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Short- and Long-Term Effects of a Child-Labor Ban

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Abstract

This is the first study to investigate the short- and long-term causal effects of a child-labor ban. The study explores the law that increased the minimum employment age from 14 to 16 in Brazil in 1998, and uncovers its impact on time allocated to schooling and work in the short term and on school attainment and labor market outcomes in the long term. The analysis uses cross-sectional data from 1998 to 2014, and applies a fuzzy regression discontinuity design to estimate the impact of the ban at different points of individuals' lifecycles. The estimates show that the ban reduced the incidence of boys in paid work activities by 4 percentage points or 27 percent. The study finds that the fall in child labor is mostly explained by the change in the proportions of boys working for pay and studying, and observes an increase in the proportion of boys only studying as a consequence. The results suggest that the ban reduced boys' participation in the labor force. The study follows the same cohort affected by the ban over the years, and finds that the short-term effects persisted until 2003 when the boys turned 18. The study pooled data from 2007 to 2014 to check whether the ban affected individuals' stock of human capital and labor market outcomes. The estimates suggest that the ban did not have long-term effects for the whole cohort, but found some indication that it did negatively affect the log earnings of individuals at the lower tail of the earnings distribution.

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1. Introduction

According to the International Labor Organization (ILO), child labor has declined worldwide in the past 15 years. But the numbers are still alarming. In 2012, 168 million children ages 5 to 17 years were in child labor. More than half of them, 85 million, were in hazardous work.

Labor laws and regulation are the first-order policy instruments advocated by international organizations such as the ILO and used by many national governments worldwide to fight child labor. Indeed, ILO Convention 138 on the Minimum Age for Admission to Employment of 1973 and ILO Convention 182 on the Prohibition and Immediate Action of the Worst Forms of Child Labor of 1999 have been ratified by 168 and 180 countries, respectively, at the time of writing. Convention 138 recommends that national laws should set the minimum employment age above 15. Convention 182 defines an individual under age 18 as a child. It lists the types of occupations that should be restricted until age 18 and defines the worst forms of child labor that should be prohibited in all circumstances.

There is a consensus that very early exposure to work is harmful to a child's human development. In fact, most countries have put in place laws prohibiting work for children under a certain age. However, there is not much agreement on the minimum age at which a child should be allowed to work.

For instance, of the 62 mostly developing countries with publicly available data on child labor from the second and third rounds of UNICEF´s Multiple Indicator Cluster Surveys (MICS) for the 2000s, two countries set the minimum employment age at 12, 23 at age 14, 21 at age 15, 11 at age 16, and five countries at age 18 (Edmonds and Shrestha, 2012).

In the United States, the *Fair Labor Standard Act* (FLSA) uses age 14 as a general rule for the minimum employment age, but that does not prevent significant variation in regulations across states. According to the Department of Labor, 18 states require employment certificates from employers who hire individuals under age 18; one state requires a certificate for individuals under age 17; 21 for under age 16, and one for under age 14. Twelve states either do not require or do not issue employment certificates.[1]

[1] See http://www.dol.gov/whd/state/certification.htm.

Even though legislators and policy makers have urged for child-labor laws as a way to reduce child labor worldwide, evidence of such laws reducing child labor remains scant, in particular for developing countries. Our study aims to fill part of this gap.

We use the increase in minimum employment age from 14 to 16 in 1998 in Brazil as a natural experiment with the intention to contribute to filling various gaps in the empirical literature on child-labor bans and add new evidence of the growing literature on the long-term effects of interventions targeted at youth.

The change in minimum working age gave rise to a natural experiment, as individuals' exact dates of birth at the time the law changed defined their eligibility to participate in the formal labor force. By delving into the consequences of this change in the law, we will see whether preventing individuals from entering the formal labor force for two more years (from 14 to 16) has long-term consequences for educational achievements and labor-market outcomes. The research objective is to investigate the effect of early exposure to the labor market on long-term outcomes. This question parallels the literature on the impact of youth employment on an individual's long-term outcomes.

By preventing individuals aged 14 from entering the formal labor force, the ban ended up reducing individuals' choice-sets as no compensating policy, such as conditional cash-transfer programs or apprenticeship programs, was put into effect at that time.[2] Consequently, the banned cohort was left with the options of accumulating human capital in the informal labor force and/or in low-quality public schools. This paper investigates the empirical matter of whether that policy paid off in the short and long terms.

Unlike previous studies that used a difference-in-differences approach, this is the first empirical study to use regression-discontinuity design (RDD), exploiting a discontinuity at some arbitrary age cut-off. And unlike most empirical studies that looked at the impact of child-labor bans on employment rates, we consider a broader set of outcomes and run the analysis in the short and long terms. To our knowledge, this is the first study to perform such analysis of a child-labor ban.

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

Finally, we use several years of household surveys to investigate dynamic effects in the short and long terms and perform heterogeneous analysis to shed some light on the mechanisms underlying the main results.

Most studies that use dates of birth to estimate the long-term effects of a law or intervention, focus on the impact of early school enrollment. The literature outlines the educational channel as the main mechanism linking dates of birth to labor-market outcomes (see for example, Angrist and Krueger 1991; Bedard and Dhuey 2006; Black et al. 2011). A few empirical papers provide causal estimates for long-term effects of youth employment (or child labor) and outline the potential experience in the labor market as the plausible mechanism. This study therefore helps fill this gap in the empirical literature.[3]

It is documented elsewhere (Piza and Souza, 2016) that the 1998 child-labor ban reduced labor-force participation rates of boys but not girls in the short term. This is so because the labor-market participation of girls was already low. They show that the decrease in participation rates of boys is driven by a fall in formal and informal work. Thus, Piza and Souza's (2016) results suggest that the law was reasonably well enforced among males. In this paper, we will therefore concentrate only on the sample of males.[4]

We find that, in the short term, the child-labor ban reduced the proportion of boys doing paid work and studying and increased the proportion of boys only studying. We also found that the ban reduced participation rates in the labor force and not just employment rates. We observe some persistence in these results until the affected cohort is age 18.

Our long-term analysis follows males between ages 22 and 29. Our estimates suggest that the ban did not have long-lasting effects on schooling attainment and labor-market outcomes of the affected cohort. We use distributional analysis in the long term to shed light on the potential impact of the ban on the subgroup of boys more likely to drop out from the labor force. Quantile treatment effect estimates suggest the law had a negative effect in the first quintile of log earnings distribution in the long term. We

[3] There is some evidence of the impact of vocational training on labor-market outcomes (for example, Hanushek et al. 2015). The question addressed in this paper is different, as it aims to uncover the impact of formally hindering labor-market participation of 14-year-olds for up to two years.

[4] Our short-term RDD estimates confirmed Piza and Souza's (2016) findings for girls and are available on request.

interpret this estimate on log earnings as the foregone returns on experience in the formal labor market among those with potentially lower earnings.

Two points need to be emphasized. First, the results are valid for the cohort born in the first half of 1985 and turned 14 in the first half of 1999. In other words, the results and conclusions cannot be extrapolated to different age groups or cohorts. Second, one should not read the results as an implicit advocacy for child labor but as a cautionary note that laws should be designed according to the context and compensatory policies such as conditional cash-transfer programs and complementary policies such as apprenticeship programs, vocational training, or appropriate educational policies should be in place to broaden opportunities for those negatively affected by the ban.

This paper is organized as follows. The second section discusses related literature. The institutional setting and intervention are discussed in the third section. The fourth section presents the empirical strategy and the fifth describes the data. The sixth and seventh sections present the short and long-term results, respectively. The eighth section discusses the main results and outlines some policy implications. We then conclude.

2. Related Literature

The literature on child-labor bans is mostly theoretical, with the main theoretical predictions being the following: (1) poverty and imperfect credit markets are the main drivers of parental decisions to send their children to work (Basu and Van 1998; Ranjan 1999 and 2001; Baland and Robinson 2000; Horowitz and Wang 2004; Dessy and Pallage 2001), (2) under some circumstances, though not always, a ban on child labor can move the economy to an equilibrium without child labor (Basu and Van 1998; Baland and Robinson 2000), and (3) a ban policy can potentially harm the poorest households, which rely on children's incomes (Baland and Robinson 2000; Horowitz and Wang 2004; Basu 2005; Dessy and Knowles 2008).[5]

All these previous studies treated the child-labor ban as an exogenous change. Doepke and Zilibotti (2005), on the other hand, develop a model where child-labor ban

[5] See Edmonds (2008) for a comprehensive discussion of the child-labor literature. There is suggestive evidence from Brazil showing that there are other determinant factors for child labor over and above poverty; such as parental preferences for early exposure to labor markets (Emerson and Souza 2003).

is endogenously determined. Families are prone to support child-labor bans in situations where children compete with adult workers in the labor market and children's incomes are not a large share of the family income. Otherwise, families would be against child-labor bans. Fertility decisions lock families into the child-labor equilibrium and child-labor bans can be triggered if some exogenous change induces parents to have smaller families; such as skill-biased technology changes.[6] They predict that even in countries that initially have strong opposition to child-labor restrictions, the support for child-labor bans may increase over time once the ban is in place. This happens if the cost of schooling is sufficiently low and the value of child work (opportunity cost) is not too high. If these two conditions are not met, families and children themselves would be worse off.

The evidence of consequences of child labor has increased in the last 15 years or so. Studies have found that child labor can harm individuals on different dimensions. Tyler (2003) used U.S. data from the 1980s and found that working while studying is detrimental to learning among high-school students. Bezerra et al. (2009) and Emerson, Ponczek, and Souza (2016) reached similar conclusions for Brazil.

Using data from Brazil, Emerson and Souza (2011) show that very early entry into the labor market harms individuals' outcomes in adult life, over and above the effect on schooling. However, this negative effect is reversed when entry into the labor market occurs after age 14.

Also for Brazil, Lee and Orazem (2010) found that a simultaneous effect of an early entrance into the labor force and a premature school dropout resulted in worse health outcomes.[7]

Beegle et al. (2009) investigated the medium-term consequences of child labor on schooling, the labor-market, and health outcomes in rural Vietnam and found child labor had a negative effect on school attendance and educational attainment but a positive effect on labor-market outcomes such as employability in paid work and earnings. They argue, that for some individuals, the returns on experience seem to overcome the returns on education.[8] This result is consistent with some theoretical

[6] 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.
[7] Such as higher probability of back problems, arthritis, and reduced stamina.
[8] Beegle et al. (2009) also found no impact on health outcomes.

models and suggests that some children would be better off if allowed to accumulate human capital through on-the-job training rather than in low-quality schools.

There is much less evidence on the consequences of child-labor laws. The evidence of the impact of child-labor laws can be divided between studies that assessed the effect of compulsory schooling laws (CSL), studies that evaluate the impact of minimum employment ages (MEA), and studies that looked at a combination of both laws. The number of studies that paid attention to the impact of MEA is limited. What is known about the consequences of such laws is limited almost exclusively to the U.S. experience during the first three decades of the last century (Moheling 1999; Margo and Finegan 1996; Lleras-Muney 2002; Manacorda 2006). Except Manacorda (2006), who looked at the impact of the minimum legal-age legislation in the U.S. on time allocation of other household members, the literature focused exclusively on one outcome (employment rates) and results point to a small effect of the MEA on child labor.

Some authors explore the combination of CSL and MEA to assess whether they are an effective way to fight child labor (Margo and Finegan 1996; Lleras-Muney 2002; Goldin and Katz 2008). In fact, Margo and Finegan (1996) and Lleras-Muney (2002) found a stronger fall in child labor after combining compulsory schooling and child-labor laws.

The evidence of the impact of child-labor bans in developing countries is almost non-existent. The only three studies we are aware of that investigate the impact of MEA legislation in developing countries are Piza and Souza (2016), Bharadwaj et al. (2013), and Edmonds and Shrestha (2012).

Piza and Souza (2016) found that an increase in the minimum legal age in Brazil from 14 to 16 in December 1998 more than halved the labor-force participation rates of boys but had no effect on girls. Curiously, they show that the fall in participation rate of boys was mostly explained by a fall in participation rates in the informal sector.

Bharadwaj et al. (2013) investigate the effectiveness of the child-labor ban in India through the Child Labour Act of 1986 that set the minimum legal age of entry into the labor market at 14. Their findings suggest that the law increased child labor 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) investigate the impact of MEA on child labor and schooling using micro-data from around 60 countries, mostly low-income countries. They found no short-term effects of enforcement of MEA in all countries but one.

The evidence of long-term consequences of child-labor laws is also very limited. Most of what is known has used CSL as an instrumental variable to estimate returns on education (Angrist and Krueger 1991; Oreopoulos 2006a and 2006b). As far as we are concerned, this is the first study to look at long-term effects of a child-labor ban. Depending on how the problem is framed—either as an earlier or later exposure to the labor market—some of the findings discussed in this paper can contribute specifically to the literature on returns to experience (Imbens and van der Klaauw 1995; Connolly and Gottschalk 2006; Grooger 2009; Angrist 1990; Angrist and Krueger, 1994; Angrist et al. 2011).

