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IZA DP No. 15144

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ISSN: 2365-9793

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ABSTRACT

Child Labor Bans, Employment, and School Attendance: Evidence from Changes in the Minimum Working Age*

This paper investigates the effect of a unique child labor ban regulation on employment and school enrollment. The ban implemented in Mexico in 2015, increased the minimum working age from 14 to 15, introduced restrictions to employ underage individuals, and imposed penalties for the violation of the law. Our identification strategy relies on a DiD approach that exploits the date of birth as a natural cutoff to assign individuals into treatment and control groups. The ban led to a decrease in the probability to work by 1.2 percentage points and an increase in the probability of being enrolled in school by 2.2 percentage points for the treatment group. These results are driven by a reduction in employment in paid activities, and in the secondary and tertiary sectors. The effects are persistent several years after the ban.

JEL Classification: I38, J22, J23, J82, O12

Keywords: child labor, ban, minimum working age, schooling

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* We thank Thomas K. Bauer, Ronald Bachmann, Christian Bredemeier, Julia Bredtmann, Caio Piza, Kerstin Schneider, Franz Westermaier, and David Zuchowski for their comments. Rachel Kuhn provided excellent research assistance. We also thank the participants of the internal research seminars at the University of Wuppertal, and the participants of the Annual Conference of the European Society for Population for their constructive comments. All remaining errors are our own.

1 Introduction

Important reductions in child labor rates have been made worldwide with more than 134 million children who stopped working from 2010 to 2016. Yet, in developing countries one out of every four children aged 5 to 17 is engaged in activities that are hazardous and risky, affect their development, or do not comply with the international minimum working age standards (UNICEF, 2019).[1] One of the main international initiatives to eradicate child labor are employment bans, through the implementation of a minimum working age and the prohibition to hire underage individuals in certain sectors (ILO, 2017). For instance, the ILO Convention No. 138 introduced in 1973 recommends a minimum age of 15 years to enter the labor force (ILO, 2018) and has been ratified by 131 developing countries.[2]

Despite the high number of countries that have ratified this convention, little is known about the effectiveness of these bans. The few studies analyzing this relationship find contradicting results (see e.g., Piza and Souza, 2017; Bharadwaj *et al.*, 2020; Bargain and Boutin, 2021). This paper evaluates the impact of a complex child labor ban, which was introduced in Mexico, on school enrollment and a number of child labor indicators. The main results in this paper present new evidence on child labor bans and offer possible explanations for previous diverging findings.

Mexico presents a unique setting to analyze the impact of child labor bans. In 2015, Mexico introduced an ambitious reform to the National Labor Law *"Ley Nacional de Trabajo"*. To date, this reform is one of the most extensive initiatives in Latin America to eradicate child labor. The reform not only shifted the working age from 14 to 15 years, but it was also coupled with i) restrictions to hire underage individuals who had not completed basic education, ii) regulations to hire individuals above 15 but younger

[1] In developing countries more than 152 million children continue to be engaged in child labor (ILO, 2017). The number of children working increased to 160 million in 2021 for the first time in two decades due to the COVID-19 pandemic. This definition of child labor excludes light work that does not interfere with schooling activities.

[2] 79 out of 131 developing countries have banned children younger than 15 years from the labor market. 52 out of the 131 developing countries have set the minimum working age at 14 years.

than 18 with respect to the type of activities, hours worked, and working schedule, and iii) concrete penalties for employers that violate the regulations.[3]

The theoretical literature suggests that child labor bans have an ambiguous impact on both schooling and child labor. On the one hand, the minimum working age could lead to a Pareto improvement for the household due to the endogenous change in adult wages induced by the ban (Baland and Robinson, 2000). On the other hand, policies aimed at decreasing child labor, may actually help perpetrate the problem given this shift in relative wages, in particular if children compete in the labor market with unskilled adults (Doepke and Zilibotti, 2009). In addition, in very poor regions such bans may not be effective because children need to work in order to avoid hunger or if the household depends on the child's income (Basu, 1999).[4] Several studies show that the main reason for parents to rely on child labor is poverty, which leads parents to give priority to current consumption and thus trade-off between child labor and schooling, i.e., future earnings (Basu and Van, 1998; Baland and Robinson, 2000; Ranjan, 2001; Cigno *et al.*, 2002; Jafarey and Lahiri, 2002; Horowitz and Wang, 2004; Edmonds, 2007).

The empirical evidence on child labor bans in developing countries also shows this contradicting pattern.[5] Piza and Souza (2017) evaluate the impact of a shift in the minimum working age from 14 to 16 in Brazil. The study finds that in the short-run, boys decrease their labor force participation, mainly in the informal sector, while girls do not respond to the ban. In the long-run, the authors find no impact on earnings and work, but they find that the affected cohort is less likely to have a formal occupation. In contrast

[3]Employers who violate this constitutional reform shall be punished with a prison term from 1 to 4 years and a fine of 250 to 5000 times the general minimum wage (DOF, 2015).

[4]For an extensive literature on how a child labor ban can be harmful if poor households depend on children's income, see e.g., Baland and Robinson (2000); Horowitz and Wang (2004); Basu and Zarghamee (2009); Doepke and Zilibotti (2009).

[5]Other studies have analyzed the impact of the minimum working age on child labor for developed countries using historical data. Moehling (1999) finds that the minimum working age laws in the U.S. had a very small effect on the occupational choice of children and only explained partially the decline in the child labor rate between 1880 and 1930. Manacorda (2006) exploits the variation in child labor laws across U.S. 16 states in 1920 and finds the minimum working age decreased the labor force participation of younger siblings and increased labor force participation of older siblings. Finally, Del Rey *et al.* (2018) analyze the effect of minimum working age laws in Spain focusing on the long-run impact. The laws lead to an increase in educational attainment and improved labor market outcomes.

for the same ban in Brazil, Bargain and Boutin (2021) find in the short run no overall significant impacts for the same ban.[6] Only when the authors take into account different heterogeneity analysis of the enforcement of the law, they find suggestive evidence that child labor decreases in areas where labor inspections were high. Similarly, Edmonds and Shrestha (2012) find no influence of the minimum working age on child time allocation using micro-data from 59 low-income countries. Finally, Bharadwaj *et al.* (2020) analyze a landmark legislation against child labor in India. In contrast to the previous findings, the study shows that the ban led to an increase in child labor, due to the decrease in child wages relative to adult wages. The ban shifted child labor from banned sectors to other sectors and shifted work from younger to older siblings.[7]

We contribute to the literature on child labor bans in developing countries in three different ways. First, this study reconciles previous opposing results on the impact of child labor bans. For instance, the increase in child labor in India could be explained by the law restricting child work only in some sectors (Bharadwaj *et al.*, 2020), which led to a shift in activities to other sectors and age groups. In the case of Brazil, the estimates reflect a small decrease in child labor (Piza and Souza, 2016, 2017), or are not statistically significant (Bargain and Boutin, 2021). Yet, the studies rely on a very small sample size which could lead to under-powered estimates.

We add to these studies by focusing on a more complex reform that increases the minimum working age in all sectors and limits outside options for the affected cohort. Specifically, we highlight the role of penalties, regulation of underage work, and requirements to complete basic education to access the labor market. To do so, we examine

[6] Bargain and Boutin (2021) rely not only on a regression discontinuity design, but also use difference-in-discontinuity designs to identify the effect. They also provide detailed heterogeneity analysis showing a decreasing trend in child labor in areas where inspections were high. Where as, Piza and Souza (2017) use a fuzzy regression discontinuity design to estimate the impact of child labor. Moreover, Bargain and Boutin (2021) analyze the effect for a shorter period of time (1998 and 1999) compared to Piza and Souza (2017) that analyze both the short-run (1999-2001) and the long-run (2007-2014).

[7] Other studies, for example, have evaluated the impact of compulsory schooling laws on schooling, or the impact of child labor laws on schooling (see e.g. Landes and Solmon, 1972; Edwards, 1978; Angrist and Keueger, 1991; Margo and Finegan, 1996; Moehling, 1999; Acemoglu and Angrist, 2000; Lleras-Muney, 2002; Oreopoulos, 2007; Gathmann *et al.*, 2015).

a Constitutional Amendment in 2014 that announced the shift in the working age, but established no concrete penalties or further regulation. We then compare the results to the more complex package introduced by the reform to the Labor Law in 2015. Moreover, our sample is much larger than in previous studies, overcoming the issue of under-powered estimates.

Second, in our baseline specification, we implement a DiD focusing on the affected cohorts and estimate both short-run and long-run impact of the ban using cross-sectional variation. For the short-run, however, we can further test the robustness of our results taking into account individual fixed effects as the database collects individual panel data for five quarters. This is an important addition to the literature, as no previous study on child labor bans has accounted for time invariant unobserved characteristics at the individual level. For the long-run, we focus on individuals in the affected cohort shortly after reaching legal adulthood (at age 18). We deviate from (Piza and Souza, 2016), by estimating the ban's impact year-by-year on the affected cohort relative to the control group. This allows us to rule out the existence of pre-trends, to evaluate when the impact of the reform kicks-in, and to analyze if the effects are persistent once the individual is eligible to work.

Third, our database allows us to identify a rich set of child labor indicators that goes beyond what previous studies have analyzed, i.e., we focus not only on the probability to work, (in-)formal work, (un-)paid work, and school enrollment, but also on weekly hours worked, part-time and full-time work, sector of employment, wages, access to a contract, and employment benefits.

To estimate the impact of the ban, this paper uses data from the Mexican Labor Force Survey (ENOE) for the years 2012 to 2019 collected on a quarterly basis, which contains rich information on schooling and employment. The database is a rotating panel that collects household information for five quarters. Our empirical strategy, exploits the shift in the minimum working age as a natural experiment. To identify the effect, we focus on the cohort that was directly affected by the ban. We implement a DiD design that exploits the date of birth as a natural cutoff to define assignment into treatment and control groups.[8]

Our within-birth-cohort approach assigns individuals born in the second half of 2000 to the treatment group. These individuals are 14 years old when the law is enacted, and therefore banned from the labor force. Individuals born in the first half of 2000 are assigned to the control group, as they are 15 years old when the law is enacted. To test the robustness of our results we conduct several placebo tests and across-cohort comparisons. We start by evaluating the short-run impact of the ban. As a second step, we extend the analysis to evaluate the impact in the long-run, i.e., shortly after the individuals in the affected cohort reach adulthood, and estimate the treatment effects on a yearly basis.

Our findings show that a simple increase in the minimum working age leads to significant decreases in child labor, only when this shift is coupled with additional regulation and concrete penalties. The Constitutional Amendment in 2014 –that increased the working age from 14 to 15– had no significant impact on child labor, but a small positive impact on schooling. In contrast, the reform to the Labor Law in 2015 which coupled the ban with concrete regulation of underage work and penalties for potential employers, increased school enrollment by 2.2 percentage points and decreased the probability to work for the treatment group by 1.2 percentage points relative to the comparison group. This represents a decrease in the child labor rate by 16%.

The media coverage of these reforms could partially explain these results. For the Constitutional Amendment in 2014, the media made a strong emphasis on schooling: *"...kids and teenagers should remain in school, to improve their quality of life, (...) and increase their likelihood of having a better job and higher wages..."* (Senado de la República, 2014).

[8]While some studies such as Piza and Souza (2017) and Bargain and Boutin (2021) exploit the discontinuity of the date of birth, we refrain from using an RDD approach. The shift in the minimum working age implies that important variation comes from observations that are further away from the cutoff. For instance, individuals born closer to the cutoff would only need to wait for some weeks to qualify to work, which could only slightly delay their entrance to the labor market. In contrast, individuals who are born further away from the cutoff would have to wait longer in order to qualify for work.

In contrast, the media coverage of the Labor Law reform in 2015, highlighted the restrictions and penalties imposed for potential violations to the law. *"...failure to comply with obligations regarding minors is punishable by imprisonment for one to four years and a fine of 250 (17,525.00 MXN) to 5000 (350,500.00 MXN) times the general minimum wage."* (Martínez, 2015).

