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Caio Piza
World Bank

André Portela Souza
Fundação Getúlio Vargas (EESP-FGV) and Center for Applied Microeconomics Studies

Patrick M. Emerson
Oregon State University, IZA and Center for Applied Microeconomics Studies (EESP-FGV)

Vivian Amorim
World Bank

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

The Short- And Longer-Term Effects of a Child Labor Ban*

Are bans effective at lowering child labor and increasing school attendance and, if so, do these effects lead to positive outcomes later in life? This paper seeks to answer these questions by examining the effect of a 1998 Brazilian law that increased the minimum employment age from 14 to 16. To examine this question we use two different regression discontinuity designs to analyze Brazilian household data. We find that the ban had no overall impact across affected children in Brazil, but that it led to a significant decrease in the labor market participation of urban boys, whose paid labor dropped 35 percent, driven mainly by a decrease in informal work. We also find a concomitant 10 percent increase in the share of urban boys only attending school. Interestingly, we find that by age 18 this cohort was still almost 20 percent less likely to have a paid job and was less likely to be economically active even when they were legally allowed to work. However, we find no evidence that the impact of the ban lasted over time as reflected in measures of educational attainment, employment rates, and wages. Our results suggest that when enforced, bans on child labor can have significant immediate impacts amongst affected populations, leading to a decrease in work and an increase in school attendance. It remains unclear if these impacts translate to improved adult outcomes.

JEL Classification: C21, J08, J22, J24, K31

Keywords: child labor, education, labor laws

Corresponding author:
Patrick M. Emerson
School of Public Policy
Oregon State University
Corvallis, OR 97331
USA

E-mail: patrick.emerson@oregonstate.edu

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1 Introduction

Numerous studies have shown the deleterious effects of working as a child, particularly at younger ages. Though child labor rates have been declining worldwide, the numbers are still alarmingly high. In 2020, the International Labor Organization estimated that 160 million children aged 5 to 17 were working, and almost half of them were involved in hazardous activities (International Labor Organization (2020)). However, recent evidence shows that the number is likely to be more than two times higher due to under-reporting (Lichand and Wolf (2022)). In Brazil, more than 2.2 million, or 18 percent, of children aged 14 to 17 are economically active. Among those children, 1.2 million are effectively working, and 80 percent of those children are in the informal sector.[1]

Labor laws and regulations are the policy instruments favored by international organizations and used by national governments worldwide to fight child labor. Indeed, ILO Convention 138 recommends that national laws set the minimum employment age above 15. But the question of whether labor laws that limit child labor are effective is still an open one. In fact, the few studies of child labor bans in lower income countries have generally found little evidence to suggest they work or have long-run effects.

This paper seeks to contribute to the knowledge of the effects of child labor restrictions. To do so, we use the 1998 increase in minimum employment age from 14 to 16 in Brazil as a natural experiment. In that year, more than one-quarter of all 14-year-olds in Brazil were economically active, and 20 percent were working in paid or unpaid jobs. By preventing 14-year-olds from entering the formal labor force, and as compensating policies such as conditional cash-transfer programs or apprenticeship programs had not yet been implemented, the immediate effect of the ban was to reduce the choice-set of time allocation for the affected group. If they were forced to postpone their entrance into the formal labor market, they could potentially either continue to be active in the informal labor market, spend more time attending school, or do neither.[2]

[1] *Pesquisa Nacional por Amostra de Domicílios Contínua* (PNADC), 2020.

[2] The Brazilian conditional cash-transfer program, Bolsa Escola/Bolsa Família was in its pilot stage in 1999 (Glewwe and Kassouf (2012)), and the Brazilian apprenticeship program was institutionalized in December 2000, two years after the child labor law change.

In addition, if the ban effectively prevented children from working and led to more time in school, it might be expected that the affected children would have better employment positions when older. However, if teen jobs have a significant vocational training component, being prevented from engaging in them might lead to lower labor market outcomes a few years later (Alfonsi et al. (2020), Le Barbanchon et al. (2021)).

To assess the impact of the child labor ban, we use repeated cross-sectional data from the Brazilian Household Survey from 1998 to 2014 and employ two regression discontinuity designs: the traditional continuity-based approach, and one that relies on the idea of a local randomization mechanism. We find that the law had no overall measurable impact across all Brazilian children, but that it did have an immediate impact on 14-year-old urban boys, the largest cohort of child labor in Brazil, who postponed their entrance into the labor market and whose paid labor dropped more than 35 percent, driven mainly by a decrease in informal work. No short-term effects were found for children in rural settings or for girls in urban areas.

The ban also influenced the time allocation of the affected children. From ages 14 to 18, the affected cohort of urban boys was significantly more likely to be only attending school. These effects appear to linger, four years after the ban; at age 18, the affected cohort was 20 percent less likely to be engaged in paid activities compared to the unaffected cohort and it was also less likely to be economically active.

In the long-term, between eight and fifteen years after the policy change, we find there is no significant difference in the percentage of employment, formal occupation, wage per hour, and undergrad enrollment or completion among urban boys. It might be that the ban led to no net long-term effects in outcomes, or that the relatively small number of 14-year-old boys affected by the ban among the entire population led to difficulty identifying the true effect.

The rest of this paper is organized as follows: Section 2 discusses related literature. Section 3 discusses the institutional setting and the intervention. Section 4 presents the empirical strategy. Section 5 describes the data. Section 6 presents the short-term results. Section 7 presents the long-term results. Section 8 discusses policy implications and concludes.

2 Related Literature

There is extensive theoretical literature on child labor starting with Basu and Van (1998). The authors discuss how bans on child labor can either increase adult wages enough to move the economy to an equilibrium without child labor, or can harm households if the increase in adult wages does not adequately replace the loss in household income (see also Ranjan (1999, 2001), Baland and Robinson (2000), Horowitz and Wang (2004), Dessy and Pallage (2001), Dessy and Knowles (2008)).[3] Doepke and Zilibotti (2005) develop a model in which a child labor ban is endogenously determined.[4] They predict that support for child labor bans may increase over time once the bans are in place, if the cost of schooling is sufficiently low and the value of child work is not too high. If these conditions are not met, child labor policies might make families and children worse off.

Evidence of the consequences of child labor has increased in the last 20 years. Tyler (2003) uses U.S. data from the 1980s and finds that working while studying is detrimental to learning among high-school students. Bezerra et al. (2009), Emerson et al. (2017) show that very early entry into the labor market harms individuals' outcomes in adult life over and above the effect on schooling but this negative effect is reversed as youth age. Lee and Orazem (2010) find that an early entrance into the labor force coincides with premature school dropout results and in worse health outcomes in Brazil.[5] Beegle et al. (2009) investigates the medium-term consequences of child labor on schooling, labor market, and health outcomes in rural Vietnam and finds that child labor has a negative effect on school attendance and educational attainment but a positive effect on labor market outcomes such as paid work and earnings.[6]

Le Barbanchon et al. (2021) assess whether working while in school smooths students' transition into the labor market, exploring a youth employment program offered by a lottery in Uruguay. The intervention targets 16- to 20-year-olds and randomly selects lottery winners for a part-time job in a state-owned company for 9 to 12 months. The authors find evidence that two years

[3]See Edmonds and Shrestha (2012) for a comprehensive discussion of the child labor literature. Evidence from Brazil suggests that there are other determining factors for child labor over and above poverty, such as parental preferences for early exposure to labor markets (Emerson and Souza (2003)), see also Emerson and Knabb (2013, 2007, 2006).

[4]In our case, even if the change in law is endogenous, we believe our results are still valid since we compare families at the margins of the cut-off at the timing of the birth realizations.

[5]Health-related outcomes include a higher probability of back problems, arthritis, and reduced stamina.

[6]Beegle et al. (2009) find no impact on health outcomes.

after the intervention, the treated youth had earnings 6 percent higher than the control group, suggesting that working while in school increased productivity. Enrollment rates after program participation were also 4 percentage points higher among the treated group, indicating that the intervention does not crowd out school investment.

However, there is much less evidence on the consequences of child labor laws, such as an increase in the minimum employment age (MEA) or compulsory schooling. The available literature on MEA is limited almost exclusively to the American experience during the first three decades of the last century (Moehling (1999), Margo and Finegan (1996), Lleras-Muney (2002), Manacorda (2006)). With the exception of Manacorda (2006), who looks at the impact of the increase in the minimum legal working age on time allocation of other household members in the U.S., the literature focuses exclusively on one outcome—employment rates—and results point to a small effect of such a policy. A few studies have explored the combination of minimum employment age laws and compulsory schooling to assess whether they are an effective way to fight child labor (Goldin and Katz (2011)). Two studies, Margo and Finegan (1996) and Lleras-Muney (2002) find greater reductions in child labor rates after combining compulsory schooling and child labor laws.

We are aware of only five studies that investigate the effects of an increase in the minimum employment age in developing countries: the aforementioned Piza and Souza (2016) and Bargain and Boutin (2021), Bharadwaj et al. (2020), Kozhaya and Martinez Flores (2022), and Edmonds and Shrestha (2012). Bharadwaj et al. (2020) investigate the impact of the child labor ban in India and find that the law increased child labor in the informal sector 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. Edmonds and Shrestha (2012) study the enforcement of such laws using microdata from around 60 low-income countries and find mixed results. Kozhaya and Martinez Flores (2022) assess the effects of a similar change in the minimum working age law in Mexico in 2015 using a DID strategy and find a decrease in the probability of working and and increase in the probability of being enrolled in school and that these effects persist for several years.

Evidence of the long-term consequences of child labor laws is also very limited and most is derived from studies using compulsory schooling as an instrumental variable to estimate returns on education (Angrist (1990); Oreopoulos (2006, 2007)). Many of these studies look at the impacts of educational policies on high-school quality (Dustmann et al. (2012)), high-school accountability (Deming et al. (2016)), teacher quality (Chetty et al. (2014)), school choice (Lavy (2015a), and teacher pay-for-performance (Lavy (2015b)). Others assess the impact of youth training or vocational education on labor market outcomes (Card et al. (2011), Hicks et al. (2013); Hirshleifer et al. (2016), Attanasio et al. (2015), Kluve et al. (2015)) or 'remedying' interventions targeted at disadvantaged children (Angrist et al. (2006)). Evidence from this literature points to a positive long-term effect on educational attainment and labor market outcomes.

Though no study has explored the long-term effects of a child-labor ban, two previous studies have investigated the short-term effects of the increase in the minimum legal working age in 1998 in Brazil. Piza and Souza (2016) employ a difference-in-differences design and find evidence of a four percentage point reduction in the work incidence of urban boys, roughly a one-third decrease in child labor. This drop was mostly explained by a fall in informal work. They found no impacts on girls. Similar to the findings in this paper, Bargain and Boutin (2021) employ a RDD but find that overall the legislation did not have a measurable effect, however they do find effects in regions characterized by stronger labor law enforcement. In states with above-median inspection rates, the authors detect a 4 percentage points decrease in child labor. Even though Piza and Souza (2016) and Bargain and Boutin (2021) work on the 1999 wave of the Brazilian Household Survey (PNAD), it is important to describe the key differences between the two studies.

There are three factors driving the differences in the results found by Bargain and Boutin (2021) and piz (????). First, the main dependent variable used by Bargain and Boutin (2021) is whether the child has a paid job or works in an unpaid activity for her own family or others.[7] Piza and Souza (2016) used paid work since almost 98 percent of 14-year-olds in unpaid activities were either members of the household in which they work or were workers for self-consumption/production.[8] Since it is unlikely that the ban would impact children in

[7]Except children for whom the main occupation is work in agriculture or construction for self-consumption.The replication package for Bargain and Boutin (2021) is available at: https://academic.oup.com/wber/article-abstract/35/1/234/5681375.

[8]Among the ones working in unpaid activities, 98 percent of 14-year-old boys and girls in urban and rural areas worked for the household they lived in or for self- consumption/production (PNAD, 1999). Among those children, 76.8 percent worked for their household and 23.2 percent were workers for self-consumption/production. To calculate these percentages, we used the variables v9008 and v9029, which are the main activities of children

unpaid jobs, we follow piz (????) and opt to assess the impact of the ban on paid labor.

Second, Bargain and Boutin (2021) includes both urban and rural employment and both boys and girls. However, almost 80 percent of 14-year-olds who worked in rural areas were unpaid, and more than half of the girls in paid jobs in urban areas worked as housekeepers in the house of the employer, where enforcement of the law is much less likely.[9]

Finally, Bargain and Boutin (2021) excludes households in which the child is not identified as the son or daughter of the individual listed as the head of the household and households in which the head is younger than 18 or older than 60. As multigenerational households are common in Brazil (in the 1999 survey 11.8 percent of all 14-year-olds are listed as neither the son nor daughter of the head of the household, and most of these individuals are listed as "other relative"), we decided to retain these households in the sample.[10]

The sample exclusions employed by Bargain and Boutin (2021) reduced the sample by 16 percent, which represents a significant number of children who were potentially impacted by the ban.[11] We believe that the sample used by Piza and Souza (2016) is more appropriately targeted to the population likely to be affected by the ban, which allows us to effectively evaluate the impacts of the intervention.

Our study adds new evidence to the literature by evaluating the short- and long-term effects of an active labor market policy aimed at young people, a policy that acted through an under-explored channel involving the restriction of time allocation for youth.

3 Institutional Background

In 1988, the Brazilian Constitution established the minimum legal age of entry into the labor market as 14. In 1990, a federal rule (*The Statute of Children and Adolescents*) established rights for children and youth rights beyond regulating the conditions of formal labor market

in agricultural and non-agricultural activities, respectively. According to these variables, children are categorized as either an unpaid worker in the household or as a worker for self-consumption/production.

[9]PNAD, 1999.

[10]PNAD, 1999.

[11]See our online appendix: https://github.com/worldbank/child-labor-ban-brazil/blob/main/child_labor_ban_brazil_appendix.pdf presents the main differences between our work and Piza and Souza (2016) from Bargain and Boutin (2021).

entry.[12] Complementing the Constitution, the statute is considered the legal framework for children and youth in the labor market.[13]

From 1988 to 1998, the minimum legal working age in Brazil was 14, and individuals under 17 were prohibited from working in hazardous activities. On December 15, 1998, Constitutional Amendment n. 20 increased the minimum legal age of entry into the labor market from 14 to 16, with the exception that children under 16 could work as apprentices, although the regulations for apprenticeships were not enacted until the end of 2000.[14] Individuals younger than 18 were prohibited from hazardous and night-shift work. The law became effective the following day (December 16, 1998). Children younger than 16 years old, who were already employed by the time the law passed, were not affected by the ban.

The administrative data on employment, the *Relação Anual de Informações Sociais* (RAIS), basically a census of the formal sector in the country, suggests an imperfect compliance with the implementation of this legislation. In 1999, when the law was already in place, we find 14- and 15-year-olds listed in the formal market, only about a third of whom had started working before the law was enacted and were, therefore, unaffected by the ban. Among those who started working after the ban, only 2.4 percent were hired as apprentices, one of the exceptions to the ban.

Interestingly, the law mostly affected individuals who turned 14 after it was passed. Using the 1999 wave of the Brazilian Household Survey (PNAD), we find that individuals who turned 14 before the ban were three times more likely to be working in the formal sector than those who turned 14 after the law change.[15] Also, according to RAIS, in 1999, among those hired after the law was enacted, more than 60 percent turned 14 before the law changed. Therefore, the statistics support the view that these two cohorts were treated differently by law enforcers,

[12] *Lei do Estatuto da Criança e do Adolescente*, Law n. 8069, 13 July 1990.

[13] In this paper the terms 'children', 'teenagers,' and 'youth' are used interchangeably.

[14] The apprenticeship program was created in Law n. 10.097 of 19 December 2000. Before this apprenticeship law was enacted, apprentice eligibility status was unclear. Indeed, the take-up was extremely low. As discussed in Corseuil et al. (2012) the apprenticeship program integrates the Brazilian labor legislation code (*Consolidação das Leis Trabalhistas*), in place since 1943, but it had a very limited scope. Official census statistics from the formal sector show that in 1998 and 1999 there were only 215 and 82 14-year-old apprentices in Brazil, respectively. If the apprenticeship program had remained an alternative for youth entering the formal labor force at age 14 in 1999, it should have had a common effect on the affected and unaffected cohorts used in our analyses. It is also important to clarify that the law of 2000 is completely independent from the pension system reform of 1998.

[15] This exercise compares children who turned 14 between June 25, 1998, and December 15, 1998, with the ones who turned 14 between December 16, 1998, and June 14, 1999. In the first group, 1/30 were working in the formal sector. The ratio drops to 1/100 among the second group.

labor justice officials, and/or employers. We then use the fuzziness generated by the ambiguity in the law's interpretation to identify its effects, comparing the outcomes of these two cohorts.

The real motivation for raising the minimum employment age is not spelled out in the law, but the two main reasons seem to have been: one, to postpone the age of retirement under the scheme based on time of contribution to the pension system, and two, to acknowledge the fact that Brazil was in the process of ratifying Convention No. 138 of the International Labor Organization which required a minimum working age of 15.[16] Therefore, the country agreed to set the minimum employment age above the usual lower secondary-leaving age, which was 14 for those who had not experienced delay in schooling by the time the law passed.[17]

The approved legislation does not include penalties for those who employ children below the minimum age. However, according to a recent report commissioned by the Brazilian Public Prosecutor's Office, the institution responsible for monitoring child labor in Brazil, employers (including parents if the child works for a family business) can face several forms of penalties, ranging from fines and other administrative costs, to criminal prosecution depending on the type of work performed. Parents can even lose the guardianship of their children. Note that the severity of punishment for employers is greater for hiring a child worker than hiring an adult informal worker. Children are not subject to any sort of penalty in either case, as the goal of the law is to protect them (see Medeiros Neto and Marques (2013)).

