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Article

Minimum Age Regulation and Child Labor: New Evidence from Brazil

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Abstract

This study presents new evidence on the effects of minimum age regulations obtained from a natural experiment. In 1998, a constitutional reform in Brazil changed the minimum working age from 14 to 16. The reform was the legislative counterpart of a broad set of measures taken by a government strongly committed to eliminating child labor. This article investigates the role of the minimum working age in this context. The setting allows for improvements upon past approaches based on comparing employment rates of children at different ages. A discontinuity in treatment is exploited, namely the fact that only children who turned 14 after the enactment date (mid-December 1998) are banned from work. According to regression discontinuity and difference-in-discontinuity designs, the null hypothesis of no overall effect of the ban cannot be rejected. Throughout the methods and specifications, an employment effect in a confidence interval of $[-0.06, 0.03]$ (in percentage points) is found. A detailed heterogeneity analysis is performed and provides suggestive evidence of diminishing child labor trends in regions characterized by higher labor inspection intensity, which is interpreted as a trace of there being a law. However, contrary to what has been claimed in recent studies, the law seems not to have produced sizeable effects overall, at least in the short run. Power calculations and extensive sensitivity checks support these conclusions.

JEL classification: J08, J22, J23, J88

Keywords: child labor, ban, minimum working age, regression discontinuity

1. Introduction

Child labor bans in the form of minimum legal age of employment exist in most countries around the world. In principle, these laws could decrease child labor, if they are applied uniformly across different types of activities, or lead to its reallocation toward unregulated sectors. Overall, there is little evidence that minimum employment age regulations have any effect at all and, in particular, that they are influencing child engagement in paid work (Edmonds and Shrestha 2012). There are two main reasons

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for this observation. First, it seems that minimum age legislation is not enforced in general. In many poor countries, where child labor is prevalent, minimum age laws seem to be motivated by global political concerns rather than genuine intentions—and are rarely followed by appropriate enforcing actions (Boockmann 2010). The second reason for seemingly ineffective laws is the difficulty of detecting an effect. Standard approaches rely on comparing child employment rates around the minimum legal age, making it difficult to disentangle the effect of the law from the underlying age-employment trend (Edmonds 2014).

Against this background, the present article focuses on a change in the Brazilian law regarding the minimum working age. On December 15th, 1998, a constitutional amendment increased the minimum legal age for work in Brazil from 14 to 16. This case is interesting because, in contrast to the aforementioned artificiality surrounding minimum working age reforms worldwide, the Brazilian government has proven its genuine intention to curb child labor since the 1990s through a series of important policy measures. The reform under study was the legal counterpart of redistributive policies aimed at reducing child labor through conditional cash transfers, such as the local implementation of Bolsa Escola since 1995 (and support of the federal government from 1997 onward), the launch of PETI in 1996, and improvements in the availability and quality of the school system.[1] Hence, in 1998, the minimum age reform may have corresponded to a real intention of the government to make it work. Moreover, Brazil was characterized at the time by an operational system of labor inspections; these inspections had been refocused on the detection of illegal child labor in the years prior to the constitutional change (Almeida and Carneiro 2009; Coslovsky, Pires, and Bignami 2017). Nevertheless, this article finds that enforcement and compliance may have been heterogeneous across regions of Brazil.

Another interesting aspect is the design of the reform. The present study shows that the new ban affected those who turned 14 years old after mid-December 1998 but not those who turned 14 before. This discontinuous treatment allows local estimation techniques to be used to extract a causal effect of the ban, improving upon past studies. Using the national household survey *Pesquisa Nacional por Amostra de Domicilios* (PNAD), the study looks at banned and unbanned children in September 1999, nine months after the passing of the law. Regression discontinuity (RD) designs are used to exploit the discontinuous eligibility for work depending on children's birth dates. Difference-in-discontinuity estimations (DDisc) additionally apply time differencing with respect to children of the same cohort but observed before the reform (in September 1998). An alternative comparison group (children of the same age prior to the reform) is also used, as well as a combination of both same-cohort and same-age placebo groups for a local triple-difference estimation. To the extent that the discontinuous treatment is enforced, these approaches provide new evidence for the analysis of minimum employment age laws; in particular, they avoid the bias inherent in age trends and global difference-in-difference methods. As will be shown in the power calculations, this approach has the capacity to detect a potential effect of the ban that would be economically significant.

The results can be summarized as follows. RD and DDisc estimations fail to reject the null hypothesis that the new ban has no effect on child labor overall. Throughout the methods and the specification, an employment effect in a confidence interval of $[-0.06, 0.03]$ (in percentage points) is found. There is also no sign of an effect on school enrollment or on labor substitution between family members. These results are not due to a lack of statistical power and are robust to alternative methods and specifications (time windows around the cutoff, set of covariates, use of sampling weights, etc.). Heterogeneous effects are estimated using spatial variation or alternative employment definitions, focusing on the "inspectability" of a child's job in addition to the standard characterization in terms of formal versus informal employment.

1 PETI ("program to eradicate child labor") aimed to combat the worst forms of child labor through conditional cash transfers and municipal funds for after-school activities. Bolsa Escola, which preceded Bolsa Familia, was implemented on an experimental basis in 1995–98 in Distrito Federal before being generalized in 2001.

The only variant leading to statistically significant effects of the new law on child labor—a diminishing effect—concerns regions where the rate of labor inspections is high. This result can be interpreted as suggesting evidence of there being a law—and an enforceable discontinuous treatment. There is no sign of geographical heterogeneity along other dimensions. The results of this study are discussed in the context of a relatively small literature on using natural experiments to identify the potential effect of child labor bans.

2. Background

2.1. Minimum Age Regulation in the Literature

Theoretical Literature

Minimum age of employment regulations have been one of the most-used tools worldwide for combating child labor. Therefore, they have received a great deal of attention in the theoretical literature on child labor.[2] The seminal article of Basu and Van (1998) considers only one sector of employment for child labor, so that a ban can completely prohibit it. Basu (2005) added a second, unregulated sector. In a two-sector model, enforced minimum age regulations can divert children from regulated activities to nonregulated activities rather than eliminating child employment. In fact, the rise in labor costs to employers induces a lower demand for child labor and lower child wages (an equivalent result is obtained if employers recoup the risk premium by reporting these costs on children's wages). Overall child labor may even increase if depressed wages lead budget-constrained households to send more children to work (Bharadwaj, Lakdawala, and Li 2019).[3] This could also happen if children shifted away from the regulated sector no longer bring in external resources. Edmonds and Shrestha (2012) define three necessary conditions for the sector reallocation following a ban in the regulated sector to be neutral in terms of overall child employment: "adults" (i.e., parents or siblings above the minimum age) can move freely between the household and the regulated sector (competitive adult labor markets); adults and children are perfect substitutes subject to a productivity shifter (substitution axiom); and the household can freely substitute adult and child labor between productive tasks inside the household (non-saturation).

Empirical Literature

Empirical analyses are relatively scarce. Historical studies focus on high-income countries at the turn of the 19th century and found little effect of minimum age regulations due to a lack of enforcement. For the United States, Moehling (1999) examined laws implemented in manufacturing employment between 1890 and 1910. Only a small impact on child labor is found, possibly because enforcement, including labor inspections, lagged behind the passage of the law. Manacorda (2006) exploited heterogeneity in minimum working ages across US states in 1920 and found that being ineligible makes a child less likely to work but that this effect is very modest because it simultaneously makes the siblings *more* likely to work.

These spillovers within families are also emphasized in the work of Bharadwaj, Lakdawala, and Li (2019), who studied the introduction of minimum working age legislation in modern India. Using employment surveys conducted before and after the 1986 law, as well as age restrictions that determined whom the ban applied to, they showed that the relative probability of child employment actually *increased*. The explanation pertains to a fall in child wages relative to adults' wages in the industrial, regulated sector, which leads budget-constrained households to rely on more work by siblings—or by the under-minimum-age children themselves when there is no possible sibling substitution. The most comprehensive study on

2 This literature includes early contributions by Basu and Van (1998), Ranjan (1999), Baland and Robinson (2000), Dessy (2000), Basu (2005), and Dessy and Pallage (2004).

3 See Edmonds (2007) for a review of the links between household poverty and child labor.

minimum age regulations in contemporary developing countries was conducted by Edmonds and Shrestha (2012). Using data from 59 low-income countries, they showed an absence of discontinuity in child participation in paid employment around the legal minimum age, suggesting that the enforcement of such restrictions is weak at best.

One of the few studies pointing to an effective role of a child labor ban is Piza and Souza (2016). Focusing on the same Brazilian reform as the present work and using a difference-in-difference (DD) approach, they found a decrease of 4 percentage points in boys' employment, i.e., a 95 percent confidence interval of [−0.067, −0.013] in their main specification, driven by a decline of informal employment. These results differ significantly from the estimates in this article. The limitations of DD approaches will be discussed later.

2.2. Institutional Context

A Set of Measures against Child Labor

Brazil is a particularly interesting case study: there has been a significant shift in its position regarding child labor, from mere tolerance to concerted efforts to eradicate child work. The strong commitment made by the Brazilian government started in the mid-1990s after the election of President Fernando Henrique Cardoso, who declared child labor to be "an abhorrent practice and an abuse of human rights." His government displayed a strong intention, followed by the implementation of concrete measures, to "wipe out child labor" (Cardoso 1997). Del Vecchio (2005) cites three main policies undertaken during President Cardoso's mandate: conditional cash transfer programs aimed at encouraging families to keep children in school and/or out of work; inspection and enforcement at the state level directed at child labor; and programs targeted at specific sectors and industries. Child labor has effectively been reduced massively since that time, though over a longer period; according to PNAD, child labor decreased by 68 percent between 1992 and 2015. This decline is possibly due to the combined effect of labor market reforms, education, and social protection reforms, as well as sustained economic growth. There is no real consensus in the academic literature as to the main driving forces behind the decline in child labor (see Rosati et al. 2011). The present study examines new empirical evidence on the role played by the change in minimum employment age.