We further show that the reduction in child labor after the Labor Law Reform in 2015 is mainly driven by children who decreased their participation in paid activities: mainly in the manufacturing and services sectors. Unlike Piza and Souza (2016), our results indicate that girls decrease their labor force participation to a larger extent than boys because girls are more likely to work in the secondary and tertiary sectors.[9] Consistently, we find that most of the reduction in child labor rates is concentrated for children living in urban regions and household with low income levels.[10] We also show that the effect of the ban is not simply shifted to older siblings. Finally, we show that the effect persists after the affected cohort has reached legal adulthood, i.e., at age 18 individuals banned from the labor force are less likely to be employed full-time or to be employed and out of school.

This paper is structured as follows: The following section presents the background and provides additional information on the reform. Sections 3 and 4 present our empirical strategy and data, Section 5 results, and Section 6 concludes.

2 Background

2.1 Constitutional Amendment and Labor Law Reform

Before 2015, Mexico was one of the last countries in Latin America that had not ratified the ILO Convention No. 138. The convention establishes a "Minimum Age for Admission

[9]In 2013 (pre-ban), the total number of children and adolescents between 5 and 17 years of age engaged in economic activities was 2.5 million, 67.4% were male and 32.6% were female MTI (2013).

[10]23% of children coming from high income level were working before the ban vs. 77% of the children that come from low and extreme poor income levels (Table A6).

to Employment" requiring countries to set the minimum age at 15 for entry into the labor force which is in accordance with the age at which a child leaves compulsory schooling, and to create national policies to eradicate child labor.[11]

In order to ratify the convention, two main steps were implemented: First, Mexico amended the Article 123 of its Federal Constitution on **June 17, 2014** shifting the minimum working age from 14 to 15. In this phase, no additional regulation was introduced except for the change in the minimum working age (DOF, 2014).

Second, on **June 12, 2015**, Mexico reformed its Federal Labor Law ("Ley Federal del Trabajo") accordingly (DOF, 2015). The reform to the Labor Law not only shifted the minimum working age from 14 to 15, but also implemented a set of rules for employers hiring individuals aged 15 to 17, and set minimum education requirements for minors to join the labor force. The main changes can be summarized as follows:

- The Labor Law prohibits all children younger than 15 to work.
- The working day of minors under age 16 may not exceed six hours a day and must be divided into periods of no more than three hours. Between the different periods of the working day, they shall have breaks of at least one hour.
- Individuals under age 18 shall receive an annual paid vacation period of at least eighteen working days.
- It is forbidden to rely on the work of minors for extra-hours, work on Sundays or on official holidays.
- Individuals who are under age 18 and did not complete compulsory schooling are prohibited from working, unless approved by the corresponding labor authority.
- All types of work that are hazardous, risky, or morally damaging are prohibited for individuals who are under age 18.[12]

[11] By the end of 2018, in Latin America and the Caribbean, 32 out of 33 countries have ratified the ILO Convention C138; 14 of them have set the minimum working age at 14 and the rest at 15 and 16 years (ILO, 2018). For an extensive overview about the ratification of the ILO Convention C138 see ILO (2018).

[12] Please refer to the "Ley Federal del Trabajo", Article 176, for a full list of activities that are prohibited for underage individuals.

– All jobs for individuals under the age of 18 shall not interfere with education, leisure and recreation, and should not imply any risks for health and morality.

In addition, the reform to the the Labor Law in 2015 establishes concrete penalties for employers hiring individuals under 15. If the labor authorities identify violations to this regulation, the work of the underage individual shall be immediately terminated and the employers shall be punished with a prison term of 1 to 4 years and/or a fine of 250 to 5000 times the minimum wage (DOF, 2015). The same penalty can be applied for parents (mothers, fathers, or guardians) that allow the employment of children in work that affects their physical, mental, or emotional development, i.e., hazardous work.

Therefore, after the reform to the Labor Law, the Secretary of Labor and Social Security (STPS) started carrying out child labor inspections mainly in industries. For the period between June 1, 2015 and June 20, 2017, the General Directorate of the Federal Labor Inspectorate (DGIFT) conducted 245,019 inspections that cover 9,982,393 workers (ILO, 2019).[13] In 7,748 of the cases children under the age of 15 were engaged in child labor and were immediately detached from the working environment.[14]

2.2 Child Labor Regulation and Statistics

In Mexico, the first child labor regulation was set in 1962, establishing a minimum working age of 14 years through a Constitutional Amendment (article 123). This regulation remained unchanged until the year 2000 when Mexico banned all individuals under the age of 18 from working in dangerous jobs that threaten the health, safety or morality of the individual. This regulation was implemented to comply with the international standards of the ILO Convention No. 182, which calls for the prohibition and elimination of the worst forms of child labor (ILO, 2020).

[13] Inspections are of two types. First, ordinary inspections that are made on a yearly basis to confirm that the companies comply with the specific labor responsibilities. Second, extraordinary inspections that can be made at any time to make sure that the employees abide to the law (ACC, 2015).

[14] Unfortunately, we were not able to obtain high-quality data on inspections, however, we provide the results looking e.g., only at urban areas where inspections are more likely to occur.

Before the Constitutional Amendment in 2014, most of the initiatives to eradicate child labor operated indirectly through initiatives aimed at increasing school enrollment. Public policy targeting child labor indirectly includes, for example, *PROGRESA* which was launched in 1997. This program provides families with additional income conditional on children being enrolled in school, regular school attendance, and regular health check ups. *PROGRESA* led to a substantial increase in school enrollment rates and to a modest reduction in child labor (Skoufias *et al.*, 2001). Other programs have also been introduced to keep children enrolled in school, such as school feeding programs, e.g., school breakfast programs, and initiatives targeted at improving education quality, e.g., the extension of the school day through full-time schools.

In recent years, initiatives that directly target child labor have gained importance given that Mexico, similar to other countries in Latin America, has achieved the goal of universal primary coverage and has shown important increases in secondary enrollment rates. From 1990 to 2015, school enrollment increased from 89% to 98% for children aged 6 to 11, from 79% to 93% for children aged 12 to 14, and from 47% to 73% for individuals aged 15 to 17 (INEE, 2018). Therefore, the opportunities of decreasing child labor through increasing school enrollment are limited.

From 2007 to 2017, Mexico witnessed a decrease in dangerous employment from 6.9% to 3.6% for children aged 5 to 14 years old and from 26.6% to 18.2% for children aged 15 to 17 years old (INEGI, 2018). From 2015 to 2019, the child labor rate decreased from 9.8% to 7.1%. Although the reduction is considerable, 2 million children continue to be engaged in work.[15] For children aged 5 to 15, who are banned from the labor force, the child labor rate decreased from 6.9% to 4.1% from 2007 to 2019.

[15] The number increases to 3.3 million children if heavy domestic work is considered.

3 Identification Strategy

To analyze the effect of the reform to the Labor Law introduced in June 2015, we estimate a DiD model exploiting the date of birth as a natural cut-off to define treatment and control groups. In this setup, we observe individuals who were born in the same cohort and assign them to treatment and control groups according to their month of birth. To do so, we focus on the cohort of children who were born in the year 2000.

Treatment group: children born between June 13th and December 31st. These children were 14 at the time the reform was implemented.

Control group: children born between January 1st and June 12th. These children were 15 by the time the reform was introduced and thus, were excluded from the ban.

To analyze the short-run effect of the ban, we focus on the period 2013 to 2017, i.e., two years before the ban and two years after the ban was introduced, exploiting cross-sectional variation. This date restriction implies that all individuals in our sample are under the age of 18 and thus, not legal adults. We focus on this time span, to have a consistent pre- and post treatment time frame and control for potential seasonality effects. As a robustness test, we also estimate the immediate effect of the reform by focusing on the months before and after the reform for the year 2015 when the reform was announced.

To analyze the long-run effect of the ban, we further extend our analysis from the year 2012 to 2019, i.e., when all individuals in the treatment and control groups have reached legal adulthood. We estimate the following model for the within-cohort approach:

$$Y_{imt} = \alpha_0 + \beta(Treated_i \times Post{-}ban_t) + \theta'\mathbf{X}_i + \mu'\mathbf{P}_i + \delta_m + \gamma_s + \alpha_t + \mathbf{t}\lambda_s + \epsilon_{imt} \quad (1)$$

where Y_{imt}, denotes either child labor or school enrollment for child i, born in month m, at survey time t. For the child labor indicators, we explore (1) the total number of hours worked per week, (2) a binary variable indicating whether the child works (extensive margin), and (3) the number of hours worked conditional on working (intensive margin). We further distinguish between formal and informal work, paid and unpaid work, and type of employment sector. Moreover, conditional on being employed we analyze the effect on full-time employment, wages, contracts, and benefits received.

$Treated_i$ is a dummy variable that takes the value one for children in the treatment group and zero for the control group. $Post-ban_t$ is a dummy variable that takes the value one after June 2015, when the ban was introduced. β is the coefficient of interest which captures the differential change in schooling and child labor after the law enactment for individuals below the legal working age vs. those just above the legal working age.

X_i is a vector of child characteristics that are likely to affect schooling and child labor including age, household size, gender, and birth order to control for a higher probability of working for older siblings. P_i is a categorical variable controlling for parental education level. Parental education controls capture the preference to send children to school and/or work and are a proxy of household income. Furthermore, because work inspections are more likely to take place in urban areas, we include dummies to control for locality size. Localities are smaller geographical units than municipalities and capture the level of urbanization (high, middle, low, or rural) in the locality the child resides.

We include birth-month fixed effects δ_m to take into account confounding seasonal factors of being born at different times of the year as well as age differences in our within-cohort approach. We also include state fixed effects γ_s to take into account state-specific shocks and to capture the regional clustering of industries or sectors that are more prone to hire individuals under 18. γ_t represents quarter-by-year fixed effects as the database used in this analysis is collected on a quarterly bases. The time fixed effects would capture, for instance, employment or economic shocks that could influence both schooling and child labor. $\mathbf{t}\lambda_s$ takes into account a state-specific linear time trend which captures diverging pre-existing trends in outcomes at the state level or in the intensity of inspections. Finally, ϵ_{imt} is the error term. Standard errors are clustered at the birth-month by survey-year level.[16]

Using the same approach, we conduct the analysis using as the main policy change the Constitutional Amendment in 2014. In this case, the affected cohort is born one year earlier i.e., 1999. This empirical exercise allows us to show the difference between a policy that shifted the minimum working age without establishing concrete penalties for potential employers vs. the shift in 2015 when penalties and rules for hiring minors were established.

The main identifying assumptions of our DiD design is that in the absence of the child labor ban, both groups would have followed the same trajectory. Thus, the main threat to our identification strategy is that the change in the law could shift the labor demand for 15 year old individuals to replace the labor of 14 year old individuals. To show that this is not the case, we provide graphical evidence on the parallel trends and employment rates by age (see Figures 1 and 2).

A second threat to identification of our within-cohort approach is that the estimates could be driven by age differences and not by the change in the law because 14 and 15 year old individuals are not fully comparable. We address this concern in two different ways. First, we exploit the panel data structure of our sample to include individual fixed effects in the specification:

$$Y_{it} = \alpha_1 + \beta_1(Treated_i \times Post{-}ban_t) + \eta' \mathbf{Z}_{it} + \rho_i + \alpha_t + \mathbf{t}\kappa_s + \upsilon_{it} \tag{2}$$

where Y_{it}, denotes either child labor or school enrollment for child i, at survey time t. $Treated_i$ is a dummy variable that takes the value one for individuals born between June 13th and December 31st, 2000. $Post{-}ban_t$ is a dummy variable that takes the value one after June 2015, when the ban was introduced. Z_{imt} is a vector of children time varying characteristics such as age and aged squared. ρ_i captures individual fixed effects, α_t, quarter-by-year fixed effects, $\mathbf{t}\kappa_s$ captures state linear time trends and υ the error

[16] The results are robust to other clustering levels e.g., the state level, and the state-year level and at the state by month of birth level.

term. This specification allows us to estimate the within individual impact of the ban and account for unobserved time invariant characteristics at the individual level. We refrain from using this as the baseline because i) individuals are only followed for five quarters and there is attrition in the sample, which decreases considerably the number of observations; and ii) to facilitate the comparison between the short-run and long-run results.

