(0.36)	(-0.80)
Spline linear	0.025	0.021	0.045	-0.013	-0.0018	-0.023***	-0.067	0.0016	-0.18***	0.036	0.022	0.065	-0.0060	-0.027	-0.031
	(0.87)	(0.51)	(0.88)	(-1.34)	(-0.12)	(-3.08)	(-1.16)	(0.023)	(-2.86)	(1.35)	(0.52)	(1.29)	(-0.20)	(-0.56)	(-0.73)
Spline quadratic	0.020	0.011	0.035	-0.0046	0.0081	-0.016**	-0.039	0.0014	-0.087	0.024	0.0040	0.050	0.0036	0.036	-0.043
	(0.54)	(0.27)	(0.48)	(-0.39)	(0.41)	(-1.99)	(-0.58)	(0.017)	(-1.27)	(0.67)	(0.089)	(0.69)	(0.083)	(0.62)	(-0.63)
Observations	2788	1362	1426	2788	1362	1426	443	283	160	2788	1362	1426	2347	1081	1266

Source: PNAD 2002.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

The results for school attendance are a bit puzzling, particularly because school was no longer mandatory for children aged 16. Table 16 indicates that teenagers just under age 16 are about 4.5 percent less likely to attend school. To conciliate work with school, apprentices might have attended school less often. Tables A.12 and A.13 (Appendix 1, page 192-193) show the results with the bandwidth of 16 weeks to check robustness.[84]

Table 16 – Parametric ITT Estimates for the Impact of the Apprenticeship Programme on Schooling Outcome
15 vs. 16
Bandwidth of 20 weeks

Polynomial degree	School Attendance		
	All	Boys	Girls
0	-0.031**	-0.030*	-0.032*
	(-2.54)	(-1.91)	(-1.86)
1	0.036**	0.047*	0.022
	(2.28)	(1.87)	(0.75)
2	0.035**	0.048**	0.020
	(2.26)	(1.96)	(0.67)
3	0.038**	0.043	0.030
	(2.01)	(1.42)	(0.73)
Spline linear	0.036**	0.049**	0.021
	(2.32)	(1.98)	(0.68)
Spline quadratic	0.041**	0.038	0.041
	(2.04)	(1.04)	(1.02)
Observations	3300	1659	1641

Source: PNAD 2002.
Note: Clustered T-statistics in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

[84] Estimates presented in table A.12 suggest that one cannot reject the null hypothesis that the apprenticeship programme fully mitigated the impact of the ban among boys. For girls, it seems that the programme resulted in a higher proportion of 15-year-olds participating in the formal labour force than girls aged 16. Consistently, girls aged 15 are also more likely to work in a formal occupation. Table A.13 indicates that the programme may have reduced school attendance among 15-year-old boys.

6.2 Difference-in-Differences Estimates

Age 13 vs. age 14

To increase power, DD regressions use two waves of the household survey and are estimated with a 16 weeks bandwidth. Results are presented in tables 17 to 19. Almost no estimates are statistically significant. There is an indication that girls ended up working few more hours in the formal sector and fewer hours in the informal sector. There is a puzzling result for boys that is difficult to reconcile with the estimates for the extensive margin of labour supply, as there is no indication of an increase in formalisation among the employed. Estimates at the intensive margin are very noisy and imprecise given the relatively small proportion of 13- and 14-year-olds in the labour force.

Age 15 vs. age 16

Tables A.14 to A.16 in Appendix 1 (pages 194-196) bring the estimates for the apprenticeship programme for children under age 16. None of the estimates are statistically significant. It seems that the apprenticeship programme of 2000 fully counterbalanced the impact of the ban on participation rate in the formal labour force amongst those just under age 16.

Table 17 – Difference-in-Differences Estimates – Apprenticeship Programme of 2000 – Extensive Margin of Labour Supply
13 vs. 14
Bandwidth of 16 weeks

	LFPR			LFPR - Formal			Formal Paid Work - Occupation			LFPR - Informal		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
T*Year	-0.0014	-0.0023	-0.00038	-0.0062	-0.0057	-0.0073	-0.0022	0.035	-0.050	0.0049	0.0033	0.0069
	(-0.14)	(-0.15)	(-0.030)	(-1.43)	(-0.81)	(-1.34)	(-0.053)	(0.62)	(-0.58)	(0.53)	(0.24)	(0.58)
T (13=0; 14=1)	-0.0027	0.0020	-0.0058	-0.00016	0.0013*	-0.00087	-0.038	-0.072*	-0.0062	-0.0025	0.00072	-0.0049
	(-0.70)	(0.29)	(-1.56)	(-0.45)	(1.69)	(-1.62)	(-1.34)	(-1.86)	(-0.079)	(-0.66)	(0.11)	(-1.34)
Year (1999=0; 2002=1)	0.11***	0.12***	0.11***	0.029***	0.036***	0.024***	0.19***	0.19***	0.19***	0.083***	0.082***	0.085***
	(16.0)	(10.8)	(11.9)	(9.03)	(6.95)	(5.79)	(6.90)	(5.11)	(3.74)	(12.9)	(8.29)	(10.2)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	9883	4866	5017	9883	4866	5017	785	448	337	9883	4866	5017
Adjusted R2	0.09	0.08	0.09	0.05	0.05	0.05	0.07	0.07	0.09	0.05	0.04	0.05

Source: PNADs 1999 and 2002.
Note: Robust T-statistics in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The controls include dummy variables for gender (male), ethnicity (white), head years of schooling, age and gender (=1 if male), dummy for states and metropolitan area.

Table 18 – Difference-in-Differences Estimates – Apprenticeship Programme of 2000 – Household Chores and Intensive Margin of Labour Supply
13 vs. 14
Bandwidth of 16 weeks

	Household Chores			Weekly Hours Worked – Formal			Weekly Hours Worked – Informal		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
T*Year	-0.0056	-0.034	0.021	2.84	-10.01***	7.46**	-1.85	2.75	-12.70*
	(-0.29)	(-1.12)	(0.90)	(1.35)	(-3.72)	(2.34)	(-0.52)	(0.63)	(-1.88)
T (13=0; 14=1)	0.017	0.029	0.0057	4.76**	18.17***	0.19	1.13	-2.70	10.96*
	(1.24)	(1.39)	(0.34)	(2.26)	(6.69)	(0.06)	(0.36)	(-0.73)	(1.71)
Year (1999=0; 2002=1)	0.0028	-0.012	0.016	299.98***	303.05***	279.23***	-1.64	-5.40*	7.20
	(0.20)	(-0.55)	(0.92)	(133.92)	(107.99)	(82.38)	(-0.66)	(-1.69)	(1.45)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Sigma				47.9***	48.6***	44.4***	28.5***	28.0***	28.0***
				(46.4)	(36.4)	(27.3)	(50.3)	(39.8)	(30.2)
Observations	8576	4165	4411	785	448	337	785	448	337
Adjusted R2/Pseudo-R2	0.15	0.04	0.02						

Source: PNADs 1999 and 2002.
Note: Robust T-statistics in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The controls include dummy variables for gender (male), ethnicity (white), head years of schooling, age and gender (=1 if male), dummy for states and metropolitan area.

Table 19 – Difference-in-Differences Estimates – Apprenticeship Programme of 2000 – Schooling outcome
13 vs. 14
Bandwidth of 16 weeks

	School Attendance		
	All	Boys	Girls
T*Year	0.0022	0.0045	-0.00033
	(0.22)	(0.33)	(-0.022)
T (13=0; 14=1)	-0.0019	-0.000019	-0.0039
	(-0.35)	(-0.0027)	(-0.47)
Year (1999=0; 2002=1)	-0.067***	-0.067***	-0.066***
	(-9.34)	(-6.76)	(-6.37)
Controls?	*Yes*	*Yes*	*Yes*
Observations	9883	4866	5017
Adjusted R2	0.08	0.08	0.08

Source: PNADs 1999 and 2002.
Note: Robust T-statistics in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The controls include dummy variables for gender (male), ethnicity (white), head years of schooling, age and gender (=1 if male), dummy for states and metropolitan area.

6.3 Difference-in-Differences Estimates for the Composite Effect of the Two Laws

This section turns to the net effect of both laws. Under the hypothesis that the apprenticeship programme constitutes a weak incentive for children to participate in the formal labour force, compared estimates can be seen as the lower bound estimates for the ban. The estimates use 1998 as baseline and 2002 as endline with a 16 weeks bandwidth.[85]

Age 13 vs. age 14

According to the estimates presented in table 20, the ban dominated the effect of the apprenticeship programme as the coefficients for participation in the formal labour force and for formal occupations are negative and statistically significant. In fact, the magnitude of the composite effect on the participation rate of boys is almost identical to the DD estimates of the 1998 ban. The composite effect of the laws on participation rate in the labour force is also negative, although statistically insignificant for girls. As a consequence of the two laws, there was a complete reallocation of children’s time.

[85] Instead of using the PNAD of 2001 I use 2002, because in 2001 the proportion of 14-year-olds working in the formal sector was practically zero.

Age 15 vs. age 16

The composite effect of the two laws is also estimated to compare outcomes of youth around age 16. According to the results shown in table A.17 in Appendix 1 (page 197), the composite effect of the laws was nil, as none of the coefficients are statistically significant. The coefficients for the extensive margin of labour supply approach zero.

Table 20 – Difference-in-Differences Estimates – Composite Effect of the Laws of 1998 and 2000 – Extensive Margin of Labour Supply
13 vs. 14
Bandwidth of 16 weeks

	LFPR			LFPR - Formal			Formal Paid Work – Occupation			LFPR - Informal		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
T*Year	-0.028**	-0.033*	-0.021	-0.018***	-0.023**	-0.014*	-0.12*	-0.12*	-0.20	-0.0097	-0.010	-0.0071
	(-2.39)	(-1.80)	(-1.47)	(-3.11)	(-2.54)	(-1.89)	(-1.81)	(-1.67)	(-1.00)	(-0.92)	(-0.60)	(-0.56)
T (13=0; 14=1)	0.021**	0.021	0.020	0.018***	0.021**	0.014**	0.082***	0.076*	0.092**	0.0033	0.000064	0.0057
	(2.08)	(1.36)	(1.49)	(3.18)	(2.41)	(1.98)	(2.75)	(1.85)	(2.06)	(0.37)	(0.0047)	(0.48)
Year (1998=0; 2002=1)	-0.13***	-0.13***	-0.12***	-0.035***	-0.038***	-0.031***	-0.14**	-0.14**	-0.061	-0.092***	-0.096***	-0.090***
	(-14.3)	(-9.34)	(-11.2)	(-8.51)	(-6.12)	(-5.64)	(-2.54)	(-2.50)	(-0.37)	(-11.3)	(-7.21)	(-9.40)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	8680	4261	4419	8680	4261	4419	914	527	387	8680	4261	4419
Adjusted R2	0.07	0.07	0.06	0.03	0.03	0.02	0.09	0.08	0.10	0.04	0.04	0.04

Source: PNADs 1998 and 2002.
Note: Robust T-statistics in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The controls include dummy variables for gender (male), ethnicity (white), head years of schooling, age and gender (=1 if male), dummy for states and metropolitan area.

7 CONCLUSION

This chapter contributes to the literature an analysis of the impact of child labour legislation by examining the short-run impacts of a Brazilian Constitutional Amendment of December 1998 and the apprenticeship programme formally conceptualised in December 2000.

RDD and DD regressions provide estimates of the causal impacts of the interventions on measures of extensive and intensive margins of child labour supply and schooling outcome. The results suggest that the increase in the minimum legal age of entry into the labour market of December 1998 affected boys and girls differently. Boys were more likely to drop out of the labour force, whereas girls were more likely to attend school.

DD estimates show that the ban also affected hours worked, and the results are consistent with the impact on participation rate. Boys ended up working fewer hours per week in the informal sector and girls worked more hours per week in the same sector. Although RDD estimates find that the ban affected school attendance, DD regressions do not show the same results. One could argue that the DD estimates might be confounded in cases in which the cohorts are not comparable and therefore might follow different trends over the years. The estimates for children just under age 16 were similar but relatively weaker, particularly when attention is turned to DD results.

With regard to the apprenticeship programme, the estimates indicate positive but very small effects on participation rate in the formal labour force among children aged 14. Most of the estimates are not statistically significant. There is an indication that 14-year-old boys and girls are more likely to attend school. The comparison between children around age 16 shows that the programme resulted in a higher participation rate in the formal labour force, in particular for 15-year-old girls. The estimates also point to a lower probability of boys under age 16 attending school.

An interesting finding related to the apprenticeship programme is that it made the minimum legal age an unbinding constraint for youth aged 15, as there is no indication of a statistically significant jump in the participation rate in the formal labour force around age 16.

These findings suggest that policymakers should have a broader perspective when they pass a law. The consequences can be difficult to predict, since laws are likely to affect the time allocation of children and potentially other household members and have heterogeneous effects by gender and socioeconomic background.

CHAPTER 2: LONG-TERM EFFECTS OF CHILD LABOUR BANS ON ADULT OUTCOMES: EVIDENCE FROM BRAZIL

1 INTRODUCTION

It is a plausible assumption that most policy makers are shortsighted in that they might not take into consideration long-term consequences of their decisions. When changes in the 'rules of the game' are made indiscriminately, policy makers may not care if the changes can affect individuals differently, particularly in cases in which the effectiveness of the rules depend on the individual's background. The purpose of this chapter is to assess the long-term consequences of a child labour ban on labour market and schooling outcomes of adult males.

In December 1998, Brazil passed a Constitutional Amendment increasing the minimum legal age of entry into the labour market from 14 to 16. The change in the minimum working age gave rise to a natural experiment, as an individual's eligibility to participate in the formal labour force depends on his or her date of birth.

Chapter one looks at the short-run effects of the law on urban children and finds that the law affected mostly 14-year-old boys, resulting in a reduction in participation rates in the formal and informal sectors.[86]

This chapter uses the law of 1998 to investigate the long-term effects of postponing entrance into the formal labour force by up to two years (from 14 to 16). The research question can be altered to also investigate the effect of early exposure to the labour market on long-term outcomes. This question parallels the literature on the impact of youth employment on an individual's long-run outcomes. Most studies that use date of birth to estimate the long-term effects of a law or intervention focused on the impact of early school enrolment[87]. The literature outlines the

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[86] Chapter one also covers the impact of an apprenticeship programme. Due to the small take up rate into the programme and the small effects discussed in chapter one, I opt to focus on the long-term effects of the ban only.

[87] Angrist and Krueger (1991) were the first to use date of birth to identify eligible and ineligible groups for a treatment. After Angrist and Krueger (1991), many other authors have used date of birth as an instrumental variable. See, for instance, Oreopoulos (2006a, 2006b), Dobkin and Ferreira (2010), Bedard and Dhuey (2011), and Black, Devereux, and Salvanes (2011). Bound et al. (1995) and Bound and Jaeger (2000) show that quarter of birth can be a weak instrument, but more recently Buckles and Hungerman (2013) cast doubt on the validity of quarter of birth as instrumental variables, at least for the US, as mothers who give birth during the winter and summer seem to have very different socioeconomic backgrounds. This does not seem to be a problem in the present chapter, as suggested by a placebo test that compares two other cohorts.

educational channel as the main mechanism linking date of birth to labour market outcomes. Few empirical papers provide causal estimates for long-term effects of youth employment (or child labour) and outline the potential experience in the labour market as the plausible mechanism through which such laws can affect individuals' outcomes. This chapter helps fill this gap in the empirical literature.[88]

To assess whether the law affected individuals with different socioeconomic backgrounds differently, the cohort affected by the ban is split into groups of white and non-whites males. Skin colour (or race) is used, because it correlates well with several socioeconomic indicators (including income poverty), as is discussed below, and is exogenous.[89] Thus, we compare long-term outcomes of white males affected and unaffected and non-white males affected and unaffected by the ban. This chapter draws on theoretical contributions of Ranjan (1999), Baland and Robinson (2000), and Horowitz and Wang (2004) to hypothesise the long-run effects for each group of males.

The research question addressed in this chapter has several policy implications: (1) it informs policy makers of the potential long-run effects of across the board changes in child labour legislation; (2) it reveals there are returns to an earlier entrance into to the labour force; (3) it shows the returns to experience depend on the individual's socioeconomic background; and (4) it sheds light on long run unintended consequences of such decisions and signals whether this type of policy should be accompanied by compensating policies for those to whom it is more likely cause harm.

Common sense suggests that early exposure to the labour market is likely harmful. In fact, child labour bans have been justified on theoretical grounds (Baland

[88] There is plenty of evidence of the impact of vocational training on youth outcomes. The question addressed in this chapter is different, as it aims to discover the impact of hindering the labour market participation of 14-year-olds for up to two years.
[89] The literature on returns to education has shown heterogeneous effects due to ethnicity as well (see Angrist and Krueger (1991) for the US and Stefani and Biderman (2006) for Brazil). An alternative way of estimating heterogeneous effects is to split the sample according to household income per capita, but this would have at least two implications for the empirical exercise. First, splitting the sample into quartiles, for instance, would reduce the sample in such a way that the first stage regressions would be very difficult to estimate. As will be discussed below, first stage regressions are reported using a household survey from 1999, and the sample size with 3 and 6 months bandwidth is relatively small. Second, using household income per capita could result in biased estimates, because it is very likely to affect time allocation of household members. To circumvent the issue of low power, quantile treatment effects are provided instead. The advantage of quantile regression in the present case is that it provides estimates for the impact of the intervention in different quantiles of the earnings distribution. However, with quantile regressions we are unable to have a distribution of average treatment effects. For a discussion, see Abbring and Heckman (2007).

and Robinson, 2000; Dessy and Knowles, 2008), although some also argue that depending on the context of the household, a ban can actually backfire.[90] The long-term consequences of banning individuals from entering the formal labour force at age 14 are ultimately an empirical question.

Emerson and Souza (2011) show that child labour harms individuals' outcomes in adult life. They use the Brazilian household survey PNAD of 1996 to show that the wage earned by the cohort of adults who entered the labour market earlier in their youth is lower compared to those who entered later. Using the number of schools and teachers per 1,000 children in the state of their birth as instruments for participation in the labour market and school attendance, they show that child labour has a short-run negative effect with lower investment in human capital and a long-run negative effect with lower (adult) earnings. However, their findings suggest that the negative effects vanish around age 30.

Lee and Orazem (2010) borrow Emerson and Souza's (2011)[91] identification strategy to estimate the long-run effects of child labour on the health outcomes of adult Brazilians using PNAD 1998.[92] The estimates suggest that a simultaneous effect of an early entrance into the labour force and premature school dropout resulted in higher probability of back problems, arthritis, and reduced stamina. Despite using an instrumental variable (IV) strategy, the authors are incapable of disentangling the effects of child labour and more time spent in school on adult health outcomes.

Beegle et al. (2009) use an IV approach to investigate the medium-term consequences of child labour on schooling, labour market, and health outcomes in rural Vietnam. They use two waves of a panel data collected in 1992-93 and 1997-98 and rice price and community shocks as instruments to identify the causal impact of child labour in individual outcomes five years later. They consider the sample of individuals aged 8 to 13 as the baseline. Their findings suggest that child labour had a negative effect on school attendance and educational attainment, but a positive effect on labour market outcomes such as employability in paid work and earnings.

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[90] Basu and Van (1998) and Basu (2005) theorize that child labour bans can backfire. Theoretical models of Ranjan (1999) and Dessy and Knowles (2008) also suggest that a child labour ban is more likely to be binding among the not-so-poor and more likely to harm the poorest.
[91] In fact, Lee and Orazem (2010) refer to Emerson and Souza's (2011) working paper.
[92] PNAD 1998 has a special supplement on health outcomes.

They found no impact on health outcomes. Based on these mixed results, Beegle et al. (2009) argue that for some individuals the returns to experience seem to overcome the returns to education, at least in the medium term in rural Vietnam. These results help explain why child labour exists and cast doubt on the hypothesis that parents are myopic or that children who enter the labour force relatively early do so due to credit constraints or lack of information on the returns to education. As discussed later, the results for the cohort of non-white males follow similar lines.

This chapter uses RDD to investigate the impact of the ban of December 1998 on the following adult outcomes: hourly wage (in natural log), likelihood of being employed, likelihood of being employed in the formal sector, and likelihood of either holding or pursuing a college degree. Cohorts of individuals born in the first half of 1985—age 14 in the first half of 1999—are compared to the cohorts of males born in the second half of 1984 and who were 14 in the second half of 1998. Estimates are provided for a 6-month bandwidth on each side of cut-off point (the date of the law). To check robustness, estimates are also provided with controls and a bandwidth of three months.

Unconditional quantile treatment effects (QTE) are estimated to shed light on the distributive impacts of the change in the law. The main results show that the ban had long-lasting effects on the groups of white and non-white males, contributing to increased wage differentials between these two groups.

There is some indication that the affected cohort of white males benefited from higher wages, higher probability of being employed in a highly skilled occupation, and higher probability of holding a college degree. For non-white males, the results suggest the opposite—that is, the ban implied lower wages for non-white males and lower probability of being employed and having a formal occupation. These results are consistent with the theoretical predictions of Ranjan (1999), Dessy and Knowles (2008), and Horowitz and Wang (2004).

Unconditional quantile treatment effects point to distributive effects among white and non-white males. Under rank preserving assumption, it could be argued that the ban harmed non-white males but benefited white males at the lower end of the hourly wage distribution—a result consistent with Baland and Robinson’s (2000) theoretical predictions.

Two points are important to emphasise. First, the results are valid for the cohort who turned 14 in the first half of 1999. In other words, the results and conclusions cannot be extrapolated to different age groups or cohorts. Second, one should not read the results among non-whites as an implicit advocacy towards child labour, as the counterfactual are children allowed to work in the formal sector at age 14.

2 THE INTERVENTION: THE LAW OF DECEMBER 1998

As discussed in chapter one, Brazil's Constitutional Amendment No. 20 on 15 December 1998 increased the minimum legal age of entry into the labour market from 14 to 16. The law approved in December 1998 mostly affected individuals who turned 14 from January 1999 onwards. The law became a binding constraint only for a subgroup of children who turned 14 after December 1998. Those who turned 14 before the law was passed were unaffected by the ban and could carry on working.

One consequence of the ban was the division of similar individuals into two groups: the affected (eligible) and the unaffected (control). Note that if some of those affected by the ban shifted to the informal sector, they automatically entered the child labour statistics, whereas those of a similar age (and who are plausibly similar, on average, in other characteristics) in the control group did not. This is interesting in itself, as the results of this chapter can shed some light on the long-term consequences of child labour.

The main question this chapter investigates is how these two cohorts who turned 14 close to the change in the minimum legal age and facing different constraints to participation in the labour force, performed in the long term.

3 THEORETICAL FRAMEWORK

This section presents the theoretical framework used to rationalise the effect of the child labour ban on children's labour supply in the short-run and its possible consequences in the long-run when children become adults. The two key references are the theoretical model by Baland and Robinson (2000) and its extension by

Horowitz and Wang (2004).[93] The model aims to provide the rationale for parents' investment decision making in their offspring when the capital market is imperfect. The main difference between the two models is that Horowitz and Wang (2004) introduce intra-household children's heterogeneity and derive theoretical implications for parents' decisions in favouring human capital investment in a child.

Baland and Robinson's (2000) model consists of two periods, t=1, 2. The model makes a simplified assumption of no discounting factor. At the beginning of period 1, there are L_p identical parents alive and they choose how to allocate their children's time between child labour and human capital accumulation. In period one, parents control all household income. In period two, children are adults and control their own income. Parents supply labour inelastically. Each parent has *A* efficiency units of labour in each period so that in period t=1 parental labour supply is AL_p. The child labour supply is given by nL_pl_c, where n is the number of children and l_c the fraction of children's time devoted to work. The term nL_pl_c is thus the fraction of time devoted to work by all children of living parents. In period t=2, children become adults and work. Their total labour supply in t=2 is given by $L_ph(1-l_c)$, where $h(1-l_c)$ are the units of human capital accumulated by an adult who worked l_c of his time as a child. The human capital function *h*(.) is assumed to be twice continuously differentiable, strictly concave with *h*(0)=1. That is, if a child only worked during her childhood, she will have a single efficiency of unit labour (A) in t=2.

Other agents in the economy run firms and use labour to produce a numeraire good. Agents who represent the firms live for both periods and have no children. Firms have a linear technology. With the assumption of constant returns to scale, economic profits are expected to be zero in equilibrium. This assumption implies perfect competition in the production of the numeraire good. Firms and workers are price takers. Thus, adults and working children will be paid the same wages, w_{p1}, w_{p2}, w_{c1}, w_{c2}. For the sake of simplification, wages are set to one.

Parents have a joint utility function that depends on their consumption of the numeraire good defined by c_p^t for t=1, 2; the number of children they have, *n*; and the utility of their children. Children's preferences are assumed to be identical to those of their parents. Note that this framework is an example of an inter-temporal unitary

[93] More specifically the version of the model with one-sided altruism and exogenous fertility.

household model. Parental utility is given by $W_p\left(c_p^1, c_p^2, n, W_c(c_c)\right)$. Child's utility depends only on child's consumption, c_c. This function is assumed to be separable so that

$W_p\left(c_p^1, c_p^2, n, W_c(c_c)\right) \equiv U(c_p^1) + U(c_p^2) + n\delta W_c(c_c)$, where $1 > \delta > 0$ is a parameter measuring the level of altruism of parents towards their children. U(.) and W_c(.) are assumed to be twice continuously differentiable and strictly concave. For the sake of simplification, n is treated as exogenous and set as equal to 1.[94]

In this setting, parents choose l_c, how much income to transfer to period t=2 as bequests or savings. Bequests, *b*, cannot be negative. With imperfect capital markets parents cannot borrow, thus savings, *s*, are also non-negative. Parents face the following budget constraints:

$$c_p^1 = A + l_c - s \tag{1}$$

$$c_p^2 = A - b + s \tag{2}$$

$$c_c = h(1 - l_c) + b \tag{3}$$

The first order conditions are obtained through this constraint maximisation problem. Using (1), (2), and (3) in the utility function, the first order conditions can be obtained through an unconstrained maximisation problem as follows:

$U(A + l_c - s) + U(A - b + s) + \delta W_c(h(1 - l_c) + b)$, for n=1

(i) $\frac{\partial L}{\partial b} = 0: U'(c_p^2) = \delta W_c'$ for $b > 0$ or $U'(c_p^2) > \delta W_c'$ for $b = 0$

(ii) $\frac{\partial L}{\partial l_c} = 0: U'(c_p^1) = \delta W_c' h'(1 - l_c)$

(iii) $\frac{\partial L}{\partial s} = 0: U'(c_p^1) = U'(c_p^2)$, for $s > 0$ or $U'(c_p^1) > U'(c_p^2)$ for $s = 0$

It is assumed that there exists an interior solution for child labour with the interior optimal level given by l_c^* . From (ii), $h'(1 - l_c) = \frac{U'(c_p^1)}{\delta W_c'(.)}$. For the optimal level of child labour, l_c^*, $h'(1 - l_c) = 1$, because the marginal utility of consuming one extra unit of c_p^1 should be equal to the marginal utility of consuming one extra unit of c_c for a given δ. Note that for $h'(1 - l_c^*) > 1$ the level of child labour will be inefficiently high. From (ii), it means that $U'(c_p^1) > \delta W_c'(.)$, that is, to equalise the marginal utilities the level of child labour has to increase.

[94] Baland and Robinson (2000) discuss the implications of the model of treating fertility endogenously. We left this discussion aside, because it is not the main purpose of this chapter.

In this setting, the decision to send children to work in period t=1 is motivated by imperfections in the capital market. Parents cannot borrow, but they can transfer income to their offspring through savings or bequests.

Baland and Robinson (2000) show in Proposition 1 that when bequests and savings are interior, that is when for $s > 0$ and for $b > 0$, the level of child labour in the economy is efficient, because parents will equalise the marginal return to education in terms of income with the opportunity cost in terms of lower child labour. On the other side, if bequests are at a corner, $b = 0$, the level of child labour in the economy will be inefficiently high since $h'(1 - l_c^*) > 1$ (see Proposition 2).

The inefficient level of child labour could also occur when bequests are interior but parents cannot borrow either, because they are liquid constrained (poor) or due to imperfections in capital markets. Since children cannot enter into a credit contract with their parents because it would not be credible, households cannot solve the inefficiency problem themselves.

In the Baland and Robinson (2000) model, the two causes of the high level of child labour are income poverty and parents' lack of altruism toward their children – low level of δ. From (ii) above, it can be shown that $\frac{\partial l_c^*}{\partial \delta} < 0$ and $\frac{\partial l_c^*}{\partial A} < 0$, that is, the higher the weight parents assign to children's utility, the lower the child labour, and the higher the parental endowment, the lower the child labour.

Since households cannot solve inefficiencies accruing from capital market imperfections, there is room for the government to intervene, and this simple framework allows one to assess the potential consequences of a child labour ban.[95] According to the authors, with perfect competition in both output and input markets, firms and workers are price takers and a marginal ban on child labour will be Pareto improving, since it will not have direct effects on firms' profits and wages.[96]

A ban can have equilibrium effects. With the ban, the child labour should be lower and the adult labour supply should be higher in the long-run. Children and adult current wages should therefore increase and future wages fall. However, if banned children accumulate human capital, adult wages should thus be lower in the long-run for the educated children. Note that to be welfare enhancing, the effect of

[95] A similar argument is developed in Basu and Van (1998).
[96] They investigate the impact of a marginal ban, that is, a ban on the intensive margin of child labour supply to be able to use calculus techniques.

the ban on parental current wages should dominate any welfare loss in terms of lower wages in the long-run.

Horowitz and Wang (2004) add children's heterogeneity into this framework to explain parents' decisions to favour one child over another. Children are heterogeneous, as represented by a parameter a_i, that can either be a child's innate ability or any other environmental factor that can give a child a labour market advantage over another, such as age, race, and gender in case the labour market discriminates against non-whites and women. Let child's i human capital be defined by $h(e_i, a_i)$, where $h_e > 0$, $h_{ee} < 0$ and $h_a > 0$, that is, human capital is a strictly concave function of education and positively associated with 'talent.'

A more 'talented' child will receive increased investment in her human capital in time t=1 and enter t=2 with a higher stock of human capital, (h). Since the more 'talented' child also earns more in t=1, parents then need to decide how much of a child's time to allocate to school and labour in t=1. Horowitz and Wang (2004) show that from the first order conditions, $h_e^1(e_1^*, a_1) = h_e^2(e_2^*, a_2)$, where $e = 1 - l_c$ is defined as above, that is, the time a child devotes to education. Let child 1 be more 'talented' than child 2, then $a_1 > a_2$. Thus, for the marginal returns to education to equalise, $h_e^1 = h_e^2$, the more 'talented' child has to receive more education than the less 'talented,' since for a given labour supply, l_c (and therefore e), the rate of return to education is higher among the more 'talented.' The 'less talented' will then work more than the more 'talented' in t=1.

Horowitz and Wang (2004) model's intention is to understand parents' decision of favouring one child in the household. In doing so, it provides insights on parental decision to invest in child's schooling or not based on the child's perceived 'talent'. The model does not distinguish how households with different socioeconomic background behave, but it makes clear that the decision of favouring a child would kick in if a household is poor or faces market constraints, such as liquidity or credit constraint.

I apply this theoretical framework to the problem at hand and try to shed light on the potential effects of the ban on households with different social economic backgrounds. Although not originally envisaged for this purpose, the model provides several insights to speculate on what type of household would be more likely to face a situation of having to favour a child.

To be consistent with the model's terminology, I will call the white males the more 'talented' and the non-white males the less 'talented'. By using skin colour as a proxy for 'poverty' status, it is then assumed that non-white males belong to poorer households than white males. In fact, as discussed in the next section, descriptive statistics sustain that assumption (see Table 1 below). As such, those poorer households should have more difficulty smoothing the negative income shocks caused by the ban given all the constraints they and their child may face – e.g. imperfect credit markets, low quality public schools, higher inter-temporal discount factor (Lawrence, 1991), and more discrimination in the labour market (Horowitz and Wang, 2004) – than white males. Consequently, poorer households should be less likely to invest in their non-white child, because they have less income and because they might believe that the investment would not pay off given that the child is perceived as 'not-so-talented.' If both parents and the labour market perceive the group of white males as more 'talented,' and white male households rely less on child labour, then they should be able to invest in their child's human capital.

If that is the case, then in the short-run one would expect non-whites to be more likely in child labour and consequently having lower stock of human in the long run than whites. The long-run effect of the ban will depend on parents' capacity (including their preferences toward their children) to compensate the decrease in household income caused by the ban with higher accumulation of human capital of their children. Because white households might be better positioned to smooth the negative shock and invest in their children's human capital, one should expect the cohort of white males hindered from entering the labour force at age 14 doing at least as well as than their non-banned peers.

Note that the within group (race) wage gap can thus increase or decrease as a consequence of a child labour ban among this group. However, the combination of these two hypothesised effects on white and non-white males should lead to an increase in the wage gap between more 'talented' and less 'talented' children in the long-run.

4 EMPIRICAL STRATEGY

The objective of this chapter is to estimate the long-run effects of being hindered from participating in the (formal) labour force at age 14. The problem is that the participation decision is endogenous. An individual may participate in the labour force for a number of reasons, e.g., to complement household income, because s/he is talented enough to abdicate formal education, or because parents are not fully aware of the returns to education. Whatever the explanation, individuals may enter the labour force at a certain age for a variety of reasons. This chapter uses the ban of December 1998 to identify the long-run consequences of an exogenous variation in labour force participation at age 14.

As in Angrist and Krueger (1991),[97] the identification strategy relies on the individual's date of birth. The change of the minimum legal working age in December 1998 affected only those who turned 14 from January 1999 onwards. The analysis of the long-term effects of the law on individual outcomes consists of comparing the cohorts who turned 14 in the second half of 1998 with individuals who turned 14 in the first half of 1999. However, unlike Angrist and Krueger (1991) and many other authors who combine birth date with school entry or exit ages, parents could not have anticipated this change in law and its effects.[98]

Using the household surveys of 2007, 2008, 2009, and 2011, the impact of the ban on the outcomes of interest are estimated fitting the following reduced-form regression model,

$$y_{ict} = \alpha + \rho D_{ic} + h(Z_{ic}) + \beta X'_{ict} + \tau_2 w_{08} + \tau_3 w_{09} + \tau_4 w_{11} + u_{ict} \qquad (1)$$

where y_{ict} is the outcome of individual *i* in cohort *c* in time *t*, D_{ic} is a dummy that takes on the value of 1 if the individual belongs to cohort *c*, i.e., if s/he turned 14 in the first half of 1999 and could not participate in formal labour market due to the ban, and 0 if s/he turned 14 in the second half of 1998 and was thus allowed to participate. The eligible group is those who turned 14 between January and June of 1999. The same cohorts are compared for ages 22 to 23 and ages 26 to 27.

The function $h(Z_{ic})$ depends on age, the forcing variable, and will be referred to as the "smooth function." The variable age, Z_{ic}, is defined in weeks and is set to 0 for individuals who turned 14 on the last week of December 1998. Thus, Z_{ic} takes the value of 1 for the first week of January 1999, 2 for the second week, and so on.

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97 Many other authors have used a similar approach after the publication of this seminal paper. There is an increasing body of literature on weak instruments showing that the instrumental variable used by Angrist and Krueger (1991), the quarter of birth, may be weak. Differently from Angrist and Krueger, we estimate reduced form regressions.

98 See, for instance, Smith (2009), McCrary, and Royer (2011), and Black et al. (2011). For criticisms on using date of birth as an instrumental variable for years of schooling, see Bound, Jaeger and Baker (1995) and Staiger and Stock (1997).

Analogously, Z_{ic} takes the value of -1 for the third week of December 1998, -2 for the second week, and so on. X_{ict} is a vector of controls that includes parents' years of schooling, parent's age, household size, household non-labour income, household income net of children's income and land title, w_t for t=08, 09, and 11 are survey-year dummies (2008, 2009, and 2011), and u_{ict} is the error term. The estimates discussed in the text are estimated without controls, but robustness check control for the vector of covariates X_{ict}.

The parameter of interest, ρ, corresponds to the *intent-to-treat* as long as the analysis is performed for all individuals who belong to the cohort affected by the law rather than the subgroup of children who actually dropped out of the labour force.[99] The identification of this parameter depends on exogenous variations in the labour force participation rate of some 14-year-old individuals in the first half of 1999, as they become less likely to participate in the labour force compared to their counterparts.[100] If the law of December 1998 implied a reduction in labour force participation, then the outcomes of the cohort who were 14 years old just before December 1998 can be used as counterfactual for the cohort who turned 14 just after the law passed.[101]

With hourly wage in natural log in the left hand side of eq. (1), the model becomes very similar to the Mincer equation. However, note that eq. (1) does not include years of schooling as in the original Mincer equation. This is because in the Mincer equation the potential experience and the years of schooling are endogenous variables. It is a common practice to replace potential experience with an individual's age, leaving the researcher with the problem of dealing with the endogeneity of years of schooling. In the present case, the intent-to-treat estimates exclude the school attenders for all labour market outcomes. The empirical exercise involves identifying the most plausible mechanism through which the law affects adults' wages. As

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[99] For a comprehensive introduction to different treatment effect parameters, see Heckman, Lalonde, and Smith (1999).
[100] The condition is called the monotonicity assumption. See, for instance, Imbens and Angrist (1994).
[101] As discussed in chapter one, according to the Brazilian Constitution the apprenticeship programme was available for youth aged 14 even before the increase in the legal minimum working age. Thus, the apprenticeship programme should have a common effect in the eligible and ineligible cohorts. However, since the programme remained an alternative to youth entering the formal labour force at age 14, the impact of a ban could have been further attenuated had the number of 14-year-old apprentices been high. Courseuil et al. (2012) show that the number of apprentices in Brazil before December 2000 was less than 100.

mentioned, this chapter suggests that experience is likely driving the effect of the ban on labour market outcomes.

If the labour force participation rate varies according to individuals' backgrounds, the law might have had heterogeneous and distributive effects on wages.[102] Given the exogeneity of the law, unconditional quantile treatment effects are estimated to determine if that was the case. As with the ITT, estimates are provided by pooling the years and allowing for different year effects.

To check robustness, eq. (1) is estimated with controls and with a bandwidth size of three months. A placebo test is also performed, comparing two cohorts that supposedly would not be affected by the law. For this exercise, the comparison is between individuals who turned 14 in the first and second halves of 1999.