The 1998 Reform and Labor Inspections

The Constitution of 1988 and a specific federal law passed in 1990 (*Lei do Estatuto e do Adolescente*) had set the minimum legal age of entry into the labor market at 14 years. In December 1998, the Brazilian congress raised the legal age for "admission to work" to 16. This law was enshrined as a change in the constitution (Constitutional Amendment No. 20).[4] It was part of a more general reform of the social security system, including a rise in the retirement age. The minimum age component laid the legal framework for combating child labor even before the ratification of International Labour Organization (ILO) conventions C138 on the Minimum Age of Employment (in 2001) and C182 on the Worst Forms of Child Labor (in 2000). While many countries had adopted minimum age legislation without devoting the necessary resources to credible enforcement (Edmonds and Shrestha 2012), it can be argued that Brazil was in a better position, especially given its aforementioned comprehensive strategy for combating child labor (Fortin, Lacroix, and Drolet 2004). It also benefited from an operational system of labor inspections to detect firms employing children and fine them if labor conditions were not appropriate or the legal age not respected. This system was phased in throughout the 1990s; the early years were dedicated to measures

4 An exception was made for the apprenticeship status, which was allowed from 14 years of age (compared to 12 previously). Apprenticeship in Brazil is, however, a specific program that involves supervision by specialized vocational training institutions, so in practice it affects only a tiny fraction of children. Note also that the law prohibits employees younger than 18 from working in unhealthy, dangerous, or arduous conditions. Therefore the age condition under study does not pertain to hazardous work.

of labor formalization[5] while the 1996–98 period prioritized the reduction of child labor through labor inspections (Rivero de Araujo and Maduro 2010; Coslovsky 2014).[6]

Discontinuous Treatment and the Case of Registered Work

The natural experiment studied in this article relies on the fact that in mid-December of 1998, children who had not yet turned 14 were banned from the labor force. Local estimations will therefore be based on birth date around mid-December 1984 as the forcing variable. More background information is provided here on how effective this discontinuous treatment may have been. Enforcement of the new law based on birth date rather than age must have been facilitated by the fact that birth certificates are widespread in Brazil and hard to falsify (see later discussion). The discontinuous treatment is primarily expected in formal/registered employment. In the Brazilian context, an individual being in formal work is traditionally defined by their holding a labor card (*carteira assinada*) and/or contributing to the social security system.[7] Concretely, the Ministry of Labor and Employment stopped issuing labor cards to children under 16 just after the passing of the new law (Almeida and Carneiro 2009), so that the treated group in this study—children turning 14 after mid-December 1998—could not officially work after this date.[8] This should especially be true for registered employment. With the data described hereafter, an average registered employment rate of 0.6 was actually found for the unbanned children (those who turned 14 just before the reform date), versus a rate of 0.2 among the newly banned children.[9] Formal employment is used here for illustrative purposes, to exemplify the possibility of discontinuous treatment. However, children with a labor card represent a very small fraction of all working children in Brazil, so one should not expect much effect in the formal sector (half a percentage point at most).

Incentives and Awareness

The law and its discontinuous treatment may have primarily affected children not holding a labor card, who account for the bulk of child labor in Brazil. Note that in the Brazilian labor market many firms

5 Regularization actually increased in the 1990s following micro-policy, notably the "SIMPLES" law that facilitated registration, lowered tax on small businesses, and improved labor inspection and legal awareness among workers (Berg 2011).

6 From 1995 onwards, child labor was included in the agenda of the Labor Inspection Secretariat (the agency in charge of labor inspection within the Ministry of Labor and Employment), and a State Committee Against Child Labor was set up, comprising a group of inspectors who specialized in fighting child labor (De Almeida and Kassouf 2016). A "Map of Child and Adolescent Labor Indicators," first published in 1996, also gathered information collected by inspectors, with the aim of making the prevention and eradication of child labor more effective. By 1998, the Ministry of Labor and Employment was continuously conducting labor inspections at work sites—either routine inspections of labor market regulations or specific inspections focused on child labor violations (often following complaints or tips to labor inspectors from workers, NGOs, teachers, the media, and other sources). Labor inspectors worked together with prosecutors from the Brazilian Public Prosecutor's Office, who had broader powers and were able impose larger fines in cases of illegal child labor (see US Department of Labor 2002). In the 1996–2000 period alone, 49,106 adolescents had their situation regulated and their labor rights ensured; 4,542 penalties were imposed on companies that disobeyed the laws prohibiting child labor and protecting adolescent workers (Federative Republic of Brazil 2001).

7 The holding of a labor card is a commonly used definition in past studies of the Brazilian labor market; see, for example, Tannuri-Pianto and Pianto (2002), Henley, Arabsheibani, and Carneiro (2009), Bargain and Kwenda (2014), and Fairris and Jonasson (2016).

8 In principle, children in the control group could still work. Arguably, there is some legal ambiguity regarding the possibility of children in the control group obtaining a work permit if they did not ask for it before that date. Yet these children could still contribute to the social security system and are therefore considered informal workers only with respect to the work registration; this point will be discussed further below.

9 This contrast between subcohorts is confirmed by an administrative data source, the *Relação Anual de Informações Sociais* (RAIS), for the year 1999, which gives similar proportions of two-thirds unbanned and one-third banned children in formal activity.

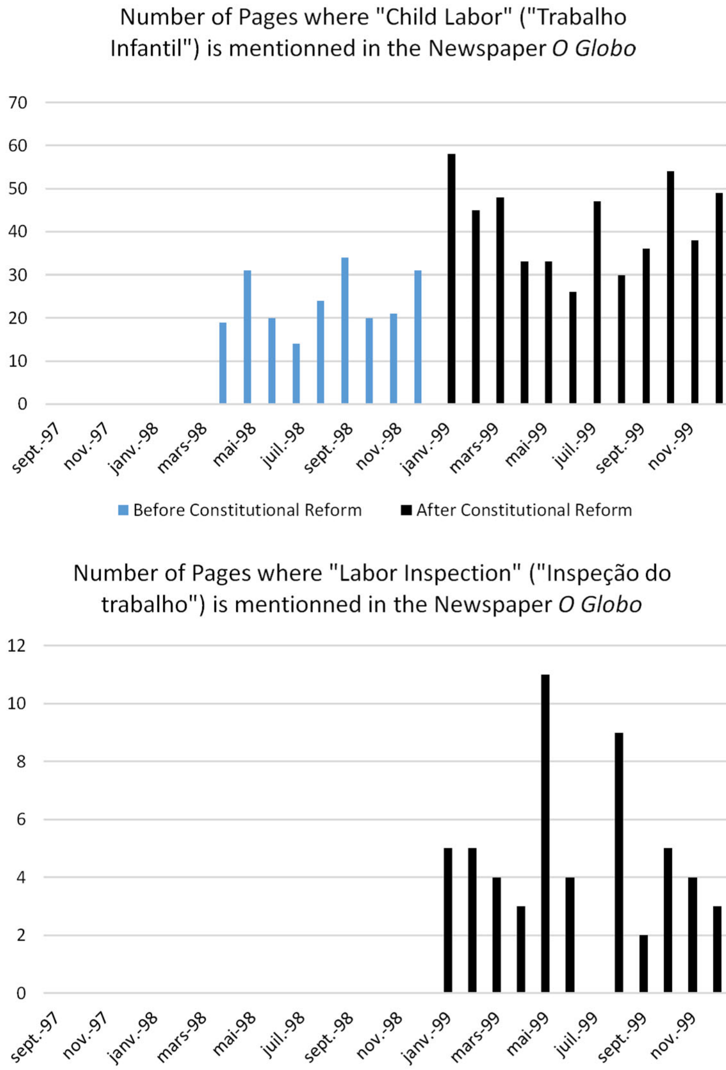
are actually not fully informal; large firms tend to employ both workers with a labor card and workers without. These firms may nonetheless have had incentives to follow some of the other labor market regulations. Almeida and Carneiro (2009) found that labor inspections in Brazil focused more heavily on child labor (minimum age as well as health and safety conditions)—in aspects such as payment into the job security fund—than on worker registration. Also, penalties imposed on employers for the illegal hiring of children were generally more severe than (and would add to) those due to lack of registration. Hence, firms had more of an incentive to verify children's ages than to check other legal requirements such as labor cards. The question is whether firms would have been aware of the implications of the 1998 reform at the time of the PNAD (September 1999) and had a chance to screen children according to the new labor law. Informal evidence from the main Brazilian newspapers indicates that the reform was widely advertised (e.g., in O Globo, https://acervo.extra.globo.com). Admittedly, the media focused on the core of the reform, i.e., a change in the pension system, rather than on the specific amendment (no. 20) concerning the legal working age. Nonetheless, the number of articles in the press addressing the topic of "child labor" and the number of mentions of "labor inspections" increased sharply just after December 1998, as illustrated in fig. 1.

Employment and Geographical Heterogeneity

Despite the elements discussed above, it is possible that the new law missed informal firms that are typically out of the reach of labor inspectors. According to US Department of Labor (2002), Brazil's Ministry of Labor and Employment had expanded its mandate to inspect informal-sector establishments but faced the difficulty of not being able to enter private homes, for instance. For this reason, it is of interest to consider job characteristics other than the usual divide between formal and informal employment. As discussed above, a formality status based on the labor card is probably not the most relevant definition when looking at the potential impact of the minimum age regulation. In principle, the Brazilian constitution does not specify that the minimum age law should apply to a particular type of activity or sector; but in practice the law is more likely to be binding in types of employment that are more "inspectable," a distinction that will be considered in the heterogeneous analysis. Heterogeneous effects may also reveal cases in which the law had more chances of being enforced. In particular, it is likely that the effectiveness of the law has been geographically heterogeneous. This question will be addressed by considering rural/urban variation and spatial heterogeneity in the intensity of labor inspection. In Brazil, the enforcement of labor inspection is decentralized at the state and subregional levels. Labor inspectors are assigned to enforcement offices located in cities across the country. Each *subdelegacia* may have several enforcement offices, depending on the size and economic activity of the region. Analyzing the effects of enforcement of employment regulations on various outcomes, including formal and informal employment, Almeida and Carneiro (2009, 2012) found that labor regulations are not enforced uniformly in Brazil. In what follows, a measure of inspection intensity will be developed using their data. A stronger expression of the law, if any, would be expected in high-inspection states.[10] The basic statistics are consistent with this intuition: in low-inspection states, employment rates are similar for children turning 14 before and after the reform (0.205 and 0.206, respectively), whereas in high-inspection states there is a greater contrast in the figures (0.237 versus 0.191).

10 Note that the measure of inspection intensity is not necessarily correlated with the prevalence of child labor, as several factors may affect the allocation and productivity of inspectors in a decentralized country such as Brazil. The descriptive statistics in Aransiola and Justus (2017), using state-average levels of child labor and inspections for the period 2004–11 in Brazil, actually point to a negative relationship that may be (i) understated if the Ministry of Labor and Employment increases inspection effort in states that are intrinsically characterized by a higher prevalence of child labor, or (ii) overstated if some characteristics positively influence inspection rates while being negatively correlated with the rate of child labor (such as the degree of institutional development; cf. Almeida and Carneiro [2012]). The present article actually finds a cross-regional correlation of 0.11 between child labor and inspection rates.

Figure 1. Press Coverage of Child Labor and Labor Inspections



Source: Authors' calculations based on results from the search engine at https://acervo.extra.globo.com/.
Note: These graphs represent the monthly frequency of occurrences of "child labor" and "labor inspection" in one of the main Brazilian newspapers in the months preceding the reform.

