INTRAHOUSEHOLD CAUSAL EFFECTS OF A CHILD LABOUR BAN

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Abstract

This paper contributes to the scant literature on the consequences of child labour by investigating the impact of a child labour ban on several outcomes of other household members. We use the increase in the minimum legal age of entry into the labour force that came into effect in December 1998 in Brazil to investigate the impact of banning child work at age 14 on the work and schooling outcomes of younger siblings and the labour supply of parents. The findings advance the understanding of the consequences of child labour ban by investigating whether the effects differ according to family composition and occupation of parents in the labour force. We use regression discontinuity design to estimate local intent-to-treat effects. The main findings suggest that the impact of a ban on younger siblings is minor when compared to the impact on parents. The ban affected parents' labour supply, particularly in couple parent households. Results also support the hypothesis that single parents might face binding liquidity constraints and that mothers' labour supply in couple parent families can be used as an imperfect insurance mechanism. To check robustness we test different specifications of the smooth function and use two bandwidth sizes. In addition, we conduct two placebo tests.

JEL: D13, J12, J13, J16.

Keywords: Child labour ban, intra-household allocation, treatment effects, spillover effects

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INTRODUCTION

The literature on the effects 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 does not necessarily result in optimal allocations of scarce resources among household members.[4] The empirical modeling of intra-household time allocation, for instance, faces methodological challenges that are difficult to overcome, such as the simultaneity problem embedded in collective household models (Vermeulen, 2002; Strauss et al. 2000).[5] As discussed by Strauss et al. (2000), an additional challenge in this literature involves unpacking the mechanisms underlying the reallocation of resources caused by an exogenous intervention. In many situations, having a solid identification strategy might be insufficient for revealing the causal chain triggered by an intervention. This argument is elaborated furthered by Imai et al. (2011), who show the necessary conditions for identifying causal mechanisms in experimental and observational studies.[6]

This chapter digs into the causal consequences of the law of 1998, taking into account different 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.

[4] This chapter does not embrace or test any particular household model – such as unitary vs. collective models. In fact, most of the literature that has looked at intra-household allocation through the lens of a particular household model uses structural models (Blundell et al. 2005; Blundell et al. 2007; Apps and Rees, 1997; Couprie, 2007) and most of these studies ignore the presence of children in the household and issues related to child labour. For an introduction to collective models of the household, see Donni (2008).

[5] This point is also made by Edmonds (2008) in the context of child labour literature.

[6] According to the authors, to unpack the causal chain it is necessary that, conditioned on treatment status and other observed characteristics, the mediating variables (or intermediary outcomes) are independent of the final outcome variables. If that condition does not hold, the analysis of mediating factors will not have causal interpretation. They argue that many randomised controlled trials may not satisfy this condition.

This chapter adds to the literature in various ways. First, most of the papers investigating the 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, this chapter explores the complementary literature of sibling rivalry. This literature usually asks what would be the completed years of schooling of younger siblings if their older siblings were all girls (Morduch, 2000 and Dammert, 2010). In this chapter we approach that issue from a different perspective. We try to understand parents' preferences for the activities of boys and girls by looking at time allocation and other schooling outcomes of sons and daughters when resources are scarce and parents do not necessarily count on optimal risk-coping mechanisms.[7]

Third, this chapter also differs from others with regard to the method used. 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.[8]

We are only aware of two other papers that provide estimates for 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 child labour force participation rate 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 participation rate into the labour force, 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

[7] Sibling rivalry has been investigated by different authors and in different contexts. For instance, Morduch (2000) provides evidence for Ghana, Garg and Morduch (1998) provide evidence for South Africa and Tanzania, Akresh et al. (2012) provide evidence for Burkina Faso, Bommier and Lambert (2006) for Brazil, and Dammert (2010) for Guatemala and Nicaragua.

[8] Manacorda (2006) had to disentangle the *within* and *between* household effects of the child labour ban in the US.

of children eligible to work had positive effects on siblings (lower participation rate and higher school attendance) but no impact on parents.[9]

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

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 the best of our knowledge, this is the first paper 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.[11] 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 paper 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 became more likely to participate in the formal labour force, fathers worked fewer hours per week, and younger siblings became less likely to work. These results support the

[9] Although Manacorda's (2006) results are very 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 decided to adopt stricter rules and/or move more quickly in adopting the law, then the law would be an invalid instrument, because 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 exclusion restriction. For more on this point, see Lee and Lemieux (2009).

[10] 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.

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

hypothesis that mothers' labour supply in couple parent families can be used as an imperfect insurance mechanism.

For single parent households, it is found that single fathers shift from the formal to the informal sector, whereas younger brothers became more likely to attend school. We 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. Since brothers banned from the formal labour force did not shift to the informal sector, it might be that they ended up helping with household chores so that the younger brother could go to school.[12] The robustness check and placebo tests confirm most of these findings. Beyond this introduction, the paper is organised as follows. Section two describes the law of 1998 and explains to what extent it can be interpreted as a natural experiment. The identification strategy is discussed in section three, whereas section four describes the data and presents the descriptive statistics. Study results are discussed in sections five and six. Section seven shows the robustness check whereas section 8 discusses the results of two placebo tests. The conclusion summarises the main findings and outlines some policy implications.

2 THE INTERVENTION: THE LAW OF DECEMBER 1998

The Brazilian Constitution of 1988 set the minimum legal age of entry to the labour market at 14, and in 1990 a federal rule named 'The Statute of Children and Adolescents'[13] established children's and youth rights beyond regulating the conditions of entry to the formal labour market. Complementary to the Constitution of 1988, the statute is considered the legal framework for children and youth in the labour market.[14] 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 in December 12th 1998, the Constitutional Amendment No. 20 also increased the minimum legal age for entry to labour market from 14 to 16[15]. Individuals under 14 could work only as apprentices, whereas individuals younger than 18 were prohibited from hazardous and night work.

[12] Chapter one found some weak indication that boys banned from the labour force became slightly more likely to do household chores.
[13] *Lei do Estatuto e do Adolescente*, Law No.8069 from 07/13/1990. Complementary to the Constitution of 1988, the statute is considered the legal framework for children and youth in the labour market.
[14] Although ILO considers as child an individual 17 years old or younger, in this paper terms 'children', 'teenagers' and 'youth' are used interchangeably.
[15] The law passed on December 15th and was made effective in the following day.

The law makes reference to apprenticeship status at the labour force despite the fact that the programme was institutionalised only in December 2000. Actually, this helps explain why the number of apprentices was so low before that year.[16] This ambiguity in the law seems to have generated some discussion in the Brazilian courts. The law is unclear about whether those who turned 14 before the law passed but were not participating into the labour force could still do so or not[17].

Thus, the ban affected those who turned 14 years old in the second half of December 1998, that is, the law was a binding constraint only for a subgroup of children who turned age 14 after December 15th 1998 and would participate in the formal labour force had the Amendment not been passed.

With the change in the law the Ministry of Labour stopped issuing work permits for individuals who turned age 14 after the law passed. Consequently, the law divided similar children into two groups: one affected (eligible group) and one unaffected by the law (control). Note that children affected by the ban who shifted to the informal sector automatically entered the child labour statistics whereas those with similar age (and plausibly other characteristics) but unaffected by the law did not.

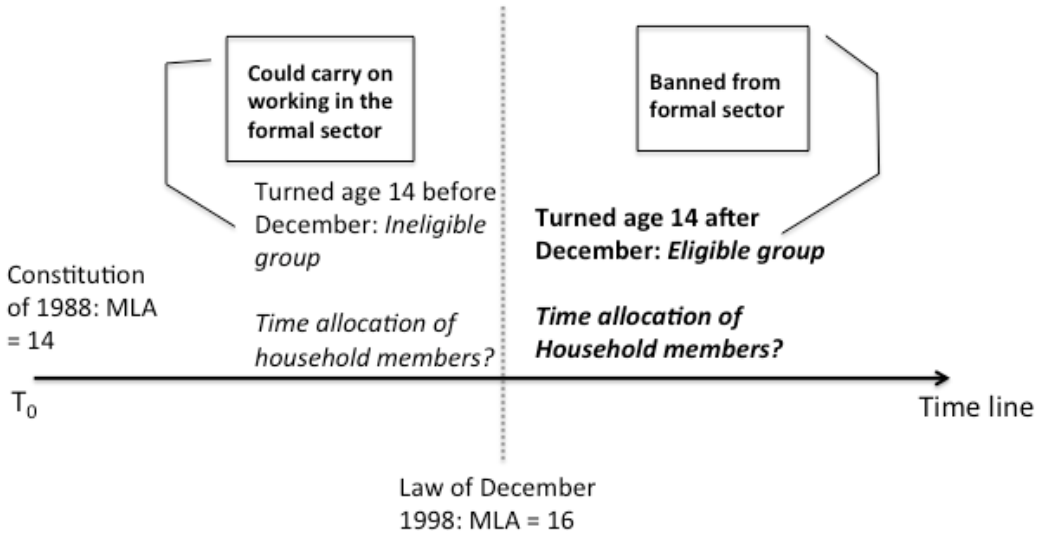
The large informal sector in Brazil can cast doubt on the effectiveness of such type of law. However, the effect of this intervention on participation rate of the eligible group depends on its enforceability but also on the size of problem it is trying to fix. The small participation rate in the formal labour force among teenagers under age 16 and the large informal sector in Brazil may suggest that the law would have had limited impact on children's participation rate. If the law were fully enforced in the formal sector, the effect on participation rate would had been small since only 1-2 percent of children in that age group were actually participating in the formal labour force. If some of children participating in the formal sector simply shifted to informality after the ban, the effect of the law on children's participation rate would have been negligible or even positive. But, if some employers decided no longer to employ children under age 16 to avoid legal consequences – such as paying fines –, the law would probably reduce participation rate in the informal sector as well.