Second, following Eq. 1 we implement an across-cohort comparison and use the cohort born in 1999 as a control group to estimate the effect of the ban. For this, we construct two definitions for the treatment and control groups: i) the treatment group are individuals born in 2000 and the control group individuals born in 1999; ii) the treatment are individuals born in the second half of 2000 compared to the control who are defined as individuals born in the second half of 1999. In addition, we provide a number of placebo tests focusing on non-affected cohorts to show that our estimates are not driven by age differences. Table A1 in the Appendix summarizes the relevant dates and definitions for the treatment and control groups for the within-cohort and across-cohort comparisons.

Finally, our baseline specification does not take into account the income at the household level. We refrain from including income as a control variable in the main specification due to potential endogeneity concerns and the high number of missing values in the income variable. However, we exploit this information to test heterogeneous effects for different poverty definitions which are less likely to be endogenous e.g., living below or above the poverty line, and income quantiles.

4 Data and Descriptive Statistics

We use data from the Mexican National Survey on Occupation and Employment (ENOE). The ENOE is the largest continuous (rotating) household survey in Mexico collected every year on a quarterly basis. The ENOE is the main source of information on the labor market, employment, informality, and unemployment. The databases include information on 126 thousand households per quarter and are representative at the state level. The data provides information on all household members aged 12 years and older. The guidelines of the survey establish that there is one main informant who provides the information: the individual is usually the household head or the spouse. However, if household members older than 12 are present at the time of the interview, they each provide their own information.

The ENOE provides rich information on schooling and employment[17], as well as demographic characteristics of the child, the parents, and the place of residence. We further complement this database using the marginalization level data obtained from the *Consejo Nacional de Población* (CONAPO) for the year 2010 at the municipality level. The marginalization index is a multidimensional poverty measure which takes into account education, dwelling characteristics, population geographical distribution, and income level (CONAPO, 2019).

For the main analysis, we focus on the cohort of children who were directly affected by the reform, i.e., the cohort of individuals born in 2000, who are 14-15 years old in 2015. We focus on the survey years 2013 to 2017 i.e., two years before and after the ban, to investigate the short-run impact of the ban. We then extend the time frame from 2012 to 2019 to investigate if the effects are persistent after the individual has reached legal adulthood. Additionally, we use data for the cohorts born in 1997, 1998, and 1999 for the across-cohort comparison and placebo tests.

Table 1 provides descriptive statistics for the treatment and control group before the ban was implemented. The final column provides the t-test indicating if the difference in means between groups is significant. The table shows that 95% of children are enrolled in school, 8% are engaged in child labor, and conditional on working, they work 21 hours per week. Almost all working children are in the informal sector and only 45% receive

[17]That is employment status, whether active in the labor market, earnings, and number of hours worked). As of 2018 the survey does not report employment information for individuals younger than 14, because of the change in the minimum working age. This does not affect our estimates because we focus on the sample of 14 years and older starting the year 2013 of the ENOE data.

compensation for their work. 30% of working children work in the primary (agriculture) sector, 18% in the secondary sector (manufacturing), and 52% in the tertiary sector (services). The majority of them do not have either a written contract nor access to benefits. 15% of them work full-time and on average they earn 8 Pesos per hour, which equals the Mexican hourly minimum wage in 2015.

The children in our sample live on average in households with 5 members, and 42% of them are the first-born. 80% of children live with both parents. With respect to parental education, 68% (72.2%) of children have mothers (fathers) who have at most secondary education or higher.[18] Finally, 53% of them live in localities that are highly urbanized areas i.e., localities with more than 100,000 inhabitants.

Comparing the pre-treatment means of the control and treatment groups reveals some differences, but in most cases these differences are negligible. With respect to the outcome variables, children in the treatment group are slightly more likely to be enrolled in school, and less likely to work before the reform. This occurs due to the control group being slightly older than the treatment group, but our specification takes this into account by including month of birth fixed effects. Individuals in the treatment group work about half an hours less than individuals in the control group (conditional on working the difference is about 1.7 hours). For other demographic characteristics the differences are significant, however, they are very small.

4.1 Descriptive Analysis

We start by providing graphical evidence on the evolution of school enrollment and employment rates for the treatment and control groups. Figure 1 shows that before the ban, schooling and employment followed a similar trend with a minor level difference between groups. Focusing on schooling, the figure shows a sharp drop in school enrollment after the second quarter of 2015 for the control group. This drop is not surprising because

[18] For households where the father is not present, we classify the education level of the father as "none".

usually lower secondary is completed at age 15.[19] For the treatment group, we also observe a drop in school enrollment, but not as steep as in the control group. The size of the gap between both groups increases considerably after 2015. By the end of 2017, the school enrollment rate of the treatment group remains higher than that of the control group. Focusing on employment rates, we observe a similar pattern, i.e., a common trend before the ban and a gap that opens up after 2015. The empirical analyses in the long-run further allows us to rule out the existence of diverging pre-trends.

One concern might be that the reduction in child labor for one group could be at the expense of the increase in child labor for another group. Figure 2 shows the average employment rate and the average school drop out rate for individuals aged 13 to 16 by survey year. The figure shows that the child labor supply did not shift from the group ineligible to work (13 and 14 years old), to the group eligible to work (15 and 16 years old). With respect to schooling, Figure 2 also shows no large jumps around the introduction of the ban.

5 Results

In this section, we start by discussing the baseline results focusing on the reform to the Labor Law in 2015. Next, we compare and contrast these results with the results focusing on the Constitutional Amendment in 2014 and a placebo reform. We then examine the results in the long-run by extending the period of analysis. Finally, we then provide the results of the heterogeneity and robustness analysis.

5.1 Baseline Results

We estimate the effect of the child labor ban on the probability of being enrolled in school and on the probability of being employed and report the results in Table 2 following our

[19] The condition to be able to enroll in primary school is turning 6 before the 31st of December of the respective year. There are 6 years of primary school and 3 of lower secondary. A student that followed this path without interruptions or grade repetitions should be in the 9th grade at age 14/15.

specification in Eq.(1). Columns I and II report the results focusing on school enrollment and columns III and IV focusing on employment. For each outcome variable, we provide a specification controlling for the full set of control variables, but excluding month of birth fixed effects (column I and III). In addition, we provide a specification including month of birth fixed effects (column II and IV), which should capture the age difference between treatment and control groups.

The estimated coefficients show that the ban led to an increase in school enrollment by 2.2 percentage points for children in the treatment group relative to children in the control group. The coefficient remains stable after taking into account month of birth fixed effects. The results further indicate that the ban lead to a decrease in the probability of being employed by 1.8 percentage points, but this coefficient drops slightly, to 1.2 percentage points, after taking into account the month of birth fixed effects.

Relative to the pre-ban mean, these coefficients translate to an increase in school enrollment by 2% and a decrease in child labor rates by 16%. These results contradict Edmonds and Shrestha (2012) who find no effect of the minimum working age in 59 low-income countries. Unlike Bharadwaj *et al.* (2020), we find that the probability of work decreases (not increases) after the ban. The results, however, are in line with the findings in Piza and Souza (2016) and are similar in magnitude as in (Bargain and Boutin, 2021), except that the latter do not find significant effects.

Table A3 in the Appendix further shows the results focusing on total weekly hours worked and hours worked conditional on employment. We observe a decrease in the number of weekly hours worked by about 0.75 hours (45 minutes). The estimated coefficient for conditional hours worked is negative but not statistically significant. The latter suggests that the reduction in hours worked is mainly driven by the extensive margin rather than the intensive margin.

Next, we estimate the impact of the ban exploiting within individual variation. In this case, we restrict the sample to individuals who are observed at least once before and after ban. The main advantage of this strategy is that we are able to control for all unobserved heterogeneity at the individual level. However, as individuals are only observed for a maximum of five quarters, this limits the time frame to shortly before and after the ban. The results presented in Table 3 show similar findings as in the baseline results. We find an increase in the probability of being enrolled in school as well as a decrease in the probability of being employed. We further estimate the baseline results, but restricting the sample to the year 2015, i.e, shortly before and after the ban. The results in Table A4 in the Appendix show a similar pattern.

5.2 Constitutional Amendment vs. Labor Law Reform

Next, we analyze the difference between the impact of the Constitutional Amendment in 2014 and the change in the Labor Law in 2015. This empirical exercise is of particular interest because it allows us to examine possible anticipation effects, as well as differences in the impact of the two changes to the legal framework. In addition, to show that our estimated coefficients are not driven by underlying trends we also provide the results of a placebo reform introduced in 2013. For each of these policy changes, we estimate the results using a within-cohort approach, where the affected cohorts are determined by the year when the (placebo) policy is changed i.e., 1998 cohort for the placebo ban, 1999 cohort for the Constitutional Amendment, and 2000 cohort for the Labor Law reform.

Table 4 reports the results focusing on school enrollment (panel A) and employment (panel B). The results of the placebo ban reported in column I are not statistically significant for schooling nor for employment, reducing the concern that our findings are driven by underlying group-specific trends.

Turning to the results of the Constitutional Amendment in 2014 reported in column II, we observe a statistically significant increase in the probability to be enrolled in school, but no impact on the probability of being employed. The estimated coefficient is close to zero and not statistically significant. In contrast, the estimated coefficients for the reform to the Labor Law in 2015 in column III, are larger in magnitude and are both statistically significant.[20]

While we cannot test directly why the Constitutional Amendment only operates through schooling rates, newspaper articles can provide some (auxiliary) evidence on these results. The public coverage in newspaper and official government channels of the Constitutional Amendment in 2014 justified the increase in working age as a mechanism to decrease schooling dropout rates (see e.g., DOF, 2014; Senado de la República, 2014).

In contrast, the newspaper coverage in 2015 of the reform to the Labor Law, highlighted specifically the restrictions and penalties imposed for potential violations to the law (see e.g., Martínez, 2015). These findings suggest that a mere shift in the minimum working age without establishing i) concrete penalties for violations to the law, ii) the corresponding legal framework and its enforcement, so that child labor is not simply shifted from one group to another, is not an effective tool to decrease child labor rates.

5.3 Long-Run Results

Next, we estimate if the labor force reform in 2015 had persistent effects over time. We extend the time frame for the analysis from 2012 to 2019 and follow the cohort born in 2000 until they reach adulthood. The empirical strategy follows the same logic as in Eq. (1), but we estimate the effect by survey-year to observe i) differences between treatment and control groups in the pre-treatment period, ii) differences in the period after the reform, and iii) if these differences are persistent over time.

For this analysis, we focus on the same outcome variables: school enrollment and the probability of being employed.[21] However, as working may not be a disadvantage as the cohort gets older and is permitted to work after individuals turn 15; therefore, we also

[20]We further estimated the results with a sample pooling the cohorts to jointly evaluate the impact of the change in 2014, 2015, and the placebo ban. The estimated coefficients are qualitatively similar and are available upon request.

[21]The main drawback is that the cohort is still young and has not completed their education, which hinders us from estimating the results on high-school or university completion.

investigate the impact on other employment variables that may hinder education i.e., being employed full-time or being employed conditional on not being in education.

Figure 3 reports the point estimates and the confidence interval at the 95% level by survey-year. The reference year is 2012. All graphs show no significant differences between treatment and control group in the pre-treatment period. For the post-treatment period, we observe a significant increase in school enrollment and a decrease in employment mainly driven by: a decrease in the probability of working full-time and/or in the probability of working and being out of school (lower panel).