One might question the enforceability of such law in a country where informal work is widespread. In the formal sector, the enforceability of the law is almost deterministic – though imperfect as suggested by official statistics – given that the Ministry of Labor is the institution responsible for issuing work permits. With the change in the law, the Ministry should not have issued work permits to individuals who turned 14 after the ban.[18] However, since only 1.5 percent of 14-year-olds were working in the formal sector in 1999, we expected the effect of the ban to be very small among this group.[19]

[16]In Brazil, there are two retirement mechanisms: an age cut-off and the amount of time one has contributed to the pension system. Because many start working early in life, they end up retiring relatively early. With the increase in the minimum employment age, people had to postpone their entrance into the formal labor force by two years. Consequently, they would retire two years later.

[17]At that time, primary and lower secondary education was mandatory in Brazil. According to the Constitutional Amendment 14, in 1996, the state was required to provide public and mandatory first to ninth grades, including for those who did not attend school at the correct age for the grade.

[18]Except for apprenticeship contracts, allowed by the law. These employees have the labor contract recorded on the worker register card.

[19]PNAD, 1999.

The results found by Piza and Souza (2016), pointing to a decrease in informal paid work among boys in urban areas, suggest that some employers decided to stop employing children under the age of 16 to avoid legal consequences. The interpretation that the ban reduced labor demand is consistent with Basu (2005) theoretical predictions.[20] This behavioral response of employers suggests that the cost of verifying child labor practices in firms is lower than the cost of verifying any other type of informal labor contract.

The Constitution of 1988, the *Statute of Children and Adolescents* from 1990, and the *Guidelines for National Education* from 1996 are the main legal frameworks that establish the educational parameters to be followed for the cohorts we analyze in this paper.[21] By 1999, according to these laws, primary and lower secondary education (first to ninth grade) were mandatory, and parents or guardians were obliged to enroll children in school at age seven.[22] With no grade repetitions, children would finish their mandatory schooling at age 14. However, in case of educational delays, children would need to stay in school until lower secondary education is finished, and therefore, some students might be older than 14 at the time of graduation.

These compulsory schooling laws could be seen as a confounding factor for our identification strategy. The birthdate cut-off adopted by the school system to determine that a seven-year-old child can enroll in school in a given year can create a discontinuity around the cut-off used to identify the effects of the child labor ban. If school enrollment and attendance are no longer mandatory for the children who turned 14 before the increase in the minimum working age, but remain mandatory for those who turned 14 after, the discontinuity observed around the cut-off could not be fully attributable to the child labor ban. This would not invalidate the exogeneity of the discontinuity, but the results could not be interpreted as being exclusively a consequence of the child labor ban.

As we argue in the results section, we are confident that the discontinuity we observe is due to the child labor ban. First, the school system in Brazil is highly decentralized, and each local

[20] Bharadwaj et al. (2020) interpret the increase in the minimum legal age in India along the same lines. We thank an anonymous referee for pointing out this commonality.

[21] *Statute of Children and Adolescents* is the law n. 8069 from July 13, 1990 (*Lei do Estatuto e do Adolescente*). *Guidelines for the National Education* is the law n. 9394 from December 20, 1996 (*Lei das Diretrizes e Bases da Educação Nacional*).

[22] Later, there were modifications of these parameters. Laws 11.114/2005 and 11.274/2006 mandated nine years of primary and lower secondary education (first to ninth grades) starting at age six. Constitutional Amendment No. 59 of 2009 established mandatory primary and secondary schooling (first to ninth grades and high school) from age 4 to 17.

district uses different cut-off birth dates to allow the enrollment of students in the first grade. Second, students must stay in school until they complete lower secondary education. Since delays were pervasive at the time the law change, only 3.3 percent of the of 14-year-olds were no longer obliged to stay in school.[23] Finally, a series of placebo tests using affected and unaffected cohorts do not reveal discontinuities in the outcomes of interest.

4 Empirical Strategy

Our identification strategy relies on the children's dates of birth, since the change in the minimum legal working age on December 15, 1998, affected those who turned 14 after this date. Unlike Angrist and Krueger (1991) and many other authors who combine date of birth with school entry or exit ages, parents could not have anticipated this change or its effects.[24] Since the law affected those children who had their 14th birthday just prior to the cutoff differently than those whose 14th birthday fell just after it, the regression discontinuity approach is the most appropriate for our analysis.

The cutoff, $\bar{Z}$, is set at December 16, 1984, that is, fourteen years before the law started being applied. The running variable, Z_i, is the number of weeks between the cutoff and date of birth of child i. Therefore, $Z_i = 0$ if child i was born on the cutoff date or up to one week after that. $Z_i = 1$ if child i was born between one and two weeks after $\bar{Z}$, $Z_i = 2$ between two and three weeks after $\bar{Z}$, and so on. Analogously, $Z_i = -1$, if child i was born up to one week before $\bar{Z}$, $Z_i = -2$ for between one and two weeks before, and so on. The affected cohort, $D_i = 1$, is set when $Z_i \geq 0$, and the unaffected cohort, $D_i = 0$, when $Z_i < 0$.

We use two inference approaches of regression discontinuity design (RDD) to examine the effect of the ban on the outcomes of those the law impacted. The first utilizes a continuity-based approach and the second relies on the idea of local randomization, in which the treatment assignment is regarded as a known randomization mechanism near the threshold (Cattaneo et al. (2016)).

[23] PNAD, 1999.

[24] For similar identification strategies see Smith (2009); McCrary and Royer (2011); Black et al. (2011), Oreopoulos (2006); Dickens et al. (2014); Lavy (2015a); Lavy (2015b).

4.1 Continuity-Based Approach

In the first approach, we run a RDD on the 1999 PNAD wave and, as PNAD is a sample survey, we also pooled 1999 and 2001 waves to gain sample size and precision. Since in 2001, the affected cohort had reached 16 years old and were therefore allowed to have a full-time job, the pooled results might be interpreted as a lower bound of the estimates. We run the following equation:

$$y_{it} = \alpha + \gamma D_i + h(Z_i) + \beta X_{it} + \theta_t + \upsilon_{it} \tag{1}$$

In which, y_{it} is the outcome of interest of child i of the household survey in year t; D_{ic} is an indicator function that assumes the value of 1 if $Z_i \geq 0$; $h(Z_i)$ is a polynomial function of the running variable Z_i, X_{it} are the control variables that include the age, gender and education of the household head, household size, dummy variables identifying urban areas and the Brazilian regions of residence, and a dummy variable if child's i is white; θ_t are year fixed effects; and υ_{it} is the idiosyncratic error for children i in year t. This approach relies on extrapolation and large-sample approximations of the conditional expectation using observations near the cutoff.

4.2 Local Randomization Mechanism

In the second approach, we run an RDD that relies on the idea of local randomization, in which the treatment assignment may be regarded as a known randomization mechanism near the cutoff (Cattaneo et al. (2016)). In our case, we assume that turning age 14 just before or after the law change is random. Moreover, different from the continuity-based approach, the local randomization procedure does not rely on infinite extrapolation assumptions. Instead, it relies on the finite-sample exact inference method. This is more appropriate when the dataset is small, as it is in our case. The design provides a local treatment effect estimate.

To define the window around the cutoff where the local randomization is assumed to hold, we follow the data-driven method proposed by Cattaneo et al. (2016). Since the methodology assumes the treatment is random inside a window around the cutoff, $W_0 = [\bar{Z} - w, \bar{Z} + w]$, $w >$

0, the distribution of pre-intervention characteristics or post-intervention unaffected outcomes should be the same, on average, between the affected and unaffected cohorts inside W_0. On the other hand, for the procedure to be useful, the distribution of these covariates should be unbalanced outside the optimal window.[25]

To understand this in our context, consider two ($k = 2$) covariates that are unaffected by the treatment: mother's years of schooling (x_{1it}) and household size (x_{2it}) in year $t = 1999$, for which we run the following regression equation:

$$x_{kit} = \alpha + \beta D_i + \upsilon_i, \quad k = 1, 2 \;\; t = 1999 \tag{2}$$

The window-selection algorithm consists of finding the largest window (W_0) in which the p-value for the null hypothesis of no effect of the treatment ($H_0 : \beta = 0$) is always larger than some pre-specified level, for example, 0.15. Therefore, inside the determined window, we should not observe a p-value lower than 0.15. If we do, it means the window size is too wide. A smaller window size is then proposed and the balance test rerun. The simulation stops only when one cannot observe any p-value in the balance test exercise below 0.15 inside a given window. Performing this data-driven process on our sample resulted in a 14-week bandwidth around the cutoff. Given that a 14-week window size might be considered relatively large in a framework reliant on a local randomization design, we opted to be conservative and focus on the results for a 10-week bandwidth.

Hence, the analysis of the short- and long-term effects of the law on individual outcomes consists of comparing the cohorts who turned 14 years old up to ten weeks before the law changed (born between October 6, 1984, and December 15, 1984) with those who turned 14 up to ten weeks after the law changed (born between December 16, 1984, and February 23, 1985). If the increase in the minimum employment age led to a change in labor-force participation and employment, then the outcomes of the unaffected cohort would inform what would have happened to those hindered from working had the law not changed.

To assess the short-term effects of the law and its persistence over time, we follow the affected and unaffected cohorts from age 14 to age 21. We use labor force participation, the incidence of

[25] We run a test of difference in means and the p-values are calculated according to Fisherian inference as in Cattaneo et al. (2016).

children working for pay (formal and informal), the incidence of unpaid work, the incidence of children attending school, and the incidence of children neither working nor attending school. Therefore, we aim to examine whether the law significantly reduced the share of children working for pay and whether those prevented from working opted to postpone their entrance into the labor market, continue looking for a job (still in the work force, but unemployed), were more likely to continue going to school, or ended up increasing the share of those neither working nor attending school. By looking at the 2001 data, we can check if the law had an impact on the affected cohort's likelihood of being employed after reaching the minimum legal working age of 16.

To investigate the long-term effects of the ban, we follow the affected and unaffected cohorts from ages 22 to 29. The outcomes of interest are the likelihood of having completed secondary school, being employed, being employed in the formal sector, and the log of monthly earnings. In theory, the long-term effects of the ban could go in one of two directions: More time for school might result in better job prospects, school attainment, and wages. However, the poor quality of Brazilian public education could attenuate these effects. On the other hand, working as a child might provide valuable vocational training and on-the-job experience and limiting this could have negative consequences for job prospects and wages when the affected children are older. Thus the net effect is an empirical issue.

5 Data

We use several years of the Brazilian household survey (PNAD), which was conducted annually from the late 1970s to 2015 by the Brazilian Institute of Geography and Statistics (IBGE). The year 1998 is used for placebo tests and for data on child labor characteristics before the change in the minimum legal working age. The year 1999 is used for descriptive statistics for affected and unaffected cohorts and short-term estimates. We then use PNADs from 2001 to 2006 to assess the persistence of short-term effects, and those from 2007 to 2014 to assess the long-term effects.

Generally conducted in the last week of September, the PNAD interviews about 380,000 individuals in around 100,000 households.[26] The survey is nationally representative and constitutes one of the main sources of microdata in Brazil. It contains information on household socioeconomic characteristics, demographic data, educational attainment, household sources of income, and labor force status.

Our sample consists of two cohorts of individuals who were 13 or 14 years old by the time of the increase in the minimum legal working age. The first cohort, which we consider the comparison group, includes individuals who turned 14 before December 16, 1998, the first day the law was applied. The second cohort, defined as the affected group, consists of individuals who turned 14 after that. In the continuity based approach, our baseline specification has a 9-month bandwidth (Table A.1). For the local randomization approach, our baseline is a 10-week bandwidth.

From the PNAD, we create nine variables of interest.[27] *Economically active* assumes value 1 if the person is employed or unemployed and 0 if the person is out of the labor market.[28] *Unpaid work* assumes value 1 for those employed but without monetary compensation and 0, otherwise.[29] *Paid work* assumes value 1 for those employed for a wage and 0 otherwise. The paid work category is also divided between those working in the formal sector, *Formal paid work*, and the informal sector, *Informal paid work*.[30]. *Attending school* assumes values 1 for children who attend school and 0 otherwise. The last three variables are categories for those who are working for pay and not attending school, *Only paid work*, those not working but attending school, *Only attending school*, and those who neither work nor attend school, *Neither working nor attending school*.

[26] Average between 1998 and 2014.

[27] See the code in https://github.com/worldbank/child-labor-ban-brazil.

[28] The Brazilian Institute of Geography and Statistics (IBGE) defines as employed people that worked in the reference week of the survey (if V9001 = 1), that worked for self-consumption (if V9002 = 2) or self-production (V9003 = 1), or that had a paid job but were on leave in the reference week (V9004 = 2). Therefore, the variable employed is equal to 1 if V9001 = 1 or V9003 == 1 or V9002 = 2 or V9004 == 2 and 0 if the person was not working but did look for a job in the reference week of the survey (V9115 = 1). The unemployed are those who were not working in the reference week but did look for a job (if V4705 = 2). Therefore, unemployed is equal to 1 if employed is equal to 0, and is equal to 0 if employed is equal to 1. In the 1999 PNAD wave, the IBGE set up the variable V4704 as equal to 1 for those economically active and equal to 2 for those out of the labor market. All the variables mentioned are in the 1999 PNAD wave.

[29] Unpaid work assumes value 1 if the person works for self-consumption (V4706 = 11), for self-production (V4706 = 12), or those who work without a monetary compensation (V4706 = 13) and 0 otherwise

[30] The employees are considered formal if their employer signed their labor card (V4706 = 1 or V4706 = 6), which is the document that guarantees the labor rights established by the Brazilian Legislation. Civil servants (V4706 = 2 or V4706 = 3) are also considered formal employees as well as those people that, even though do not have a signed labor card, pay pension contributions to the National Institute of Social Security (*Instituto Nacional de Seguridade Social*, INSS (V4711 = 1). Therefore, formal paid work assumes value 1 for those employed with a signed labor card, for civil servants, and for employees that contribute to pensions and 0 otherwise. Informal paid work assumes value 1 when formal paid work is 0 and 0 when formal paid work is 1.

5.1 Descriptive Statistics

We begin by introducing descriptive statistics for child labor in Brazil in 1998, immediately before the increase in the minimum legal working age. We distinguish between urban and rural areas, and between boys and girls, due to significant differences in labor force participation and type of work performed.

In 1998, 26.7 percent of 14-year-olds were economically active. Their unemployment rate was 18 percent, more than twice as high as of 18- to 65-year-olds. 90 percent of these children were enrolled in school, with almost 97 percent in lower primary or upper primary education, meaning they still hadn't completed the mandatory educational level.[31] We call Among the children working, 81.4 percent combine work and school and 56 percent were in unpaid activities.[32]

In households with 14-year-old members, on average, 5 percent of the total income came from wages of individuals up to 17 years old. In 10 percent of these households, more than 20 percent of the income came from children's wages.[33] This might indicate that the effects of the ban might vary according to the socioeconomic level of the families, as some households rely more on these resources.

A quarter of 14-year-olds were living in rural areas, where almost half of them were working. Among the ones working, 80 percent were in unpaid activities, working an average of 24 hours per week. 93 percent were in the agriculture sector, and almost all worked for the household they lived in (98 percent) (Table A.2). Therefore, we do not expect the ban to have a significant impact among these children.

In urban areas, 14 percent of 14-year-olds were working in 1998, with boys twice as likely to work as girls. When working, three quarters of girls were engaged in paid activities, for an average of 35 hours per week. More than half of them worked as housekeepers, the vast majority in their employer's household, where labor law enforcement is limited or nonexistent. None of these

[31] In Brazil the education system is divided into the following categories: Pré-escola (4- to 5-years-old), Fundamental I (6- to 10-years-old) which we call lower primary, Fundamental II (11- to 14-years-old) which we call upper primary, and Ensino Médio (15- to 17-years-old) which we call secondary. Schooling is mandatory through Fundamental II.

[32] 1998 PNAD wave. Check our replication package to reproduce the descriptive statistics: https://github.com/worldbank/child-labor-ban-brazil.

[33] 1998 PNAD wave.

girls worked in the formal sector. Among boys working, two thirds were in paid jobs in factories or offices, where inspection might occur more frequently, although only 1 percent were formal sector jobs (Table A.3).

Between 1999 and 2005, we observe a steady increase in the percentage of individuals working for pay. However, there is an increasing gap between those unaffected by the ban and the affected cohort. In 1998, before the law changed, around 6 percent of both cohorts were working for pay. In 1999, the gap increased by 0.9 percentage points, then by 2 percentage points in 2001 and 6.4 percentage points in 2002. The gap starts decreasing in 2003, by age 18, and falls to 0.2 percentage points in 2006, when the affected cohort turned 21 (Figure A.2).[34]

The increasing gap in working for pay was accompanied by a gap in labor force participation, which might indicate that the affected cohort postponed their entrance into labor market. In addition, although we see a significant decrease in school attendance for both cohorts, more than one third were still in school by age 19. Since by this age, most of them were should have graduated from high school, this suggests a significant school delay. In fact, 12.5 percent of them were still enrolled in lower secondary education, around 40 percent in high school, and 29.6 percent were in college.[35]

5.2 Visual Check

To determine whether the increase in the minimum legal working age affected school attendance, children's employment and labor force participation, we compare the affected and unaffected cohorts using the 1999 PNAD. There is evidence that the law was enforced to some extent. For the 12-week bandwidth the percentage of 14-year-olds in paid activities is 3 percentage points smaller among the affected cohort (Figure A.1).[36] One may wonder whether this cohort, although legally prevented from working, continued looking for a job. That does not seem to be the case, as the percentage of economically active children is 3.4 percentage points smaller for the affected cohort (Figure A.1.).

We further inspect this relationship using local linear regressions. We run non-parametric

[34] Considering a 10-week bandwidth around the cutoff data. 1998-2014 PNAD waves.
[35] 2.4 percent were enrolled in primary education. 2004 PNAD wave.
[36] Figure A.1) shows the percentages for the 10-week bandwidth as well.

regressions on each side of the cutoff point for the following outcomes: paid work, formal paid work, informal paid work, paid work and school attendance, and only school attendance. These five outcomes are the main ones in which we expect short-term impacts of the ban.