[16] 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.

[17] We consulted with few Labour Lawyers in Brazil and got different views on this regard.

The figure below illustrates the change in the minimum legal age (MLA) and how it split individuals turning 14 just before and just after its enactment. The figure shows the groups of households that ended up in the 'treatment' and comparison groups.

Figure 1 – The Change in the Minimum Employment Age



3 THEORETICAL BACKGROUND

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 be used as a risk-coping mechanism to deal with such shocks. It is thus also plausible to expect different effects of the ban on younger siblings in single and couple parent households.[18] One could 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, we expect most of the impact of the ban to be absorbed by the parents, particularly mothers, since about 93 percent of the household heads in couple parent

[18] 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.

households are male (see Table C.3) and the participation rate of women is relatively low (43.6 percent). Therefore, we 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 would 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. It is also possible that some parents believe that male teenagers may have some comparative advantage in performing low-skilled paid work.

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 children's time to the informal labour force.

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.[19] Since the Brazilian PNAD 1999 does not have information on time spent on household chores and older siblings are not covered in the empirical analysis, we expect most of the cost will accrue to the household head, particularly in cases in which they can afford to consume children's leisure. Therefore, for single parent families we expect to see more impact on the intensive margin of labour supply and some increase in participation rate in the informal sector in case there are liquidity constraints.

[19] Using data from Nepal, Edmonds (2006) shows that having younger siblings increases older sisters' hours worked in 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.

4 IDENTIFICAITON STRATEGY

This paper 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 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 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.[20]

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 cutoff 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.[21]

Although the RDD only identifies the local average treatment effect—the treatment effect for the individuals close to the cutoff—Hahn et al. (2001), van der Klaauw (2008), Imbens and Lemeiux (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.[22]

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

[20] 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.

[21] In the fuzzy design there is an 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.

[22] 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.

impact of the law on household members is estimated on the following outcome variables of siblings: (i) the labour force participation rate as a whole (LFPR), (ii) household chores, (iii) school attendance, and (iv) completed years of schooling.[23] For parents we look at: (a) LFPR, (b) LFPR disaggregated between formal and informal sectors, and (c) 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:

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

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

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 $h\left(Z_{ij}\right)$ 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 using the effect of the actual participation rate of 14-year-old children on other household members would very likely result in a 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

[23] Completed years of schooling of siblings is an outcome extensively used in the literature of sibling rivalry. See Morduch (2000).

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 LATE of the law on outcomes of individual *k* of household *j* could be obtained dividing the reduced-form estimate β_1 by the participation rate of individual *i* predicted in Eq. (2), δ_1. As mentioned above, we 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. We look at different family compositions to try to better understand the intra-household decision-making response to the law of 1998 that prevented 14-year-old boys from participating in the labour force. We also try to shed light on potential liquidity constraint by exploring the labour force status of parents.

Because we will split the sample according to family composition, we use a larger bandwidth of 51 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.

Tables C.1 and C.2 present the number of individuals in the sample. The sample used in this chapter focuses on the groups of individuals in households with one or two parents present. Table C.2 splits the sample into single and couple parent families and identifies the gender of

the head. Of about 22,000 single parents, 38 percent are male and 62 percent are female. On the other hand, most of the heads in couple parent families are male (88 percent). The sample used in the empirical exercises starts with 2,420 households, 47 percent of which are single headed families with two-thirds of them headed by women.[24]

Tables 1a to 1d 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 more likely to change if households have access to better infrastructure, such as schools, and if there is a more active labour market.[25]

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.[26] 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 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 the school entry age in Brazil was mandatory for children turning 6 by 30 June of current year.

Table 1a shows the samples of younger siblings with a bandwidth of 20 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 detect almost no difference in the outcomes. From this simple test, there is an indication that the law did not affect younger siblings. Table 1b shows the statistics for 51 weeks bandwidth. With the larger bandwidth the null hypothesis of equal means is rejected in three cases, but overall the samples seem balanced even with the relatively large bandwidth.

[24] It is important to mention that the PNAD of 1999 does not identify married couples. We defined couples if the head and spouse live in the same household. However, couples in stable relationships that do not share the same household will be 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.

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

[26] 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 1c shows descriptive statistics and difference in means for household heads using a 20 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. Table 1d redoes the analysis with 51 weeks bandwidth. Though the t-test does not indicate an effect on participation rate of the head, it still indicates that the balance condition is satisfied.

It is important to mention that WEobserve 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 51 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 has to assume that the groups are also balanced in terms of unobserved characteristics. This assumption 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 efficiency.

Figures 1 to 7 illustrate, visually, the main results of the chapter. The figures report linear regressions with a confidence interval on each side of the cutoff point with a 51 weeks bandwidth. Figure 1 illustrates an estimate of Eq. (2) with the smooth function specified as a polynomial of degree zero. This corresponds to the effect of the ban on 14-year-old children, that is, the first stage. Figures 2 to 7 can be seen as graphic representations of reduced-form estimates.[27]

[27] Figures C.2 to C.4 in the annex use data from one year earlier and show no discontinuity in participation rate for boys aged 14 around December 1997.

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

First Stage – 51 Weeks Bandwidth

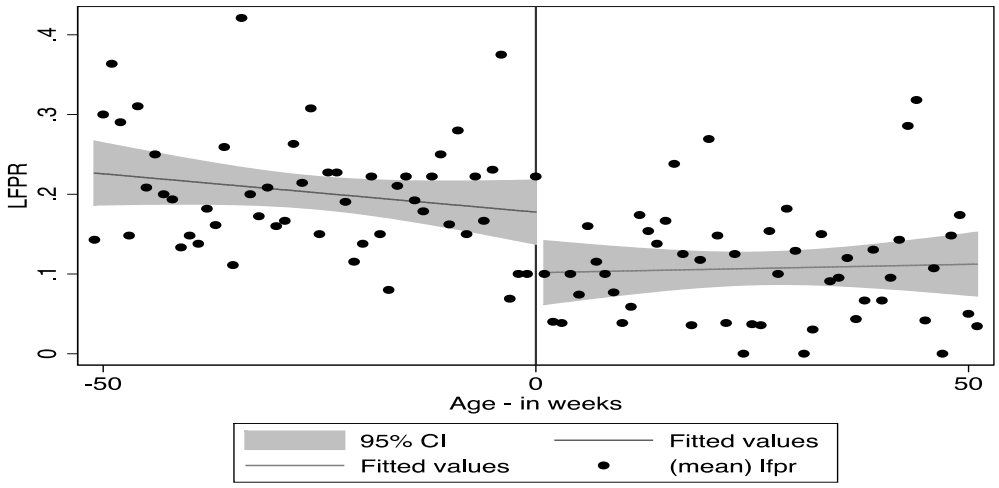


Figure 1 shows a significant decrease in the labour force participation rate as a whole (formal and informal) of boys who turned 14 after the ban. The reason for looking at participation rate as a whole instead of focusing on participation in the formal sector is because (i) participation in the formal sector is very low, even among youth not affected by the law, and (ii) as discussed in chapter one the law affected mostly boys in the informal sector. In face of this fall in participation rate, the question becomes whether this fall affected the time allocation of other household members.

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

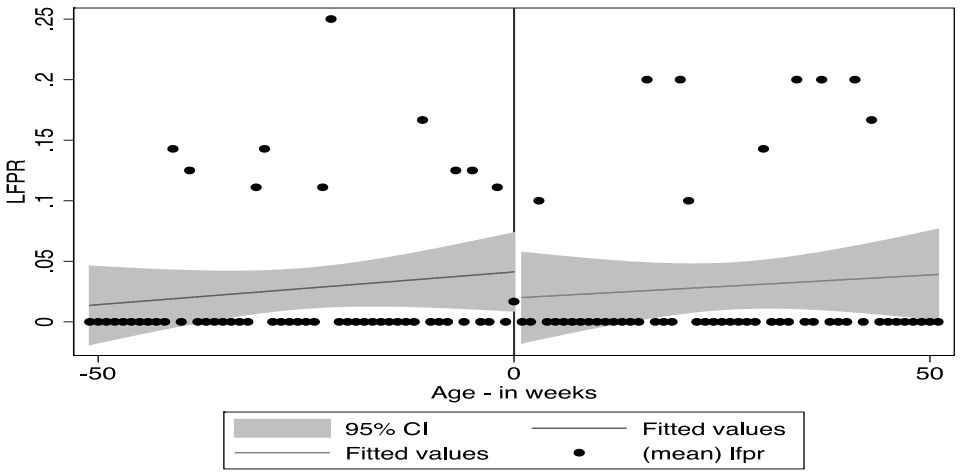


Figure 3 – Linear Regressions for LFPR of Mothers in Couple Parent Households

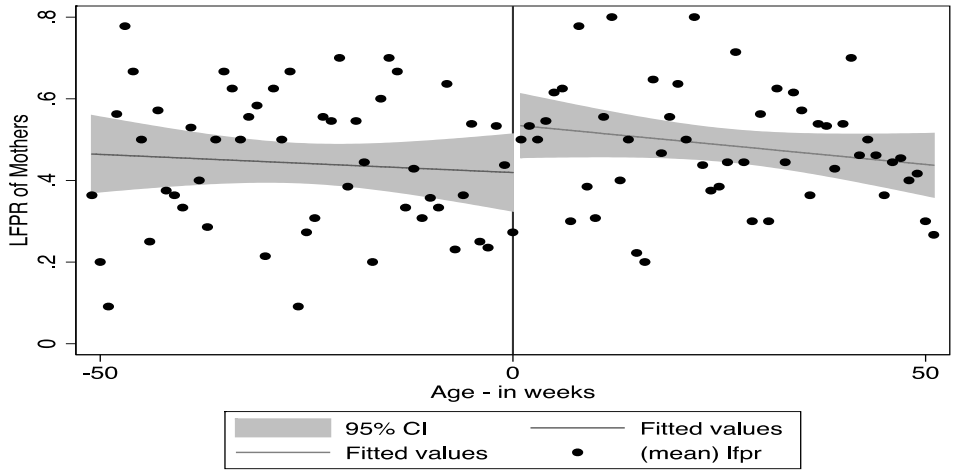


Figure 4 – Linear Regressions for LFPR in the Formal Sector of Mothers in Couple Parent Households