5 DATA

This chapter uses several years of the Brazilian household survey PNAD. Data from 1998 and 1999 are used for descriptive statistics and short-run estimates. For the long-run analysis, I pool the surveys from 2007, 2008, 2009, and 2011.[103] Because the survey is not collected in census years, 2010 could not be considered.

The PNAD has been conducted annually by the IBGE since the end of the 1970s and covers around 100,000 households and 320,000 individuals. The survey is collected between October and December each year, and it constitutes one of the main sources of microdata in Brazil.[104] The PNAD is nationally representative, containing information on household socioeconomic characteristics, demographic data, household sources of income, and labour force status.

The purpose of pooling several years of the household survey is threefold. First, covering several waves of the survey is important if one aims to investigate the impact of the ban on schooling and labour market outcomes when individuals are transitioning from school to work. Second, pooling allows for a better understanding

[102] We look at heterogeneous effects across gender and explore distributional impacts through unconditional quantile treatment effects. Unconditional quantile treatment effects are estimated only for hourly wage, since the other outcome variables are binary. The heterogeneity in wage distribution also justifies the estimation of the effect of the ban at different points of the wage distribution.
[103] Rural areas are under-represented in the PNADs. See www.ibge.gov.br.
[104] The survey documents provide the month (September), week (last of the month), and day (usually 27th of the month) of reference for the Brazilian PNAD. According to emails exchanged with members of the Brazilian Bureau of Statistics, the survey is usually collected between October and December each year.

of the mechanisms underlying individuals' decisions regarding the accumulation of human capital through formal education or labour market experience. Third, a larger sample size increases the estimates' precision. As in chapter one, the same sample used in the empirical analysis, focusing on households in urban areas.

5.1 Descriptive statistics

As mentioned, skin colour is used as a proxy for individuals' backgrounds, because it is highly correlated with individuals' backgrounds. Table 1 compares whites and non-whites across several socioeconomic characteristics. On average, non-whites lag behind in all dimensions, with differences in means being statistically significant except in one case. The impact of the ban on participation rate is therefore not straightforward, as child workers can move to the informal economy. If children have moved to the informal economy, it could be argued that accumulated experience in the labour market is likely the mechanism underlying the long-term impact of the law on this subgroup, unless the returns to experience differ according to the sector in which experience was accumulated. If labour force participation drops and completed years of schooling remains the same between eligible and ineligible groups, then it can be argued that experience is the main driver.

On the other hand, if one observes a drop in participation rate and differences in human capital accumulation through schooling, then education is likely the main driver for the observed differences in labour market outcomes.

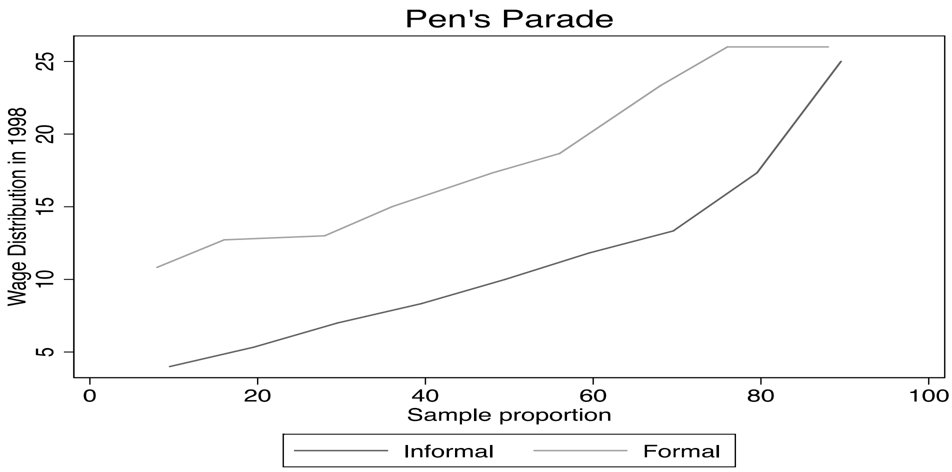
Table 1 – T-test for Difference in Means in 1998 – White vs. Non-White Males

	Non-white	White	*P-value*
Log of hourly wage	2.21	2.90	0.00
Labour force participation rate	0.21	0.15	0.00
Labour force participation rate – Formal	0.00	0.01	0.03
Occupation rate – Formal	0.05	0.15	0.01
Informal	0.07	0.06	0.12
Domestic work	0.69	0.67	0.14
School attendance	0.90	0.94	0.00
Mother's Education	4.60	6.30	0.00
Father's Education	3.60	5.50	0.00
Household size	5.00	4.60	0.00

Source: PNAD 1998.

With the ban, similar individuals would receive different wage rates. Figure 1 indicates that individuals aged 14 before the ban received a higher wage rate than those who turned 14 after the ban was enacted, as they could still participate in the formal labour force.[105] This is consistent with the assumption in the theoretical framework and may have been one of the drivers of children's decisions to leave the labour force after December 1998. The other critical assumption is related to labour demand, as risk-averse employers would rather dismiss children under age 16 fearing prosecution by Brazilian judicial authorities.

Figure 1 – Hourly Wage Distributions for Formal and Informal Workers at Age 14 in 1998



Source: PNAD 1998.
Note: The Penn's Parade is just an alternative way of reporting the cumulative distribution function (CDF) (see Jenkins and Van Kerm, 2009). In 1998, the Brazilian monthly minimum wage was R$ 130.

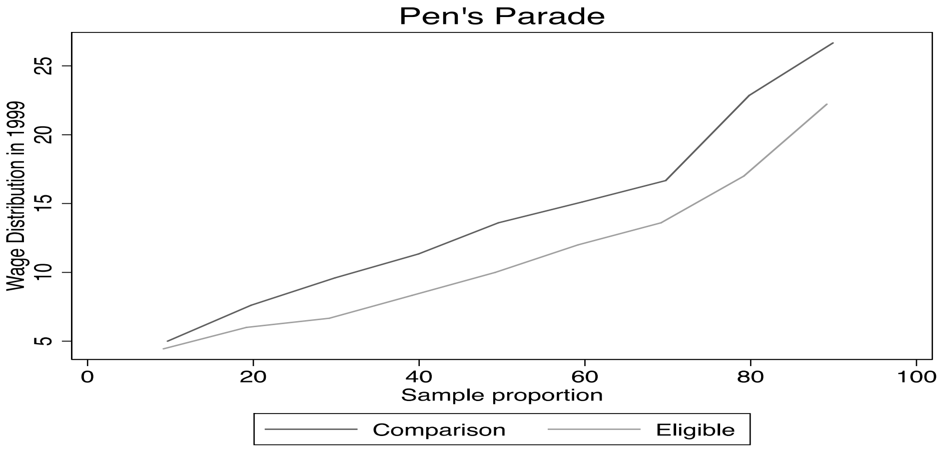
Figure 1 shows the wage rate distribution for the formal and informal sectors for all individuals aged 14 in 1998. The observed pattern in wage rate distributions has direct welfare implications. Given that the wage rate in the formal sector lies above the wage rate in the informal sector, all children working in the formal sector were better off than children working informally. In order words, the wage rate in the formal sector first order dominates the distribution in the informal sector.[106] The

[105] A T-test for difference in means rejects the null hypothesis of equal means at the one percent level. The wage paid in the formal sector was, on average, about 46 percent higher (R$ 187.5 vs. R$ 128.5). According to the PNAD 1999, the monthly wage in the informal sector was lower than in 1998 (R$ 86.4). This could be partially explained by the economic recession in that year.
[106] In the inequality literature, this way of representing the two variables' distribution is known as Pen's Parade. As shown in Jenkins Van Kerm, 2009, this is an alternative way of plotting cumulative distribution functions and is usually used in the literature of welfare economics. The wage distributions correspond to two alternative states of nature, and the purpose is to know in which of the two individuals are better off, i.e., which has the highest welfare. As discussed in Jenkins Van Kerm (2009), when one CDF lies above another, one can say that it dominates in first order and

lower wage rate in the informal sector may have contributed to the decrease in the labour force participation rate, since the wage rate in the informal economy would be lower than the reservation wage for some individuals.

In fact, figure 2 shows that the hourly wage of the eligible group was below the wage rate received by children in the comparison group along all distributions. In other words, comparison children were better off than children banned from the formal labour force.

Figure 2 – Hourly Wage Distributions for Children Aged 14 Before and After December 1998



Source: PNAD 1999.

Taking this set of descriptive results into account, it is possible to roughly estimate the effect of a change in wage rate on individuals' participation in the labour force. Since individuals who are very close in age are likely to have similar observed and unobserved characteristics, the ban gave rise to a natural experiment wherein the 'same' individual faced two different wage rates. Thus, it is plausible that a fraction of individuals who have a reservation wage above the wage rate paid in the informal sector dropped out of the labour force after the ban.

The difference in wage rate between the eligible and ineligible groups in 1999 was, on average, about 16 percent.[107] To get a sense of the elasticity of labour supply at the intensive margin, the following reduced-form equation is estimated,

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consequently all individuals who belong to that distribution are better off than those represented in the one that lies below.

[107] The average wage rate of the comparison group is 15.7 reais, whereas the eligible group received an average wage rate of 14.15 reais. The difference in means is not statistically significant, but the

$$lnwhw_i = \alpha + \beta_1 lnwage_i + \beta_2 lnwage_i * D_i + h(Z_i) + u_i \quad (2)$$

where *lnwhw* holds for weekly hours worked in natural log, *lnwage* is the natural log of hourly wage, and *h(.)* is defined as before. For the sake of simplicity, eq. (2) is fitted with 3-months bandwidth and with the smooth function specified as polynomials of 0 to 3 degrees and as linear, quadratic, and cubic splines. The parameter of interest is β_2. Note that the equation is estimated only for individuals participating in the labour market and whose wage is non-zero. Table B.16 in Appendix 2 (page 174) shows the results. The coefficient for the elasticity of labour supply is about -0.3 and statistically significant at the 1 percent level in all cases, indicating that a decrease in hourly wage of 10 percent would increase hours worked by 3 percent. The negative coefficient suggests that leisure is a normal good, as demand for leisure reduces as a consequence of a negative income shock. In addition, it suggests that the labour supply of male youth is not very responsive to variations in wage rate, meaning that boys have to work harder to compensate for a reduction in wage. This estimate is similar to that which is considered the benchmark in the literature.[108] This result is consistent with the hypothesis that child labour is influenced by the poverty status of the household (Bhalotra 2007).[109]

The figures below present the visual checks of the short-run effects of the ban. Local linear regressions with a triangle kernel with a bandwidth of one month are fitted to each side of the cut-off point. The figures consider an interval of one year each side of the cut-off point, as in chapter one.

Since the survey provides the exact birth date of each individual, age is defined in months to mitigate excess noise. The forcing variable *Z* is represented in the horizontal axis and the cut-off is set at zero in December 1998. Individuals on the right side of the cutoff point are banned from the labour force, as they turned 14 after December 1998.

Kolmogorov-Smirnov test rejects the null of equal distributions at the 5 percent level (p-value of 0.049) with a 6-month bandwidth.

[108] For an extensive survey of this literature, see Blundell and MaCurdy (1999). Recent evidence includes Ziliak and Kniesner (2005) and Bargain et al. (2012). The estimate of -0.3 for young males is within the range found in the empirical literature and is almost identical to the estimate found by Bhalotra (2007).

[109] Bhalotra (2007) argues that the wage elasticity of child labour supply should be negative under the hypothesis that child labour is compelled by poverty. Using data from Pakistan, she finds support for this hypothesis for boys and mixed results for girls.

Figures 3a and 3b show a decrease in labour force participation rate for white and non-white males in 1999.[110]

Figure 3a – Local Linear Regression for Labour Force Participation Rate in 1999
White Males

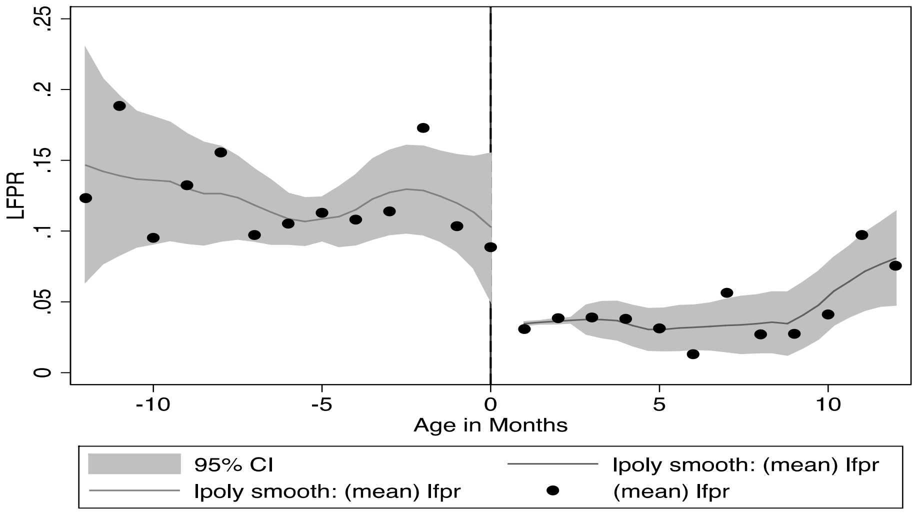
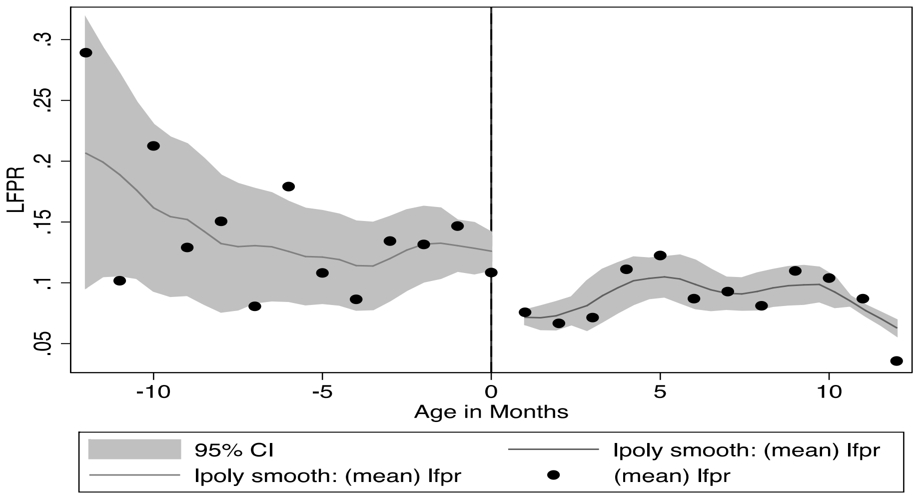


Figure 3b – Local Linear Regression for Labour Force Participation Rate in 1999
Non-white Males



The figures suggest that the ban might have had local effects on the participation rate of white and non-white males. The decrease in labour force participation rate among the eligible group might be associated with the lower wage rate in the informal sector. It may also be associated with an improved public monitoring system, making it easier for public authorities to identify underage working youth rather than verifying whether s/he has a formal labour contract with

[110] The participation rate for girls is not shown, because the short-run estimates discussed in chapter one are not statistically significant.

the employer—an attempt of some parents to avoid the *stigma* of having a son working informally. In addition, the decrease may be attributable to a change in labour demand if some risk-averse employers decided to avoid employing underage workers in their business. It is interesting to note that the drop in participation rate was clearly stronger among white males than among non-white males. This is consistent with the assumptions in theoretical models indicating that child labour is largely explained by the poverty status of the household.

To check whether the discontinuities in figures 3a and 3b capture exclusively the effect of the ban, I compare the cohorts who turned 14 before and after December 1997, one year before the increase in the minimum employment age. Figures B.1 and B.2 (Appendix 2, page 216) show no visual discontinuity in participation rate for the 'placebo' cohorts. This visual check will be complemented by parametric regressions in the section that will check robustness of the main results.

Figures 4a and 4b illustrate the effect of the ban on females.

Figure 4a – Local Linear Regression for Labour Force Participation Rate in 1999
White Females

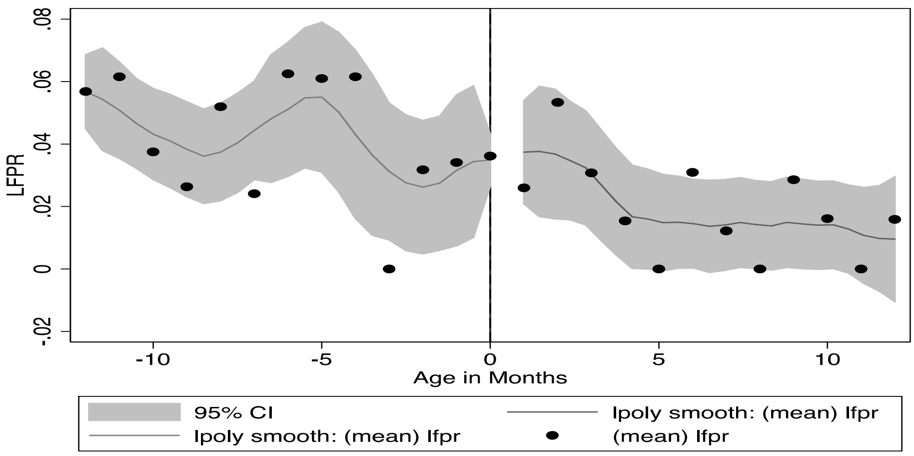


Figure 4b – Local Linear Regression for Labour Force Participation Rate in 1999
Non-white Females

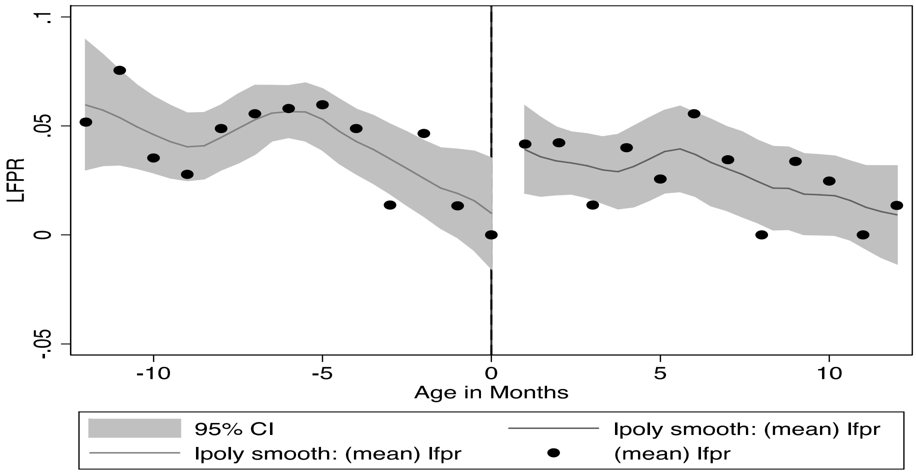


Figure 4a shows no discontinuity, but figure 4b suggests that the ban might have increased the labour force participation of non-white females.

As discussed in chapter one, the validity of the RD design depends on a balanced sample around the threshold and some evidence of imperfect manipulation of the assignment variable. Table 2 presents the t-test for difference in means for some covariates with a six months (or 26 weeks) bandwidth. The table reports the coefficients of simple regressions of each covariate on a constant and the indicator function *D*, with *D* defined as in eq. (1). The sample seem to be very well balanced around the cutoff point, as the null hypothesis of equal means is rejected in very few cases. It is worth making a couple of comments on the coefficients of monthly earnings for both groups of males separately. The coefficient of white males is positive and thus consistent with the theoretical framework. With the ban, children's wages should increase in the short run. Interestingly, the coefficient for non-white males is negative and very precisely estimated.

As hypothesised, non-whites are less likely to drop out of the labour force and are more likely to shift to the informal sector. The difference in wages reflects the average wage differential observed in the formal and informal sectors and a composition effect. With the ban, child labour supply increased in the informal sector and probably widened the wage differential of the two sectors even further. The long-run effects for non-whites thus depend on the quality of work experience accumulated in the informal sector and, perhaps more fundamentally, on how employers perceive the set of skills of workers whose experience was accumulated in the informal sector.

Table 2 – T-test for Difference in Means in 1999 – Males
26 Weeks Bandwidth

	All	Whites	Non-whites
Mother's education	0.15	-0.072	0.38
	(0.68)	(-0.22)	(1.41)
N	1839	891	948
Father's education	-0.0041	-0.038	0.051
	(-0.019)	(-0.12)	(0.19)
N	*1839*	*891*	*948*
Mother's age	-0.22	-0.95	0.48
	(-0.23)	(-0.71)	(0.35)
N	*1839*	*891*	*948*
Father's age	-1.09	-0.98	-1.23
	(-1.15)	(-0.72)	(-0.92)
N	*1839*	*891*	*948*
Household size	0.034	0.085	-0.020
	(0.46)	(0.91)	(-0.18)
N	*1839*	*891*	*948*
Land title	-0.013	-0.034*	0.0080
	(-0.91)	(-1.88)	(0.37)
N	*1456*	*707*	*749*
Household non-labour income	-0.0014	-0.19	0.21
	(-0.0013)	(-0.10)	(0.22)
N	*1839*	*891*	*948*
Monthly earnings	-23.5*	10.4	-28.7***
	(-1.84)	(0.36)	(-2.63)
N	163	67	96
Monthly household net income (net of children's income)	19.3	43.4	1.22
	(0.49)	(0.61)	(0.035)
N	*1839*	*891*	*948*

Source: PNAD 1999.
Note: The T-test is performed through simple regressions with each covariate *X* being regressed on a constant and the indicator variable *D*. T-statistic in parenthesis. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Figures 5 to 8 illustrate what may have happened to the cohorts in the long run. The figures are plotted with the pooled data from 2007 to 2011 (excluding 2010). As before, local linear regressions are fitted to each side of the cut-off point.

Figure 5 – Local Linear Regression for Log of Hourly Wage – Long Run
White Males

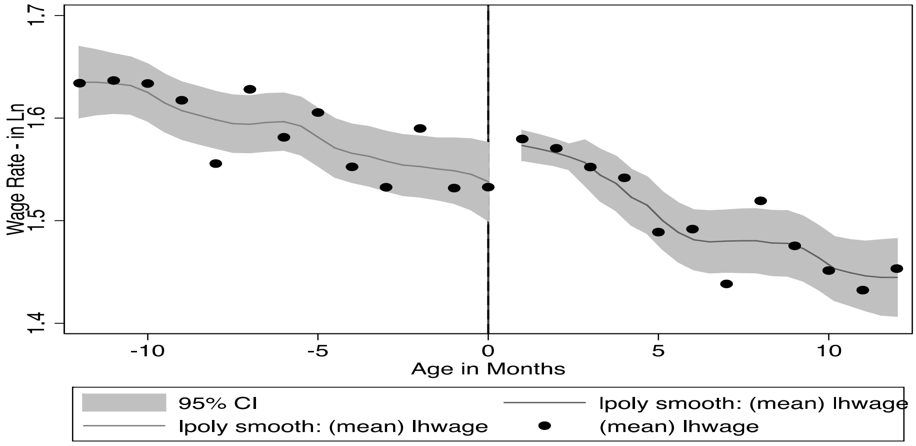


Figure 6 – Local Linear Regression for Labour Force Participation Rate – Long Run
Non-white Males

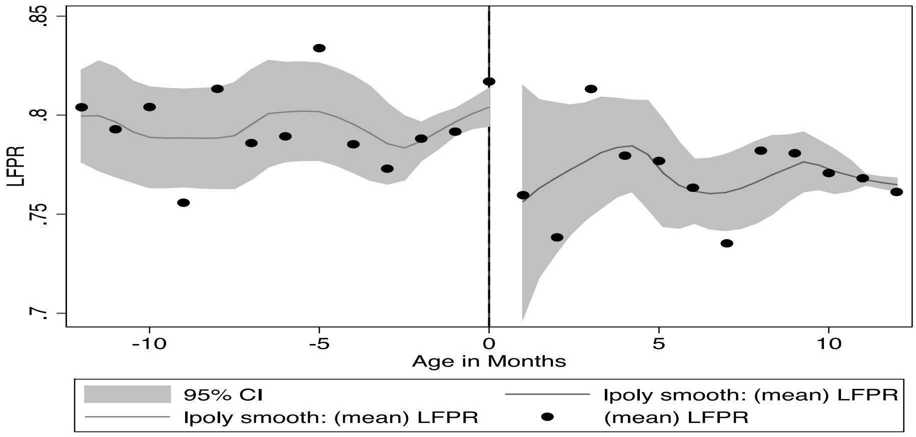


Figure 7 – Local Linear Regression for Participation Rate in the Formal Labour Force – Long Run
Non-white Males

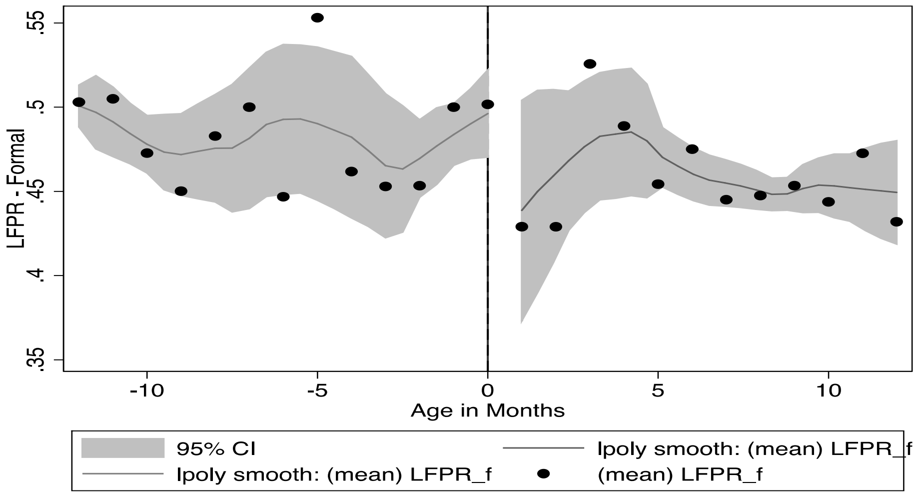


Figure 8 – Local Linear Regression for Having a College Degree – Long Run
White Males

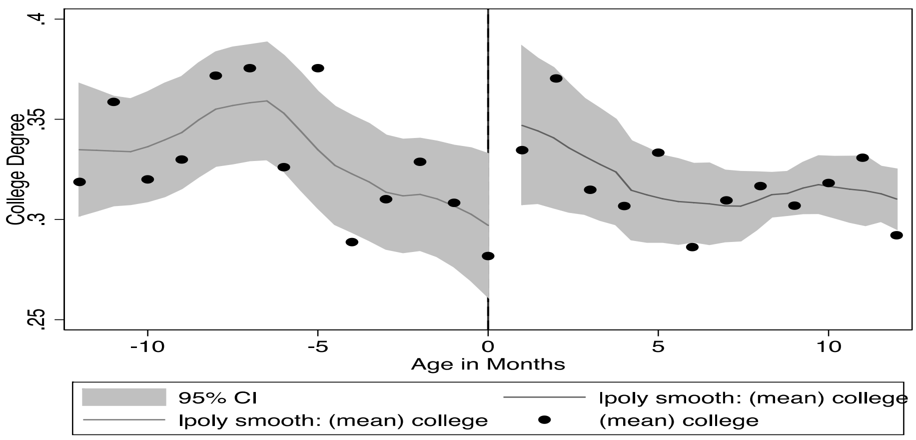


Figure 5 shows a small positive jump in the wage rate distribution of white males at the cutoff.

Figures 6 and 7 suggest that the ban seems to have reduced the likelihood of non-white males' being employed and having a formal job. Figure B.3 in Appendix 2 (page 217) indicates that white males affected by the ban are as likely as the comparison cohort to be employed in a formal job.

Figure 8 points to an increase in the likelihood of white males having a college degree, a result that is also not observed among non-white males (see Figure B.4, Appendix 2 page 217). These are interesting results, as they indicate that the ban may have affected white and non-white males banned from the formal labour force in opposite ways.

To check whether the sample of eligible and comparison cohorts is balanced around the cutoff point in the long run, a t-test for difference in means for the outcomes and some covariates are reported using the pooled sample of 2007 to 2011 (excluding 2010). The t-test is presented in Table 3 and it shows very few differences in means for the covariates.

Given the imbalance in some covariates, a robustness check is performed controlling for a vector of covariates as discussed below. This balance check is informative for two reasons: it shows that eligible and ineligible youth have similar socioeconomic backgrounds, and it suggests that the ban did not affect the human capital of non-whites through education, as the 'eligible' and comparison non-white males around the cut-off have the same number of completed years of schooling, and no difference in the probability of having a college degree is suggested by figure B.6.

To further check whether education might be a channel through which the ban affected long-term outcomes, a Kolmogorov-Smirnov test is conducted to test the null hypothesis of equal distributions of completed years of schooling between eligible and comparison non-white males. The null cannot be rejected for the pooled sample of males or for the subsample of non-white males.[111] It is therefore argued below that accumulated experience is likely the main driver of results for non-white males, while for white males education seems to be the underlying mechanism.

[111] The *p-values* are 0.85 and 0.99 respectively.

Table 3 – Difference in Means for the Outcome Variables and Some Covariates – Males

6 Months Bandwidth

	All	Whites	Non-whites
Covariates			
White	0.016	*Na*	*Na*
	(1.45)	*Na*	*Na*
N	*7471*	*3248*	*4223*
Years of Schooling (*exclude school attenders*)	-0.077	-0.13	-0.050
	(-0.85)	(-0.90)	(-0.37)
N	*5879*	*2367*	*3512*
Father's Education	-0.17	-0.065	-0.28*
	(-1.65)	(-0.48)	(-1.96)
N	*7471*	*3248*	*4223*
Mother's Education	-0.083	-0.12	-0.083
	(-1.01)	(-1.01)	(-0.72)
N	*7471*	*3248*	*4223*
Father's Age	0.089	0.73	-0.49
	(0.18)	(1.06)	(-0.80)
N	*7471*	*3248*	*4223*
Mother's Age	0.33	-0.053	0.57
	(0.94)	(-0.11)	(1.23)
N	*7471*	*3248*	*4223*
Metropolitan Region	-0.0083	-0.022	0.0020
	(-0.87)	(-1.51)	(0.15)
N	*7471*	*3248*	*4223*

Source: PNADs 2007, 2008, 2009 and 2011.

Note: The T-test is performed through simple regressions with each covariate *X* being regressed on a constant and the indicator variable *D*. T-statistic in parenthesis. ****, **, * Statistically significant at 1%, 5%, and 10% percent respectively.

6 RESULTS

6.1 Short Term Effects of the Ban of December 1998

To check whether the discontinuities illustrated in participation rates of white and non-white males and females are statistically significant, the following regression model is used:

$$y_i = \alpha + \delta D_i + h(Z_i) + \varepsilon_i \qquad (3)$$

where D_i is a dummy that takes on the value of 1 for individuals who turned 14 after December 1998 and 0 otherwise, and Z_i is the forcing variable *age* defined in weeks, as explained in section 4. Analogously, eq. (2) is estimated with $h(Z_i)$ defined as polynomials of degree 0 to 3 and as linear and quadratic piecewise polynomials. For the sake of robustness, eq. (3) is estimated with 3- and 6-month bandwidths. The parameter of interest δ captures the (local) *intent-to-treat* of the ban. Table 4 shows the estimates for white and non-white males and females.

Given that the direction of the effect of the ban on the participation rate of males is known, one can test the hypothesis using a one-sided alternative to gain precision. The coefficients for the impact of the ban on white and non-white males are similar, stable, and precisely estimated in most of the cases for the bandwidth of 6 months. The coefficients for the narrower bandwidth are negative but imprecise. For white males in particular, most of the point estimates are small.[112] Given that the sample is balanced around the cut-off point for the larger bandwidth, I focus attention on the results estimated with the larger window. The results in table 4 indicate that the ban reduced participation in the labour force for both white and non-white males by about 6 and 7 percentage points respectively. In relative terms, they represent a reduction in child labour of about 30 percent.

Another notable finding is the absence of the effect of the law on girls. This is consistent with the visual check and with results obtained in the first chapter, providing further support to the evidence that the law affected males exclusively.

With regard to long run consequences of the ban, if the decrease in participation rate affected individuals' work histories, one can expect an effect on

[112] Chapter one shows that the results of the pooled sample of males are more precise and stable. Most of the estimates are statistically significant at standard levels and point to a decrease in overall participation rate of about 6 to 7 percentage points for boys. See Table 10a in chapter one.

employability. Along the same lines, if the wage rate of youth is somehow responsive to accumulated experience in the labour market, one can also hypothesise that the cohort of males affected by the law will have a different (lower) wage in the long run compared to the other group.

Table 4 – Short Run Effects of the Ban on Labour Force Participation Rate of Males in Urban Area

Functional Formal of *h(z)*	White Males	Non-white Males	White Males	Non-white Males	White Females	Non-white Females	White Males	Non-white Females
	3 Months Bandwidth		*6 Months Bandwidth*		*3 Months Bandwidth*		*6 Months Bandwidth*	
0	-0.00037	-0.084	-0.080***	-0.034*	-0.00087	0.0042	-0.012	-0.023
	(-0.0081)	(-1.54)	(-4.48)	(-1.87)	(-0.047)	(0.18)	(-0.95)	(-1.31)
1	-0.0059	-0.089	-0.058*	-0.065*	-0.014	0.048	0.012	0.040
	(-0.13)	(-1.64)	(-1.95)	(-1.68)	(-0.46)	(1.03)	(0.49)	(1.15)
2	-0.030	-0.030	-0.058*	-0.065*	-0.015	0.047	0.012	0.045
	(-0.46)	(-0.44)	(-1.93)	(-1.68)	(-0.46)	(1.01)	(0.48)	(1.37)
3	-0.0072	-0.091	-0.020	-0.11**	-0.011	0.019	-0.0094	0.035
	(-0.16)	(-1.63)	(-0.46)	(-2.14)	(-0.28)	(0.36)	(-0.32)	(0.82)
Spline linear	-0.027	-0.0097	-0.058*	-0.067*	-0.014	0.046	0.011	0.047
	(-0.37)	(-0.13)	(-1.94)	(-1.72)	(-0.44)	(0.97)	(0.45)	(1.40)
Spline quadratic	-0.00037	-0.084	-0.011	-0.13**	0.0012	-0.0067	-0.021	0.028
	(-0.0081)	(-1.54)	(-0.23)	(-2.10)	(0.030)	(-0.11)	(-0.64)	(0.60)
Mean of the comparison group	0.15	0.21	0.17	0.20	0.059	0.045	0.056	0.075
Observations	422	412	891	948	439	434	934	933

Source: PNAD 1999.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. Coefficients in bold are statistically significant against an one-sided alternative at 5% and 10% levels.

6.2 Long Run Effects of The Ban

ITT estimates on wages: Returns to experience?

Table 5 presents the ITT estimates without controls and a 6-month bandwidth. The table shows two sets of estimates. In the first set (columns 1 to 6), the ban is assumed to have a constant effect during the period. The second set of estimates (columns 7 to 12) relaxes this assumption and allows for heterogeneous time effects. Since contemporaneous education can have a direct effect on earnings, the estimates exclude school attenders.[113]

Estimates are provided with different specifications of the smooth function. The first row of the table shows six distinct specifications, with the first column consisting of a difference in means (polynomial of degree zero), whereas in the second, third, and fourth columns the smooth function is specified as polynomials of degree one, two, and three respectively. The last two columns consist of linear and quadratic splines. In these two cases, the slope of the functions fitted to each side of the cut-off point is permitted to differ.

The estimates suggest that white males were positively affected by the ban. Although the point estimates are sensitive to the specification of the smooth function and not precisely estimated, the pooled estimates suggest that postponing entrance into the labour force may have resulted in higher wages in the long run. It is worth mentioning that these estimates reflect the lower bound effects of the ban. Also, potential measurement errors of the dependent variable might partially explain the relatively high standard errors.

For non-whites, the opposite is observed. Most of the coefficients are negative, but only in 2009 are they robust to different specifications of the smooth function and more precisely estimated. For 2009, the cohort of non-white males earned about 12 percent less than the comparison group. It is difficult to justify such an effect in that particular year. A possible explanation is the contraction of the economy in the aftermath of the financial crisis. The Brazilian gross domestic product grew only 0.33 percent in real terms in 2009.[114] It might be that, for this

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[113] In fact, table 2 shows that school attendance is higher among the eligible group and the difference is statistically significant at the 1 percent level.
[114] Data available at www.ipeadata.gov.br.

group, more years of experience in the labour market helped smooth the negative macroeconomic shock.

Taking the statistically significant point estimates for white and non-white males at face value, they suggest that only white males managed to counterbalance the lower (potential) experience accumulated in the formal labour market with more education. The magnitude of the estimates for white males is actually similar to some of the best evidence of returns to education to the US.[115]

Non-whites, on the other hand, did not have the same success. In fact, the results for non-whites suggest accumulated experience in the formal sector is a key component in their human capital formation. Taking the estimate for non-white males as an estimate for their returns to experience, one realises that their magnitude is similar to what the empirical literature has reported for different countries. Despite being lower-bound estimates,[116] they are very similar to those of Angrist (1990) and Bratsberg and Terreall (1998) for the case of the US, and Imbens and van der Klaauw (1995) for Netherlands.[117]

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[115] Using a sample of twins in the US, Ashenfelter and Rouse (1998) estimate the return to education through a system of equations (Three Stage Least Squares). Controlling for family fixed effects, they explore the differences of years of schooling among twins in order to get a good estimate of the return to education. Their estimates point to a rate of return to education of around 10 percent.

[116] The estimates consider the eligible cohort rather than those who actually dropped out of the labour force as consequence of the law.

[117] Angrist (1990) looks at the impact of serving in Vietnam on adults' earnings and find that two years of serving implied an adult wage of 15 percent lower than that of non-servers. Imbens and van der Klaauw (1995) look at the impact of conscription in the Netherlands and find that one year of military service reduced the servers' annual wage by 5 percent. Both authors interpret these results through the effect of being recruited on potential experience.