We estimate the local linear regressions using triangle kernel with a 12-week bandwidth, and 4 weeks bin-size. Figure 1 suggests that the decrease in the proportion of children working for pay was mainly driven by those combining paid work with schooling. Overall, the visual analyses suggest some children traded work experience for education, while others decided to keep working in the informal sector. It is difficult to draw welfare conclusions about the effect of this ban on the affected cohort, but looking at medium and long-term effects may help us draw conclusions about whether this was a successful active labor policy. We discuss the policy implications of this law change later in the paper.

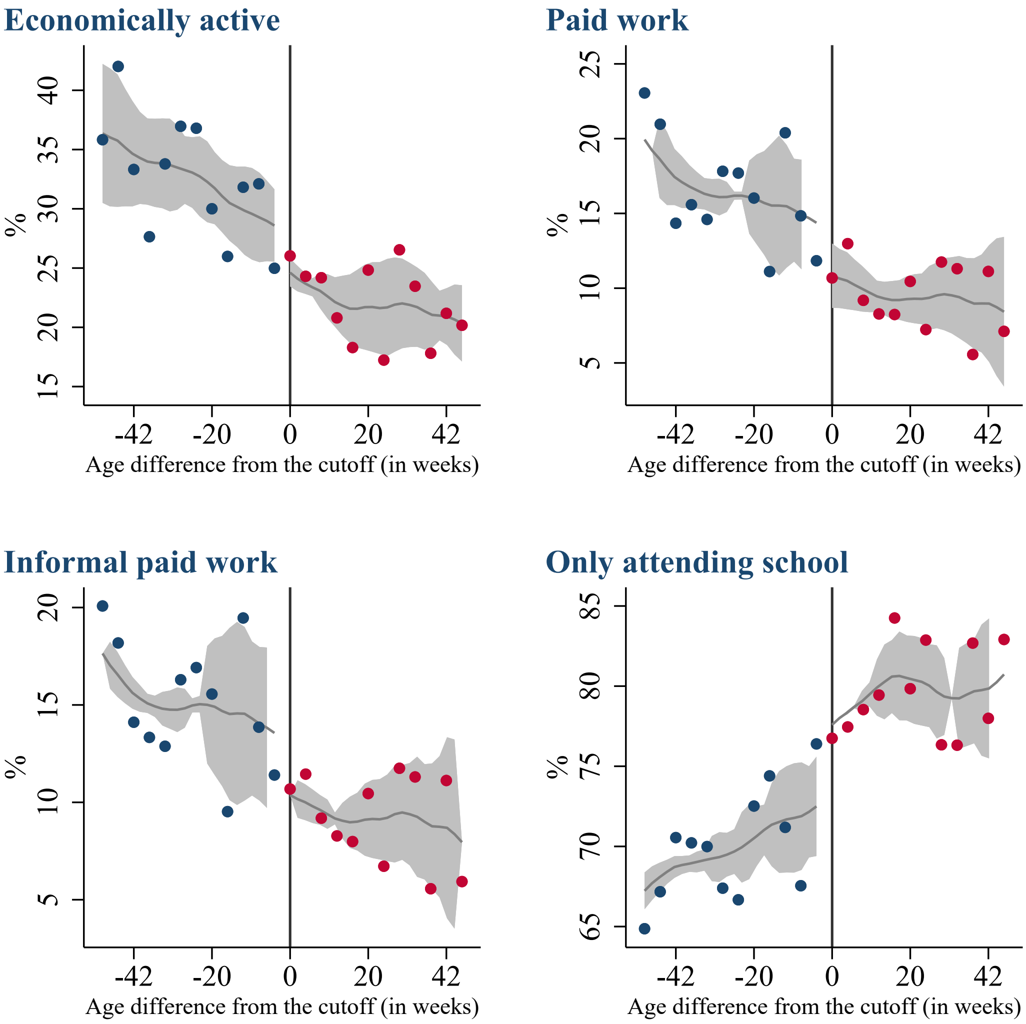
5.3 The Plausibility of the Identification Assumption

The identification strategy used in this paper assumes that turning 14 by the time of the increase in the minimum working age can be defined as a known randomization mechanism around the cutoff. Although our birth data comes from household surveys conducted by the Brazilian Census Bureau, which is not related to the public institutions responsible for the surveillance and enforcement of the law, there is the possibility that families misreport dates of birth of their children, particularly in cases where they are working illegally. If this occurs systematically, one would observe a discontinuity in the density function of the forcing variable around the cutoff point, which would call into question the plausibility of our identification strategy.

We do not think that manipulation is an issue of concern in our setting, because Brazil's Ministry of Labor, responsible for issuing working permits, requires an individual's birth certificate or other official identification. Even so, we perform a McCrary density test to investigate whether there is indication of manipulation. The test consists of comparing the density distributions of the forcing variable around the cutoff point (McCrary (2008)).[37] A rejection of the null hypothesis would indicate perfect manipulation of the forcing variable. Figure A.4 illustrates

[37] Because household heads or responsible adults report the ages of household members to the surveyor, misreporting or manipulation is possible. It is important to emphasize that RDD accommodates some degree of sorting or manipulation of the forcing variable. To invalidate the identification strategy, a perfect manipulation of the forcing variable should be observed. See Lee and Lemieux (2010) for a discussion.

Figure 1: Visual Check, Urban Boys (1999)



Note: Authors' estimate based on 1999 PNAD wave. The figures show local linear regressions fitted each side of the cutoff point. We use a 4-week bin size and a triangle kernel with 12-month bandwidth on the sample of 14-year-old urban boys. 1999 PNAD Wave.

the results graphically and indicates that there was no perfect manipulation of the forcing variable.

The validity of our identification strategy also requires a smooth distribution of observed characteristics of 14-year-olds around the cutoff. Under the assumption that the law gave rise to a natural experiment, we should observe affected and unaffected cohorts with similar observed characteristics, on average. For balance in observed characteristics, we use covariates usually employed in labor supply estimates such as parents' age and education, household size, color of the skin and urban or rural areas, and check whether they are smoothly distributed around the cutoff point. We find no systematic differences in the mean values of these variables between unaffected and affected cohorts (Table A.1). This result supports the assumption that the assignment to treatment was locally random.

6 Short-Term Effects of the Ban

To assess the effects of the increase in the minimum legal working age from 14 to 16 years old, we first estimate the traditional continuity-based RDD model. We do not find any overall impact among all affected children in Brazil.[38] Therefore we will focus on the results for the group in which we do find a significant effect of the ban: boys in urban areas. This group represents half of all of the children in paid jobs in Brazil in 1999, and more than one-quarter of 14-year-old urban boys were in the economically active population, the ones most likely to be impacted by the ban.

Table 1 shows the estimates for the sample of boys in urban areas using 4-, 6-, and 9-month bandwidths. We estimate our model using both the 1999 PNAD wave and also on the pooled 1999 and 2001 waves to increase the number of observations and gain precision. The results from the pooled sample can be interpreted as a lower bound since in 2001 the affected cohort was already 16 years old and allowed to have a full-time job. Using the 1999 PNAD wave, we find evidence of a significant decrease in paid work and in informal paid work only under the 9-month bandwidth. On the other hand, for the pooled sample, significant results occur in and are consistent across all three bandwidths. The estimates suggest that the change in minimum working age led to a decrease in paid work of at least 3.5 percentage points among the affected urban boys impacted by the ban, a drop of almost 25 percent.[39] Also, the decrease of informal paid labor seems to be driving this result.

[38]See Tables A.7, A.8 A.9, A.10 and A.11 for estimates for all 14-year-olds (boys and girls, in urban and rural areas).

[39]Considering a 4-month bandwidth, among those not affected by the ban, 14.7 percent of 14-year-old males in urban areas had paid jobs. Therefore, a 3.5 pp reduction is equivalent to a 23.8 percent decrease.

Table 1: Continuity Based Approach, boys in urban areas

1999

	4-month bandwidth			6-month bandwidth			9-month bandwidth		
	Liner	Quadratic	Piecewise	Liner	Quadratic	Piecewise	Liner	Quadratic	Piecewise
Economically Active	-1.72	-1.79	-1.93	-0.77	-0.76	-0.75	-4.28	-4.30	-4.27
	(6.64)	(6.80)	(6.95)	(4.77)	(4.76)	(4.79)	(3.69)	(3.67)	(3.68)
Paid Work	-2.32	-2.37	-2.43	-1.98	-1.98	-1.96	-4.05*	-4.06*	-4.04*
	(3.80)	(3.99)	(3.97)	(2.94)	(2.93)	(2.93)	(2.27)	(2.26)	(2.26)
Unpaid Work	-0.84	-0.84	-0.87	0.96	0.95	0.91	0.10	0.10	0.10
	(2.39)	(2.41)	(2.48)	(1.96)	(1.95)	(1.97)	(1.59)	(1.59)	(1.59)
Formal Paid Work	0.36	0.36	0.39	-0.30	-0.30	-0.30	0.14	0.13	0.14
	(0.55)	(0.53)	(0.53)	(0.55)	(0.55)	(0.54)	(0.51)	(0.50)	(0.51)
Informal Paid Work	-2.68	-2.74	-2.82	-1.68	-1.68	-1.67	-4.18*	-4.19*	-4.18*
	(3.82)	(4.02)	(4.03)	(2.88)	(2.87)	(2.88)	(2.24)	(2.23)	(2.23)
Attending School	2.56	2.54	2.45	3.01	3.01	3.00	2.67	2.69	2.66
	(3.65)	(3.56)	(3.45)	(2.74)	(2.75)	(2.75)	(2.15)	(2.13)	(2.12)
Only Paid Work	-1.89	-1.91	-1.90	-2.20	-2.20	-2.20	-1.58	-1.57	-1.58
	(1.97)	(2.06)	(2.05)	(1.56)	(1.57)	(1.58)	(1.30)	(1.29)	(1.30)
Only Attending School	4.11	4.15	4.18	2.35	2.35	2.38	5.77	5.80	5.76
	(6.59)	(6.74)	(6.74)	(4.75)	(4.77)	(4.80)	(3.56)	(3.54)	(3.53)
Neither Work Nor School	-0.95	-0.93	-0.88	-1.33	-1.33	-1.33	-1.83	-1.84	-1.82
	(2.38)	(2.31)	(2.25)	(1.91)	(1.91)	(1.90)	(1.43)	(1.42)	(1.42)

Pooling 1999 and 2001

	4-month bandwidth			6-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
Economically Active	-1.88	-1.85	-1.82	-1.96	-1.84	-1.76	-3.75*	-3.78*	-3.76*
	(3.16)	(3.19)	(3.24)	(2.30)	(2.26)	(2.28)	(1.92)	(1.94)	(1.95)
Paid Work	-4.15**	-4.10**	-4.11**	-3.52**	-3.46**	-3.41**	-3.64**	-3.69**	-3.70**
	(1.81)	(1.78)	(1.77)	(1.55)	(1.53)	(1.53)	(1.46)	(1.46)	(1.47)
Unpaid Work	-0.50	-0.49	-0.43	0.35	0.35	0.36	0.07	0.08	0.10
	(1.54)	(1.58)	(1.58)	(1.25)	(1.27)	(1.27)	(1.03)	(1.02)	(1.02)
Formal Paid Work	0.16	0.22	0.27	-0.04	-0.05	-0.03	0.55	0.52	0.50
	(0.97)	(0.98)	(1.01)	(0.80)	(0.80)	(0.80)	(0.67)	(0.68)	(0.68)
Informal Paid Work	-4.31**	-4.33**	-4.37**	-3.47**	-3.41**	-3.38**	-4.19***	-4.21***	-4.20***
	(2.00)	(1.99)	(2.00)	(1.58)	(1.57)	(1.57)	(1.41)	(1.42)	(1.42)
Attending School	-2.77	-2.77	-2.74	-1.98	-2.00	-2.01	-1.28	-1.32	-1.35
	(2.33)	(2.33)	(2.35)	(1.87)	(1.88)	(1.89)	(1.63)	(1.63)	(1.64)
Only Paid Work	-0.01	0.02	0.03	0.00	0.03	0.07	-0.52	-0.52	-0.50
	(1.21)	(1.21)	(1.23)	(1.12)	(1.12)	(1.13)	(1.02)	(1.03)	(1.03)
Only Attending School	1.87	1.88	1.91	1.33	1.32	1.31	2.11	2.12	2.11
	(2.79)	(2.80)	(2.85)	(2.12)	(2.15)	(2.17)	(1.86)	(1.86)	(1.86)
Neither Work Nor School	2.78	2.71	2.62	1.84	1.80	1.74	1.46	1.48	1.49
	(1.88)	(1.85)	(1.85)	(1.51)	(1.48)	(1.47)	(1.16)	(1.17)	(1.17)

Notes: Data from 1999 and 2001 PNAD. Regression Discontinuity Design (equation 1) in which the running variable is the number of weeks between the date of birth and the cutoff (December 16, 1984). The the linear specification, $h(Z_i)$ is equal to z_i. In the quadratic one, $h(Z_i)$ is equal to $z_i + z_i^2$. Finally, in the piecewise, $h(Z_i)$ is equal to $z_i + z_i \times D_i$, in which D_i equal to 1 for the affected cohort and 0, otherwise. Standard errors clustered at the running variable. Controls: age, education and gender of the head of the household, Brazilian region of residence, urban areas, household size, and children's skin color.

When working with only one wave of cross-sectional data, due to the relatively small size of the affected cohort, we needed a relatively wide bandwidth to be able to reject the null hypothesis of no effects. When we pool the two years of the Brazilian PNADs, we have enough statistical power to detect effects in a much narrower window. The point estimates for paid work and informal paid work are very similar and stable across bandwidth sizes.

We also test the effects of the ban using a local randomization approach, which is ideal in our setting as inference can be carried out with small samples around the threshold and is fully non-parametric. Table 2 shows the results with three bandwidth sizes for urban boys in the 1999 PNAD: 10, 12, and 14 weeks. Immediately below the 1999 rows, we present the placebo results, where we use the same cohort of urban boys but in the 1998 PNAD when they were 13 years old. As the ban had not been implemented when the cohort was 13, we do not expect to see significant differences in outcomes due to the law.[40]

The results suggest that the child labor ban led to affected children postponing their entrance into the labor market. Indeed, we estimate a drop of more than 20 percent among those economically active. Consistent with the results in Table 1, we estimate a decrease in paid work of at least 35 percent. The decrease in informal paid work seems to be the largest component of the overall effect with a reduction of 32 percent. Together, these results suggest that the affected cohort worked for pay less and did not migrate from paid work to unemployment. The estimates also show an increase in school attendance of 4 percent, and the share of those only attending school jumped 11 percent, indicating that the affected group of boys opted to return to school or continue attending school, instead of opting to neither work nor go to school.

The placebo results are expected: there are no significant differences between the groups before the increase of the minimum employment age for any of the outcomes of interest and across all of the bandwidths which is strong evidence that the effects we estimate using the 1999 PNAD are due to the change in minimum working age.

[40]We did not run local randomization comparing the same-age cohort in 1998 (those who were born in a 14-week window around a cutoff defined on December 16, 1983). Figure A.6 shows that there is not a window where local randomization holds for 14-years-olds in the 1998 PNAD wave.

Table 2: Local Randomization, boys in urban areas

	10 weeks				12 weeks				14 weeks			
	ATE	95% CI	Mean outcome	as %	ATE	95% CI	Mean outcome	as %	ATE	95% CI	Mean outcome	as %
Economically active												
1999	-6.66**	[-11.0,-1.5]	28.55	-23.35	-7.22***	[-11.,-2.7]	29.67	-24.34	-7.82***	[-12.,-3.6]	29.97	-26.10
1998 (Placebo)	-3.99*	[-8.5,.37]	21.18	-18.85	-3.21	[-7.3,.92]	20.19		-2.84	[-6.5,1.0]	20.34	
Paid work												
1999	-5.23**	[-9.3,-1.1]	14.90	-35.10	-6.11***	[-9.2,-2.7]	15.48	-39.49	-6.68***	[-10.,-3.3]	15.70	-42.55
1998 (Placebo)	-1.82	[-4.8,1.4]	8.00		-1.90	[-4.6,.61]	7.77		-2.04	[-4.4,.65]	8.41	
Unpaid work												
1999	-.296	[-3.7,2.9]	7.60		.2336	[-2.7,3.0]	7.79		.5493	[-2.0,3.1]	7.29	
1998 (Placebo)	.0265	[-2.9,2.9]	6.40		.4045	[-2.4,3.3]	6.45		.4486	[-2.1,2.8]	6.11	
Formal paid work												
1999	-.541	[-1.4,.37]	0.61		-.446	[-1.2,.30]	0.50		-.771*	[-1.5,-1.1]	0.89	-86.58
1998 (Placebo)	-.360	[-1.1,.37]	0.56		-.302	[-.92,.30]	0.47		-.391	[-1.0,.26]	0.56	
Informal paid work												
1999	-4.68**	[-8.9,-.74]	14.29	-32.80	-5.66***	[-8.9,-2.4]	14.98	-37.84	-5.91***	[-9.0,-2.8]	14.81	-39.90
1998 (Placebo)	-1.46	[-4.4,1.4]	7.44		-1.59	[-4.2,.92]	7.31		-1.65	[-3.9,.91]	7.85	
Attending school												
1999	3.671**	[.37,7.1]	89.89	4.08	3.319**	[.30,6.1]	90.72	3.66	3.897***	[1.3,6.5]	90.46	4.31
1998 (Placebo)	-.009	[-2.9,2.7]	93.47		-.552	[-3.0,2.1]	93.62		-.441	[-2.8,1.9]	94.22	
Only paid work												
1999	-1.21	[-2.9,.74]	3.96		-1.30	[-3.0,.61]	3.71		-1.64**	[-3.1,-.26]	3.72	-44.37
1998 (Placebo)	-.338	[-1.4,.74]	1.21		-.139	[-1.2,.92]	1.01		-.124	[-1.0,.78]	0.87	
Only attending												
school	7.996***	[2.6,13.]	71.7	11.2	8.058***	[3.3,12.]	71.4	11.3	8.392***	[4.1,12.]	71.5	11.7
1999	1.659	[-2.9,6.3]	80.65		.8147	[-3.5,4.6]	81.09		.9062	[-2.8,4.9]	81.32	
1998 (Placebo)												
Neither working nor attending school												
1999	-2.46*	[-5.2,-7.3]	5.81	-42.50	-2.17	[-4.3,.30]	5.30		-2.25**	[-4.4,-.26]	5.49	-41.14
1998 (Placebo)	.1432	[-2.2,2.5]	4.95		.6818	[-1.5,3.0]	4.69		.6917	[-1.3,2.8]	4.17	

Notes: 1998 and 1999 PNAD waves. Local Randomization in which the running variable is the number of weeks between the date of birth and the cutoff (December 16, 1984). We employed the *stata* command *rdrandinf* proposed by Cattaneo et al. (2016). Constant effect model (polynomial of order 0). The 1999 rows are the main local randomization specification in which we compare the affected cohort (children that turned 14 years old up to 10, 12 or 14 weeks after December 16, 1998) and the unaffected one (those that turned 14 years old up to 10, 12 or 14 weeks before that). The 1998 rows are a placebo local randomization in which we compare the same cohort of children but in 1998 PNAD wave. That is, before the increase of the minimum employment age, when the cohorts were 13 years old.