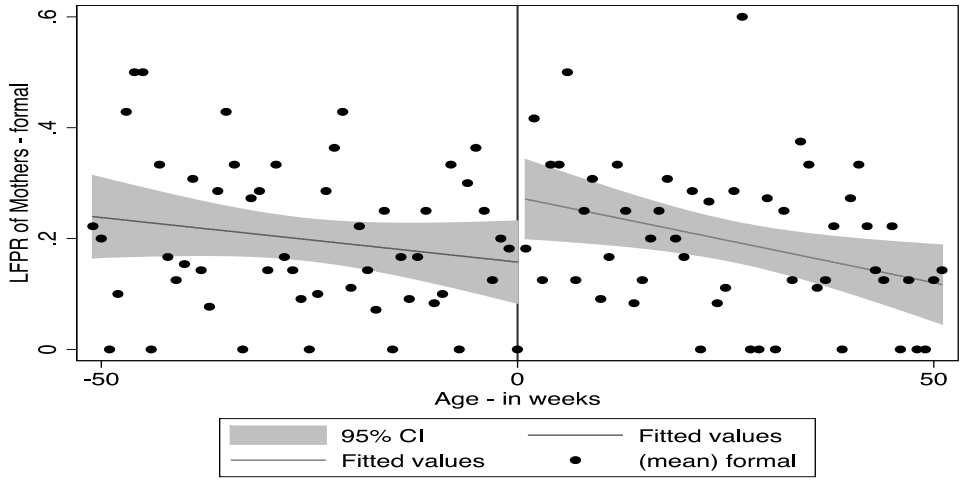


Figure 5 – Linear Regressions for Weekly Hours Worked of Fathers in Couple Parent Households

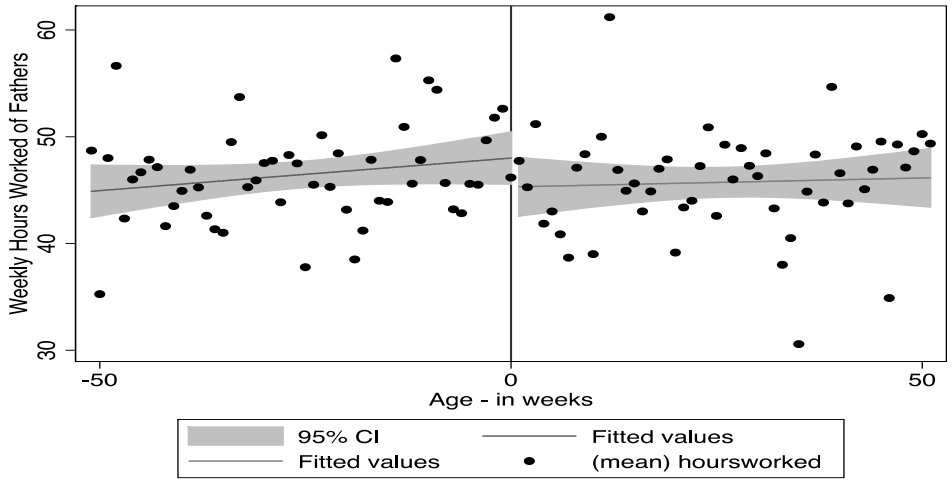


Figure 6 – Linear Regressions for LFPR in the Formal Sector of Fathers in Single Parent Households

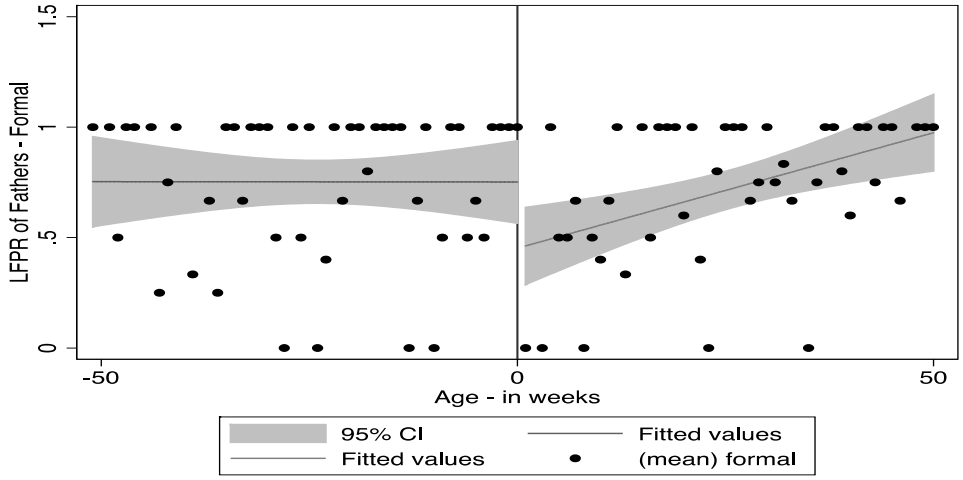


Figure 7 – Linear Regressions for LFPR in the Informal Sector of Fathers in Single Parent Households

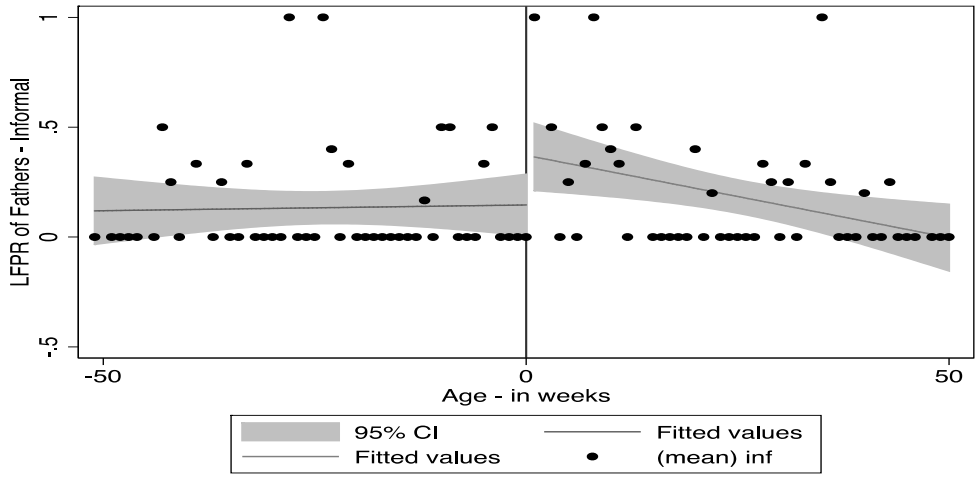


Figure 2 points to a small decrease in LFPR for younger siblings, but figures 3 and 4 show that mothers seem to be more likely to participate in the labour force, particularly in the formal sector. Figure 5 shows a small reduction in weekly hours worked among fathers in couple parent households.

Figures 2 to 5 suggest that couple parent households absorbed the ban, increasing mothers' participation in the labour force and reducing weekly hours worked of fathers, perhaps to help with household chores, and reducing the participation rate of younger siblings in paid work activities. This is an interesting result, as it would suggest that mothers' labour supply can be used as an imperfect insurance mechanism.

Figures 6 and 7 show a decrease in the participation rate in the formal sector and an increase of informality among single fathers. These results indicate that single fathers probably

face liquidity constraints, as discussed further below. Figures C.1 to C.8 inspect visually whether the ban affected other outcomes of household members in couple parent families. Figures C.3 and C.4 show a similar pattern to figures 6 and 7— that is, fathers seem to have shifted from the formal to informal sector after the ban, but the magnitude of effects seem to be smaller. Figure C.6 indicates that younger siblings became less likely to do household chores. Figures C.9 to C.18 show a similar analysis with single parent families. In addition to the results on the labour force participation rate of fathers, the only result that stands out is shown in figure C.17. Younger siblings apparently became more likely to attend school. Thus, with the ban, fathers moved to the informal sector while younger siblings became more likely to attend school.

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 51 weeks bandwidth size.

6 RESULTS

This section presents the short run estimates for the impact of the increase in minimum legal age on children affected by the ban and on younger siblings and parents. Estimates are provided for younger brothers and sisters, and mothers and fathers separately.

6.1 The Impact of the Ban on Children Aged 14

This section presents 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, 51 weeks and 20 weeks. The $h(z_i)$ function is specified as polynomials of degree zero to three and as linear and quadratic piecewise polynomials.[28]

Regressions are estimated for three outcome measures: participation rate, participation in the formal labour force, and participation in the informal labour force. Table 2 shows the first stage estimates with both bandwidth sizes.

[28] 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.

Most of the estimates of the participation rate of boys are negative and statistically significant at conventional levels, particularly for the larger bandwidth. Though the estimates with the narrower bandwidth are slightly smaller in absolute terms and less precise, most of the estimates indicate a reduction in the labour force participation rate of about 6 to 7 percentage points. In relative terms, this represents a fall of 52.6 to 61.4 percent.[29] 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 fall in participation rate in the informal sector indicates that some employers complied with the law and stopped hiring children under age 16.[30]

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 have 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 hence 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.[31]

[29] The participation rate of the control group is 11.4 percent with 51 weeks bandwidth.
[30] 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.
[31] 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, 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.