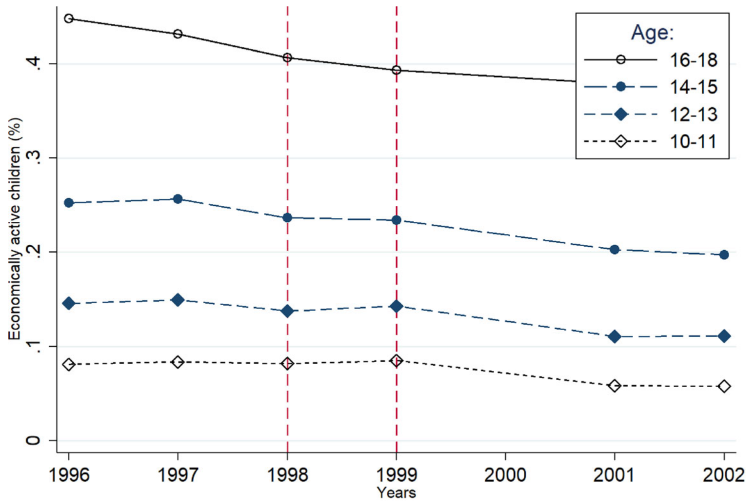
3. Empirical Approach

3.1. Data and Definitions

This study uses data from the PNAD, collected at the end of September every year.[11] The PNAD is the largest household survey in Brazil, covering around 350,000 individuals per year and containing usual sociodemographic information and the labor market status of every family member. Several subcohorts of children are examined in this article, as described below. The PNAD data contain the exact birthday of each child. The consistency of this variable has been verified, and children with missing values or obvious

11 The PNAD is conducted by the Brazilian census agency, *Instituto Brasileiro de Geografia e Estatstica* (IBGE). See the documentation at https://www.ibge.gov.br/.

Figure 2. Trends in Child Labor in Brazil



Source: Authors' analysis based on data from the *Pesquisa Nacional por Amostra de Domicilios*.
Note: This graph represents the time trends in child employment by age groups.

problems regarding their birth date information were omitted.[12] Children whose head of household was aged 18–60 were retained in the sample. Figure 2 displays time trends in the rate of economically active children by age groups using this selection. It shows higher rates of employment among older children and a broad decline in child labor over the period, possibly reflecting the combined effect of various policy measures, as discussed above. The employment rate of children aged 14 and 15 decreased only slightly between 1998 and 1999, without a marked differential compared to younger children. This simple observation based on broad age groups does not allow conclusions to be drawn about the potential effects of the reform.

Child Labor Definitions and Sectors

The main outcome of interest is a child labor market participation dummy. It is an indicator of economically active children, defined as children performing paid or unpaid work for their own family or others, excluding those who perform only household chores inside the family.[13] The resulting participation rate for the 1999 selection is 20.7 percent, with a marked gender difference (14.3 percent among girls and 27.6 percent among boys). The main activities include commerce and food-related services (shops, outdoor food sales, food services, etc., amounting to 28 percent of child labor), manufacture and transport (15 percent), domestic services, (11 percent), agricultural work (10 percent), construction (8 percent), and various other services. Heterogeneity in broad employment types will be considered:

12 Due to rounding errors, these problematic cases amount to only 1.8 percent of the sample of households with children.

13 Note that the definition of child employment used in Piza and Souza (2016, footnote 10) is different from the one used here. Their definition is extensive and includes children who are employed (variable v9001 in the original PNAD data files), who are looking for a job (v9115), who are active workers but prevented from working due to external causes in the week of reference (v9004), or who worked in the last 12 months (v9106). The definition used in this article is more in line with the ILO notion of economically active children, i.e., employed (v9001), looking for a job (v9115), active but prevented from working (v9004), or working in agriculture (v9002) or construction (v9003), but excluding children who are working exclusively for self-consumption (v9008) or self-production (v9029). Replication files, available from the authors upon request, show that these differences do not explain why a significant effect was found by Piza and Souza (2016).

registered versus unregistered work (based on the standard definition of holding a labor card), and highly versus less inspectable activities. The latter distinction is based on whether the workplace is visible/noticeable and accessible to labor inspectors. Inspectable work is defined as that taking place in shops or factories; less-inspectable work is that done in private homes (e.g., domestic work), in vehicles, or outdoors (e.g., street vending). Farms are visible workplaces, but they are private properties and thus not necessarily accessible; hence they will be treated alternately as inspectable or not. Total child employment (20.7 percent) is decomposed as follows: 7.7 percentage points in inspectable work, 4.8 percentage points in less inspectable work, and 8.3 percentage points in farm work.

Enforcement: Inspection Data

When checking for potential heterogeneity in the effect of the law, the focus will be on states characterized by high rates of labor inspection. Administrative data on the municipality-level rates of inspection by firm are used, which were collected for the year 2002 by the Department of Inspections at the Ministry of Labor and Employment and provided by Almeida and Carneiro (2009). Two potential concerns must be addressed. First, this information is not available for 1999. It is unlikely that inspection rates changed much between 1999 and 2002, but it would be interesting to have a more contemporaneous measure.[14] Second, the Ministry of Labor and Employment may have increased the intensity of inspections in places where the law was more difficult to enforce in 1999. Both these issues are addressed here by drawing from the work of Almeida and Carneiro (2009, 2012), who instrumented the inspection rate $I_{m,\,t}$ at date $t = 2002$ in municipality m by the regional number of inspectors, $N_{r,\,t}$, and the travel distance between the municipality and the nearest enforcement office, D_m. The local inspection intensity is assumed to be a relatively stable function of these two inputs, $I_{m,\,t} = I(N_{r,\,t}, D_m)$. Hence, this function can be estimated using the 2002 data and its value predicted for the number of inspectors in 1999, giving $\widetilde{I}(N_{r,1999}, D_m)$ as a proxy for $I_{m,1999}$. In this way, an inspection intensity measure can be obtained that is less endogenous to local conditions and more contemporaneous to the working sample of the present study. Note that municipality-level inspection rates per firm, both $I_{m,\,2002}$ and $\widetilde{I}(N_{r,1999}, D_m)$, are averaged at the state level, so much of the local heterogeneity should also be averaged up. The focus is on whether a child is in a state with above-median predicted inspection intensity.

3.2. Empirical Strategy

Regression Discontinuity (RD)

If the ban is effective, a discontinuous drop in child employment should be observed at the cutoff, i.e., for children turning 14 after mid-December of 1998. Hence, RD estimations require only cross-sectional observations of the cohort born around mid-December of 1984 once the reform is in place (September 1999). The running variable (birth date) is denoted by τ, expressed in days relative to the cutoff. Let $Y_{i\tau}$ denote the outcome (labor supply) for a child i born on date τ, and let the treatment variable $1(\tau >$ mid-Dec$)$ be equal to 1 if the child is born after mid-December of 1984. The parametric model estimated on 1999 data is

$$Y_{i\tau} = \alpha_0 + X_i\alpha_1 + \beta^{\mathrm{RD}} \cdot 1(\tau > \text{mid-Dec}) + \delta(\tau) + \eta_{i\tau}, \tag{1}$$

where β^{RD} represents the potential employment effect of the ban, identified by discontinuous eligibility among similar children. The key identification assumption is that $\delta(\,\cdot\,)$ is a continuous function, i.e., child labor has no reason to vary discontinuously with birth date. The function $\delta(\,\cdot\,)$ should certainly be flexible enough to accommodate nonlinearities due to age or subcohort heterogeneities around the cutoff. The results should not depend on a specific functional form; their stability across a variety of parametric

14 Indeed, the distribution of the administrative workforce across states did not change much over this period (see Coslovsky, Pires, and Bignami 2017). The relative rates of inspections across states are shown to be very stable over time; see fig. 2 in De Almeida and Kassouf (2016).

specifications will be tested. Covariates X_{it} are also included, which may improve the efficiency of the estimation but are not necessary for identification. They include ethnicity, household size and adults' total income, the age, gender, and years of schooling of the head of household, and regional and urban/rural dummies. The baseline estimations will make use of sample weights, as provided in the PNAD surveys. Estimates without weights will also be reported for sensitivity checks.[15] Finally, Y_i will simply be a dummy for child work in the estimations, so that a linear probability model can be used while clustering standard errors by cohort × month of birth.

The RD analysis focuses on PNAD data collected in the year following the reform, i.e., the wave of September 1999. The main cohort of interest consists of children who turned 14 years old within three months before and after the cutoff of mid-December 1998, i.e., children born between mid-September 1984 and mid-March 1985. This sample comprises 3,007 children, representing 1,392,634 children when population weights are used.

Discussion and Checks

Some potential issues with the RD approach are as follows. First, identification hinges on the assumption that children born just before and just after the reform date are almost identical. Naturally, the further away from the cutoff, the more fragile this assumption is. This point illustrates the fundamental distinction between the RD approach and identification relying on double differences. It also gives more credit, but less precision, to a narrow time window such as the one used here (±3 months around the cutoff). Supplementary online appendix S2 performs checks of whether household characteristics are identical on both sides of the discontinuity, using observations at either ±3 months or ±6 months around the cutoff. The second concern is that possible manipulation of the running variable cannot be ruled out. Birth registration is compulsory in Brazil, and without a birth certificate a child cannot be vaccinated, enroll in school, or obtain a worker's card.[16] This should limit measurement error in the empirical application, and it implies that firms should in principle be aware of the precise ages of their workers, or should be able to report it to labor inspectors. Nonetheless, potential falsification of children's birth dates by families or employers cannot be precluded. It does not necessarily entail misreporting of children's information to the PNAD enumerators, though. If child age is correct in the PNAD, then the analysis would just be in the presence of noncompliance, which would make the resulting estimates intention-to-treat (ITT) rather than average treatment effects.[17] If there is misreported birth-date information in the PNAD data as well—the same type of misdeclaration as to labor inspectors—then a discontinuous density in the age distribution of children should be observed around the reform date.[18] The standard McCrary tests are conducted.

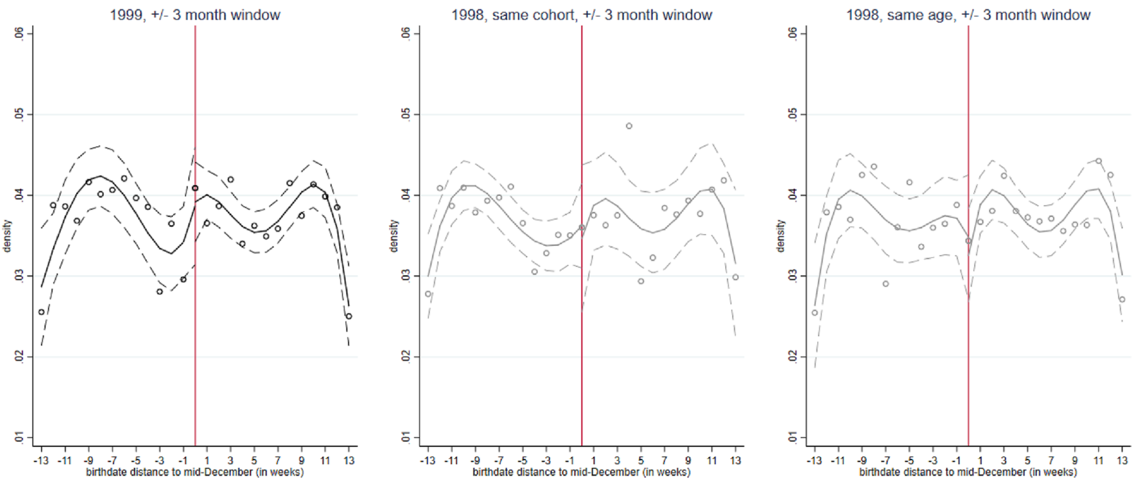
15 The PNAD is first stratified by large geographic strata (states or sub-states), and within each of these a two-stage cluster sampling is performed, where primary sample units are census enumeration areas while size measures (sample weights) correspond to the number of private households from the latest population census (see Silva, Pessoa, and Lila 2002). This procedure leads to reasonably good representativeness of the sample in terms of demographic composition (see the extensive comparison between the PNAD and census data in UNESCO Institute for Statistics [2017]).