Table 5 – Long Run Effects on Hourly Log Wages – White and Non-white Males

6 Months Bandwidth – Exclude School Attenders

	White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	-0.011	0.099	0.096	0.18*	0.097	0.21*	-0.036	0.078	0.076	0.16	0.086	0.19
	(-0.33)	(1.38)	(1.33)	(1.84)	(1.34)	(1.84)	(-0.60)	(0.89)	(0.87)	(1.45)	(0.97)	(1.58)
D*2008							0.028	0.027	0.024	0.023	0.011	0.011
							(0.32)	(0.31)	(0.28)	(0.27)	(0.12)	(0.12)
D*2009							0.010	0.0013	0.0013	0.0080	-0.0025	0.0043
							(0.12)	(0.016)	(0.015)	(0.097)	(-0.030)	(0.052)
D*2011							0.048	0.043	0.042	0.046	0.031	0.037
							(0.50)	(0.46)	(0.44)	(0.49)	(0.32)	(0.38)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1966	1966	1966	1966	1966	1966	1966	1966	1966	1966	1966	1966

	Non-White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	-0.029	0.0078	0.0014	-0.074	-0.0057	-0.065	-0.016	0.024	0.017	-0.059	0.015	-0.046
	(-1.29)	(0.16)	(0.030)	(-1.09)	(-0.12)	(-0.82)	(-0.38)	(0.38)	(0.28)	(-0.74)	(0.24)	(-0.50)
D*2008							0.052	0.051	0.049	0.052	0.042	0.045
							(0.89)	(0.88)	(0.85)	(0.89)	(0.71)	(0.75)
D*2009							-0.11*	-0.12*	-0.12*	-0.11*	-0.13*	-0.12*
							(-1.76)	(-1.79)	(-1.80)	(-1.75)	(-1.93)	(-1.92)
D*2011							0.0065	0.0052	0.0080	0.0094	0.0069	0.0076
							(0.11)	(0.086)	(0.13)	(0.16)	(0.11)	(0.12)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2831	2831	2831	2831	2831	2831	2831	2831	2831	2831	2831	2831

Source: PNADs 2007, 2008, 2009, and 2011.

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

Employability and education

The next two tables (6 and 7) show the long-term effects of the ban on employability defined as the probability of being employed and on being employed in the formal sector. As before, the estimates exclude school attenders, except for those pursing a college degree.

The ITT estimates suggest that the employability of the cohort of white males was unaffected by the ban, whereas non-white males became less likely to be employed or to be employed in the formal sector. Although only a few coefficients are statistically significant, most of the coefficients are positive for the cohort of whites and negative for the cohort of non-whites.

Table 6 – Long Run Effects on Being Employed – White and Non-white Males

6 Months Bandwidth – Exclude School Attenders

	White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	-0.00054	-0.010	-0.012	-0.018	-0.017	-0.022	0.013	0.0022	0.0012	-0.0040	0.000038	-0.0038
	(-0.033)	(-0.29)	(-0.34)	(-0.40)	(-0.47)	(-0.42)	(0.33)	(0.042)	(0.022)	(-0.066)	(0.00072)	(-0.057)
D*2008							-0.044	-0.044	-0.045	-0.045	-0.056	-0.056
							(-0.83)	(-0.83)	(-0.86)	(-0.86)	(-1.05)	(-1.06)
D*2009							0.0024	0.0034	0.0031	0.0026	0.0053	0.0049
							(0.043)	(0.061)	(0.055)	(0.048)	(0.096)	(0.090)
D*2011							-0.012	-0.012	-0.013	-0.013	-0.021	-0.021
							(-0.24)	(-0.24)	(-0.26)	(-0.27)	(-0.41)	(-0.40)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2367	2367	2367	2367	2367	2367	2367	2367	2367	2367	2367	2367
	Non-White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	-0.0045	-0.017	-0.017	-0.071*	-0.021	-0.079*	0.031	0.019	0.019	-0.036	0.015	-0.043
	(-0.30)	(-0.59)	(-0.60)	(-1.88)	(-0.71)	(-1.78)	(1.02)	(0.47)	(0.46)	(-0.73)	(0.36)	(-0.80)
D*2008							-0.043	-0.043	-0.043	-0.042	-0.041	-0.039
							(-1.03)	(-1.03)	(-1.03)	(-0.99)	(-0.97)	(-0.92)
D*2009							-0.039	-0.039	-0.039	-0.037	-0.041	-0.039
							(-0.88)	(-0.87)	(-0.87)	(-0.83)	(-0.92)	(-0.88)
D*2011							-0.056	-0.055	-0.055	-0.053	-0.054	-0.052
							(-1.35)	(-1.34)	(-1.33)	(-1.28)	(-1.29)	(-1.24)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3512	3512	3512	3512	3512	3512	3512	3512	3512	3512	3512	3512

Source: PNADs 2007, 2008, 2009, and 2011.

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

Table 7 – Long Run Effects on Being a Formal Employee – White and Non-white Males

6 Months Bandwidth – Exclude School Attenders

						White Males						
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	0.0083	0.028	0.027	0.075	0.035	0.082	0.032	0.054	0.053	0.099	0.064	0.11
	(0.33)	(0.61)	(0.58)	(1.25)	(0.74)	(1.21)	(0.61)	(0.80)	(0.79)	(1.26)	(0.93)	(1.27)
D*2008							-0.038	-0.038	-0.039	-0.039	-0.054	-0.054
							(-0.52)	(-0.53)	(-0.55)	(-0.55)	(-0.74)	(-0.75)
D*2009							-0.044	-0.047	-0.047	-0.043	-0.040	-0.038
							(-0.65)	(-0.68)	(-0.68)	(-0.63)	(-0.58)	(-0.55)
D*2011							-0.012	-0.012	-0.013	-0.011	-0.017	-0.014
							(-0.18)	(-0.18)	(-0.19)	(-0.16)	(-0.25)	(-0.20)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2283	2283	2283	2283	2245	2245	2283	2283	2283	2283	2245	2245
						Non-White Males						
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	0.011	-0.018	-0.020	-0.080*	-0.019	-0.095*	0.031	0.0019	-0.000013	-0.062	0.0017	-0.076
	(0.58)	(-0.49)	(-0.54)	(-1.69)	(-0.51)	(-1.72)	(0.82)	(0.038)	(-0.00026)	(-1.01)	(0.033)	(-1.11)
D*2008							-0.021	-0.020	-0.021	-0.019	-0.019	-0.017
							(-0.39)	(-0.38)	(-0.39)	(-0.36)	(-0.36)	(-0.32)
D*2009							-0.023	-0.022	-0.022	-0.020	-0.027	-0.025
							(-0.41)	(-0.39)	(-0.39)	(-0.35)	(-0.48)	(-0.44)
D*2011							-0.033	-0.033	-0.032	-0.030	-0.031	-0.029
							(-0.64)	(-0.63)	(-0.61)	(-0.58)	(-0.59)	(-0.55)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3403	3403	3403	3403	3403	3403	3403	3403	3403	3403	3403	3403

Source: PNADs 2007, 2008, 2009, and 2011.

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

Table 8 shows that white males are more likely to hold or be in pursuit of a college degree.[118] Combining the results on employability and education, it may be the case that some of the white males in the eligible group are, in fact, employed in higher skilled occupations. Tables B.1 to B.4 in Appendix 2 (pages 200-203) show estimates with 26 weeks bandwidth controlling for a vector of covariates. This is estimated with the intention of increasing power in case the covariates have any predictive power on the outcome variables. In fact, the coefficients are very similar, and there is very little gain in precision in adding controls.

Tables B.5 and B.6 (Appendix 2, page 204-205) show pooled and heterogeneous linear probability model estimates for nine occupational groups. The occupation dummies are regressed on a constant, the indicator D, a piecewise linear function of the forcing variable, $h(Z)$, and year dummies for 2008, 2009, and 2011. The standard errors are clustered at week level as before.

The results in Table B.5 point to an increase of about 5 percentage points in participation rate in skilled occupations among white males, a decrease of about 2 percentage points in participation rate in the armed forces, and a weak indication of a decrease in participation rate in civil construction. The coefficients in Table B.5 tell a similar story, but are less precisely estimated.

These results are striking. They suggest that the law had a positive effect on the better off (white males) and a remarkable negative impact on the worse off (non-white males). While these are local estimates for a very specific cohort, the results indicate that earlier entrance into the labour force benefited non-white males. This could be due to the fact that this group experiences more constraints in life, such as low quality public education, problems of self-control that imply a sub-optimal accumulation of human capital, or even myopic parents who underestimate the returns to education.

[118] In recent years, access to college degrees for people with relatively poor backgrounds was made much easier. The federal government has begun to fully or partially subsidise student loans and scholarships. However, most of the universities these people manage to attend do not have good reputations. Note that the estimates shown in table 7 include school attenders.

Table 8 – Long Run Effects on Holding or Being Pursuing a College Degree –White and Non-white Males

6 Months Bandwidth

	White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic	0	1	2	3	spline linear	quadratic
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	0.022	0.12***	0.12***	0.11**	0.12***	0.11**	0.034	0.13**	0.13**	0.13**	0.13**	0.13**
	(1.12)	(3.15)	(3.13)	(2.47)	(3.13)	(2.07)	(0.94)	(2.55)	(2.54)	(2.20)	(2.57)	(2.00)
D*2008							-0.015	-0.014	-0.014	-0.015	-0.0076	-0.0079
							(-0.29)	(-0.27)	(-0.28)	(-0.28)	(-0.15)	(-0.15)
D*2009							-0.020	-0.026	-0.025	-0.026	-0.024	-0.025
							(-0.38)	(-0.48)	(-0.48)	(-0.48)	(-0.45)	(-0.46)
D*2011							-0.012	-0.013	-0.013	-0.013	-0.024	-0.025
							(-0.25)	(-0.25)	(-0.26)	(-0.26)	(-0.49)	(-0.49)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3248	3248	3248	3248	3248	3248	3248	3248	3248	3248	3248	3248

	Non-White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	-0.0034	0.015	0.016	0.00066	0.019	0.0086	-0.00053	0.018	0.019	0.0034	0.021	-0.0014
	(-0.27)	(0.58)	(0.64)	(0.020)	(0.75)	(0.24)	(-0.025)	(0.56)	(0.61)	(0.094)	(0.65)	(-0.034)
D*2008							-0.013	-0.013	-0.013	-0.013	-0.011	-0.010
							(-0.47)	(-0.48)	(-0.46)	(-0.45)	(-0.38)	(-0.37)
D*2009							0.0061	0.0057	0.0059	0.0063	0.0068	0.0072
							(0.17)	(0.16)	(0.17)	(0.18)	(0.19)	(0.20)
D*2011							-0.0039	-0.0040	-0.0050	-0.0049	-0.0024	-0.0020
							(-0.11)	(-0.12)	(-0.15)	(-0.14)	(-0.069)	(-0.058)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	4223	4223	4223	4223	4223	4223	4223	4223	4223	4223	4223	4223

Source: PNADs 2007, 2008, 2009, and 2011.

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

Although drawing on a different method and country, these results are qualitatively similar to some evidence found for the US. Connolly and Gottschalk (2006) use ten years (1986 to 1996) of the Survey of Income and Program Participation, a panel that collected monthly continuous information on workers for a period of up to 48 months. They use this long panel to investigate whether the less educated gain less from returns to experience. According to their results, the returns to experience are higher for more highly educated workers regardless of the occupation.[119]

In this chapter, skin colour is used as a proxy for individuals' backgrounds, the characteristics of which might be difficult to observe, such as quality of school attended and other educational outcomes unavailable in the data. If white males hindered from working were to reallocate more time and effort towards education, one could then expect a higher return to experience for white males than their counterparts who were unaffected by the ban. For non-whites, on the other hand, one should not expect much difference in returns to experience between eligible and ineligible groups given that the reduction in participation rate was lower and, consequently, a smaller proportion of non-white males may have ended up studying more intensively.

The estimation of the average effect on the eligible group (ITT) is very informative from a policy perspective, but might be of limited interest if the ban had different effects in different quantiles of the wage distribution. The next section provides unconditional quantile treatment effects of the ban to check whether it had distributive effects. The objective is to deepen understanding of the impact of the ban, taking into account the asymmetry in wage distribution.

6.3 Distributive effects of the law

To estimate the distributive effects of the increase in the minimum legal age, the unconditional quantitle regression method proposed by Firpo et al. (2009) is used. The estimation of the unconditional quantile treatment effects takes advantage of the exogeneity of the 1998 law, and it consists of comparing the horizontal

[119] Brasterg and Terrell (1998) use several rounds of the National Longitudinal Survey of Youth to investigate whether the returns to experience are different between white and black workers in the US. They find that the return to experience is higher among whites, but the return to tenure is higher for blacks.

distance of two unconditional wage distributions (cumulative distribution functions) for any given quantile.

Table 9 presents the impact of the law on the wage gap of the two groups at different points of the unconditional hourly log wage distribution, assuming common time effects. The results suggest that the ban had a significant positive effect at the first decile of the hourly wage distribution for white males, but a large and negative effect for non-whites at the median of the hourly wage distribution. These results corroborate the ITT estimates. Under rank preserving conditions, the results indicate that the law led to an increase in earnings inequality among non-white males, a decrease in earnings inequality among white males, and a wider wage gap across race.

These results have to be linked to individual participation rates in the labour force. The drop in participation rate among white males was greater than among non-white males. White males were more likely to dedicate more time to school than non-white males, and the effect on the probability of a child banned from the labour force obtaining a college degree suggests that was the case. White males more than compensated less work experience with more education. The effect on wages for white males suggests that investing in education paid off. The results suggest that non-white males did not succeed in counterbalancing the lower stock of work experience that could be accumulated in the formal sector. According to the theoretical framework, this suggests that their parents either did not manage to save for the future or did not leave bequests to their offspring. Since the ban does not seem to have affected educational outcomes of non-white males, the lower median wages of banned non-whites could suggest either that employers do not value work experience in the informal sector as much as experience accumulated in the formal sector, or that the informal sector does not add as many skills to workers' human capital as does the formal sector. Unfortunately, the data do not allow one to investigate which of these two mechanisms is more plausible.

Whatever the mechanism, the bottom line is that the 1998 law put white males banned from the work force ahead of their peers, but put non-white males banned from the workforce behind. The intra-group wage gap increased slightly among white males and non-whites. The reason for the increase was distinct for these two groups, as per the discussion above. However, the ban sharply increased the inter-groups wage gap. The wage gap between banned white and non-white males is larger than it was before the ban.

Table 10 presents the QTE estimates with heterogeneous time effects. Most of the estimates are positive for whites and negative for non-whites. The coefficients for white males are positive and statistically significant at the bottom decile and first quartile of the hourly wage distribution. With regard to non-white males, there is an indication of a negative effect at the median of the hourly wage distribution, although the effects become larger and more precisely estimated in 2009. The results suggest that the returns to work experience (human capital) are positive for white males as long as the eligible group of white males receives higher wages despite having less potential experience. The returns to work experience are negative for non-white males.

These findings are somewhat similar to what Bratsberg and Terrell (1998) find in their study of the US economy. They use 12 years of the National Longitudinal Survey of Youth (1979 to 1991) to estimate returns to experience and job tenure for white and black workers. Their results indicate a higher return to general experience for white workers than black workers, but black workers experience higher returns to tenure than white workers.

Table 9 – Long Run QTE on Hourly Log Wages – White and Non-White Males

6 Months Bandwidth – Excluding School Attenders – Homogeneous time effects

	Q10	Q25	Q50	Q75	Q90
			White		
D	0.19**	0.15	0.14	0.23	0.20
	(2.04)	(1.54)	(1.28)	(1.42)	(0.82)
			Non-White		
D	0.027	-0.092	-0.24***	-0.054	0.18
	(0.39)	(-1.38)	(-2.88)	(-0.49)	(1.02)

Source: PNADs 2007, 2008, 2009, and 2011.

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

Table 10 – Long Run QTE on Hourly Log Wages –White and Non-White Males

6 Months Bandwidth – Excluding School Attenders – Heterogeneous time effects

	White					Non-White				
	Q10	Q25	Q50	Q75	Q90	Q10	Q25	Q50	Q75	Q90
D	0.22*	0.21*	0.16	0.18	0.097	0.092	-0.13	-0.23**	-0.039	0.10
	(1.75)	(1.81)	(1.33)	(1.03)	(0.41)	(1.08)	(-1.58)	(-2.52)	(-0.35)	(0.55)
D*2008	-0.023	-0.034	0.043	0.13	0.014	-0.020	0.064	0.022	0.038	0.093
	(-0.19)	(-0.33)	(0.41)	(1.02)	(0.087)	(-0.28)	(0.86)	(0.29)	(0.46)	(0.85)
D*2009	-0.054	-0.045	-0.0025	0.12	0.34*	-0.16**	-0.047	-0.17**	-0.17*	-0.094
	(-0.52)	(-0.45)	(-0.024)	(0.87)	(1.74)	(-2.38)	(-0.72)	(-2.30)	(-1.93)	(-0.73)
D*2011	0.013	-0.081	0.0040	0.057	0.13	-0.083	0.017	-0.0072	-0.057	0.14
	(0.13)	(-0.92)	(0.043)	(0.41)	(0.64)	(-1.45)	(0.29)	(-0.11)	(-0.63)	(0.94)
Dummies for years?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1966	1966	1966	1966	1966	2831	2831	2831	2831	2831

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5%m and 10% respectively

Using a different approach and PNAD data from 1996, Emerson and Souza (2011) show that, on average, the returns to experience tend to be lower than returns to education up to age 31. Given that the cohorts followed in this study are in their mid-20s, this seems consistent with the results for white males. However, the impact of the ban on the wages of the cohort of non-white males suggests that the returns to experience might be higher than the returns to education for individuals at the lower end of the wage distribution. Although returns to education are not provided here, they are unlikely to reach 20 percent. If this is the case, Emerson and Souza's (2011) findings may hold only for the subgroup of white males. Our estimates show that the impact of an early entrance into the formal labour force varies with the individual's socioeconomic background and along the unconditional distribution of hourly wage.

This finding has immediate implications for public policy. It shows that prohibiting households from sending young boys to the formal labour force at age 14 may not pay off if the returns to education for poor individuals who have to attend low quality public schools and carry on working informally might not be high. Conversely, returns to education might be high for better off males who face fewer constraints to attending high quality schools. Returns to experience might be more relevant to those from disadvantaged backgrounds.

The main findings of this chapter are also supported by theoretical predictions. Dessy and Knowles (2008) use a theoretical model to argue that a child labour ban can make the not-so-poor better off. However, their model shows that a ban can jeopardise the poorest households by reducing the total household income and children's opportunities for education. There is no evidence that the Brazilian ban reduced children's education in the short-run in terms of the distributions of completed years of schooling of the eligible and ineligible groups. On the other hand, the raw data shows that the ban reduced sharply the total household income of non-whites by 28 percent, but did not affect the household income of whites.[120] In that sense, at least for the group of non-white males, the ban seems to have affected household welfare through its impact on total household income.

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[120] This number was obtained by dividing the difference in average monthly wages between eligible and ineligible individuals by the household net income of the ineligible group. A T-test for difference in means shows that the difference in monthly wage for non-white males was -28.7 reais and statistically significant, but insignificant for white males. Over an average household net monthly income of 100.7 reais, this represents about 28.5 percent. The analysis considers a 6-month bandwidth.

7 ROBUSTNESS CHECK

In this section, the same regressions are re-estimated with a bandwidth of 3 months. The disadvantage of using a narrower bandwidth is that it increases the sampling variance and reduces the estimates' precision (power). The small sample size increases the chances of type II error, i.e., one might not be able to reject the null when it is false.

The eligibility dummy *D* is redefined so as to take the value of 1 if an individual turned 14 between October and December of 1998 and 0 if s/he turned 14 in the first three months of 1999. If the effect were very local, then one would expect a slightly higher impact in absolute terms. Table B.7 (Appendix 2, page 206) shows the ITT estimates for the impact of the law on the log of hourly wage. The reduction in precision results in statistically insignificant point estimates. Although qualitatively similar to those obtained with a larger bandwidth size, most of the estimates are statistically significant only against a one-sided alternative.

Tables B.8 and B.9 in Appendix 2 (pages 207-208) present the effects for the probability of an individual being employed and for the probability of having a formal job. The results for the labour force participation rate are very similar to those obtained with a larger bandwidth. There is no indication of impact on white males and some weak evidence of a negative effect on non-white males.[121] Conversely, unlike the estimates with a broader bandwidth, the results of Table B.8 strongly suggest that the law had a very local negative effect on the formal labour force participation rate of non-white males. Non-white males became about 12 percentage points less likely to participate in the formal labour force. These results reinforce the previous findings and suggest, once again, that the law negatively affected children from disadvantaged backgrounds.

Table B.10 (page 209) presents ITT estimates for college degree. The treatment effects on attaining a college degree are very similar to those reported in Table 7, but they are less precisely estimated in the interactive model.

Tables B.11 and B.12 (Appendix 2, page 210) show the QTE estimates with a narrower bandwidth. The point estimates are slightly lower and less precise. None of the estimates for white males are statistically significant. Although the negative impact at the median of the hourly wage distribution for non-whites remains, there is

[121] Some point estimates are statistically significant against a one-sided alternative.

an indication that the ban positively affected non-whites at the top decile of the wage distribution.

The heterogeneous effects presented in table B.12 are similar to those estimated with a 6-month bandwidth, with few coefficients for non-white males being statistically significant.

7.1 Placebo Test

This section presents a placebo test using the cohorts of individuals who turned 14 between January and December of 1999. Eq. (3) is re-estimated with the dummy *D* replaced by a placebo variable that takes on the value of 0 if the individual turned 14 between 1 January and 30 June 1999 and if s/he turned 14 between 1 July and 31 December 1999. Tables B.13 to B.16 (Appendix 2, pages 170-173) show the results for white and non-white males.

None of the coefficients of the placebo variable are statistically significant in tables B.13-B15 (Appendix 2, page 211-213. Table B.15 shows statistically significant coefficients for 2011. According to the estimates, white males are more likely to hold or be in pursuit of a college degree. Although the coefficients are significant for 2011, an F-test cannot reject the null that the coefficients for the impact of the placebo are jointly equal to zero.

The placebo estimates provide further support for the main long run effects of the ban of December 1998. The ban that hindered individuals from participating in the formal labour force at age 14 had heterogeneous and distributive effects, as it affected mostly the subsample of non-white males, particularly those at the lower end of the hourly wage distribution. The evidence suggests that the law resulted in a higher wage gap between white and non-white males, and probably resulted in a more concentrated earnings distribution by increasing the wage gap between those at the bottom and top of the earnings distribution.

8. CONCLUSION

This chapter investigated the long-run effects of a Brazilian law from December 1998 that increased the minimum legal age of entry into the labour market from 14 to 16. This chapter contributes to the scarce evidence of the long run effects of early participation in the labour force on adult outcomes. To my knowledge, this is the first study to provide long run causal estimates for the impact on the cohort affected by a change in the minimum legal age of entry into the labour market.

This chapter draws on Angrist and Krueger (1991) and explores dates of birth around the date the law was enacted to estimate local treatment effects. The results suggest that the law had heterogeneous effects across gender and race. Short-run estimates show that the law affected only boys, and long run estimates indicate that the law benefits white males but harms non-whites. Overall, the estimates indicate that white males prevented from entering the labour force at age 14 had better outcomes compared to those unaffected by the law. On the other hand, the estimates indicate that non-white males prevented from working at age 14 had worse outcomes in adult life compared to the comparison group.

The ITT estimates on wages were interpreted as lower bound for the returns to experience as long as the eligible and comparison groups have the same distribution of completed years of schooling, and estimates were obtained for non-school attenders. Unconditional quantile treatment effects were estimated to shed light on the distributive impact of the law. The results showed higher earnings for white males at the bottom decile of earnings distribution and negative effects for non-white males at the median of earnings distribution. Under rank preserving condition, this indicates that the law led to an increase in earnings inequality among non-white males, a decrease in earnings inequality among white males, and a wider wage gap across race. The results are robust to different bandwidth sizes and specifications of the smooth function.

The results for non-white males suggest that allowing this group to participate in the formal labour force at age 14 may pay off if the returns to experience overcome the returns to education. Thus, creating incentives for this group to enroll in the Brazilian apprenticeship programme may help these children accumulate experience in the formal labour force and achieve better long-term outcomes.

The results indicate that policy makers should take into account long run consequences of decisions on changes in law that can potentially have heterogeneous effects on individuals with distinct backgrounds.

CHAPTER 3: INTRAHOUSEHOLD EFFECTS OF A CHILD LABOUR BAN: EVIDENCE FROM BRAZIL

INTRODUCTION

The literature on the determinants of child labour has evolved considerably in the last few years, but too little is known about its consequences (Manacorda 2006; Beegle et al. 2005 and 2009). The majority of papers that look at the consequences of child labour emphasise the effects on children themselves (see Edmonds (2008) for a recent survey and chapter one for discussion). Empirical evidence of the effects of child labour on other household members is much less explored. This chapter uses the increase in the minimum employment age of December 1998 in Brazil as a source of exogenous variation in children's participation rate in the labour force to investigate how the ban affected the time allocation of other household members.

Uncovering causal effects of an intervention on intra-household allocation is a challenging task. The intra-household decision-making process is complex and, as shown by non-cooperative bargaining household models, it does not necessarily result in optimal allocations of scarce resources among household members (Doss, 1996). The empirical modeling of intra-household time allocation faces methodological challenges that are difficult to overcome with reduced-form regression models, such as the simultaneity problem embedded in collective household models (Vermeulen 2002; Strauss et al. 2000), and data constraints, as the hypothesis derived from those models can hardly be tested with cross-sectional household surveys unless one is lucky enough to identify a natural experiment that, say, affects the bargaining power of one of the parents without affecting households' total income (see Rangel 2006 for an example).

To test empirically either unitary household models – models that assume that households members pool their resources and allocate them optimally across household members either through a cooperative process or through some *benevolent dictator*[122] – or collective models that assume that households' resource allocation is

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[122] Gary Becker and Jacob Mincer are widely seen as the pioneers in developing unitary household models, but it was Becker's (1976) version of the unitary model that first used the idea of a benevolent dictator. For a review of unitary household models, see Grossbard (2010).

Pareto efficient – longitudinal data is often required unless one is willing to make strong assumptions (see Attanasio and Lechene, 2002 for an example).

The empirical literature has provided an increasing amount of evidence against unitary household models (see e.g. Attanasio and Lechene, 2002; Quisumbing and Maluccio, 2003; and Tommasi, 2015); however, little is known about which collective model best represents households' behaviour. Collective models suppose the existence of some vector of variables (also called *distribution factors*) that affect intra-household resource allocation, keeping preferences and household budget constants (Donni, 2008). In order to recover the *sharing rule* – the rule households follow to share resources among household members – the model requires a set of assumptions that can be better accommodated in structural models (Doss, 1996).[123] In fact, according to Doss (1996, p. 1603), 'it is not possible to test whether collective model, cooperative bargaining model, or non-cooperative bargaining model best characterizes the intra-household allocation of resources. What can be tested is whether a Pareto efficient outcome is attained'. In addition, testing household model longitudinal data on household members' consumption and labour supply would be ideal, even in a context of a randomised controlled trial (see Attanasio, Meghir and Santiago, 2011 for an example), otherwise the model would have to rely on too many assumptions (see e.g. Attanasio and Lechene, 2002).

The number of empirical studies testing a particular intra-household model has grown steadily over the years (e.g. Blundell et al. 2005; Blundell et al. 2007; Apps and Rees 1997; Couprie 2007; Macours et al. 2012; Tommasi, 2015). However, Attanasio and Kaufmann (2014) argue, the great majority of household models treat children as public goods instead of potential decision-makers.

It is not the intention of this chapter to contribute to the theoretical literature of intra-household models. Instead, this chapter follows Rubino-Codina (2010) who use a unitary household labour supply model to frame the problem under investigation through the lens of economic theory. Although the model does not provide straightforward testable hypotheses, it uses standard consumer theory to decompose the effect of the ban into a cross-substitution effect and an income effect,

[123] As discussed in Doss (1996), the cooperative bargaining model and unitary household models are nested in more general collective models.

and in doing so it allows one to check whether the labour supply of children banned from the labour force and other household members are substitutes or complements.

If one is willing to assume that children and adults are substitute inputs for firms, an assumption usually made in theoretical models of child labour (Basu and Van, 1998; Baland and Robinson, 2000; Horowitz and Wang, 2004; Basu, 2005), then the results will provide suggestive evidence on whether households allocate labour based on some household production function and whether there is some degree of substitutability and complementarity between children's and adults' labour supply to produce home and market goods. To do so, this chapter digs into the consequences of the law of 1998, taking into account dynamics that might prevail in single and couple parent families. Looking at different family compositions can uncover different choices regarding time allocation of household members, particularly if households face market imperfections such as credit or liquidity constraints (Ranjan, 1999; Baland and Robinson, 2000; Horowitz and Wang, 2004).

This chapter contributes to the literature on child labour bans in different ways. First, most of the papers investigating links between children's outcomes and intra-household allocation, such as Duncan (1990 and 1994), Bhalotra (2004), Oreopoulos et al. (2006c), Emerson and Souza (2003 and 2007), and McCrary and Royer (2011), examine the impact of parents' inputs, ranging from education to participation in the labour market during their childhood years, on their children's human capital related-outcomes, such as participation in child labour and school attendance. This chapter does the opposite, as it looks at the effect of an enforced change to child labour on other household members, particularly younger siblings and parents. Second, it explores different household compositions, seeking to better understand whether a policy of this kind affects 'insured' and 'uninsured' households differently—that is, households in which the mother is single ('uninsured') or a spouse ('insure') (see Gruber and Culler, 1996 for a similar interpretation). Third, this is the first study to use regression discontinuity design (RDD) to investigate the impact of a child labour ban on younger siblings and parents. Apart from being widely regarded as the quasi-experimental method that most resembles an experimental design (Cook 2008; Green et al. 2009), in the present study RDD provides a straightforward means of identifying between-household effects caused by the change in the law.[124]

I am only aware of two other papers that provide estimates of the impact of child labour on other household members. One of these is Manacorda (2006), who uses the US Census of 1920 to investigate the impact of an exogenous increase in the labour force participation rate of children just above the minimum legal age on time allocation of household members. He draws on exogenous variations in child labour caused by different minimum legal ages of entry into the labour force across states. Using child labour laws as an instrument for labour force participation rate, Manacorda (2006) estimates the 'spillover effects' of child labour, looking at what happened to the time allocation of younger siblings and parents by the time at least one child in the household became eligible to work. His findings show that the increase in the participation rate of children eligible to work had positive effects on siblings (lower participation rate and increased school attendance) but no impact on parents.[125]

Braradwaj et al. (2013) investigate the effectiveness of the child labour ban in India through the Child Labour Act of 1986 that set the minimum legal age of entry into the labour market at 14. Using data from employment surveys before and after the law and in two different sectors, Braradwaj et al. apply the difference-in-differences technique to check the impact of the law on the extensive margin of children affected by the ban and on their parents and siblings. Their findings suggest that the law increased child labour and reduced wages. They also find an increase in

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[124] Manacorda (2006) had to disentangle the *within* and *between* household effects of the child labour ban in the US, because in his identification strategy children in the same household could be in the treatment and control groups; therefore, estimating the impact of minimum legal age without taking this issue into consideration would render a composite effect. Manacorda uses the proportion of children per household above the minimum employment age (thus, eligible to work) as an instrumental variable to participation rate to circumvent this issue, enabling *between* household estimates.

[125] Although Manacorda's (2006) results are precisely estimated, it is unclear why he was unable to use regression discontinuity design to compare the outcomes of individuals close to the age threshold. Instead, he uses difference-in-differences and instrumental variable techniques, exploring variations across states and time. His identification strategy depends on different minimum legal ages across states; if states with the highest incidence of child labour decide to adopt stricter rules and/or move more quickly in adopting the law, then the law would be an invalid instrument. It would be directly correlated with the incidence of child labour at state level. Note that regression discontinuity design would circumvent this issue, because it does not require an exclusion restriction. For more on this point, see Lee and Lemieux (2009).

the participation rate of siblings aged 10 to 13, particularly girls, and a reduction in school attendance.[126]

This chapter draws on Manacorda (2006) but widens the understanding of the intra-household effects caused by a child labour ban by covering a broader set of outcomes. To my knowledge, this is the first study to investigate the effects of a child labour ban on the extensive and intensive margins of parents' labour supply, exploring labour force status of parents and different family compositions.[127] By looking at different single and couple parent families and parents' status in the labour force, this chapter sheds light on the potential mechanisms underlying the decision-making process within households facing different constraints.

The main result of the chapter is the finding that the intra-household impact of a ban can differ remarkably according to family composition. For couple parent households, it is shown that mothers become more likely to participate in the formal labour force, fathers work slightly fewer hours per week, whereas younger siblings became less likely to work. These results support the hypothesis that mothers' labour supply in couple parent families can be used as an imperfect insurance mechanism, but alternative explanations cannot be completely ruled out.

For single parent households, it is found that single fathers shift from the formal to the informal sector, whereas younger brothers become more likely to attend school. I interpret this result as an indication of a binding liquidity constraint and that single fathers may privilege younger brothers based on assumptions regarding the returns to education, as in the Horowitz and Wang (2004) model discussed in chapter two. Since brothers banned from the formal labour force did not shift to the informal sector, it might be the case that they ended up helping with household chores so that

[126] Although anchored in a theoretical model, the above results are counter-intuitive and difficult to reconcile. There are various possible issues that would call these results into question. First, the assumption of parallel trends would be unlikely to hold, as the comparison is made between youth working in the manufacturing and agricultural sectors. Another explanation could stem from the age of the groups considered in the analysis. Rather than focusing on individuals close to the threshold age (14) and then using discontinuity in exact date of birth, the study compares children aged 12 to 13 against those aged 14 to 15. In addition to being unable to show parallel trends for these two age groups due to a lack of pre-ban data, the authors are also unable to use regression discontinuity or explore the impact of the law on the intensive margin of children's labour supply.

[127] Due to data restrictions, Manacorda (2006) offers no evidence of the impact of US child labour legislation on the intensity of parents' labour supply.

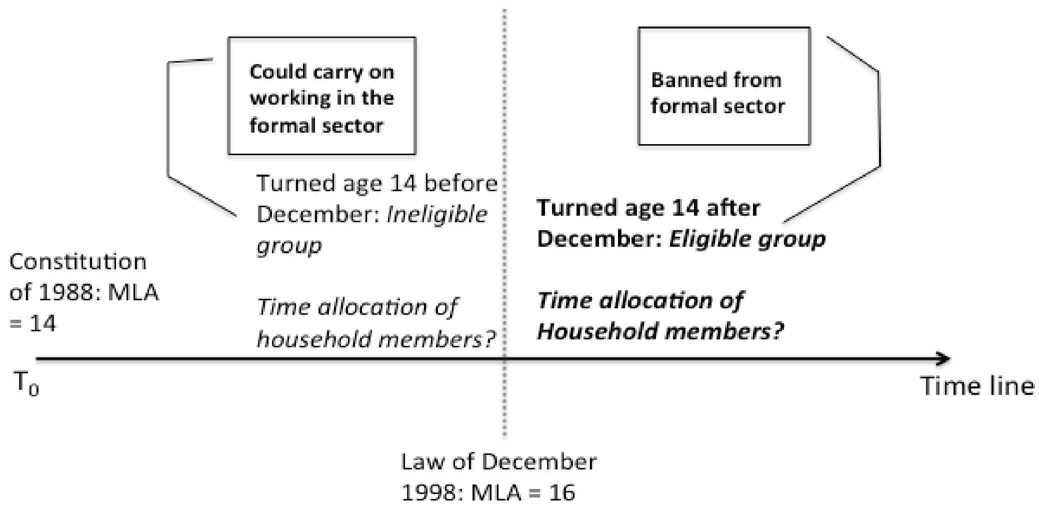
the younger brother could go to school.[128] The robustness check and placebo tests confirm most of these findings.

2 THE INTERVENTION: THE LAW OF DECEMBER 1998

As discussed in chapter one, the Brazilian Constitution of 1988 set the minimum legal age of entry into the labour market at 14, and from 1988 to mid-December 1998, the minimum legal working age in Brazil was 14. Individuals under the age of 17 were prohibited from working in hazardous activities.

Constitutional Amendment No. 20, enacted on 15 December 1998, increased the minimum legal age of entry into labour market from 14 to 16. With the change in the law, children who turned 14 after the law passed were hindered from participating in the formal labour force, whereas children who turned 14 before the law passed were not. In other words, individuals with very close ages were exogenously split into two groups: those who had their labour force status unaffected by the law (ineligible or control group) and those banned from participation in the (formal) labour force by the law (eligible or 'treatment' group).[129] The figure below illustrates the change in the minimum legal age (MLA) and its effect on individuals turning 14 just before and just after its enactment.

Diagram 1 – The Change in the Minimum Employment Age



[128] Chapter one finds no effect on domestic work for boys and girls, but most of the estimates for boys are positive though statistically insignificant.
[129] Chapter one provides a more detailed discussion of the law.

As shown in chapter one, participation rate in the labour force as a whole (regardless the sector) dropped only among boys and mostly in the informal sector. In this chapter I use the exogenous variation in the labour force participation rate (LFPR) (formal and informal) of 14-year-old boys to investigate its consequences on time allocation of other household members.

3 THEORETICAL BACKGROUND

Rubino-Codina (2010) developed an intra-family model to test the impact of *Oportunidades* (previously named *Progresa*), a conditional cash transfer (CCT) program in Mexico, on household members' labour supply. She interprets the transfer as a positive income shock and allows the conditionalities to affect the relative price of school and work of eligible children in the household.

The model is an extension of Ashenfelter and Heckman (1974) and a different version of Newman and Gertler (1994). Ashenfelter and Heckman's (1974) concern was how shocks in the labour market (change in wages or unemployment) experienced by either the husband or wife affected the spouse's labour supply. The model focused on couples and ignored any other household members. This model can be seen as a first version of what Lundberg (1985) calls the 'added worker effect,'—that is, an increase in labour force participation rate of wives when husbands experience a negative shock in the labour market. Rubino-Codina (2010) follows Newman and Gertler (1994) and adds children to that framework. By doing so, she permits that children contribute to the total household income and to producing both home and market goods. In her setting, the labour supply of children and adults can be substitutes or complements in producing those goods.