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.[32] We also investigate whether the impact of the ban changes according to family composition to understand household decision-making in these two different 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 3. Though the patterns for single mothers and single fathers suggest opposite responses to the ban, none of the coefficients are statistically significant. On the other hand, table 4 shows striking results. 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 consequence of the ban. 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 the single parents ended up.[33] We 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 risk coping mechanisms, such as unemployment insurance and/or credit markets, among single parent households affected by the 1998 ban. It seems that single fathers were more likely to respond to the income shock. The results indicate that they are likely low skilled workers and liquidity constrained to some extent.

The estimates for couple parent families are shown in table 5. 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 6. In relative

[32] 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.
[33] See figure C.22 and table C.20 in appendix. We also looked at hourly wages and the conclusions are the same.

terms, the participation rate of mothers increased by about 25 percent.[34] Dividing this number by the relative fall in the participation rate of sons aged 14, we find a cross-elasticity of labour supply of -0.41 to -0.48. A 10 percent fall in the participation rate of a son aged 14 increased mother's participation rate in the labour force by 4.1 to 4.8 percent. This is an interesting result, because it suggests that some mothers, though having skills to participate in the formal labour force, would otherwise stay home. 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.[35]

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 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 is likely because they could share the burden of the adverse shock with their spouses. The results for couple parent families suggest that mothers entered the labour force and fathers spent a few more hours at home, probably helping with household chores.

Tables 7 and 8 show the results for younger siblings in single parent households. Table 7 suggests no impact on the labour supply of younger siblings, but table 8 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.[36] The decision to invest in the human capital of younger sons might be due to an assumption among single fathers regarding the household production function and the returns to education of boys and girls.[37]

The effects of the ban on younger siblings in couple parent families are shown in Tables 9 and 10. 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

[34] 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.
[35] One could argue that this violates the luxury axiom that parents always prefer to consume their children's leisure if they can afford it. We 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.
[36] According to the results in chapter one, there is an indication that boys affected by the ban became more likely to do household chores.
[37] 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 marriage (Eswaran, 1996; Ennew, 1982).

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 fall in relative terms and a cross-elasticity of labour supply of 1.6 to 1.9. This result suggests a fairly elastic labour supply of younger siblings in couple parent households. This is actually an expected result, as younger siblings' labour supply is expected 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 being complementary inputs to some extent.

Putting the effects of the ban on the labour supply of parents and younger siblings together, one could argue that mothers entered the labour force to mitigate the shock for younger siblings. Since mothers are likely to get paid more than young children, particularly in the formal sector, this reallocation of time among household members caused by the ban seems to have resulted in efficiency gains.[38]

7 ROBUSTNESS CHECK

To check robustness, some regressions are estimated with 20 weeks bandwidth. Since smaller samples lead to loss in precision, we 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 11 and 12 present the 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 became 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 on the labour supply of single parents are shown in table 13. The coefficients for single mothers and fathers are qualitatively similar to those observed with the larger bandwidth; however, the estimates on weekly hours worked of mothers are larger and statistically significant. Table 14 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.

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

Results for siblings in couple parent families can be seen in tables 15 and 16. 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 17 and 18).

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 were 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.3 shows the first stage estimates and tables C.4 to C.14 present the results of household members. Estimates are provided with 51 weeks bandwidth.[39] 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 the linear specification is not robust and suggests that different specifications should be tested to check robustness when the model is estimated parametrically.

Most of the estimates for younger siblings and parents are statistically insignificant. Tables C.6 to C.8 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 different specifications and statistically significant.

In the results discussed above, we 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 we claim to be the effects of the law, we do not believe they harm our findings.

Nevertheless, in order to unpack this puzzling result we provide visual and regression checks. Figures C.19 to C.21 inspect the placebo results for single mothers visually in order to identify which observations might be driven these large coefficients. The figures plot local

[39] As before, for 51 weeks bandwidth the same vector of covariates used in the previous estimates are used to control for potential confounders related to observed characteristics.

polynomial regressions for the labour force participation rate of single mothers. Figure C.19 indicates that observations close to the threshold seem to drive the average effects. Figure C.20 plots similar regressions dropping observation in the (-4, 4) interval, that is, children who turned 14 between December 1997 and January 1998, whereas figure C.21 drops observations in the (-6, 6) interval. As suggested by the figures, the strong effects of the participation rate of single mothers seem to be very local. Table C.9 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.[40] Although we believe that this placebo result does not harm the main findings discussed above, we cannot rule out the hypothesis that the placebo regressions for non-white single mothers might be picking some seasonal birth effects.[41]

With regard to siblings, most of the estimates are statistically insignificant. Table C.15 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 we did not find any impact of the ban on school outcomes of younger siblings in couple parent families.

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.[42]

[40] 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.

[41] Based on very 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 explained by and large by women's socioeconomic background and by the 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, we 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 51 weeks bandwidth, we 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).

[42] Footnote 21 explains how school start could affect a mother's labour supply.

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 the 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 most of these papers face has to do with 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, the 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,[43] 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 being followed by most families and to some extent enforced by Brazilian schools by the time the 1998 law passed, my estimates could be reflecting 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 where 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

[43] Since 2010 children have 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.

human capital through work experience (Black et al., 2011).[44] 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 be capturing 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 are capturing the effect of the school entry law, table C.16 in the appendix shows first stage estimates with the cutoff defined as 30 June 1999. Estimates are provided with 51 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.17 to C.19 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 paper and suggest that age at school entry is unlikely to influence the results.

We 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 of birth effects, an issue raised recently by Buckles and Hungerman (2013). As discussed above, the results with 20 and 51 weeks bandwidth are very similar.

8. CONCLUSION

This paper 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 children aged 14 on 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 very minor among younger siblings but more relevant among parents, particularly when family composition and the occupation of the household head are taken into account.

[44] 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.

We looked at the labour force status of the household head to shed light on whether the household could face some sort of credit constraint. We 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. We interpret this result as an indication that credit could be a binding constraint for some households.

Splitting the sample according to family composition revealed 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. We 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 we found no impact on single mothers' labour supply. On the other hand, we found an almost perfect shift of single fathers from the formal to the informal sector. We interpret these results as an indication that single parent households supply labour more inelastically and 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 backfire. Insurance mechanisms such as unemployment insurance or even conditional cash transfers could be offered to households affected by the ban.

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Table 1a – 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 1b – Descriptive Statistics and Difference in Means

Younger siblings aged 10 to 13 with a brother aged 14 around December 1998
51 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 1c – 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%.

Table 1d – Descriptive Statistics and Difference in Means

Household head aged 30 to 60 with a son aged 14 around December 1998
51 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 2 – 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			*51 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.

Family Composition and Labour Force Status of Parents

Table 3 – Parametric ITT Estimates of the Impact of the Ban on Household Head's Labour Supply – Single Parent Households

51 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.

Table 4 – Impact of the Ban on Labour Force Status of the Household Head – Single Parent Households

51 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.

Table 5 – Parametric ITT Estimates of the Impact of the Ban on Parents' Labour Supply – Couple Parent Households

51 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 6 – Impact of the Ban on Labour Force Status of Parents – Couple Parent Households

51 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 7 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Single Parent Households

51 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 8 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Single Parent Households

51 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.

Table 9 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Couple Parent Households

51 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 10 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Couple Parent Households

51 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.

Robustness Check

Table 11 – 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 12 – 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 13 – 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 14 – 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 15 – 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 16 – 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 17 – 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 18 – 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.

Table C.1 – Family Composition – # of individuals

	Frequency	Percent	Cumulative Percent
No parents present	8,004	4.67	4.67
One parent	54,890	32.03	36.69
Two parents	93,426	54.51	91.2
More than one family in the household	15,077	8.8	100
Total	*171,397*	*100*	

Table C.2 – Gender of the Head in Single and Couple Parent Households

	Single Parent HHs	Couple Parent HHs
Female	13,560	2,899
Column%	61.69	11.44
Male	8,421	22,452
Column%	38.31	88.56
Total	*21,981*	*25,351*
Column%	100	100

Placebo Test

Table C.3 – 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

51 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.4 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Placebo Test

51 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.5 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Placebo Test

51 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.7 – Parametric ITT Estimates of the Impact of the Ban on Household Head's Labour Supply – Single Parent Households

51 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.8 – Impact of the Ban on Labour Force Status of Parents – Single Parent Households

51 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.9 – Placebo Regressions for Labour Force Participation of Single Mothers

51 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.10 – Parametric ITT Estimates of the Impact of the Ban on Parents' Labour Supply – Couple Parent Households

51 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.11 – Impact of the Ban on Labour Force Status of Parents – Couple Parent Households

51 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.12 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Single Parent Households

51 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.13 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Single Parent Households

51 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.14 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Couple Parent Households

51 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.15 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Couple Parent Households

51 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.16 – 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

51 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.17 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Placebo Test

51 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.18 – Parametric ITT Estimates of the Impact of the Ban on Younger Siblings – Placebo Test

51 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.19 – Parametric ITT Estimates of the Impact of the Ban on Household Head's Labour Supply – Placebo Test

51 weeks bandwidth

	Household Head		Female Head		Male Head	
h(z)	LFPR	Hours Worked per	LFPR	Hours Worked per	LFPR	Hours Worked per
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.