16 The majority of children have a birth certificate, which is also the result of an increased access to birth registration (UNESCO Institute for Statistics 2017). A small fraction of children in rural areas had no birth certificate. This accounts for around 20 percent of children in the late 1990s according to the IBGE, especially among indigenous populations. According to Paes-Sousa, Santos, and Miazaki (2011), lack of a birth certificate is an indicator of extreme poverty and of residence in an isolated area. The heterogeneous analysis provided in this article will focus, among other things, on urban children.

17 In other words, some of the children assigned to treatment (i.e., banned from work) due to their age are in fact declared older than they are and allowed to work.

18 A related problem is the presence of measurement errors in age declaration. This type of issue is usually due to rounding errors that arise from using age in years (Dong 2015), whereas age in days is used in this study. These errors should be uniformly distributed or, at least, not discontinuously distributed. In the worst case, they would simply make the estimates ITT effects.

Figure 3. McCrary Density Checks around the Cutoff



Source: Authors' analysis based on data from the 1998 and 1999 *Pesquisa Nacional por Amostra de Domicilios*.
Note: These graphs represent the density of observations by birth-date cells (in weeks) around the cutoff.

The first graph of fig. 3 shows that in September 1999, there is no significant difference in birth-date density around the cutoff. Formal tests cannot reject the null hypothesis of equal density (McCrary 2008).[19] Finally, misreporting may be higher in states with greater enforcement. To check this, the McCrary density graphs are replicated for quartiles of regional inspection intensity; the results in fig. 4 confirm the absence of obvious manipulation effects.

Difference-in-Discontinuity (DDisc)

With the DDisc approach, it is possible to combine the advantages of a RD design, notably the identification around the cutoff, with those of a DD method, which exploits the time change due to the reform. Statistical estimations are conducted on pooled data for 1998 and 1999. The DDisc model is

$$\begin{aligned} Y_{i\tau t} &= \alpha_0 + X_{it}\alpha_1 + \lambda_1^{\text{DDisc}} \cdot 1(\tau > \text{mid-Dec}) + \lambda_2^{\text{DDisc}} \cdot 1(t = 1999) \\ &+ \beta^{\text{DDisc}} \cdot 1(\tau > \text{mid-Dec}) \cdot 1(t = 1999) + \delta_t(\tau) + \eta_{i\tau t}. \end{aligned} \tag{2}$$

Here, the dummy $1(\tau > \text{mid-Dec})$ captures only the birth-date effect, i.e., whether a child is born after mid-December of any year, and potential seasonality in child labor. The dummy $1(t = 1999)$ is a mere time effect that may capture business cycles. The treatment effect β^{DDisc} corresponds to their interaction, i.e., being subject to the new ban. Taking the time difference between 1999 and 1998 gives

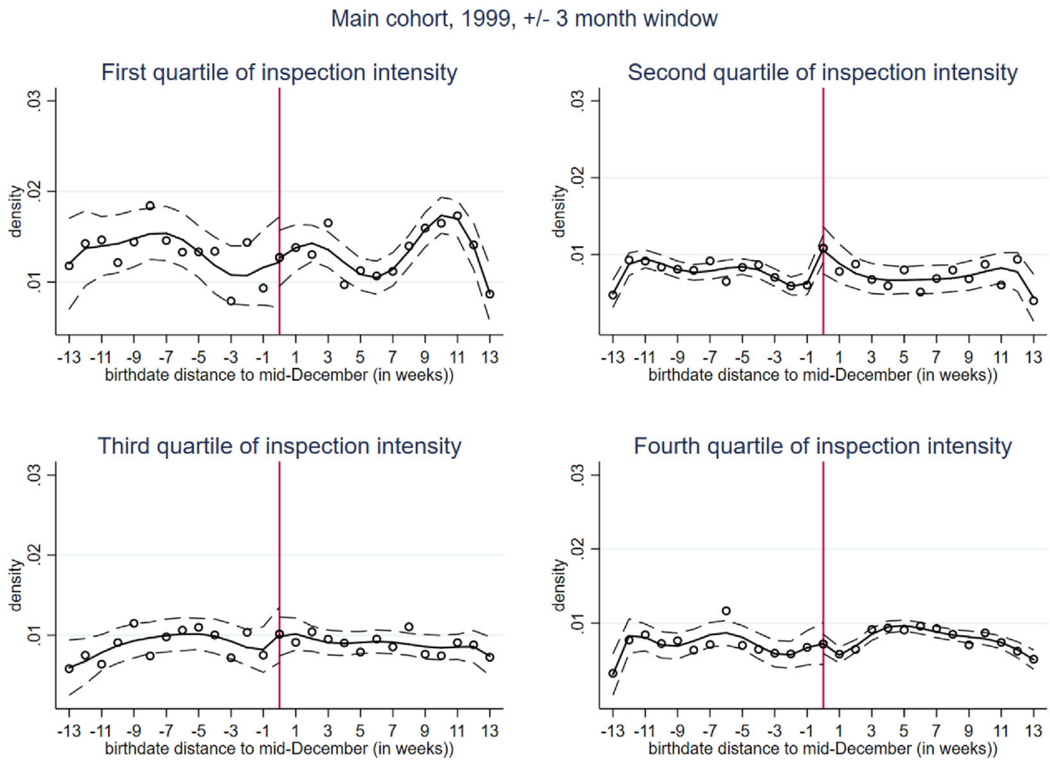
$$\Delta Y_{i\tau} = \left(\lambda_2^{\text{DDisc}} + \Delta X_{it}\alpha_1\right) + \beta^{\text{DDisc}} \cdot 1(\tau > \text{mid-Dec}) + \Delta\delta(\tau) + \Delta\eta_{i\tau}, \tag{3}$$

where $\Delta Y_{i\tau}$ is the difference in outcomes between 1999 and 1998, and $\Delta\delta(\tau_i)$ is a polynomial form of the same specification as $\delta(\tau)$ in the RD design.[20] The treatment effect captures time variation in the potential employment change around the birth-date cutoff. The usual placebo group used in double differences is the same cohort prior to the reform. In this case, these *same-cohort* children are observed a year

19 For comparison, similar graphs for the year before are also shown, which use either the same cohort (a year younger) or children of the same age (previous cohort).

20 This is easily illustrated. For instance, take a linear spline of the form $\delta_t(\tau) = \delta_t^{\text{left}} \cdot 1(\tau \leq 0) \cdot \tau + \delta_t^{\text{right}} \cdot 1(\tau > 0) \cdot \tau$. It gives a time-demeaned term $\Delta\delta(\tau) = \Delta\delta^{\text{left}} \cdot 1(\tau \leq 0) \cdot \tau + \Delta\delta^{\text{right}} \cdot 1(\tau > 0) \cdot \tau$ with $\Delta\delta^k = \delta_{1999}^k - \delta_{1998}^k$ for $k =$ left, right.

Figure 4. McCrary Density Checks by Quartiles of State Intensity of Labor Inspections



Source: Authors' analysis based on data from the 1999 *Pesquisa Nacional por Amostra de Domicilios*.
Note: These graphs represent the density of observations by birth-date cells (in weeks) around the cutoff, for different quartiles of state intensity of labor inspections.

younger—they are under 14 years of age in 1998 and hence ineligible for work according to the old ban. Alternatively, *same-age* children could be used, i.e., children from the previous cohort who were above 14 at the time of observation (September 1998) and hence allowed to work according to the old ban.

Using a ±3-month window, the 1998 samples comprise 2,966 children for the same-cohort approach (representing 1,364,225 children when sample weights are applied) and 2,915 children for the same-age approach (1,349,511 children when sample weights are applied). These samples, along with the main cohort in 1999, are described in the upper panel of fig. S1.1 (supplementary online appendix S1). For sensitivity analyses, it is possible to experiment with a ±6-month window around the cutoff, as described in the lower panel of fig. S1.1. With the same-cohort approach, however, this broader window adds a group that mixes banned children (aged under 14 in September 1998) and unbanned children (aged over 14 in September 1998) under the old regime.

Comparison of Methods

This subsection discusses how DDisc designs overcome several shortcomings of the methods used to analyze minimum age legislations, of DD methods, and of RD designs (see also Grembi, Nannicini, and Troiano [2016] and Somers et al. [2013]). First, the literature on minimum employment age tends to compare employment rates of children below and above the minimum age while controlling for age trends in employment using employment rates at younger ages (Edmonds and Shrestha 2012). As stated by Edmonds (2014), this approach answers only the question of what would happen to paid employment if minimum age of employment regulations were extended for an additional year: "If minimum age laws shift the entire age distribution of employment, or have gradual, cumulative effects on employment, this approach could not detect these effects. The limited question considered in these studies could be resolved by focusing on countries that change their laws." This is precisely what the DDisc method does.

Exploiting a change in the minimum age is also the idea behind DD estimations, which make *global* comparisons of employment rate around the cutoff (Piza and Souza 2016). With the *same-cohort* approach, these estimates control for the employment-age relationship as proxied by children who are a year younger. This is very similar to the "age trend" approach described above; it is not clear whether the employment growth at ages 12 and 13 can be used for a valid comparison of the employment growth at ages 13 and 14.[21] With the DDisc, identification is *local*, around the cutoff, and is therefore less affected by different employment trends at different ages. With an alternative approach based on the *same-age* placebo group, DDisc estimations control for the "natural" relationship between employment and age around 14 years and 9 months (the median age at the time of data collection). Yet the backdrop relies on a different cohort, which may be affected by specific shocks; in particular, this older cohort may have experienced different phasing-in of conditional cash transfers, or may just be at a different point of the secular declining trend in child labor. This issue will be investigated in the sensitivity analyses.