The model considers a household *h* consisting of $i = 1, \dots, I$ members, in which $a = 1, \dots, A \leq I$ adults, $q = 1, \dots, Q \leq I$ children, and $k = 1, \dots, K \leq Q$ children eligible to participate in the CCT. Thus, $A + Q = I$ and $K \subset Q$.

The utility function is given by:

$$U = U(L_1, \dots, L_I, C; X, \varepsilon), \qquad (1)$$

where *C* is the total household consumption (expenditure), L_i is the leisure time of individual *i*, *X* is a vector of observed heterogeneity among household members, and ε is a vector of unobserved heterogeneity among household members.

Each household member is endowed with a total time *T* that can be allocated into three activities: market (m), farm (f), and domestic (d). Children can also allocate time into schooling (s). The household faces the following budget constraint:

$$\sum_i \sum_{j \neq s} W_i^j H_i^j + Y \geq PC + \sum_{i=q} W_i^s H_i^s, \qquad (2)$$

where H_i^j is the time individual *i* allocates to activity *j* and W_i^j is the marginal return (shadow price) of that activity. The price of schooling W_i^s includes fees, books, transportation costs, etc. *P* is the price of the composite consumption good, and *Y* is the non-labour income.

The *I* time constraints are given by:

$$\sum_j H_i^j + L_i = T, \forall i, j \qquad (3)$$

The *I* constraints can be re-written in terms of L_i and then replaced in the utility function. The household problem then consists of finding *C* and the time each household member allocates to each activity J that maximises the household utility function given the budget constraint. Solving the first order conditions one obtains the following Marshallian demand functions:

$$C = C(W, P, Y; X, \varepsilon), \qquad (4)$$

$$L_i = L_i(W, P, Y; X, \varepsilon), \qquad (5)$$

where $W = \left(W_1^m, \dots, W_I^m, W_1^f, \dots, W_I^f, W_1^d, \dots, W_I^d, W_1^s, \dots, W_Q^s\right)$.

Replacing L_i in the *I* time constraints, one obtains the labour supply of individual *i* in activity *j*:

$$H_i^j = H_i^j(W, P, Y; X, \varepsilon), \forall i, j \qquad (6)$$

Rubino-Codina shows that the effect of the program ($\sum_k dW_k^s, dY$) on time spent on activity *j* by each household member *i* in a treated household is given by[130]

$$dH_i^j = \sum_k \frac{\partial H_i^j}{\partial W_k^s} d\, W_k^s + \frac{\partial H_i^j}{\partial Y} dY, \forall i, j \qquad (7)$$

To get the total effect of the program on labour supply of individual *i* to activity *j* she uses the Slustky equation,

$$dH_i^j = \frac{\partial \hat{H}_i^j}{\partial W_i^s} dW_i^s + \sum_{k \neq i} \frac{\partial \hat{H}_i^j}{\partial W_k^s} d\, W_k^s + \left[-\sum_k H_k^j d\, W_k^s + dY\right] \frac{\partial H_i^j}{\partial Y}, \forall i, j \qquad (8)$$

[130] Where $\sum_k dW_k^s$ is the reduction in the price of schooling given the educational grant and dY is the increase in non-labour income. The model assumes that there is more than one eligible child per household; otherwise, the reduction in price of schooling would be dW_k^s. Note that $dW_k^s < 0$ as it represents a reduction in price of schooling.

where $\widehat{H}_i^j = H_i^j(W, P, U; X, \varepsilon)$ is the Hickisian (utility compensated) labour supply function.

The first term on the right hand side of (8) is the own-substitution effect of the program – the extra time individual i=k spent on activity j –, the second term is the cross-substitution effect – the extra time individual $i \neq k$ spent on activity j given the grant received by k children in the treatment household – and the term in brackets is the total income effect.

If one assumes that work activities are inferior goods and that schooling is a normal good, then $\frac{\partial H_i^j}{\partial Y} < 0$ for any household member other than school-aged children, and $\frac{\partial H_i^s}{\partial Y} > 0$ for school-aged children. This implies a negative total income effect for other household members and a positive effect for school-aged children.

To derive the sign of the own-substitution effect, Rubino-Codina assumes (i) that schooling is an ordinary good so that $\frac{\partial \widehat{H}_i^s}{\partial W_k^s} < 0$, that is, a reduction in the price of schooling will increase time allocated to schooling activity, and (ii) that schooling and any other work activity J are substitutes, so that $\frac{\partial \widehat{H}_i^j}{\partial W_k^s} > 0$. With these assumptions, one can know the direction of own-substitution effect and income effect. For school-aged children, the own-substitution and total income effects would be positive, whereas for other household members the total income effect would be negative. Note that the model makes an implicit assumption that the grant is large enough to circumvent any credit market imperfection so that the 'treated' children no longer need to work.

As discussed in Rubino-Codina (2010), the overall effect of the program depends on the cross-substitution effect, and its sign depends on whether schooling and work of a school-aged child and other household members are substitutes or complements. For instance, whereas the total income effect predicts a decrease in participation rate and hours worked for all household members, for school-aged children one could still expect an increase in schooling among non-beneficiary children in a treated household. However, whether non-beneficiary children will dedicate more time to schooling or any other work activity depends on whether schooling and work between beneficiary and non-beneficiary children in treated households are substitutes or complements. The total effect of the grant on the labour supply of non-beneficiary children in the household therefore depends upon the sign and magnitude of the cross-substitution effect. Rubino-Codina (2010, p. 228) concludes that 'the total effect on schooling demand and labor supply depends on the magnitude and direction of the different effects at play and remains an empirical question.'

Adapting Rubino-Codina's framework for the problem at hand, a modified version of the Slutsky equation described above can be derived. First, since my sample includes only urban households, farm activity can be dropped from the model. Second, there is only one 'eligible' child per household. Third, non-labour income effects can be fully ignored. Fourth, the data I use do not inform the time spent on both school and domestic work (household chores).[131] Fifth, the ban of 1998 does not have any type of conditionality; hence, there is no reason to expect an increase in demand for schooling among children banned from the work force.

In fact, if the ban is seen as a negative income shock, the own-substitution effect should be zero. Thus, if one replaces dW_k^S for dY_k, where Y_k is the forgone income of child *k* banned from the formal labour force, equation (8) can be re-written as:

$$dH_i^j = \sum_{k \neq i} \frac{\partial \hat{H}_i^j}{\partial Y_k} d\,Y_k - \hat{H}_k^j \frac{\partial H_i^j}{\partial TI} dY_k, \tag{9}$$

where TI is total household income and $\hat{H}_k^j = \frac{\partial TI}{\partial Y_k}$ by Shephard's lemma. If work activities are inferior goods, the total income effect should thus be positive, as $dY_k < 0$ and $\frac{\partial H_i^j}{\partial TI} > 0$. In other words, with the decrease in total household income, one would expect an increase in the individual's $i \neq k$ participation rate in the labour force and hours worked in activity j. As with the original model, the total effect of the ban on individual's *i* labour supply would depend on the sign and magnitude of the cross-substitution effect. With a few additional assumptions, one can try to back out of the household production function to get a better idea of whether time allocation of household members is driven by some (plausible) efficiency criterion.

3.1 EXPECTED RESULTS

[131] Rubino-Codina (2010) uses the residual of the total hours spent on school and in work activities, including household chores, to compute time allocated to leisure.

The household production function and the household decision-making process might be very different in single and couple parent families. In couple parent families, for instance, mothers' labour supply may be undersold in the market. Thus, if the labour force participation rate of mothers is relatively low compared to fathers, couple parent households could respond to negative income shocks such as children's forgone income, adjusting mothers' labour supply at the extensive margin. Mothers' labour supply could thus be used as an imperfect risk-coping mechanism to deal with such shocks. It is also plausible to expect different effects of the ban on younger siblings in single and couple parent households.[132] One can argue that the head of a single parent family would supply labour more inelastically than the head in of a couple parent family, and almost surely more inelastically than the spouse in a couple parent household.

For couple parent families, one could expect most of the impact of the ban to be absorbed by the parents, particularly mothers, since about 93 percent of household heads in couple parent households are male (see Table C.3) and the participation rate of women is relatively low (43.6 percent). Therefore, one could expect to see an impact on the mother's labour supply, mainly at the extensive margin.

According to Basu and Van's (1998) *luxury axiom*, altruistic parents always prefer to buy children's leisure rather than sending them to work if they can afford to do so. Thus, one could question why mothers decide to stay home and send their sons to the labour force in the first place. The luxury axiom evokes equity minded parents, but depending on the household production function, it could be that in couple parent households mothers are thought to have a comparative advantage in doing household chores and consequently prefer having teenaged boys working outside the home. It is also possible that some parents believe that male teenagers may have some comparative advantage in performing low-skilled paid work in the labour market, a decision that would be consistent with the view of Horowitz and Wang (2004) discussed in the previous chapter.[133]

[132] See Gruber and Culler (1996) for a similar interpretation in the context of the US. Lundberg (1988) finds that parents' hours worked are simultaneously determined when there are young children in the household. She shows that a wife's hours of work have a positive effect on a husband's labour supply, regardless of the number of children under age 6. However, a husband's hours of work have a negative effect on a wife's hours when there is only one child under age 6.
[133] According to the model, credit constrained parents favour the most 'talented' with higher investment in education, sending the less 'talented' to labour market.

The question then becomes whether banning children under age 14 from the formal labour market triggered a relocation of time of other household members or simply implied a reallocation of time of children directly affected by the ban.

If parents care about (a) the type of work performed by their children, (b) the extra time they would have to work in the informal sector to keep monthly household income more or less constant, and/or (c) some stigma effect that could be attached to having their children working in the informal sector, then a ban can actually reallocate mothers' labour supply towards paid work. This reallocation of mothers' labour supply can be also supported by an argument based on efficiency gains. For instance, one could argue that mothers would enter the labour force since they could work in the formal sector and have higher earnings than young children working informally. Children banned from the labour force could spend more time doing household chores, such as looking after their younger siblings.

For single parent families the story might be very different, since households cannot use spouses' labour supply to help smooth the shock. The shock will have to be almost fully absorbed by the household head – and probably by older daughters through more time allocated to household chores.[134] The Brazilian PNAD 1999 does not have information on time spent on household chores and older siblings are not covered in the empirical analysis, but I expect most of the cost will accrue to the household head, particularly in cases in which they can afford to consume children's leisure.

4 IDENTIFICAITON STRATEGY

This chapter applies RDD to estimate the local effect of an exogenous variation in children's participation in the labour force on time allocation of younger siblings and parents. By relying on the discontinuity in the labour force participation rate of 14-year-old boys, local intent-to-treat estimates can be obtained by comparing outcomes of younger siblings (or parents) whose brother (or son) was 14 years of age

[134] Using data from Nepal, Edmonds (2006) shows that having younger siblings increases older sisters' hours worked for household chores. Older boys work extra hours per week in paid work in the presence of younger brothers, but not in the presence of younger sisters. However, Edmonds shows that the effects depend both on the household size (number of siblings) and the age gap between the oldest and youngest siblings.

just before and just after the law was passed. This method provides a sharp empirical strategy for the estimation of the *between*-household effect of child eligibility.[135]

In the RDD context, the identification of the local treatment effect requires a clear-cut assignment rule. Once this condition is satisfied, the assumption is that, on average, individuals just on the right and just on the left of the cut-off point will have, in statistical terms, identical observed and unobserved characteristics; the only difference between them is that one group can take up the treatment while the other cannot.[136]

Although the RDD only identifies the local average treatment effect—the treatment effect for the individuals close to the cut-off—Hahn et al. (2001), van der Klaauw (2008), Imbens and Lemieux (2008), and Lee and Lemieux (2009) note that that this method has many advantages compared to other quasi-experimental approaches. RDD is less dependent on functional form assumptions and does not require identifying instruments—particularly for narrow bandwidth—or the vector of observed variables that determines the eligibility of units for the treatment. Lee and Lemieux (2008) also argue that unlike the instrumental variable estimator, RDD does not require exclusion restriction, since the forcing variable is allowed to have a direct effect on the outcome.[137]

With the law of 15 December 1998, individuals who turned 14 before the ban could still participate in the formal labour force, whereas those who turned 14 after the law was passed were hindered from doing so. Since the 1998 law precludes the participation of individuals under age 16 – as long as they turned 14 after the ban – in formal occupations, individuals affected by the law had to drop out of the formal labour force or shift to informal occupations.

The law gave rise to a fuzzy design, as some individuals may have dropped out of the labour force while others moved or carried on working in the informal sector. The short-run impact of the law on household members is estimated on the following outcome variables of siblings: (i) the labour force participation rate as a

[135] Manacorda (2006) identifies the *within* and *between* household estimates. In the present case, none of the households in the sample have more than one child affected by the law.
[136] In the fuzzy design there is imperfect compliance, as eligible individuals are given the final decision to participate or not in the intervention. In the case of sharp design, the compliance is perfect since the take-up is a deterministic function of the forcing variable. See below.
[137] For the identification of the local average treatment effect (LATE) under the fuzzy design, the monotonicity condition needs to hold, i.e., the take up among the eligible group has to be higher than the take up among the ineligible group.

whole (LFPR), (ii) household chores, (iii) school attendance, and (iv) completed years of schooling. For parents I look at: LFPR, LFPR disaggregated between formal and informal sectors, and weekly hours worked.

As in chapter one, LFPR takes the value of 1 if an individual worked in the week of reference, if s/he worked in the last 12 months, and if s/he was an active worker in the week of reference but was prevented from working due to external causes and zero otherwise. Household chores takes the value 1 if the individual worked did some domestic work, such as cooking and cleaning, in the week of reference and zero otherwise. School attendance takes the value of 1 if a child attended school in the week of reference and zero otherwise.

The effect of the law on other household members can be estimated as follows:

$$y_{kj} = \beta_0 + \beta_1 D_{ij} + h(Z_{ij}) + \varepsilon_{ij} \tag{1}$$

$$P_{ij} = \delta_0 + \delta_1 D_{ij} + h(Z_{ij}) + \mu_{ij} \tag{2}$$

where y_{kj} is the outcome variable of individual *k* (sibling or parents) of household *j*, D_{ij} is a dummy variable that takes on a value of 1 if individual *i* of household *j* turned 14 after the law passed and 0 if s/he turned 14 before the law passed. This variable captures individual *i* eligibility status to participate in the labour force on outcomes of his/her siblings and parents. The smooth function $h(Z_{ij})$ depends on the forcing variable *Z* (age) of individual *i* of household *j*. Variable *Z* is defined in weeks and takes on a value of zero for individuals who turned 14 on the last week of December 1998, 1 for individuals who turned 14 in the first week of 1999, and so on.

Eq. (1) is the reduced-form equation, as it provides the effect of the eligibility status of individual *i* on siblings' and parents' outcomes rather than the impact of actual treatment. The coefficient β_1 corresponds to the *intent-to-treat* (ITT) estimate. Given the relatively narrow bandwidth sizes used, the estimate remains local. Note that this coefficient will inform whether labour supply of children from the labour market and other household members are substitutes or complements.

Using the effect of the actual participation rate of 14-year-old children on other household members would very likely result in biased estimates. This could be either because siblings and parents allocate their time together, or because time allocation of household members is affected, for instance, by unobserved characteristics such as parents' preferences for work and school and children's innate skills. Because the law exogenously affects individual eligibility status in the labour market, eligibility status can be used as an instrument for actual participation and deal with problems of self-selection into the labour force.

Eq. (2) models the probability that individual *i* of household *j* participates in the labour force, P_{ij}, as a function of a constant, the eligibility dummy, the smooth function, and a stochastic error term. Eq. (2) provides the first stage, that is, the effect of the law on the participation rate of individual *i*. The local average treatment effect (LATE) of the law on outcomes of individual *k* of household *j* can be obtained by dividing the reduced-form estimate β_1 by the participation rate of individual *i* predicted in Eq. (2), δ_1. As mentioned above, I focus on local ITT estimates, because if time allocation of household members is a result of a simultaneous decision making process this could invalidate the instrument.

With binary outcomes, equations are estimated with the linear probability model. With censored outcomes, such as weekly hours worked, a Tobit model is used instead. Since members of the same household are likely to allocate time taking into account the time constraints of other household members, standard errors will be clustered at the household level.

To check whether the law had heterogeneous impact, estimates are provided for younger brothers and sisters and parents (mother and father) in single and couple parent families. I look at different family compositions to try to better understand the intra-household decision-making response to the 1998 law that prevented 14-year-old boys from participating in the labour force. I also try to shed light on potential liquidity constraints by exploring the labour force status of parents. Because I will split the sample according to family composition, I use a larger bandwidth of 52 weeks. However, to check robustness, estimates are also provided with a bandwidth of 20 weeks.

5 DATA AND DESCRIPTIVE STATISTICS

This chapter uses the 1999 PNAD to estimate the short run effects of the child labour ban on household members.

Table 1 – Sample Composition – Households with at Least One Parent – Urban Area Only

	Frequency	Percent	Cumulative Percent
One parent	36,999	31.43	42.48
Single Mother	*22,461*	*73.82*	*73.82*
Single Father	*14,538*	*24.32*	*100*
Two parents	80,772	68.57	100
Father Head	*72,757*	*83.35*	*83.35*
Mother Head	*7,965*	*26.18*	*100*
Total	*117,721*	*100*	

Source: PNAD 1999.

The sample used in this chapter excludes from the analysis households with no parents present, households with multiple families, and households without at least one child.[138]

Table 2 splits the sample into households with a son affected by the ban ('treated') and households with a son unaffected by the ban. Estimates are obtained with a year bandwidth. Both tables one and two show that the great majority of single parents are female, and when both parents are present the head is usually the father.

Table 2 – Sample Composition – 'Treated' and 'Control' Households with at Least One Parent – Urban Area Only

	Frequency	Percent	Cumulative Percent
One parent	2,332	42.48	42.48
Single Mother	*1,428*	*80.54*	*80.54*
Single Father	*904*	*24.32*	*100*
Two parents	3,158	57.52	100
Father Head	*2,813*	*75.68*	*75.68*
Mother Head	*345*	*19.46*	*100*
Total	*5,490*	*100*	

Source: PNAD 1999.

The sample used in the empirical exercises is even smaller, as it considers only 'treated' and 'control' households with at least one parent and at least one

[138] It is important to mention that the PNAD of 1999 does not identify married couples. I defined couples if the head and spouse live in the same household. However, couples in stable relationships that do not share the same household are considered single. The definition used here will therefore underestimate the number of couple parent families and overestimate the number of single parent families. Interestingly, the official statistics show that since the early 2000s, the proportion of single parent families has been following an upward trend in Brazil, with the number of married couples declining monotonically. For more information, see www.ibge.gov.br.

younger sibling. The final starts with 2,420 households, 47 percent of which are single headed families with two-thirds of headed by women. About 80 percent of couple parent families in the final sample is headed by men.

Tables 3a to 3b show the mean, standard deviation, and t-test for the difference in means for two samples of younger siblings and household head: one with a brother (son) who turned 14 just before the ban (ineligible group) and another with a brother (son) who turned 14 just after the ban (eligible group). The sample excludes households in rural areas, because the law might not be as well enforced in rural as in urban areas, and most of the outcomes are likely to change if households have access to better infrastructure, such as schools, and if there is a more active labour market.[139]

Unlike chapter one, which includes all samples of 14-year-old children, the sample used in this chapter consists of 14-year-old children who have at least one parent present in the household and excludes households with multiple families.[140] The analysis concentrates on siblings aged 10 to 13 and parents aged 30 to 60. The selection of this subsample of siblings stems from the fact that in urban areas school attendance approaches 100 percent among children under age 10, whereas the labour force participation rate is close to zero, although some children do household chores. Note that focusing on siblings aged 10 to 13 minimises the potential effects of school entry ages on parents' labour supply, as in 1999 it was mandatory for children turning 6 by 30 June of the current year to be enrolled in school in Brazil; therefore, having children aged 6 in the sample could confound the estimates on the labour force participation rate of mothers (see Berlinski et al. 2011).

Table 3a shows the samples of younger siblings with a bandwidth of 52 weeks. The samples seem very similar in terms of observed characteristics (the list of covariates in the table) with the null hypothesis of equal means being rejected in two cases only. Even in those cases, the difference in means is not large. It is also interesting to observe that the difference in means detects almost no difference in the outcomes. From this simple test, there is an indication that the law did not affect younger siblings.

[139] Also, rural households are underrepresented in the PNADs.

[140] About 5 percent of 14-year-olds in the PNAD 1999 have both parents absent, whereas 9 percent of 14-year-olds live in households with multiple (more than one) families. See table C.1 in the appendix.

Table 3a – Descriptive Statistics and Difference in Means
Younger siblings aged 10 to 13 with a brother aged 14 around December 1998
52 weeks bandwidth

	Siblings with older brother non-affected by the law (14 before Dec 1998)		Siblings with older brother affected by the law (14 after Dec 1998)			
	Mean	SE	Mean	SE	Difference	Clustered T-statistic
Outcomes						
Labour force participation rate	0.04	0.18	0.03	0.16	0.01	(0.94)
Domestic work	0.68	0.47	0.66	0.47	0.02	(0.74)
School attendance	0.97	0.17	0.97	0.18	0.00	(0.36)
Years of schooling	3.40	1.58	3.09	1.46	0.31***	(3.64)
Covariates						
White	0.43	0.50	0.43	0.50	0.00	(-0.01)
Male	0.49	0.50	0.47	0.50	0.02	(0.72)
Single Parent Families	0.45	0.50	0.44	0.50	0.01	(0.21)
Head's years of schooling	5.98	4.17	5.69	4.18	0.29	(1.23)
Head's age	41.71	5.96	41.34	6.44	0.36	(1.03)
Metropolitan region	0.64	0.48	0.67	0.47	-0.04	(-1.39)
Household size	5.64	1.75	5.81	1.68	-0.18*	(-1.84)
Household Income (net of children's income)	504.69	646.57	574.11	776.24	-69.42*	(-1.73)
Observations	*619*		*630*			

Source: PNAD 1999. *** Statistically significant at 1%.

Table 3b – Descriptive Statistics and Difference in Means
Household head aged 30 to 60 with a son aged 14 around December 1998
52 weeks bandwidth

	Household head with a son non-affected by the law (14 before Dec 1998)		Household head with a son affected by the law (14 after Dec 1998)			
	Mean	SE	Mean	SE	Difference	Clustered T-statistic
Outcomes						
Labour force participation rate	0.81	0.39	0.82	0.38	-0.01	(-0.58)
Participation rate – formal labour force	0.57	0.49	0.56	0.50	0.02	(0.69)
Participation rate – informal labour force	0.14	0.35	0.18	0.38	-0.04**	(-2.04)
Weekly hours worked	45.18	13.27	44.60	12.63	0.60	(0.97)
Covariates						
Age	42.97	6.48	42.39	6.64	0.58**	(2.14)
White	0.51	0.50	0.51	0.50	-0.005	(-0.24)
Years of schooling	6.75	4.23	6.75	4.30	-0.003	(-0.01)
Metropolitan region	0.66	0.47	0.69	0.46	-0.028	(-1.43)
Household size	4.78	1.49	4.82	1.42	-0.037	(-0.62)
Household Income (net of children's income)	667.63	936.09	723.48	1079.80	-55.850	(-1.33)
Observations	*1038*		*1107*			

Source: PNAD 1999. *** Statistically significant at 1%.

Table 3b shows descriptive statistics and difference in means for household heads using a 52 weeks bandwidth. As with the sample of younger siblings, the sample seems well balanced around the threshold. The t-test suggests that the law affected the participation of the household head in both the formal and informal sectors. Robustness checks with 20 weeks bandwidth are shown in Tables C.1 and C.2 (Appendix 3, page 218-219).

It is important to mention that I observe in the data a high number of missing values for the dummy that identifies whether the worker is in the formal or informal sector – about 33 percent of household heads in a sample with 52 weeks bandwidth did not respond whether s/he was a registered (formal) worker. The percentage is slightly higher among male heads. This is expected, since male heads account for 69 percent of the heads in the sample.

The balanced sample around the threshold indicates that the law can be seen as a natural experiment so that the comparison of outcomes of these two samples of households can be interpreted as a local causal impact of the law on household members. For the effect of the ban on household members to have a causal interpretation, one requires that the groups be balanced in terms of unobserved characteristics.

With a bandwidth of 52 weeks, about 15 percent of children in the comparison group are 15 years old by the time the survey was collected, and some can argue that the Compulsory Schooling Law may therefore confound the impact of the ban.[141] To check whether that might be the case, I first compare the difference in labour force participation rate, school attendance rates, and completed years of schooling between control children aged 14 (who turned 14 before the ban) and 15. There is no statistically significant difference in labour force participation rate and school attendance between these two groups. The p-value for a t-test of difference in proportions is equal to 0.34 and 0.69 respectively. In terms of completed years of schooling, children aged 15 have, on average, 0.12 more years of schooling than their younger peers, and this difference is statistically significant at the 1 percent level. Children aged 15 in the comparison group seem, on average, at least as likely as their 14-year-old peers to carry on with their studies.

[141] As discussed in the introduction of the thesis, in 1999 school was mandatory for children aged 7 to 14.

In addition, I run two placebo tests as discussed below. The first uses December 1997 as threshold, and the second uses 30 June, an unofficial cut-off for age at school entry, to check whether either age at school entry or school leaving can confound what is argued to be the impact of the ban. The balance in unobserved characteristics is more likely to hold for narrower bandwidths; however, with a split sample based on family composition a narrow bandwidth will likely result in very imprecise estimates. Thus, with the narrower bandwidth of 20 weeks one should focus more on the magnitude and signal of the coefficients rather than their precision.

Figures 1 to 5 illustrate the main results of the chapter. The figures report local linear regressions on each side the cut-off point over the -12 months, 12 months interval, as in chapters one and two. The regression lines are estimated with a triangle kernel and a one-month bandwidth; a 95 percent confidence interval is also reported. Figure 1 illustrates a non-parametric estimate of Eq. (2). This corresponds to the effect of the ban on 14-year-old boys, that is, the first stage.[142]

Figure 1 – Linear Regressions: LFPR of Eligible and Ineligible Boys

First Stage – 12 Months Bandwidth

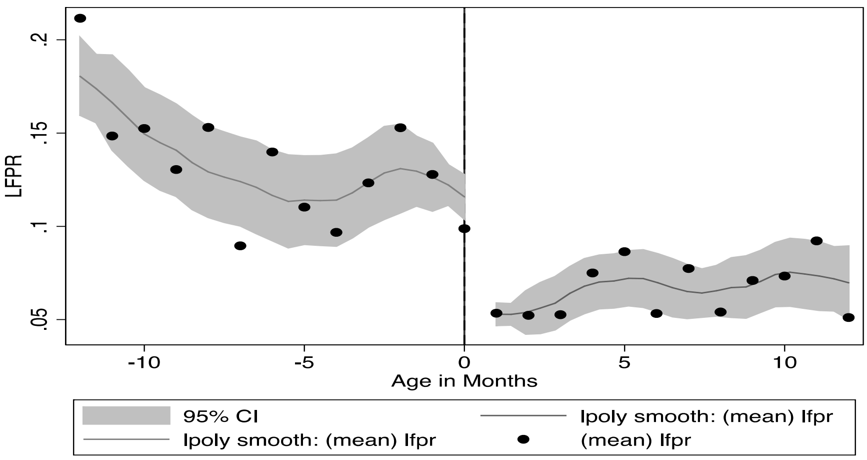


Figure 1 shows a significant decrease in the labour force participation rate as a whole (formal and informal) of boys who turned 14 after the ban. As discussed in the previous chapters, the reason for looking at participation rate as a whole is because the law affected mostly boys in the informal sector. Based on these results, one can then ask whether this decrease affected the time allocation of other

[142] Figure C.1 in Appendix 3 (page 81) uses data from one year earlier and shows no discontinuity in participation rate for boys aged 14 around December 1997.

household members. Figures 2 to 5 are graphic representations of non-parametric reduced-form (ITT) estimates.

Figure 2 – Linear Regressions for LFPR of Mothers in Couple Parent Households

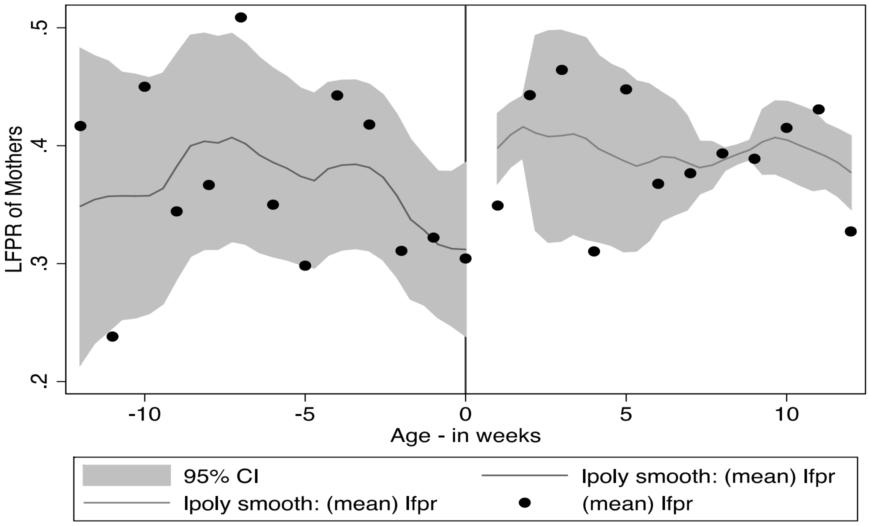


Figure 3 – Linear Regressions for LFPR in the Formal Sector of Mothers in Couple Parent Households

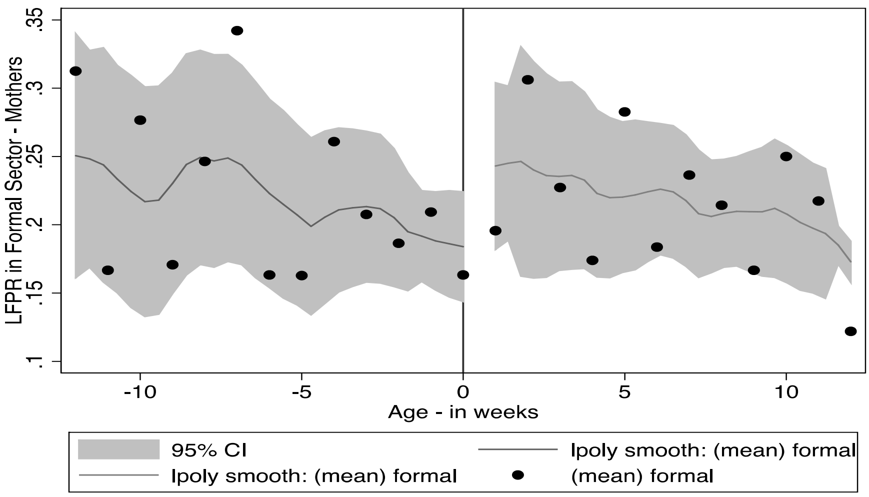


Figure 4 – Linear Regressions for LFPR of Fathers in Couple Parent Households

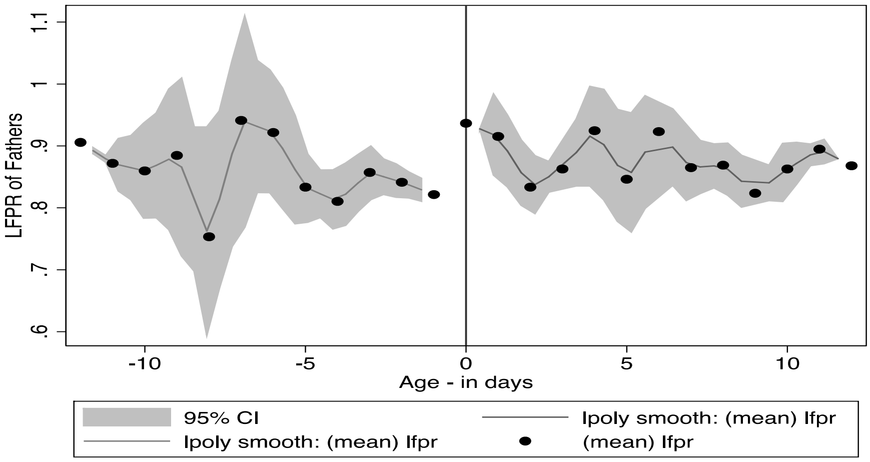
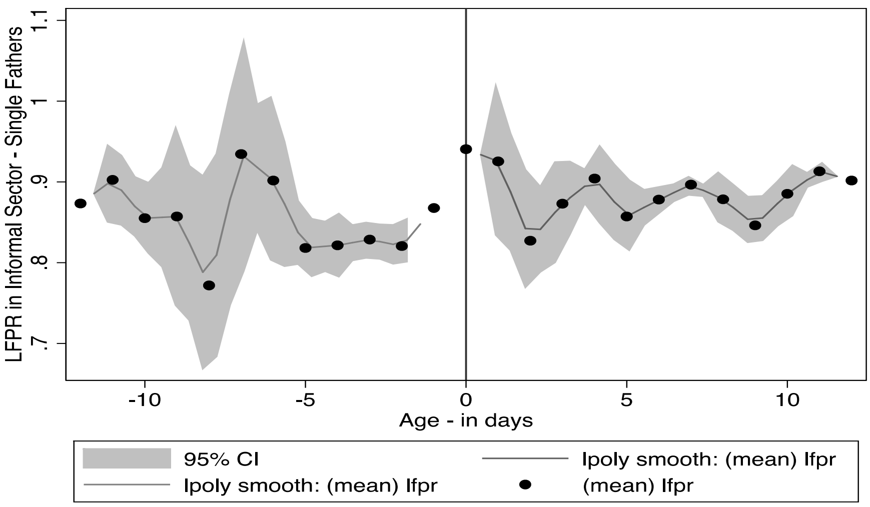


Figure 5 – Linear Regressions for LFPR in the Informal Sector of Fathers in Single Parent Households



Figures 2 and 3 show that mothers seem to be more likely to participate in the labour force, particularly in the formal sector. Figures 4 and 5 show a small increase in fathers' participation rate both in single and couple parent families.

Figures 2 to 4 suggest that couple parent households absorbed the ban, increasing mothers' and fathers' participation rates in the labour force. This suggests that mothers' labour supply was used as an imperfect insurance mechanism, a response that is consistent with the added worker effect hypothesis (Ashenfelter and Heckman 1974; Lundberg 1985). This effect helps explain the possible reduction in the participation rate of younger siblings in couple households shown in figure C.2 in Appendix 3 (page 245).

Figure 6 shows an increase in the participation rate of single fathers in the informal sector. These results may indicate that single fathers either face liquidity constraints or an equilibrium effect when the decrease in the participation rate of boys in the informal sector increased the wage rate of adults in the same sector. Figure C.3 (Appendix 3, page 245) shows a small increase in the school attendance of younger siblings in single headed households.[143]

The next section presents the results and discusses the impact of the ban on household members with equations (1) and (2) fitted with different specifications and 52 weeks bandwidth size.

6 RESULTS

6.1 The Impact of the Ban on Children Aged 14

Here we consider parametric regressions of the impact of the ban on children hindered from participating in the formal labour force at age 14. Estimates are only provided for 14-year-old boys, as chapter one shows that the law did not have any effect on the participation rate of girls. The model is run with two bandwidth sizes, 52 weeks and 20 weeks. The $h\left(Z_i\right)$ function is specified as polynomials of degree one to three and as linear and quadratic piecewise polynomials.[144]

Regressions are estimated for three outcome measures: participation rate, participation in the formal labour force, and participation in the informal labour force. Table 4 shows the first stage estimates with both bandwidth sizes.

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[143] Figures C2 and C3 are plotted without the 95 percent confidence intervals, because the relatively small sample size results in very noisy patterns. Local linear regressions can be very noisy with a small sample size given the lower rate of convergence in non-parametric methods. See Fölich (2004) and Cameron and Trivedi (2005).

[144] The main difference between this exercise and that of chapter one is the bandwidth and sample composition. In this chapter the sample includes 14-year-old boys who have at least one parent present and excludes households with multiple families.

Table 4 – Parametric ITT Estimates for the Impact of the Laws of 1998 on Extensive Margin of Labour Supply of Boys aged 14
14 before Dec 1998 vs. 14 after Dec 1998

	20 Weeks Bandwidth			*52 weeks bandwidth*		
Polynomial degree	Participation Rate	Participation Rate Formal	Participation Rate Informal	Participation Rate	Participation Rate Formal	Participation Rate Informal
Linear	-0.054***	-0.012**	-0.041**	-0.094***	-0.011***	-0.057***
	(-3.08)	(-2.16)	(-2.48)	(-6.33)	(-2.76)	(-4.95)
Quadratic	-0.059*	-0.014	-0.045	-0.069**	-0.0017	-0.041*
	(-1.85)	(-1.21)	(-1.49)	(-2.39)	(-0.24)	(-1.90)
Cubic	-0.057*	-0.015	-0.043	-0.068**	-0.0011	-0.041*
	(-1.77)	(-1.20)	(-1.40)	(-2.37)	(-0.16)	(-1.85)
Spline Linear	0.012	-0.019	0.031	-0.071*	-0.0015	-0.059**
	(0.29)	(-1.31)	(0.77)	(-1.93)	(-0.17)	(-2.13)
Spline Quadratic	-0.056*	-0.015	-0.041	-0.068**	-0.0011	-0.041*
	(-1.73)	(-1.19)	(-1.36)	(-2.36)	(-0.16)	(-1.85)
Observations	*1014*	*1014*	*1014*	*2145*	*2145*	*2145*

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

Most of the estimates of the participation rate of boys are negative and statistically significant at conventional levels, particularly for the larger bandwidth. Although the estimates with the narrower bandwidth are slightly smaller in absolute terms and less precise, most of them show a reduction in the labour force participation rate of about 6 to 7 percentage points. In relative terms, this represents a decrease of 52.6 to 61.4 percent.[145] Consistent with results in chapter one, the results for participation rate as a whole are mostly driven by a reduction in participation rate in the informal sector. The decrease in participation rate in the informal sector indicates that some employers complied with the law and stopped hiring children under age 16.[146]

The impact of an exogenous variation in the labour force participation rate of 14-year-old boys on their younger siblings sheds some light on whether same sex individuals are complementary or substitute inputs in the household production function, and whether parents' preferences for boys and girls are different. The next section provides estimates for the impact of the ban on the outcomes of younger siblings and parents.