In fact, this paper relates more broadly to the growing literature on long-term effects of interventions designed for the youth groups. A great deal of these studies look at educational policies such as Dustmann et al (2012) on high-school quality, Deming et al (2013) on high-school accountability, Chetty et al (2014) on teacher quality, Lavy (2015a) on school choice, and Lavy (2015b) on teacher pay-for-performance. Others assess the impact of either youth training or vocational education on labor-market outcomes (Card et al. 2011; Hicks et al. 2013; Hirshleifer et al. 2013; Ripani et al. 2015; Attanasio et al. 2015) or 'remedying' interventions targeted at disadvantaged children (Angrist et al. 2006). The evidence that comes out of these studies point to a long-term effect on education attainment or labor-market outcomes. Our study adds new evidence to this body of the literature by evaluating the long-term effects of an active labor-market policy aimed at young people that acts through a channel underexplored so far—restricting the choice-set of time allocation of the youth.

3. The Institutional Setting and the Intervention

The Brazilian Constitution of 1988 set the minimum legal age of entry into the labor market at 14. In 1990, a federal rule named 'The Statute of Children and Adolescents'[9] established children and youth rights beyond regulating the conditions of formal labor-market entry. Complementing the Constitution of 1988, the statute is

[9] *Lei do Estatuto e do Adolescente*, Law No. 8069 from July 13, 1990.

considered the legal framework for children and youth in the labor market.[10] From 1988 to November 1998, the minimum legal working age in Brazil was 14 and individuals under 17 were prohibited from working in hazardous activities.

As a consequence of comprehensive modifications approved for the pension system on December 15, 1998, Constitutional Amendment No. 20 also increased the minimum legal age of entry into the labor market from 14 to 16. The law was passed on December 15 and was made effective the following day. According to the law, individuals under 16 could work only as apprentices, though the regulations for apprenticeships occurred only later, at the end of 2000.[11] Individuals younger than 18 were prohibited from hazardous and nightshift work.

The real motivation for raising the minimum employment age is not spelled out in the law, but the two main reasons seem to be (i) the change in retirement age based on time of contribution to the pension system, which increased by two years due to the Constitutional Amendment,[12] and (ii) Brazil was in a process of ratifying the ILO Convention No. 138, and by doing so Brazil agreed to set the minimum employment age above the usual school-leaving age, which used to be 14 for those not delayed by the time the law passed.[13]

The Constitutional Amendment of December 15, 1998 itself does not talk about penalties for those who employ children below the minimum age. However, according to a recent report commissioned by the Brazilian Public Prosecutor's Office (PPO or *Ministério Público Federal* in Portuguese), the institution responsible for monitoring the practice of child labor in Brazil, employers (including parents in case the child works

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

[11] The Law No. 10.097 of 19 December 2000. In fact, before this apprenticeship law was enacted, the apprentice eligibility status was dubious. According to the Brazilian Constitution, the apprenticeship program was available for youth aged 12 to 14 before the increase in the legal minimum working age. When the child labor ban was extended to age 16, it was not clear if apprenticeship was allowed for 14-year-olds. Still, for our purpose, if the apprenticeship remained an alternative to youth entering the formal labor force at age 14 in 1999, the apprenticeship program should have a common effect on the eligible and ineligible cohorts. The impact of a ban could have been further attenuated had the number of 14-year-old apprentices been high. Indeed, the take up was extremely low. Corseuil et al. (2012) show that the total number of 14-year-old apprentices in Brazil in 1999 was 82 individuals only.

[12] In Brazil there are two retirement mechanisms, an age cut-off and the time contributed to the pension system. Because many Brazilians start working early in life, they end up retiring relatively early. 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 year later.

[13] At that time, primary school was mandatory in Brazil. Thus, that rule applied only for individuals not delayed in school, that is, individuals enrolled it the right grade for a given age. Even now, Brazil does not have an official school-leaving age cut-off.

for a family firm) can face several forms of penalties, ranging from fines and other administrative costs to criminal prosecution, depending on the type of work performed by the child. Children below the minimum employment age are not allowed to be self-employed either. Note that the severity of the punishments for employers is greater for hiring a child worker than hiring an adult informal worker.[14]

Parents can be penalized in two ways. First, they can lose guardianship of their children and, second, they can be prosecuted depending on the work performed by the child. It is important to emphasize that in neither case is the child subjected to any sort of penalty. The ultimate goal of the law is to protect the child (see Medeiros Neto, 2013).

One can question the enforceability of such a law in a country where informal activities are widespread. In the formal sector, the enforceability of the law is almost deterministic. The Ministry of Labor and Employment is the institution responsible for issuing the working permits. With the change in the law, it stopped issuing work permits to individuals who turned 14 after the law passed. Thus, the ban policy split similar children into two groups: those banned from the formal labor force (treatment group) and those unaffected by the ban (control group). Indeed, as shown below, while the law was perfectly enforced in the formal sector, it was less so in the informal sector.

If the law had no effect on the informal sector, its effect on child labor would have been very small as the formal labor participation was around 1 to 2 percentage points before the law change. If some children in the formal sector simply shifted to the informal sector after the ban, the law's effect on child labor would have been negligible or even positive, if increase in the informal sector exceeded fall in the formal sector. In fact, Piza and Souza (2016) show that the decrease in the incidence of child labor was mostly driven by the fall in informal work. It seems some employers decided to no longer employ children under 16 to avoid legal consequences. It is reasonable to assume that the cost of verification of child-labor practices in firms is lower than the cost of verification of any other type of informal labor contracts.

The Federal Constitution of 1988, the Law No. 8069 from July 13, 1990 *(Lei do Estatuto e do Adolescente)*, and Law No. 9394 from December 20, 1996 *(Lei das*

[14] In Brazil, all formal labor contracts are recorded in the worker register card called *carteira de trabalho*. This document is issued by the Ministry of Labor and Employment. A formal worker has access to social pension and other benefits (for example, vacation) other than market-rate wages. An informal worker is just paid earnings and thus has no work history formally recorded.

Diretrizes e Bases da Educação Nacional) are the main legal frameworks that established the educational parameters to be followed for the cohorts we analyze in this paper. According to these laws, (i) primary education (first to eighth grades at that time) was mandatory and should be provided in public schools free of charge, (ii) children should start school at age 7, and (iii) the parents or guardians of the child were obliged to enroll the children in school by the time they were age 7.[15] It implies that a child in the correct grade should start school at age 7 and would be allowed to leave school after age 14. Notice that the law states that a child should stay in school until primary education is finished. In case it was delayed, the child should stay till graduation from primary school, regardless of age.

The compulsory school legislation could be seen as a potential confounding factor to our identification strategy. The birth-date cut-off adopted by the school system to determine that a child turned age 7 and could enroll in school in a given year could create a discontinuity around the same cut-off used to identify the effects of the child-labor ban.

If school enrollment and attendance is no longer mandatory for the slightly older cohort but is still made mandatory for the immediate younger cohort, and school attendance is negatively correlated with child labor, we will observe a discontinuity around the same cut-off that we attribute to the child-labor ban. But that could partially be explained by the compulsory-schooling law. 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 is further argued in section 6.1, we are confident the discontinuity we observe is due to the child-labor ban because (1) the school system in Brazil is highly decentralized and each system uses different cut-off birth-dates to allow the enrollment of students in the initial grade; (2) the student must stay in school until they complete primary education. Since delay was pervasive at that time, a relatively small fraction of 14-year-olds were no longer obliged to stay in school; and (3) a series of placebo tests using cohorts, either non-affected or equally affected by the child-labor ban, found no discontinuities in any of the outcomes of interest.

[15] Later, there were modifications of these parameters. The Laws 11.114/2005 and 11.274/2006 mandated nine years of primary education starting at age 6, and the Constitutional Amendment No. 59 of 2009 established mandatory primary and secondary schooling and compulsory age from 4 to 17 years old.

4. Empirical Strategy

Our identification strategy relies on the individuals' exact dates of birth at the time of the change in the law.[16] The change of the minimum legal working age in December 1998 affected only those who turned 14 from January 1999 onwards. The analysis of the long-term effects of the law on individual outcomes consists of comparing the cohorts who turned 14 in the second semester of 1998 (born in the second semester of 1984) with individuals who turned 14 in the first semester of 1999 (born in the first semester of 1985). However, unlike Angrist and Krueger (1991) and many other authors who combine birth-date with school entry or exit ages, parents could not have anticipated this law change and its effects.[17]

We carry out both non-parametric and parametric estimations (see Hahn et al. 2001; Imbens and Lee 2008; Lee and Lemieux 2010) to assess the impact of the ban. We show local linear regression results graphically only, but parametric regressions confirm what is suggested visually.

We estimate the following reduced-form regression model to obtain short and long-term estimates of the ban:

$$y_{ict} = \alpha + \rho D_{ic} + h(Z_{ic} - \bar{Z}) + \beta X_{ict}^{'} + u_{ict} \qquad (1)$$

where y_{ict} is the outcome of individual *i* in cohort *c* in time *t*, D_{ic} is a dummy that takes on the value of 1 if the individual belongs to the affected cohort c, that is, those who were born in 1985 and turned 14 in 1999 and thus were hindered from participating in the formal labor market due to the ban; and 0 for those born in 1984 and turned 14 in 1998 and were thus allowed to participate. The function *h(.)* depends on age, the forcing variable, and will be referred to as the 'smooth function'. We use linear and quadratic polynomials and piecewise linear specifications. Following Gelman and Imbens (2014), we do not fit a higher-order polynomial to avoid assigning much weight to observations far away from the threshold.

[16] Unlike Angrist and Krueger (1991) and other authors (for example, Bedard and Dhuey 2006) who combine birth-date with school entry or exit ages, parents could not have anticipated this change in law and its effects. (1991)

[17] For a similar identification strategy, see, for instance, Smith (2009), McCrary and Royer (2011), and Black et al. (2011). Oreopoulos (2006b), Dickens et al. (2014) and Lavy (2015a and 2015b).

The forcing variable age, Z_{ic}, is defined in weeks and the cut-off $\bar{Z}$ is the last week of December 1998. Thus, Z_{ic} takes the value of 1 for the first week of January 1999, 2 for the second week, and so on. Analogously, Z_{ic} takes the value of -1 for the third week of December 1998, -2 for the second week, and so on. X_{ict} is a vector of covariates that includes mother's education in the main specification. We also estimate an alternative specification that includes the additional controls of father's years of schooling, mother's and father's age, and household size; and u_{ict} is the error term.

For the short-term effects, we use the surveys of 1999 and 2001. We also use surveys from 1998 to 2006 to check robustness as well as the persistence of the effect of the ban. In the short-term analysis, we used the following outcomes: work incidence in paid and unpaid activities, school attendance, and proportion of children 'only working (in paid activities and unpaid activities)', 'only studying (attending school)', 'working (in paid and unpaid activities) and studying', and 'neither working nor studying'. For binary outcomes, we estimate linear probability models.

For the long-term effects, we pooled the surveys from 2007 to 2014 to look at education attainment and labor-market outcomes when the individuals of the affected cohort were aged 22 to 29. We selected the following outcomes: the likelihood of having completed at least high school, the likelihood of holding a college degree, the likelihood of being employed, the likelihood of being employed in the formal sector, and the log of monthly earnings.

We estimate the effects for each year individually and then pool all years to improve efficiency. When doing that, we added survey-year dummies to Eq. (1) to control for both cohort composition and time effects. This approach is similar to Angrist et al. (2011), Lemieux and Milligan (2008), Dickens et al. (2014), and Lavy (2015a).

The parameter of interest, ρ, corresponds to the *intent-to-treat* (ITT) since the analysis is performed for all individuals who belong to the cohort affected by the law. The identification of this parameter depends on the variations in the labor force participation rates of some 14-year-olds in the first half of 1999 driven by the exogenous change in the law. If the law of December 1998 led to a change in labor-force participation, then the outcomes of the cohort who were 14 years old just before December 1998 can be used as counterfactual for the cohort who turned 14 just after the law passed. A key issue in our identification strategy has to do with whether children aged 14 or 15 at the time of the law change would remain eligible to work in the formal sector or that would be conditional on them already working and/or having a working

permit.[18] Unfortunately, the law of 1998 is not clear about this issue and we cannot observe in our data if that was the case or not. Figure C.2 in the appendix shows that in 1999 the formalization rate was small but positive among the control cohort and null among the eligible cohort. In one of our placebo tests, we use December 1997 as cutoff and find statistically significant difference in formalization rates between children who turned age 14 before and after December 1997.[19]

To check robustness, eq. (1) is estimated with two different bandwidth sizes, six and nine months. We use nine-month bandwidth to perform analysis when only one cross-sectional data is used. Estimates with six-month bandwidth are provided when several waves of the survey data are pooled. We perform two placebo tests in the short term and one in the long term to check robustness.