Similar to the findings for Brazil (Piza and Souza, 2016), these effects seem to last until at least the age of 18, once the individual reaches adulthood. After 2018, individuals in the treatment group reach adulthood and all previous restrictions to enter the labor force do not apply anymore. In this year, there is a spike in school enrollment and employment for the treatment group, which is most likely driven by the slight age difference between groups. The control group reaches adulthood sooner than the treatment group, which leads to a decrease in their school enrollment and an increase in their labor force participation relative to the treatment group. In 2019 when both groups have reached adulthood this spike disappears, however, we continue to observe significant differences for the treatment group, but the differences are smaller.

Finally, we estimate the results of a placebo ban in 2013, for the unaffected cohort born in 1998 and report the results in Figure 4. The results of the placebo ban show no statistically significant differences in the probability of being enrolled in school or working between treatment and control groups for the pre- and post-treatment periods, nor for the probability of working full-time or working and being out of school. These results further support the findings in Figure 3 showing that the effect of the ban on school enrollment and employment is a causal estimate and not mere correlations.

5.4 Heterogeneous Effects

To further analyze the main drivers of the reduction in child labor, we estimate the impact focusing on gender differences, type of employment, and income (poverty) level differences focusing on the baseline specification for the short-run.

We start by analyzing differential impacts by gender and present the results for the interaction term in Table 5. Looking at the impact on school enrollment, the table shows that after the ban girls increase their school enrollment by 3.6 percentage points. For boys, the effect is smaller at 0.9 percentage points. Turning to the child labor results, we find larger impacts for girls (in contrast to Piza and Souza (2017), who find that only boys are affected by the ban). Column II shows that although boys tend to work more hours, girls are the ones who respond more strongly to the ban. Girls decrease total hours worked by almost 1.9 hours. The extensive margin (column III) and intensive margin (column IV) show a similar pattern.

Although these results may seem surprising, we provide additional descriptive statistics in Table A5 by gender. The table shows that indeed fewer girls work in comparison to boys, and on average, girls work less hours per week than boys. However, the largest differences are found in the sector of work: the majority of girls work in the tertiary sector (74% in contrast to 50% of boys), followed by the secondary sector (16% in contrast to 18% for boys), and the primary sector (10% in contrast to 39% for boys).[22]

To further examine the heterogeneous effect for the type of employment, we test how the ban affects formal vs. informal work, paid vs. unpaid work, and sector of employment and report the results in Table 6.[23] The results show a decrease in the probability of being employed in the formal and informal sectors (column I and II). Yet, when examining

[22]In addition, looking at the occupation level the top-5 occupations for boys are farming, fishing and forestry (39%), retail trade (21%), manufactures (12%), hotel and food services (9%), and construction (6%). For girls the top-5 occupations are retail trade (41%), hotel and food services (18%), manufactures (16%), farming, fishing, forestry (10%), other services (9%).

[23]The number of observations differs from the previous results. The underlying sample is the same; however, the variables are set to missing for some groups. For example, the variable formal is equal to one if the individual works in the informal sector. The same logic applies to the remaining variables.

paid and unpaid work (columns III and IV), we observe that the ban had a stronger negative impact on paid activities. The coefficient for unpaid work is close to zero and not statistically significant. The impact of the ban by sector of employment (columns V-VII) shows no significant impact on agricultural work (primary), but a reduction on employment in manufactures (secondary) and services (tertiary).

This is consistent with the idea that subsistence work (which usually takes the form of unpaid agricultural activities) will remain unaltered because the family depends on it to cover their basic needs (see e.g., Basu *et al.*, 2010). These results also explain the larger decrease in employment for girls, who tend to work in the secondary and tertiary sectors, while boys concentrate in the primary sector. The decrease in paid work as well as work in the secondary and tertiary sectors can also be explained by a higher probability of the employer being subject to penalties.

Most of the penalties e.g., through inspections, take place in urban areas for the services and manufacture industries. For potential employers it is costlier to hire underage individuals because of the new set of regulations to hire individuals under the age of 18, and in case of violations they are more likely to be subject to a penalty. Although these restrictions could be overseen for employers in the informal sector, if child labor is visible[24] they could also be subject to penalties.

Next, we examine specifically what happens for those children who continue to work i.e., at the sample conditional on being employed. Table 7 shows the results focusing on full-time work, access to a written contract, to social security benefits, and wages. After the ban, children in the treatment group are less likely to work full-time which is consistent with the new regulations established by the reform to the law. However, we also observe that those children who continue to work are less likely to have a written contract or access to benefits. Previous studies have found that enforcement of labor regulation can push workers to informality (Almeida and Carneiro, 2012). While we do not find an overall increase in informal employment, we observe that conditional on being

[24] Examples include working in markets, selling goods or services in the streets, and packing goods.

employed, the treatment group could be more likely to work in informal activities due to the decrease in the probability of having a contract or access to benefits. We find no significant impact on wages.

We also explore if the results are heterogeneous using different definitions to proxy the poverty level of the household. The results are reported in Figure 5, which show the point estimates and confidence intervals of the effect of the ban interacted with the respective income (regional) classification. The figure reports marginal effects.

Panel A shows the results of an interaction between the effect of the ban and a poverty indicator. This poverty variable indicates if the household lives in i) extreme poverty, ii) moderate poverty, or iii) above the poverty line.[25] Second, in Panel B, we show the results interacting the effect of the ban with the household income per person in quantiles.

In Panel C, we focus on the locality size which captures the urbanization level and is also correlated with the poverty level of the region. Finally, in Panel D, we focus on a categorical indicator that reflects if the municipality where the child lives has a low, average, or high marginalization index and interact it with the effect of the ban, using data from the (CONAPO, 2019).

Accordingly, Figure 5 shows that the probability of being employed decreases for children who live in poor households (panels A and B). The effect is concentrated for children living below the extreme poverty line and for the lowest income quantiles. The results on school enrollment are positive and significant for all poverty categories and income quantiles. In contrast, the decrease in employment mainly happens in areas with a low marginalization level, which are mostly urban areas (panels C and D). The increase in the probability to attend school, is also driven by children living in these areas.

Taken together, the results in Figure 5 suggest that children who are poor, but who live in urban areas are the ones that respond more to the ban. If child labor would only be present in rural areas or only in very low income families, then the results in this section

[25]For this classification, we use information of the yearly average costs of the basket for rural and urban areas provided by the CONEVAL (2020).

may not represent a large reduction in employment. However, the descriptive statistics in Table A6 show that 34% of children who work aged 14-17 live in urban areas, and that 77% of children who work live in households that are extremely poor, or poor, and 23% of the working children live in households above the poverty line.

5.5 Robustness Checks

A potential concern is that employment is simply shifted from younger siblings to older siblings. Figure 2 shows descriptively no sudden increase in the employment rate of individuals older than 15. We show this empirically, by estimating the impact of the reform on individuals who have a younger sibling affected by the reform. The same logic applies as in Eq.(1); however, we define the treatment as individuals aged 15 to 17 years old who have a younger sibling aged 7 to 14 years old and, thus, banned from the labor force. For the comparison group, we focus on individuals aged 15 to 17 years old that have no younger siblings aged 7 to 14. Table 8 shows no significant effects on the ban on the labor force participation of individuals who have a younger sibling affected by the ban.

Next, we address the main concern that our estimates could be partially driven by the age difference between our treatment and control group. In Table 9 we test the sensitivity of our results implementing across-cohort comparisons. We provide the results focusing on a placebo ban in 2013 (column I), on the Constitutional Amendment in 2014 (column II), and on the shift to the Labor Law in 2015 (column III).

For this specification, we focus on the cohort directly affected and the cohort born one year earlier. For instance, for the Labor Law reform in 2015, we use information on the cohorts born in 1999 and 2000 (see Table A1 in the Appendix for the full description). The treatment group is defined as individuals who are born in the second half of the year (June to December), and the control group as all individuals born in the first half of the year (January to June). We interact this variable with a policy variable that takes the value 1 after June 2015. We include the full set of control variables, fixed effects, and further control for cohort fixed effects. The results show a very similar pattern as in the baseline results, with slightly smaller point estimates. The coefficients, show that on average children in the treatment are more likely to enroll in school, and are less likely to work.

In Table 10 we further refine the definition of the treatment and control group of the across-cohort comparison, by restricting the sample to individuals who are born in the second half of the year e.g., for the baseline estimates we define the treatment group as individuals born between June and December of 2000, and the control group as individuals born between June and December of 1999. Similarly, for the Constitutional Amendment and placebo reform we focus on the cohorts 1997-1998 and 1998-1999, respectively. The estimates remain robust. We observe an increase in schooling and a decrease in employment when focusing on the Labor Law reform in 2015.

6 Conclusion

This paper adds to the scarce empirical research on the effect of child labor bans on school enrollment and child labor in developing countries and reconciles previous findings (see e.g. Piza and Souza, 2017; Bharadwaj *et al.*, 2020; Bargain and Boutin, 2021).

We provide evidence of two relevant events that define the legislation in Mexico with respect to child labor: a Constitutional Amendment in 2014 that shifts the minimum working age from 14 to 15, and the reform to the Labor Law in 2015 that couples the increase in the minimum working age with i) concrete penalties for employers, ii) minimum schooling regulations to hire people under 18, and iii) specific regulations to hire individuals over the age of 15 but who have not reached legal adulthood.

Using data from the Mexican Labor Force Survey (ENOE), we implement a DiD approach that exploits the date of birth as a natural cutoff to assign individuals into treatment and control groups. Unlike child labor ban studies in India and Brazil (Bharadwaj *et al.*, 2020; Bargain and Boutin, 2021), we find that the ban indeed led to a decrease in child labor. However, this decrease is only observed after the reform of the Labor Law in 2015 included a more complex package of regulations to eradicate child labor.

Our results for the short-run, show that the reform led to an increase in the probability of being enrolled in school by 2.2 percentage points and to decrease in the probability to work by 1.2 percentage points. These results remain robust to the inclusion of individual fixed effects. This is a sizeable effect, as it is equivalent to an increase in school enrollment by 2% and a decrease in the child labor rate by 16%. Exactly at the threshold between 14 and 15, a back of the envelope calculation shows that due to the ban in 2015, 25 thousand teens who engaged in child labor activities stopped working and almost 50 thousand who would have likely dropped out of school to join the labor force did not drop out of school.

We show that the decrease in the probability to work is mainly driven by a decrease in paid activities and in the secondary and tertiary sectors. Unlike Piza and Souza (2016) we find that the ban has a stronger impact on the reduction of child labor for girls because they tend to work in these two sectors. We find no effect for children working in the agricultural sector or those who are living in highly marginalized rural communities. Instead the effects are concentrated among the poor population in urban regions. We also show that the increase in school enrollment and decrease in employment due to the ban is persistent over time. The treatment group is less likely to work full-time or to be employed and out of school after reaching legal adulthood.

The results in this study are of relevance given the current initiative to decrease the minimum working age in agriculture in Mexico from 18 to 16, which is currently discussed in the Senate (Cantú, 2022). Agriculture is classified as a hazardous activity given that it often involves heavy physical work and handling of pesticides. If the shift is approved, policymakers should guarantee that young people in rural areas have appropriate working conditions, and that their work does not interfere with school.

For policymakers, our study highlights the importance of policies that establish a minimum working age to join the labor force. These policies are a powerful instrument not only to decrease child labor, but also to increase school enrollment. However, our results also show that the enforcement of the law is important and that a mere shift in the minimum working age is not effective if these policies are not coupled with on the one hand, concrete penalties for potential employers who might hire child labor, and on the other hand, with specific regulation to hire underage individuals (e.g., minimum education requirements, reduction in working hours). Finally, the limitations of these policies to decrease child labor related to subsistence work for very poor households in rural areas also needs to be acknowledged.