Compared to RD estimations for 1999, the DDisc nets out potential pre-existing differences between the subcohorts on either side of the mid-December cutoff. Hence, DDisc can handle other time changes, such as co-treatments, at the cutoff; it only requires the effect of any co-treatment to remain constant between pre- and post-reform periods. In the present case, the RD interpretation may be defeated by specific shocks for individuals born on one side of the birth-date cutoff, such as weather shocks (e.g., severe flooding in Brazil in January 1985) or macro/political shocks (e.g., an acceleration of democratic changes in early 1985). Note that both RD and DDisc may still suffer from confounding policies occurring at the same time as the reform under study. This point has been checked extensively, and no indication of such policy measures that would come into effect around the reform date has been found. The exception is the social security reform itself, but the aspects of this reform other than the minimum working age legislation should not affect child labor and do not pertain to the same birth-date cutoff.[22]

21 Formally, it is easy to show the essential difference between DD and DDisc estimations. A standard DD model is of the form

$$\begin{aligned} Y_{it} = {} & \alpha_0^{\mathrm{DD}} + X_{it}\alpha_1^{\mathrm{DD}} + \lambda_1^{\mathrm{DD}} \cdot 1(\tau_i > \text{mid-Dec}) + \lambda_2^{\mathrm{DD}} \cdot 1(t = 1999) \\ & + \beta^{\mathrm{DD}} \cdot 1(\tau_i > \text{mid-Dec}) \cdot 1(t = 1999) + \varepsilon_{it}, \end{aligned}$$

where the coefficient β^{DD} captures the heterogeneous treatment status in September 1999 relative to the 1998 situation. In time-demeaned form, it is

$$\Delta Y_{it} = (\alpha_3 + \Delta X_{it}\alpha_1) + \beta \cdot 1(\tau_i > \text{mid-Dec}) + \Delta\varepsilon_{it},$$

so the main difference from DDisc designs is that the DD estimation does not control for the continuous relationship between the outcome and the running variable, i.e., it does not identify the effect locally. This is true even when τ_i is included in the set of characteristics X_{it} in the DD, since it is netted out. Assume that it is now interacted with time. Then replacing $X_{it}\alpha_1$ with $X_{it}\alpha_1 + \alpha'_{1t}\tau_i$ gives

$$\Delta Y_{it} = (\alpha_3 + \Delta X_{it}\alpha_1) + \beta \cdot 1(\tau_i < \text{mid-Dec}) + (\alpha'_{1,99} - \alpha'_{1,98})\tau_i + \Delta\varepsilon_{it},$$

which boils down to a linear form of the parametric DDisc model, i.e., quite a restrictive one.

22 Two reforms of the education system (*Lei do Estatuto e do Adolescente* in 1990 and *Lei das Diretrizes e Bases da Educação Nacional* in 1996) have imposed compulsory schooling until 14 years of age (or until completion of primary education if it is not achieved at 14). These policies cannot confound the minimum working age reform since the birth-date cutoffs are different in their cases (and even if the cutoffs were similar, the effect of educational reforms would be netted out in DDisc).

Difference-in-Difference-in-Discontinuity (DDDisc)

Finally, as part of the sensitivity analysis, a triple-difference version is considered.[23] Define two age variables: τ for the date (as above) and κ for the cohort. Denote by $T_\tau = 1(\tau > \text{mid-Dec})$ the binary variable indicating the day of birth relative to the cutoff, and introduce another dummy $T_\kappa = 1(\kappa = \text{maincohort})$ that equals 1 if the person is born between June 1984 and June 1985 (the main cohort) and equals 0 if the person is born between June 1983 and June 1984 (the previous cohort). The DDDisc model is

$$\begin{aligned} Y_{i\tau\kappa t} = {} & \alpha_0 + X_{it}\alpha_1 + \lambda_1^{\text{DDDisc}} \cdot T_\tau + \lambda_2^{\text{DDDisc}} \cdot T_\kappa \\ & + \lambda_3^{\text{DDDisc}} \cdot 1(t = 1999) + \gamma_4^{\text{DDDisc}} \cdot T_\tau \cdot T_\kappa \\ & + \gamma_5^{\text{DDDisc}} \cdot T_\tau \cdot 1(t = 1999) + \gamma_6^{\text{DDDisc}} \cdot T_\kappa \cdot 1(t = 1999) \\ & + \beta^{\text{DDDisc}} \cdot T_\tau \cdot T_\kappa \cdot 1(t = 1999) + \delta_t(\tau) + \eta_{i\tau\kappa t}. \end{aligned} \tag{4}$$

It exploits the variation induced by the law at the level of interaction between age and cohort, i.e., the fact that being subject to the new ban requires the individual to be born after mid-December and in the cohort born between June 1984 and June 1985. The model incorporates all the other terms, including time-invariant unrestricted age and cohort effects, T_τ and T_κ, as well as interaction terms.

4. Results

4.1. Main Results

Graphical Evidence

The first step of the analysis involves a visual inspection of the relationship between employment and birth date around the reform cutoff, which is an integral part of the RD analysis (Imbens and Lemieux 2008). Focusing on the selection with a ±3-month window, the first graph of fig. 5 plots average child employment rates by birth-date cells around mid-December of 1984 (in weeks), using employment status in September 1999 (nine months after the reform) from the PNAD. Linear trends on either side of the cutoff are depicted, with dashed lines representing the 95 percent confidence intervals. Children to the right of the cutoff were under 14 years old when the new ban became effective (under 14 years and 9 months at the time of observation). The conclusion from this graph is that there is no significant drop in employment at the cutoff. Linear splines indicate a lower average employment level among banned children, i.e., to the right of the cutoff, which is simply due to their younger age and hence lower labor market integration.[24]

The second and third graphs of fig. 5 show child employment rates in the placebo situation of September 1998 (three months before the reform), using same-cohort and same-age children, respectively. In both cases, no particular discontinuity is observed at the cutoff. This confirms the absence of pre-existing confounding policies or events that might have permanently affected specific subcohorts on each side of the cutoff and biased the RD approach.[25]

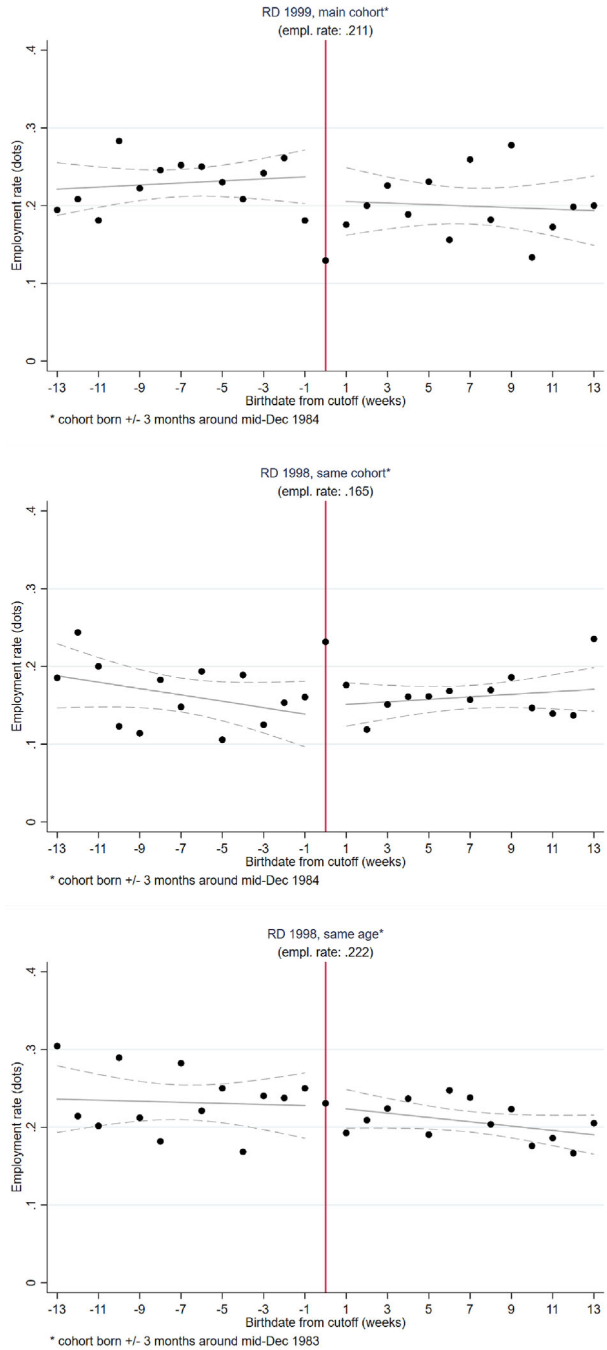
Next, the DDisc approach is directly purged of potential time-invariant heterogeneity in employment around the cutoff. Its graphical representation is given in fig. 6, first with the same-cohort placebo group; this is simply constructed as a differential RD, i.e., the difference in employment rates by birth-date cells (in weeks) between September 1999 and September 1998. The graph confirms the absence of a discontinuous change in child employment rate around the threshold. Note that the differential trend reflects the steeper

23 The authors thank a referee for this suggestion.

24 In September 1999, at the time of observation, children to the right of the cutoff were between 14.5 and 14.75 years old while children to the left were between 14.75 and 15 years old.

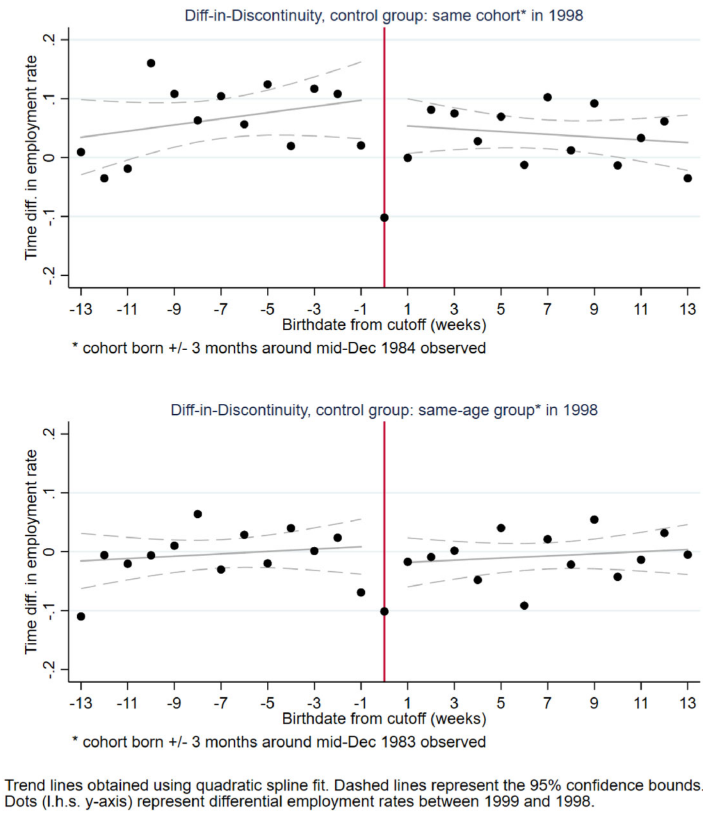
25 For instance, assume that a previous shock had discontinuously increased child employment for the subcohort born after mid-December of 1984. It may have partly offset the potential policy effect of the new law, explaining the above conclusions about the absence of visible enforcement overall.