Couple Parent Households

Figure C.1 – Linear Regressions for LFPR of Mothers in the Informal Sector in 1999

51 Weeks Bandwidth

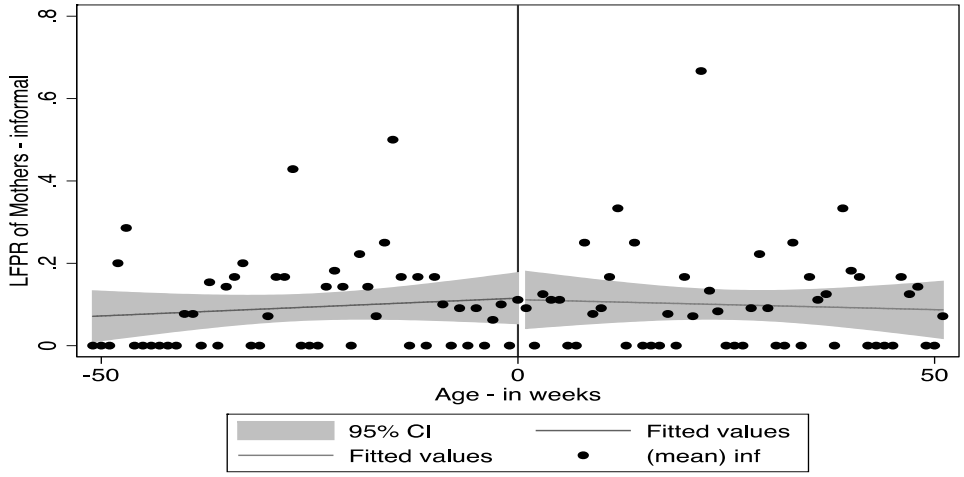


Figure C.2 – Linear Regressions for LFPR of Fathers in 1999

51 Weeks Bandwidth

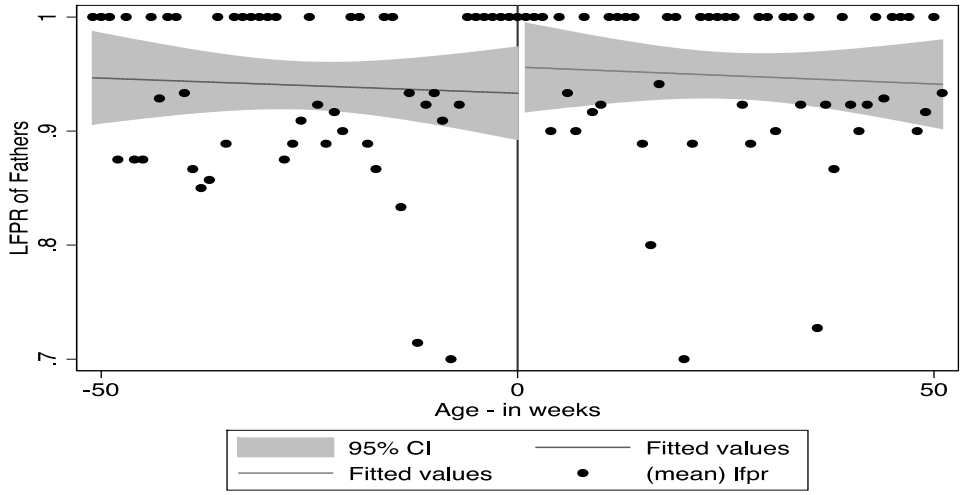


Figure C.3 – Linear Regressions for LFPR of Fathers in the Formal Sector in 1999

51 Weeks Bandwidth

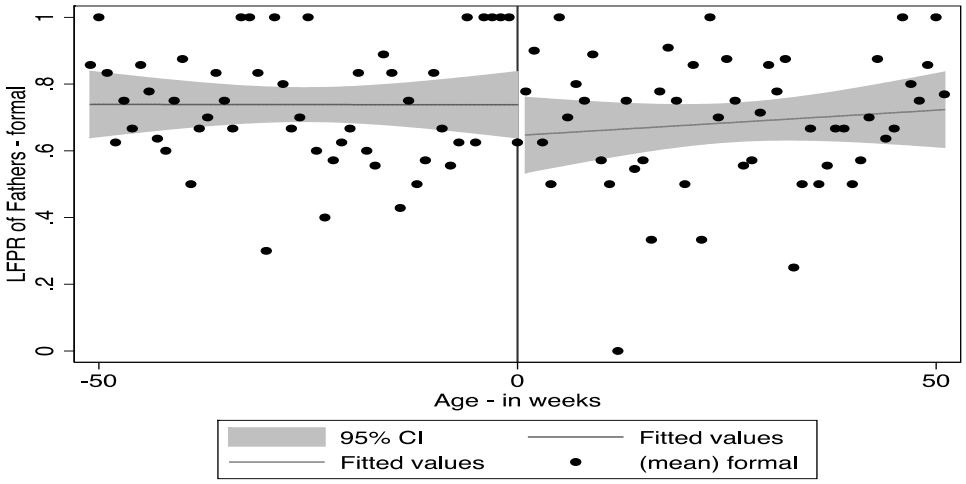


Figure C.4 – Linear Regressions for LFPR of Fathers in the Informal Sector in 1999