The change in minimum working age does not appear to have had a significant impact on unpaid work. This is not surprising as 95 percent of 14-year-old boys in urban areas working in unpaid activities were either working for self-consumption or were a member of the household for which they work. Nor were effects found for children in rural settings or for girls in urban areas. For both groups, these results were expected. One year before the law changed, four out of five 14-year-olds working in rural areas were in unpaid activities, the vast majority for the household they lived in (98 percent). In urban areas, almost three-fourths of 14-year-old girls working were in paid jobs. More than half of them were housekeepers in the house of the employer, where enforcement of the law is much less likely or nonexistent (Table A.9).[41]

We next investigate the socioeconomic background of the children who were likely most affected by the ban. We aim to check whether the intervention had a greater impact on children from the wealthiest families, those who were more able to compensate for the reduction of the household income, or those with a lower socioeconomic status. Since mother's education is highly correlated with per capita household income, we perform the same analysis on the sample of children whose mother did not reach high school and on the sample of children whose mother did.[42] We find evidence that the decrease in the incidence of paid work was driven by children of lower socioeconomic status: a reduction of almost 30 percent in 1999. This is likely explained by children from lower socioeconomic backgrounds representing more than 80 percent of 14-year-olds in paid jobs in 1999 and, therefore, those needed more by their families to contribute to household income. In fact, almost 16 percent of them had paid jobs, more than double the children from wealthier families (Table A.13).

6.1 Robustness of the Cutoff

To ensure these results are robust to different windows around the cutoff, we run a robustness check, proposed by Cattaneo et al. (2016). We calculate the p-values to test several hypotheses over a range of window lengths. Figure 2 shows the average treatment effects under the null hypothesis on the y-axis and the number of weeks around the cutoff on the x-axis. Instead of

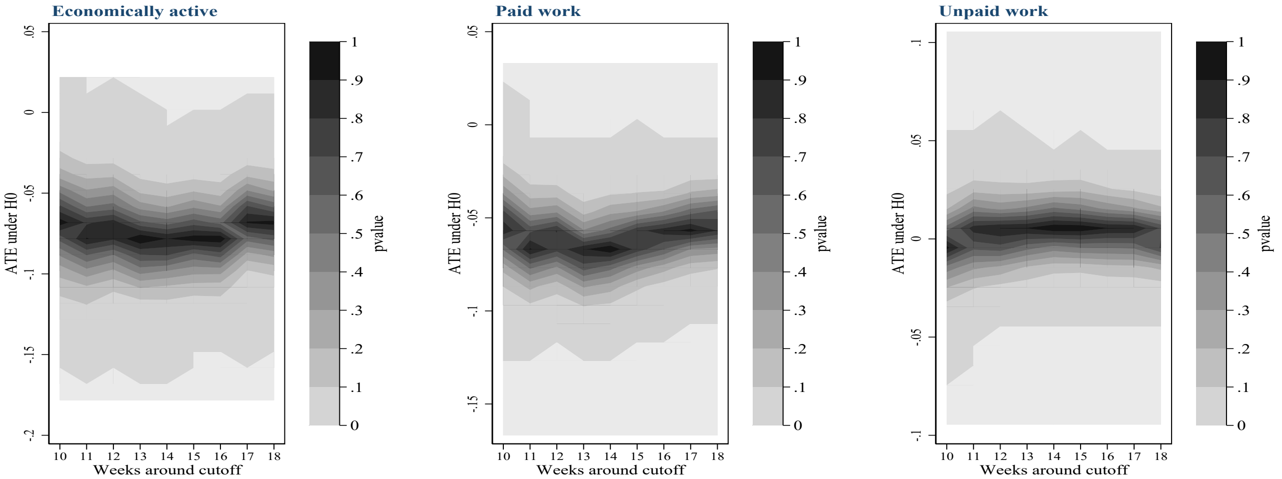
[41] 1998 PNAD wave. Check our replication package to reproduce the descriptive statistics: https://github.com/worldbank/child-labor-ban-brazil.

[42] According to 1999 PNAD, the correlation between mother's years of schooling and per capita household income for a household with 14-year-olds was 0.52. We did not disaggregate the analysis by adult's income as this variable is endogenous to the decision of the children to work.

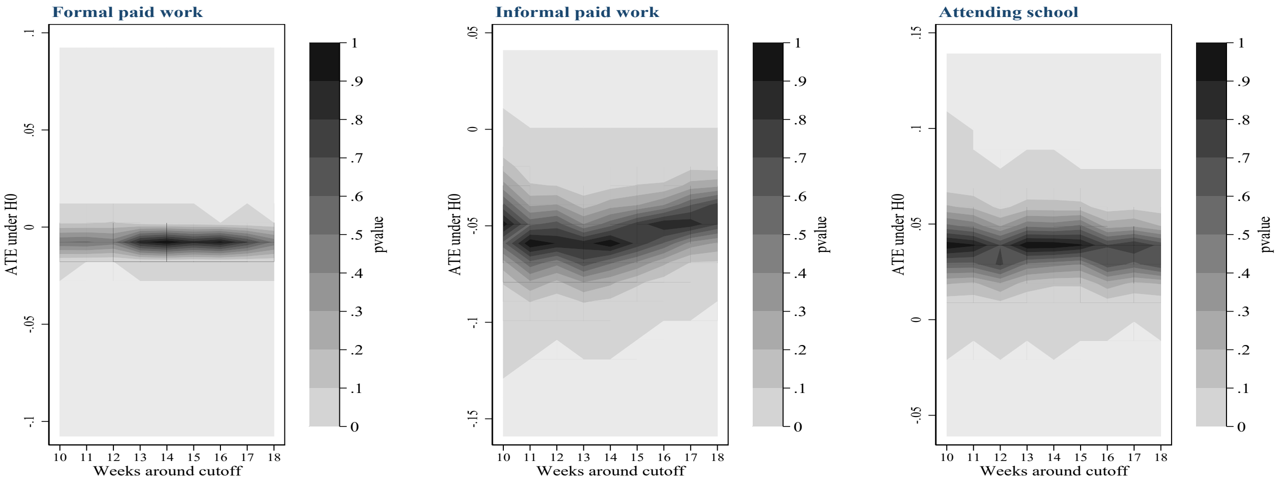
testing whether the H_0: ATE $= 0$ versus ATE $\neq 0$, we are testing H_0: ATE $= \gamma$, with $\gamma \neq 0$, versus H_1: ATE $\neq \gamma$. We find no evidence to reject the null hypothesis of the effects of the ban on economically active children, paid work, informal paid work, school attendance and for only attending school and neither working nor attending school considering a bandwidth range between 10 and 18 weeks.

Figure 2: Robustness check, 1999

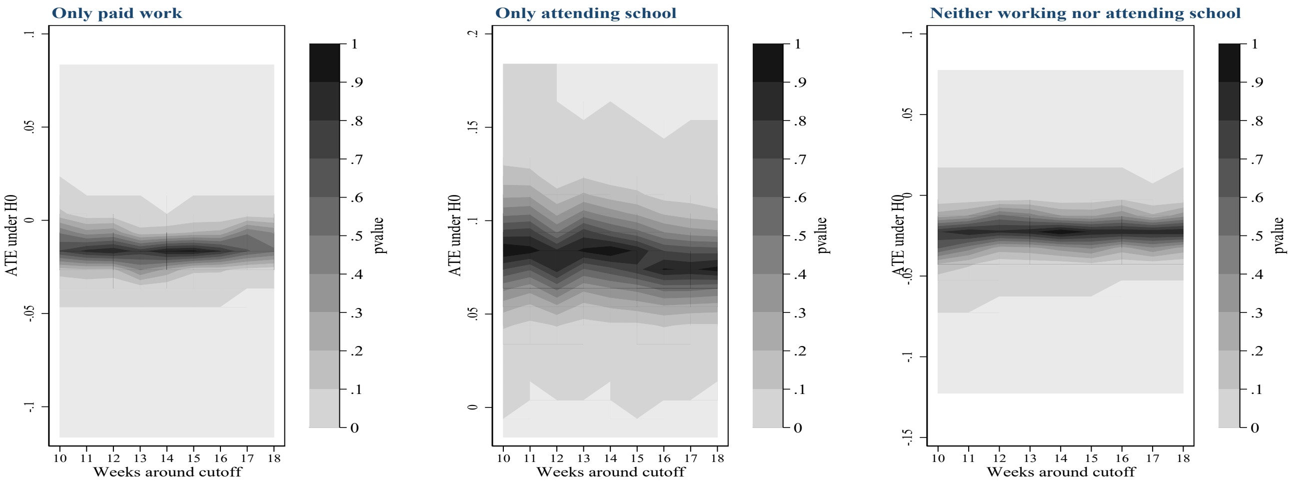
H



(a)



(b)



(c)

Note: Local Randomization in which the running variable is the number of weeks between the date of birth and the cutoff (December 16, 1984). We employed the *stata* command *rdsensitivity* proposed by Cattaneo et al. (2016). The command conducts a sequence of hypothesis tests for different windows around the RD cutoff. H_0: ATE $= \gamma$, with $\gamma \neq 0$. H_1: ATE $\neq \gamma$. Constant effect model (polynomial of order 0). We do not reject the null hypothesis for a range of 10 to 18 weeks.

6.2 Persistence of Short-Term Effects

To understand if the short-term effects are temporary or long-lasting, we explore their persistence by following the affected and unaffected cohorts from 1998, one year before the ban when they were 13 years old, to 2006, when they turned 21. We also employ the local randomization approach since it allows us to investigate this question without relying on parametric assumptions and extrapolations when using cross-sectional data. Figure 3 shows the point estimates with a conservative bandwidth size of 10 weeks and a 95 percent confidence interval.

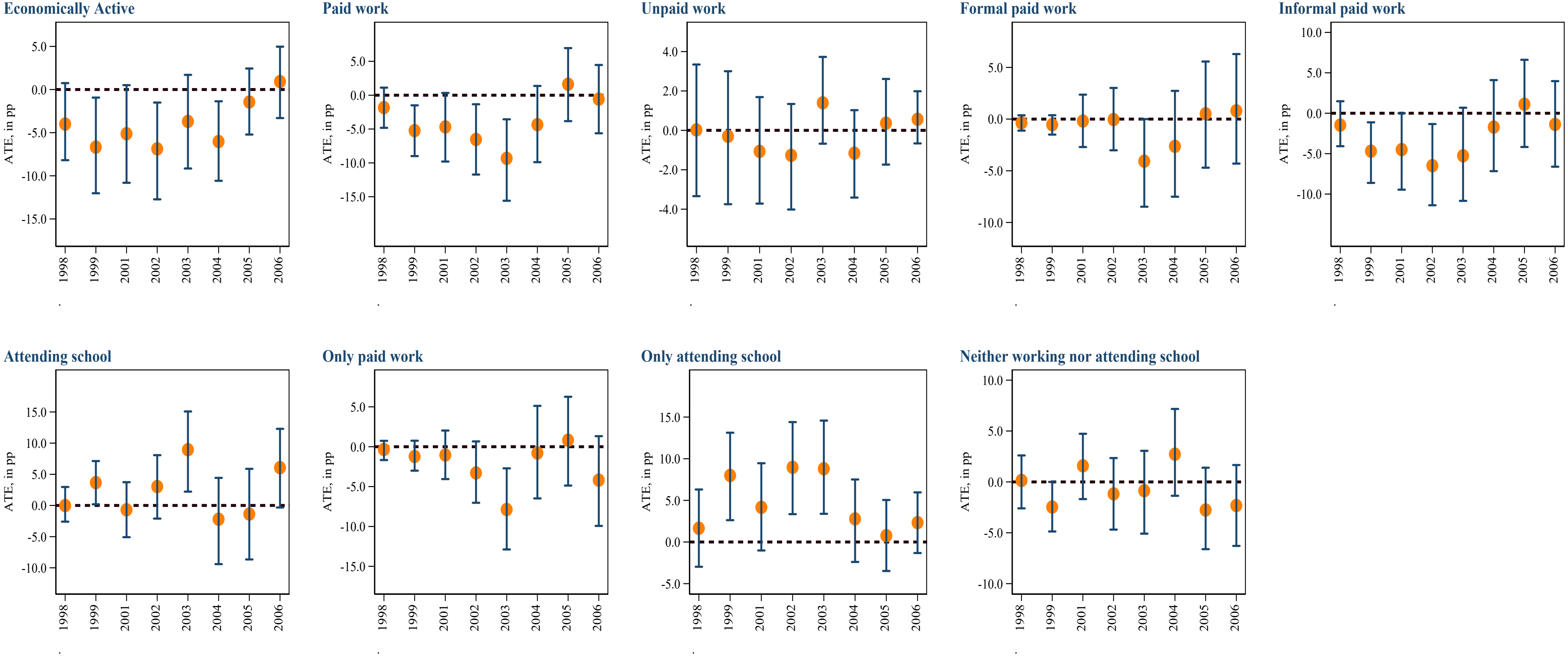
The results indicate that the reduction in paid work remains in place until 2003 when boys turned 18. During this period, the affected cohort was nearly 20 percent less likely to be engaged in paid work than the unaffected one. We observe that the affected cohort became economically active when they reached the minimum employment age (16) and were legally allowed to work. However, being less likely to find a paid job might have discouraged them to continue looking for a job as the difference in the percentage of those economically active became significant again when the cohorts turned 17. One year later, these 18-year-olds seemed to have returned to the economically active population but they appeared to face challenges engaging in paid activities.

The challenges faced in getting a job opportunity might be due to the lack of experience that seems to have surpassed the benefits of the availability of more hours a day to engage in academic activities. In fact, more than 95 percent of the affected cohort were enrolled in public schools, the majority of them known for the low quality of their education.[43] The postponement of entry into the labor market and consequent increase in the percentage of youth only attending school did not lead to a significant difference in the acquisition of a high school diploma (Figure A.9). Indeed, when affected and unaffected cohorts turned 19 and should have finished this level of education, there is no significant difference in the percentage of the groups with a high school degree (39.7 percent and 41.1 percent, respectively).[44] The result suggests that the unaffected group, despite being more likely to have a paid job, did not stop attending school. There are no significant differences in school attendance when the cohorts were 16 and 17.

[43] According to the 2003 Programme for International Student Assessment (PISA), among 40 countries, 15-year-olds in Brazil had the worst proficiency score in math, the second-worst score in science, and the third-worst score in language.

[44] 2004 PNAD wave. Sample of urban boys with a 10-week bandwidth around the cutoff.

Figure 3: Persistence of short-term effects of the ban, boys in urban areas (10-week bandwidth)



Note: 1998-2006 PNAD waves. Local Randomization in which the running variable is the number of weeks between the date of birth and the cutoff (December 16, 1984). We employed the *stata* command *rdrandinf* proposed by Cattaneo et al. (2016). Constant effect model (polynomial of order 0). 10-week bandwidth. We followed the affected and unaffected cohorts from 1998 to 2006, that is, from 13 to 21 years old.