5. Data

We use several years of the Brazilian household survey PNAD. Data from 1999 and 2001 are used for descriptive statistics and short-term estimates. Data from 1998 are used in one of the placebo analyses. We use all PNADs from 2001 to 2014 to look both at persistency effects in the outcomes used in the short-term analysis and at trajectories of the outcomes used in the long-term analysis.

IBGE has annually conducted the PNAD since the late 1970s. It covers around 100,000 households and 320,000 individuals. The survey is conducted between October and December each year and constitutes one of the main sources of microdata in Brazil.[20] The PNAD is nationally representative, containing information on household socio-economic characteristics, demographic data, household sources of income, and labor-force status.

The sample of interest is given by two cohorts of individuals aged 14 by the time the law passed. The first cohort, which we consider the comparison group, includes individuals who turned 14 before the ban of December 1998. The second cohort, defined as the eligible group, consists of individuals who turned 14 after the change in the minimum legal age. These two cohorts are compared from ages 13 to 29.

[18] We would like to thank an anonymous referee for pointing this out.
[19] In a very short paper Piza and Souza (2016) combined RDD with difference-in-differences methods to control for cohort effects. The results are almost identical to the ones we found for incidence in paid work activities.
[20] The survey documents provide the month (September), week (last of the month) and day (usually 27th) as the period of reference for the Brazilian PNAD.

The empirical analysis is performed in urban areas because rural areas are under-represented in the PNADs, the law may not be fully enforced in rural areas, and because rural areas lack well-developed school systems and labor markets. According to the 1999 PNAD, around 82 percent of Brazil's population lives in urban areas.

5.1. Descriptive Statistics

This section presents the descriptive statistics of the main sample used for the short-term analysis and some stylized facts of the unaffected cohort that turned 14 immediately before the law change.

Our main sample for the short-term analysis consists of all male individuals living in urban areas and had turned 14 between April 1998 and September 1999. The basic descriptive statistics for the 1999 PNAD sample of males are presented in table 1. There are 4549 observations, 47 percent of boys are white, about two-thirds of boys working (19 percent) were in paid activities, 92 percent were in school, and 15 percent worked while in school. The average years of schooling of mothers and fathers suggest they did not hold a primary education degree. The average household size is about five members.

The figures 1.a and 1.b depict the incidences of time allocation in school and work for boys and girls separately. The sample consists of all individuals in the comparison group, that is, those who turned 14 in the last nine months of 1998. The outcomes examined are work incidence in paid and unpaid work,[21] and school attendance.[22] We also look at these outcomes in six exclusive combinations: working only in paid activities, working only in unpaid activities, only studying, paid work and studying, unpaid work and studying, and neither working nor studying. None of the definitions above include household chores. We excluded household chores because these are performed predominantly by girls and our estimates focus only on boys.

Figure 1.a shows that 90 percent of 14-year-olds were enrolled in school, but boys were more likely to work (either in paid or unpaid) than girls.

Figure 1.b presents the share of those only working, only studying, or combining both activities. Most boys and girls were only studying, but girls were 10 percentage points more likely to do so. Very few boys and girls were only working, but the

[21] We use the PNAD question: Whether the individual worked last week (the week of reference)?
[22] The PNAD question used is: Do you attend school?

proportion of boys combining working with studying was twice as large compared to girls (18 versus 9 percent). About 6 to 7 percent of boys and girls were neither working nor studying.

6. Results: Short-Term Effects of the Ban of December 1998

This section presents the results for the short-term effects of the ban. By short term we mean the effects for a few months after the change in the law. To estimate the impact of the ban in September 1999, nine months after the increase in the minimum legal age, we use the PNAD of 1999. Given that those who turned 14 in 1999 would still be out of the formal labor force in 2000, we could pool the surveys of 1999 and 2000 for greater efficiency. However, the year of 2000 was a census year and the PNAD was not collected. We thus pool the years 1999 and 2001 and perform the analysis with a narrower bandwidth.

Visual Check: Local Linear Regressions

For the local linear regressions we use a twelve-month bandwidth. Estimates are obtained with a triangle kernel with a one-month bandwidth. The forcing variable is defined in months.[23] The cut-off value is defined at December 1998, that is, the forcing variable takes the value of 0 that month, 1 in January 1999, and so on. All figures show local linear regression with a 95 percent confidence interval.

Figures B.1 to B.9 are shown in the appendix. Figures B.1, B.4, and B.6 suggest that the proportion of boys doing paid work, only paid work, and paid work and studying, dropped. There is some strong indication that the proportion of those only studying increased. Based on these figures it seems that boys traded off work experience for more education. If that was a rationale behind the change in the law, it seems that it paid off at least in the very short term.

The parametric results discussed below confirm what is suggested by the local linear regressions.

[23] We followed Lee and Lemieux (2010) and used a size of bin that gives a good visualization of the potential local effect.

Parametric Regressions

Our main results are shown in tables 2a and 2b. We pooled the surveys of 1999 and 2001 and estimated the regressions with a six-month bandwidth.[24] Tables A.1 and A.2 in the appendix show only the results with the PNAD of 1999. The results are very similar, though less precise.

Tables 2a and 2b show the results with three different specifications of the smooth function, linear and quadratic polynomials, and piecewise linear. We add mothers' years of schooling as controls. In addition, the tables present the mean values of the outcomes of the comparison group.

Table 2a shows a decrease in paid-work incidence of 4.1 percentage points (p.p.) for all males or 27 percent. The result is very stable across the three specifications and is statistically significant at a 5-percent level. The same results are obtained when only using the 1999 PNAD and are shown in table A.1 in the appendix. For 1999 only, the child-labor decline is mostly driven by the decrease in paid formal work.

To shed more light on the time allocation in work and schooling activities, we look at the proportions only working, working and studying, and neither working and studying. We decompose working into working in paid and unpaid activities respectively. The results are shown in table 2.b.

There is fall of 3.4 p.p. (about 28 percent) in the proportion of boys doing paid work and studying and an increase of 5.6 p.p. (about 8 percent) in the proportion of boys only studying. Both these results are statistically significant, different from 0 at 5 percent. It seems that some affected boys returned to school after the ban. Although not shown in the table, we estimate that the affected cohort is 2 p.p. more likely to be delayed in school just after the ban. Given that 45 percent of 14-year-old boys were delayed in school in 1999, we do not have enough statistical power to detect an impact on age-grade distortion statistically different from zero.[25] This is, in fact, consistent with the reduction in the proportion of boys only working for pay from 2002 onwards, as will be shown below.

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[24] We estimated the pooled model with four and five-month bandwidth. The results are almost identical and are available upon request. We used the algorithm proposed by Calonico et al. (2014) that was designed to select the optimal bandwidth in non-parametric fuzzy design settings and found very similar bandwidth sizes to the ones used in our pooled estimates.

[25] Using the *sampsi* command in Stata, we would need about 20,000 observations to observe a statistically significant result with power of 80 percent and significance level of 5 percent.

We use the special module on child labor available in the PNAD of 2001 to check whether the affected cohort is studying more hours per day than the comparison cohort and whether there is any evidence they are still in the labor force. Table A.3 in the appendix shows that the 'treated' cohort is 1.5 p.p. less likely to be looking for a job in the last 30 days than the comparison group. This means that, with the ban, the affected cohort left the labor force.

These results indicate that the law change made some boys who would combine work with schooling leave the labor force to dedicate full time to study. We perform the same estimates adding controls and the results are identical.[26]

Finally, we investigate if the ban affected the occupations of banned boys. We perform the same exercise of equation (1) using the sample of working boys in 1999. Table A.4 presents the results. Among the comparison group, 36 percent work in the service sector, 25 percent in manufacturing, 15 percent in agribusiness, and 4 percent in administrative tasks. Although we do not find any statistically significant change in these compositions, the results suggests that there are some changes from service and agribusiness to manufacturing and administrative tasks. However, these exercises have low power due to the relatively small number of boys working for pay.

6.1. The Plausibility of the Identification Assumption

The identification strategy used in this paper assumes that turning age 14 by the time the law changed (December 1998) was "random". Thus, observed and unobserved characteristics are unexpected to be systematically correlated to both turning 14 around December 1998 and the outcomes of interest.

The official statistics for children at ages 14 and 15 in 1998 shows that 21.6 percent were in the workforce with about half (46 percent) in the formal sector. In 1999, the proportion of 14 and 15-year-olds in the labor force dropped slightly to 20.5 percent, but the proportion in the formal sector almost halved (9.6 percent vs. 5.3 percent). Most of the fall in participation in the formal sector were among children aged 14 (1.76 percent in 1999 compared to 5.8 percent in 1998). These numbers suggest that those who turned 14 before the ban were less likely to be affected by the law change. This is consistent with the monotonicity assumption made in the fuzzy RD design.

[26] The results are available upon request.

To check whether individuals (or their parents) tried to manipulate the forcing variable to being eligible to participate in the formal labor force or to pretend they complied with the law, we perform the McCrary test. We also run some placebo tests to check whether our estimates might be confounded.

The McCrary Density Test

The McCrary density test consists of verifying whether there is evidence of perfect manipulation of the forcing variable around the cut-off point. The test compares the density distributions of the forcing variable around the threshold (McCrary 2008).

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, one could still think of the possibility that a family misreports dates of birth to the surveyor, particularly in cases where children are working illegally. If this occurs systematically, one would observe a discontinuity in the density function of the forcing variable around the cut-off point and this would call into question the plausibility of our identification strategy.

We illustrate the results graphically with age defined in weeks and days. The reason for using age in days is to make it as continuous as possible given that the McCrary test is non-parametric (see Lee and Card 2008 for this point).

The figures 2 and 3 show the McCrary density test for age in weeks and age in days respectively. Both figures indicate that there is no discontinuity in the forcing variable around the threshold, that is, there is no evidence of perfect manipulation of the forcing variable.

Balance Check

Another important check of the plausibility of the RDD consists of a balance check comparing observed characteristics of children around the cut-off point. Under the assumption that the law change can be seen as a natural experiment, we should observe ‘treated’ and ‘control’ boys with similar observed characteristics, on average. We perform a test for difference in means and the results are presented in table 3.

In addition to control variable we use in our regressions, we also compare children in terms of household income per capita. We find no systematic differences of the mean values of these variables between the two unaffected and affected cohorts. This result

supports the assumption that the assignment to treatment was “random” at the immediacy of the cut-off value of the forcing variable.

Placebo Tests

To associate the discontinuity in working and schooling outcomes to the change in minimum legal age, there should be no other factor associated with the discontinuity of the forcing variable at the cut-off. One could argue, for instance, that the Brazilian compulsory schooling law could confound our results.

By 1999, the mandatory school entry age was 7 and a child had to stay in school until he/she completes primary education (grade 8). Although a Brazilian federal law states the compulsory age range to attending school, the school system is decentralized so that basic education is provided in state and municipal schools. Each school system uses different cut-off dates to determine what months in the year an individual should complete seven years of age to enroll in a school.

The school calendar in Brazil goes from March to November of a calendar year. Some systems chose January 1 to December 31 as the dates and individuals should complete seven years to enroll in the same school calendar year. Other systems chose July 1 of the previous calendar year to June 30 of the next calendar year. Some also chose March 1 of a calendar year to February 28 of the following calendar year. Even though there is such variation across school systems, it is possible that some individuals of our unaffected cohort (those that completed age 14 in the second semester of 1998) might have already completed their compulsory school years while some of our affected cohort did not.

At this point it is important to recall that our estimations have not found any effect on overall school attendance. The mandatory schooling age does not seem to be generating any discontinuity in school attendance rates. Still, we do find discontinuities in proportions of boys ‘working and studying’ and ‘only studying’ and one could argue that these effects could be partially driven by the compulsory schooling law. To examine this channel further, we perform two placebos tests.

The first placebo test examines the discontinuities in the outcomes of interest using the last week of December 1997, one year before the ban, as cut-off point. The sample composition is thus very similar to the one in our main exercise except that now the “comparison” cohort are those who completed age 14 in 1997 and the “treated”

cohort are those who completed age 14 in 1998. We use the data from the PNAD 1998 to perform the estimates.

The second placebo test uses data from 2001 and compares boys who turned 14 before and after December 2000, two years after the change in the law. The third placebo inspects the existence of discontinuities in the outcomes of interest at the cut-off of first week of July 1999. The two cohorts examined are the males that completed 14 years in the first half of 1999 and the ones that completed 14 years in the second half of 1999. The treatment dummy is equal to 1 for those that completed 14 in the second half of 1999. We use the data from the PNAD 1999. This analysis compares two cohorts affected by the ban.