6.2 Spillover Effects on Household Members

This section provides estimates for household members in single and couple parent families. Splitting the sample according to family composition can help us develop a better understanding of the potential mechanisms underlying the results. In order to investigate whether credit/liquidity constraint affects parents' response to the ban, the labour force status of the parents is used as an imperfect proxy for credit constraint. Unfortunately, the Brazilian PNAD does not contain information on household access to insurance and credit markets, use of credit, or household debt burden. Participation in the informal sector is used as a proxy for credit constraint, because informal workers do not have access to a variety of credit lines available through Brazilian commercial banks.[147]

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[145] The participation rate of the control group is 11.4 percent with 52 weeks bandwidth.

[146] This is the main channel in the Basu (2005) model through which a ban could affect child labour and the wage rate paid to children after the ban. Braradwaj et al. (2013) argue along the same lines and use an extended version of the model to understand the impact of the Indian child labour ban of 1986.

[147] A common practice in Brazil is the salary-deducted loan where the worker commits a fraction of his/her salary to pay back the outstanding loan. The occupation of the household head is an imperfect proxy, as formal workers tend to have easier access to credit but are also more liquidity constrained,

Thus, an increase in participation rate in the informal labour force would suggest that the household did not manage to smooth the shock through borrowing or through some insurance mechanism and instead shifted to the informal labour market, trading off higher consumption in the long-run – since they have to stop contributing to the pension system – for higher liquidity in the short-run. If that is the case, a higher participation rate in the informal labour market could suggest that the household is credit or liquidity constrained.[148] I also investigate whether the impact of the ban changes according to family composition to understand household decision-making in these two settings.

6.2.1 Family Composition and Labour Force Status of Parents

This section looks at the formality status of the household head to check whether credit constraint is likely a binding constraint for some households affected by the ban. The results for single parent households can be seen in Table 5. Though the patterns for single mothers and single fathers suggest opposite responses to the ban, none of the coefficients are statistically significant.

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because some of the benefits are not as liquid as cash – such as health insurance and mandatory contributions to the pension system, both deductible from gross monthly earnings. That helps explain why, controlling for self-selection into the formal labour market, Menezes Filho et al. (2004) find that earnings in the informal sector are actually higher than in the formal sector.

[148] This is a binary variable that takes the value of one for those participating in the informal sector and zero otherwise (not participating at all or participating as formal sector workers). Participation in the formal sector is defined similarly.

Table 5 – Parametric ITT Estimates of the Impact of the Ban on Household Head's Labour Supply – Single Parent Households
52 weeks bandwidth – with controls

	Female Head		Male Head	
h(z)	LFPR	Hours Worked per Week	LFPR	Hours Worked per Week
Linear	0.033	5.39	-0.013	-1.29
	(0.41)	(1.51)	(-0.28)	(-0.45)
Quadratic	0.031	5.60	-0.013	-1.20
	(0.38)	(1.55)	(-0.28)	(-0.42)
Cubic	0.013	3.90	-0.030	0.13
	(0.12)	(0.80)	(-0.50)	(0.036)
Spline Linear	0.030	5.48	-0.013	-1.27
	(0.38)	(1.52)	(-0.27)	(-0.44)
Spline Quadratic	0.040	4.61	-0.035	0.23
	(0.33)	(0.84)	(-0.54)	(0.058)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*
Sigma		14.5***		12.9***
		(20.1)		(14.8)
Observations	*565*	*276*	*371*	*323*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. Given the proportion of parents not participating in the labour force, the coefficients on weekly hours worked refer to Tobit estimates.

On the other hand, table 6 shows striking results when participation rate is split into formal and informal sectors. There is a clear indication that single fathers became more likely to participate in the informal sector. A great part of this increase seems to be explained by a reduction in the participation rate in the formal sector, but the magnitude of the point estimates show that some male heads entered the labour force as a consequence of the ban. The magnitude of the effects is very large and might be a bit noisy given the relatively small sample size. It is difficult to justify that decision based on the monthly wage in both sectors as well as the occupations in the formal and informal sectors in which single parents ended up.[149] As suggested earlier, I interpret this shift as an indication that at least for some of these households, credit was a binding constraint. In essence, the estimates point to a lack of traditional

[149] See figure C.22 and table C.20 in Appendix 3. I also consider hourly wages, and the conclusions are the same.

risk coping mechanisms, such as unemployment insurance and/or credit markets, among single parent households affected by the 1998 ban.

The estimates for couple parent families are shown in table 7. Interestingly, for this family composition there is a clear indication that mothers became more likely to enter the labour force. Most of the increase took place in the formal sector, as shown in table 8. In relative terms, the participation rate of mothers increased by about 25 percent.[150] Dividing this number by the relative decrease in the participation rate of sons aged 14, I find a cross-elasticity of labour supply of -0.41 to -0.48. A 10 percent decrease in the participation rate of a son aged 14 increased his mother's participation rate in the labour force by 4.1 to 4.8 percent. This is an interesting result, since it could suggest either that (i) some mothers, despite having skills to participate in the formal labour force, would otherwise stay home, or (ii) males and females in the same household are specialised in different tasks. This result remains consistent with the hypothesis that in couple parent families the labour supply of spouses can be used as an imperfect insurance mechanism.[151]

[150] The participation rate of control mothers in couple parent households was 40.1 percent in 1999. A 10 percentage point increase in participation rate corresponds to about 25 percent in relative terms.
[151] One could argue that this violates the luxury axiom that parents always prefer to consume children's leisure if they can afford it. I understand that the assumption made by Basu and Van (1998) might hold, particularly for younger children participating in hazardous activities. For children aged 14, participation in the labour force may have positive effects on the individual's human capital in the form of accumulated experience.

Table 6 – Impact of the Ban on Labour Force Status of the Household Head – Single Parent Households

52 weeks bandwidth – with controls

	Female Head		Male Head	
h(z)	Formal	Informal	Formal	Informal
Linear	0.047	0.0085	-0.29**	0.26***
	(0.57)	(0.16)	(-2.54)	(2.63)
Quadratic	0.041	0.0060	-0.29**	0.26***
	(0.49)	(0.11)	(-2.55)	(2.65)
Cubic	-0.010	-0.083	-0.47***	0.40***
	(-0.091)	(-1.13)	(-3.10)	(2.85)
Spline Linear	0.040	0.0034	-0.30**	0.26***
	(0.48)	(0.065)	(-2.56)	(2.66)
Spline Quadratic	-0.015	-0.10	-0.53***	0.45***
	(-0.11)	(-1.25)	(-3.08)	(2.76)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*443*	*443*	*227*	*227*

Source: PNAD 1999.

Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The regressions include a dummy for metropolitan region, a dummy for skin colour (white), and years of schooling.

Looking at tables 5 and 6 together, one notices that fathers, on the other hand, increased their participation in the informal labour force but ended up working slightly fewer hours per week. The ITT estimates for the participation rate of fathers in couple parent households are almost half of those for single fathers in absolute terms (10 percentage points), but are still high in relative terms (56.5 percent), and this might be because they can share the burden of the adverse shock with their spouses.

Table 7 – Parametric ITT Estimates of the Impact of the Ban on Parents' Labour Supply – Couple Parent Households
52 weeks bandwidth – with controls

	Mother		Father	
h(z)	LFPR	Hours Worked per Week	LFPR	Hours Worked per Week
Linear	0.10**	0.80	0.036	-3.28**
	(1.99)	(0.33)	(1.36)	(-2.49)
Quadratic	0.10**	0.83	0.036	-3.29**
	(1.98)	(0.35)	(1.36)	(-2.49)
Cubic	0.11*	1.02	0.041	-4.31***
	(1.65)	(0.38)	(1.27)	(-2.77)
Spline Linear	0.11**	0.81	0.036	-3.23**
	(2.00)	(0.34)	(1.39)	(-2.46)
Spline Quadratic	0.074	2.79	0.059**	-3.19**
	(1.11)	(1.04)	(2.03)	(-2.10)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*
Sigma		15.0***		11.8***
		(25.8)		(30.5)
Observations	*1208*	*462*	*1208*	*1083*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. Given the proportion of parents not participating in the labour force, the coefficients on weekly hours worked refer to Tobit estimates.

Tables 8 and 9 show the results for younger siblings in single parent households. Table 8 suggests no impact on the labour supply of younger siblings, but table 9 shows that younger brothers became more likely to attend school. The point estimate is large and very stable, pointing to an increase in school attendance at around 10 percentage points. It seems that with the ban, 14-year-old boys who left the labour force allowed their younger brothers to attend school. It is possible that parents assigned boys banned from the labour force to household chores to be able to invest in the education of the younger sons.[152] The decision to invest in the human capital of younger sons might indicate that single fathers, considering the household production function and the returns to education of boys and girls, see boys as more 'talented' than girls, as in the Horowitz and Wang (2004) model.[153]

[152] According to the results in chapter one, there is an indication that boys affected by the ban became more likely to do household chores.
[153] Note that this would be also consistent with the assumption that poor households tend to prefer sons to daughters, because sons are more likely to take care of parents in the long-run, whereas daughters tend to move once they get married (Eswaran 1996; Ennew 1982).

Table 8 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Single Parent Households
52 weeks bandwidth – with controls
Work Outcomes

	All		Brothers		Sisters	
h(z)	LFPR	Domestic Work	LFPR	Domestic Work	LFPR	Domestic Work
Linear	-0.0018	0.069	0.00075	0.15	-0.0055	0.0065
	(-0.059)	(0.84)	(0.014)	(1.21)	(-0.20)	(0.069)
Quadratic	-0.0018	0.078	0.00098	0.16	-0.0053	0.0086
	(-0.061)	(0.95)	(0.019)	(1.27)	(-0.18)	(0.090)
Cubic	0.012	0.15	0.056	0.15	-0.030	0.14
	(0.32)	(1.45)	(0.89)	(0.98)	(-0.82)	(1.07)
Spline Linear	-0.0024	0.090	0.00084	0.18	-0.0054	0.011
	(-0.079)	(1.09)	(0.016)	(1.42)	(-0.17)	(0.11)
Spline Quadratic	-0.0041	0.16	0.041	0.13	-0.050	0.15
	(-0.083)	(1.30)	(0.57)	(0.79)	(-0.82)	(1.05)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*534*	*517*	*250*	*237*	*284*	*280*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The vector of controls include a dummy for skin colour (1 if white), years of schooling of the household head, number of children aged 0 to 13, and a dummy for metropolitan region.

Table 9 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Single Parent Households
52 weeks bandwidth – with controls
School Outcomes

	All		Brothers		Sisters	
h(z)	School Attendance	Years of Schooling	School Attendance	Years of Schooling	School Attendance	Years of Schooling
Linear	0.064**	-0.17	0.10**	-0.047	0.029	-0.30
	(2.25)	(-0.67)	(2.00)	(-0.13)	(1.05)	(-0.90)
Quadratic	0.065**	-0.17	0.10**	-0.066	0.028	-0.29
	(2.24)	(-0.67)	(1.99)	(-0.18)	(1.03)	(-0.86)
Cubic	0.053*	-0.39	0.065	-0.39	0.035	-0.40
	(1.77)	(-1.28)	(1.37)	(-0.93)	(0.99)	(-0.99)
Spline Linear	0.068**	-0.18	0.11*	-0.090	0.029	-0.29
	(2.21)	(-0.66)	(1.97)	(-0.24)	(0.99)	(-0.83)
Spline Quadratic	0.059	-0.59	0.056	-0.57	0.056	-0.59
	(1.60)	(-1.55)	(1.18)	(-1.14)	(1.00)	(-1.15)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*534*	*534*	*250*	*250*	*284*	*284*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The vector of controls include a dummy for skin colour (1 if white), years of schooling of the household head, number of children aged 0 to 13, and a dummy for metropolitan region.

The results for couple parent families suggest that mothers and fathers entered the labour force, but fathers already employed spent fewer hours at work, probably to help with household chores such as looking after children.

Table 10 – Parametric ITT Estimates of the Impact of the Ban on Parents' Labour Supply – Couple Parent Households
52 weeks bandwidth – with controls

	Mother		Father	
h(z)	LFPR	Hours Worked per Week	LFPR	Hours Worked per Week
Linear	0.10**	0.80	0.036	-3.28**
	(1.99)	(0.33)	(1.36)	(-2.49)
Quadratic	0.10**	0.83	0.036	-3.29**
	(1.98)	(0.35)	(1.36)	(-2.49)
Cubic	0.11*	1.02	0.041	-4.31***
	(1.65)	(0.38)	(1.27)	(-2.77)
Spline Linear	0.11**	0.81	0.036	-3.23**
	(2.00)	(0.34)	(1.39)	(-2.46)
Spline Quadratic	0.074	2.79	0.059**	-3.19**
	(1.11)	(1.04)	(2.03)	(-2.10)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*
Sigma		15.0***		11.8***
		(25.8)		(30.5)
Observations	*1208*	*462*	*1208*	*1083*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. Given the proportion of parents not participating in the labour force, the coefficients on weekly hours worked refer to Tobit estimates.

Table 11 – Impact of the Ban on Labour Force Status of Parents – Couple Parent Households
52 weeks bandwidth – with controls

	Mother		Father	
h(z)	Formal	Informal	Formal	Informal
Linear	0.094**	-0.013	-0.054	0.11**
	(2.06)	(-0.38)	(-0.90)	(2.32)
Quadratic	0.095**	-0.012	-0.053	0.11**
	(2.07)	(-0.36)	(-0.89)	(2.31)
Cubic	0.098*	0.0055	-0.052	0.12**
	(1.79)	(0.14)	(-0.71)	(2.19)
Spline Linear	0.099**	-0.010	-0.053	0.11**
	(2.11)	(-0.31)	(-0.89)	(2.24)
Spline Quadratic	0.092	-0.015	0.023	0.071
	(1.54)	(-0.35)	(0.31)	(1.09)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*924*	*924*	*772*	*772*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The regressions include a dummy for metropolitan region, a dummy for skin colour (white), and years of schooling.

Table 11 shows that mothers entered the formal labour force. This is one of the most consistent results in this chapter and seems consistent with mothers' labour supply being used to smooth the negative income shock.

The effects of the ban on younger siblings in couple parent families are shown in Tables 12 and 13. The ban does not seem to have affected school outcomes of younger siblings, but Table 9 suggests that younger siblings became less likely to participate in the labour force. The result is stable and statistically significant at 10 percent in three specifications. It indicates a fall of 3 percentage points in the probability of younger siblings participating in the labour force.

This represents a 100 percent decrease in relative terms and a cross-elasticity of labour supply of 1.6 to 1.9, suggesting a fairly elastic labour supply of younger siblings in couple parent households. This is actually an expected result, as younger siblings' labour supply is supposed to be used only in extreme situations where no alternative risk-coping options are available. Besides, there seems to be some gender specialisation in the household production function, with younger and older brothers as complementary inputs to some extent.

Table 12 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Couple Parent Households
52 weeks bandwidth – with controls
Work Outcomes

	All		Brothers		Sisters	
h(z)	LFPR	Domestic Work	LFPR	Domestic Work	LFPR	Domestic Work
Linear	-0.032*	-0.084	-0.038	-0.056	-0.027	-0.10
	(-1.66)	(-1.27)	(-1.05)	(-0.58)	(-1.64)	(-1.25)
Quadratic	-0.032*	-0.084	-0.038	-0.055	-0.026	-0.100
	(-1.65)	(-1.27)	(-1.05)	(-0.57)	(-1.60)	(-1.23)
Cubic	-0.040*	-0.015	-0.071*	-0.037	-0.011	0.012
	(-1.86)	(-0.19)	(-1.73)	(-0.32)	(-0.81)	(0.13)
Spline Linear	-0.031	-0.091	-0.037	-0.070	-0.024	-0.10
	(-1.48)	(-1.35)	(-0.96)	(-0.71)	(-1.40)	(-1.21)
Spline Quadratic	-0.030	-0.026	-0.060	-0.014	0.0041	-0.035
	(-0.96)	(-0.30)	(-1.01)	(-0.11)	(0.22)	(-0.31)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*715*	*705*	*341*	*335*	*374*	*370*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The vector of controls include a dummy for skin colour (1 if white), years of schooling of the household head, number of children aged 0 to 13, and a dummy for metropolitan region.

Table 13 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Couple Parent Households
52 weeks bandwidth – with controls
School Outcomes

	All		Brothers		Sisters	
h(z)	School Attendance	Years of Schooling	School Attendance	Years of Schooling	School Attendance	Years of Schooling
Linear	-0.012	-0.16	-0.012	-0.16	-0.012	-0.15
	(-0.56)	(-0.72)	(-0.35)	(-0.58)	(-0.56)	(-0.50)
Quadratic	-0.012	-0.16	-0.012	-0.16	-0.014	-0.16
	(-0.58)	(-0.73)	(-0.34)	(-0.57)	(-0.65)	(-0.51)
Cubic	-0.012	-0.12	-0.011	0.077	-0.011	-0.30
	(-0.63)	(-0.44)	(-0.34)	(0.22)	(-0.60)	(-0.83)
Spline Linear	-0.021	-0.19	-0.019	-0.21	-0.023	-0.16
	(-0.99)	(-0.84)	(-0.56)	(-0.75)	(-1.06)	(-0.52)
Spline Quadratic	-0.031*	-0.11	-0.044	0.20	-0.017	-0.47
	(-1.65)	(-0.38)	(-1.48)	(0.55)	(-0.78)	(-1.24)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*715*	*715*	*341*	*341*	*374*	*374*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The vector of controls include a dummy for skin colour (1 if white), years of schooling of the household head, number of children aged 0 to 13, and a dummy for metropolitan region.

Combining the effects of the ban on the labour supply of parents and younger siblings, one can argue that mothers enter the labour force to mitigate the shock for younger siblings. Since mothers are get paid more than young boys, particularly if the occupation is in the formal sector, this reallocation of time among household members caused by the ban may have enhanced households' welfare.[154]

7 ROBUSTNESS CHECK

To check robustness, some regressions are estimated with 20 weeks bandwidth. Since smaller samples lead to loss in precision, I concentrate on the qualitative aspect of the estimates (sign and magnitude of the effect) rather than their significance in statistical terms. Estimates are provided for linear, quadratic, and spline linear specifications of the smooth function, since with narrower bandwidth linear specifications of the smooth function are less restrictive and estimates of polynomials of high order may imply noisier estimates.

Tables C.3 and C.4 in Appendix 3 (page 220-221) present estimates for the impact of the ban on younger siblings in single parent families. Just as with the larger bandwidth, there is an indication that younger siblings are more likely to attend school. Based on the magnitudes of the estimates for younger brothers and sisters, most of the effect seems to be coming from brothers.

Estimates of the labour supply of single parents are shown in table C.5 (page 222). The coefficients for single mothers and fathers are qualitatively similar to those observed with the larger bandwidth; however, the estimates of weekly hours worked of mothers are larger and statistically significant. Table C.6 (Appendix 3, page 223) shows the coefficients for labour force status of parents. The coefficients for single parents are qualitatively similar to previous coefficients but are too large to be interpreted at face value.

Results for siblings in couple parent families can be seen in tables C.7 and C.8 in Appendix 3 (page 224-225). Though not statistically significant, the coefficients on the labour force participation rate are very similar to those found with larger bandwidths. The estimates for mothers and fathers are also very similar to the previous estimates (see tables C.9 and C.10 in the Appendix 3 on pages 226-227).

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[154] This result is consistent with Basu and Van's *luxury axiom* (1998).

7.1 PLACEBO TEST

To check whether the results are exclusively due to the 1998 ban, a placebo test is conducted using the 1998 PNAD. The cutoff point is defined as 31 December 1983. Boys born before and after 31 December 1983 are unaffected by the ban, because they turned 14 one year before the law passed. Thus, one should not expect a difference in the outcomes of children who turned 14 just before and just after December 1997.

Table C.11 (Appendix 3, page 228) shows the first stage estimates and tables C.12 to C.22 (Appendix 3, page 229-239) present the results of household members. Estimates are provided with 52 weeks bandwidth.[155] The coefficients of the first stage are negative, relatively large, and statistically significant in the linear specification, but they become much smaller, positive, and statistically insignificant in all other specifications. This shows that one should use different specifications when estimating an RDD parametrically to avoid misleading conclusions.

Most of the estimates for younger siblings and parents are statistically insignificant. Tables C.14 to C.16 (page 231-233) show that single mothers of boys born after December 1983 were more likely to participate in the formal labour force. The coefficients are large and stable across specifications and are statistically significant.

In the results discussed above, I find no impact of the ban on single mothers' labour supply. This result is a bit difficult to understand, as there is no particular reason that single mothers of boys born in 1984 (who turned 14 after December 1997) would be more likely to work than mothers of boys born in 1983 (who turned 14 before December 1997). Since the results have no apparent connection with the participation rate of 14-year-old boys and are very different from what I claim to be the effects of the law, I do not believe they harm the main findings.

Nevertheless, in order to unpack this puzzling result I provide visual and regression checks. Figures C.4 to C.6 (Appendix 3, page 205) inspect the placebo results for single mothers visually to identify which observations might be driving these large coefficients. The figures plot local polynomial regressions for the labour

[155] For 52 weeks bandwidth, the vector of covariates of the previous estimates are used to control for potential confounders related to observed characteristics.

force participation rate of single mothers. Figure C.4 (Appendix 3, page 246) indicates that observations close to the threshold seem to drive the average effects. Figure C.5 (Appendix 3, page 246) plots similar regressions dropping observations in the (-4, 4) interval, that is, children who turned 14 between December 1997 and January 1998, whereas figure C.6 drops observations in the (-6, 6) interval (Appendix 3, page 246). As the figures suggest, the strong effects of the participation rate of single mothers are very local. Table C.16 (Appendix 3, page 233) provides regression estimates for participation rate in the labour force for three samples of single mothers: sample of white single mothers, sample of non-white single mothers, and sample of mothers excluding observations in the (-6, 6) interval. The results in the first and second columns indicate that placebo effects are driven by the sub-sample of non-white single mothers, whereas estimates in the third column show that results are very local, confirming the visual inspection.[156] Although I believe that this placebo result does not harm the main findings discussed above, I cannot rule out the hypothesis that the placebo regressions for non-white single mothers might pick up some seasonal birth effects.[157]

With regard to siblings, most of the estimates are statistically insignificant. Table C.22 (Appendix 3, page 239) indicates that younger sisters of boys who turned 14 after December 1997 were more likely to attend school. There is no particular reason to expect such a result, particularly because I did not find any impact of the ban on school outcomes of younger siblings in couple parent families, particularly for sisters.

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[156] With the addition of a vector of covariates that includes the age of the household head, household size, number of children under age 5, number of children aged 6 to 9, number of children aged 10 to 12, number of children above age 14, and household total income net from children's income, the point estimates for white single mothers shrink. This is not the case for non-white single mothers, as results remain quite large and statistically significant.

[157] Based on comprehensive data for the US, Buckles and Hungerman (2013) find strong evidence against applied papers in which the identification strategy relies on the use of quarter of birth as the instrumental variable. They show that quarter of birth might capture seasonal birth effects that are largely explained by and women's socioeconomic background and expected weather at birth. The main pattern in their analysis suggests that women who have a child in the winter (January to May) are very different in socioeconomic terms from those who have a child in the other seasons. They are more likely to be teenagers, unmarried, and less likely to have a high school diploma. They observe that children born to these women are different in several dimensions. If the same pattern applies to Brazil, I could expect children born from June to August (the winter period in Brazil) to have different outcomes compared to those born in other seasons of the year. Since the law was enacted in December 1998 and estimates are provided with 20 and 52 weeks bandwidth, I believe that the results are unlikely to be contaminated by such seasonal effects. The balance checks around the cutoff point suggest that children on each side of the threshold have similar characteristics and socioeconomic backgrounds (e.g., parents' education and household income).

Overall, the placebo tests support the main results discussed previously, mainly for couple parent families. The next section checks whether this might have to do with school starting age effect.

7.2 CAVEATS

The seminal paper by Angrist and Kruger (1991) triggered the use of date of birth as an instrumental variable for completed years of schooling. Despite criticisms regarding the use of season of birth as a valid instrument (Bound et al. 1995; Bound and Jaeger 2000; Buckles and Hungerman 2013), many authors have combined exact date of birth with compulsory schooling laws to estimate returns to education (see Oreopoulos, 2006a and 2006b). Others have combined the exact date of birth at school entry to estimate the impact of entering school later on short and long run outcomes, such as academic performance in primary and secondary education, earnings, employability, and teenage pregnancy (Dobkin and Ferreira 2010; McCrary and Royer 2011; Black et al. 2011; Bedard and Dhuey 2006).

Most of these papers find that students who enter school later due to school entry laws tend to perform better in school, but not necessarily in the labour market. Despite the mixed evidence regarding long-term effects of school entry laws, there is evidence of positive effects on earnings and employability at least until a certain age (Black et al. 2011; Bedard and Dhuey 2006).

One challenge of most of these papers face is the difference between absolute and relative age effects. The absolute age effect captures the maturity effect at certain ages. This ‘maturity effect’ can explain, for instance, differences in academic performance at early ages. Black et al. (2011) and Fredriksson and Öckert (2013) argue that what matters for policy is the relative age effect, i.e., whether ‘being the oldest in class gives an early advantage which may persist in the longer run’ (Fredriksson and Öckert 2013, p. 2).

Until recently there was no official school entry law in Brazil,[158] although the common practice is for parents to enroll their children in school up to 30 July in the year in which the child turns 6. If this informal rule were followed by most families

[158] Since 2010, children have had to be enrolled in school in the current academic year if they turn 6 by 30 March of the current calendar year. Those who turn 6 after 30 March are enrolled the next academic year.

and to some extent enforced by Brazilian schools by the time the 1998 law passed, my estimates could reflect the effect of school starting age in labour market outcomes. Due to this enrollment rule, individuals who turned 14 in the second half of 1998—before the law passed—entered school jointly with those who turned 14 in the first half of 1999. Since they were equally affected by the rule, the estimates would at most be affected by the 'maturity effect.' In other words, if entering school older has long lasting effects, including labour market outcomes, one could argue that these individuals would anticipate their entrance into the labour market to accumulate human capital through work experience (Black et al. 2011).[159] In that case, the difference in participation rate among boys who turned 14 before and after December 1998 could be explained by the effect of entering school younger. The estimates would therefore capture the combined effect of school starting age and the child labour ban. The contamination of the results by the school entry law may also affect labour market outcomes of mothers (see Berlinski et al. 2011).

To check whether the results capture the effect of the school entry law, table C.23 (Appendix 3, page 240) shows first stage estimates with the cut-off defined as 30 June 1999. Estimates are provided with 52 weeks bandwidth. As with the previous placebo test, coefficients are negative and statistically significant in the linear specification, but become positive and statistically insignificant in all other cases. The absence of discontinuity in the participation rate suggests that the age at school entry does not play a role in the estimates. Tables C.24 to C.26 in Appendix 3 (page 241-243) show the estimates for younger siblings and the household head. None of the estimates is statistically significant. These results support the main findings of the chapter and suggest that age at school entry is unlikely to influence results.

I also tried to minimise the potential influence of the school entry rule by using a larger bandwidth size with controls. With a larger bandwidth, the results are less likely to be affected by seasonal birth effects, an issue raised recently by Buckles and Hungerman (2013). As discussed above, the results with 20 and 52 weeks bandwidth are very similar.

8. CONCLUSION

[159] Fredriksson and Öckert (2013) argue that older students who finish all school cycles have less experience in the labour market, because they enter the labour market at an older age. Since the returns to experience decrease with age, they would have lower returns to experience for a given age.

This chapter contributes to the nascent literature of the consequences of child labour by investigating the intra-household consequences of the increase in the minimum legal age of entry into the labour force of December 1998 and, more specifically, the impact of banning participation in the formal labour force of 14-year-old children on the time allocation of younger siblings and the household head.

RDD is used to estimate the impact of the ban with different bandwidth sizes and flexible functional forms. The main findings suggest that the impact of the law was minor among younger siblings but more relevant among parents, particularly when family composition and the occupation of the household head are taken into account.

I looked at the labour force status of the household head to shed light on whether the household could face credit constraints. I found that male heads became more likely to participate in the informal labour market. The results indicate that fathers, particularly single fathers, shifted from the formal to the informal sector. This could suggest that with the shock they traded off illiquid perks embedded in a formal job contract for more cash in the informal sector. I interpret this result as an indication that credit could be a binding constraint for some households.

Splitting the sample according to family composition reveals an interesting and consistent story. Mothers in couple parent families became more likely to participate in the formal labour force, whereas fathers entered the informal sector but worked fewer hours per week. I also found that younger siblings in couple parent families were less likely to work. These results suggest that couple parent families use mothers' work as a risk-coping mechanism, a strategy not available to single parent families.

In fact, for single parent families I found no impact on single mothers' labour supply. On the other hand, I found an almost perfect shift of single fathers from the formal to the informal sector. I interpret these results as an indication that single parent households supply labour is more inelastic and these households are more likely to be headed by unskilled workers.

The results indicate that the consequences of a child labour ban can go beyond its immediate effect on children below a certain age, since it might affect several outcomes of other household members, particularly if the household head has few skills and access to suboptimal risk coping mechanisms. For households that rely on child labour to complement household income, banning child labour can indeed backfire (Ranjan 1999; Dessy and Knowles 2008). Insurance mechanisms such as unemployment insurance or even conditional cash transfers could be offered to households affected by the ban.

CONCLUSION

This thesis contributes to the scant literature on the causal impacts of legislation designed to reduce child labour in developing countries. In chapter one I examine the short run impacts of a Brazilian Constitutional Amendment of December 1998 that increased the minimum legal age of entry into the labour force from 14 to 16, and the impact of the Brazilian apprenticeship programme of December 2000 aimed at youth aged 14 to 17. This chapter pertains to the strand of literature that looks at the consequences of child labour for children themselves.

Regression discontinuity design is used to estimate the impact of both laws, exploring discontinuities around age thresholds. In some cases the analysis is complemented by difference-in-differences estimates. The results of the ban show that it affected 14-year-old boys and girls differently, with boys more likely to drop out of the labour force and girls more likely to attend school. The ban also affected hours worked, and the results were consistent with the impact on participation rate. For children just under age 16, almost no effect is found.

With regard to the apprenticeship programme, the estimates show a positive though very small effect on participation rate in the formal labour force among 14-year-olds. I find an indication that 14-year-old boys and girls became more likely to attend school. The comparison between children around age 16 shows that the programme fully counterbalanced the effect of the ban on participation rate for this age group.

In chapter two I focus on the long run consequences of the December 1998 ban on schooling and labour market outcomes of white and non-white males. I also look at distributional impacts of the ban by estimating unconditional quantile treatment effects on the log of hourly wage distribution. The estimates sustain the hypothesis that the law benefits white males with higher wages, higher probability of enrollment into college, and better occupations, whereas non-white males look less likely to be employed and have lower earnings. The results suggest that, on average, the law worked as a nudge to parents of white males to reallocate children’s time towards activities with higher market returns, but for non-white males the law may have had negative consequences. Putting these results together, while males seem to accumulate more human capital through education, non-white males appears to accumulate human capital through labour market experience. The chapter also provides estimates for elasticity of labour supply on the intensive margin, and the results are in line with what seems to be the benchmark in the literature. With regard to the potential mechanisms, the estimates suggest that accumulated experience in the labour force is the main driver underlying these results.

Finally, the third chapter looks at the intra-household consequences of banning participation in the formal labour force of 14-year-olds on time allocation of younger siblings and parents. I explore different family compositions and the labour force status of the household head in order to better understand the decision-making process when the spouse is present and when households face liquidity constraints. As with the previous two chapters, I use regression discontinuity design to estimate the impact of the ban of 1998 on household members. The main findings suggest that fathers, mainly single fathers, shift from the formal to the informal sector. I interpret this result as an indication that single fathers face liquidity constraints and decide to shift to the informal sector, trading off benefits provided by formal occupations (such as pensions) for informal jobs that tend to be more cash-oriented (liquid). I also find that younger brothers are more likely to attend school. It seems that parents shift to the informal sector and brothers banned from the labour force take over household responsibilities and help with chores to allow their younger brothers to go to school. This is consistent with a household production function in which older siblings have comparative advantages on both paid and unpaid (domestic) work activities and parents' preference for sending older children to work so that they can invest in the human capital of younger sons.

The most interesting and robust results are for couple parent families. I find that mothers in couple parent families became more likely to participate in the formal labour force, whereas fathers entered the informal sector but worked fewer hours per week. I also find that younger siblings in couple parent families were less likely to work. These results show that couple parent families use mother's work as a risk-coping mechanism—a strategy not available to single parent families.

The results of this thesis support the hypothesis that a simple change in the minimum legal age of entry into the labour force can have consequences on other household members and can potentially harm relatively poor households that could, perhaps, benefit more from an earlier entrance into the formal labour force than from low quality public education. For households that rely on child labour to complement household income, insurance mechanisms such as unemployment insurance or even conditional cash transfers could be used to help households smooth the negative consequences of the ban, even though the evidence found in this thesis suggests that some children could also benefit from an expansion of the Brazilian apprenticeship programme. Because laws as such can potentially harm children from disadvantageous backgrounds and affect the time allocation of other household members, this thesis shows that public interventions should be carefully designed to avoid misleading policy recommendations, a point made by Basu and Van (1998), Basu (2005), Ranjan (1999), Horowitz and Wang (2004), and Dessy and Knowles (2008) based on theoretical predictions.

Despite the fact that the findings strongly indicate that the ban impacted children in the short and long run as well as their household members, it is also worth discussing the scope for future research. First, in many cases throughout the chapters the estimates showed sensitivity to the specification of the smooth function and lacked statistical power. For future work, I suggest exploring non-parametric specifications and using the census of 2010 to estimate long-run effects with a larger sample size. Unfortunately, the census of 2010 does not provide individuals' exact date of birth. Access to this information relies on the approval of a formal request to be made to the Brazilian Bureau of Statistics.

Second, the household survey used to investigate the impact of the apprenticeship programme is not ideal given the low take up rate into the programme. A lot more can be said about the apprenticeship programme with the *Relação Anual de Informações Sociais* (RAIS) – a census of formal firms that is annually collected by the Brazilian Ministry of Labour and Employment but not publicly available – particularly with regard to its effects in the long-run.

Third, chapter two sets out the labour market experience as the underlying mechanism. That channel cannot be formally analysed with repeated household surveys, but might be investigated to some extent with the Brazilian labour force survey (*Pesquisa Mensal de Emprego*), a rotating quarterly panel.

Fourth, since intra-household time allocation, as investigated in chapter three, requires large sample sizes to have statistical power, more efficient estimates could be obtained with the census of 2000. However, as mentioned above, the census does not report individuals' exact date of birth.

Finally, the puzzling placebo results for single mothers found in chapter three suggest that, as in the US, mothers giving birth in different quarters of year might come from different socioeconomic backgrounds. This issue is key and needs further investigation as long as it would cast doubt on RDD estimates performed with relatively narrow bandwidths.

This thesis reveals that policymakers should have a broader perspective when designing laws. The results in the three chapters strongly recommend that policy makers take into account potential unintended consequences of law changes, such as heterogeneous effects across race and gender, spillover effects on time allocation of household members, and potential increases in wage inequality across race.