Figure 5. Child Employment around the Cutoff in September 1999 (Main RD) and 1998 (Placebo RDs)



Source: Authors' analysis based on data from the 1998 and 1999 *Pesquisa Nacional por Amostra de Domicilios*.
Note: This figure displays the graphical regression discontinuity (RD) analysis. Dots represent employment rates by age cell. Trend lines were obtained using a quadratic spline fit. Dashed lines represent the 95 percent confidence bounds. For "RD 1999, main cohort" and "RD 1999, same age," child age is 14.5 at +13 weeks and 15 at −13 weeks. For "RD 1998, same cohort," child age is 13.5 at +13 weeks and 14 at −13 weeks.

Figure 6. Difference in Child Employment between 1999 (Main Cohort) and 1998 (Using the Same Cohort or Same Age Group)



Source: Authors' analysis based on data from the 1998 and 1999 *Pesquisa Nacional por Amostra de Domicilios*.

Note: This figure displays the graphical difference-in-discontinuity analysis. Dots (measured along the vertical axis on the left) represent differential employment rates, by birth-date weeks around the cutoff, between 1999 and 1998. Trend lines were obtained using a quadratic spline fit. Dashed lines represent the 95 percent confidence bounds.

employment-age relationship among older children.[26] Alternatively, the second graph of fig. 6 uses the same-age placebo group for a sensitivity check. The differential employment-age pattern is necessarily different from that in the first graph: the placebo group now has the same age but belongs to a different cohort.[27] Reassuringly, the conclusion is similar: there is no sign of an abrupt drop in employment at the cutoff.

Finally, whatever the placebo group, an effect of the ban may be exaggerated by an increase in child employment among unbanned older children (i.e., to the left of the cutoff); this is expected particularly among poor households that need to compensate for the ban of the younger children (Bharadwaj, Lakdawala, and Li 2019). However, this is unlikely in the present context, as a clear decrease in employment among newly banned children is not observed in the first place.[28]

26 This illustrates the aforementioned issue with the DD method: when using the same-cohort approach, the time difference is necessarily larger to the left of the cutoff, so that DD estimates may wrongly point to a diminishing effect of the ban on child labor.

27 Another difference is that the same-cohort children are banned under the old system whereas the same-age children are not, as discussed above.

28 Moreover, the second graph of fig. 6 conveys that there is no overall difference in employment rates between the main cohort in 1999 and the same-age group in 1998.

Table 1. Effect of the Minimum Working Age Reform on Child Labor: Baseline Specification

Specification (smooth function of birth date)	RD	DDisc (same cohort)
Linear	−0.0154	−0.0127
	(0.0174)	(0.0241)
Quadratic	−0.0154	−0.0127
	(0.0171)	(0.0241)
Cubic	−0.00786	−0.0128
	(0.0197)	(0.0241)
Linear spline	−0.0154	−0.0128
	(0.0187)	(0.0241)
Quadratic spline	−0.0162	−0.0129
	(0.0209)	(0.0242)
Sample size	3,007	5,973

Source: Authors' summary of data from the 1998 and 1999 *Pesquisa Nacional por Amostra de Domicilios* (PNAD).

Note: This table shows estimates of child employment using 1999 PNAD data for regression discontinuity (RD) and using both 1998 and 1999 data for difference-in-discontinuity (DDisc). Baseline estimations use a ±3-month window, smooth functions of the running variable as indicated, a set of controls (household ethnicity; household head's age, gender, and years of schooling; region; rural or not; income; household size), and PNAD sample weights. Robust standard errors, given in parentheses, are clustered at the cohort × month of birth level. None of the estimates in this table are statistically significant at conventional levels.

Baseline Estimations and Sensitivity Checks

The main estimation results are summarized in table 1. To test the null hypothesis of no program effect, a default specification is used that is based on the ±3-month window (as previously justified), additional controls X_i, and the PNAD sampling weights. The first column reports the estimates of β for the RD design, equation (1), carried out on 1999 data. The second column presents estimates from the DDisc approach, equation (2), on pooled data, using the same-cohort comparison group to avoid any bias due to cohort effects. Since results should not depend on the specific functional form of the smooth function $\delta(\cdot)$, in each case a series of estimates based on various specifications (linear, quadratic, cubic, linear spline, and quadratic spline) are presented. Corroborating the graphical evidence, both series of estimations do not support rejecting the null hypothesis of no effect of the ban. Estimates are very similar across specifications and for the RD and DDisc approaches, with a confidence interval always close to $[-0.06,\ 0.03]$.

The non-finding of the default approach in this study was evaluated through a detailed sensitivity analysis, reported in table 2. To check whether the design choices were responsible for producing the non-result, the set of methods was extended (in the columns of the table) to the alternative DDisc approach using the same-age comparison group and to a triple-difference approach, namely the DDDisc method of equation (4). The rows of the table check the potential effects of using a broader time window (±6 months around the cutoff), of not using sampling weights, and of not introducing the controls X_i in the estimation. Examining these variants allows one to assess the confidence in the overall conclusions obtained from local estimations. It turns out that the main finding is unchanged: there is no sign of a discontinuous change in employment among children who turned 14 after the enactment of the law. These results were obtained using a linear spline specification of $\delta(\cdot)$, but unreported estimates with other specifications led to the same conclusions.[29]

29 In the unreported estimations, a sensitivity analysis with respect to the way standard errors are clustered was also conducted. Three clustering levels are considered: (i) cohort × month of birth (baseline); (ii) cohort × day of birth; and (iii) cohort × month of birth × state. The standard errors vary across these different options but do not change the conclusion of an insignificant average effect of the new law.

Table 2. Effect of the Minimum Working Age Reform on Child Labor: Sensitivity Analysis

Variants	Time window	RD	DDisc (same cohort)	DDisc (same age)	DDDisc
Baseline specification	±3 months	−0.0154	−0.0128	0.0223	−0.0187
		(0.0187)	(0.0241)	(0.0144)	(0.0293)
Alternative time window	±6 months	0.0123	−0.00363	0.0236	−0.00887
		(0.0193)	(0.0179)	(0.0140)	(0.0209)
Estimations without sample weights	±3 months	−0.0242	−0.0279	0.00152	−0.0336
		(0.0152)	(0.0206)	(0.0136)	(0.0253)
Estimations without additional controls	±3 months	−0.0168	−0.0141	0.0217	−0.0235
		(0.0233)	(0.0264)	(0.0135)	(0.0326)
Sample size	±3 months	3,007	5,973	5,922	11,785
	±6 months	6,081	12,191	12,262	24,330

Source: Authors' analysis based on data from the 1998 and 1999 *Pesquisa Nacional por Amostra de Domicilios* (PNAD).
Note: This table reports estimates of child employment using regression discontinuity (RD), difference-in-discontinuity (DDisc), and difference-in-difference-in-discontinuity (DDDisc). Estimations use either a ±3-month or a ±6-month window and a linear spline smooth function of the running variable, with or without a set of controls (household ethnicity; household head's age, gender, and years of schooling; region; rural or not; income; household size), and with or without PNAD sample weights. Robust standard errors, given in parentheses, are clustered at the cohort × month of birth level. None of the estimates in this table are statistically significant at conventional levels.

4.2. Power Calculations

A genuine concern pertains to the ability of the estimations to detect a significant effect of the ban on child labor at the discontinuity. Therefore, power calculations were carried out for local estimations; see Cattaneo, Titiunik, and Vazquez-Bare (2019) and Schochet (2008).[30] The precision of estimated treatment effects is also expressed in terms of the minimum detectable effect (MDE), the smallest true effect that has a reasonable chance (usually a power of 80 percent) of producing an estimate that is statistically significant at the $\alpha = 0.05$ level. The results are reported in fig. 7, focusing on DDisc designs using the ±3-month window (and with either same-cohort or same-age children as placebo groups). The power is plotted against levels of potential child labor response. The intersection of the power curve with the horizontal line at 80 percent yields, on the horizontal axis, the corresponding level of MDE that can be attained with the empirical approach.

Power curves show how the power increases with the size of the effect that one is aiming to detect. At the conventional level of 80 percent and for $\alpha = 0.05$, MDEs of 5.3–5.7 percentage points in absolute level are found, equivalent to 25–27 percent relative to the control group outcome in 1998 (i.e., prior to treatment). With $\alpha = 0.10$ (unreported), MDEs of 4.7–5.0 (21–24 percent relative to the control mean outcomes) are obtained. With a ±6-month window (unreported), MDEs of 3.3–3.5 percentage points (15–17 percent) are obtained. Hence, the method should be able to detect moderate or large effects of the new law (if such effects existed).[31] More modest effects could not be detected.[32]

30 Formally, the MDE for an RD design is defined as

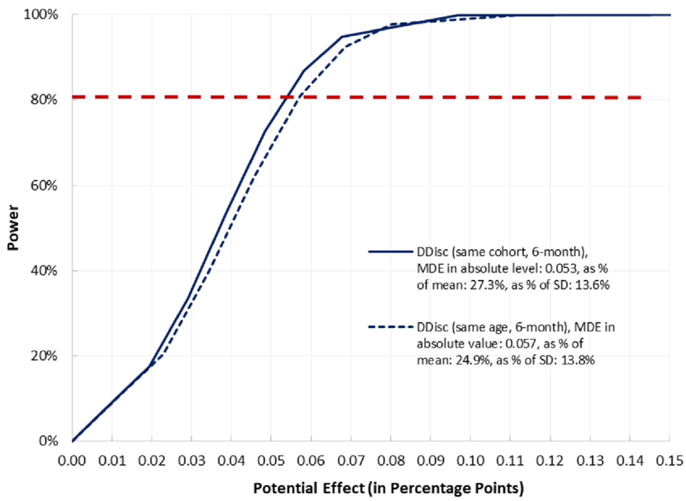
$$\text{MDE} = 2.8 \left(\frac{(1 - R_Y^2)\sigma_Y^2}{NP(1-P)(1-R_T^2)} \right)^{0.5},$$

where R_Y^2 is the coefficient of determination of the statistical model, R_T^2 is the correlation between treatment status and the running variable, N is the sample size, and P is the proportion of treated units.

31 Piza and Souza (2016) found relative estimates ranging between 25 percent and 80 percent of the employment rate of the base period control group, depending on the choice of control group. The MDEs found in the present article are clearly on the lower side of these intervals.

32 Standardized MDEs (SMDEs), which are MDEs expressed as percentages of the standard deviation of the control group outcomes, were also computed, giving a reasonable range of 9.6–13.8 percent.

Figure 7. Child Employment Effect: Power and Minimum Detectable Effect



Source: Authors' analysis based on data from the 1998 and 1999 *Pesquisa Nacional por Amostra de Domicilios*.
Note: This graph represents the power calculated at different potential magnitudes of the effect (child employment level in percentages). The horizontal axis indicates the minimum detectable effect (MDE) in absolute level and the vertical axis indicates the associated level of power. The power curves intersect the horizontal dashed line at the 80 percent (conventional) level. The legend also shows the MDE in absolute level, as a percentage of the control group's mean outcome and as a percentage of the outcome standard deviation (SD), known as standardized MDE. A 5 percent risk of type I error is assumed. The model used is the difference in discontinuity (DDisc) model, with alternative placebo group (same cohort or same age) and a ±3-month window.