Table 4 shows the results of the three placebo tests with piecewise linear specification. Except for one outcome in 2001, the results suggest no discontinuities in the outcomes variables.

6.2. Persistence of Short-Term Effects

This sub-section explores the persistence of the ban effect along the school ages. Specifically, we look at the ban effect comparing the outcomes of 'treated' and 'control' cohorts between 1998 and 2006.[27] We then follow the 'treated' cohort between ages 13 and 21. We start with 1998 so that we have additional robustness checks since we look at the affected and unaffected cohorts one year prior to the change in the law.

Figure 4 shows the time evolution of school attendance rates, and the work incidence in paid and unpaid work for the unaffected cohort of boys. School attendance declines steadily from about 94 percent at age 13 to around 27 percent at age 21. In contrast, paid work incidence increases steadily from 8.4 percent at age 13 to 71 percent at age 21. The unpaid work incidence decreases steadily as boys get older.

Figure 5 presents the evolution of the six different combinations between schooling and work activities for the unaffected cohort. About 82 percent of 13-year-old boys were only studying. That number drops to about 11 percent by the time they reach age 21. As expected, the proportion of those doing paid work only increased over time, reaching 55 percent by the time individuals were age 21. Interestingly, about 20 percent

[27] There is no PNAD in the year 2000.

of boys were working and studying at age 16. At that age most boys were supposed to have graduated high school. This could suggest either that some are delayed in school or are doing other courses such as pursuing college degrees.

We investigate the persistence of the impact of the ban running the regression Eq. (1) for each year separately.[28] Results are illustrated in figures 6 to 14. Figure 6 presents the results for paid work. As expected, there was no difference in paid work incidences between 'treated' and comparison groups in 1998. The effect kicks in in 1999 and remains till 2003 when the boys prevented from entering the formal labor force at age 14, reached age 17. Indeed, the affected cohort of boys was around 4 to 10 p.p. less likely to be doing paid work than the comparison group. From 2004 (age 18) onwards, these differences are no longer observed.

The effect of the ban on school attendance is shown in figure 8. Even though no difference is observed immediately after the law change, we do observe an increase in school attendance between 2001 and 2003. This coincides with the period during which the affected cohort of boys seemed less likely to be doing paid work.

Figure 9 shows a reduction in the proportion doing paid work and studying. However, the fall shows up statistically significant only in 1999 and 2002. This is consistent with the fall in the proportion of boys doing only paid work as can be seen in figure 11. In 2005 and 2006, there is a statistically significant increase in the proportion of boys doing paid work and studying. That increase is explained by the steady growth in the proportion of the affected cohort of boys only studying between 1999 and 2003 as shown in figure 13. We found no impacts on the proportion doing unpaid work and those neither working nor studying.

The results then suggest that the ban reduced the proportion of boys in the labor force and increased the proportion of those only studying. The figures show that these effects lasted for about five years, from 1999 to 2003. The question is then, who benefited most in the long-term, the group allowed to accumulate experience in the labor market or the group that was somehow nudged by the ban to shift to school? In the next section we investigate the long-term effects of the ban on human capital and labor-market outcomes.

[28] Estimates are obtained with nine-month bandwidth. We show the results for the piecewise linear specification—the forcing variable enters in level and then interacts with the 'treatment' dummy—only. The results with linear and quadratic polynomials were almost identical and are available on request.

7. Long-Term Effects of the Ban

To estimate long-term effects of the ban, we follow the same cohorts examined in the short-term analysis between 2007 and 2014. During this period, the affected cohort was aged 22 to 29.[29] We pool the PNADs of 2007-2014 and run the same Eq. (1) controlling for survey-year dummies. We turn attention to five outcomes: completed years of schooling, probability of having at least a high-school degree, probability of having a college degree, probability of being employed, probability of being employed in the formal sector, and log of monthly earnings. As before, for binary outcomes, we estimate linear probability models.

Parametric Results

We use the same three specifications used in the short-term analysis and a six-month bandwidth since we have higher statistical power. The results are presented in table 5.

According to the results shown in table 5, there is no evidence of long-term effects of the ban on stock of human capital and labor-market outcomes, at least when all years are pooled together.

Table 6 in the appendix shows the placebo test for long-term effects using the cohorts born in 1983 and 1984. These cohorts turned 14 before and after December 1997 and were not affected by the ban.[30] Even though the coefficient for college degree stands out statistically significant at 5 percent, overall, the results are statistically insignificant.

Long-Term Trajectories from Ages 22 to 29

We investigate further the long-term effects by looking at trajectories from 2007 to 2014. In this analysis, we estimate Eq. (1) for each year individually and use a bandwidth of nine months to increase precision. The point estimates with 90-percent confidence interval are shown in figures 15 to 20.

[29] The choice of 2007 as first year of the long term effects needs more clarification indeed. We used 2007 because 22-23 is the expected age of a college graduating student.
[30] The same cohorts were used in one of the placebo tests in the short-term analysis.

Figure 15 shows that boys in the affected cohort were less likely to hold a high-school degree in 2008. That effect is likely to be noise, given that, in other years, the point estimate was relatively small and imprecisely estimated. A similar pattern is observed in figure 16. Interestingly though, the estimates for 2008 and 2012 in figure 17 suggest that the affected cohort had fewer years of schooling. The estimates for 2013 and 2014 suggest they catch up later on. This would explain why, when we pooled all years together, we found no difference in completed years of schooling between the two cohorts.

Although we found no impact on work incidence and log earnings, figure 19 suggests that the ban might have affected individual's occupation to some extent. According to table 5, the affected cohort was about 2 p.p. (or 3.5 percent) less likely to have a formal job than the unaffected cohort. We also found some indication of negative effect on log earnings, but the point estimate was statistically insignificant. Even pooling seven years of survey data, it seems we did not have the statistical power to detect such small effects for log earnings and occupation.[31]

7.1. Heterogeneous Effects

It may be the case that a particular sub-group of individuals is strongly affected by the child-labor ban but these impacts are overshadowed in our long-term estimates for the overall treated cohort. Unfortunately, we are unable to identify the group of compliers in the long-term in our database since we work with repeated cross-sections. We could still try to estimate the impact of labor-force participation in the short term on, say, log wages in the long term using the two-sample instrumental strategy originally applied by Angrist and Krueger (1992) (see Inoue and Solon 2005 for a discussion). However, since the impact of the law on participation rates is partial and we also observe an increase in school attendance among those boys banned from the labor force, we actually have two endogenous variables (participation rates and school attendance) and only one instrument (the law).

To shed some light on the long-term heterogeneous effects of the ban on log earnings, we estimate quantile regressions. Table D.1 presents the estimates for each

[31] Using the *sampsi* command in Stata to be able to detect a fall of 2 p.p. in formal occupation with a power of 80 percent and significance level of 10 percent, we would need a sample of 18,328 observations. That explains why the point estimates of table 5 are almost significant at 10-percent level.

quintile. Estimates control for survey-year fixed effects. We interpret the point estimates as conditioned quantile treatment effects.[32] There is some evidence of negative effect of the ban on log earnings, but it is concentrated at the lower tail of log earnings distribution. We found a negative and precise effect of the ban on log earnings of 6.2 percent. The estimates at other quintiles are indistinguishable from zero.

This effect on earnings is in fact very similar to the impact of serving in the military program in Netherlands (5 percent) and in the U.S. The effect of conscription on earnings has been interpreted as the return to experience (see Imbens and van der Klaauw 1995 and Angrist et al. 2011 respectively).

The effect is also very similar to the ITT estimates for the returns to education in the United Kingdom (6.5 percent) and Northern Ireland (5.5 percent) (Oreopoulos 2006), and to the effect of the high-school choice program in Israel (5 to 7 percent by the time individuals are 28-30 years old) (Lavy, 2015a). As in Angrist et al. (2011) and Lavy (2015a), we found limited effects on employment.

8. Discussion of the Main Results and Policy Implications

The short-term results suggest the child-labor ban of December 1998 was partially a step in the right direction as it reduced paid work and schooling incidence and increased the incidence of boys only studying. It seems that those effects lasted for few years till 2003 when the affected cohort reached age 18.

Interestingly, we found no systematic long-term consequences of the ban on schooling attainment and labor-market outcomes. There are two ways of interpreting these results. One could say that the law did not harm the generation prevented from entering the labor market at age 14 because the return to early experience in the labor market, conditional on schooling, is not economically significant. The lack of such exposure has no detrimental effect in the end.

On the other hand, one could argue that the early exposure to the labor market is important. The no effect in the long term is explained by the fact that the lack of such experience was compensated by the gain on the quality of the education and learning by

[32] We condition the regression on the forcing variable. It is important to clarify that this is not the same estimator as proposed by Firpo (2007) where selection on observed characteristics holds. Firpo (2007) estimator can be seen as a quantile version of a matching estimator.

studying only.[33] Unfortunately, our data do not allow us to further investigate these channels.

However, the ban seems to have caused detrimental effects on a sub-group of the affected cohort, that is, those who have relative lower earnings potential. Indeed, we find that earnings are 6.2 percent lower among those affected by the law who are in the bottom 20th percentile of the log-earnings distribution.

We believe the results found in this paper has some policy implications: (1) it informs policy makers how the law affected time allocation between working and schooling activities in the short term; (2) it shows that the ban has persistent effects on time allocation between schooling and working activities; (3) it suggests that the returns to experience for the unaffected cohort were similar to the returns to education for the affected cohort; (4) it can have detrimental effects among those with lower earnings potential; and finally (5) it suggests that policies that reduce individuals' choice-sets should be accompanied by compensating or complementary policies—for example, a conditional cash-transfer program or an apprenticeship program—for those to whom it is more likely to cause harm.[34]

Conclusions

This is the first paper to investigate the short- and long-term effects of a child-labor ban. The increase in the Brazilian minimum legal age of entry into the labor market from 14 to 16 in December 1998 was interpreted as a natural experiment. Several years of Brazilian household surveys and a fuzzy RDD were used to estimate the ITT impact of the ban on the outcomes of interest.

[33] Indeed, Emerson, Ponczek and Souza (2016) find that only studying improves learning as compared to studying and working in Brazil.
[34] The Brazilian CCT *Bolsa Família* was in a pilot stage at that time (Glewwe and Kassouf, 2012) and the Brazilian apprenticeship program was launched only in December 2000 (Corseuil et al. 2012).

The short-term effects showed that the ban reduced work incidence in paid work activities by 4.1 p.p. or 27 percent. It seems the ban reduced the proportion of boys doing paid work and studying and increased the proportion of boys only studying. We found that these effects lasted for five years, from 1999 to 2003 when boys hindered from entering the formal labor force at age 14 reached age 18.

We pooled the PNADs of 2007 to 2014 to estimate the long-term effects of the ban on educational attainment and labor-market outcomes when the cohorts were age 22 to 29. Our pooled effect estimates suggest the ban did not affect those average outcomes among the entire affected cohort in the long term. It thus seems that the higher proportion of boys only studying just counterbalanced the lower level of accumulated labor-market experience. However, it seems to have detrimental effects on a sub-group of the affected cohort that will likely have lower earnings when adults.

It is important to contextualize the findings of this paper. First, we estimated ITT effects, that is, the average effect of the child-labor ban on the entire cohort of boys who turned age 14 in 1999. Since the child-labor ban affected a small share of affected boys (4 p.p.), the ITT estimate captures the lower bound of a local average treatment effect (LATE), that is, the effect on those who dropped out of the labor force at ages 14 or 15 because of the ban.

Second, although we are confident of the internal validity of our results, one should be cautious to extrapolate to different age groups or cohorts. This cohort faced a particular set of constraints and policies that are not the same faced by other cohorts.

One should not read it as an implicit advocacy towards child labor but as a cautionary note that the law should be designed according to the context and compensatory policies—such as a conditional cash-transfer program and/or an apprenticeship program—should be put in place; otherwise it would make individuals worse off by reducing their choice-set.[35] In this regard, our results are in line with a broader literature (for example, Lavy 2015b, Doepke and Zilibotti 2005) that shows that interventions that increase the choice-set and opportunities of children are important policies to help them to achieve their intentional goals.

[35] The Brazilian conditional cash-transfer program was created in 1996 as a pilot but expanded only in 2001. The Brazilian apprenticeship program was institutionalized only in December 2000.

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Tables

Table 1 – Descriptive Statistics of the Whole Sample of Males – PNAD 1999
9-Month Bandwidth

Variables	Obs	Mean	SD	Min	Max
Outcomes					
Working	4549	0.19	0.39	0	1
Paid Work	4549	0.12	0.33	0	1
Paid Work – Formal	4549	0.006	0.08	0	1
Paid Work – Informal	4549	0.18	0.38	0	1
Unpaid Work	4549	0.07	0.25	0	1
School Attendance	4549	0.92	0.27	0	1
Paid Work & Studying	4549	0.09	0.29	0	1
Unpaid Work & Studying	4549	0.06	0.23	0	1
Only Paid Work	4549	0.03	0.16	0	1
Only Unpaid Work	4549	0.01	0.08	0	1
Only Studying	4549	0.77	0.42	0	1
Neither Working Nor Studying	4549	0.05	0.22	0	1
Covariates					
White	4549	0.47	0.50	0	1
Father's Years of Schooling	4549	5.95	4.41	0	15
Mother's Years of Schooling	4549	5.87	4.37	0	15
Household size	4549	4.86	1.66	1	14

Source: PNAD 1999.