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Figures

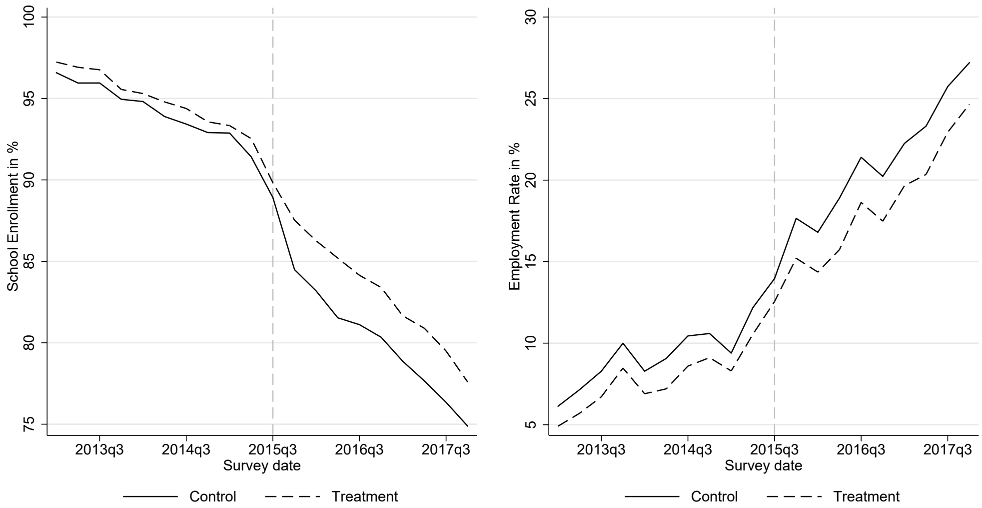

Figure 1: Parallel Trends by Treatment and Control Group
Source: ENOE, authors' analysis.

Notes: – The figure illustrates the shares which are calculated predicting both school attendance and the probability to work controlling for the full set of observable characteristics.All children are born in the year 2000. Children in the control group are born between January 1 and June 12. Children in the treatment group are born between June 13 and December 31. Figure 1 shows that before the ban, schooling and employment followed a similar trend with a minor level difference between groups. Schooling is decreasing after the second quarter of 2015 for the control group because lower secondary education is completed at age 15.19. For the treatment group, school enrollment also decreases, but not as steep as in the control group. The size of the gap between both groups increases considerably after 2015. For Employment rate we observe a similar pattern, i.e. a small level difference and a gap that opens up after the third quarter of 2015.

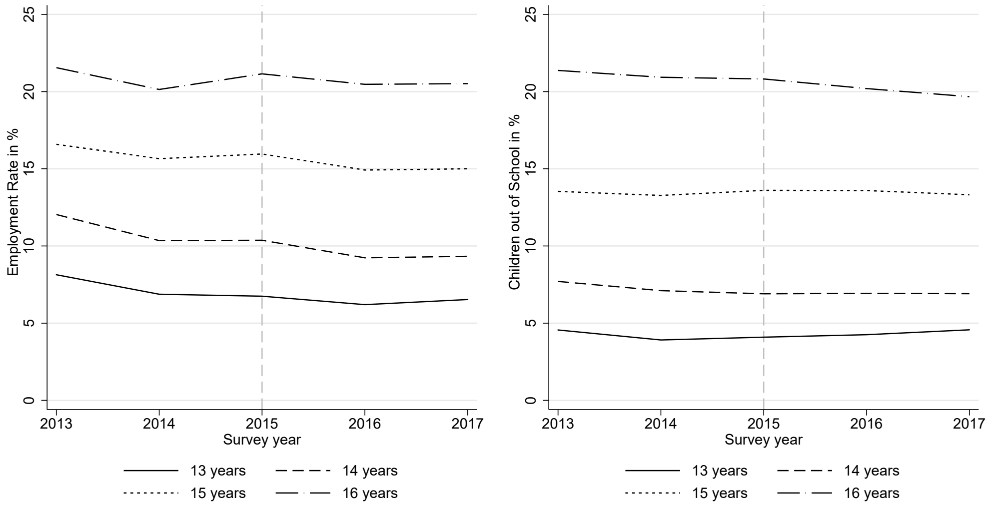

Figure 2: Percentage of Children Employed and Out-of-School by Age
Source: ENOE , authors' analysis.

Notes: – The shares are calculated using the ENOE databases. Both graphs show neither large jumps around the introduction of the ban for the group eligible to work in terms of employment nor for schooling drop out rates presented in the right graph.

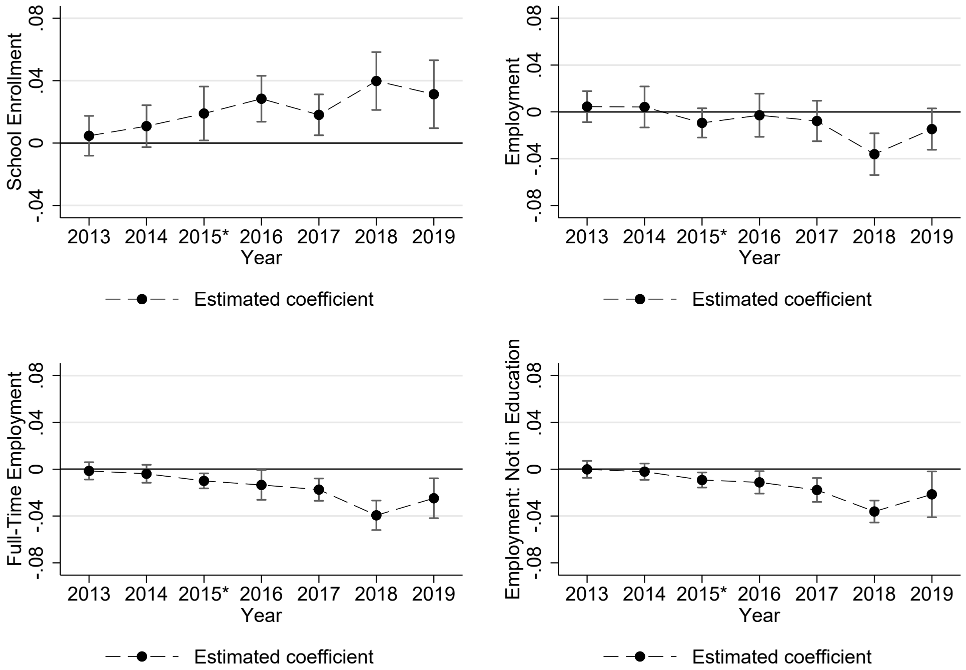

Figure 3: Impact of Ban by Year - Long Run
Source: ENOE , authors' analysis.

Notes: – The results are obtained from linear regression models including the full set of controls, fixed effects, and a state-specific linear time trend. Confidence intervals are reported at the 95% level and the standard errors are calculated at the month of birth-survey year level. The results focus on the cohorts born in 2000. The treatment takes the value 1 if the individuals were born after June 13th. The ban was officially enacted on the third quarter of 2015.

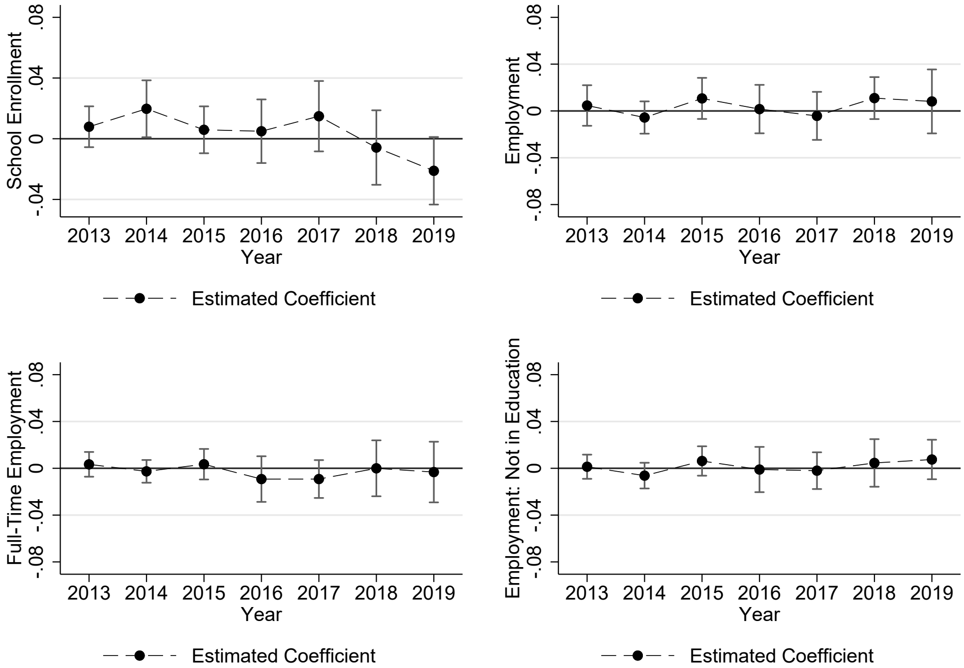

Figure 4: Impact of Ban by Year - Long run: Placebo Ban in 2013

Source: ENOE , authors' analysis.

Notes: – The results are obtained from linear regression models including the full set of controls, fixed effects, age-linear time trends and a state-specific linear time trend. Confidence intervals are reported at the 95% level and the standard errors are calculated at the month of birth-survey year level. The results focus on the cohort born in 1998. The treatment takes the value 1 if the individuals were born after June 13th. The placebo ban is introduced in the third quarter of 2013.

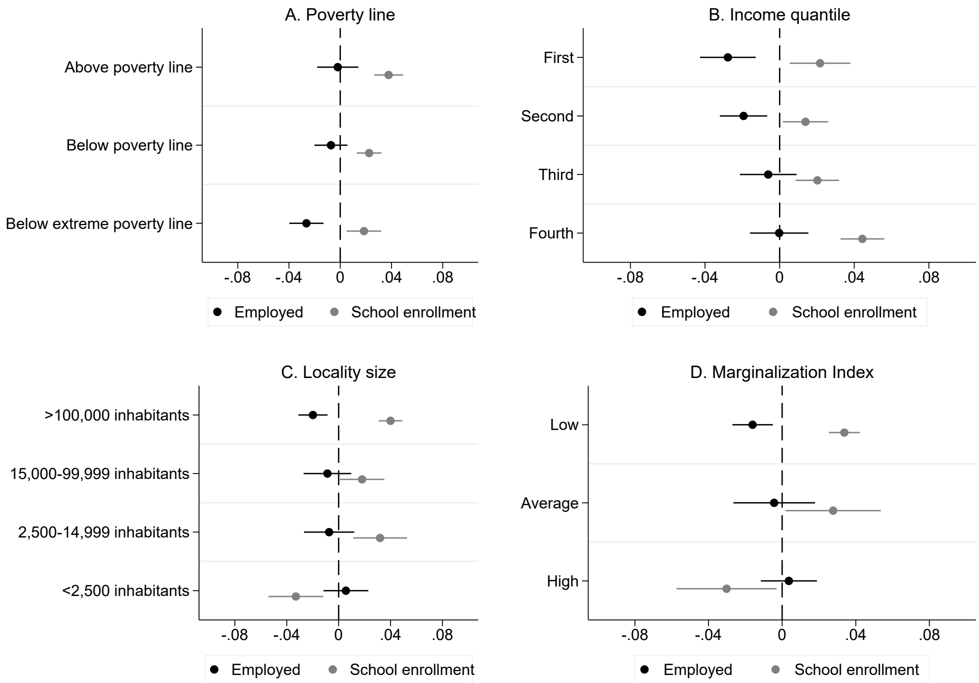

Figure 5: HETEROGENEOUS IMPACTS OF THE CHILD LABOR BAN
Source: ENOE , authors' analysis.