To put things in perspective, first recall that labor among children 13–14 years old roughly halved between 1999 and 2008 in Brazil (Rosati et al. 2011). Hence, according to the power analysis above, the method used here should be able to detect whether the minimum age law had been one of the key drivers of this trend. The conclusions of the study go in the opposite direction. Second, one cannot expect a single measure to eradicate child labor completely, and MDE levels should represent reasonable minimal targets. For example, enforcement of labor bans may have more success in inspectable activities and/or in regions with a high intensity of labor inspections; both of these dimensions are explored in the next subsection. Assuming that the law eliminates the type of child work that would be affected the most—as defined by inspectable labor in high-inspection regions—the latter represents 5.3 percent of the control group, a magnitude of change that can actually be detected.[33]

4.3. Heterogeneous Results

Heterogeneity in Outcomes

The results presented earlier tend to indicate that despite the Brazilian context—a strong intention of the government to eradicate child labor, the conjunction of redistributive and legal policies, and a pre-existing and operational system of labor inspections—enforcement of the child labor ban may not have been effective overall. Nonetheless, it is worth exploring whether some heterogeneous cases reveal local impacts of the law.

First, consider various forms of employment. As discussed, it is possible that the nature of a child's activity is more relevant than the traditional divide between formal and informal work, especially as most

33 Note that there is no easy way to increase the sample size and thus obtain more precise estimates. The PNAD is not available for year 2000, and using PNAD data from 2001 does not seem right. In effect, the PNAD is collected in September of each year, so with a ±6-month window (as used in their estimations), the banned cohort would be aged between 16 years and 3 months and 16 years and 9 months in September 2001. In other words, this cohort would become eligible for work. This reasoning applies a fortiori if later years such as 2002 or 2003 are included.

children are unregistered workers. In particular, child activities in workplaces that are deemed more visible or accessible to labor inspectors (i.e., more "inspectable") may induce better compliance with the new law. Table 3 reports RD and DDisc results with a ±3-month window and the usual series of specifications. There is only very mild evidence of an effect of the new ban on these more inspectable activities (whether or not farm work is included, as the inspectability of farm work is ambiguous). Indeed, the effect is significant and negative only with the RD design.

Next, consider the more standard dichotomy between formal and informal work (using the labor card definition). As mentioned before, the new law should automatically apply to registered child labor. However, there is not much to expect given children's very low formal employment rate. Table 3 indeed points to negative estimates of a very small magnitude and hence mostly insignificant. Table 3 also shows that there is no specific effect on paid work. Next, potential effects of the new law on the education of children around age 14 are investigated.[34] There is very limited evidence of a robust increase in school enrollment; the effect is significant only with the RD design and some of the specifications. Finally, since no direct effects of the law on child employment were found, it is a fortiori unlikely that compensatory effects among older siblings or parents would be found (see Bharadwaj, Lakdawala, and Li 2019). Unreported estimations confirm this point.[35]

Geographical Heterogeneity

The final check is whether there is geographical heterogeneity in the effect of the ban. This analysis starts with a standard source of spatial variation: comparing rural and urban households. Urban children represent 81.7 percent of the selected sample. These children might be more likely to be employed in large firms with higher chances of inspection; nonetheless, this characterization seems less precise than the "inspectability" notion used previously. So as not to reduce the sample size too much—especially for rural households—RD and DDisc estimations are simply replicated while interacting the urban and rural dummies with the treatment variable. The results are reported in the upper panel of table 4; no major differences are observed between these groups, with insignificant effects in both cases.

The rest of table 4 focuses on regional inspection intensity. The treatment variable is interacted with a dummy that equals 1 if the child lives in a state with above-median inspection rates. As explained in the data section, local per-firm inspection rates in 2002 provided by Almeida and Carneiro (2009) are used, averaged at the state level. Recall that here the focus is not on the causal effect of inspection intensity per se, but rather on the spatial heterogeneity in potential enforcement conditions of the new law. Interestingly, the various types of local estimations point to a negative and significant effect of the law. These results can be viewed as suggestive evidence of there being a law and there being a discontinuous treatment enforced in regions with higher chances of inspection. The interpretation may pertain to inspections themselves, to

34 The question asked is "Frequenta escola?" (attend a school), but it does not really provide information on whether children do effectively attend school. Rather, it may simply capture enrollment and hence be less relevant. Indeed, attendance is typically lower and more responsive to shocks than is enrollment, which tends to be a longer-term measure. Moreover, schooling rates are high (91 percent in the control group in 1999), so it is unlikely that the reform would increase schooling on this margin.

35 This is not surprising for another reason: if older siblings and mothers are already working prior to the new law taking effect, it is unlikely that the reform will affect their participation. Note that the average labor participation rates of a child's older siblings and mother are 40 percent and 52 percent, respectively, when the child is not working, but are 72 percent and 65 percent when the child is in work. Moreover, no changes were found in child wage rates; a wage drop could explain intra-household substitutions but also a shift from unpaid to paid work among older relatives.

Table 3. Effect of the Minimum Working Age Reform: Alternative Outcomes

	No. observations	Linear	Quadratic	Cubic	Linear spline	Quadratic spline
Inspectable activities (visible and accessible workplaces, including farms); control mean in 1999 = 0.171						
RD	3,007	−0.0217*	−0.0217**	−0.0185*	−0.0217*	−0.0214*
		(0.0117)	(0.0104)	(0.0100)	(0.0118)	(0.0127)
DDisc (same cohort)	5,973	−0.00468	−0.00469	−0.00467	−0.00471	−0.00469
		(0.0154)	(0.0154)	(0.0154)	(0.0154)	(0.0155)
DDisc (same age)	5,922	0.0102	0.0102	0.00998	0.0102	0.0100
		(0.0183)	(0.0182)	(0.0175)	(0.0183)	(0.0176)
Inspectable activities (visible and accessible workplaces, excluding farms); control mean in 1999 = 0.083						
RD	3,007	−0.024***	−0.024***	−0.0113	−0.024**	−0.0136
		(0.00925)	(0.00926)	(0.00793)	(0.00951)	(0.00878)
DDisc (same cohort)	5,973	0.00868	0.00867	0.00876	0.00865	0.00869
		(0.0117)	(0.0117)	(0.0117)	(0.0117)	(0.0117)
DDisc (same age)	5,922	0.00978	0.00979	0.00972	0.00981	0.00975
		(0.0112)	(0.0112)	(0.0110)	(0.0112)	(0.0110)
Formal work (labor card and social security contribution); control mean in 1999 = 0.007						
RD	3,007	−0.00501	−0.00503	−0.0125***	−0.00503	−0.0137***
		(0.00476)	(0.00508)	(0.00300)	(0.00505)	(0.00309)
DDisc (same cohort)	5,973	−0.00370	−0.00369	−0.00376	−0.00368	−0.00374
		(0.00251)	(0.00251)	(0.00248)	(0.00250)	(0.00248)
DDisc (same age)	5,922	−0.000704	−0.000690	−0.000751	−0.000689	−0.000741
		(0.00436)	(0.00412)	(0.00410)	(0.00415)	(0.00404)
Informal work; control mean in 1999 = 0.217						
RD	3,007	−0.0104	−0.0104	0.00464	−0.0104	−0.00251
		(0.0171)	(0.0170)	(0.0193)	(0.0191)	(0.0220)
DDisc (same cohort)	5,973	−0.00902	−0.00905	−0.00906	−0.00908	−0.00913
		(0.0244)	(0.0245)	(0.0245)	(0.0245)	(0.0246)
DDisc (same age)	5,922	0.0230	0.0229	0.0227	0.0230	0.0228
		(0.0146)	(0.0145)	(0.0141)	(0.0152)	(0.0153)
Paid work; control mean in 1999 = 0.107						
RD	3,007	0.0117	0.0118	0.0152	0.0118	0.00729
		(0.0108)	(0.0109)	(0.0167)	(0.0116)	(0.0152)
DDisc (same cohort)	5,973	0.00217	0.00216	0.00210	0.00215	0.00202
		(0.0154)	(0.0154)	(0.0153)	(0.0154)	(0.0154)
DDisc (same age)	5,922	0.00699	0.00700	0.00681	0.00702	0.00686
		(0.0111)	(0.0111)	(0.0104)	(0.0112)	(0.00974)
School enrollment; control mean in 1999 = 0.913						
RD	3,007	0.0273*	0.0274*	0.0228	0.0273*	0.0174
		(0.0161)	(0.0151)	(0.0206)	(0.0154)	(0.0193)
DDisc (same cohort)	5,973	0.0166	0.0166	0.0167	0.0165	0.0167
		(0.0115)	(0.0115)	(0.0116)	(0.0115)	(0.0116)
DDisc (same age)	5,922	0.00961	0.00965	0.00975	0.00964	0.00962
		(0.0101)	(0.0103)	(0.0105)	(0.0101)	(0.0108)

Source: Data for the estimations come from the 1998 and 1999 *Pesquisa Nacional por Amostra de Domicilios*.

Note: This table reports estimates of employment (inspectable, formal, informal, or paid) and school enrollment using regression discontinuity (RD) and difference-in-discontinuity (DDisc), both with a ±3-month window, alternative smooth functions of the running variable (linear, quadratic, cubic, linear spline, and quadratic spline), and sample weights. Additional controls include household ethnicity; household head's age, gender, and years of schooling; region; rural or not; income; and household size. Robust standard errors, given in parentheses, are clustered at the cohort × month of birth level. * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 4. Effect of the Minimum Working Age Reform: Geographic Heterogeneity