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APPENDICES

Appendix 1: Tables and Figures from Chapter 1

Placebo: 14 before and After December 1997

Figure A.1 – Labour Force Participation Rate

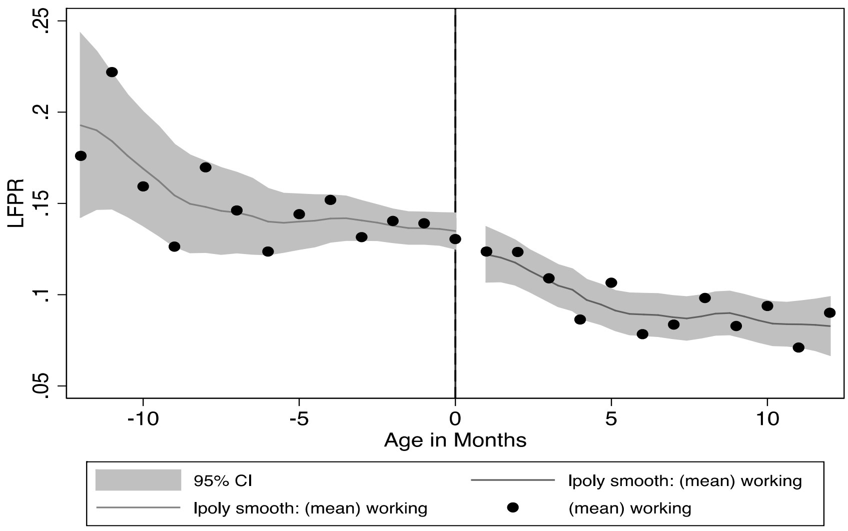
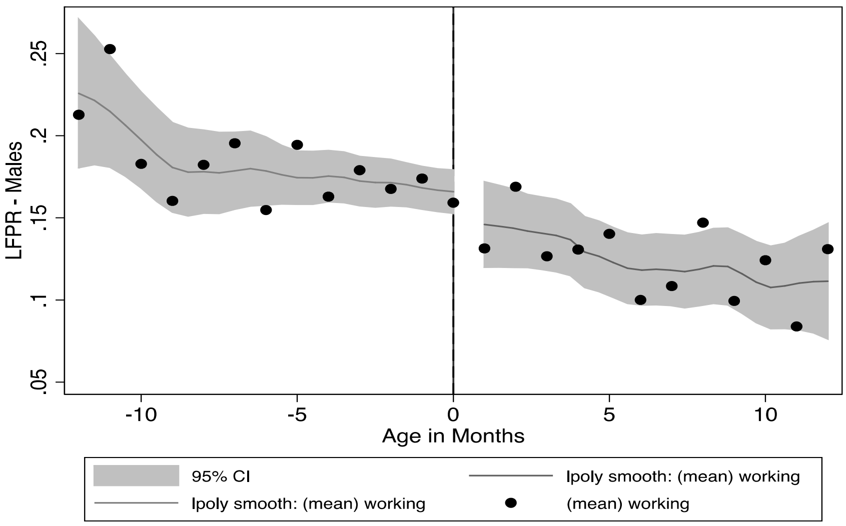


Figure A.2 – Labour Force Participation Rate for Males



Placebo: Age 15 vs. age 16 in September 1998

Figure A.3 – Participation Rate in the Labour Force

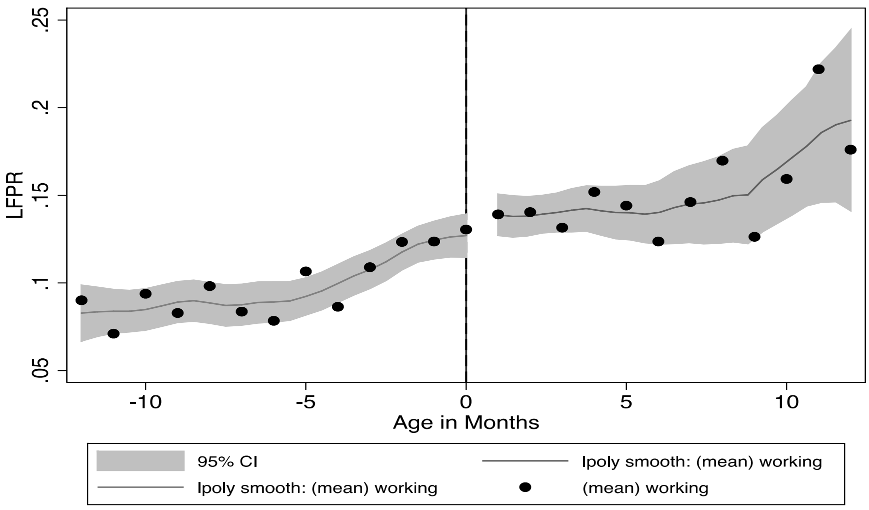
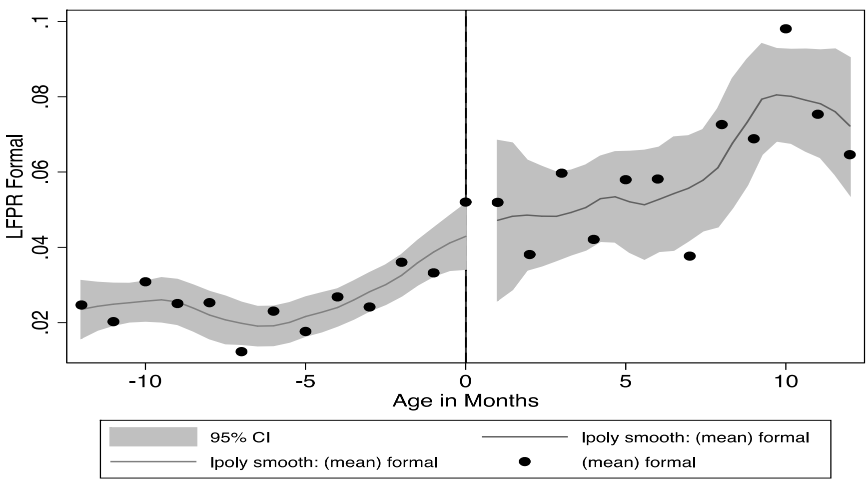


Figure A.4 – Participation Rate in the Formal Labour Force
15 vs. 16 in Sept 1998



LFPR Formal
.02
.04
.06
.08
.1
-10
-5
0
5
10
Age in Months
95% CI
lpoly smooth: (mean) formal
lpoly smooth: (mean) formal
(mean) formal

Table A.1 – T-test for Difference in Means – Urban Area Only
15 vs. 16 – Sept 1999

	Comparison Group		Eligible Group		T-statistic
Outcomes	Mean	SD	Mean	SD	
LFPR	0.21	0.40	0.24	0.43	(-2.45)
LFPR - Formal	0.02	0.15	0.05	0.21	(-3.20)
LFPR – Informal	0.19	0.39	0.20	0.40	(-1.21)
Domestic work	0.69	0.46	0.72	0.45	(-1.61)
School attendance	0.89	0.31	0.86	0.35	(2.51)
Covariates					
White	0.49	0.50	0.49	0.50	(0.03)
Father's years of schooling	5.41	4.50	5.21	4.47	(1.17)
Mother's years of schooling	4.95	4.45	4.83	4.48	(0.73)
Father's age	37.30	20.57	37.86	20.57	(-0.72)
Mother's age	32.36	20.83	32.41	21.11	(-0.06)
Household size	4.66	1.56	4.60	1.64	(1.13)
Land title	0.92	0.28	0.93	0.26	(-0.85)
Non-labor income	4.70	44.92	6.14	43.80	(-0.86)
Observations	*1338*		*1499*		*2837*

Source: PNAD of 1999.

Table A.2 – T-test for Difference in Means – Urban Area Only
15 vs. 16 – Sept 2002

	Comparison Group		Eligible Group		T-statistic
Outcomes	Mean	SD	Mean	SD	
LFPR	0.27	0.44	0.31	0.46	(-2.70)
LFPR - Formal	0.01	0.10	0.02	0.15	(-2.55)
LFPR – Informal	0.26	0.44	0.29	0.45	(-2.10)
Domestic work	0.68	0.47	0.66	0.47	(0.84)
School attendance	0.90	0.30	0.87	0.34	(2.73)
Covariates					
White	0.46	0.50	0.45	0.50	(0.75)
Father’s years of schooling	5.42	4.80	5.13	4.83	(1.75)
Mother’s years of schooling	6.99	4.32	6.77	4.25	(1.48)
Father’s age	34.69	20.89	34.08	21.54	(0.82)
Mother’s age	41.87	11.99	41.98	12.54	(-0.24)
Household size	4.53	1.55	4.50	1.63	(0.50)
Land title	2.15	0.58	2.14	0.54	(0.36)
Non-labor income	2.56	18.83	4.36	27.92	(-0.92)
Observations	*1569*		*1731*		*3045*

Source: PNAD of 2002.

Table A.3 – Parametric ITT Estimates for the Impact of the Laws of 1998 on Extensive Margin of Labour Supply
14 before Dec 1998 vs. 14 after Dec 1998
Bandwidth of 16 weeks

Polynomial degree	Labour Force Participation			Formal			Informal			Domestic work		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
0	-0.034***	-0.077***	0.0057	-0.0067**	-0.010**	-0.0036	-0.028***	-0.068***	0.0092	-0.011	0.0034	-0.0039
	(-3.19)	(-3.76)	(0.42)	(-2.47)	(-2.22)	(-1.07)	(-2.72)	(-3.39)	(0.68)	(-0.47)	(0.092)	(-0.20)
1	-0.030	-0.066	0.0069	-0.0091*	-0.0038	-0.014*	-0.022	-0.063	0.021	0.032	0.052	0.013
	(-1.09)	(-1.31)	(0.25)	(-1.65)	(-0.47)	(-1.94)	(-0.83)	(-1.26)	(0.81)	(0.64)	(0.74)	(0.29)
2	-0.029	-0.062	0.0056	-0.0090	-0.0035	-0.014*	-0.021	-0.059	0.019	0.033	0.056	0.013
	(-1.07)	(-1.29)	(0.20)	(-1.58)	(-0.43)	(-1.90)	(-0.81)	(-1.22)	(0.74)	(0.68)	(0.76)	(0.30)
3	-0.00023	-0.018	0.015	-0.011*	-0.012	-0.0094	0.0098	-0.0067	0.024	-0.023	-0.035	-0.014
	(-0.0070)	(-0.31)	(0.43)	(-1.70)	(-1.27)	(-0.88)	(0.33)	(-0.12)	(0.76)	(-0.28)	(-0.32)	(-0.21)
Spline linear	-0.028	-0.060	0.0042	-0.0090	-0.0034	-0.014*	-0.020	-0.057	0.018	0.033	0.054	0.014
	(-1.03)	(-1.23)	(0.15)	(-1.54)	(-0.41)	(-1.89)	(-0.77)	(-1.17)	(0.70)	(0.69)	(0.74)	(0.33)
Spline quadratic	0.0062	-0.000050	0.013	-0.014*	-0.013	-0.014	0.019	0.012	0.026	-0.048	-0.093	-0.017
	(0.17)	(-0.00083)	(0.33)	(-1.92)	(-1.13)	(-1.18)	(0.55)	(0.20)	(0.84)	(-0.59)	(-0.97)	(-0.24)
Observations	2134	1030	1104	2134	1030	1104	2134	1030	1104	2049	969	1080

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

Table A.4 – Parametric ITT Estimates for the Impact of the Laws of 1998 on Schooling outcome

14 before Dec 1998 vs. 14 after Dec 1998

Bandwidth of 16 weeks

Polynomial degree	School Attendance		
	All	Boys	Girls
0	0.012	0.013	0.012
	(1.49)	(0.88)	(1.18)
1	0.020	-0.0049	0.044*
	(1.13)	(-0.15)	(2.02)
2	0.019	-0.0064	0.044**
	(1.08)	(-0.19)	(1.96)
3	0.0071	-0.057	0.070**
	(0.29)	(-1.41)	(2.51)
Spline linear	0.019	-0.0073	0.045**
	(1.07)	(-0.22)	(2.03)
Spline quadratic	0.0072	-0.078*	0.093***
	(0.31)	(-1.84)	(5.06)
Observations	2134	1030	1104

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

Table A.5 – Parametric ITT Estimates for the Impact of the Laws of 1998 on Extensive Margin of Labour Supply - *15 vs. 16*
Bandwidth of 16 weeks

Polynomial degree	Labour Force Participation			Formal			Informal			Domestic work		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
0	0.046**	0.084***	0.0054	0.025***	0.042***	0.0089	0.025	0.050*	-0.0030	0.010	0.025	-0.015
	(2.20)	(2.80)	(0.29)	(3.55)	(3.50)	(1.34)	(1.17)	(1.68)	(-0.15)	(0.48)	(0.68)	(-0.66)
1	-0.0092	0.029	-0.044	-0.0027	0.0013	-0.0039	-0.0054	0.032	-0.040	0.028	0.045	0.0078
	(-0.27)	(0.52)	(-1.48)	(-0.26)	(0.067)	(-0.35)	(-0.14)	(0.55)	(-1.18)	(0.59)	(0.77)	(0.20)
2	-0.014	0.021	-0.046	-0.0029	0.0019	-0.0058	-0.0099	0.023	-0.041	0.025	0.048	0.00030
	(-0.44)	(0.43)	(-1.54)	(-0.28)	(0.099)	(-0.54)	(-0.27)	(0.45)	(-1.21)	(0.52)	(0.79)	(0.0077)
3	-0.014	0.067	-0.068*	0.0043	0.020	-0.0044	-0.016	0.057	-0.064	0.036	0.014	-0.00015
	(-0.33)	(0.98)	(-1.91)	(0.41)	(1.05)	(-0.37)	(-0.34)	(0.77)	(-1.53)	(0.54)	(0.18)	(-0.0028)
Spline linear	-0.014	0.020	-0.045	-0.0026	0.0023	-0.0061	-0.011	0.021	-0.040	0.023	0.048	-0.0017
	(-0.45)	(0.41)	(-1.49)	(-0.25)	(0.12)	(-0.57)	(-0.30)	(0.43)	(-1.17)	(0.47)	(0.79)	(-0.042)
Spline quadratic	-0.016	0.071	-0.062*	0.014	0.035*	0.00078	-0.027	0.047	-0.063	0.033	-0.019	0.0075
	(-0.35)	(0.95)	(-1.89)	(1.28)	(1.89)	(0.058)	(-0.54)	(0.62)	(-1.55)	(0.39)	(-0.22)	(0.100)
Observations	2046	996	1050	2046	996	1050	2046	996	1050	1833	833	1000

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

Table A.6 – Parametric ITT Estimates for the Impact of the Laws of 1998 on Schooling Outcome - *15 vs. 16*
Bandwidth of 16 weeks

Polynomial degree	School Attendance		
	All	Boys	Girls
0	-0.020*	-0.051***	0.010
	(-1.74)	(-2.96)	(0.63)
1	-0.018	-0.034	-0.0034
	(-0.74)	(-0.95)	(-0.11)
2	-0.015	-0.034	0.0029
	(-0.70)	(-0.95)	(0.10)
3	-0.044	-0.079	-0.0086
	(-1.60)	(-1.65)	(-0.23)
Spline linear	-0.011	-0.031	0.0072
	(-0.61)	(-0.94)	(0.26)
Spline quadratic	-0.028	-0.067**	0.0097
	(-1.23)	(-2.26)	(0.23)
Observations	2046	996	1050

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

Table A.7 – Difference-in-Differences Estimates for the Impact of the Law of 1998 on Extensive Margin of Labour Supply - *15 vs. 16*
Bandwidth of 16 weeks

	Labour Force Participation Rate			Participation Rate – Formal Labour Force			Occupation in Formal Sector			Participation Rate – Informal Labour Force		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Eligible*D_{99} (*DD*)	0.0067	-0.0043	0.014	0.0071	0.00036	0.013	0.049	0.014	0.11	-0.00044	-0.0047	0.0016
	(0.51)	(-0.20)	(0.98)	(0.89)	(0.027)	(1.42)	(0.95)	(0.23)	(1.01)	(-0.039)	(-0.25)	(0.13)
Eligible	0.020**	0.038**	0.0059	0.0081	0.020*	-0.0025	0.018	0.047	-0.036	0.012	0.018	0.0085
	(2.03)	(2.29)	(0.54)	(1.28)	(1.90)	(-0.36)	(0.50)	(1.18)	(-0.50)	(1.45)	(1.27)	(0.98)
D_{99} (1998=0, 1999=1)	-0.048***	-0.072***	-0.024**	-0.026***	-0.033***	-0.019***	-0.091**	-0.056	-0.17**	-0.022***	-0.039***	-0.0052
	(-5.26)	(-4.72)	(-2.42)	(-4.95)	(-3.80)	(-3.07)	(-2.38)	(-1.29)	(-2.15)	(-2.75)	(-2.90)	(-0.64)
Male	0.11***	*Na*	*Na*	0.033***	*Na*	*Na*	-0.012	*Na*	*Na*	0.080***	*Na*	*Na*
	(17.3)	*Na*	*Na*	(8.37)	*Na*	*Na*	(-0.38)	*Na*	*Na*	(14.4)	*Na*	*Na*
White	0.0074	-0.00043	0.015*	0.011***	0.015**	0.0069	0.078***	0.085**	0.058	-0.0036	-0.015	0.0080
	(1.05)	(-0.036)	(1.95)	(2.62)	(2.10)	(1.54)	(2.69)	(2.50)	(1.01)	(-0.59)	(-1.49)	(1.25)
Years of Schooling of the Household Head	-0.0017*	-0.0043**	0.00092	0.000090	-0.000073	0.00028	0.0046	0.0056	-0.0045	-0.0018**	-0.0042***	0.00064
	(-1.66)	(-2.52)	(0.80)	(0.15)	(-0.074)	(0.41)	(1.17)	(1.25)	(-0.54)	(-2.02)	(-2.83)	(0.67)
Age of Household Head	-0.00018	-0.00017	-0.00023	0.000092	0.00012	0.000036	0.00071	0.00060	0.00057	-0.00027	-0.00029	-0.00026
	(-0.68)	(-0.38)	(-0.78)	(0.58)	(0.43)	(0.21)	(0.67)	(0.49)	(0.28)	(-1.20)	(-0.74)	(-1.09)
Gender of the Household Head (=1 if male)	-0.071**	-0.14**	0.0093	-0.012	-0.043	0.022***	0.044	-0.011	0.43***	-0.059*	-0.098*	-0.013
	(-2.08)	(-2.40)	(0.33)	(-0.64)	(-1.22)	(6.12)	(0.49)	(-0.11)	(2.91)	(-1.89)	(-1.84)	(-0.44)
Metropolitan Region	-0.026**	-0.040**	-0.015	-0.0068	-0.00080	-0.014*	-0.014	0.0067	-0.10	-0.019**	-0.039***	-0.0010
	(-2.43)	(-2.29)	(-1.29)	(-1.03)	(-0.077)	(-1.76)	(-0.38)	(0.17)	(-1.31)	(-2.12)	(-2.58)	(-0.11)
Dummies for states?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Constant	0.15***	0.36***	0.034	0.035	0.13**	-0.021	0.34	0.50**	-0.39*	0.11***	0.24***	0.055
	(3.07)	(4.25)	(0.80)	(1.25)	(2.24)	(-1.59)	(1.63)	(2.18)	(-1.67)	(2.69)	(3.33)	(1.33)
Observations	9748	4842	4906	9748	4842	4906	1234	896	338	9748	4842	4906
Adjusted R2	0.05	0.03	0.02	0.03	0.03	0.01	0.08	0.10	0.06	0.03	0.02	0.00

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

Table A.8 – Difference-in-Differences Estimates for the Impact of the Law of 1998 on the Intensive Margin of Labour Supply and Household Chores - *15 vs. 16*

Bandwidth of 16 weeks

	Household Chores			Weekly Hours Worked - Formal			Weekly Hours Worked - Informal		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Eligible*D_{99} (*DD*)	-0.020	-0.049	0.0067	7.07	4.27	11.6***	-2.04	-0.26	-4.37
	(-1.03)	(-1.50)	(0.31)	(1.09)	(0.54)	(6.10)	(-0.61)	(-0.067)	(-0.67)
Eligible	-0.00044	0.0094	-0.0091	2.86	5.73	-1.93	-1.16	-3.00	2.88
	(-0.032)	(0.39)	(-0.62)	(0.72)	(1.20)	(-1.15)	(-0.52)	(-1.19)	(0.62)
D_{99} (1998=0, 1999=1)	0.00038	0.050**	-0.041***	-12.8**	-9.39	-20.4***	4.09*	2.54	7.83
	(0.028)	(2.15)	(-2.69)	(-2.55)	(-1.54)	(-11.9)	(1.69)	(0.91)	(1.60)
Male	-0.35***	*Na*	*Na*	-1.68	*Na*	*Na*	2.09	*Na*	*Na*
	(-35.6)	*Na*	*Na*	(-0.48)	*Na*	*Na*	(1.09)	*Na*	*Na*
White	-0.087***	-0.10***	-0.078***	9.97***	10.7**	7.91***	-4.94***	-6.23***	-0.72
	(-8.40)	(-5.73)	(-6.61)	(2.80)	(2.55)	(4.75)	(-2.70)	(-2.95)	(-0.20)
Years of Schooling of the Household Head	-0.0055***	0.000078	-0.0098***	0.67	0.83	-0.33**	-0.31	-0.36	0.29
	(-3.68)	(0.031)	(-5.68)	(1.38)	(1.43)	(-2.16)	(-1.23)	(-1.28)	(0.56)
Age of Household Head	-0.00014	-0.00060	0.00023	0.082	0.083	-0.0087	-0.054	-0.084	0.057
	(-0.37)	(-0.90)	(0.54)	(0.65)	(0.56)	(-0.28)	(-0.80)	(-1.07)	(0.44)
Gender of the Household Head (=1 if male)	-0.020	0.0024	-0.032	1.03	-6.88	256.5***	-2.75	-0.94	-13.8
	(-0.47)	(0.033)	(-0.69)	(0.077)	(-0.51)	(130.3)	(-0.50)	(-0.15)	(-1.34)
Metropolitan Region	-0.053***	-0.025	-0.075***	-1.54	1.79	-11.9***	0.78	0.11	6.11
	(-3.81)	(-0.98)	(-5.42)	(-0.36)	(0.34)	(-6.96)	(0.34)	(0.042)	(1.17)
Dummies for states?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Constant	0.92***	0.52***	0.95***	-13.3	1.47	-494.3***	22.6	13.9	53.3***
	(12.8)	(4.21)	(11.5)	(-0.56)	(0.062)	(-251.0)	(1.60)	(0.89)	(3.52)
Sigma				44.0***	43.7***	40.1***	27.3***	26.5***	27.1***
				(37.7)	(31.2)	(53.6)	(43.6)	(37.4)	(22.3)
Observations	7829	3605	4224	1234	896	338	1234	896	338
Adjusted R2/Pseudo-R2	0.17	0.04	0.03						

Note: Robust T-statistics in parentheses, and robust standard errors in brackets. *, **,*** Statistically significant at 10%, 5%, and 1% respectively.

Table A.9 – Difference-in-Differences Estimates for the Impact of the Law of 1998 on Schooling outcome - *15 vs. 16*
Bandwidth of 16 weeks

	School Attendance		
	All	Boys	Girls
Eligible*D_{99} (*DD*)	-0.016	0.0040	-0.038**
	(-1.18)	(0.20)	(-2.00)
Eligible	-0.010	-0.025*	0.0060
	(-1.02)	(-1.73)	(0.41)
D_{99} (1998=0, 1999=1)	0.042***	0.020	0.065***
	(4.37)	(1.47)	(4.76)
Male	-0.0064	*Na*	*Na*
	(-0.94)	*Na*	*Na*
White	0.047***	0.050***	0.044***
	(6.21)	(4.62)	(4.25)
Years of Schooling of the Household Head	0.0048***	0.0046***	0.0049***
	(4.44)	(2.97)	(3.25)
Age of Household Head	-0.00010	-0.00029	0.000090
	(-0.38)	(-0.74)	(0.23)
Gender of the Household Head (=1 if male)	0.035	0.077	-0.010
	(1.03)	(1.54)	(-0.24)
Metropolitan Region	0.011	-0.010	0.031**
	(1.07)	(-0.72)	(2.10)
Dummies for states?	*Yes*	*Yes*	*Yes*
Constant	0.70***	0.69***	0.71***
	(12.5)	(8.39)	(9.38)
Observations	9768	4855	4913
Adjusted R2	0.01	0.01	0.02

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

Table A.10 – Parametric ITT Estimates for the Impact of the Apprenticeship Programme on the Extensive Margin of Labour Supply
13 vs. 14
Bandwidth of 16 weeks

Polynomial degree	Labour Force Participation			Participation Rate in Formal Sector			Formal Paid Work – occupation			Informal			Domestic Work		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
0	0.022	0.035*	0.0028	0.0044**	0.0032	0.0030	0.098**	0.077	0.069	0.018	0.032*	0.0062	0.016	-0.0087	0.010
	(1.32)	(1.81)	(0.17)	(2.04)	(1.46)	(1.48)	(2.15)	(1.45)	(1.43)	(1.04)	(1.67)	(0.35)	(0.58)	(-0.24)	(0.64)
1	-0.084***	-0.047	-0.088***	0.0014	0.0034	0.0024	0.098	0.022	0.14	-0.086***	-0.050*	-0.088***	0.026	-0.025	0.024
	(-4.13)	(-1.60)	(-3.74)	(0.49)	(0.66)	(1.11)	(1.51)	(0.37)	(1.40)	(-4.09)	(-1.73)	(-3.26)	(0.56)	(-0.34)	(0.94)
2	-0.084***	-0.049	-0.087***	0.0016	0.0035	0.0023	0.097	0.030	0.14	-0.086***	-0.052*	-0.085***	0.015	-0.031	0.025
	(-3.96)	(-1.62)	(-3.65)	(0.66)	(0.70)	(1.08)	(1.53)	(0.57)	(1.36)	(-3.98)	(-1.74)	(-3.19)	(0.33)	(-0.43)	(1.00)
3	-0.067**	0.0040	-0.073**	-0.0019	0.0064	-0.0039	-0.011	0.026	0.012	-0.065**	-0.0020	-0.049	0.093*	0.071	0.042
	(-2.18)	(0.13)	(-2.26)	(-0.66)	(0.92)	(-1.27)	(-0.16)	(0.34)	(0.15)	(-2.10)	(-0.064)	(-1.28)	(1.72)	(0.72)	(1.06)
Spline linear	-0.084***	-0.049	-0.088***	0.0018	0.0034	0.0025	0.098	0.029	0.15	-0.086***	-0.052*	-0.086***	0.012	-0.031	0.026
	(-3.87)	(-1.60)	(-3.55)	(0.77)	(0.70)	(1.21)	(1.51)	(0.55)	(1.34)	(-3.89)	(-1.72)	(-3.16)	(0.27)	(-0.43)	(1.09)
Spline quadratic	-0.067*	0.031	-0.078*	-0.0012	0.0067	-0.0034	-0.038	0.043	-0.065	-0.066*	0.025	-0.055	0.13***	0.12	0.046
	(-1.73)	(0.99)	(-1.88)	(-0.52)	(0.96)	(-1.30)	(-0.54)	(0.55)	(-0.71)	(-1.69)	(0.80)	(-1.23)	(2.69)	(1.21)	(1.28)
Observations	2440	1201	1239	2440	1201	1239	110	59	51	2440	1201	1239	2330	1141	1189

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

Table A.11 – Parametric ITT Estimates for the Impact of the Apprenticeship Programme on Schooling outcome
13 vs. 14
Bandwidth of 16 weeks

Polynomial degree	School Attendance		
	All	Boys	Girls
0	-0.0012	0.0080	-0.0083
	(-0.14)	(0.64)	(-0.61)
1	0.025	0.059**	0.010
	(1.55)	(2.51)	(0.39)
2	0.025	0.057**	0.014
	(1.52)	(2.38)	(0.53)
3	0.040**	0.043	0.050*
	(2.02)	(1.34)	(1.89)
Spline linear	0.025	0.058**	0.014
	(1.49)	(2.36)	(0.54)
Spline quadratic	0.045*	0.042	0.062**
	(1.90)	(1.20)	(2.41)
Observations	2440	1201	1239

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

Table A.12 – Parametric ITT Estimates for the Impact of the Apprenticeship Programme on the Extensive Margin of Labour Supply - *15 vs. 16*
Bandwidth of 16 weeks

Polynomial degree	Labour Force Participation			Participation Rate in Formal Sector			Formal Paid Work – occupation			Informal			Domestic Work		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
0	0.041**	0.050**	0.034	0.0055	0.014	-0.0025	0.032	0.060	-0.018	0.037**	0.040	0.036	-0.016	-0.028	-0.018
	(2.29)	(2.27)	(1.20)	(0.93)	(1.49)	(-0.46)	(0.90)	(1.34)	(-0.41)	(2.07)	(1.62)	(1.32)	(-0.77)	(-0.91)	(-0.74)
1	0.021	0.017	0.041	-0.0059	0.0073	-0.017**	-0.029	0.023	-0.12**	0.027	0.011	0.057	-0.0017	-0.0029	-0.044
	(0.65)	(0.40)	(0.71)	(-0.58)	(0.43)	(-2.36)	(-0.48)	(0.28)	(-1.96)	(0.86)	(0.23)	(0.99)	(-0.050)	(-0.059)	(-0.89)
2	0.021	0.017	0.040	-0.0058	0.0075	-0.017**	-0.030	0.023	-0.12*	0.026	0.010	0.056	-0.0029	-0.0037	-0.043
	(0.65)	(0.38)	(0.71)	(-0.58)	(0.46)	(-2.35)	(-0.52)	(0.29)	(-1.97)	(0.86)	(0.21)	(1.01)	(-0.087)	(-0.074)	(-0.89)
3	0.041	0.047	0.045	-0.017	-0.015	-0.019**	-0.12*	-0.11	-0.14**	0.056	0.058	0.062	-0.011	-0.036	-0.021
	(1.02)	(0.89)	(0.61)	(-1.65)	(-0.85)	(-2.45)	(-1.90)	(-1.32)	(-2.32)	(1.40)	(0.98)	(0.85)	(-0.24)	(-0.55)	(-0.30)
Spline linear	0.020	0.015	0.040	-0.0055	0.0082	-0.017**	-0.028	0.029	-0.12*	0.025	0.0076	0.056	-0.0024	-0.0026	-0.043
	(0.63)	(0.34)	(0.73)	(-0.56)	(0.53)	(-2.33)	(-0.48)	(0.39)	(-1.95)	(0.84)	(0.16)	(1.04)	(-0.075)	(-0.052)	(-0.88)
Spline quadratic	0.032	0.019	0.049	-0.020**	-0.017	-0.022***	-0.14**	-0.11	-0.16**	0.049	0.032	0.070	-0.00046	0.0030	-0.010
	(0.79)	(0.39)	(0.63)	(-2.04)	(-1.11)	(-2.82)	(-2.22)	(-1.41)	(-2.40)	(1.24)	(0.60)	(0.91)	(-0.0099)	(0.056)	(-0.14)
Observations	2243	1113	1130	2243	1113	1130	356	228	128	2243	1113	1130	1887	885	1002

Source: PNAD 2002.

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

Table A.13 – Parametric ITT Estimates for the Impact of the Apprenticeship Programme on Schooling Outcome - *15 vs. 16*
Bandwidth of 16 weeks

Polynomial degree	School Attendance		
	All	Boys	Girls
0	-0.020	-0.020	-0.019
	(-1.55)	(-1.24)	(-1.02)
1	0.041***	0.059**	0.020
	(2.60)	(2.27)	(0.58)
2	0.039**	0.058**	0.017
	(2.44)	(2.26)	(0.49)
3	0.025	0.0077	0.039
	(1.22)	(0.23)	(0.91)
Spline linear	0.040**	0.058**	0.018
	(2.54)	(2.26)	(0.52)
Spline quadratic	0.035*	0.0063	0.061
	(1.79)	(0.15)	(1.64)
Observations	2243	1113	1130

Source: PNAD 2002.
Note: Clustered T-statistics in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table A.14 – Difference-in-Differences Estimates – Apprenticeship Programme of 2000 – Extensive Margin of Labour Supply - *15 vs. 16*
Bandwidth of 16 weeks

	LFPR			LFPR - Formal			Formal Paid Work - Occupation			LFPR - Informal		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
T*Year	-0.012	-0.013	-0.012	0.0046	0.0022	0.0067	-0.042	-0.050	-0.013	-0.017	-0.015	-0.018
	(-0.87)	(-0.57)	(-0.69)	(0.53)	(0.16)	(0.62)	(-0.78)	(-0.81)	(-0.10)	(-1.39)	(-0.76)	(-1.28)
T (15=0; 16=1)	0.021***	0.035***	0.0088	0.0089**	0.017**	0.0018	0.074	0.071	0.059	0.012*	0.018	0.0069
	(2.92)	(2.67)	(1.28)	(2.39)	(2.55)	(0.48)	(1.56)	(1.37)	(0.49)	(1.93)	(1.58)	(1.19)
Year (1999=0; 2002=1)	0.19***	0.20***	0.18***	0.076***	0.086***	0.066***	0.16***	0.16***	0.12	0.12***	0.12***	0.12***
	(19.6)	(13.0)	(15.2)	(12.5)	(8.98)	(8.88)	(3.80)	(3.52)	(1.16)	(13.6)	(8.49)	(11.4)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	9876	4832	5044	9876	4832	5044	1637	1018	619	9876	4832	5044
Adjusted R2	0.09	0.08	0.09	0.05	0.05	0.05	0.07	0.07	0.09	0.05	0.04	0.05

Source: PNADs 1999 and 2002.
Note: Robust T-statistics in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The controls include dummy variables for gender (male), ethnicity (white), head years of schooling, age and gender (=1 if male), dummy for states and metropolitan area.

Table A.15 – Difference-in-Differences Estimates – Apprenticeship Programme of 2000 – Household Chores and Intensive Margin of Labour Supply - *15 vs. 16*

Bandwidth of 16 weeks

	Household Chores			Weekly Hours Worked – Formal			Weekly Hours Worked – Informal		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
T*Year	-0.035	-0.046	-0.031	-8.13	-9.42	-2.75	2.50	3.54	0.97
	(-1.49)	(-1.17)	(-1.12)	(-0.99)	(-0.98)	(-0.16)	(0.72)	(0.90)	(0.12)
T (15=0; 16=1)	0.015	0.013	0.020	11.5	11.7	7.98	-5.11*	-4.86	-5.02
	(1.13)	(0.57)	(1.26)	(1.51)	(1.35)	(0.49)	(-1.70)	(-1.50)	(-0.63)
Year (1999=0; 2002=1)	-0.0062	-0.023	0.013	23.4***	25.5***	14.7	-8.49***	-8.45***	-8.53
	(-0.37)	(-0.84)	(0.64)	(3.54)	(3.33)	(1.10)	(-3.27)	(-2.94)	(-1.31)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Sigma				47.9***	48.6***	44.4***	28.5***	28.0***	28.0***
				(46.4)	(36.4)	(27.3)	(50.3)	(39.8)	(30.2)
Observations	6155	2892	3263	1662	1038	624	1662	1038	624
Adjusted R2/Pseudo-R2	0.15	0.04	0.02						

Source: PNADs 1999 and 2002.

Note: Robust T-statistics in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The controls include dummy variables for gender (male), ethnicity (white), head years of schooling, age and gender (=1 if male), dummy for states and metropolitan area.

Table A.16 – Difference-in-Differences Estimates – Apprenticeship Programme of 2000 – Schooling outcome - *15 vs. 16*
Bandwidth of 16 weeks

	School Attendance		
	All	Boys	Girls
T*Year	-0.021	-0.020	-0.022
	(-1.45)	(-0.96)	(-1.06)
T (15=0; 16=1)	-0.0071	-0.020*	0.0055
	(-0.94)	(-1.82)	(0.52)
Year (1999=0; 2002=1)	-0.19***	-0.19***	-0.20***
	(-18.7)	(-13.0)	(-13.3)
Controls?	*Yes*	*Yes*	*Yes*
Observations	9876	4832	5044
Adjusted R2	0.08	0.08	0.08

Source: PNADs 1999 and 2002.
Note: Robust T-statistics in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The controls include dummy variables for gender (male), ethnicity (white), head years of schooling, age and gender (=1 if male), dummy for states and metropolitan area.

Table A.17 – Difference-in-Differences Estimates – Composite Effect of the Laws of 1998 and 2000 – Extensive Margin of Labour Supply - *15 vs. 16*

Bandwidth of 16 weeks

	LFPR			LFPR - Formal			Formal Paid Work - Occupation			LFPR - Informal		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
T*Year	0.0036	0.011	-0.0040	0.0056	0.012	-0.00057	0.057	0.060	0.064	-0.0020	-0.0011	-0.0034
	(0.25)	(0.51)	(-0.22)	(0.56)	(0.80)	(-0.045)	(1.12)	(0.96)	(0.68)	(-0.17)	(-0.058)	(-0.23)
T (15=0; 16=1)	0.0063	0.0039	0.0087	0.0030	0.0011	0.0044	0.0021	-0.0018	0.0063	0.0033	0.0028	0.0043
	(0.51)	(0.21)	(0.54)	(0.33)	(0.082)	(0.38)	(0.087)	(-0.055)	(0.17)	(0.33)	(0.18)	(0.33)
Year (1998=0; 2002=1)	-0.23***	-0.26***	-0.21***	-0.12***	-0.13***	-0.10***	-0.20***	-0.20***	-0.20***	-0.12***	-0.13***	-0.11***
	(-22.8)	(-16.2)	(-15.9)	(-16.6)	(-12.1)	(-11.3)	(-5.55)	(-4.56)	(-2.90)	(-13.6)	(-9.20)	(-10.0)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	10422	5076	5346	10422	5076	5346	1998	1172	826	10422	5076	5346
Adjusted R2	0.11	0.11	0.09	0.06	0.07	0.06	0.08	0.07	0.08	0.05	0.04	0.04

Source: PNADs 1998 and 2001.

Note: Robust T-statistics in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The controls include dummy variables for gender (male), ethnicity (white), head years of schooling, age and gender (=1 if male), dummy for states and metropolitan area.

Table A.18 – Linear Probability Model: Marginal Effects for Participation in the Formal Labour Force in 1997

13 and 14 in Sept 1997

VARIABLES	Formal Worker
Male	0.027
	(0.48)
White	0.00030
	(0.0063)
Hourly Wage (in ln)	0.059*
	(1.71)
Household Income (in ln) – net of children's income	0.056*
	(1.77)
Mother's years of schooling	0.0043
	(0.43)
Father's years of schooling	0.0017
	(1.05)
Mother's Age	-0.0051
	(-0.55)
Father's Age	0.000029
	(0.015)
# of Siblings 0-5	-0.017
	(-0.33)
# of Siblings 6-11	-0.0062
	(-0.10)
# of Siblings 12-13	-0.072
	(-1.25)
# of Siblings 14-15	0.092**
	(2.02)
# of Siblings 16-17	0.057
	(0.95)
# of Siblings >=18	0.026
	(0.49)
Land Title	-0.032
	(-0.43)
School Attendance	-0.054
	(-0.79)
Metropolitan Region	0.013
	(0.25)
Constant	-0.41*
	(-1.94)
Observations	193
R2	0.03

Source: PNAD 1997. Robust standard errors in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table A.19 – Participation Rate and Occupation in the Formal Labour Force – 1997
13 vs. 14 in Sept 1997
Bandwidth of 52 weeks

Functional Formal of *h(z)*	Formal Labour Force	Formal Occupation
0	0.0096***	0.16***
	(5.93)	(6.39)
1	0.0025	0.11*
	(0.93)	(1.89)
2	0.0029	0.11***
	(1.21)	(2.70)
3	0.0022	0.060
	(0.76)	(0.94)
Spline linear	0.0031	0.12***
	(1.29)	(2.67)
Spline quadratic	0.0026	0.063
	(0.90)	(1.07)
Observations	7336	315

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

APPENDIX 2: Tables and Figures from Chapter 2

Table B.1 – Long Run Effects on Hourly Log Wages – White and Non-White Males

26 Weeks Bandwidth – with controls

	White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	-0.016	0.038	0.038	0.16**	0.038	0.19**	-0.064	-0.012	-0.0093	0.11	-0.0089	0.14
	(-0.52)	(0.64)	(0.66)	(2.14)	(0.65)	(2.23)	(-1.11)	(-0.15)	(-0.12)	(1.31)	(-0.11)	(1.49)
D*2008							0.0027	0.0017	-0.0025	-0.0057	-0.0033	-0.0064
							(0.034)	(0.022)	(-0.032)	(-0.074)	(-0.043)	(-0.082)
D*2009							0.068	0.064	0.061	0.071	0.060	0.068
							(0.89)	(0.83)	(0.80)	(0.92)	(0.79)	(0.88)
D*2011							0.10	0.099	0.097	0.11	0.097	0.10
							(1.22)	(1.20)	(1.18)	(1.29)	(1.18)	(1.27)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1793	1793	1793	1793	1793	1793	1793	1793	1793	1793	1793	1793
	Non-White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	-0.024	-0.027	-0.029	-0.11*	-0.029	-0.12	-0.0011	-0.0018	-0.0043	-0.091	-0.0043	-0.098
	(-1.10)	(-0.56)	(-0.60)	(-1.71)	(-0.60)	(-1.60)	(-0.025)	(-0.028)	(-0.068)	(-1.13)	(-0.069)	(-1.09)
D*2008							0.022	0.022	0.020	0.024	0.020	0.023
							(0.38)	(0.38)	(0.34)	(0.41)	(0.34)	(0.40)
D*2009							-0.11*	-0.11*	-0.11*	-0.11*	-0.11*	-0.11*
							(-1.77)	(-1.77)	(-1.77)	(-1.69)	(-1.77)	(-1.71)
D*2011							-0.0087	-0.0087	-0.0051	-0.0035	-0.0048	-0.0042
							(-0.14)	(-0.14)	(-0.086)	(-0.059)	(-0.079)	(-0.071)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2653	2653	2653	2653	2653	2653	2653	2653	2653	2653	2653	2653

Source: PNADs 2007, 2008, 2009, and 2011. Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5%, and 10% respectively

Table B.2 – Long Run Effects on Being Employed – White and Non-White Males

26 Weeks Bandwidth – with controls

	White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	0.0053	-0.0036	-0.0033	-0.0016	-0.0035	-0.0048	0.020	0.0096	0.011	0.015	0.011	0.011
	(0.30)	(-0.093)	(-0.087)	(-0.032)	(-0.090)	(-0.090)	(0.46)	(0.17)	(0.21)	(0.23)	(0.21)	(0.17)
D*2008							-0.047	-0.046	-0.049	-0.049	-0.049	-0.049
							(-0.82)	(-0.82)	(-0.87)	(-0.87)	(-0.87)	(-0.87)
D*2009							0.0027	0.0036	0.0018	0.0020	0.0016	0.0017
							(0.046)	(0.061)	(0.030)	(0.034)	(0.028)	(0.030)
D*2011							-0.013	-0.013	-0.014	-0.014	-0.014	-0.014
							(-0.24)	(-0.24)	(-0.27)	(-0.27)	(-0.27)	(-0.27)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2174	2174	2174	2174	2174	2174	2174	2174	2174	2174	2174	2174

	Non-White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	-0.0056	-0.024	-0.024	-0.079**	-0.024	-0.092**	0.036	0.019	0.018	-0.037	0.018	-0.050
	(-0.37)	(-0.80)	(-0.80)	(-2.01)	(-0.80)	(-2.09)	(1.15)	(0.44)	(0.44)	(-0.74)	(0.44)	(-0.91)
D*2008							-0.037	-0.037	-0.037	-0.035	-0.037	-0.035
							(-0.88)	(-0.88)	(-0.88)	(-0.83)	(-0.88)	(-0.82)
D*2009							-0.050	-0.049	-0.049	-0.046	-0.049	-0.047
							(-1.07)	(-1.05)	(-1.05)	(-0.98)	(-1.05)	(-1.00)
D*2011							-0.073*	-0.072*	-0.072*	-0.070	-0.072*	-0.071*
							(-1.70)	(-1.69)	(-1.68)	(-1.63)	(-1.68)	(-1.65)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3298	3298	3298	3298	3298	3298	3298	3298	3298	3298	3298	3298

Source: PNADs 2007, 2008, 2009, and 2011.