Table 2a: RDD Regression Results of the Intent-to-Treatment Effects (ITT) - 14 Year Old Males. PNADs 1999&2001 - *6-Month Bandwidth*

	Linear	Quadratic	Piecewise linear
		Paid Work	
ITT	-0.042**	-0.043**	-0.043**
	(0.018)	(0.017)	(0.017)
Control Mean	0.15	0.15	0.15
		Paid Work - Formal	
ITT	-0.0034	-0.0034	-0.0034
	(0.0025)	(0.0026)	(0.0025)
Control Mean	0.006	0.006	0.006
		Paid Work - Informal	
ITT	-0.020	-0.020	-0.021
	(0.015)	(0.015)	(0.014)
Control Mean	0.08	0.08	0.08
		Unpaid Work	
ITT	0.0055	0.0056	0.0056
	-0.01	-0.01	-0.01
Control Mean	0.068	0.068	0.068
		School Attendance	
ITT	0.025	0.025	0.025
	-0.02	-0.02	-0.02
Control Mean	0.90	0.90	0.90
N	6152	6152	6152

Note: ***, **, * Statistically significant at 1, 5, and 10 percent respectively. In parentheses clustered standard errors are clustered at the forcing variable level (weeks). Specifications: within each sample group, the first column includes age in weeks linearly, year dummies, and the 'treatment' dummy. The second column includes a second-order polynomial of the forcing variable, whereas the third column includes the forcing variable in level and interacted with the 'treatment' dummy. All regressions control for mother's years of schooling.

Table 2b: RDD Regression Results of the Intent-to-Treatment Effects (ITT) - 14-Year-Old Males. PNADs 1999&2001 - *6-Month Bandwidth*

	Linear	Quadratic	Piecewise linear
		Paid Work & Studying	
ITT	-0.034**	-0.034**	-0.034**
	(0.02)	(0.02)	(0.02)
Control Mean	0.12	0.12	0.12
		Unpaid Work & Studying	
ITT	0.0031	0.0031	0.0032
	(0.01)	(0.01)	(0.01)
Control Mean	0.06	0.06	0.06
		Only Paid Work	
ITT	-0.0071	-0.0072	-0.0074
	(0.01)	(0.01)	(0.01)
Control Mean	0.03	0.03	0.03
		Only Unpaid Work	
ITT	0.0025	0.0025	0.0024
	(0.00)	(0.00)	(0.00)
Control Mean	0.01	0.01	0.01
		Only Studying	
ITT	0.056**	0.056**	0.057**
	(0.02)	(0.02)	(0.02)
Control Mean	0.72	0.72	0.72
		Neither Working nor Studying	
ITT	-0.021	-0.021	-0.02
	(0.02)	(0.01)	(0.01)
Control Mean	0.06	0.06	0.06
N	6152	6152	6152

Note: ***, **, * Statistically significant at 1, 5, and 10 percent respectively. In parentheses clustered standard errors at the forcing variable level (weeks). Specifications: within each sample group, the first column includes age in weeks linearly, year dummies, and the 'treatment' dummy. The second column includes a second-order polynomial of the forcing variable, whereas the third column includes the forcing variable in level and interacted with the 'treatment' dummy. All regressions control for mother's years of schooling.

Table 3 – T-test for Difference in Means in 1999 Unaffected vs. Affected Cohorts

9-Month Bandwidth

	Males				
	Unaffected		Affected		***Difference (t-statistics)***
	Mean	**S.D.**	**Mean**	**S.D.**	
Father's Years of Schooling	5.92	4.37	5.99	4.44	(-0.58)
Mother's Years of Schooling	5.86	4.32	5.88	4.42	(-0.16)
Father's Age	46.15	23.04	45.17	11.93	(1.79)
Mother's Age	42.19	30.93	41.28	23.65	(1.12)
Household Size	4.85	1.65	4.86	1.66	(-0.27)
Household Per-Capita Income (R$)	803.49	1434.51	781.54	1196.75	(0.56)
Number of Observations	*2273*		*2276*		*4549*

Source: PNAD 1999.

Table 4: Placebo Tests – Cut-offs: Last Week of December 1997 and Last Week of December 2000. Intent-to-Treatment Effects (ITT) – 14-Year-Old Males. PNADs 1998, 1999 and 2001

	Placebo 1	Placebo 2	Placebo 3
	Dec 1997 = cut-off	Dec 2000 = cut-off	June 1999 = cut-off
	9 month-bandwidth	*9 month-bandwidth*	*6 month-bandwidth*
		Paid Work	
ITT	-0.012	0.003	0.0016
	(0.014)	(0.02)	(0.022)
		Paid Work - Informal	
ITT	0.026	0.019	0.0095
	(0.023)	(0.025)	(0.028)
		Unpaid Work	
ITT	0.0066	0.0028	0.0014
	(0.015)	(0.01)	(0.015)
		School Attendance	
ITT	-0.0019	-0.00039	-0.012
	(0.013)	(0.02)	(0.017)
		Paid Work & Studying	
ITT	-0.0057	0.0097	0.0033
	(0.013)	(0.02)	(0.020)
		Unpaid Work & Studying	
ITT	0.0035	0.0048	-0.0056
	(0.015)	(0.01)	(0.014)
		Paid Work Only	
ITT	-0.0059	-0.0066	-0.0017
	(0.0061)	(0.01)	(0.0080)
		Unpaid Work Only	
ITT	0.0031	-0.002	0.007*
	(0.0046)	(0.01)	(0.0039)
		Studying Only	
ITT	0.00035	-0.015	-0.0099
	(0.021)	(0.02)	(0.026)
		Neither Working nor Studying	
ITT	0.0046	0.009	0.0069
	(0.012)	(0.01)	(0.013)
N	4131	5100	2661

Note: Standard errors are clustered at the forcing variable level (weeks) and t-statistics are showed in the parentheses. We only report the estimates with the smooth function specified as piecewise linear – the forcing variable in level and interacted with the 'treatment' dummy. The estimates with the first and second-order polynomial are almost identical to the ones above and are available upon request. All estimates control for mother's years of schooling.

Table 5: RDD Regression Results of the Long-Term Intent-to-Treatment Effects (ITT). Birth Cohorts of 1984 and 1985. Pooled PNADs 2007-2014 – *6-Month Bandwidth*

	Males		
	Linear	**Quadratic**	**Piecewise linear**
	Completed Years of Schooling		
ITT	-0.071	-0.071	-0.070
	(0.091)	(0.092)	(0.092)
Control Mean	10,22	10,22	10,22
N	17990	17990	17990
	At Least High School Degree		
ITT	-0.018	-0.018	-0.019
	(0.013)	(0.013)	(0.012)
Control Mean	0,62	0,62	0,62
N	17990	17990	17990
	College Degree		
ITT	-0.0011	-0.0011	-0.0013
	(0.010)	(0.0098)	(0.0099)
Control Mean	0,13	0,13	0,13
N	17990	17990	17990
	Employed		
ITT	-0.0025	-0.0026	-0.0029
	(0.012)	(0.011)	(0.011)
Control Mean	0,83	0,83	0,83
N	17990	17990	17990
	Formal Occupation		
ITT	-0.020	-0.020	-0.020
	(0.013)	(0.013)	(0.013)
Control Mean	0,57	0,57	0,57
N	15703	15703	15703
	Log-Earnings		
ITT	-0.019	-0.019	-0.020
	(0.021)	(0.019)	(0.019)
Control Mean	6,77	6,77	6,77
N	14522	14522	14522

Note: ** Statistically significant at 5 percent. Standard errors are clustered at the forcing variable level (weeks) and t-statistics are showed in the parentheses. Specifications: within each sample group, the first column includes age in weeks linearly, year dummies, and the 'treatment' dummy. The second column includes a second-order polynomial of the forcing variable, whereas the third column includes the forcing variable in level and interacted with the 'treatment' dummy.

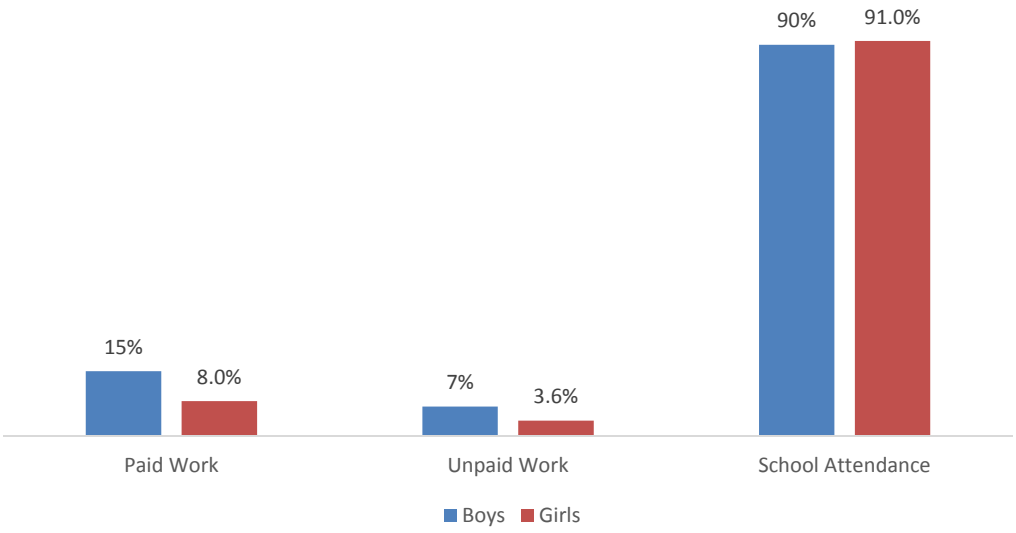
Table 6: Placebo Tests: RDD Regression Results of the Long-Term Intent-to-Treatment Effects (ITT). Birth Cohorts of 1983 and 1984. 6-Month Bandwidth. Pooled PNADs 2007-2014 - *6-Month Bandwidth*

	Males		
	Linear	Quadratic	Piecewise linear
		Completed Years of Schooling	
ITT	-0.035	-0.038	-0.04
	(-0.33)	(-0.37)	(-0.39)
N	18036	18036	18036
		At Least High School Degree	
ITT	0.013	0.012	0.012
	(0.92)	(0.89)	(0.89)
N	18036	18036	18036
		College Degree	
ITT	0.020**	0.020**	0.020**
	(2.23)	(2.23)	(2.22)
N	18036	18036	18036
		Employed	
ITT	0.012	0.011	0.011
	(1.13)	(1.09)	(1.09)
N	18036	18036	18036
		Formal Worker	
ITT	-0.015	-0.016	-0.016
	(-1.04)	(-1.12)	(-1.13)
N	15936	15936	15936
		Log-Earnings	
ITT	-0.0036	-0.002	-0.0019
	(-0.13)	(-0.073)	(-0.070)
N	14763	14763	14763

Note: Standard errors are clustered at the forcing variable level (weeks) and t-statistics are showed in the parentheses. Specifications: within each sample group, the first column includes age in weeks linearly, year dummies, and the 'treatment' dummy. The second column includes a second-order polynomial of the forcing variable, whereas the third column includes the forcing variable in level and interacted with the 'treatment' dummy.

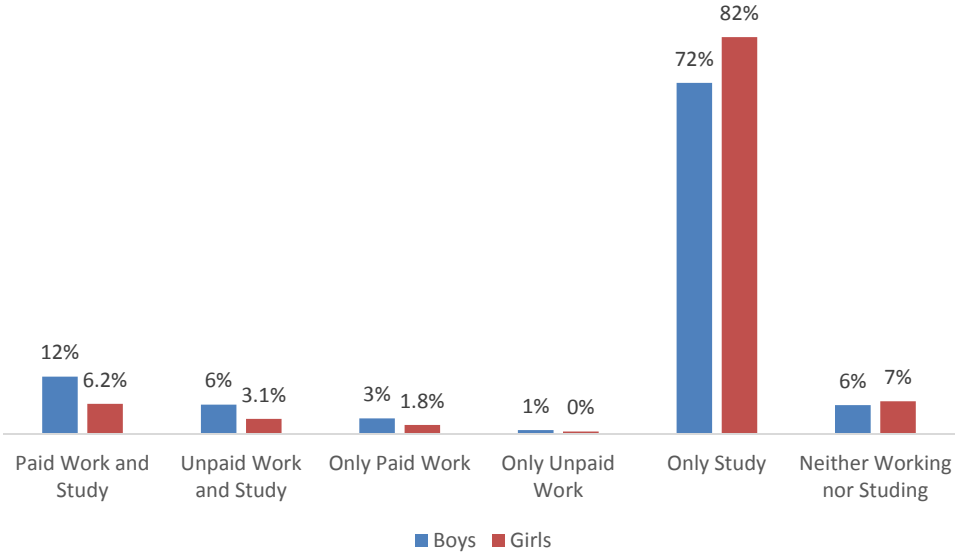
Figures

Figure 1.a: Time Allocation of the Comparison Group – *9-Month Bandwidth*



Source: PNAD 1999.