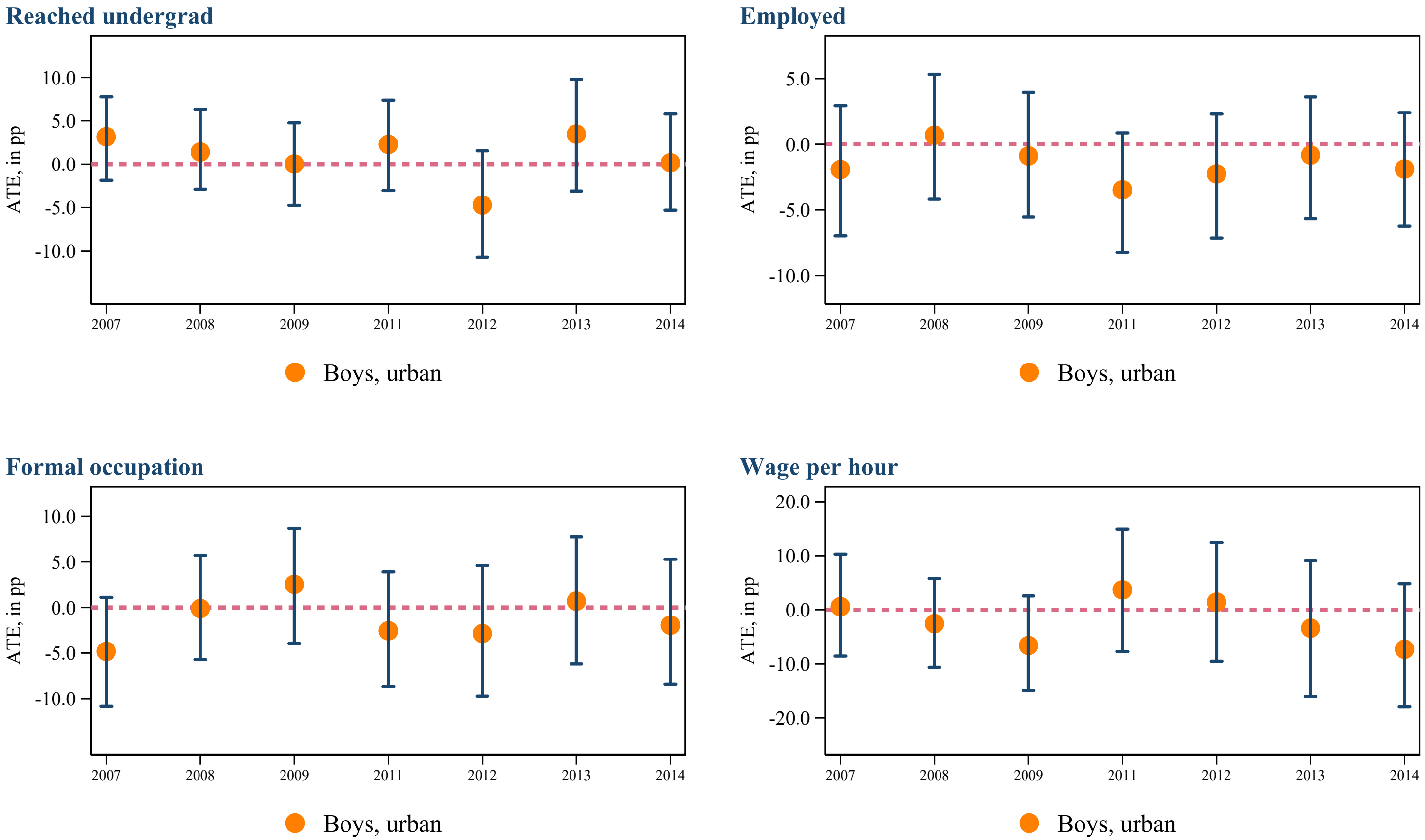
7 Long-Term Effects

To assess the long-term effects of the ban, we follow the affected and unaffected cohorts of urban boys between 2007 and 2014, when they were 22 to 29 years old. As mentioned above, the postponement of entry into the labor market and the increased share of those who only attend school could have led to higher human capital attainment and better employment positions or continued education. This effect might be limited by the quality of education available.[45] On the other hand, postponement might lead to less vocational and on-the-job training which might lead to lower human capital attainment. The net effect is an empirical question. Figure 4 shows that there is no significant difference in the percentage of employment, formal occupation, wage per hour, and undergrad enrollment or completion suggesting that the net effect might be close to zero.

These results should be interpreted cautiously. The lack of significant long-term effects could be due to the relatively small number of 14-year-olds affected by the ban in the sample. In 1999, 25 percent of urban boys were economically active, meaning that the majority of 14-year-olds we find in the PNAD 8 years later were never affected by the ban. Since we do not have the ability to identify individuals who were economically active in 1999, it is likely that the effect on employment and educational outcomes would have to be very high in order to identify the true effect.

[45] Between 2001 and 2004, more than 95 percent of the affected cohort was enrolled in public schools. Even though the ban is associated with an increase in school attendance, students spent more time in low quality schools which could help explain why the education acquired is not reflected in higher wages later in life.

Figure 4: Long-term effects of the ban, boys in urban areas (10-week bandwidth)



Note: 2007-2014 PNAD waves. Local Randomization in which the running variable is the number of weeks between the date of birth and the cutoff (December 16, 1984). We employed the *stata* command *rdrandinf* proposed by Cattaneo et al. (2016). Constant effect model (polynomial of order 0). 10-week bandwidth We followed the affected and unaffected cohorts from 2007 to 2014, that is, from 22 to 29 years old.

8 Discussion and Policy Implications

On December 15, 1998, the minimum employment age in Brazil changed from 14 to 16 years old and was applied the following day. The law appears to have had an immediate impact on 14-year-old urban boys who postponed their entrance into the labor market. This coincided with an increase in full-time schooling among this group between ages 14 and 18. Disappointingly, this increase in schooling did not lead to a a higher percentage of boys getting a high school degree. This appears to be due to the high degree of drop-out between upper primary and secondary school - only 40 percent of them had a secondary school degree at 19 years old. Interestingly, the cohort unaffected by the ban was not less likely to be attending school at 16 and 17 years old, suggesting that those who worked were successful in combining work and school.

The affected cohort were able to join the working population at age 16, but they continued to be less likely to get a paying job. Indeed, four years after the ban, at age 18, they were almost 20 percent less likely to be engaged in paid activities than the unaffected cohort. The difficulty in finding a job opportunity might have discouraged them from continuing to look for a job as there are significant differences in the the percentage of those economically active when boys turned 17 and 19.

The challenges these boys faced in getting a job opportunity might be due to the lack of previous experience, imposing barriers later on. On-the-job experience seems to have surpassed the benefits of the availability of more hours a day to engage in academic activities. In fact, more than 95 percent of the affected cohort were enrolled in public schools which are known for their low quality of education. Despite those challenges, the affected boys did not seem to have started with lower wages when compared to the unaffected ones.

We find that the child labor ban adopted in Brazil was successful in reducing child labor as it intended. The increase in the minimum working age, aimed to delay entry into work, had a measurable effect on labor force participation and employment. Children prevented from working appear to have concentrated their time in attending school. However the potential benefits of this change seemed to be constrained by a combination of school dropout before completing high school, low quality in public education, and lack of compensating outside options such as vocational training and apprenticeship programs. Together, these effects suggest that it might not be enough to enact policies that limit the work of children but might be equally important, in tandem, to improve the quality of public education. It might also be important to allow formal part-time jobs with a strong vocational component, where children can gain skills required by the adult labor market. More research is needed to understand this interaction.

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A Tables

Table A.1: Balance test for affected and unaffected cohorts, 9-month bandwidth (1999)

Variable	(1) Unnafected cohort N	Mean/SE	(2) Affected cohort N	Mean/SE	T-test P-value (1)-(2)
Mother's years of schooling	5358	5.05	5433	5.09	0.61
		(0.06)		(0.06)	
North	5599	0.07	5647	0.05	0.00***
		(0.00)		(0.00)	
Northeast	5599	0.32	5647	0.35	0.01**
		(0.01)		(0.01)	
Southeast	5599	0.41	5647	0.39	0.18
		(0.01)		(0.01)	
Midwest	5599	0.14	5647	0.14	0.81
		(0.00)		(0.00)	
South	5599	0.07	5647	0.07	0.88
		(0.00)		(0.00)	
White	5598	0.50	5646	0.48	0.19
		(0.01)		(0.01)	
Pardo	5598	0.45	5646	0.46	0.09*
		(0.01)		(0.01)	
Years of schooling of the head of the household	5581	4.72	5627	4.84	0.19
		(0.06)		(0.06)	
Head of the household is male	5599	0.79	5647	0.80	0.48
		(0.01)		(0.01)	
Age of the head of the household	5597	46.05	5647	45.78	0.21
		(0.15)		(0.15)	
Urban	5599	0.76	5647	0.76	0.98
		(0.01)		(0.01)	
Household size, relevant members	5599	5.42	5647	5.40	0.71
		(0.03)		(0.03)	

Notes: Source: PNAD, 1999. The value displayed for t-tests are p-values. Observations are weighted using PNAD sample weights.***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table A.2: Characteristics of 14-year-olds in urban and rural areas (1998)

Variable	N	(1) Rural Mean/SE	N	(2) Urban Mean/SE	T-test Difference (1)-(2)
Economically active children	1504	0.49	5970	0.20	0.29***
		(0.01)		(0.01)	
Share unemployed among the economically active	716	0.04	1189	0.28	-0.25***
		(0.01)		(0.01)	
Attending school	1504	0.84	5978	0.92	-0.09***
		(0.01)		(0.00)	
Among those attending school, share enrolled 1st to 9th grade	1256	0.99	5498	0.96	0.03***
		(0.00)		(0.00)	
Share of 14-year-olds working	1504	0.47	5978	0.14	0.33***
		(0.01)		(0.00)	
Among those working, share in paid work	685	0.19	867	0.67	-0.48***
		(0.02)		(0.02)	
Among those paid, weekly hours of work	148	37.17	578	34.63	2.54*
		(1.37)		(0.68)	
Wage of all jobs (2020 BRL)	147	336.20	575	341.39	-5.18
		(21.67)		(10.57)	
Among those paid, share working in offices/factories	148	0.14	578	0.55	-0.40***
		(0.03)		(0.02)	
Among those paid, share working household employee	148	0.19	578	0.20	-0.01
		(0.03)		(0.02)	
Among those paid, share working as housekeepers	148	0.19	578	0.21	-0.02
		(0.03)		(0.02)	
Among those paid, share in the formal sector	967	0.01	5689	0.01	0.01*
		(0.00)		(0.00)	
Among those working, share in unpaid work	685	0.81	867	0.33	0.48***
		(0.02)		(0.02)	
Among those unpaid, weekly hours of work	537	24.25	289	23.71	0.55
		(0.51)		(0.85)	
Among those unpaid, share working in agriculture	537	0.93	289	0.31	0.62***
		(0.01)		(0.03)	
Among those unpaid, share working for the household	537	0.98	289	0.93	0.05***
		(0.01)		(0.02)	
Only paid work	1504	0.03	5978	0.02	0.01***
		(0.00)		(0.00)	
Only unpaid work	1504	0.06	5978	0.01	0.06***
		(0.01)		(0.00)	
Paid work and attending school	1504	0.06	5978	0.08	-0.02**
		(0.01)		(0.00)	
Unpaid work and attending school	1504	0.32	5978	0.04	0.28***
		(0.01)		(0.00)	
Only attending school	1504	0.46	5978	0.80	-0.35***
		(0.01)		(0.01)	
Neither working nor attending school	1504	0.07	5978	0.05	0.02**
		(0.01)		(0.00)	

Notes: Source: PNAD, 1998. The value displayed for t-tests are the differences in the means across the groups. Observations are weighted using variable PNAD sample weights.***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table A.3: Characteristics of 14-year-old boys and girls in urban areas (1998)

		(1)		(2)	T-test
		Girls		Boys	Difference
Variable	N	Mean/SE	N	Mean/SE	(1)-(2)
Economically active children	2959	0.15	2950	0.25	-0.11***
		(0.01)		(0.01)	
Share unemployed among the economically active	437	0.33	737	0.26	0.07**
		(0.03)		(0.02)	
Attending school	2962	0.93	2955	0.92	0.01
		(0.01)		(0.01)	
Among those attending school, share enrolled 1st to 9th grade	2742	0.95	2707	0.97	-0.02***
		(0.00)		(0.00)	
Share of 14-year-olds working	2962	0.10	2955	0.19	-0.09***
		(0.01)		(0.01)	
Among those working, share in paid work	298	0.73	555	0.63	0.09**
		(0.03)		(0.02)	
Among those paid, weekly hours of work	212	35.55	354	33.84	1.70
		(1.25)		(0.80)	
Wage of all jobs (2020 BRL)	210	333.71	353	346.29	-12.58
		(19.40)		(12.65)	
Among those paid, share working in offices/factories	212	0.36	354	0.66	-0.30***
		(0.04)		(0.03)	
Among those paid, share working household employee	212	0.47	354	0.04	0.43***
		(0.04)		(0.01)	
Among those paid, share working as housekeepers	212	0.51	354	0.03	0.47***
		(0.04)		(0.01)	
Among those paid, share in the formal sector	2876	0.00	2754	0.01	-0.01***
		(0.00)		(0.00)	
Among those working, share in unpaid work	298	0.27	555	0.37	-0.09**
		(0.03)		(0.02)	
Among those unpaid, weekly hours of work	86	23.94	201	23.68	0.26
		(1.54)		(1.03)	
Among those unpaid, share working in agriculture	86	0.23	201	0.35	-0.12*
		(0.05)		(0.04)	
Among those unpaid, share working for the household	86	0.90	201	0.95	-0.05
		(0.04)		(0.02)	
Only paid work	2962	0.01	2955	0.02	-0.01***
		(0.00)		(0.00)	
Only unpaid work	2962	0.00	2955	0.01	-0.01***
		(0.00)		(0.00)	
Paid work and attending school	2962	0.06	2955	0.10	-0.04***
		(0.00)		(0.01)	
Unpaid work and attending school	2962	0.02	2955	0.06	-0.03***
		(0.00)		(0.00)	
Only attending school	2962	0.85	2955	0.76	0.08***
		(0.01)		(0.01)	
Neither working nor attending school	2962	0.06	2955	0.05	0.01
		(0.00)		(0.00)	

Notes: Source: PNAD, 1998. The value displayed for t-tests are the differences in the means across the groups. Observations are weighted using PNAD sample weights.***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table A.4: Characteristics of 14-year-old boys and girls in rural areas (1998)

Variable	N	(1) Girls Mean/SE	N	(2) Boys Mean/SE	T-test Difference (1)-(2)
Economically active children	717	0.34	767	0.64	-0.30***
		(0.02)		(0.02)	
Share unemployed among the economically active	228	0.04	481	0.04	0.01
		(0.01)		(0.01)	
Attending school	717	0.82	767	0.85	-0.03
		(0.02)		(0.01)	
Among those attending school, share enrolled 1st to 9th grade	593	0.98	645	0.99	-0.01
		(0.01)		(0.00)	
Share of 14-year-olds working	717	0.32	767	0.61	-0.29***
		(0.02)		(0.02)	
Among those working, share in paid work	216	0.19	462	0.20	-0.01
		(0.03)		(0.02)	
Among those paid, weekly hours of work	45	34.85	102	38.16	-3.31
		(3.00)		(1.46)	
Wage of all jobs (2020 BRL)	45	244.55	101	378.53	-133.98***
		(34.00)		(26.69)	
Among those paid, share working in offices/factories	45	0.10	102	0.17	-0.07
		(0.05)		(0.04)	
Among those paid, share working household employee	45	0.56	102	0.02	0.54***
		(0.08)		(0.02)	
Among those paid, share working as housekeepers	45	0.58	102	0.02	0.56***
		(0.08)		(0.02)	
Among those paid, share in the formal sector	546	0.01	407	0.03	-0.02**
		(0.00)		(0.01)	
Among those working, share in unpaid work	216	0.81	462	0.80	0.01
		(0.03)		(0.02)	
Among those unpaid, weekly hours of work	171	21.60	360	25.42	-3.82***
		(0.94)		(0.59)	
Among those unpaid, share working in agriculture	171	0.89	360	0.95	-0.06**
		(0.02)		(0.01)	
Among those unpaid, share working for the household	171	0.98	360	0.98	-0.00
		(0.01)		(0.01)	
Only paid work	717	0.02	767	0.04	-0.02***
		(0.01)		(0.01)	
Only unpaid work	717	0.05	767	0.07	-0.03*
		(0.01)		(0.01)	
Paid work and attending school	717	0.04	767	0.08	-0.04***
		(0.01)		(0.01)	
Unpaid work and attending school	717	0.21	767	0.42	-0.20***
		(0.02)		(0.02)	
Only attending school	717	0.57	767	0.36	0.21***
		(0.02)		(0.02)	
Neither working nor attending school	717	0.11	767	0.03	0.08***
		(0.01)		(0.01)	

Notes: Source: PNAD, 1998. The value displayed for t-tests are the differences in the means across the groups. Observations are weighted using PNAD sample weights.***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table A.5: Number of formal employees identified in RAIS, 3-month bandwidth

	Males				Females			
RAIS year	Unaffected cohort	%	Affected cohort	%	Unaffected cohort	%	Affected cohort	%
1999	2,982	66.6	1,498	33.4	991	67.0	488	33.0
2001	32,757	53.0	29,064	47.0	16,401	54.4	13,768	45.6
2002	64,346	54.0	54,895	46.0	40,326	54.5	33,646	45.5
2003	110,116	53.5	95,774	46.5	66,398	51.3	62,999	48.7
2007	258,410	49.6	263,010	50.4	157,948	49.4	161,835	50.6

Notes: Source: RAIS. Columns *All* show all formal work contracts during the RAIS year. If the same person had more than one formal occupation in the year, she/he is counted more than once. The 3-month bandwidth shows individuals born between September 22, 1984, and December 15, 1984 (unaffected cohort); and those born between December 16, 1984, and March 9, 1985 (affected cohort).