51 Weeks Bandwidth

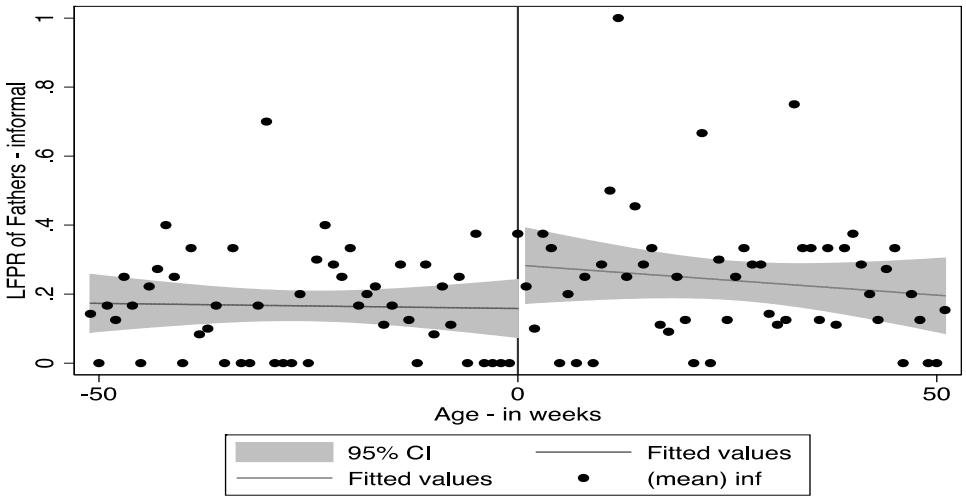


Figure C.5 – Linear Regressions for Weekly Hours Worked of Mothers in 1999

51 Weeks Bandwidth (exclude zeros)

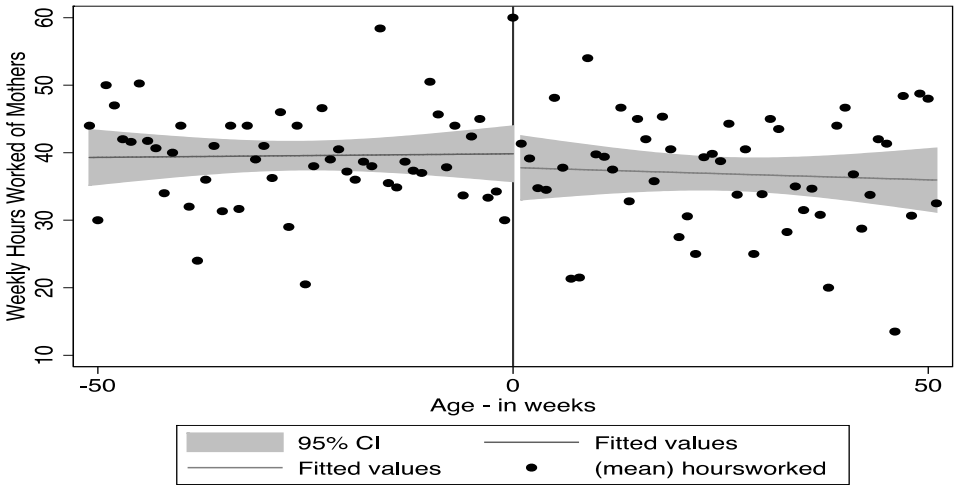


Figure C.6 – Linear Regressions for Household Chores of Siblings in 1999

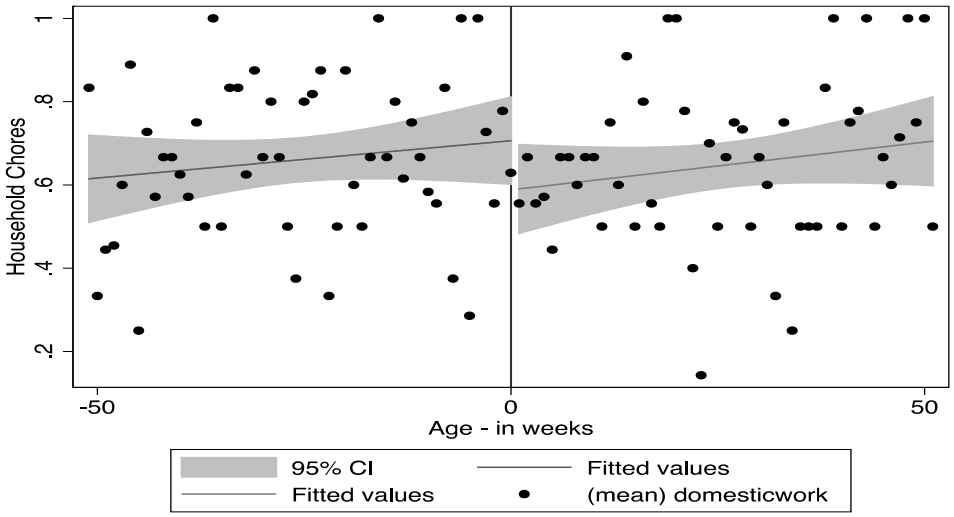


Figure C.7 – Linear Regressions for School Attendance of Siblings in 1999

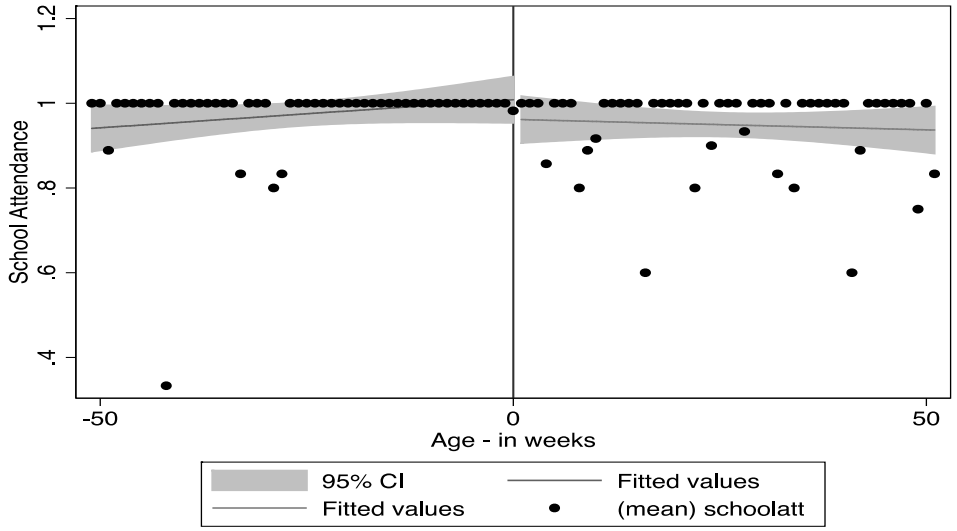
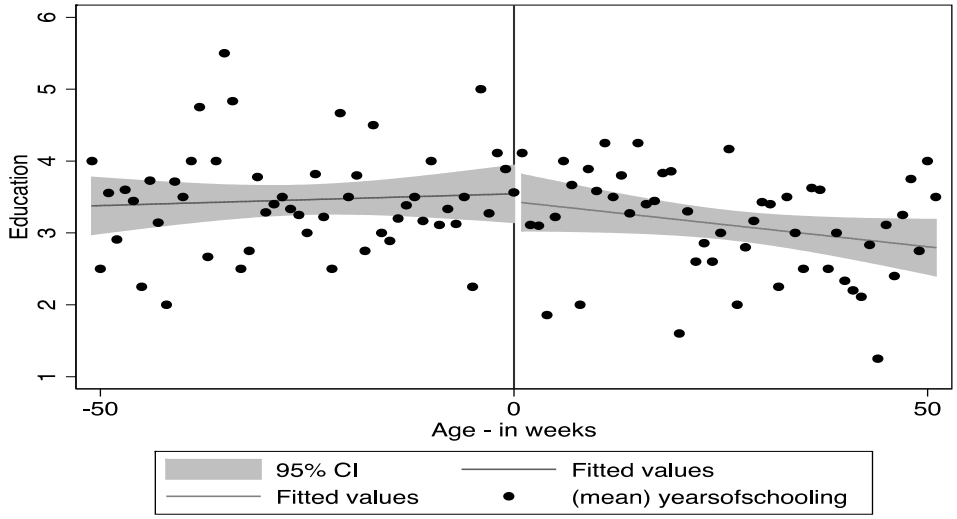


Figure C.8 – Linear Regressions for Completed Years of Schooling of Siblings in 1999



Single Parent Households

Figure C.9 – Linear Regressions for LFPR of Mothers in 1999

51 Weeks Bandwidth

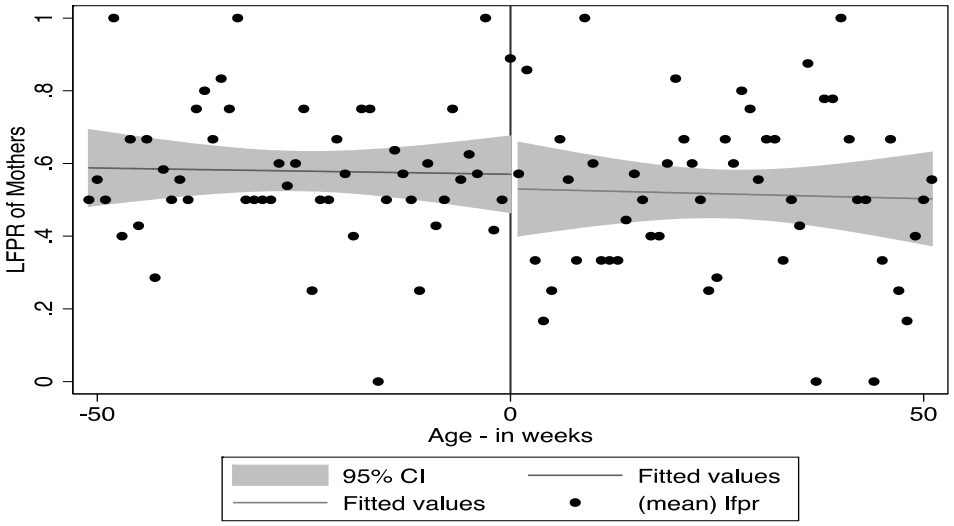


Figure C.10 – Linear Regressions for LFPR of Mothers in the Formal Sector in 1999

51 Weeks Bandwidth

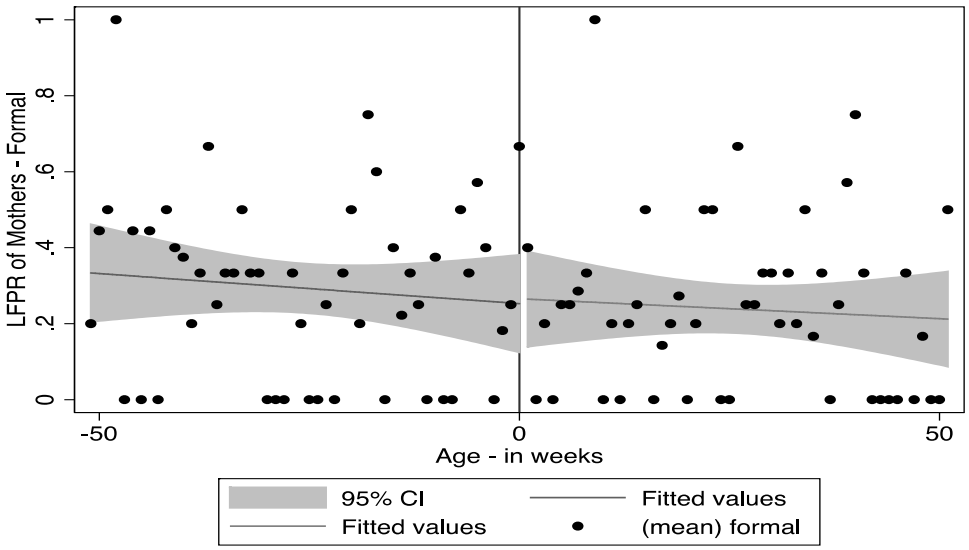


Figure C.11 – Linear Regressions for LFPR of Mothers in the Informal Sector in 1999