Figure 1.b: Time Allocation of the Comparison Group – *9-Month Bandwidth*



Source: PNAD 1999.

Figure 2: McCrary Density Test: Age in Weeks – *9-Month Bandwidth*

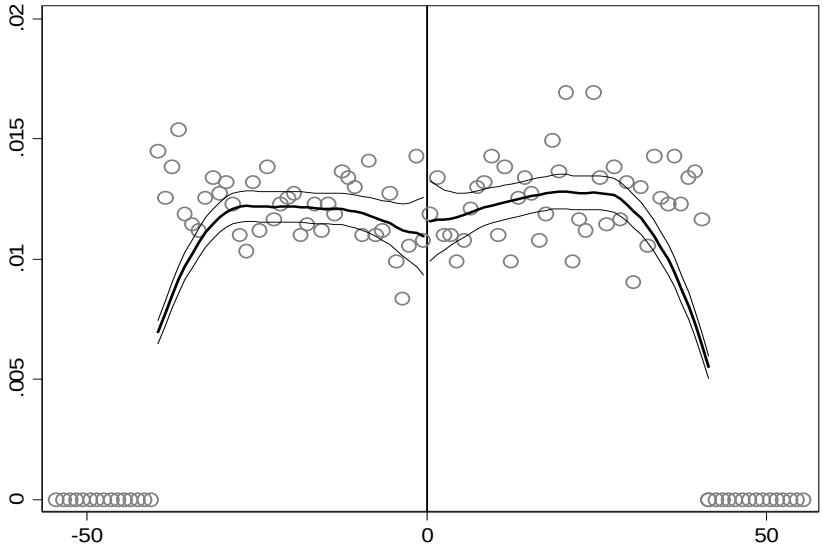


Figure 3: McCrary Density Test: Age in Days – *9-Month Bandwidth*

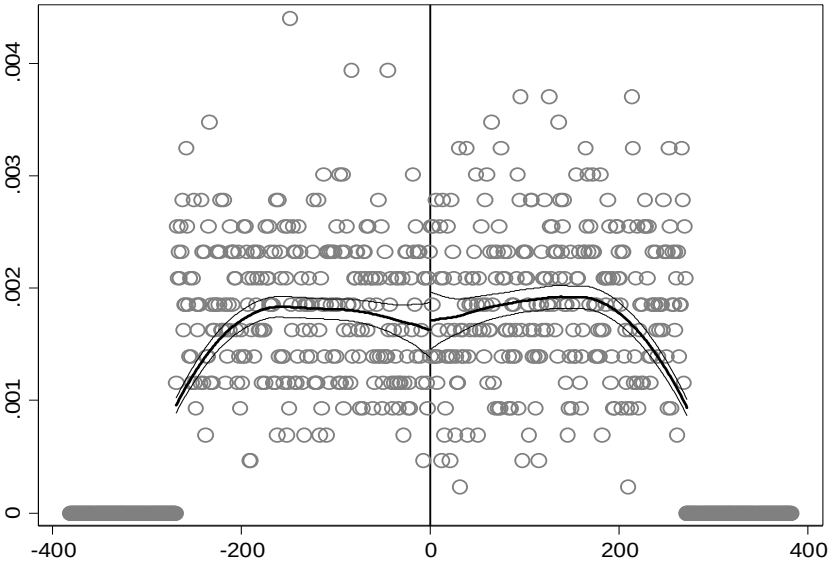


Figure 4 – Participation Rates and School Attendance of the Control Cohort—Males

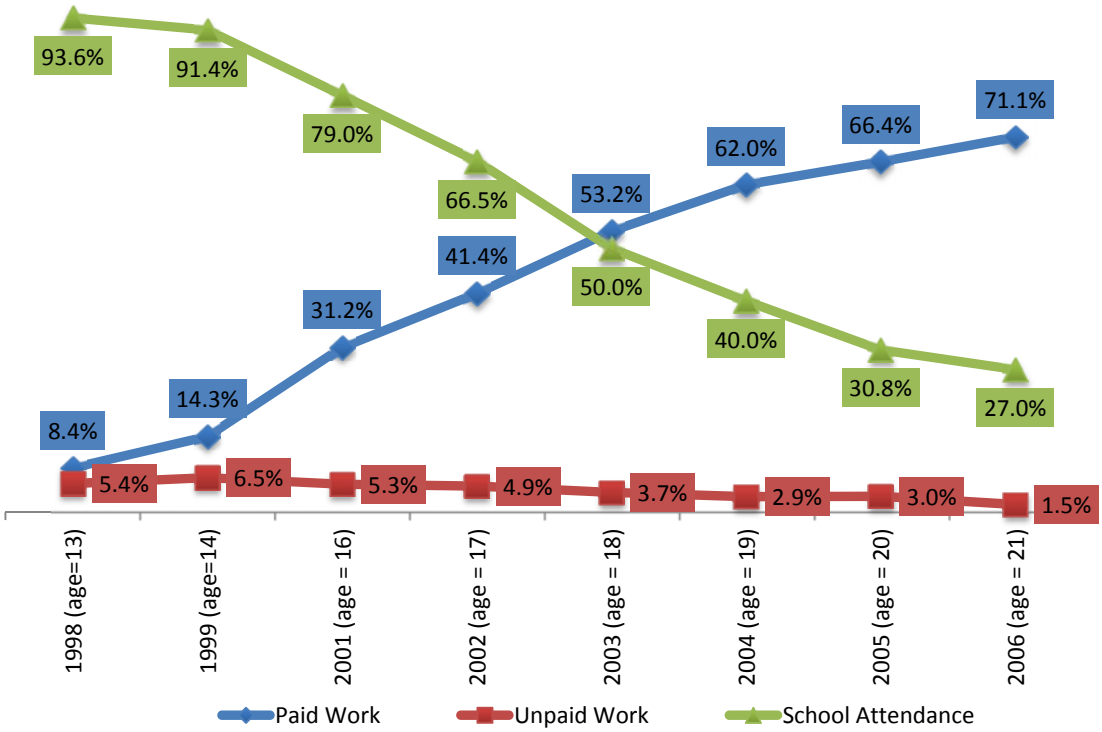


Figure 5 – Time Allocation Between Working and Schooling Activities of the Control Cohort—Males

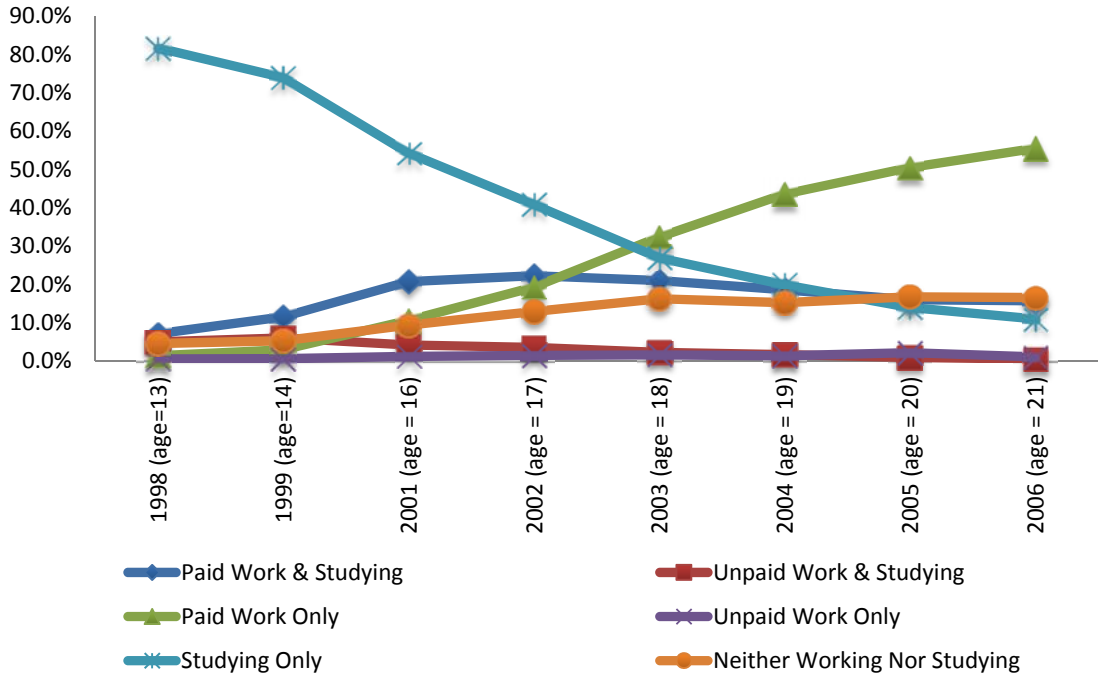


Figure 6 – Persistent Effect of the Ban on Paid Work—Males
90-percent confidence interval

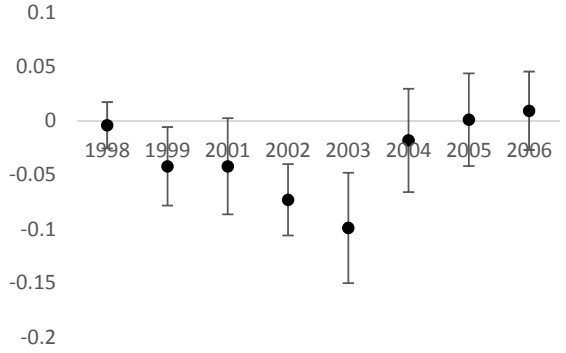


Figure 7 – Persistent Effect of the Ban on Unpaid Work—Males
90-percent confidence interval

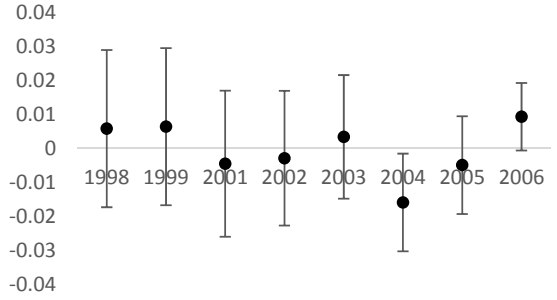


Figure 8 – Persistent Effect of the Ban on School Attendance—Males
90-percent confidence interval

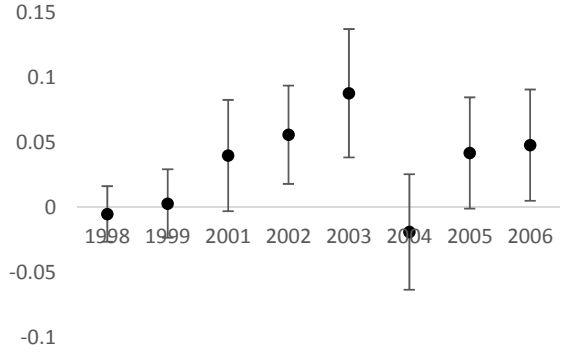


Figure 9 – Persistent Effect of the Ban on Paid Work & Studying—Males
90-percent confidence interval

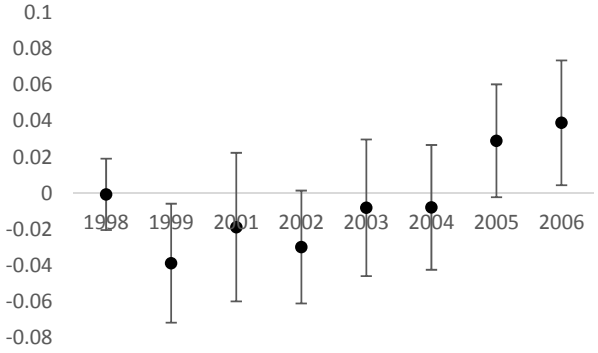


Figure 10 – Persistent Effect of the Ban on Unpaid Work and Schooling—Males
90-percent confidence interval

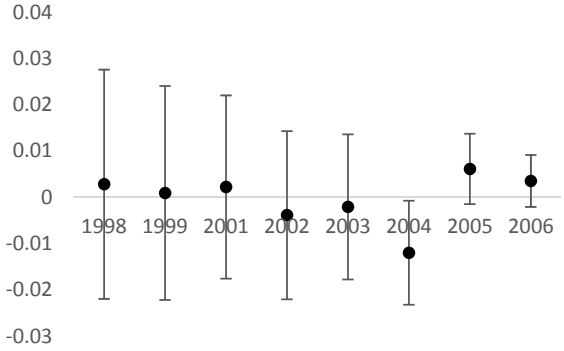


Figure 11 – Persistent Effect of the Ban on Paid Work Only—Males
90-percent confidence interval