Table A.6: Continuity Based Approach, boys in urban areas (1998)

1998, same age	4-month bandwidth			6-month bandwidth			9-month bandwidth		
	Liner	Quadratic	Piecewise	Liner	Quadratic	Piecewise	Liner	Quadratic	Piecewise
Economically	-4.98	-4.98	-4.88	-1.41	-1.41	-1.36	-2.87	-2.87	-2.86
Active Children	(3.96)	(3.96)	(3.79)	(3.19)	(3.16)	(3.10)	(2.82)	(2.82)	(2.81)
Paid	-2.64	-2.66	-2.62	-0.56	-0.56	-0.56	-3.75*	-3.76*	-3.76*
work	(3.49)	(3.49)	(3.45)	(2.70)	(2.71)	(2.70)	(2.22)	(2.22)	(2.22)
Unpaid	-1.38	-1.29	-1.18	-0.97	-0.97	-0.92	0.26	0.26	0.27
work	(2.59)	(2.39)	(2.33)	(2.03)	(1.96)	(1.92)	(1.61)	(1.61)	(1.60)
Formal paid	-2.77*	-2.73*	-2.68*	-2.02*	-2.02*	-2.00*	-2.14**	-2.14**	-2.14**
work	(1.45)	(1.44)	(1.46)	(1.18)	(1.17)	(1.17)	(0.99)	(0.99)	(0.99)
Informal paid	0.13	0.07	0.06	1.47	1.46	1.44	-1.61	-1.61	-1.62
work	(3.30)	(3.30)	(3.25)	(2.54)	(2.54)	(2.53)	(2.06)	(2.06)	(2.06)
Attending	0.41	0.49	0.55	1.94	1.94	1.97	2.40	2.40	2.42
school	(2.28)	(2.25)	(2.24)	(1.95)	(1.95)	(1.94)	(1.61)	(1.59)	(1.59)
Only	-4.17**	-4.21**	-4.25**	-3.48***	-3.48***	-3.49**	-3.54***	-3.55***	-3.56***
paid work	(1.75)	(1.76)	(1.82)	(1.29)	(1.29)	(1.31)	(1.03)	(1.02)	(1.04)
Only attending	0.75	0.72	0.58	-0.15	-0.15	-0.16	2.17	2.17	2.18
school	(4.32)	(4.22)	(4.04)	(3.32)	(3.33)	(3.28)	(2.91)	(2.91)	(2.90)
Neither working	3.28	3.24	3.22	1.68	1.67	1.63	1.33	1.32	1.31
nor attending school	(2.34)	(2.33)	(2.33)	(1.89)	(1.87)	(1.87)	(1.47)	(1.47)	(1.47)

1998, same cohort	4-month bandwidth			6-month bandwidth			9-month bandwidth		
	Liner	Quadratic	Piecewise	Liner	Quadratic	Piecewise	Liner	Quadratic	Piecewise
Economically	-1.64	-1.68	-1.83	0.67	0.67	0.61	-1.56	-1.56	-1.56
Active Children	(3.67)	(3.60)	(3.55)	(3.12)	(3.09)	(3.05)	(2.47)	(2.47)	(2.46)
Paid	-2.92	-2.89	-2.75	-1.54	-1.52	-1.41	-2.14	-2.16	-2.12
work	(2.05)	(1.90)	(1.84)	(2.01)	(1.85)	(1.81)	(1.53)	(1.48)	(1.45)
Unpaid	1.86	1.83	1.68	2.47	2.47	2.43	1.77	1.77	1.76
work	(3.00)	(2.93)	(2.91)	(2.46)	(2.45)	(2.41)	(1.87)	(1.86)	(1.85)
Formal paid	-0.92*	-0.92*	-0.94*	-0.69*	-0.69*	-0.70*	-0.19	-0.20	-0.19
work	(0.49)	(0.47)	(0.47)	(0.40)	(0.40)	(0.40)	(0.39)	(0.39)	(0.40)
Informal paid	-2.00	-1.97	-1.80	-0.85	-0.83	-0.71	-1.95	-1.96	-1.93
work	(2.07)	(1.86)	(1.75)	(2.00)	(1.81)	(1.75)	(1.47)	(1.44)	(1.40)
Attending	0.72	0.74	0.85	0.61	0.61	0.59	0.23	0.22	0.24
school	(2.57)	(2.52)	(2.52)	(2.10)	(2.13)	(2.13)	(1.58)	(1.57)	(1.57)
Only	0.24	0.25	0.25	-0.45	-0.45	-0.41	-0.16	-0.17	-0.15
paid work	(0.89)	(0.90)	(0.92)	(0.82)	(0.80)	(0.81)	(0.61)	(0.60)	(0.60)
Only attending	2.30	2.32	2.43	-0.64	-0.66	-0.71	0.30	0.30	0.30
school	(2.65)	(2.62)	(2.63)	(2.49)	(2.57)	(2.59)	(1.96)	(1.95)	(1.96)
Neither working	-1.24	-1.26	-1.37	-0.29	-0.29	-0.30	0.08	0.09	0.06
nor attending school	(1.98)	(1.91)	(1.87)	(1.75)	(1.76)	(1.73)	(1.29)	(1.27)	(1.26)

Notes: 1998 PNAD wave. The first block of results is a placebo exercise as if the increase of the minimum employment age was adopted on December 16, 1997, and the cutoff date is set at December 16, 1983. Therefore, we compare 14-years-old born up to 9 months before this date with those born up to 9 months after. The second block of results is also a placebo exercise where we compare affected and unaffected cohorts in the 1998 PNAD wave, that is, before the increase of the minimum employment age when children were 13 years old. As shown in Table 1. the cutoff is December 16, 1984. Standard errors clustered at the running variable. Controls: age, education, and gender of the head of the household, Brazilian region of residence, urban areas, household size, and children's color of the skin.

Table A.7: Continuity Based Approach for Economically Active Children (1999 and Pooled 1999 and 2001)

Boys and girls, urban and rural (1999)

	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	-1.85	-1.99	-2.11	-0.56	-0.51	-0.43	-0.72	-0.68	-0.62	-1.15	-1.14	-1.1
	(1.89)	(1.87)	(2.07)	(1.76)	(1.69)	(1.72)	(1.66)	(1.54)	(1.60)	(1.46)	(1.44)	(1.46)
Obs	4794	4794	4794	7300	7300	7300	9842	9842	9842	11148	11148	11148

Boys and girls, urban and rural (Pooled 1999 and 2001)

	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	0.16	0.17	0.23	0.11	0.22	0.33	-0.48	-0.45	-0.39	-1.1	-1.1	-1.07
	(1.47)	(1.45)	(1.40)	(1.56)	(1.04)	(0.98)	(1.26)	(1.06)	(0.98)	(1.22)	(1.22)	(1.17)
Obs	9816	9816	9816	14892	14892	14892	20068	20068	20068	22624	22624	22624

Boys urban (1999)

	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	-1.72	-1.79	-1.93	-0.77	-0.76	-0.75	-1.7	-1.72	-1.64	-4.28	-4.3	-4.27
	(3.94)	(4.28)	(4.48)	(2.77)	(2.76)	(2.80)	(2.54)	(2.45)	(2.49)	(2.50)	(2.52)	(2.55)
Obs	1868	1868	1868	2873	2873	2873	3844	3844	3844	4357	4357	4357

Boys urban (Pooled 1999 and 2001)

	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	-1.88	-1.85	-1.82	-1.96	-1.84	-1.76	-2.21	-2.16	-2.09	-3.75*	-3.78*	-3.76*
	(2.97)	(2.96)	(2.98)	(2.36)	(2.15)	(2.17)	(1.91)	(1.70)	(1.68)	(2.06)	(2.12)	(2.12)
Obs	3937	3937	3937	6033	6033	6033	8125	8125	8125	9171	9171	9171

Notes: 1999 and 2001 PNAD waves. Regression Discontinuity Design in which the running variable is the number of weeks between the date of birth and the cutoff (December 16, 1984). Standard errors clustered at the running variable. Controls: age, education and gender of the head of the household, Brazilian region of residence, urban areas, household size, and children's color of the skin.

Table A.8: Continuity Based Approach for Paid Work (1999 and Pooled 1999 and 2001)

Boys and girls, urban and rural (1999)												
	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	-0.52	-0.64	-0.73	0.17	0.18	0.18	0.05	0.06	0.08	-0.2	-0.2	-0.19
	(1.38)	(1.27)	(1.33)	(1.18)	(1.18)	(1.17)	(1.18)	(1.19)	(1.19)	(1.08)	(1.09)	(1.10)
Obs	4799	4799	4799	7307	7307	7307	9850	9850	9850	11157	11157	11157
Boys and girls, urban and rural (Pooled 1999 and 2001)												
	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	-0.09	-0.11	-0.13	0.16	0.2	0.24	-0.02	-0.02	-0.01	-0.33	-0.34	-0.34
	(0.63)	(0.60)	(0.58)	(0.76)	(0.70)	(0.72)	(0.74)	(0.73)	(0.72)	(0.77)	(0.79)	(0.78)
Obs	9822	9822	9822	14900	14900	14900	20077	20077	20077	22634	22634	22634
Boys urban (1999)												
	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	-2.32	-2.37	-2.43	-1.98	-1.98	-1.96	-3.5	-3.5	-3.49	-4.05*	-4.06*	-4.04*
	(2.82)	(3.35)	(3.27)	(2.21)	(2.16)	(2.15)	(2.30)	(2.29)	(2.29)	(2.15)	(2.13)	(2.13)
Obs	1872	1872	1872	2878	2878	2878	3849	3849	3849	4362	4362	4362
Boys urban (Pooled 1999 and 2001)												
	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	-4.15**	-4.10**	-4.11**	-3.52**	-3.46**	-3.41**	-3.27**	-3.29**	-3.28**	-3.64**	-3.69**	-3.70**
	(1.48)	(1.49)	(1.43)	(1.28)	(1.23)	(1.25)	(1.17)	(1.21)	(1.17)	(1.27)	(1.32)	(1.30)
Obs	3942	3942	3942	6039	6039	6039	8131	8131	8131	9177	9177	9177

Notes: 1999 and 2001 PNAD waves. Regression Discontinuity Design in which the running variable is the number of weeks between the date of birth and the cutoff (December 16, 1984). Standard errors clustered at the running variable. Controls: age, education and gender of the head of the household, Brazilian region of residence, urban areas, household size, and children's color of the skin.

Table A.9: Continuity Based Approach for Unpaid Work (1999 and Pooled 1999 and 2001)

Boys and girls, urban and rural (1999)

	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	-1.77	-1.79	-1.8	-0.37	-0.36	-0.33	-0.66	-0.64	-0.62	-0.41	-0.41	-0.39
	(1.31)	(1.39)	(1.41)	(1.27)	(1.25)	(1.25)	(1.13)	(1.10)	(1.11)	(1.04)	(1.02)	(1.03)
Obs	4799	4799	4799	7307	7307	7307	9850	9850	9850	11157	11157	11157

Boys and girls, urban and rural (Pooled 1999 and 2001)

	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	-0.67	-0.65	-0.59	-0.02	0.00	0.03	-0.56	-0.55	-0.52	-0.31	-0.3	-0.28
	(1.29)	(1.23)	(1.08)	(0.99)	(0.92)	(0.86)	(0.85)	(0.76)	(0.74)	(0.77)	(0.71)	(0.68)
Obs	9822	9822	9822	14900	14900	14900	20077	20077	20077	22634	22634	22634

Boys urban (1999)

	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	-0.84	-0.84	-0.87	0.96	0.95	0.91	1.08	1.07	1.10	0.10	0.10	0.10
	(1.59)	(1.63)	(1.69)	(1.95)	(1.87)	(1.91)	(1.89)	(1.88)	(1.90)	(1.67)	(1.67)	(1.68)
Obs	1872	1872	1872	2878	2878	2878	3849	3849	3849	4362	4362	4362

Boys urban (Pooled 1999 and 2001)

	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	-0.5	-0.49	-0.43	0.35	0.35	0.36	-0.11	-0.09	-0.06	0.07	0.08	0.10
	(1.70)	(1.65)	(1.49)	(1.36)	(1.37)	(1.36)	(1.25)	(1.17)	(1.16)	(1.11)	(1.10)	(1.08)
Obs	3942	3942	3942	6039	6039	6039	8131	8131	8131	9177	9177	9177

Notes: 1999 and 2001 PNAD waves. Regression Discontinuity Design in which the running variable is the number of weeks between the date of birth and the cutoff (December 16, 1984). Standard errors clustered at the running variable. Controls: age, education and gender of the head of the household, Brazilian region of residence, urban areas, household size, and children's color of the skin.

Table A.10: Continuity Based Approach for Informal Paid Work (1999 and Pooled 1999 and 2001)

Boys and girls, urban and rural (1999)												
	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	-0.35	-0.47	-0.56	0.52	0.52	0.53	0.17	0.18	0.2	-0.1	-0.1	-0.09
	(1.17)	(1.10)	(1.17)	(0.96)	(0.96)	(0.95)	(1.01)	(1.01)	(1.02)	(0.93)	(0.93)	(0.94)
Obs	4799	4799	4799	7307	7307	7307	9850	9850	9850	11157	11157	11157
Boys and girls, urban and rural (Pooled 1999 and 2001)												
	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	-0.91	-0.95	-0.99*	-0.86	-0.82	-0.78	-0.8	-0.79	-0.76	-1.15*	-1.14*	-1.13*
	(0.67)	(0.55)	(0.51)	(0.60)	(0.55)	(0.59)	(0.62)	(0.52)	(0.55)	(0.59)	(0.58)	(0.57)
Obs	9822	9822	9822	14900	14900	14900	20077	20077	20077	22634	22634	22634
Boys urban (1999)												
	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	-2.68	-2.74	-2.82	-1.68	-1.68	-1.67	-3.53	-3.53	-3.52	-4.18*	-4.19*	-4.18*
	(2.59)	(3.09)	(3.07)	(1.96)	(1.93)	(1.94)	(2.21)	(2.20)	(2.22)	(2.07)	(2.05)	(2.07)
Obs	1872	1872	1872	2878	2878	2878	3849	3849	3849	4362	4362	4362
Boys urban (Pooled 1999 and 2001)												
	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	-4.31**	-4.33**	-4.37**	-3.47***	-3.41***	-3.38***	-3.54***	-3.53***	-3.50***	-4.19***	-4.21***	-4.20***
	(1.55)	(1.50)	(1.47)	(0.99)	(0.96)	(0.98)	(1.05)	(1.03)	(1.04)	(1.03)	(1.06)	(1.05)
Obs	3942	3942	3942	6039	6039	6039	8131	8131	8131	9177	9177	9177

Notes: 1999 and 2001 PNAD waves. Regression Discontinuity Design in which the running variable is the number of weeks between the date of birth and the cutoff (December 16, 1984). Standard errors clustered at the running variable. Controls: age, education and gender of the head of the household, Brazilian region of residence, urban areas, household size, and children's color of the skin.

Table A.11: Continuity Based Approach for Only Attending School (1999 and Pooled 1999 and 2001)

Boys and girls, urban and rural (1999)												
	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	1.71	1.78	1.84	0.12	0.09	0.05	0.6	0.58	0.54	0.59	0.59	0.54
	(1.67)	(1.88)	(1.93)	(1.68)	(1.62)	(1.63)	(1.35)	(1.30)	(1.32)	(1.27)	(1.18)	(1.20)
Obs	4799	4799	4799	7307	7307	7307	9850	9850	9850	11157	11157	11157
Boys and girls, urban and rural (Pooled 1999 and 2001)												
	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	0.32	0.33	0.31	0.03	0.01	-0.02	0.51	0.49	0.47	0.62	0.61	0.6
	(0.98)	(1.03)	(1.03)	(0.92)	(0.88)	(0.87)	(0.84)	(0.77)	(0.77)	(0.70)	(0.69)	(0.68)
Obs	9821	9821	9821	14899	14899	14899	20076	20076	20076	22633	22633	22633
Boys urban (1999)												
	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	4.11	4.15	4.18	2.35	2.35	2.38	4.12	4.13	4.1	5.77**	5.80**	5.76**
	(4.06)	(4.48)	(4.42)	(3.07)	(3.10)	(3.19)	(2.64)	(2.63)	(2.64)	(2.52)	(2.50)	(2.52)
Obs	1872	1872	1872	2878	2878	2878	3849	3849	3849	4362	4362	4362
Boys urban (Pooled 1999 and 2001)												
	4-month bandwidth			6-month bandwidth			8-month bandwidth			9-month bandwidth		
	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise	Linear	Quadratic	Piecewise
ITT	1.87	1.88	1.91	1.33	1.32	1.31	1.75	1.72	1.69	2.11	2.12	2.11
	(2.11)	(2.15)	(2.21)	(1.90)	(1.94)	(1.96)	(1.68)	(1.63)	(1.67)	(1.56)	(1.57)	(1.59)
Obs	3942	3942	3942	6039	6039	6039	8131	8131	8131	9177	9177	9177

Notes: 1999 and 2001 PNAD waves. Regression Discontinuity Design in which the running variable is the number of weeks between the date of birth and the cutoff (December 16, 1984). Standard errors clustered at the running variable. Controls: age, education and gender of the head of the household, Brazilian region of residence, urban areas, household size, and children's color of the skin.