51 Weeks Bandwidth

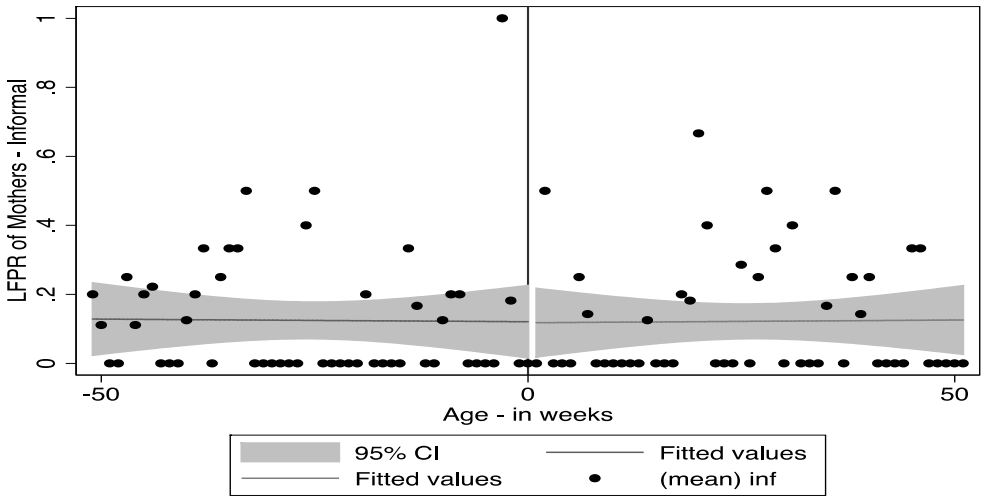


Figure C.12 – Linear Regressions for LFPR of Fathers in 1999

51 Weeks Bandwidth

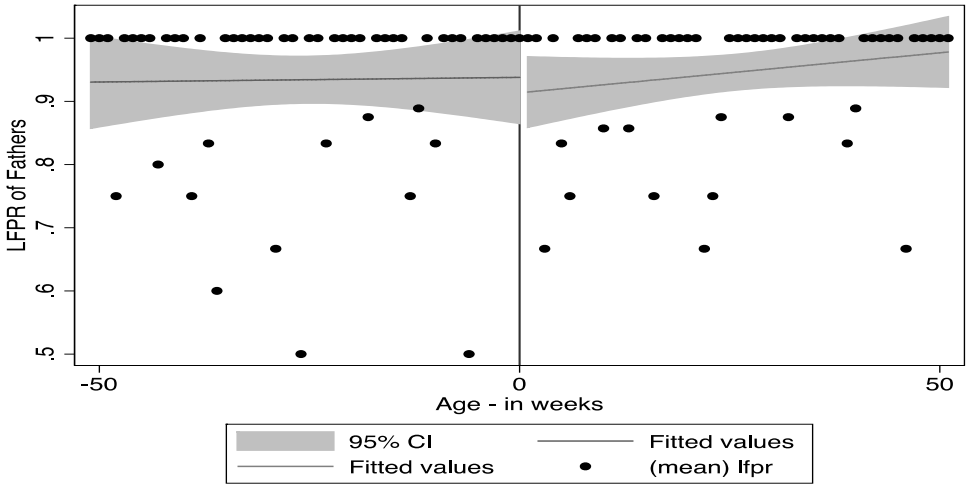


Figure C.13 – Linear Regressions for Weekly Hours Worked of Mothers in 1999

51 Weeks Bandwidth (exclude zeros)

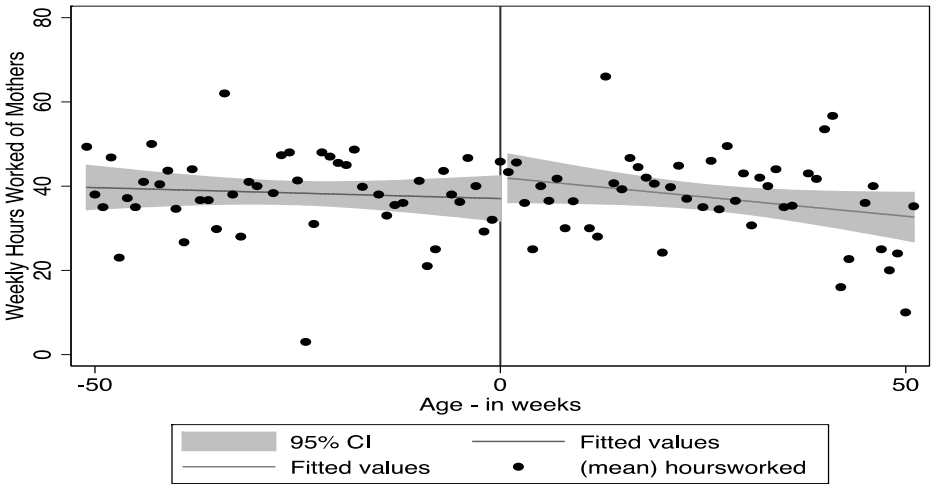


Figure C.14 – Linear Regressions for Weekly Hours Worked of Fathers in 1999

51 Weeks Bandwidth (exclude zeros)

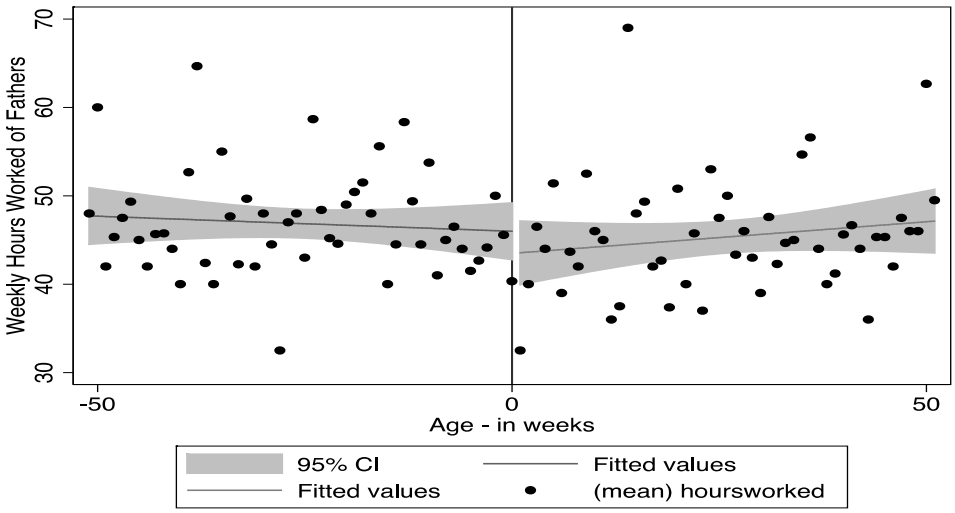


Figure C.15 – Linear Regressions for LFPR of Siblings in 1999

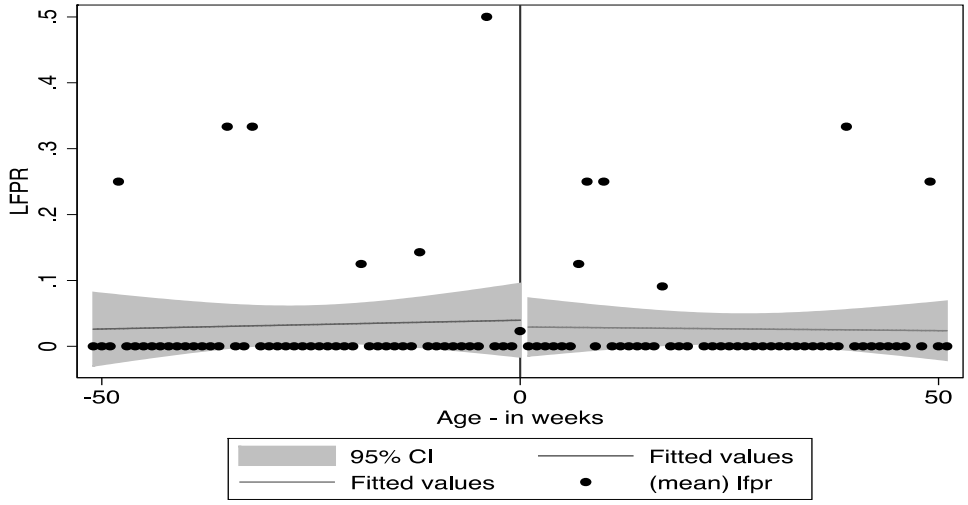


Figure C.16 – Linear Regressions for Household Chores of Siblings in 1999

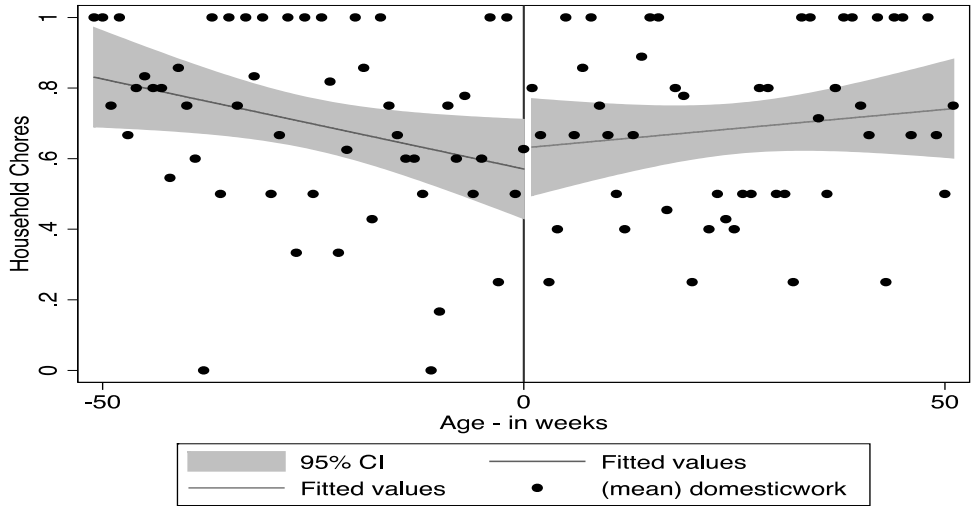


Figure C.17 – Linear Regressions for School Attendance of Siblings in 1999

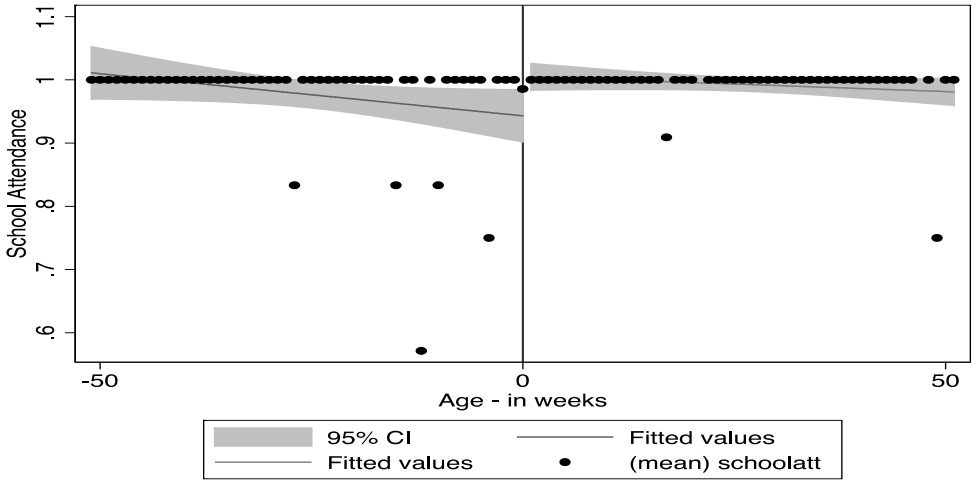


Figure C.18 – Linear Regressions for Completed Years of Schooling of Siblings in 1999