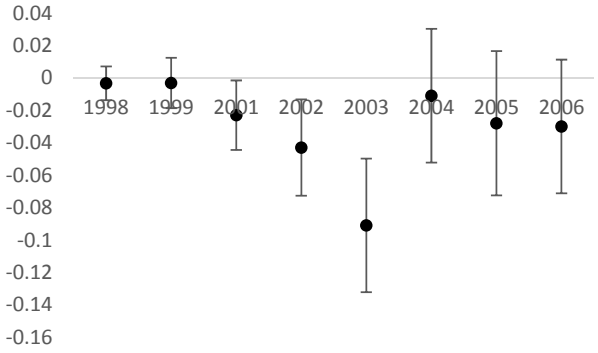


Figure 12 – Persistent Effect of the Ban on Unpaid Work Only—Males
90-percent confidence interval

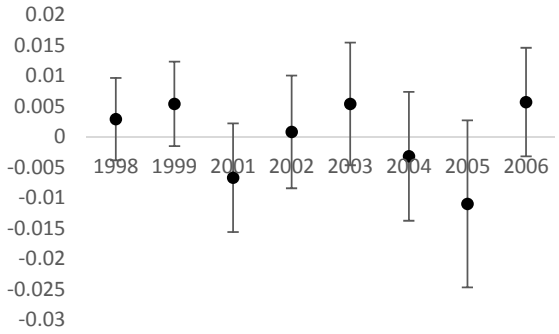


Figure 13 – Persistent Effect of the Ban on Study Only—Males
90-percent confidence interval

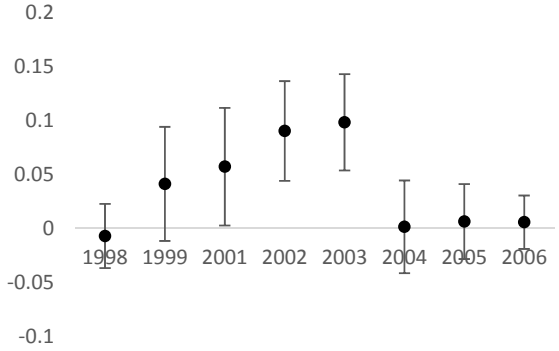
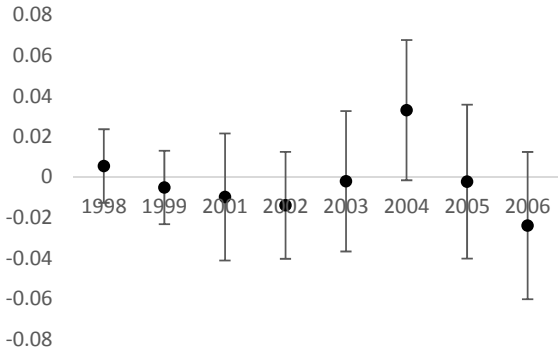


Figure 14 – Persistent Effect of the Ban on Neither Working Nor Studying—Males
90-percent confidence interval



Long-Term Trajectories

Figure 15 – Effect of the Ban on the Probability of Holding a High-School Degree – *90-percent confidence interval*

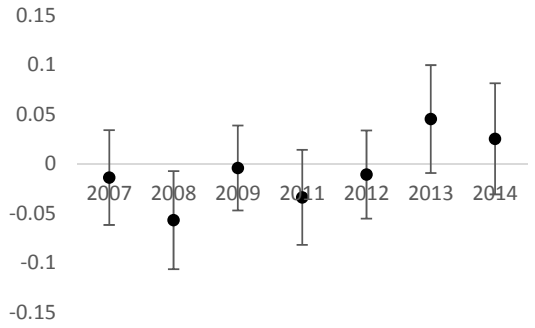


Figure 16 – Effect of the Ban on the Probability of Holding a College Degree – *90-percent confidence interval*

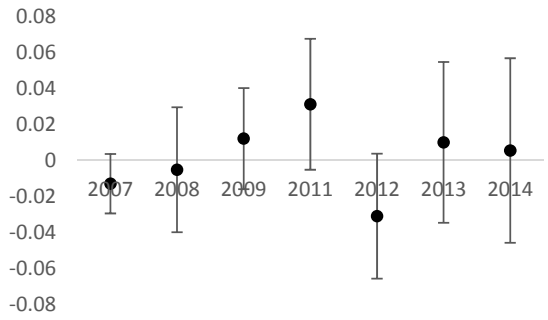


Figure 17 – Effect of the Ban on the Probability of Years of Schooling – *90-percent confidence interval*

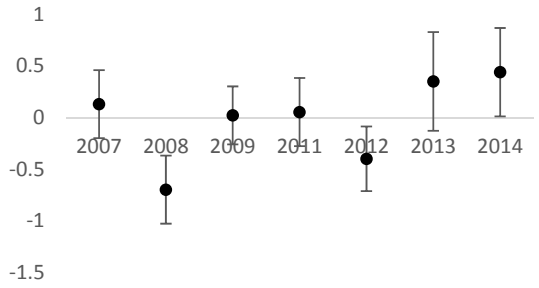


Figure 18 – Effect of the Ban on the Probability of Being Employed – *90-percent confidence interval*

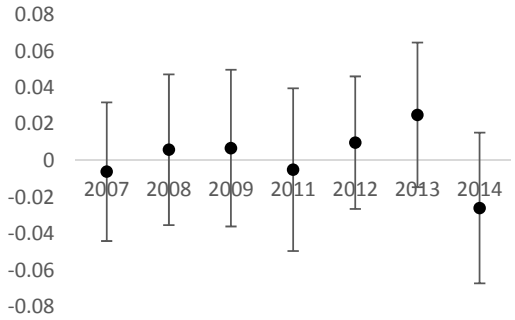


Figure 19 – Effect of the Ban on the Probability of Having a Formal Occupation – *90-percent confidence interval*

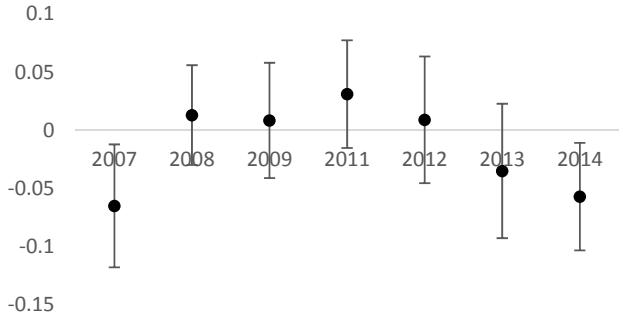
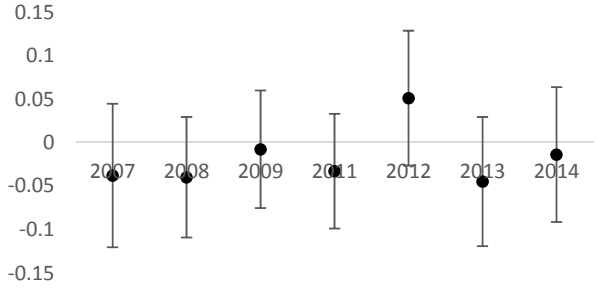


Figure 20 – Effect of the Ban on Log Earnings – *90-percent confidence interval*



Appendix A

Table A.1: RDD Regression Results of the Intent-to-Treatment Effects (ITT) – 14-Year-Old Males. PNAD 1999 - *9-Month Bandwidth*

	Males		
	Paid Work		
ITT	-0.043**	-0.043**	-0.043**
	(0.02)	(0.02)	(0.02)
Control Mean	0.15	0.15	0.15
	Paid Work - Formal		
ITT	-0.0049	-0.0051	-0.0052
	(0.0041)	(0.0039)	(0.0039)
Control Mean	0.01	0.01	0.01
	Paid Work - Informal		
ITT	-0.043**	-0.043**	-0.043**
	(0.020)	(0.020)	(0.020)
Control Mean	0.21	0.21	0.21
	Unpaid Work		
ITT	0.0059	0.0058	0.0058
	(0.01)	(0.01)	(0.01)
Control Mean	0.068	0.068	0.068
	School Attendance		
ITT	0.0035	0.0039	0.0042
	(0.02)	(0.02)	(0.02)
Control Mean	0.90	0.90	0.90
N	4549	4549	4549

Note: ** Statistically significant at 5 percent. Clustered standard errors at forcing variable level in parentheses; (ii) Specifications: within each group, the specification in the first column includes age in weeks linearly as control. The specification in the second column includes quadratic in age. And the specification in the third column includes age and an interaction term between age and the affected cohort dummy; (iii) all specifications controls for mother´s years of schooling; (iv) standard errors are clustered at the forcing variable level (weeks).

Table A.2: RDD Regression Results of the Intent-to-Treatment Effects (ITT) – 14-Year-Old Males. PNAD 1999 – *9-Month Bandwidth*

	Males		
	Paid Work & Studying		
ITT	-0.039*	-0.040**	-0.040**
	(0.02)	(0.02)	(0.02)
Control Mean	0.12	0.12	0.12
	Unpaid Work & Studying		
ITT	0.00049	0.0005	0.00056
	(0.01)	(0.01)	(0.01)
Control Mean	0.06	0.06	0.06
	Only Paid Work		
ITT	-0.0035	-0.0034	-0.0035
	(0.01)	(0.01)	(0.01)
Control Mean	0.03	0.03	0.03
	Only Unpaid Work		
ITT	0.0054	0.0053	0.0052
	(0.004)	(0.004)	(0.004)
Control Mean	0.01	0.01	0.01
	Only Studying		
ITT	0.042	0.043	0.043
	(0.03)	(0.03)	(0.03)
Control Mean	0.72	0.72	0.72
	Neither Working nor Studying		
ITT	-0.0054	-0.0057	-0.0059
	(0.01)	(0.01)	(0.01)
Control Mean	0.06	0.06	0.06
N	4549	4549	4549

Note: ***, **, * Statistically significant at 1, 5, and 10 percent respectively. Clustered standard errors at forcing variable level in parentheses; (ii) Specifications: within each group, the specification in the first column includes age in weeks linearly as control. The specification in the second column includes quadratic in age. And the specification in the third column includes age and an interaction term between age and the affected cohort dummy.

Table A.3 – Short-Term Heterogeneous Effects – ITT Estimates. PNAD 2001 – *9-Month Bandwidth*

	Missed at least one school day in the last month	Studying more than 4 hours/day	Looked for a job in the last 12 months
D	0.010	0.033	-0.015*
	(0.028)	(0.022)	(0.0080)
Control mean	0.43	0.34	0.19
N	4081	4971	4105

Note: * Statistically significant at 10 percent. Clustered standard errors at forcing variable level in parentheses. The reported results refer to the piecewise linear specification of the smooth function. The results for linear and quadratic polynomial are almost identical.

Table A.4: Occupation Structure Among Working Children – PNAD 1999 – *9-Month Bandwidth*

	Services	**Administrative**	**Agribusiness**	**Manufacturing**
ITT	-0.034	0.019	-0.057	0.048
	(0.066)	(0.029)	(0.054)	(0.051)
Control Mean	*0.36*	*0.04*	*0.15*	*0.25*
N	843	843	843	843

Note: Clustered standard errors at forcing variable level in parenthesis. ***, **, * Statistically significant at 1, 5, and 10 percent level. Services include technical and scientific, commerce, transport, and communication.