Table A.12: Local Randomization (1999)

	Boys, Girls, Urban, Rural			Boys, Urban			Girls, Urban			Boys, Girls, Rural		
	ATE	95% CI	Mean outcome	ATE	95% CI	Mean outcome	ATE	95% CI	Mean outcome	ATE	95% CI	Mean outcome
Economically Active Children												
10 weeks	-4.03**	[-7.5,-.72]	30.77	-6.66**	[-11.,-1.5]	28.55	-2.51	[-6.6,1.8]	16.26	-2.98	[-10.,4.5]	55.06
12 weeks	-4.11**	[-7.1,-.90]	29.96	-7.22***	[-11.,-2.7]	29.67	-1.95	[-5.4,1.8]	15.02	-2.68	[-10.,4.5]	53.24
14 weeks	-3.81**	[-6.6,-1.1]	29.85	-7.82***	[-12.,-3.6]	29.97	-.716	[-4.4,2.7]	14.84	-2.59	[-9.7,4.0]	52.51
Paid work												
10 weeks	-1.71	[-4.0,.65]	11.10	-5.23**	[-9.3,-1.1]	14.90	.6630	[-2.2,3.6]	5.74	.1411	[-5.2,5.8]	13.19
12 weeks	-2.30**	[-4.2,-.24]	11.00	-6.11***	[-9.2,-2.7]	15.48	1.142	[-1.2,3.6]	4.95	-1.66	[-6.8,3.4]	13.30
14 weeks	-2.37***	[-4.2,-.51]	10.88	-6.68***	[-10.,-3.3]	15.70	1.348	[-1.0,3.8]	4.91	-1.45	[-6.1,2.8]	12.62
Unpaid work												
10 weeks	-1.38	[-3.9,1.3]	14.78	-.296	[-3.7,2.9]	7.60	-1.59	[-3.6,.73]	4.30	-3.75	[-11.,3.2]	40.64
12 weeks	-.997	[-3.6,1.2]	14.14	.2336	[-2.7,3.0]	7.79	-1.93	[-4.2,.30]	4.58	-1.88	[-9.6,5.6]	38.56
14 weeks	-.374	[-2.4,1.6]	13.66	.5493	[-2.0,3.1]	7.29	-1.17	[-3.1,.77]	4.26	-.817	[-7.6,5.7]	37.57
Formal paid work												
10 weeks	-.279	[-.86,.28]	0.79	-.541	[-1.4,.37]	0.61	.0092	[-.73,.73]	0.80	-.336	[-1.9,1.3]	1.05
12 weeks	-.171	[-.72,.36]	0.66	-.446	[-1.2,.30]	0.50	.1589	[-.60,.91]	0.66	-.281	[-1.7,1.1]	0.90
14 weeks	-.394	[-.81,.10]	0.85	-.771*	[-1.5,-1.1]	0.89	.0154	[-.77,.77]	0.71	-.463	[-1.4,.47]	1.00
Informal paid work												
10 weeks	-1.43	[-3.6,.72]	10.30	-4.68**	[-8.9,-.74]	14.29	.6538	[-2.2,3.6]	4.94	.4774	[-4.5,5.8]	12.14
12 weeks	-2.13**	[-3.9,-.18]	10.34	-5.66***	[-8.9,-2.4]	14.98	.9834	[-1.5,3.3]	4.29	-1.38	[-6.2,3.4]	12.40
14 weeks	-1.98**	[-3.8,-.10]	10.03	-5.91***	[-9.0,-2.8]	14.81	1.333	[-.90,3.6]	4.20	-.989	[-5.2,3.5]	11.62
Attending school												
10 weeks	2.600**	[.43,4.9]	89.04	3.671**	[.37,7.1]	89.89	.4450	[-2.5,3.6]	93.35	4.772	[-.97,10.]	81.53
12 weeks	2.562**	[.36,4.4]	89.46	3.319**	[.30,6.1]	90.72	.6813	[-1.8,3.3]	93.55	4.775*	[-.56,10.]	81.23
14 weeks	3.004***	[1.2,5.0]	89.25	3.897***	[1.3,6.5]	90.46	1.735	[-.77,4.1]	92.57	3.771	[-.95,8.5]	82.38
Only paid work												
10 weeks	-1.25*	[-2.4,3.2]	3.64	-1.21	[-2.9,.74]	3.96	-.344	[-1.8,1.1]	1.76	-2.99	[-6.5,.65]	5.88
12 weeks	-1.28**	[-2.4,-.12]	3.40	-1.30	[-3.0,.61]	3.71	-.136	[-1.5,.91]	1.44	-3.39*	[-6.2,-7.5]	5.93
14 weeks	-1.37***	[-2.3,-.40]	3.28	-1.64**	[-3.1,-.26]	3.72	-.351	[-1.2,.77]	1.39	-2.77*	[-5.2,6.3]	5.48
Only attending school												
10 weeks	3.887**	[.28,7.3]	69.10	7.996***	[2.6,13.]	71.69	.4870	[-4.0,4.4]	85.73	3.676	[-4.5,11.]	41.18
12 weeks	4.067***	[1.0,7.1]	69.84	8.058***	[3.3,12.]	71.43	.5772	[-3.3,4.2]	86.15	3.529	[-3.4,10.]	42.45
14 weeks	3.960***	[.91,6.8]	70.01	8.392***	[4.1,12.]	71.52	.5732	[-2.8,4.1]	85.36	2.419	[-3.8,9.0]	44.42
Neither working nor attending school												
10 weeks	-.791	[-2.3,.86]	5.02	-2.46*	[-5.2,-7.3]	5.81	.4491	[-2.2,2.9]	4.23	-.063	[-3.2,3.2]	4.99
12 weeks	-.763	[-2.1,.78]	5.03	-2.17	[-4.3,.30]	5.30	.2128	[-2.1,2.7]	4.32	.0160	[-3.4,3.4]	5.69
14 weeks	-1.21	[-2.6,.20]	5.46	-2.25**	[-4.4,-.26]	5.49	-.746	[-2.9,1.2]	5.46	-.148	[-2.8,2.8]	5.39

Notes: 1999 PNAD wave. Local Randomization in which the running variable is the number of weeks between the date of birth and the cutoff (December 16, 1984). Constant effect model (polynomial of order 0).

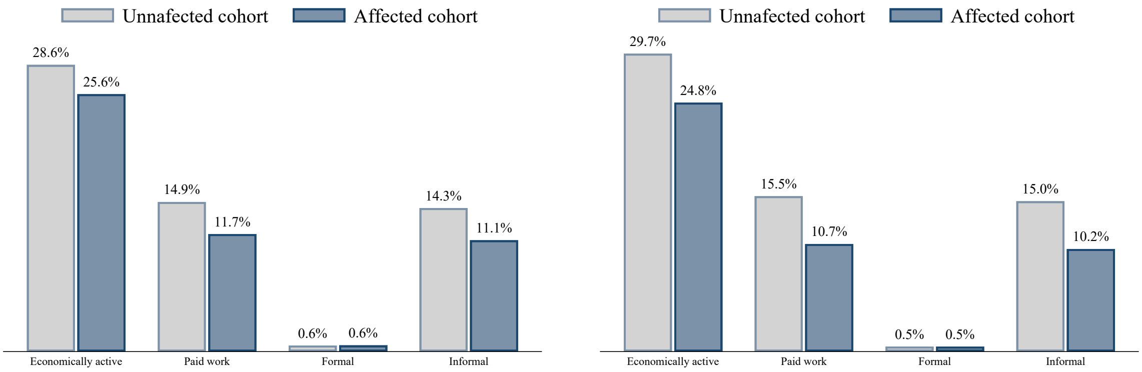
Table A.13: Local Randomization and mother's education, boys in urban areas (10-week bandwidth)

	ATE	95% CI	Mean outcome	as %	ATE	95% CI	Mean outcome	as %
Economically Active								
1999	-6.76*	[-13.,-2.5]	32.41	-20.86	-6.17	[-14.,1.4]	16.93	
2001	-6.11*	[-13.,1.2]	55.58	-11.01	-1.04	[-10.,9.3]	30.14	
2003	.2112	[-5.6,6.7]	77.44		-3.74	[-14.,7.2]	58.01	
Paid work								
1999	-4.66*	[-10.,.26]	16.87	-27.68	-4.48	[-10.,.71]	8.24	
2001	-4.44	[-11.,1.9]	34.86		-.909	[-10.,8.0]	18.91	
2003	-7.53**	[-14.,-.54]	57.09	-13.21	-9.05	[-19.,1.2]	43.71	
Unpaid work								
1999	-.550	[-4.2,3.1]	8.33		.4729	[-5.7,7.1]	5.44	
2001	-.802	[-4.3,2.4]	7.44		-1.40	[-6.7,4.0]	6.90	
2003	2.269	[-1.0,5.4]	3.46		-.694	[-4.8,3.6]	4.11	
Formal paid work								
1999	-.481	[-1.5,.52]	0.69		-.729	[-1.4,-5.7]	0.43	
2001	.4973	[-2.6,3.3]	5.86		-3.40	[-8.0,1.3]	7.13	
2003	-4.36	[-9.7,1.0]	20.35		-2.11	[-9.6,5.4]	16.76	
Informal paid work								
1999	-4.18	[-9.4,.52]	16.18		-3.75	[-8.6,1.4]	7.81	
2001	-4.94	[-11.,.96]	29.00		2.495	[-5.3,10.]	11.78	
2003	-3.17	[-9.7,3.2]	36.74		-6.93	[-15.,2.4]	26.95	
Attending school								
1999	3.604	[-1.0,7.9]	88.06		2.215	[1.0,5.7]	96.55	
2001	1.373	[-4.3,6.7]	79.05		-2.63	[-6.7,1.3]	97.60	
2003	7.443*	[-.27,14.]	47.21	15.77	4.349	[-4.8,14.]	65.72	
Only paid work								
1999	-1.11	[-3.6,1.5]	4.66		-.729	[-1.4,-5.7]	0.92	
2001	-1.74	[-5.8,2.4]	9.89		.6486	[-2.6,4.0]	0.71	
2003	-5.37	[-11.,1.6]	31.93		-7.96*	[-15.,-9.2]	22.59	-35.24
Only attending school								
1999	7.758**	[1.0,14.]	67.98	11.41	5.495	[-2.8,12.]	83.80	
2001	4.796	[-2.1,11.]	47.88		.3333	[-9.3,10.]	72.51	
2003	7.984**	[1.0,14.]	19.27	41.44	7.657	[-2.4,18.]	41.31	
Neither working nor attending school								
1999	-2.53	[-5.7,.52]	6.82		-1.48	[-4.3,1.4]	2.53	
2001	.4501	[-3.3,5.0]	9.81		1.981	[-1.3,5.3]	1.69	
2003	-2.71	[-8.1,2.7]	20.18		2.088	[-4.8,9.0]	10.88	

Notes: Source: 1999, 2001, and 2003 PNAD waves. Local Randomization in which the running variable is the number of weeks between the date of birth and the cutoff (December 16, 1984). Constant effect model (polynomial of order 0). We run one model for children whose mother did not reach high school and one for children whose mother reached high school.

A Figures

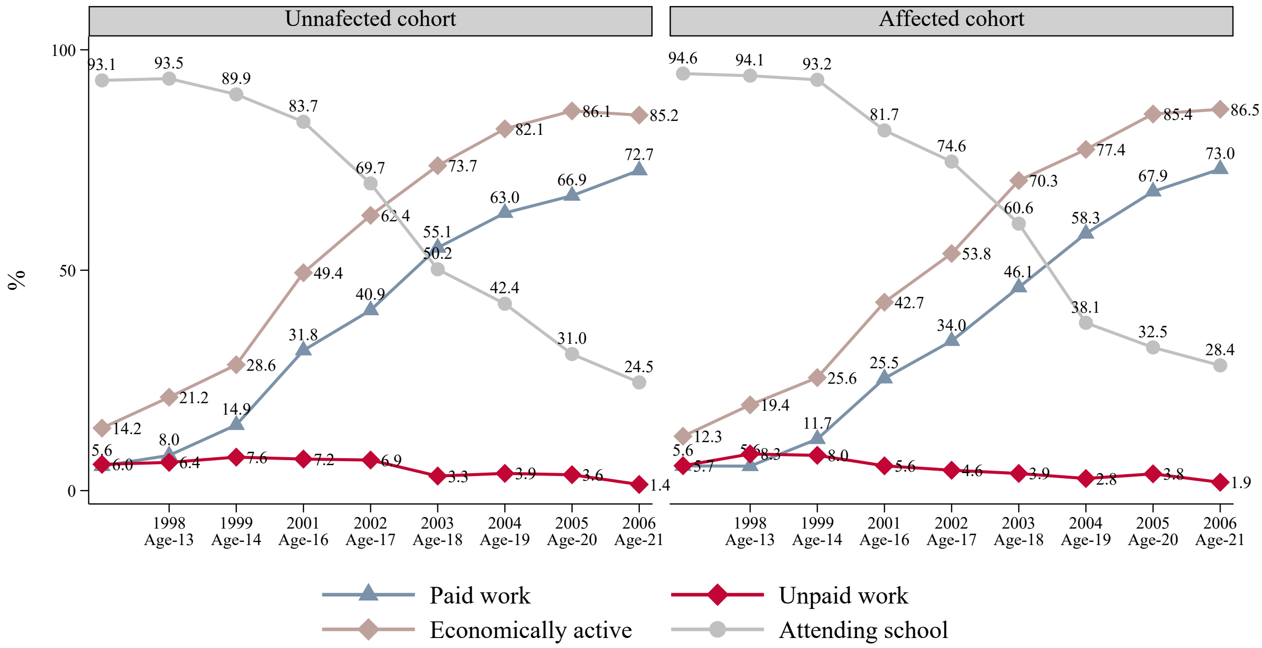
Figure A.1: Incidence of paid work in formal and informal sectors (1999)



(a) 10-week bandwidth (b) 14-week bandwidth

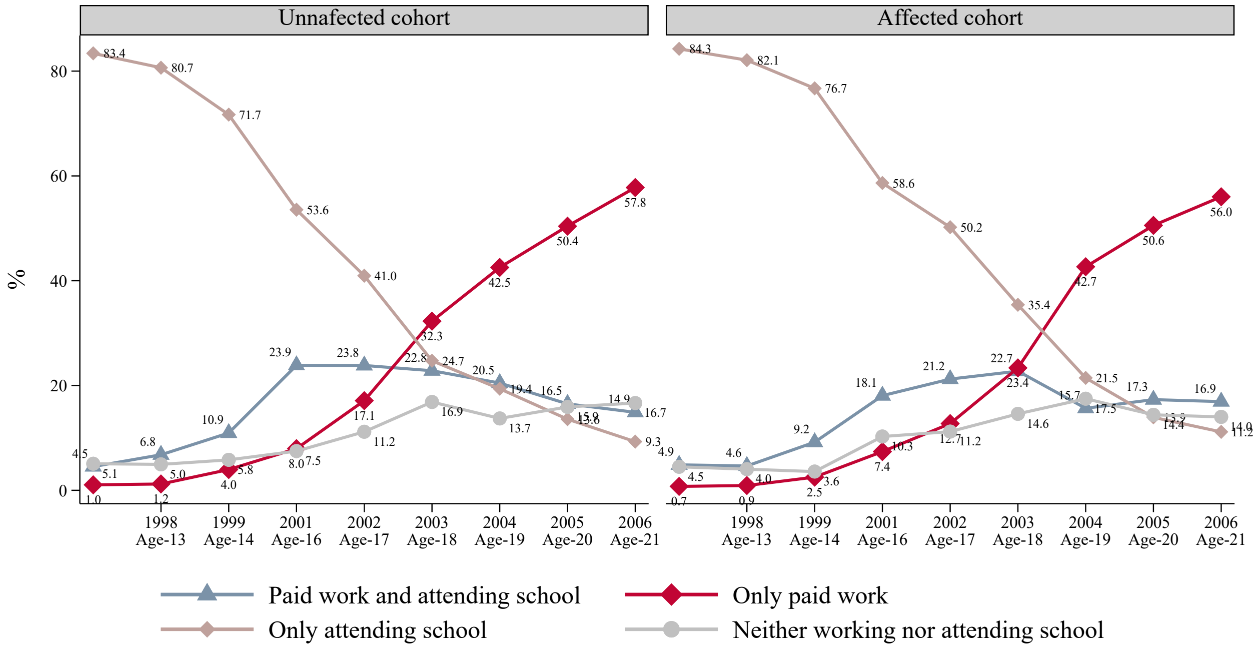
Note: PNAD 1999. 14-year-old urban boys.

Figure A.2: Labor and schooling for affected and unaffected cohorts, 10-week bandwidth (1998-2006)



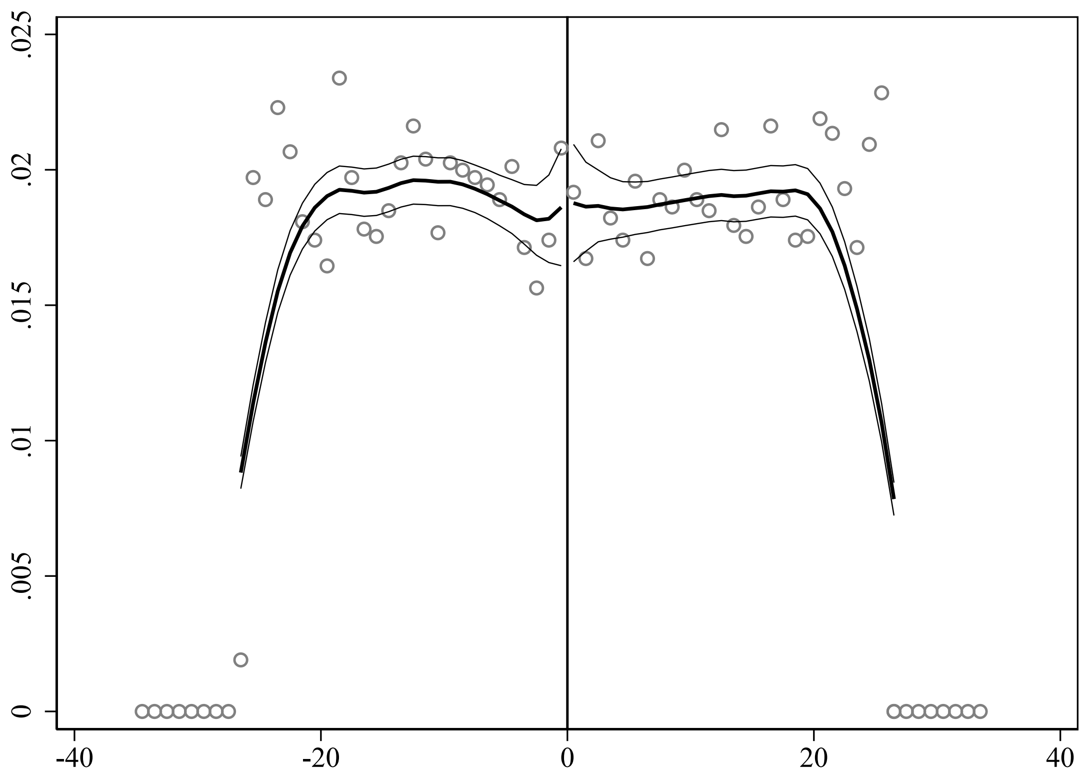
Source: PNAD 1997-2006. 14-year-old urban boys.