Notes: – Each panel shows for the years 2013 till 2017 the marginal effects of interacting the "Treated x Post-ban" indicator with the respective categorical variable i.e., poverty level, marginalization index, locality size, and income quantile. The results are calculated using as the dependent variable a binary variable indicating if the child i) is employed and ii) is enrolled in school. The regressions include the full set of control variables, time fixed effects, and a state-specific time trend.

Tables

Table 1: Pre-Ban Descriptive Statistics

	All		Treatment		Control		T-test
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Δ Mean[a]
Dependent variables							
Attends school	0.954	0.209	0.959	0.199	0.948	0.223	0.011***
Employed	0.080	0.272	0.074	0.261	0.089	0.285	−0.015***
Total hours worked	1.650	7.160	1.459	6.697	1.911	7.739	−0.452***
Conditional hours worked	20.566	15.805	19.767	15.674	21.468	15.906	−1.702***
Cond. Dependent variables							
Informal work	0.997	0.055	0.998	0.045	0.996	0.064	0.002
Paid employment	0.456	0.498	0.446	0.497	0.467	0.499	−0.021
Sector							
Primary	0.305	0.460	0.304	0.460	0.307	0.461	−0.003
Secondary	0.177	0.382	0.165	0.372	0.190	0.392	−0.024**
Tertiary	0.518	0.500	0.531	0.499	0.503	0.500	0.028**
Contract	0.006	0.080	0.004	0.066	0.009	0.093	−0.004**
Benefits	0.003	0.057	0.002	0.048	0.004	0.064	−0.002
Full-time	0.154	0.361	0.145	0.352	0.164	0.370	−0.019**
Hourly wage	8.177	15.515	7.815	14.439	8.586	16.640	−0.771*
Control variables							
Treatment	0.577	0.494	1.000	0.000	0.000	0.000	1.000
Post-ban	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Male	0.506	0.500	0.506	0.500	0.506	0.500	0.000
Age	13.364	0.896	13.140	0.840	13.668	0.880	−0.527***
Household size	5.034	1.570	5.012	1.534	5.065	1.618	−0.053***
Month of birth	6.652	3.440	9.184	1.879	3.203	1.586	5.981***
Both parents present	0.790	0.407	0.793	0.405	0.787	0.410	0.006**
Family order							
First-born	0.421	0.494	0.413	0.492	0.433	0.495	−0.020***
Second-born	0.287	0.453	0.289	0.453	0.285	0.451	0.004
Last-born	0.291	0.454	0.298	0.457	0.282	0.450	0.016***
Mother's education level							
No education	0.041	0.199	0.039	0.193	0.045	0.208	−0.006***
Primary education	0.298	0.457	0.296	0.457	0.299	0.458	−0.003
Secondary education	0.341	0.474	0.342	0.474	0.341	0.474	0.001
High-school	0.130	0.336	0.131	0.338	0.128	0.335	0.003
Vocational training	0.078	0.268	0.079	0.270	0.076	0.265	0.003
University degree	0.112	0.315	0.113	0.316	0.111	0.314	0.002
Father's education level							
No Education	0.235	0.424	0.230	0.421	0.242	0.428	−0.012***
Primary education	0.233	0.422	0.232	0.422	0.234	0.423	−0.002
Secondary education	0.254	0.436	0.258	0.438	0.249	0.433	0.009**
High-school	0.129	0.335	0.130	0.336	0.127	0.333	0.002
Vocational training	0.029	0.168	0.029	0.169	0.029	0.167	0.001
University degree	0.120	0.325	0.121	0.326	0.119	0.323	0.002
Locality size							
More than 100,000 inhabitants	0.532	0.499	0.527	0.499	0.537	0.499	−0.010**
15,000-99,999 inhabitants	0.133	0.339	0.134	0.341	0.131	0.337	0.004
2,500-14,999 inhabitants	0.134	0.341	0.136	0.343	0.132	0.338	0.004*
Less than 2,500 inhabitants	0.201	0.401	0.202	0.401	0.200	0.400	0.002
Observations	70,053		40,397		29,656		

Notes: – The table presents pre-ban descriptive statistics taken from the ENOE for the years 2013 till 2015 before the change in the minimum working age in 2015. All children are born in the year 2000. Children in the control group are born between January 1 and June 12. Children in the treatment group are born between June 13 and December 31. Other dependent variables are calculated conditional on being employed. [a] This column represents the difference between treatment and control and the respective p-value of the t-test.

Table 2: EFFECT OF THE LABOR LAW REFORM IN 2015 ON SCHOOL ENROLLMENT AND EMPLOYMENT

	School enrollment	School enrollment	Employed	Employed
	I	II	III	IV
Treated x Post-ban	0.021^{***}	0.022^{***}	-0.018^{***}	-0.012^{**}
	(0.005)	(0.004)	(0.006)	(0.005)
Male	-0.024^{***}	-0.024^{***}	0.102^{***}	0.102^{***}
	(0.004)	(0.004)	(0.006)	(0.006)
Age	-0.012^{***}	-0.006^{**}	0.016^{***}	0.004
	(0.003)	(0.002)	(0.003)	(0.003)
HH size	-0.016^{***}	-0.016^{***}	0.009^{***}	0.009^{***}
	(0.002)	(0.002)	(0.001)	(0.001)
Birth rank: Ref.: First-born				
Middle-born	0.007^{**}	0.008^{**}	0.002	0.001
	(0.003)	(0.003)	(0.003)	(0.003)
Last-born	0.005^{**}	0.005^{**}	-0.018^{***}	-0.018^{***}
	(0.002)	(0.002)	(0.002)	(0.002)
Both parents present	-0.054^{***}	-0.052^{***}	0.036^{***}	0.035^{***}
	(0.014)	(0.014)	(0.011)	(0.011)
Mother's education level: Ref.: None				
Primary education	0.084^{***}	0.084^{***}	-0.052^{***}	-0.052^{***}
	(0.014)	(0.013)	(0.006)	(0.006)
Secondary education	0.147^{***}	0.148^{***}	-0.080^{***}	-0.080^{***}
	(0.015)	(0.015)	(0.007)	(0.007)
High-school	0.189^{***}	0.189^{***}	-0.101^{***}	-0.101^{***}
	(0.017)	(0.017)	(0.009)	(0.009)
Vocational training	0.187^{***}	0.187^{***}	-0.111^{***}	-0.111^{***}
	(0.018)	(0.018)	(0.010)	(0.010)
University degree	0.198^{***}	0.198^{***}	-0.132^{***}	-0.132^{***}
	(0.018)	(0.018)	(0.010)	(0.010)
Father's education level: Ref.: None/Father not present				
Primary education	0.052^{***}	0.051^{***}	-0.030^{***}	-0.030^{***}
	(0.015)	(0.015)	(0.011)	(0.011)
Secondary education	0.101^{***}	0.100^{***}	-0.065^{***}	-0.065^{***}
	(0.014)	(0.014)	(0.012)	(0.012)
High-school	0.123^{***}	0.122^{***}	-0.081^{***}	-0.081^{***}
	(0.015)	(0.015)	(0.012)	(0.012)
Vocational training	0.123^{***}	0.122^{***}	-0.086^{***}	-0.086^{***}
	(0.015)	(0.014)	(0.013)	(0.013)
University degree	0.115^{***}	0.114^{***}	-0.091^{***}	-0.091^{***}
	(0.014)	(0.014)	(0.012)	(0.012)
Locality size: Ref.: > 100,000 inhabitants				
15,000-99,999 inhabitants	-0.005	-0.005	0.023^{***}	0.023^{***}
	(0.004)	(0.004)	(0.003)	(0.003)
2,500-14,999 inhabitants	-0.005	-0.005	0.029^{***}	0.029^{***}
	(0.004)	(0.004)	(0.004)	(0.004)
Less than 2,500 inhabitants	-0.021^{***}	-0.021^{***}	0.060^{***}	0.061^{***}
	(0.005)	(0.005)	(0.004)	(0.004)
Constant	1.040^{***}	0.991^{***}	-0.195^{***}	-0.036
	(0.038)	(0.036)	(0.031)	(0.037)
State FE	yes	yes	yes	yes
Quarter-by-year FE	yes	yes	yes	yes
Month of birth FE	no	yes	no	yes
State-specific trend	yes	yes	yes	yes
Observations	123,487	123,487	123,487	123,487
R^2	0.132	0.135	0.110	0.112

*Notes: – Results are obtained from DiD models. Data are from the ENOE for the years 2013 till 2017.– Standard errors in parentheses (clustered at the birth month-survey year level). – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table 3: ROBUSTNESS: EFFECT OF BAN IN 2015 ON SCHOOL ENROLLMENT AND EMPLOYMENT: INDIVIDUAL FIXED EFFECTS APPROACH

Dependent variable:	**School enrollment** I	**Hours worked** II	**Extensive margin** III	**Intensive margin** IV
Post-ban	−0.042	4.237***	0.122**	−3.946
	(0.030)	(1.529)	(0.050)	(9.794)
Treated x Post-ban	0.028***	−0.733***	−0.020**	−0.486
	(0.005)	(0.267)	(0.009)	(1.519)
Individual FE	yes	yes	yes	yes
Quarter-by-year FE	yes	yes	yes	yes
State-specific trend	yes	yes	yes	yes
Observations	23,562	23,562	23,562	3,035
R^2	0.029	0.013	0.012	0.054

*Notes: – Results are obtained from DiD models. The data is taken from the ENOE for the years 2013 till 2017. – Robust standard errors in parentheses. the regressions include the full set of controls, individual fixed effects, birth rank, state fixed effects, quarter-by-year fixed effects and state-specific time trend.– *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table 4: EFFECT OF THE CHILD LABOR BAN: PLACEBO, CONSTITUTION AMENDMENT, AND LABOR LAW REFORM

	I	II	III
	A. School enrollment		
Treat cohort 1998 x Placebo-ban 2013	0.011	–	–
	(0.007)		
Treat cohort 1999 x Constitutional Amendment 2014	–	0.019***	–
		(0.007)	
Treat cohort 2000 x Post-ban 2015	–	–	0.022***
			(0.004)
Observations	80,976	105,554	123,487
	B. Employment		
Treat cohort 1998 x Placebo-ban 2013	−0.001	–	–
	(0.006)		
Treat cohort 1999 x Constitutional Amendment 2014	–	−0.004	–
		(0.004)	
Treat cohort 2000 x Post-ban 2015	–	–	−0.012**
			(0.005)
Observations	80,976	105,554	123,487

*Notes: – Year 2015 corresponds to the year the child labor law changed. Results are obtained from DiD models. the regressions include the full set of controls, state fixed effects, quarter-by-year fixed effects and state-specific time trend.– Standard errors in parentheses (clustered at the birth month-survey year). – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table 5: EFFECT OF BAN IN 2015 BY GENDER

Dependent variable:	**School enrollment** I	**Hours worked** II	**Extensive margin** III	**Intensive margin** IV
Effect of ban	0.036***	−1.876***	−0.036***	−1.952***
	(0.006)	(0.251)	(0.007)	(0.727)
Male	−0.017***	2.630***	0.090***	3.285***
	(0.004)	(0.294)	(0.007)	(0.432)
Male x Effect of ban	−0.027***	2.167***	0.047***	1.477**
	(0.008)	(0.416)	(0.009)	(0.658)
Observations	123,487	123,487	123,487	15,911
R^2	0.136	0.115	0.113	0.182

*Notes: – Results are obtained from DiD models. The data is taken from the ENOE for the years 2013 till 2017. – Standard errors in parentheses (clustered at the birth month-survey year level). the regressions include the full set of controls, state fixed effects, quarter-by-year fixed effects and state-specific time trend.– *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table 6: Effect of Ban in 2015: Formal, Paid Employment, and Sector

Dependent variable:	Formal	Informal	Paid	Unpaid	Primary	Secondary	Tertiary
	I	II	III	IV	V	VI	VII
Treated x Post-ban	-0.004^{***}	-0.010^{*}	-0.017^{***}	0.003	-0.001	-0.007^{***}	-0.011^{***}
	(0.001)	(0.005)	(0.004)	(0.003)	(0.003)	(0.002)	(0.004)
Observations	108,037	123,026	117,372	113,703	111,335	110,960	116,054
R^2	0.021	0.109	0.100	0.064	0.135	0.050	0.047