Appendix B

Figure B.1.a: Local Linear Regression
Paid Work—Males

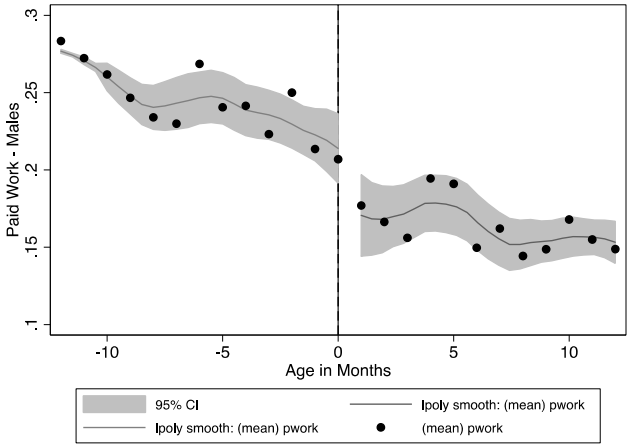


Figure B.2.a: Local Linear Regression
Unpaid Work—Males

Figure B.3.a: Local Linear Regression
School Attendance—Males

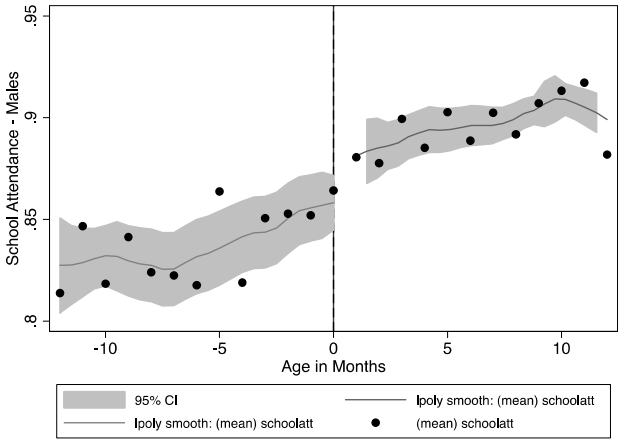


Figure B.4.a: Local Linear Regression
Paid Work Only—Males

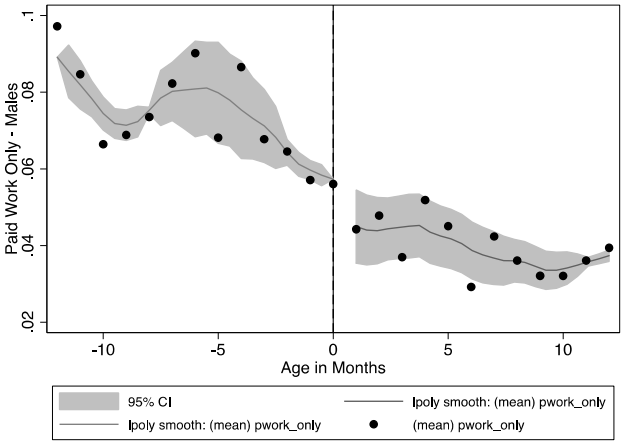


Figure B.5.a: Local Linear Regression
Unpaid Work Only—Males

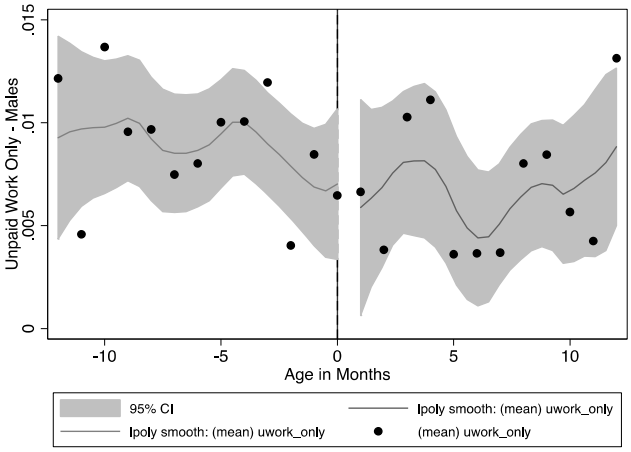


Figure B.6.a: Local Linear Regression
Paid Work and Studying—Males

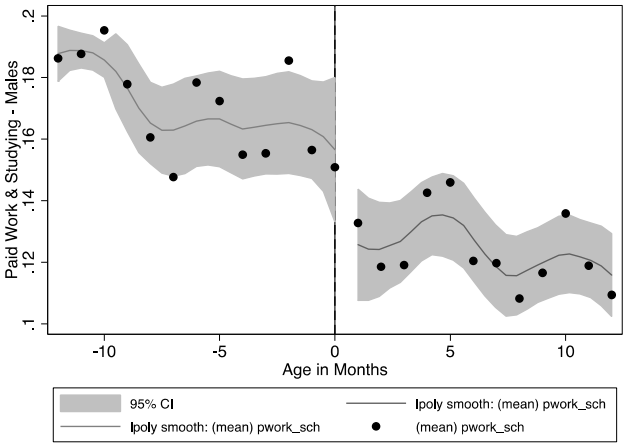


Figure B.7.a: Local Linear Regression Unpaid Work and Studying—Males

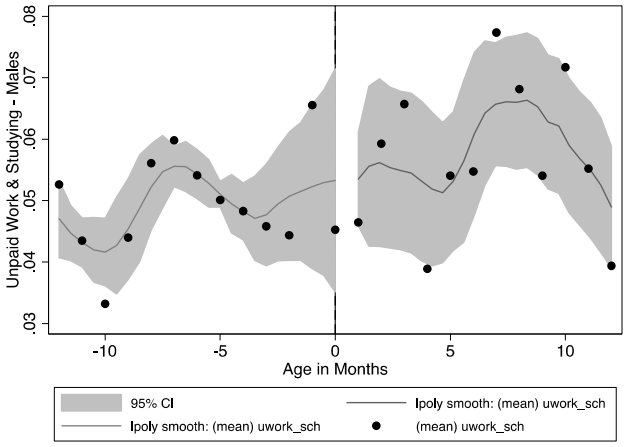


Figure B.8.a: Local Linear Regression Studying Only—Males

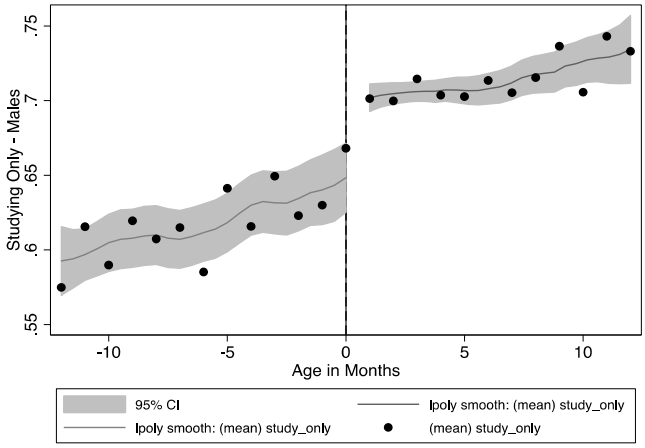


Figure B.9.a: Local Linear Regression Neither Work Nor Studying—Males

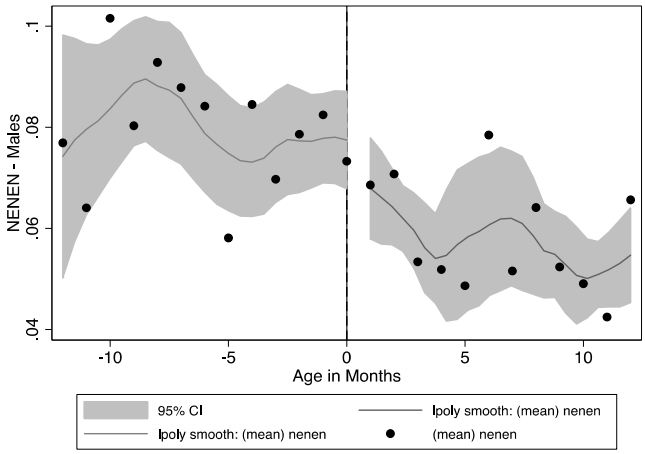
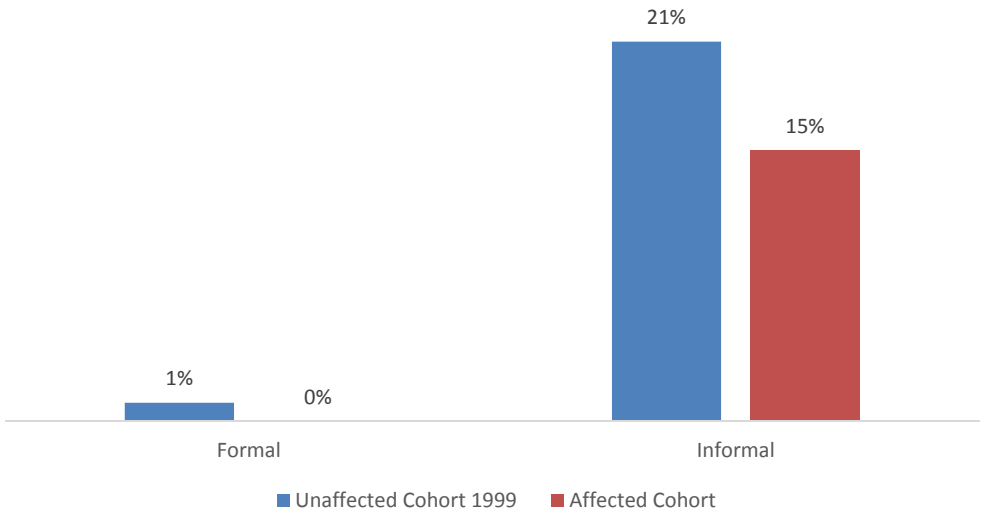


Figure C.1 – Proportion of 'Treated' and Comparison Groups in Formal and Informal Occupations in 1999



Source: PNAD 1999 (9 months bandwidth)

Figure C.2: Local Linear Regression Formal Paid Work—Males. PNAD 1999

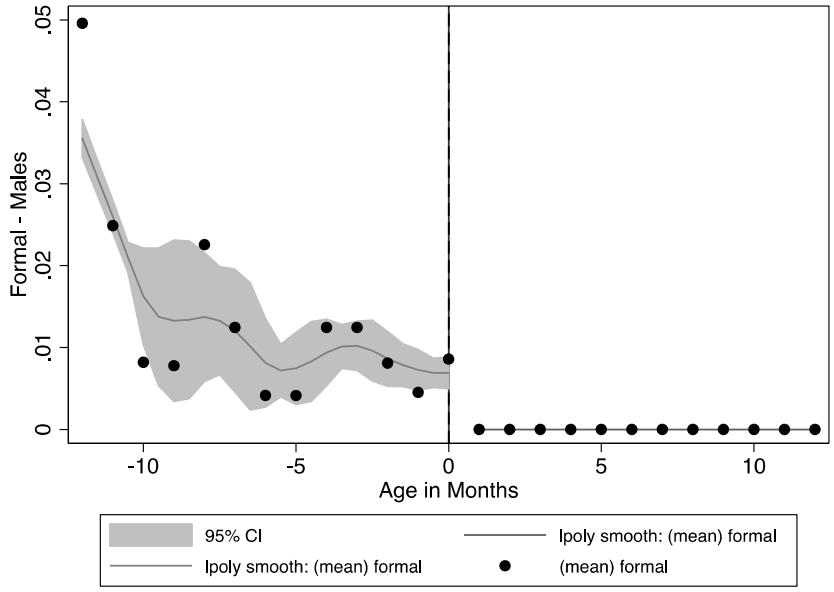


Figure C.3: Local Linear Regression Informal Paid Work—Males. PNAD 1999

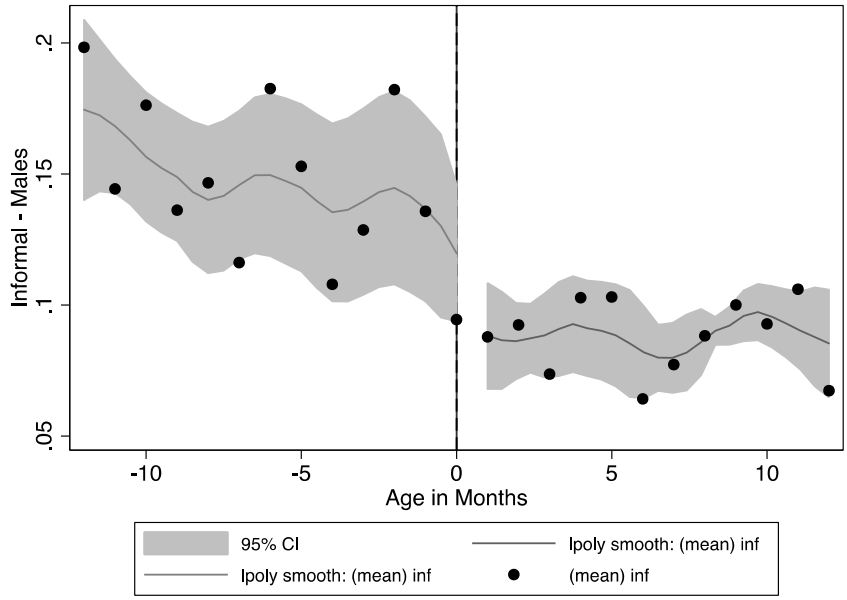


Table D.1 – Long-Term Quantile Intent-to-Treat Effects – Pooled Estimates. PNADs 2007-2014

	Q=0.2	Q=0.4	Q=0.6	Q=0.8
		Log Earnings		
D	-0.062***	0.00	-0.017	0.00
	(0.021)	(0.020)	(0.024)	(0.022)
N	14522	14522	14522	14522

Note: *** Statistically significant at 1 percent. Robust standard errors in parentheses. The reported results refer to the piecewise linear specification of the smooth function.