	No. observations	Linear	Quadratic	Cubic	Linear spline	Quadratic spline
Urban households						
RD	3007	−0.0148	−0.0148	−0.00735	−0.0148	−0.0154
		(0.0155)	(0.0151)	(0.0151)	(0.0170)	(0.0194)
DDisc (same cohort)	5973	−0.00426	−0.00438	−0.00441	−0.00440	−0.00441
		(0.0222)	(0.0222)	(0.0222)	(0.0223)	(0.0222)
DDisc (same age)	5922	0.0217	0.0217	0.0216	0.0217	0.0222
		(0.0162)	(0.0162)	(0.0150)	(0.0164)	(0.0152)
Rural households						
RD	3007	−0.0172	−0.0172	−0.00949	−0.0171	−0.0191
		(0.0353)	(0.0350)	(0.0409)	(0.0358)	(0.0384)
DDisc (same cohort)	5973	−0.0413	−0.0410	−0.0412	−0.0410	−0.0414
		(0.0337)	(0.0338)	(0.0340)	(0.0338)	(0.0343)
DDisc (same age)	5922	0.0244	0.0244	0.0233	0.0243	0.0220
		(0.0193)	(0.0193)	(0.0214)	(0.0200)	(0.0239)
States with above-median inspection rates (2002 measure)						
RD	3007	−0.0419**	−0.0419**	−0.0350*	−0.0420*	−0.0444**
		(0.0210)	(0.0209)	(0.0195)	(0.0226)	(0.0179)
DDisc (same cohort)	5973	−0.0404**	−0.0405**	−0.0406**	−0.0405**	−0.0406**
		(0.0194)	(0.0194)	(0.0193)	(0.0194)	(0.0193)
DDisc (same age)	5922	−0.0288*	−0.0288*	−0.0295**	−0.0289*	−0.0298**
		(0.0164)	(0.0167)	(0.0149)	(0.0176)	(0.0140)
States with above-median inspection rates (2002 measure, no additional control in local estimations)						
RD	3007	−0.0423	−0.0423	−0.0376	−0.0424	−0.0497*
		(0.0280)	(0.0274)	(0.0291)	(0.0291)	(0.0286)
DDisc (same cohort)	5973	−0.0541**	−0.0542**	−0.0543**	−0.0542**	−0.0543**
		(0.0229)	(0.0229)	(0.0228)	(0.0230)	(0.0229)
DDisc (same age)	5922	−0.0357**	−0.0357**	−0.0363***	−0.0357**	−0.0366***
		(0.0177)	(0.0166)	(0.0136)	(0.0175)	(0.0115)
States with above-median inspection rates (1999-predicted measure)						
RD	3007	−0.0412*	−0.0412*	−0.0339	−0.0413	−0.0433**
		(0.0249)	(0.0247)	(0.0221)	(0.0263)	(0.0200)
DDisc (same cohort)	5973	−0.0967***	−0.0968***	−0.0969***	−0.0969***	−0.0972***
		(0.0251)	(0.0251)	(0.0251)	(0.0252)	(0.0252)
DDisc (same age)	5922	−0.0364**	−0.0364**	−0.037***	−0.0364**	−0.0376***
		(0.0175)	(0.0166)	(0.0142)	(0.0173)	(0.0143)

Source: Data for the estimations come from the 1998 and 1999 *Pesquisa Nacional por Amostra de Domicilios*.
Note: This table reports estimates of employment (inspectable, formal, informal, or paid) and school enrollment using regression discontinuity (RD) and difference-in-discontinuity (DDisc), both with a ±3-month window, alternative smooth functions of the running variable (linear, quadratic, cubic, linear spline, and quadratic spline), and sample weights. Additional controls include household ethnicity; household head's age, gender, and years of schooling; region; rural or not; income; and household size. Treatment effect is interacted with geographical heterogeneity (states with above-median inspection rate, rural versus urban households). Robust standard errors, given in parentheses, are clustered at the cohort × month of birth level. * $p < .1$, ** $p < .05$, *** $p < .01$.

a higher awareness of the risks faced by firms and families in these regions, or to a faster diffusion of new norms regarding child labor in these places.[36]

Two sensitivity checks are suggested for this result. First, it is difficult to give a graphical representation of the result as it involves interacted terms (an alternative approach might consist in splitting the sample

36 Admittedly, PNAD data could be contaminated by the underreporting of child work among newly banned children, for instance due to social desirability bias. If such bias is especially prevalent in high-inspection states, this, rather than genuine enforcement of the law in these regions, could explain the effect found.

into states of low and high inspection intensities, but this would reduce the sample size too much). Hence, to get closer to a visual characterization, relying only on local employment trends around the cutoff, the estimations are replicated using functions of τ_i only, i.e., dropping the set of additional controls. The second sensitivity check uses the proxy for the state-level 1999 intensity rate; as explained in the data section, this more contemporaneous measure relies on an estimate of the inspection function and on the 1999 regional count of inspectors. As can be seen in table 4, both variants tend to confirm a diminishing effect of the law in high-inspection states. This is again suggestive evidence, as several estimates are not significant in the case of the RD design (notice, however, that the sign and magnitude of the effect are very similar across the three RD variants).

5. Conclusion

This article studies the change in the minimum working age in Brazil, which was raised from 14 to 16 years old in December 1998, and its effect on child labor, by exploiting the heterogeneous eligibility among children who turned 14 around the reform date. Local identification using RD and DDisc designs avoids the usual difficulty of age trend methods such as DD with broad comparison groups on each side of the cutoff. The null hypothesis that there is no overall effect of the ban on child employment cannot be rejected, a result that tends to confirm previous evidence on the relatively weak effectiveness of minimum age regulation (Edmonds 2014). It is not explained by a lack of statistical power overall. Also, no marked effect was observed when focusing on visible/accessible workplaces. However, a significant and reducing effect of the new ban on child work was found in high-inspection regions, which can be interpreted as suggestive evidence of there being a law and a discontinuous treatment.

Further research could proceed in several directions. First, the result of this study is disappointing in the Brazilian context, where enactment of the minimum working age law seemed to be a genuine effort and was accompanied by an operational system of labor inspections. However, this study has investigated only the short-term impacts of the law, while other approaches would be required to detect longer-run effects. The literature indeed points to legal bans helping to establish new societal norms (Rogers and Swinnerton 2008). Future work could also address the potential impact of legal bans on the worst forms of child labor; this question was not explored in the present article as policy measures against hazardous work do not pertain to the age-14 discontinuity exploited in this study.[37]

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37 Given the small share of children that this type of work concerns, a very large amount of data would be needed to identify potential shifts of children out of hazardous work sectors.

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Supplementary Online Appendix

Minimum Age Regulation and Child Labor: New Evidence from Brazil

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Supplementary Online Appendix: Additional Results

S1. Sample and Timing Description

Figure S1.1 describes the main cohort (observed in September 1999) and the same-cohort or same-age placebo groups (observed in September 1998). The upper panel describes the sample with a ±3-month window and the lower panel the sample with a ±6-month window. With the ±6-month window, the placebo group based on the same-cohort children is a mix of banned and unbanned children under the old system. Children born between June 15 and September 15, 1984, had already turned 14 and hence were eligible for work under the old regime. This is problematic, especially for DD approaches that do not rely on local identification, as in Piza and Souza (2016). This problem does not arise with the ±3-month window used in the main analysis of this article. With the shorter window, the DDisc consists of time differencing the half-cohorts of interest with similar half-cohorts observed three months before the reform (September 1998), which were then either fully banned (same cohort) or unbanned (same age). The ±6-month window is used for sensitivity checks.

Figure S1.1. Summary Information on Treatment and Control Groups

+/- 3 month window around cutoff									
PNAD 1998: same cohort, one year younger (n=2,971)			PNAD 1998: same age, previous cohort (n=2,922)			PNAD 1999 (n=3,007)			
Date of birth	age at data collection*	allowed to work (old ban)	Date of birth	age at data collection*	allowed to work (old ban)	Date of birth	age at data collection*	age on reform date	allowed to work (new ban)
Dec 15 1984 to March 15 1985	13.5-13.75	no	Dec 15 1983 to March 15 1984	14.5-14.75	yes	Dec 15 1984 to March 15 1985	14.5-14.75	13.75-14	no
Sept 15 1984 to Dec 15 1984	13.75-14	no	Sept 15 1983 to Dec 15 1983	14.75-15	yes	Sept 15 1984 to Dec 15 1984	14.75-15	14-14.25	yes

+/- 6 month window around cutoff									
PNAD 1998: same cohort, one year younger (n=6,112)			PNAD 1998: same age, previous cohort (n=6,082)			PNAD 1999 (n=6,182)			
Date of birth	age at data collection*	allowed to work (old ban)	Date of birth	age at data collection*	allowed to work (old ban)	Date of birth	age at data collection*	age on reform date	allowed to work (new ban)
Dec 15 1984 to March 15 1985	13.5-13.75	no	Dec 15 1983 to March 15 1984	14.5-14.75	yes	Dec 15 1984 to March 15 1985	14.5-14.75	13.75-14	no
March 15 1985 to June 15 1985	13.25-13.5	no	March 15 1984 to June 15 1984	14.25-14.5	yes	March 15 1985 to June 15 1985	14.25-14.5	13.5-13.75	no
June 15 1984 to Sept 15 1984	14-14.25	yes	June 15 1983 to Sept 15 1983	15-15.25	yes	June 15 1984 to Sept 15 1984	15-15.25	14.25-14.5	yes
Sept 15 1984 to Dec 15 1984	13.75-14	no	Sept 15 1983 to Dec 15 1983	14.75-15	yes	Sept 15 1984 to Dec 15 1984	14.75-15	14-14.25	yes

Source: Authors' calculations based on data from the 1998 and 1999 *Pesquisa Nacional por Amostra de Domicilios* (PNAD).
Note: This figure summarizes the structure of the PNAD regarding which type of children were or were not subject to the age ban (old or new).

S2. Comparing Subcohorts around the Cutoff

Table S2.1 shows descriptive statistics for the 1999 selection with either the ±3-month window or the ±6-month window around the cutoff. The main observation is that characteristics of the subcohorts on either side of the birth-date threshold are rarely statistically different. Whatever the time window, an *F*-test cannot reject the null that the two subcohorts have the same characteristics. Yet, as expected, the ±6-month window shows more differences between the two subgroups.

Table S2.1. Descriptive Statistics around the Cutoff (Year 1999)

	±3-month window			±6-month window		
	Birth < mid-Dec	Birth > mid-Dec	Difference	Birth < mid-Dec	Birth > mid-Dec	Difference
Region Norte	0.09	0.07	−0.02**	0.09	0.07	−0.02***
Region Nordeste	0.32	0.35	0.03*	0.33	0.36	0.03**
Region Sudeste	0.33	0.32	−0.01	0.33	0.32	−0.01
Region Sul	0.16	0.17	0.01	0.16	0.16	0.00
Rural	0.21	0.21	0.00	0.21	0.21	0.00
Metropolitan area	0.37	0.39	0.02	0.36	0.38	0.02
Ethnicity (branca)	0.49	0.52	0.03	0.48	0.48	0.00
Ethnicity (parda)	0.46	0.44	−0.02	0.46	0.48	0.02
Head: education	5.30	5.41	0.11	5.16	5.31	0.15
Head: mother	0.22	0.20	−0.02	0.22	0.20	−0.02*
Head: age	43.69	43.59	−0.1	43.70	43.54	−0.16
Assets	5.01	5.06	0.05	4.96	5.02	0.06
Inspection intensity	4.53	4.43	−0.1	4.42	4.34	−0.08
Homeowner	0.75	0.73	−0.02	0.74	0.73	−0.01
Household size	5.45	5.37	−0.08	5.44	5.35	−0.09*

Source: Authors' analysis based on data from the 1998 and 1999 *Pesquisa Nacional por Amostra de Domicilios* (PNAD).
Note: This table shows balance checks using PNAD data around the age cutoff. *, **, and *** denote significant difference in mean characteristics at the 10%, 5%, and 1% levels, respectively.