*Notes: – Results are obtained from DiD models. The data is taken from the ENOE for the years 2013 till 2017. the regressions include the full set of controls, state fixed effects, quarter-by-year fixed effects and state-specific time trend.– Standard errors in parentheses (clustered at the birth month-survey year level). – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table 7: Effect of Ban in 2015 on Child Labor: Conditional on being Employed

Dependent variable:	Full-time I	Contract II	Benefits III	Ln(wage)[a] IV
Treated x Post-ban	-0.044^{***}	-0.014^{***}	-0.008^{*}	0.045
	(0.016)	(0.004)	(0.004)	(0.033)
Mean	.293	.0345	.028	2.863
Observations	15,832	15,832	15,832	8,928
R^2	0.151	0.076	0.078	0.145

*Notes: – Results are obtained from DiD models. The data is taken from the ENOE for the years 2013 till 2017. the regressions include the full set of controls, state fixed effects, quarter-by-year fixed effects and state-specific time trend.– Standard errors in parentheses (clustered at month of birth- survey year level). – [a] Conditional on receiving payment for work. – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table 8: Effect of Ban in 2015 for Older Siblings

Dependent variable:	School enrollment I	Hours worked II	Extensive margin III	Intensive margin IV
Ind. has a sibling banned from LF	0.013^{***}	-0.032	0.005^{**}	-0.582^{***}
	(0.002)	(0.075)	(0.002)	(0.204)
Post-ban	-0.252^{***}	11.539^{***}	0.282^{***}	13.656^{***}
	(0.020)	(0.798)	(0.020)	(2.007)
Banned sibling x Post-ban	0.006^{*}	-0.065	-0.003	-0.178
	(0.003)	(0.129)	(0.003)	(0.319)
State FE	yes	yes	yes	yes
Quarter-by-year FE	yes	yes	yes	yes
Month of birth FE	yes	yes	yes	yes
State-specific trend	yes	yes	yes	yes
Observations	271,985	271,985	271,985	57,119
R^2	0.154	0.114	0.117	0.114

*Notes: – Results are obtained from DiD models. The data is taken from the ENOE for the years 2013 till 2017. – Robust standard errors in parentheses. the regressions include the full set of controls, birth rank, state fixed effects, quarter-by-year fixed effects and state-specific time trend.– *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table 9: Effect of the Child Labor Ban: Placebo, Constitution Amendment, and Labor Law Reform - Two Cohort Definition

	I	II	III
	A. School enrollment		
Treat cohort 1997/1998 x Placebo-ban 2013	0.003 (0.006)	–	–
Treat cohort 1998/1999 x Constitutional Amendment 2014	–	0.011** (0.005)	–
Treat cohort 1999/2000 x Post-ban 2015	–	–	0.009* (0.005)
Observations	140,054	186,545	229,068
	B. Employment		
Treat cohort 1997/1998 x Placebo-ban 2013	−0.005 (0.005)	–	–
Treat cohort 1998/1999 x Constitutional Amendment 2014	–	−0.003 (0.005)	–
Treat cohort 1999/2000 x Post-ban 2015	–	–	−0.008* (0.005)
Observations	140,043	186,530	229,041

*Notes: – Year 2015 corresponds to the year the child labor law changed. Results are obtained from DiD models. the regressions include the full set of controls, state fixed effects, quarter-by-year fixed effects and state-specific time trend.– Standard errors in parentheses (clustered at the birth month survey-year level). – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table 10: Effect of the Child Labor Ban: Placebo, Constitution Amendment, and Labor Law Reform - Two Cohorts (Born between July and December)

	I	II	III
	A. School enrollment		
Treat cohort 1997/1998 x Placebo-ban 2013	0.005 (0.007)	–	–
Treat cohort 1998/1999 x Constitutional Amendment 2014	–	0.019*** (0.007)	–
Treat cohort 1999/2000 x Post-ban 2015	–	–	0.021*** (0.006)
Observations	84,925	112,218	136,454
	B. Employment		
Treat cohort 1997/1998 x Placebo-ban 2013	−0.004 (0.007)	–	–
Treat cohort 1998/1999 x Constitutional Amendment 2014	–	−0.008 (0.006)	–
Treat cohort 1999/2000 x Post-ban 2015	–	–	−0.023*** (0.005)
Observations	84,918	112,209	136,438

*Notes: – Year 2015 corresponds to the year the child labor law changed. Results are obtained from DiD models. the regressions include the full set of controls, state fixed effects, quarter-by-year fixed effects and state-specific time trend.– Standard errors in parentheses (clustered at the birth month-survey year level). – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Appendix

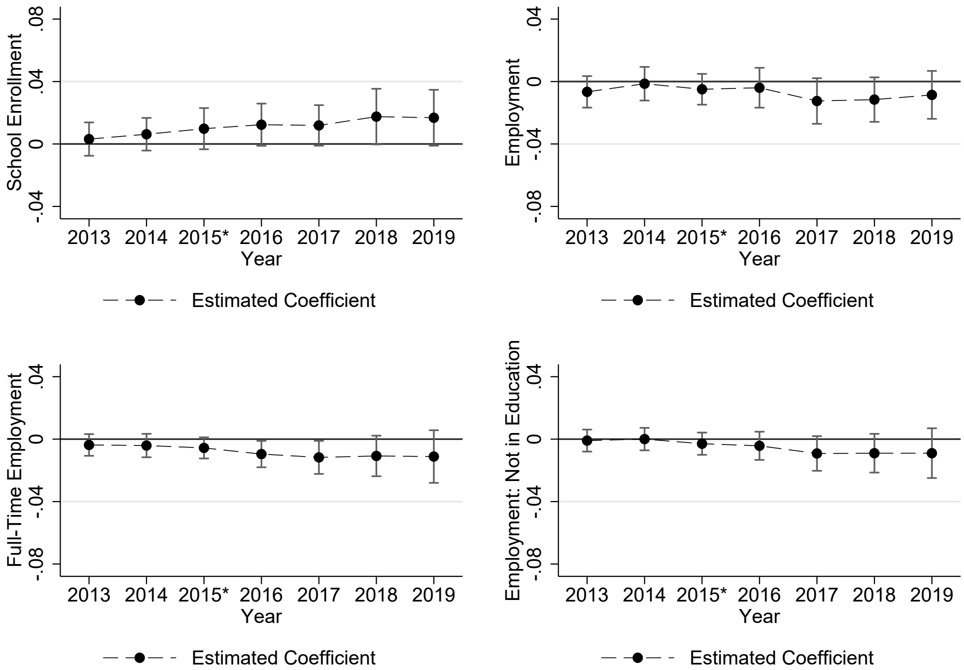

Figure A1: Impact of Ban by Year: Two-Cohort Definition

Source: ENOE , authors' analysis.

Notes: – The results are obtained from linear regression models including the full set of controls, fixed effects, and a state-specific linear time trend. Confidence intervals are reported at the 95% level and the standard errors are calculated at the month of birth-survey year level. The results focus on the cohorts born in 1999 and 2000. The treatment takes the value 1 if the individuals were born after June 13th. The ban was officially enacted on the third quarter of 2015.

Table A1: Summary of treatment and control groups

Policy change	**Main change**	**Group**	**Within-cohort approach**	**Across-cohort approach**	**Across-cohort approach (second half)**
Constitution Amendment:	Minimum working age shift from 14 to 15.	Treatment	17/06–31/12, 1999	17/06–31/12, 1998 and 1999	17/06–31/12, 1999
June 17th, 2014		Control	01/01–16/06, 1999	01/01–16/06, 1998 and 1999	17/06–31/12, 1998
Labor Law Reform:	Minimum working age shift from 14 to 15.	Treatment	12/06–31/12, 2000	12/06–31/12, 1999 and 2000	12/06–31/12, 2000
June 12th, 2015	Work regulation for individuals aged 15-17	Control	01/01–11/06, 2000	01/01–11/06, 1999 and 2000	12/06–31/12, 1999
	Penalties for violations				
Placebo Reform:	Placebo	Treatment	12/06–31/12, 1998	12/06–31/12, 1997 and 1998	12/06–31/12, 1998
June 12th, 2013		Control	01/01–11/06, 1998	01/01–11/06, 1997 and 1998	12/06–31/12, 1997

Notes: – The table presents a summary of all cutoff dates to define treatment and control groups for the Constitutional Amendment, the reform to the Labor Law, and the Placebo reform.

Table A2: Post-Ban Descriptive Statistics

	All		Treatment		Control		T-test
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Δ Mean[a]
Dependent variables							
Attends school	0.831	0.375	0.843	0.363	0.813	0.390	0.030***
Employed	0.193	0.394	0.181	0.385	0.209	0.406	−0.028***
Total hours worked	5.874	14.533	5.368	13.881	6.565	15.352	−1.197***
Conditional hours worked	30.504	18.590	29.684	18.542	31.474	18.601	−1.790***
Cond. Dependent variables							
Informal work	0.957	0.203	0.962	0.192	0.951	0.216	0.011***
Paid employment	0.702	0.457	0.687	0.464	0.719	0.449	−0.032***
Sector							
Primary	0.206	0.404	0.210	0.407	0.201	0.401	0.009
Secondary	0.238	0.426	0.235	0.424	0.242	0.429	−0.007
Tertiary	0.556	0.497	0.555	0.497	0.557	0.497	−0.001
Contract	0.050	0.219	0.042	0.200	0.061	0.239	−0.019***
Benefits	0.042	0.201	0.038	0.190	0.048	0.214	−0.011***
Full-time	0.371	0.483	0.350	0.477	0.397	0.489	−0.048***
Hourly wage	15.329	35.151	14.922	27.179	15.811	42.708	−0.890
Control variables							
Treatment	0.577	0.494	1.000	0.000	0.000	0.000	1.000
Post-ban	1.000	0.000	1.000	0.000	1.000	0.000	0.000
Male	0.519	0.500	0.516	0.500	0.522	0.500	−0.005
Age	15.841	0.759	15.642	0.713	16.114	0.734	−0.472***
Household size	4.937	1.593	4.918	1.575	4.963	1.617	−0.045***
Month of birth	6.663	3.439	9.190	1.889	3.211	1.572	5.979***
Both parents present	0.748	0.434	0.745	0.436	0.753	0.431	−0.008**
Family order							
First-born	0.491	0.500	0.488	0.500	0.495	0.500	−0.008*
Second-born	0.251	0.434	0.250	0.433	0.252	0.434	−0.002
Last-born	0.258	0.438	0.262	0.440	0.253	0.434	0.010**
Mother's education level							
No education	0.041	0.198	0.036	0.187	0.047	0.212	−0.011***
Primary education	0.266	0.442	0.262	0.440	0.270	0.444	−0.008**
Secondary education	0.362	0.481	0.369	0.482	0.353	0.478	0.016***
High-school	0.143	0.350	0.147	0.355	0.137	0.344	0.010***
Vocational training	0.071	0.257	0.070	0.256	0.072	0.258	−0.001
University degree	0.118	0.322	0.115	0.319	0.121	0.326	−0.006**
Father's education level							
No Education	0.279	0.448	0.281	0.449	0.277	0.447	0.004
Primary education	0.204	0.403	0.199	0.400	0.210	0.407	−0.010***
Secondary education	0.246	0.430	0.249	0.432	0.241	0.428	0.008**
High-school	0.129	0.335	0.129	0.335	0.130	0.336	−0.001
Vocational training	0.025	0.157	0.027	0.161	0.023	0.151	0.004**
University degree	0.117	0.322	0.116	0.320	0.119	0.324	−0.004
Locality size							
More than 100,000 inhabitants	0.538	0.499	0.537	0.499	0.539	0.498	−0.002
15,000-99,999 inhabitants	0.139	0.346	0.137	0.344	0.142	0.349	−0.005*
2,500-14,999 inhabitants	0.140	0.347	0.141	0.348	0.138	0.345	0.003
Less than 2,500 inhabitants	0.183	0.387	0.185	0.388	0.180	0.384	0.005
Observations	53,434		30,852		22,582		

Notes: – The table presents descriptive statistics after the change in the minimum working age in 2015, that is from 2015 until 2017 talen from the ENOE. All children are born in the year 2000. Children in the control group are born between January 1 and June 12. Children in the treatment group are born between June 13 and December 31.