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

Table B.3 – Long Run Effects on Being a Formal Employee – White and Non-White Males

26 Weeks Bandwidth – with controls

	White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	0.0098	0.048	0.048	0.10*	0.048	0.10*	0.026	0.066	0.067	0.12	0.067	0.12
	(0.40)	(1.04)	(1.04)	(1.76)	(1.04)	(1.66)	(0.49)	(0.96)	(0.98)	(1.54)	(0.97)	(1.47)
D*2008							-0.053	-0.053	-0.055	-0.056	-0.054	-0.055
							(-0.73)	(-0.74)	(-0.77)	(-0.78)	(-0.76)	(-0.77)
D*2009							-0.025	-0.028	-0.030	-0.025	-0.029	-0.025
							(-0.36)	(-0.41)	(-0.43)	(-0.37)	(-0.42)	(-0.37)
D*2011							0.0093	0.0086	0.0075	0.011	0.0079	0.010
							(0.13)	(0.12)	(0.11)	(0.16)	(0.11)	(0.15)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2174	2174	2174	2174	2174	2174	2174	2174	2174	2174	2174	2174
	Non-White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	0.0085	-0.029	-0.029	-0.095**	-0.029	-0.11**	0.028	-0.0097	-0.0100	-0.078	-0.0099	-0.092
	(0.45)	(-0.79)	(-0.80)	(-2.01)	(-0.79)	(-2.07)	(0.72)	(-0.19)	(-0.20)	(-1.26)	(-0.20)	(-1.37)
D*2008							-0.013	-0.013	-0.013	-0.011	-0.013	-0.011
							(-0.25)	(-0.24)	(-0.25)	(-0.19)	(-0.24)	(-0.19)
D*2009							-0.035	-0.034	-0.034	-0.030	-0.034	-0.031
							(-0.60)	(-0.59)	(-0.58)	(-0.52)	(-0.58)	(-0.53)
D*2011							-0.027	-0.026	-0.025	-0.023	-0.025	-0.024
							(-0.52)	(-0.50)	(-0.49)	(-0.45)	(-0.49)	(-0.46)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3298	3298	3298	3298	3298	3298	3298	3298	3298	3298	3298	3298

Source: PNADs 2007, 2008, 2009, and 2011.

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

Table B.4 – Long Run Effects on Holding or Pursuing a College Degree – White and Non-White Males

26 Weeks Bandwidth – with controls

							White Males					
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	0.023	0.089**	0.091**	0.064	0.091**	0.059	0.020	0.087*	0.089*	0.063	0.089*	0.058
	(1.15)	(2.30)	(2.36)	(1.30)	(2.37)	(1.11)	(0.55)	(1.68)	(1.73)	(1.06)	(1.73)	(0.91)
D*2008							0.0098	0.011	0.0098	0.0095	0.010	0.011
							(0.19)	(0.22)	(0.19)	(0.19)	(0.20)	(0.21)
D*2009							-0.0017	-0.0051	-0.0058	-0.0083	-0.0057	-0.0076
							(-0.032)	(-0.095)	(-0.11)	(-0.15)	(-0.11)	(-0.14)
D*2011							0.0045	0.0054	0.0048	0.0027	0.0050	0.0039
							(0.089)	(0.11)	(0.096)	(0.054)	(0.10)	(0.078)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2972	2972	2972	2972	2972	2972	2972	2972	2972	2972	2972	2972

							Non-White Males					
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	-0.0012	-0.0009	-0.0010	-0.010	-0.0012	-0.015	-0.0051	-0.0048	-0.0047	-0.014	-0.0047	-0.019
	(-0.01)	(-0.032)	(-0.039)	(-0.30)	(-0.045)	(-0.40)	(-0.24)	(-0.15)	(-0.14)	(-0.37)	(-0.14)	(-0.46)
D*2008							-0.00087	-0.001	-0.00015	0.00006	-0.000094	0.00016
							(-0.031)	(-0.03)	(-0.006)	(0.0021)	(-0.0034)	(0.0058)
D*2009							0.0043	0.0043	0.0044	0.0048	0.0043	0.0046
							(0.12)	(0.12)	(0.12)	(0.13)	(0.12)	(0.13)
D*2011							0.011	0.011	0.0096	0.0097	0.0092	0.0093
							(0.34)	(0.34)	(0.29)	(0.29)	(0.28)	(0.28)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3936	3936	3936	3936	3936	3936	3936	3936	3936	3936	3936	3936

Source: PNADs 2007, 2008, 2009, and 2011.

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

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Table B.5 – Effect of the Ban on Occupation of Adult Males – ITT Estimates

26 Weeks Bandwidth – Homogeneous Time Effects

	Directors in General	Science & Arts	Technicians	Administrative Services	Service Sector	Commerce Sector	Agricultural Sector	Civil Construction	Army Force	Undefined
					White Males					
D	0.027	0.047*	0.032	-0.014	0.0015	-0.010	0.0099	-0.076	-0.020*	0.0030
	(1.20)	(1.93)	(0.98)	(-0.35)	(0.044)	(-0.27)	(1.30)	(-1.56)	(-1.81)	(1.04)
Observations	*1978*	*1978*	*1978*	*1978*	*1978*	*1978*	*1978*	*1978*	*1978*	*1978*
					Non-White Males					
D	0.0054	0.015	-0.028	0.013	-0.030	-0.0034	0.011	0.010	0.0048	0.0030
	(0.35)	(0.86)	(-1.02)	(0.35)	(-0.91)	(-0.11)	(1.19)	(0.23)	(0.59)	(1.03)
Observations	*2851*	*2851*	*2851*	*2851*	*2851*	*2851*	*2851*	*2851*	*2851*	*2851*

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. * Statistically significant at the 10% level.

Table B.6 – Effect of the Ban on Occupation of Adult Males – ITT Estimates
26 Weeks Bandwidth – Heterogeneous Time Effects

	Directors in General	Science & Arts	Technicians	Administrative Services	Service Sector	Commerce Sector	Agricultural Sector	Civil Construction	Army Force	Undefined
					White Males					
D	0.053**	0.059	-0.00027	0.015	-0.0025	-0.0060	0.0088	-0.12	-0.0091	0.0051
	(2.05)	(1.46)	(-0.0063)	(0.25)	(-0.051)	(-0.14)	(0.77)	(-1.63)	(-0.45)	(1.05)
Dt2	-0.068**	-0.023	0.065	-0.026	-0.00067	0.016	0.0028	0.041	0.00052	-0.0083
	(-2.49)	(-0.58)	(1.40)	(-0.47)	(-0.012)	(0.37)	(0.29)	(0.56)	(0.024)	(-1.02)
Dt3	-0.011	-0.013	0.026	0.0091	-0.017	-0.017	-0.0038	0.047	-0.021	-0.00044
	(-0.41)	(-0.29)	(0.52)	(0.17)	(-0.30)	(-0.38)	(-0.24)	(0.70)	(-0.96)	(-0.87)
Dt4	-0.022	-0.011	0.032	-0.079	0.028	-0.0096	0.0046	0.075	-0.017	-0.00039
	(-0.78)	(-0.25)	(0.79)	(-1.60)	(0.58)	(-0.21)	(0.49)	(1.11)	(-1.07)	(-0.95)
Observations	1978	1978	1978	1978	1978	1978	1978	1978	1978	1978
					Non-White Males					
D	0.0012	0.025	-0.0013	0.039	-0.061	0.017	0.016	-0.042	-0.00070	0.0066
	(0.069)	(1.41)	(-0.038)	(0.92)	(-1.63)	(0.45)	(0.98)	(-0.71)	(-0.049)	(1.04)
Dt2	0.011	0.013	-0.026	-0.065*	0.047	0.0051	-0.016	0.021	0.014	-0.0043
	(0.79)	(0.74)	(-0.74)	(-1.69)	(1.08)	(0.15)	(-1.21)	(0.42)	(0.95)	(-1.01)
Dt3	0.00069	-0.014	-0.040	-0.031	0.051	-0.046	-0.00046	0.091	-0.0061	-0.0045
	(0.033)	(-0.63)	(-1.21)	(-0.71)	(1.13)	(-1.23)	(-0.037)	(1.65)	(-0.39)	(-1.02)
Dt4	0.0045	-0.032	-0.035	-0.011	0.024	-0.031	-0.0044	0.078	0.012	-0.0045
	(0.24)	(-1.67)	(-1.13)	(-0.31)	(0.67)	(-0.76)	(-0.28)	(1.17)	(0.90)	(-1.03)
Observations	2851	2851	2851	2851	2851	2851	2851	2851	2851	2851

Source: PNADs 2007, 2008, 2009, and 2011.
Note: Clustered T-statistics in parenthesis. **, * Statistically significant at 5% and10% respectively.

Table B.7 – Long Run Effects on Hourly Log Wages – White and Non-white Males

12 Weeks Bandwidth – Exclude School Attenders

	White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	0.063	0.16	0.16	0.12	0.15	0.096	0.11	0.21	0.21	0.17	0.20	0.14
	(1.20)	(1.44)	(1.43)	(0.84)	(1.39)	(0.54)	(1.42)	(1.60)	(1.60)	(1.04)	(1.56)	(0.72)
D*2008							-0.076	-0.078	-0.077	-0.081	-0.078	-0.072
							(-0.62)	(-0.63)	(-0.62)	(-0.66)	(-0.64)	(-0.59)
D*2009							-0.12	-0.12	-0.12	-0.13	-0.12	-0.12
							(-1.07)	(-1.09)	(-1.08)	(-1.11)	(-1.07)	(-1.05)
D*2011							0.0037	-0.00037	-0.00038	-0.0013	-0.00036	-0.00040
							(0.025)	(-0.0025)	(-0.0025)	(-0.0087)	(-0.0024)	(-0.0027)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	881	881	881	881	881	881	881	881	881	881	881	881
	Non-White Males											
	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	-0.026	-0.014	-0.026	0.020	-0.030	0.054	0.028	0.040	0.027	0.069	0.024	0.11
	(-0.75)	(-0.19)	(-0.34)	(0.20)	(-0.40)	(0.45)	(0.41)	(0.39)	(0.27)	(0.53)	(0.23)	(0.72)
D*2008							-0.021	-0.022	-0.023	-0.020	-0.024	-0.017
							(-0.25)	(-0.26)	(-0.27)	(-0.23)	(-0.28)	(-0.20)
D*2009							-0.18*	-0.18*	-0.18*	-0.18*	-0.18*	-0.18*
							(-1.77)	(-1.77)	(-1.75)	(-1.74)	(-1.74)	(-1.76)
D*2011							-0.0096	-0.010	-0.0068	-0.0086	-0.0077	-0.0085
							(-0.11)	(-0.11)	(-0.076)	(-0.096)	(-0.087)	(-0.095)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1294	1294	1294	1294	1294	1294	1294	1294	1294	1294	1294	1294

Source: PNADs 2007, 2008, 2009, and 2011.

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

Table B.8 – Long Run Effects on Being Employed – White and Non-white Males
12 Weeks Bandwidth – Exclude School Attenders

	White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	-0.0021	-0.027	-0.028	-0.048	-0.028	-0.054	0.028	0.00050	-0.00068	-0.023	-0.00089	-0.028
	(-0.082)	(-0.54)	(-0.56)	(-0.77)	(-0.55)	(-0.72)	(0.43)	(0.0061)	(-0.0082)	(-0.26)	(-0.011)	(-0.28)
D*2008							-0.10	-0.10	-0.10	-0.10	-0.10	-0.10
							(-1.14)	(-1.15)	(-1.16)	(-1.18)	(-1.15)	(-1.18)
D*2009							-0.024	-0.023	-0.023	-0.026	-0.023	-0.027
							(-0.28)	(-0.27)	(-0.27)	(-0.31)	(-0.27)	(-0.31)
D*2011							0.0036	0.0046	0.0043	0.0037	0.0044	0.0041
							(0.048)	(0.060)	(0.056)	(0.048)	(0.057)	(0.054)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1074	1074	1074	1074	1074	1074	1074	1074	1074	1074	1074	1074
	Non-White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	-0.0082	-0.077*	-0.081**	-0.022	-0.083**	0.0091	0.081	0.013	0.0095	0.069	0.0075	0.099
	(-0.39)	(-1.89)	(-1.99)	(-0.41)	(-2.01)	(0.15)	(1.65)	(0.21)	(0.15)	(0.95)	(0.12)	(1.25)
D*2008							-0.13*	-0.12*	-0.12*	-0.12*	-0.12*	-0.12*
							(-1.97)	(-1.85)	(-1.86)	(-1.80)	(-1.86)	(-1.77)
D*2009							-0.094	-0.091	-0.091	-0.091	-0.091	-0.091
							(-1.43)	(-1.38)	(-1.38)	(-1.37)	(-1.38)	(-1.38)
D*2011							-0.12*	-0.12*	-0.12*	-0.12*	-0.12*	-0.13*
							(-2.39)	(-2.38)	(-2.37)	(-2.38)	(-2.37)	(-2.44)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1591	1591	1591	1591	1591	1591	1591	1591	1591	1591	1591	1591

Source: PNADs 2007, 2008, 2009, and 2011.
Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table B.9 – Long Run Effects on Being a Formal Employee – White and Non-white Males

12 Weeks Bandwidth – Exclude School Attenders

	White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	0.044	0.0071	0.014	-0.011	0.019	-0.017	0.095	0.058	0.063	0.035	0.069	0.031
	(1.26)	(0.11)	(0.23)	(-0.15)	(0.30)	(-0.19)	(1.22)	(0.56)	(0.61)	(0.33)	(0.66)	(0.26)
D*2008							-0.090	-0.090	-0.087	-0.090	-0.088	-0.093
							(-0.83)	(-0.83)	(-0.80)	(-0.83)	(-0.81)	(-0.85)
D*2009							-0.065	-0.064	-0.064	-0.068	-0.065	-0.070
							(-0.65)	(-0.63)	(-0.63)	(-0.65)	(-0.64)	(-0.67)
D*2011							-0.043	-0.042	-0.040	-0.041	-0.040	-0.040
							(-0.42)	(-0.41)	(-0.39)	(-0.39)	(-0.38)	(-0.38)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1028	1028	1028	1028	1028	1028	1028	1028	1028	1028	1028	1028
	Non-White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	0.0044	-0.11**	-0.11**	-0.0097	-0.12**	0.043	0.11**	0.0018	-0.0053	0.10	-0.0100	0.16
	(0.16)	(-2.00)	(-2.16)	(-0.15)	(-2.22)	(0.56)	(2.00)	(0.023)	(-0.070)	(1.19)	(-0.13)	(1.62)
D*2008							-0.13*	-0.12	-0.12	-0.11	-0.12	-0.11
							(-1.68)	(-1.54)	(-1.57)	(-1.48)	(-1.58)	(-1.46)
D*2009							-0.12	-0.11	-0.11	-0.11	-0.11	-0.11
							(-1.39)	(-1.34)	(-1.35)	(-1.34)	(-1.34)	(-1.36)
D*2011							-0.17**	-0.16**	-0.17**	-0.17**	-0.17**	-0.18**
							(-2.21)	(-2.15)	(-2.16)	(-2.25)	(-2.17)	(-2.27)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1539	1539	1539	1539	1539	1539	1539	1539	1539	1539	1539	1539

Source: PNADs 2007, 2008, 2009, and 2011.

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

Table B.10 – Long Run Effects on Holding or Being Pursuing a College Degree –White and Non-White Males
12 Weeks Bandwidth

	White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	0.073**	0.12**	0.12**	0.16**	0.12**	0.24***	0.069	0.12	0.12	0.16*	0.12	0.19*
	(2.48)	(2.39)	(2.32)	(2.46)	(2.27)	(2.81)	(1.20)	(1.61)	(1.59)	(1.93)	(1.57)	(1.94)
D*2008							0.029	0.030	0.029	0.032	0.030	0.030
							(0.36)	(0.37)	(0.36)	(0.39)	(0.36)	(0.37)
D*2009							-0.0081	-0.0078	-0.0078	-0.0046	-0.0078	-0.0061
							(-0.10)	(-0.099)	(-0.098)	(-0.057)	(-0.098)	(-0.077)
D*2011							-0.0032	-0.0042	-0.0043	-0.0053	-0.0043	-0.0054
							(-0.043)	(-0.057)	(-0.059)	(-0.073)	(-0.058)	(-0.074)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1485	1485	1485	1485	1485	1485	1485	1485	1485	1485	1485	1485

	Non-White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	0.0052	0.014	0.012	-0.032	0.011	-0.058	0.040	0.051	0.050	0.0047	0.049	-0.014
	(0.27)	(0.37)	(0.33)	(-0.72)	(0.30)	(-1.22)	(1.14)	(1.08)	(1.06)	(0.089)	(1.03)	(-0.25)
D*2008							-0.059	-0.060	-0.060	-0.063	-0.060	-0.064
							(-1.25)	(-1.28)	(-1.29)	(-1.35)	(-1.30)	(-1.38)
D*2009							-0.037	-0.038	-0.038	-0.038	-0.038	-0.038
							(-0.66)	(-0.67)	(-0.68)	(-0.68)	(-0.68)	(-0.67)
D*2011							-0.044	-0.044	-0.045	-0.042	-0.045	-0.042
							(-0.85)	(-0.85)	(-0.86)	(-0.81)	(-0.86)	(-0.81)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1938	1938	1938	1938	1938	1938	1938	1938	1938	1938	1938	1938

Source: PNADs 2007, 2008, 2009, and 2011.
Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table B.11 – Long Run QTE on Hourly Log Wages –White and Non-White Males

12 Weeks Bandwidth – Exclude School Attenders

	Q10	Q25	Q50	Q75	Q90
			White		
D	0.28*	0.095	0.14	0.079	-0.053
	(1.94)	(0.64)	(0.84)	(0.28)	(-0.13)
			Non-White		
D	0.068	0.0055	-0.21*	-0.026	0.50**
	(0.64)	(0.059)	(-1.83)	(-0.18)	(2.03)

Source: PNADs 2007, 2008, 2009, and 2011.

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

Table B.12 – Long Run QTE on Hourly Log Wages –White and Non-White Males

12 Weeks Bandwidth – Exclude School Attenders

	White					Non-White				
	Q10	Q25	Q50	Q75	Q90	Q10	Q25	Q50	Q75	Q90
D	0.32	0.19	0.22	0.11	-0.044	0.14	0.029	-0.21*	-0.0038	0.42
	(1.56)	(1.03)	(1.18)	(0.38)	(-0.11)	(1.00)	(0.25)	(-1.65)	(-0.025)	(1.59)
D*2008	0.049	-0.080	-0.012	0.029	-0.16	0.0037	-0.077	0.028	-0.023	-0.14
	(0.24)	(-0.51)	(-0.076)	(0.14)	(-0.61)	(0.033)	(-0.69)	(0.26)	(-0.23)	(-1.09)
D*2009	-0.14	-0.076	-0.11	0.014	0.040	-0.14	-0.13	-0.22**	-0.15	-0.25
	(-0.82)	(-0.50)	(-0.74)	(0.066)	(0.13)	(-1.31)	(-1.38)	(-2.08)	(-1.37)	(-1.41)
D*2011	-0.062	-0.14	-0.089	-0.045	0.083	-0.088	-0.029	0.072	-0.057	0.32
	(-0.35)	(-0.95)	(-0.64)	(-0.21)	(0.28)	(-0.89)	(-0.35)	(0.77)	(-0.47)	(1.52)
Dummies for years?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	881	881	881	881	881	1294	1294	1294	1294	1294

Source: PNADs 2007, 2008, 2009, and 2011.

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

Table B.13 –Placebo Effects on Hourly Log Wages – White and Non-White Males
26 Weeks Bandwidth – Exclude School Attenders

	White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after June 1999; =0 if 14 before June)	-0.048*	0.025	0.025	-0.024	0.026	0.013	-0.047	0.025	0.026	-0.024	0.027	-0.046
	(-1.74)	(0.46)	(0.47)	(-0.35)	(0.49)	(0.21)	(-0.94)	(0.36)	(0.37)	(-0.30)	(0.39)	(-0.55)
D*2008							0.021	0.019	0.019	0.020	0.019	0.020
							(0.29)	(0.27)	(0.27)	(0.28)	(0.27)	(0.28)
D*2009							-0.056	-0.055	-0.055	-0.058	-0.056	-0.062
							(-0.78)	(-0.78)	(-0.78)	(-0.81)	(-0.79)	(-0.87)
D*2011							0.034	0.035	0.035	0.033	0.034	0.031
							(0.44)	(0.46)	(0.45)	(0.44)	(0.45)	(0.41)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2613	2613	2613	2613	2613	2613	2613	2613	2613	2613	2613	2613

	Non-White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after June 1999; =0 if 14 before June)	-0.026	-0.049	-0.050	-0.038	-0.049	-0.055	-0.013	-0.037	-0.037	-0.023	-0.037	-0.022
	(-1.14)	(-1.08)	(-1.09)	(-0.63)	(-1.08)	(-0.92)	(-0.31)	(-0.65)	(-0.66)	(-0.33)	(-0.65)	(-0.30)
D*2008							-0.040	-0.040	-0.039	-0.040	-0.039	-0.040
							(-0.64)	(-0.65)	(-0.64)	(-0.65)	(-0.64)	(-0.65)
D*2009							0.027	0.026	0.026	0.025	0.026	0.025
							(0.42)	(0.41)	(0.40)	(0.40)	(0.40)	(0.39)
D*2011							-0.033	-0.035	-0.035	-0.035	-0.035	-0.036
							(-0.56)	(-0.58)	(-0.59)	(-0.60)	(-0.59)	(-0.60)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3271	3271	3271	3271	3271	3271	3271	3271	3271	3271	3271	3271

Source: PNADs 2007, 2008, 2009, and 2011.
Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table B.14 – Placebo Effects on Being Employed – White and Non-White Males
26 Weeks Bandwidth – Exclude School Attenders

	White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after June 1999; =0 if 14 before June)	-0.032**	-0.029	-0.028	-0.017	-0.028	-0.0017	-0.049	-0.047	-0.046	-0.034	-0.045	-0.035
	(-2.04)	(-0.95)	(-0.93)	(-0.43)	(-0.91)	(-0.043)	(-1.51)	(-1.17)	(-1.15)	(-0.72)	(-1.13)	(-0.67)
D*2008							0.057	0.057	0.057	0.057	0.057	0.057
							(1.24)	(1.24)	(1.24)	(1.24)	(1.24)	(1.24)
D*2009							0.0087	0.0087	0.0082	0.0085	0.0076	0.0065
							(0.20)	(0.20)	(0.19)	(0.20)	(0.18)	(0.15)
D*2011							0.0050	0.0049	0.0048	0.0051	0.0047	0.0046
							(0.11)	(0.11)	(0.11)	(0.12)	(0.11)	(0.11)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3386	3386	3386	3386	3386	3386	3386	3386	3386	3386	3386	3386

	Non-White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after June 1999; =0 if 14 before June)	-0.0030	-0.025	-0.025	-0.021	-0.025	-0.044	0.020	-0.0011	-0.0013	0.0038	-0.0012	0.0038
	(-0.21)	(-0.87)	(-0.87)	(-0.52)	(-0.86)	(-1.17)	(0.75)	(-0.030)	(-0.035)	(0.080)	(-0.033)	(0.072)
D*2008							-0.036	-0.036	-0.035	-0.036	-0.036	-0.036
							(-0.92)	(-0.94)	(-0.92)	(-0.92)	(-0.92)	(-0.93)
D*2009							-0.046	-0.046	-0.046	-0.046	-0.046	-0.047
							(-1.22)	(-1.22)	(-1.22)	(-1.22)	(-1.22)	(-1.24)
D*2011							-0.013	-0.014	-0.014	-0.014	-0.014	-0.015
							(-0.32)	(-0.35)	(-0.36)	(-0.36)	(-0.35)	(-0.38)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	4308	4308	4308	4308	4308	4308	4308	4308	4308	4308	4308	4308

Source: PNADs 2007, 2008, 2009, and 2011.
Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table B.15 – Placebo Effects on Being a Formal Employee – White and Non-White Males
26 Weeks Bandwidth – Exclude School Attenders

	White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after June 1999; =0 if 14 before June)	-0.051**	-0.0019	-0.0031	-0.024	-0.0032	0.0082	-0.056	-0.0075	-0.0089	-0.034	-0.0090	-0.043
	(-2.46)	(-0.049)	(-0.079)	(-0.47)	(-0.082)	(0.16)	(-1.36)	(-0.14)	(-0.17)	(-0.56)	(-0.17)	(-0.66)
D*2008							0.015	0.013	0.013	0.014	0.013	0.014
							(0.25)	(0.22)	(0.22)	(0.23)	(0.22)	(0.23)
D*2009							-0.037	-0.036	-0.036	-0.037	-0.036	-0.039
							(-0.66)	(-0.64)	(-0.63)	(-0.65)	(-0.63)	(-0.68)
D*2011							0.043	0.044	0.044	0.043	0.044	0.042
							(0.73)	(0.74)	(0.75)	(0.73)	(0.75)	(0.71)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2649	2649	2649	2649	2649	2649	2649	2649	2649	2649	2649	2649
	Non-White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after June 1999; =0 if 14 before June)	-0.013	-0.016	-0.015	0.025	-0.015	-0.0049	0.0058	0.0020	0.0020	0.045	0.0022	0.052
	(-0.65)	(-0.39)	(-0.39)	(0.47)	(-0.38)	(-0.095)	(0.16)	(0.040)	(0.039)	(0.73)	(0.043)	(0.76)
D*2008							-0.024	-0.024	-0.022	-0.025	-0.022	-0.024
							(-0.44)	(-0.44)	(-0.41)	(-0.46)	(-0.42)	(-0.45)
D*2009							-0.012	-0.012	-0.013	-0.013	-0.013	-0.014
							(-0.24)	(-0.24)	(-0.25)	(-0.26)	(-0.25)	(-0.27)
D*2011							-0.038	-0.039	-0.039	-0.039	-0.038	-0.039
							(-0.74)	(-0.74)	(-0.74)	(-0.74)	(-0.74)	(-0.74)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3274	3274	3274	3274	3274	3274	3274	3274	3274	3274	3274	3274

Source: PNADs 2007, 2008, 2009, and 2011.
Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table B.16 – Placebo Effects on Holding or Being Pursuing a College Degree –White and Non-White Males
26 Weeks Bandwidth

	White Males											
D (=1 if 14 after June 1999; =0 if 14 before June)	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after June 1999; =0 if 14 before June)	-0.0045	0.012	0.011	0.0022	0.010	-0.023	-0.050	-0.034	-0.035	-0.046	-0.037	-0.050
	(-0.23)	(0.32)	(0.30)	(0.046)	(0.27)	(-0.52)	(-1.38)	(-0.70)	(-0.73)	(-0.81)	(-0.75)	(-0.82)
D*2008							0.074	0.073	0.073	0.073	0.073	0.073
							(1.41)	(1.39)	(1.39)	(1.39)	(1.39)	(1.40)
D*2009							0.024	0.024	0.025	0.025	0.026	0.027
							(0.46)	(0.46)	(0.48)	(0.47)	(0.50)	(0.52)
D*2011							0.086*	0.085*	0.086*	0.085*	0.086*	0.086*
							(1.70)	(1.70)	(1.70)	(1.70)	(1.71)	(1.70)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3386	3386	3386	3386	3386	3386	3386	3386	3386	3386	3386	3386
	Non-White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after June 1999; =0 if 14 before June)	0.0078	0.022	0.022	0.018	0.022	-0.0012	0.0054	0.020	0.020	0.015	0.020	0.010
	(0.65)	(1.00)	(1.01)	(0.64)	(1.00)	(-0.044)	(0.26)	(0.74)	(0.75)	(0.45)	(0.76)	(0.29)
D*2008							0.033	0.033	0.032	0.032	0.032	0.032
							(1.11)	(1.12)	(1.10)	(1.11)	(1.09)	(1.09)
D*2009							-0.022	-0.022	-0.022	-0.022	-0.022	-0.023
							(-0.64)	(-0.63)	(-0.63)	(-0.63)	(-0.63)	(-0.65)
D*2011							-0.0014	-0.00061	-0.00050	-0.00037	-0.00065	-0.0014
							(-0.044)	(-0.019)	(-0.015)	(-0.011)	(-0.020)	(-0.042)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	4308	4308	4308	4308	4308	4308	4308	4308	4308	4308	4308	4308

Source: PNADs 2007, 2008, 2009, and 2011.
Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table B.17 – Short Run ITT Estimates for Elasticity of Labour Supply

12 Weeks Bandwidth

	h(z) specifications						
	0	Linear	Quadratic	Cubic	Spline linear	Spline quadratic	Spline cubic
Ln WHW	-0.45***	-0.53***	-0.53***	-0.52***	-0.53***	-0.50***	-0.52***
	(-5.31)	(-7.12)	(-7.19)	(-6.63)	(-7.17)	(-6.22)	(-6.14)
Ln WHW*D1	0.024	0.23***	0.23***	0.17**	0.23***	0.19*	0.15
	(0.50)	(2.99)	(3.00)	(2.01)	(3.04)	(1.96)	(1.24)
Elasticity	*-0.43*	*-0.3*	*-0.3*	*-0.35*	*-0.3*	*-0.31*	*-0.37*
F-test (Ln WHW + Ln WHW*D1 =0)	30.39	8.96	9.68	15.82	9.55	10.22	14.54
P-value	0.000	0.006	0.005	0.005	0.005	0.004	0.001
Observations	72	72	72	72	72	72	72
Adjusted R2	0.18	0.27	0.27	0.27	0.27	0.28	0.29

Source: PNAD 1999.

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

Placebo: Short Run

Figure B.1 – Local Linear Regression for Labour Force Participation Rate
Non-white Males – 12 Months Bandwidth

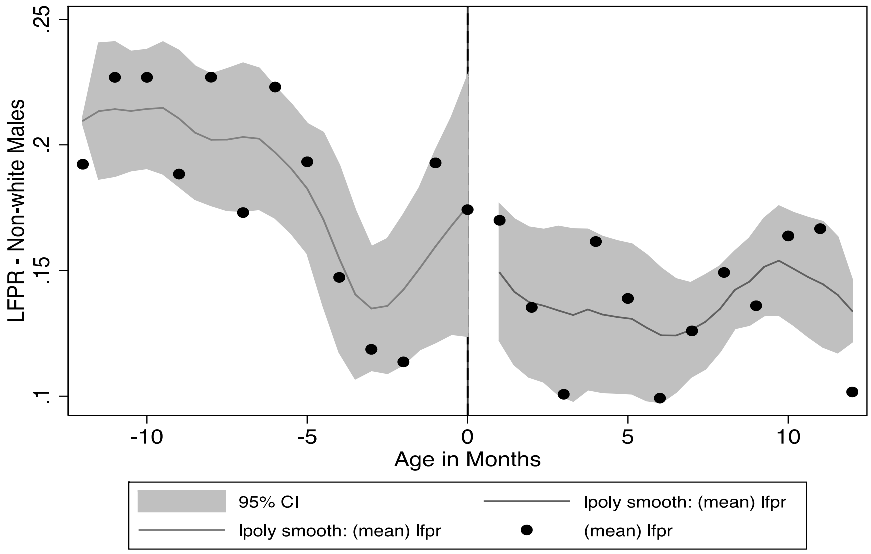
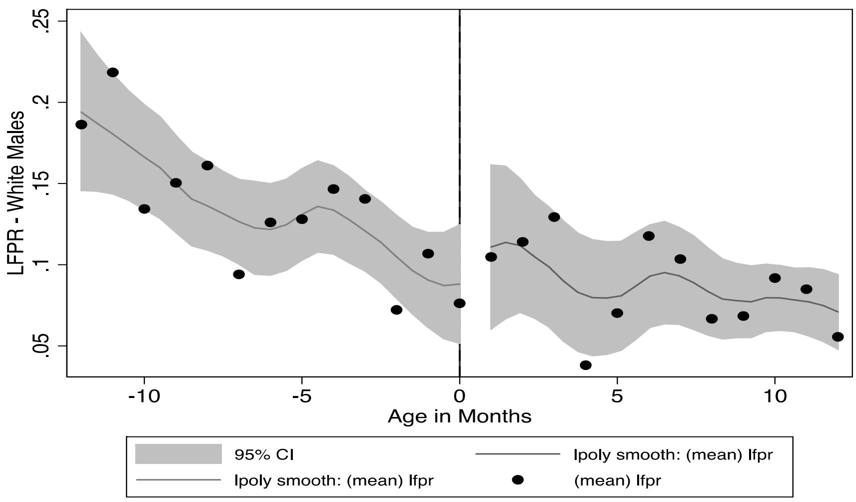


Figure B.2 – Local Linear Regression for Labour Force Participation Rate
White Males – 12 Months Bandwidth



Long Run

Figure B.3 – Local Linear Regression for Participation Rate in the Formal Labour Force – Long Run
White Males – 12 Months Bandwidth

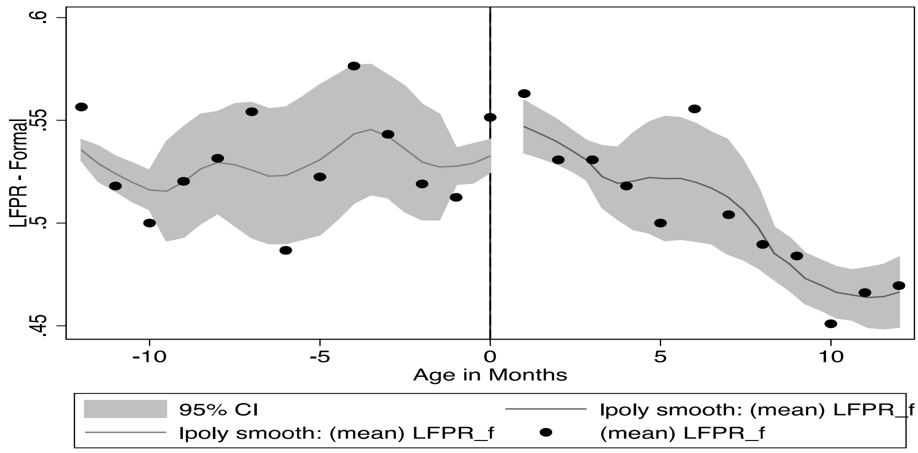
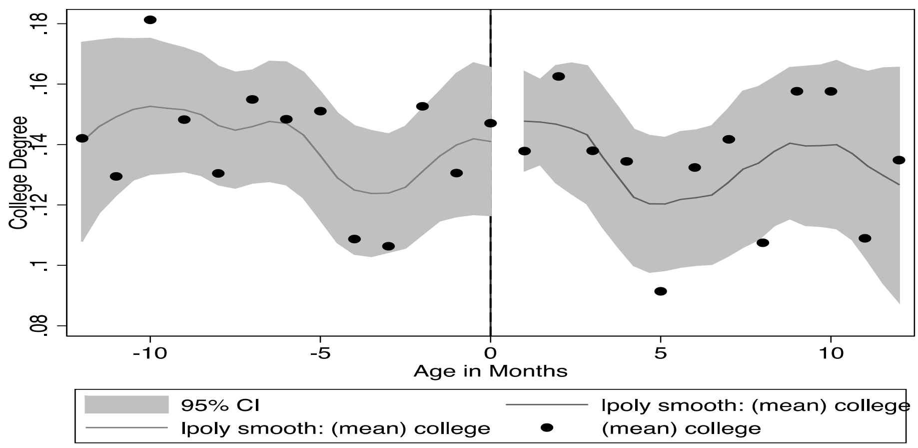


Figure B.4 – Local Linear Regression for Having College Degree – Long Run
Non-white Males – 12 Months Bandwidth



APPENDIX 3: Tables and Figures from Chapter 3

Table C.1 – Descriptive Statistics and Difference in Means
Younger siblings aged 10 to 13 with a brother aged 14 around December 1998
20 Weeks Bandwidth

	Siblings with older brother non-affected by the law (14 before Dec 1998)		Siblings with older brother affected by the law (14 after Dec 1998)			
	Mean	SE	Mean	SE	Difference	Clustered T-statistic
Outcomes						
Labour force participation rate	0.03	0.18	0.02	0.15	0.01	(0.67)
Domestic work	0.67	0.47	0.70	0.46	-0.03	(-0.64)
School attendance	0.97	0.18	0.98	0.15	-0.01	(-0.67)
Years of schooling	3.41	1.57	3.28	1.46	0.13	(0.97)
Covariates						
White	0.43	0.50	0.45	0.50	-0.02	(-0.37)
Male	0.48	0.50	0.51	0.50	-0.03	(-0.69)
Single Parent Families	0.45	0.50	0.45	0.50	0.00	(0.04)
Head's years of schooling	6.13	4.23	5.60	4.30	0.53	(1.44)
Head's age	41.67	5.50	41.38	6.46	0.29	(0.56)
Metropolitan region	0.58	0.49	0.67	0.47	-0.09**	(-2.23)
Household size	5.72	1.76	5.85	1.77	-0.13	(-0.84)
# of Siblings (0 to 13)	0.43	0.58	0.40	0.59	0.03	(0.52)
# of Siblings (15 to 21)	0.80	0.85	0.63	0.86	0.17**	(2.28)
Household Income (net of children's income)	476.78	593.68	545.41	700.71	-68.63	(-1.23)
Observations	*244*		*256*			

Source: PNAD 1999. *** Statistically significant at 1%.