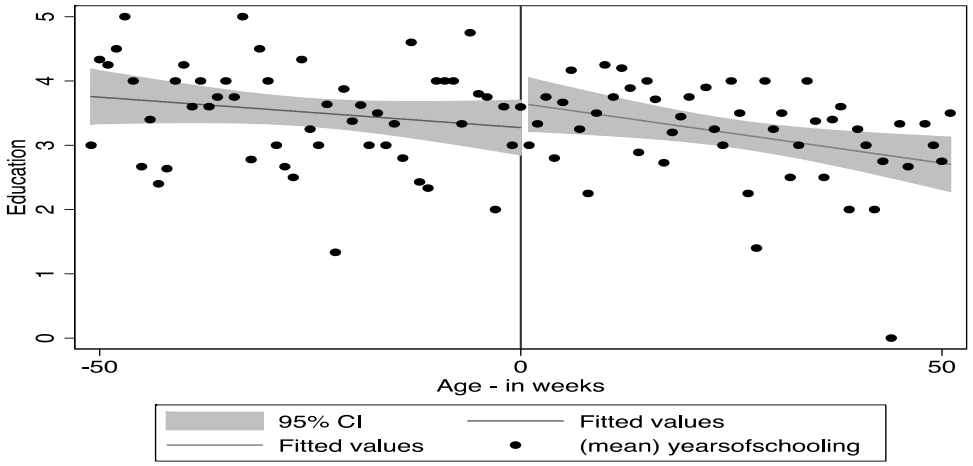


Figure C.19 – Local Polynomial Regressions for LFPR of Single Mothers in 1998

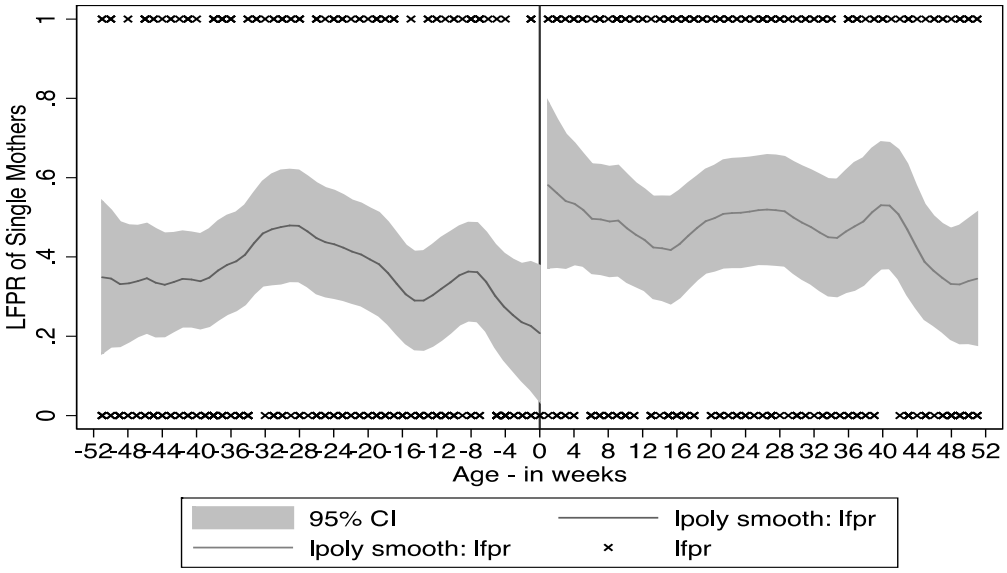


Figure C.20 – Local Polynomial Regressions for LFPR of Single Mothers in 1998

Excludes observations in the (-4, 4) interval.

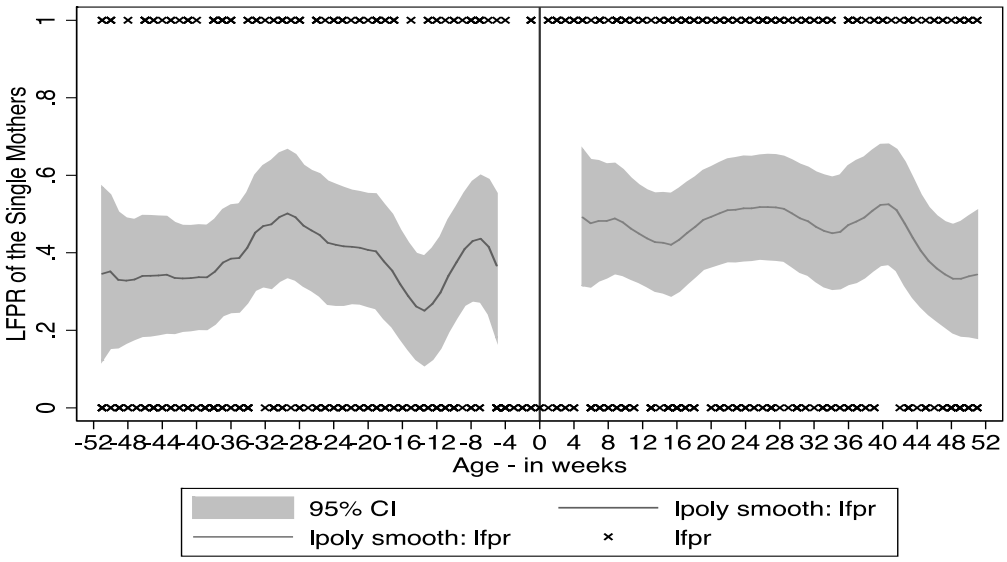


Figure C.21 – Local Polynomial Regressions for LFPR of Single Mothers in 1998

Excludes observations in the (-6, 6) interval.

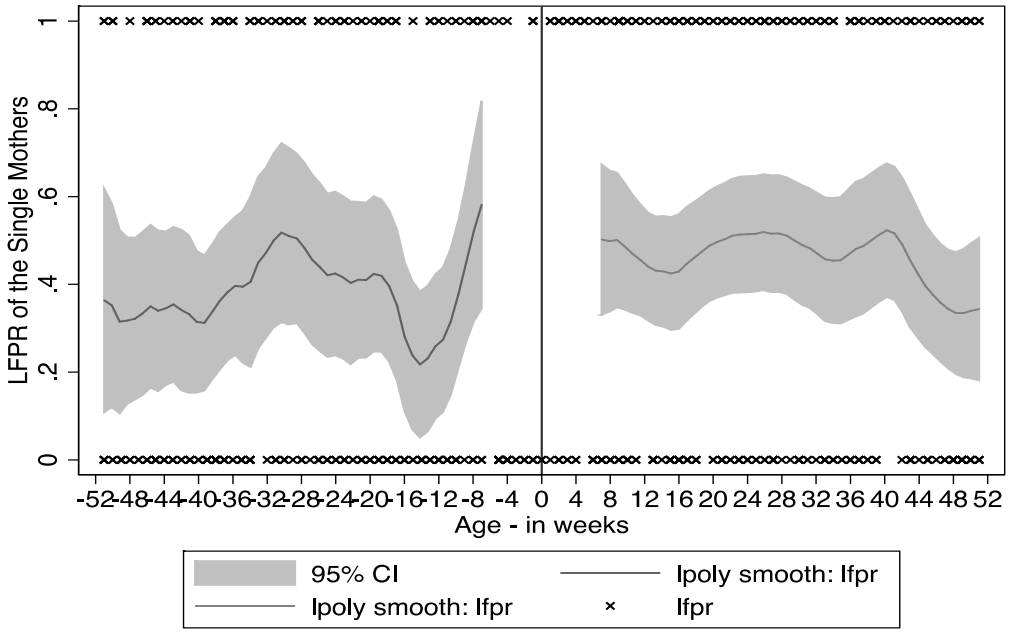


Figure C.22 – First Order Stochastic Dominance – Monthly Wage Earned by Single Fathers in the Formal and Informal Sectors

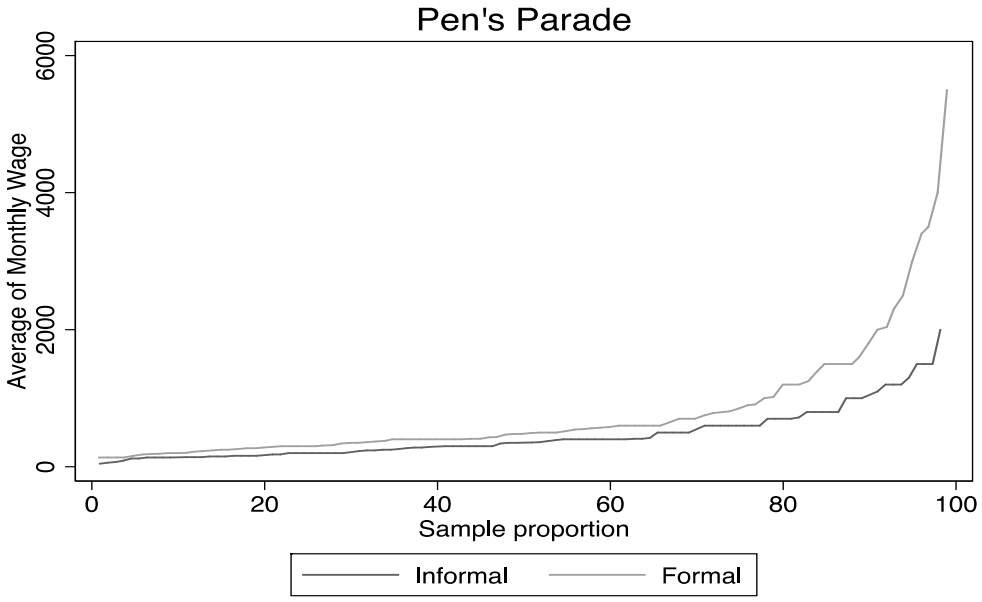


Table C.20 – ITT Estimates for Occupation of Single Fathers

	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	1309.73	907.33	293.17	414.52	528.79	615.37	302.46	370.40
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.