Figure A.3: Combination of labor and schooling for affected and unaffected cohorts, 10-week bandwidth (1998-2006)



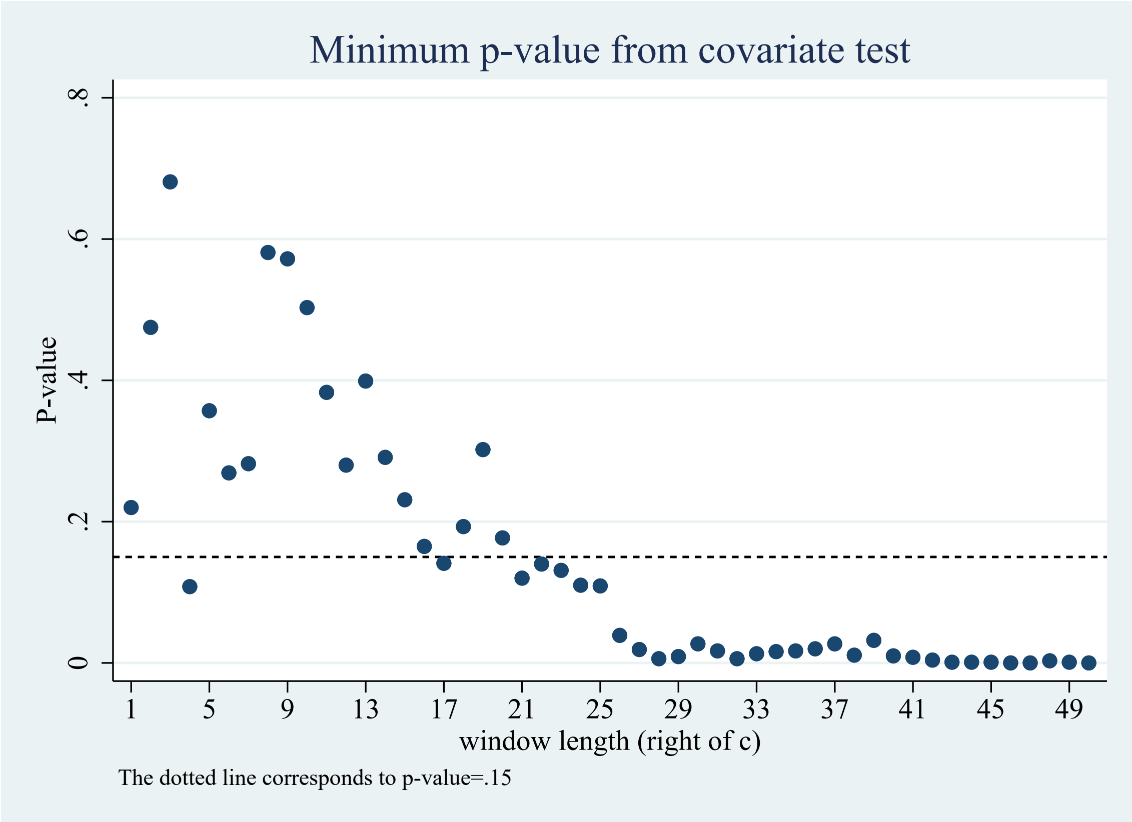
Source: PNAD 1997-2006. 14-year-old urban boys.

Figure A.4: McCrary Density Test, six-month bandwidth (1999)



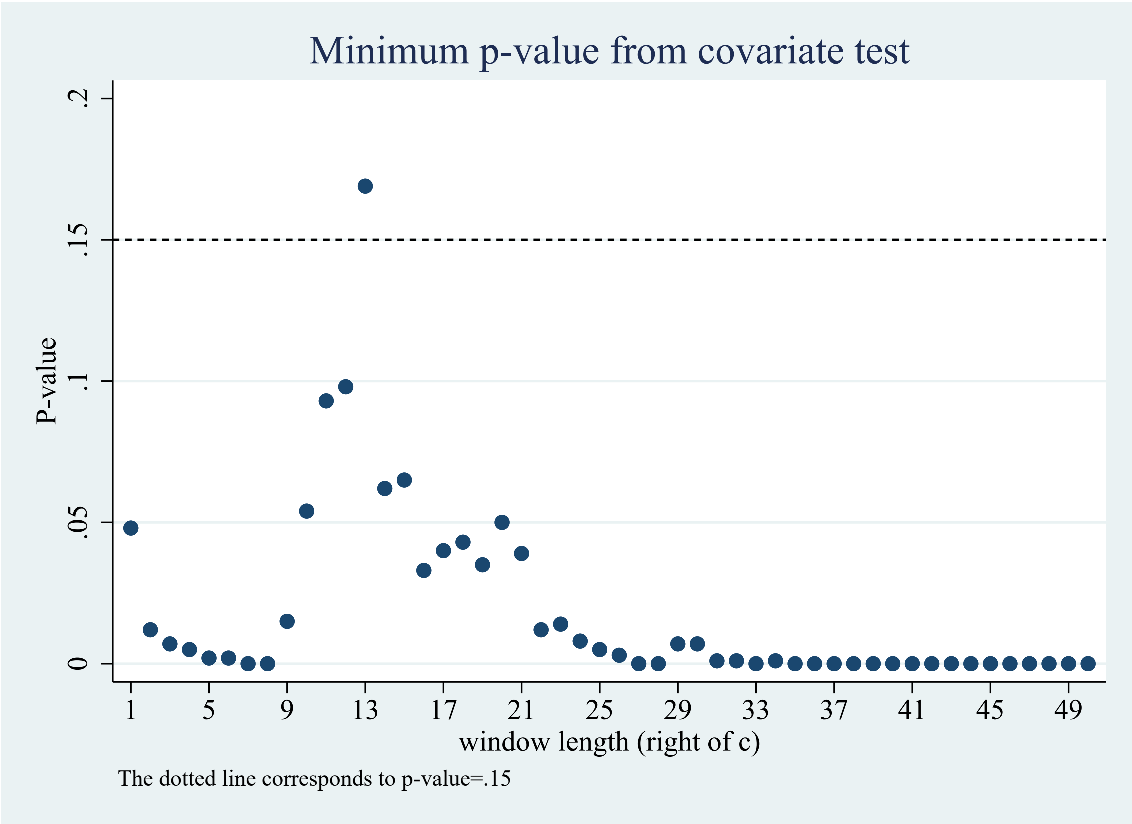
Note: The x-axis shows the age difference from the cutoff (in weeks).

Figure A.5: Estimation of optimal bandwidth (1999)



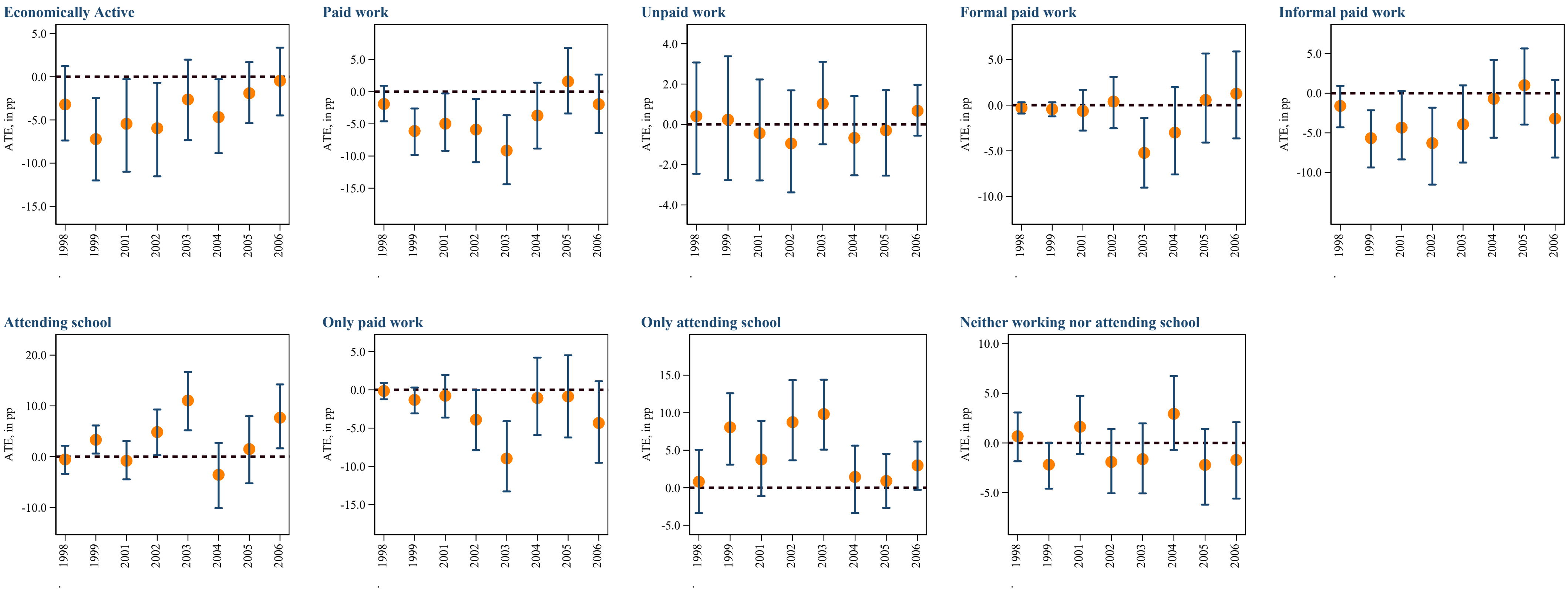
Note: Stata command *rdwinselect* by Cattaneo et al. (2016). The code runs a test of difference in means and the p-values are calculated according to Fisherian inference. The covariates used in the estimation are: mother's years of schooling, age and years of schooling of the head of the household, and household size. The x-axis show the number of weeks between date of birth and December 16, 1984.

Figure A.6: Estimation of optimal bandwidth (1998)



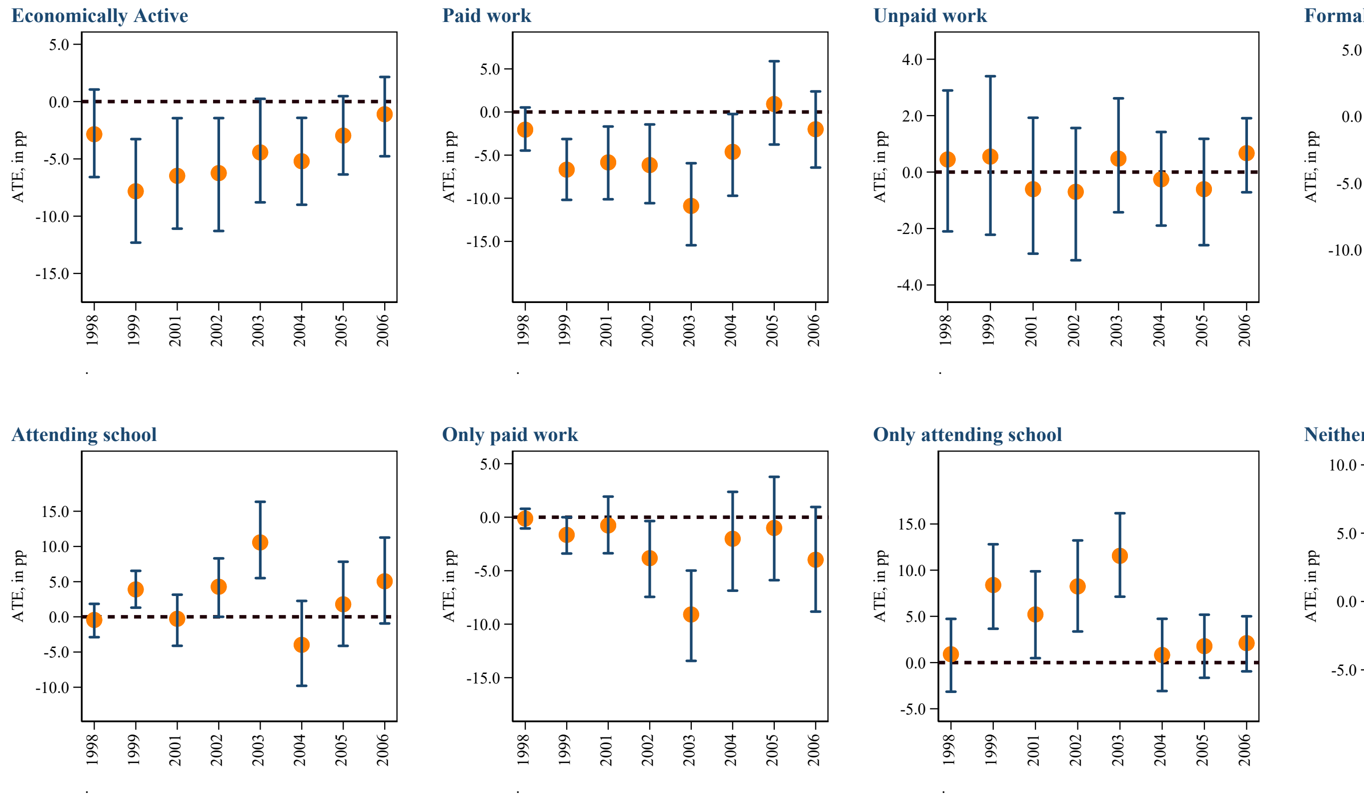
Note: Stata command *rdwinselect* by Cattaneo et al. (2016). The code runs a test of difference in means and the p-values are calculated according to Fisherian inference. The covariates used in the estimation are: mother's years of schooling, age and years of schooling of the head of the household, and household size. The x-axis show the number of weeks between date of birth and December 16, 1983.

Figure A.7: Persistence of short-term effects of the ban, boys in urban areas (12-week bandwidth))



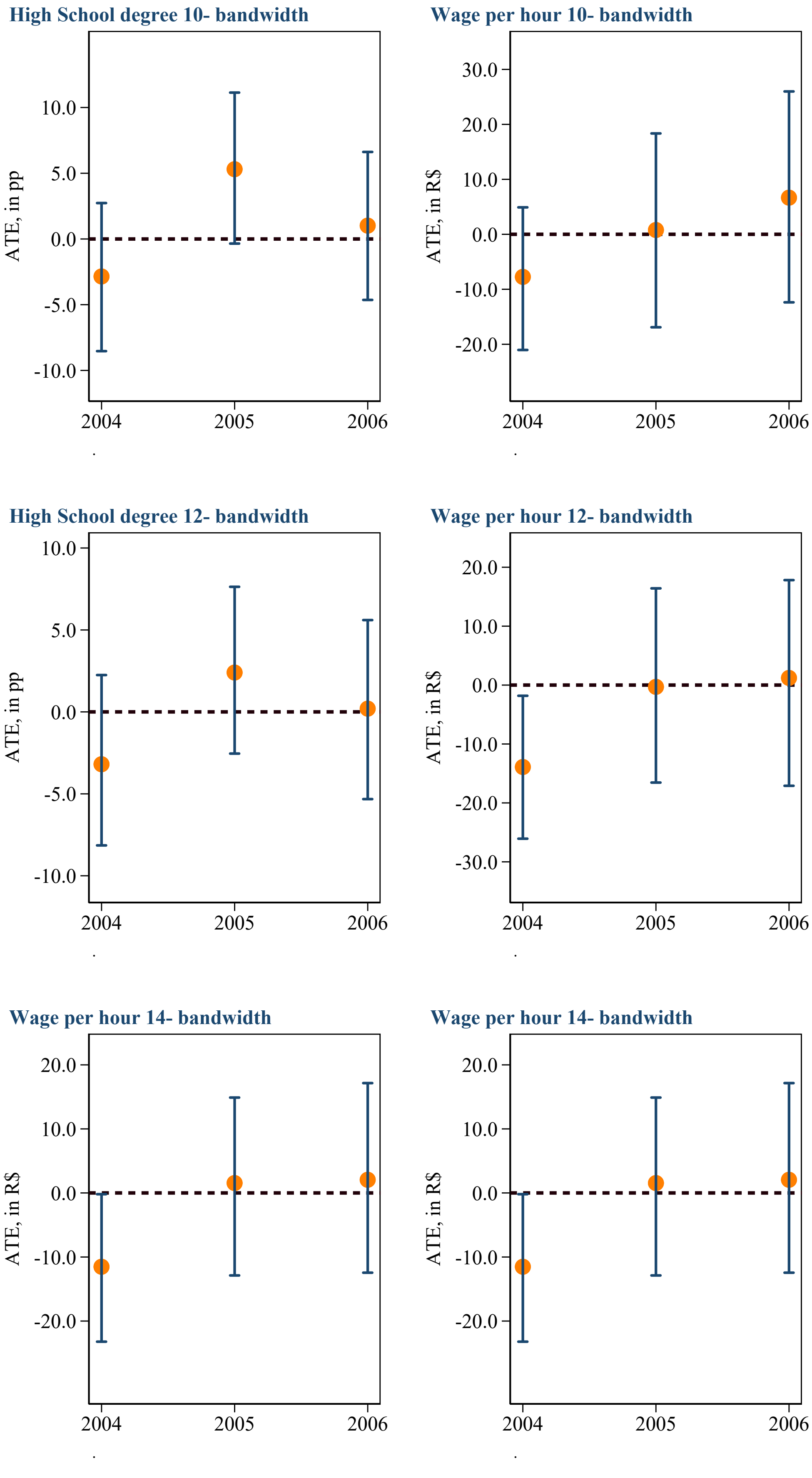
Note: 1998-2006 PNAD waves. Local Randomization in which the running variable is the number of weeks between the date of birth and the cutoff (December 16, 1984). Constant effect model (polynomial of order 0). We followed the affected and unaffected cohorts from 1998 to 2006, that is, from 13 to 21 years old.

Figure A.8: Persistence of short-term effects of the ban, boys in urban areas (14-week bandwidth))



Note: 1998-2006 PNAD waves. Local Randomization in which the running variable is the number of weeks between the date of birth and the cutoff (December 16, 1984). Constant effect model (polynomial of order 0). We followed the affected and unaffected cohorts from 1998 to 2006, that is, from 13 to 21 years old.

Figure A.9: High school degree and wage per hour



Note: 1998-2006 PNAD waves. Local Randomization in which the running variable is the number of weeks between the date of birth and the cutoff (December 16, 1984). Constant effect model (polynomial of order 0). We followed the affected and unaffected cohorts from 1998 to 2006, that is, from 13 to 21 years old.