Table C.2 – Descriptive Statistics and Difference in Means
Household head aged 30 to 60 with a son aged 14 around December 1998
20 Weeks Bandwidth

	Household head with a son non-affected by the law (14 before Dec 1998)		Household head with a son affected by the law (14 after Dec 1998)			
	Mean	SE	Mean	SE	Difference	Clustered T-statistic
Outcomes						
Labour force participation rate	0.82	0.39	0.81	0.39	0.01	(0.24)
Participation rate – formal labour force	0.61	0.49	0.51	0.50	0.09**	(2.39)
Participation rate – informal labour force	0.42	0.49	0.48	0.50	-0.06**	(-2.00)
Weekly hours worked	45.50	12.89	44.00	12.83	1.51	(1.62)
Covariates						
Age	42.88	6.28	42.73	6.66	0.16	(0.39)
White	0.51	0.50	0.51	0.50	-0.001	(-0.05)
Years of schooling	6.72	4.24	6.70	4.36	0.03	(0.11)
Metropolitan region	0.63	0.48	0.70	0.46	-0.06**	(-2.20)
Household size	4.88	1.52	4.94	1.54	-0.06	(-0.65)
Household Income (net of children's income)	675.38	896.03	672.75	887.57	2.63	(0.05)
Observations	*503*		*511*			

Source: PNAD 1999. *** Statistically significant at 1%.

Family Composition and Labour Force Status of Parents

Robustness Check

Table C.3 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Single Parent Households
20 Weeks Bandwidth
Work Outcomes

	All		Brothers		Sisters	
h(z)	LFPR	Domestic Work	LFPR	Domestic Work	LFPR	Domestic Work
Linear	0.012	0.12	0.068	0.25	-0.056	0.063
	(0.22)	(0.83)	(0.75)	(1.13)	(-1.00)	(0.39)
Quadratic	0.016	0.10	0.071	0.24	-0.051	0.042
	(0.31)	(0.71)	(0.79)	(1.09)	(-0.98)	(-1.06)
Spline Linear	0.016	0.11	0.070	0.24	-0.050	0.047
	(0.31)	(0.73)	(0.78)	(1.10)	(-1.00)	(0.29)
Controls?	*No*	*No*	*No*	*No*	*No*	*No*
Observations	*221*	*214*	*110*	*104*	*111*	*110*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table C.4 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Single Parent Households
20 Weeks Bandwidth
School Outcomes

	All		Brothers		Sisters	
h(z)	School Attendance	Years of Schooling	School Attendance	Years of Schooling	School Attendance	Years of Schooling
Linear	0.054	-0.28	0.059	0.30	0.056	-0.77
	(1.60)	(-0.59)	(1.31)	(0.45)	(1.00)	(-1.27)
Quadratic	0.055*	-0.27	0.063	0.31	0.051	-0.73
	(1.68)	(-0.56)	(1.44)	(0.47)	(0.98)	(-0.60)
Spline Linear	0.057*	-0.27	0.066	0.30	0.050	-0.73
	(1.76)	(-0.56)	(1.52)	(0.46)	(1.00)	(-1.22)
Controls?	*No*	*No*	*No*	*No*	*No*	*No*
Observations	*221*	*221*	*110*	*110*	*111*	*111*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table C.5 – Impact of the Ban on Labour Force Status of the Household Head – Single Parent Households
20 Weeks Bandwidth

	Mother		Father	
h(z)	LFPR	Hours Worked per Week	LFPR	Hours Worked per Week
Linear	-0.058	11.3**	-0.086	0.77
	(-0.40)	(2.36)	(-0.78)	(0.18)
Quadratic	-0.066	10.7**	-0.094	-0.48
	(-0.46)	(2.21)	(-0.81)	(-0.11)
Spline Linear	-0.068	9.98**	-0.090	-1.27
	(-0.47)	(2.06)	(-0.73)	(-0.30)
Controls?	*No*	*No*	*No*	*No*
Sigma		9.61***		9.45***
		(11.9)		(13.3)
Observations	*197*	*71*	*98*	*88*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. Given the proportion of parents not participating in the labour force, the coefficients on weekly hours worked refer to Tobit estimates.

Table C.6 – Impact of the Ban on Labour Force Status of Parents – Single Parent Households
20 weeks bandwidth – with controls

	Single Mothers		Single Fathers	
h(z)	Formal	Informal	Formal	Informal
Linear	0.032	-0.091	-0.54***	0.46***
	(0.24)	(-1.02)	(-3.54)	(3.00)
Quadratic	0.028	-0.095	-0.60***	0.50***
	(0.21)	(-1.06)	(-3.78)	(3.13)
Spline Linear	0.027	-0.095	-0.61***	0.52***
	(0.20)	(-1.06)	(-3.77)	(3.13)
Controls?	*No*	*No*	*No*	*No*
Observations	*197*	*197*	*98*	*98*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table C.7 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Couple Parent Households
20 Weeks Bandwidth
School Outcomes

	All		Brothers		Sisters	
h(z)	LFPR	Domestic Work	LFPR	Domestic Work	LFPR	Domestic Work
Linear	-0.052	-0.11	-0.10	-0.14	-0.0088	-0.050
	(-1.36)	(-1.01)	(-1.36)	(-0.81)	(-0.94)	(-0.33)
Quadratic	-0.052	-0.11	-0.10	-0.12	-0.0087	-0.063
	(-1.36)	(-1.03)	(-1.37)	(-0.74)	(-0.93)	(-0.41)
Spline Linear	-0.052	-0.12	-0.10	-0.13	-0.0093	-0.066
	(-1.36)	(-1.02)	(-1.39)	(-0.75)	(-0.95)	(-0.43)
Controls?	*No*	*No*	*No*	*No*	*No*	*No*
Observations	*279*	*276*	*140*	*137*	*139*	*139*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table C.8 – Impact of the Ban on Labour Force Status of the Household Head – Couple Parent Households

20 Weeks Bandwidth

	All		Brothers		Sisters	
h(z)	School Attendance	Years of Schooling	School Attendance	Years of Schooling	School Attendance	Years of Schooling
Linear	-0.038	-0.33	-0.053	0.073	-0.018	-0.70
	(-1.42)	(-0.87)	(-1.26)	(0.13)	(-0.68)	(-1.56)
Quadratic	-0.038	-0.33	-0.052	0.10	-0.018	-0.68
	(-1.42)	(-0.87)	(-1.29)	(0.19)	(-0.63)	(-1.53)
Spline Linear	-0.038	-0.33	-0.053	0.12	-0.017	-0.67
	(-1.42)	(-0.87)	(-1.32)	(0.21)	(-0.60)	(-1.51)
Controls?	*No*	*No*	*No*	*No*	*No*	*No*
Observations	*279*	*279*	*140*	*140*	*111*	*111*

Source: PNAD 1999.

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

Table C.9 – Parametric ITT Estimates of the Impact of the Ban on Parents' Labour Supply – Couple Parent Households
20 Weeks Bandwidth
Work Outcomes

	Mother		Father	
h(z)	LFPR	Hours Worked per Week	LFPR	Hours Worked per Week
Linear	0.16*	0.079	0.026	-5.54**
	(1.92)	(0.019)	(0.89)	(-2.58)
Quadratic	0.16*	-0.12	0.028	-5.40**
	(1.90)	(-0.029)	(0.98)	(-2.53)
Spline Linear	0.16*	-0.026	0.031	-5.32**
	(1.90)	(-0.0061)	(1.08)	(-2.49)
Controls?	*No*	*No*	*No*	*No*
Sigma		14.3***		11.5***
		(19.3)		(29.2)
Observations	*619*	*487*	*619*	*565*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table C.10 – Impact of the Ban on Labour Force Status of Parents – Couple Parent Households
20 weeks bandwidth – with controls

	Mother		Father	
h(z)	Formal	Informal	Formal	Informal
Linear	0.12	0.031	-0.058	0.090
	(1.55)	(0.56)	(-0.64)	(1.10)
Quadratic	0.13	0.025	-0.054	0.089
	(1.62)	(0.46)	(-0.60)	(1.09)
Spline Linear	0.13	0.024	-0.049	0.087
	(1.63)	(0.43)	(-0.55)	(1.07)
Controls?	*No*	*No*	*No*	*No*
Observations	*302*	*302*	*302*	*302*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Placebo Test

Table C.11 – Parametric ITT Estimates for the Impact of the Laws of 1998 on Extensive Margin of Labour Supply of Boys Aged 14
14 before December 1997 vs. 14 after December 1997
52 weeks Bandwidth

Polynomial degree	Participation Rate	Participation Rate Formal	Participation Rate Informal
Linear	-0.064***	-0.024***	-0.040***
	(-5.11)	(-4.14)	(-3.51)
Quadratic	0.017	-0.013	0.030
	(0.69)	(-1.12)	(1.38)
Cubic	0.018	-0.013	0.031
	(0.74)	(-1.11)	(1.44)
Spline Linear	0.032	-0.012	0.043
	(0.95)	(-0.74)	(1.44)
Spline Quadratic	0.018	-0.013	0.031
	(0.74)	(-1.11)	(1.43)
Controls?	*Yes*	*Yes*	*Yes*
Observations	*2148*	*2148*	*2148*

Source: PNAD 1998.
Note: Clustered T-statistics in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table C.12 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Placebo Test
52 weeks Bandwidth
Work Outcomes

	All		Brothers		Sisters	
h(z)	LFPR	Domestic Work	LFPR	Domestic Work	LFPR	Domestic Work
Linear	-0.0063	-0.060	-0.016	-0.046	0.0023	-0.089
	(-0.26)	(-1.07)	(-0.37)	(-0.56)	(0.11)	(-1.27)
Quadratic	-0.0073	-0.062	-0.017	-0.048	0.0023	-0.089
	(-0.30)	(-1.10)	(-0.40)	(-0.58)	(0.11)	(-1.28)
Cubic	-0.013	-0.074	-0.0095	-0.062	-0.012	-0.12
	(-0.41)	(-1.08)	(-0.17)	(-0.60)	(-0.54)	(-1.37)
Spline Linear	-0.011	-0.067	-0.022	-0.057	0.0022	-0.083
	(-0.45)	(-1.19)	(-0.53)	(-0.69)	(0.10)	(-1.20)
Spline Quadratic	-0.045	-0.048	-0.059	-0.049	-0.024	-0.028
	(-1.27)	(-0.68)	(-0.98)	(-0.47)	(-0.78)	(-0.29)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*1234*	*1200*	*631*	*605*	*603*	*595*

Source: PNAD 1998.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table C.13 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Placebo Test
52 weeks Bandwidth
School Outcomes

	All		Brothers		Sisters	
h(z)	School Attendance	Years of Schooling	School Attendance	Years of Schooling	School Attendance	Years of Schooling
Linear	0.016	0.034	0.031	-0.089	0.0026	0.11
	(0.83)	(0.22)	(1.20)	(-0.42)	(0.091)	(0.51)
Quadratic	0.015	0.039	0.031	-0.083	-0.00045	0.11
	(0.74)	(0.25)	(1.17)	(-0.39)	(-0.015)	(0.51)
Cubic	0.013	0.034	0.023	-0.16	0.0022	0.15
	(0.53)	(0.18)	(0.77)	(-0.60)	(0.058)	(0.58)
Spline Linear	0.014	0.051	0.029	-0.062	-0.00051	0.12
	(0.69)	(0.33)	(1.10)	(-0.29)	(-0.017)	(0.55)
Spline Quadratic	0.012	0.051	0.032	-0.11	-0.0012	0.18
	(0.47)	(0.25)	(0.98)	(-0.37)	(-0.031)	(0.66)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*1234*	*1234*	*631*	*631*	*603*	*603*

Source: PNAD 1998.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table C.14 – Parametric ITT Estimates of the Impact of the Ban on Household Head's Labour Supply – Single Parent Households
52 weeks bandwidth – with controls

	Female Head		Male Head	
h(z)	LFPR	Hours Worked per Week	LFPR	Hours Worked per Week
Linear	0.22***	-2.40	-0.0052	-0.56
	(2.74)	(-0.61)	(-0.090)	(-0.18)
Quadratic	0.20**	-3.62	-0.0081	-0.55
	(2.48)	(-0.90)	(-0.14)	(-0.18)
Cubic	0.28**	-6.47	0.029	-3.66
	(2.51)	(-1.21)	(0.39)	(-0.82)
Spline Linear	0.20**	-3.49	-0.0081	-0.46
	(2.50)	(-0.87)	(-0.14)	(-0.15)
Spline Quadratic	0.32**	-7.05	0.052	-3.91
	(2.55)	(-1.18)	(0.65)	(-0.78)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*
Sigma		15.8***		12.6***
		(18.8)		(17.0)
Observations	*593*	*282*	*349*	*286*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. Given the proportion of parents not participating in the labour force, the coefficients on weekly hours worked refer to Tobit estimates.

Table C.15 – Impact of the Ban on Labour Force Status of Parents – Single Parent Households
52 weeks bandwidth – with controls

	Female Head		Male Head	
h(z)	Formal	Informal	Formal	Informal
Linear	0.21**	-0.019	-0.029	-0.0035
	(2.36)	(-0.33)	(-0.22)	(-0.029)
Quadratic	0.20**	-0.016	-0.040	0.00039
	(2.28)	(-0.29)	(-0.30)	(0.0033)
Cubic	0.28**	-0.045	0.051	-0.024
	(2.28)	(-0.53)	(0.29)	(-0.15)
Spline Linear	0.20**	-0.017	-0.042	0.0029
	(2.30)	(-0.29)	(-0.32)	(0.024)
Spline Quadratic	0.31**	-0.037	0.071	0.0025
	(2.24)	(-0.38)	(0.36)	(0.014)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*437*	*437*	*194*	*194*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The regressions include a dummy for metropolitan region, a dummy for skin colour (white), and years of schooling.

Table C.16 – Placebo Regressions for Labour Force Participation of Single Mothers
52 weeks bandwidth

	White Single Mothers	Non-white single mothers	Single Mothers
Linear	0.093	0.37***	0.11
	(0.87)	(3.22)	(1.14)
Quadratic	0.089	0.35***	0.089
	(0.83)	(2.97)	(0.92)
Spline linear	0.089	0.36***	0.091
	(0.84)	(3.03)	(0.94)
Controls?	*Yes*	*Yes*	*Yes*
Observations	360	275	576

Source: PNAD 1998.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The regressions include a dummy for metropolitan region, a dummy for skin colour (white), and years of schooling. The regression for single mothers in the third column excludes observations in the (-6, 6) interval.

Table C.17 – Parametric ITT Estimates of the Impact of the Ban on Parents' Labour Supply – Couple Parent Households
52 weeks bandwidth – with controls

	Mother		Father	
h(z)	LFPR	Hours Worked per Week	LFPR	Hours Worked per Week
Linear	0.025	0.024	-0.013	1.16
	(0.43)	(0.0084)	(-0.50)	(0.72)
Quadratic	0.025	0.014	-0.013	1.15
	(0.43)	(0.0048)	(-0.49)	(0.72)
Cubic	0.074	4.20	0.0089	0.22
	(0.93)	(0.98)	(0.25)	(0.100)
Spline Linear	0.025	0.011	-0.013	1.14
	(0.43)	(0.0039)	(-0.50)	(0.71)
Spline Quadratic	0.098	5.73	0.021	-0.99
	(1.10)	(1.14)	(0.52)	(-0.40)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*
Sigma		15.7***		12.5***
		(22.9)		(29.0)
Observations	1166	*453*	*1166*	*1669*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. Given the proportion of parents not participating in the labour force, the coefficients on weekly hours worked refer to Tobit estimates.

Table C.18 – Impact of the Ban on Labour Force Status of Parents – Couple Parent Households

52 weeks bandwidth – with controls

	Female Head		Male Head	
h(z)	Formal Occupation	Informal Occupation	Formal Occupation	Informal Occupation
Linear	0.059	-0.047	0.033	-0.061
	(1.07)	(-1.53)	(0.52)	(-1.13)
Quadratic	0.060	-0.047	0.033	-0.061
	(1.08)	(-1.53)	(0.52)	(-1.13)
Cubic	0.054	0.0013	0.059	-0.056
	(0.73)	(0.038)	(0.68)	(-0.77)
Spline Linear	0.060	-0.047	0.033	-0.061
	(1.09)	(-1.54)	(0.52)	(-1.13)
Spline Quadratic	0.055	0.015	0.086	-0.063
	(0.66)	(0.40)	(0.88)	(-0.78)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*892*	*892*	*778*	*778*

Source: PNAD 1999.

Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The regressions include a dummy for metropolitan region, a dummy for skin colour (white), and years of schooling.

Table C.19 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Single Parent Households
52 weeks bandwidth – with controls
Work Outcomes

	All		Brothers		Sisters	
h(z)	LFPR	Domestic Work	LFPR	Domestic Work	LFPR	Domestic Work
Linear	-0.029	-0.013	-0.024	0.023	-0.034	-0.066
	(-0.78)	(-0.16)	(-0.35)	(0.19)	(-1.21)	(-0.60)
Quadratic	-0.030	-0.012	-0.026	0.017	-0.033	-0.057
	(-0.79)	(-0.14)	(-0.38)	(0.14)	(-1.17)	(-0.54)
Cubic	-0.035	-0.090	-0.022	0.017	-0.042	-0.16
	(-0.75)	(-0.84)	(-0.26)	(0.11)	(-1.20)	(-1.22)
Spline Linear	-0.034	-0.012	-0.037	-0.0071	-0.032	-0.041
	(-0.86)	(-0.14)	(-0.51)	(-0.057)	(-1.07)	(-0.38)
Spline Quadratic	-0.078	-0.073	-0.089	0.011	-0.064	-0.10
	(-1.42)	(-0.66)	(-0.92)	(0.076)	(-1.25)	(-0.69)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*504*	*489*	*249*	*235*	*255*	*254*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The vector of controls include a dummy for skin colour (1 if white), years of schooling of the household head, number of children aged 0 to 13, and a dummy for metropolitan region.

Table C.20 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Single Parent Households
52 weeks bandwidth – with controls
School Outcomes

	All		Brothers		Sisters	
h(z)	School Attendance	Years of Schooling	School Attendance	Years of Schooling	School Attendance	Years of Schooling
Linear	0.0033	-0.029	0.055	-0.028	-0.035	-0.097
	(0.087)	(-0.12)	(1.45)	(-0.082)	(-0.57)	(-0.29)
Quadratic	0.0015	-0.012	0.053	-0.0031	-0.037	-0.077
	(0.039)	(-0.047)	(1.43)	(-0.0089)	(-0.60)	(-0.23)
Cubic	-0.015	-0.039	-0.0016	-0.13	-0.030	0.021
	(-0.30)	(-0.13)	(-0.045)	(-0.30)	(-0.34)	(0.052)
Spline Linear	0.0011	0.044	0.051	0.068	-0.033	-0.034
	(0.031)	(0.18)	(1.36)	(0.19)	(-0.57)	(-0.10)
Spline Quadratic	-0.017	-0.0048	0.024	0.17	-0.061	-0.11
	(-0.43)	(-0.015)	(0.58)	(0.34)	(-1.18)	(-0.27)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*504*	*504*	*249*	*249*	*255*	*255*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The vector of controls include a dummy for skin colour (1 if white), years of schooling of the household head, number of children aged 0 to 13, and a dummy for metropolitan region.

Table C.21 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Couple Parent Households
52 weeks bandwidth – with controls
Work Outcomes

	All		Brothers		Sisters	
h(z)	LFPR	Domestic Work	LFPR	Domestic Work	LFPR	Domestic Work
Linear	0.015	-0.092	-0.0010	-0.095	0.031	-0.087
	(0.51)	(-1.29)	(-0.021)	(-0.86)	(1.07)	(-1.05)
Quadratic	0.013	-0.10	-0.0020	-0.098	0.029	-0.11
	(0.44)	(-1.43)	(-0.041)	(-0.88)	(1.03)	(-1.31)
Cubic	0.0097	-0.087	0.016	-0.15	0.0086	-0.085
	(0.26)	(-1.03)	(0.22)	(-1.02)	(0.31)	(-0.86)
Spline Linear	0.010	-0.11	-0.0052	-0.10	0.030	-0.12
	(0.33)	(-1.57)	(-0.11)	(-0.91)	(1.02)	(-1.38)
Spline Quadratic	-0.014	-0.065	-0.012	-0.13	0.0035	0.026
	(-0.33)	(-0.69)	(-0.16)	(-0.83)	(0.11)	(0.23)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*		*Yes*
Observations	*730*	*711*	*382*	*370*	*348*	*341*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The vector of controls include a dummy for skin colour (1 if white), years of schooling of the household head, number of children aged 0 to 13, and a dummy for metropolitan region.

Table C.22 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Couple Parent Households
52 weeks bandwidth – with controls
School Outcomes

	All		Brothers		Sisters	
h(z)	School Attendance	Years of Schooling	School Attendance	Years of Schooling	School Attendance	Years of Schooling
Linear	0.027	0.097	0.0058	-0.086	0.046**	0.22
	(1.26)	(0.49)	(0.16)	(-0.32)	(2.10)	(0.83)
Quadratic	0.025	0.085	0.0053	-0.083	0.044*	0.18
	(1.14)	(0.43)	(0.14)	(-0.31)	(1.95)	(0.67)
Cubic	0.030	0.10	0.029	-0.12	0.022	0.19
	(1.17)	(0.41)	(0.63)	(-0.33)	(0.89)	(0.61)
Spline Linear	0.024	0.073	0.0047	-0.079	0.044*	0.17
	(1.01)	(0.36)	(0.13)	(-0.29)	(1.77)	(0.60)
Spline Quadratic	0.036	0.12	0.029	-0.23	0.031	0.43
	(1.03)	(0.42)	(0.57)	(-0.61)	(0.67)	(1.13)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*730*	*730*	*382*	*382*	*341*	*341*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The vector of controls include a dummy for skin colour (1 if white), years of schooling of the household head, number of children aged 0 to 13, and a dummy for metropolitan region.

Testing for Potential Effect of Age at School Entry

Table C.23 – Parametric ITT Estimates for the Impact of the Laws of 1998 on Extensive Margin of Labour Supply of Boys Aged 14
14 before June 30th 1999 vs. 14 after June 30th 1999
52 weeks Bandwidth

Polynomial degree	Participation Rate	Participation Rate Formal	Participation Rate Informal
Linear	-0.030***	-0.0023	-0.027***
	(-2.73)	(-0.86)	(-2.59)
Quadratic	0.027	0.0030	0.024
	(1.36)	(0.94)	(1.22)
Cubic	0.027	0.0030	0.024
	(1.37)	(0.94)	(1.23)
Spline Linear	0.030	0.00092	0.029
	(1.15)	(0.48)	(1.12)
Spline Quadratic	0.027	0.0030	0.024
	(1.36)	(0.94)	(1.22)
Controls?	*Yes*	*Yes*	*Yes*
Observations	*1821*	*1821*	*1821*

Source: PNAD 1999.
Note: Clustered T-statistics in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively.

Table C.24 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Placebo Test
52 weeks bandwidth
Work Outcomes

	All		Brothers		Sisters	
h(z)	LFPR	Domestic Work	LFPR	Domestic Work	LFPR	Domestic Work
Linear	-0.0085	0.030	-0.032	0.10	0.0034	-0.021
	(-1.01)	(0.45)	(-1.40)	(0.96)	(0.94)	(-0.26)
Quadratic	-0.0083	0.031	-0.031	0.10	0.0035	-0.018
	(-0.98)	(0.48)	(-1.38)	(0.99)	(0.96)	(-0.24)
Cubic	-0.0062	-0.045	-0.014	0.0081	-0.0029	-0.052
	(-0.66)	(-0.57)	(-0.51)	(0.061)	(-0.95)	(-0.56)
Spline Linear	-0.0083	0.036	-0.032	0.11	0.0036	-0.010
	(-0.97)	(0.55)	(-1.39)	(1.01)	(0.97)	(-0.13)
Spline Quadratic	-0.0072	0.0035	-0.020	0.086	0.00068	-0.023
	(-0.97)	(0.051)	(-0.98)	(0.76)	(0.33)	(-0.25)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*836*	*739*	*390*	*346*	*446*	*393*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The vector of controls include a dummy for skin colour (1 if white), years of schooling of the household head, number of children aged 0 to 13, and a dummy for metropolitan region.

Table C.25 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Placebo Test
52 weeks bandwidth
School Outcomes

	All		Brothers		Sisters	
h(z)	School Attendance	Years of Schooling	School Attendance	Years of Schooling	School Attendance	Years of Schooling
Linear	0.020	0.017	0.016	-0.025	0.021	-0.020
	(1.10)	(0.11)	(0.54)	(-0.096)	(1.14)	(-0.10)
Quadratic	0.021	0.021	0.018	-0.010	0.021	-0.019
	(1.12)	(0.13)	(0.59)	(-0.039)	(1.15)	(-0.094)
Cubic	0.020	0.00011	0.011	-0.26	0.027	0.12
	(1.01)	(0.00056)	(0.32)	(-0.78)	(1.32)	(0.52)
Spline Linear	0.021	0.019	0.018	-0.015	0.022	-0.021
	(1.13)	(0.12)	(0.58)	(-0.055)	(1.15)	(-0.10)
Spline Quadratic	0.021	-0.022	0.018	-0.17	0.022	-0.0091
	(1.21)	(-0.14)	(0.58)	(-0.66)	(1.24)	(-0.045)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	*836*	*836*	*390*	*390*	*446*	*446*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The vector of controls include a dummy for skin colour (1 if white), years of schooling of the household head, number of children aged 0 to 13, and a dummy for metropolitan region.

Table C.26 – Parametric ITT Estimates of the Impact of the Ban on Household Head's Labour Supply – Placebo Test
52 weeks bandwidth

	Household Head		Female Head		Male Head	
h(z)	LFPR	Hours Worked per Week	LFPR	Hours Worked per Week	LFPR	Hours Worked per Week
Linear	0.057	0.14	0.032	1.54	0.015	-0.57
	(1.30)	(0.13)	(0.51)	(0.73)	(0.46)	(-0.46)
Quadratic	0.056	0.15	0.032	1.58	0.016	-0.58
	(1.28)	(0.13)	(0.50)	(0.74)	(0.47)	(-0.47)
Cubic	0.064	-0.75	0.029	1.00	0.038	-1.63
	(1.11)	(-0.56)	(0.37)	(0.39)	(0.90)	(-1.05)
Spline Linear	0.056	0.083	0.034	1.53	0.020	-0.64
	(1.28)	(0.076)	(0.53)	(0.72)	(0.59)	(-0.51)
Spline Quadratic	0.074	-0.79	0.088	2.65	0.048	-2.04
	(1.14)	(-0.58)	(1.01)	(0.97)	(1.14)	(-1.33)
Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Sigma		10.2***		9.90***		9.89***
		(27.5)		(18.4)		(22.5)
Observations	*1786*	*1204*	*832*	*287*	*1021*	*917*

Source: PNAD 1999.
Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. Given the proportion of mothers not participating in the labour force, the coefficients on weekly hours worked refer to Tobit estimates. The regressions include a dummy for metropolitan region, a dummy for skin colour (white), and years of schooling.

Table C.27 – ITT Estimates for Occupation of Single Fathers

	Science & Arts	Administrative Services	Agricultural Sector	Processing industry	Commerce and related	Transport and communication	Provision of Services	Undefined
Male head (ITT)	0.017	-0.058*	0.010	0.039	-0.014	-0.0012	0.030*	-0.022
	(0.84)	(-1.89)	(0.70)	(0.83)	(-0.39)	(-0.038)	(1.90)	(-0.60)
Mean of Monthly Wage	1309.734	907.3318	293.1663	414.5252	528.7923	615.3671	302.4568	370.3963
Observations	1455	1455	1455	1455	1455	1455	1455	1455

Source: PNAD 1999.

Note: Clustered T-statistic in parentheses. ***, **, * Statistically significant at 1%, 5%, and 10% respectively. The estimates are for spline linear specification.

Figure C.1 – Local Linear Regressions for LFPR of Boys in 1998
Cutoff = December 1997

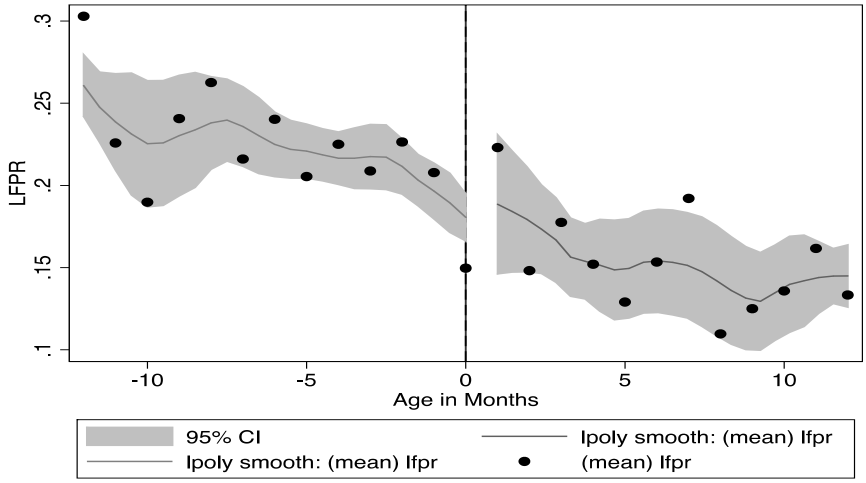


Figure C.2 – Linear Regressions for LFPR of Younger Siblings in Couple Parent Households

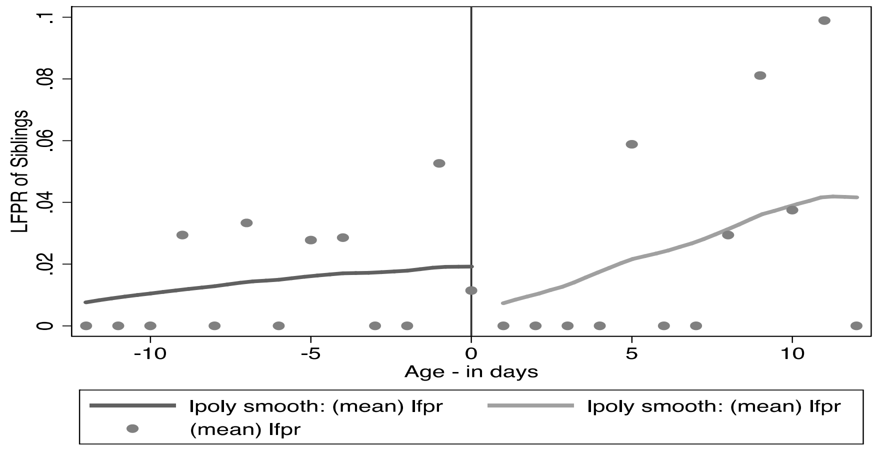


Figure C.3 – Linear Regressions for School Attendance of Siblings in 1999

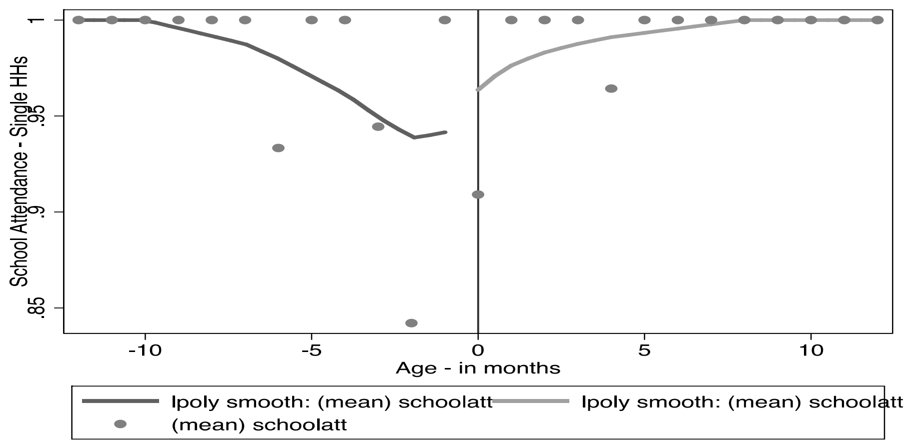


Figure C.4 – Local Polynomial Regressions for LFPR of Single Mothers in 1998

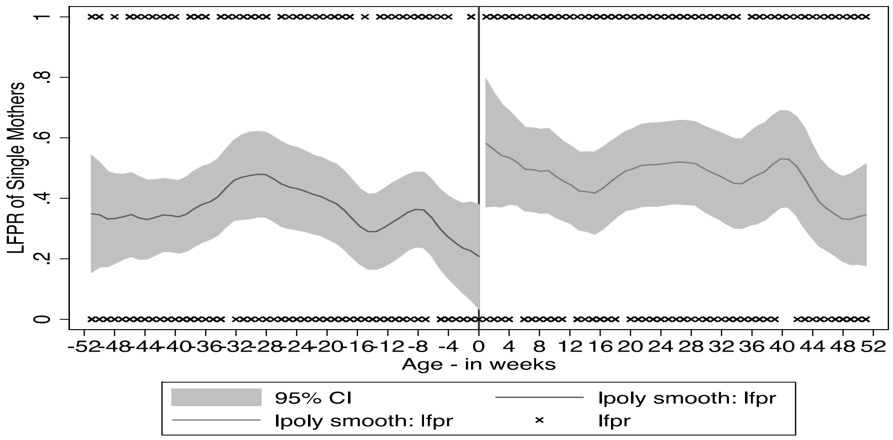


Figure C.5 – Local Polynomial Regressions for LFPR of Single Mothers in 1998
Excludes observations in the (-4, 4) interval.

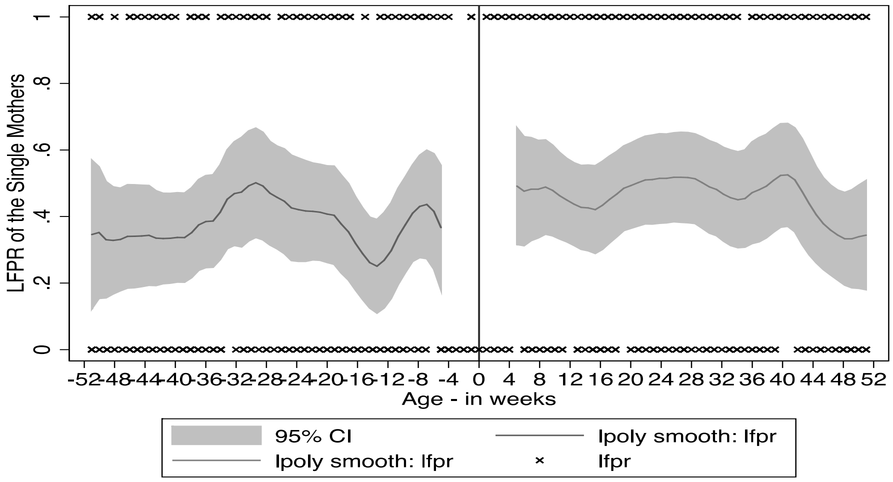


Figure C.6 – Local Polynomial Regressions for LFPR of Single Mothers in 1998
Excludes observations in the (-6, 6) interval.

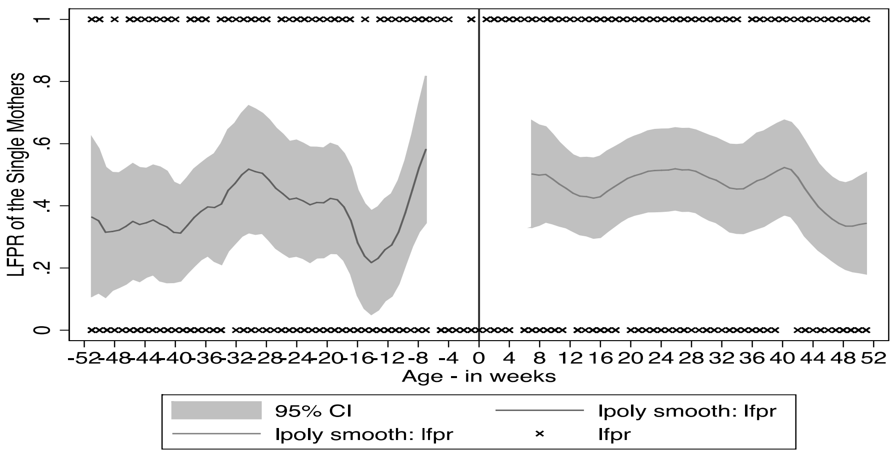


Figure C.7 –Monthly Wage Earned by Single Fathers in the Formal and Informal Sectors