Table A3: Effect of Child Labor Ban on Total Hours Worked and on Conditional Hours Worked

	Hours worked I	Hours worked II	Intensive margin III	Intensive margin IV
Treated x Post-ban	-0.861^{***}	-0.755^{***}	-1.250^{***}	-0.915
	(0.184)	(0.157)	(0.450)	(0.598)
Male	3.177^{***}	3.171^{***}	3.879^{***}	3.808^{***}
	(0.266)	(0.267)	(0.335)	(0.333)
Age	0.497^{***}	0.187^{**}	0.960^{**}	0.253
	(0.088)	(0.089)	(0.395)	(0.466)
HH size	0.408^{***}	0.409^{***}	0.846^{***}	0.833^{***}
	(0.044)	(0.045)	(0.106)	(0.105)
Birth rank: Ref.: First-born				
Middle-born	-0.017	-0.034	0.274	0.175
	(0.104)	(0.105)	(0.414)	(0.411)
Last-born	-0.461^{***}	-0.462^{***}	0.123	0.080
	(0.078)	(0.078)	(0.388)	(0.375)
Both parents present	1.248^{***}	1.233^{***}	0.030	0.098
	(0.366)	(0.362)	(0.910)	(0.916)
Mother's education level: Ref.: None				
Primary education	-2.259^{***}	-2.255^{***}	-2.723^{***}	-2.733^{***}
	(0.298)	(0.296)	(0.655)	(0.632)
Secondary education	-3.401^{***}	-3.395^{***}	-4.978^{***}	-5.059^{***}
	(0.334)	(0.333)	(0.702)	(0.689)
High-school	-4.341^{***}	-4.330^{***}	-8.499^{***}	-8.572^{***}
	(0.417)	(0.417)	(0.868)	(0.862)
Vocational training	-4.426^{***}	-4.440^{***}	-8.000^{***}	-8.035^{***}
	(0.441)	(0.441)	(0.957)	(0.952)
University degree	-5.043^{***}	-5.043^{***}	-10.875^{***}	-10.943^{***}
	(0.477)	(0.478)	(0.884)	(0.882)
Father's education level: Ref.: None/Father not present				
Primary education	-1.183^{***}	-1.161^{***}	-0.877	-0.969
	(0.387)	(0.385)	(0.844)	(0.833)
Secondary education	-2.443^{***}	-2.420^{***}	-2.774^{***}	-2.792^{***}
	(0.412)	(0.408)	(0.923)	(0.919)
High-school	-3.007^{***}	-2.998^{***}	-4.691^{***}	-4.775^{***}
	(0.419)	(0.416)	(0.920)	(0.909)
Vocational training	-3.154^{***}	-3.142^{***}	-7.012^{***}	-6.895^{***}
	(0.423)	(0.418)	(1.388)	(1.403)
University degree	-3.082^{***}	-3.070^{***}	-5.820^{***}	-5.788^{***}
	(0.414)	(0.409)	(0.932)	(0.940)
Locality size: Ref.: > 100,000 inhabitants				
15,000-99,999 inhabitants	0.591^{***}	0.592^{***}	0.532	0.614
	(0.123)	(0.124)	(0.586)	(0.584)
2,500-14,999 inhabitants	0.435^{***}	0.423^{***}	-1.546^{***}	-1.482^{***}
	(0.144)	(0.145)	(0.491)	(0.510)
Less than 2,500 inhabitants	0.958^{***}	0.970^{***}	-3.051^{***}	-2.985^{***}
	(0.130)	(0.128)	(0.326)	(0.338)
Constant	-5.509^{***}	-1.726	10.254^{*}	18.760^{***}
	(1.112)	(1.234)	(5.525)	(6.581)
State FE	yes	yes	yes	yes
Quarter-by-year FE	yes	yes	yes	yes
Month of birth FE	no	yes	no	yes
State-specific trend	yes	yes	yes	yes
Observations	123,487	123,487	15,911	15,911
R^2	0.111	0.114	0.174	0.181

*Notes: – Results are obtained from DiD models. The data is taken from the ENOE for the years 2013 till 2017. – Standard errors in parentheses (clustered at the birth month-survey year level). – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table A4: Effect of Child Labor Ban on Total Hours Worked and on Conditional Hours Worked for the Year 2015

	School enrollment I	Employment II	Extensive margin III	Intensive margin IV
Treated x Post-ban	0.039***	−0.018*	−0.967***	−3.423*
	(0.009)	(0.008)	(0.309)	(1.779)
Observations	25,373	25,373	25,373	2,927
R^2	0.099	0.099	0.086	0.129

*Notes: – Results are obtained from DiD models. The regressions include information for the year 2015 i.e., six months before the reform and six months after, to analyze immediate effects. the regressions include the full set of controls, state fixed effects, quarter-by-year fixed effects and state-specific time trend.– Standard errors in parentheses (clustered at the birth month-survey year level). – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table A5: Pre-ban Descriptive Statistics by Gender: Conditional on Working

	Girls		**Boys**		**T-test**
	Mean	S.D.	Mean	S.D.	Δ Mean[a]
Dependent variables					
Attends school	0.832	0.374	0.786	0.410	0.046***
Employed	1.000	0.000	1.000	0.000	0.000
Total hours worked	18.126	14.258	21.557	16.290	−3.431***
Conditional hours worked	18.126	14.258	21.557	16.290	−3.431***
Cond. Dependent variables					
Informal work	0.999	0.035	0.996	0.061	0.003
Paid employment	0.394	0.489	0.481	0.500	−0.086***
Sector					
Primary	0.096	0.294	0.391	0.488	−0.295***
Secondary	0.163	0.370	0.182	0.386	−0.019*
Tertiary	0.741	0.438	0.427	0.495	0.315***
Contract	0.004	0.061	0.008	0.086	−0.004
Benefits	0.001	0.035	0.004	0.063	−0.003*
Full-time	0.140	0.347	0.229	0.421	−0.089***
Part-time	0.825	0.380	0.736	0.441	0.089***
Hourly wage	7.382	15.719	8.501	15.421	−1.119**
Observations	1,625		3,997		

Notes: – The table presents descriptive statistics for children aged 14 to 17 years old that are working before the change in Labor Law in 2015 accounting for the years 2013-2015 from the ENOE data.

Table A6: Pre-Ban Descriptive statistics: Working vs. Non-Working Children

	Working Children		Non-Working Children		T-test
	Mean	S.D.	Mean	S.D.	Δ Mean[a]
Dependent variables					
Attends school	0.799	0.401	0.968	0.177	−0.168***
Employed	1.000	0.000	0.000	0.000	1.000
Conditional hours worked	20.566	15.805	0.000	0.000	20.566***
Male	0.711	0.453	0.488	0.500	0.223***
Age	13.586	0.894	13.344	0.894	0.241***
Household size	5.465	1.849	4.997	1.538	0.469***
Both parents present	0.778	0.416	0.792	0.406	−0.014**
Month of birth	6.232	3.441	6.689	3.437	−0.457***
Household income per person	1.369	1.736	1.637	2.017	−0.268***
Poverty					
Non-poor	0.229	0.420	0.237	0.426	−0.009
Poor	0.333	0.471	0.405	0.491	−0.071***
Extreme poor	0.438	0.496	0.358	0.479	0.080***
Family order					
First-born	0.394	0.489	0.424	0.494	−0.030***
Second-born	0.378	0.485	0.280	0.449	0.098***
Last-born	0.229	0.420	0.297	0.457	−0.068***
Mother's education level					
No education	0.095	0.293	0.037	0.188	0.058***
Primary education	0.417	0.493	0.287	0.452	0.130***
Secondary education	0.327	0.469	0.343	0.475	−0.016**
High-school	0.085	0.279	0.134	0.341	−0.049***
Vocational training	0.041	0.198	0.081	0.273	−0.040***
University degree	0.035	0.184	0.119	0.324	−0.084***
Father's education level					
No Education	0.281	0.449	0.231	0.422	0.049***
Primary education	0.350	0.477	0.222	0.416	0.127***
Secondary education	0.235	0.424	0.256	0.436	−0.021***
High-school	0.084	0.277	0.133	0.339	−0.049***
Vocational training	0.014	0.117	0.030	0.172	−0.017***
University degree	0.037	0.189	0.127	0.333	−0.090***
Locality size					
More than 100,000 inhabitants	0.337	0.473	0.549	0.498	−0.211***
15,000-99,999 inhabitants	0.137	0.344	0.132	0.339	0.005
2,500-14,999 inhabitants	0.166	0.372	0.132	0.338	0.034***
Less than 2,500 inhabitants	0.360	0.480	0.187	0.390	0.173***
Observations	5,622		64,431		

Notes: – The table presents descriptive statistics for children aged 14 to 17 years old that are working vs. those children of the same age that are not working before the change in Labor Law in 2015 accounting for the years 2013-2015 taken from the ENOE data. [a] *This column represents the difference between treatment and control and the respective p-value of the t-test.*

Table A7: Post-Ban Descriptive statistics: Working vs. Non-Working Children

	Working Children		Non-Working Children		T-test
	Mean	S.D.	Mean	S.D.	Δ Mean[a]
Dependent variables					
Attends school	0.559	0.497	0.895	0.306	−0.337***
Employed	1.000	0.000	0.000	0.000	1.000
Conditional hours worked	30.504	18.590	0.000	0.000	30.504***
Male	0.715	0.452	0.472	0.499	0.243***
Age	15.982	0.760	15.808	0.755	0.174***
Household size	5.264	1.808	4.859	1.527	0.405***
Both parents present	0.730	0.444	0.752	0.432	−0.022***
Month of birth	6.361	3.401	6.735	3.445	−0.374***
Household income per person	1.918	1.731	1.879	2.015	0.039*
Poverty					
Non-poor	0.319	0.466	0.263	0.440	0.056***
Poor	0.318	0.466	0.319	0.466	−0.001
Extreme poor	0.363	0.481	0.418	0.493	−0.055***
Family order					
First-born	0.476	0.499	0.494	0.500	−0.018***
Second-born	0.303	0.460	0.239	0.426	0.064***
Last-born	0.221	0.415	0.267	0.442	−0.046***
Mother's education level					
No education	0.079	0.270	0.032	0.175	0.047***
Primary education	0.379	0.485	0.238	0.426	0.141***
Secondary education	0.365	0.481	0.361	0.480	0.004
High-school	0.098	0.297	0.154	0.361	−0.056***
Vocational training	0.037	0.188	0.079	0.270	−0.043***
University degree	0.043	0.202	0.135	0.342	−0.093***
Father's education level					
No Education	0.320	0.467	0.269	0.443	0.051***
Primary education	0.309	0.462	0.179	0.383	0.130***
Secondary education	0.236	0.425	0.248	0.432	−0.012**
High-school	0.077	0.267	0.141	0.349	−0.064***
Vocational training	0.013	0.113	0.028	0.165	−0.015***
University degree	0.044	0.205	0.135	0.341	−0.091***
Locality size					
More than 100,000 inhabitants	0.422	0.494	0.566	0.496	−0.144***
15,000-99,999 inhabitants	0.142	0.349	0.138	0.345	0.004
2,500-14,999 inhabitants	0.156	0.362	0.136	0.343	0.019***
Less than 2,500 inhabitants	0.280	0.449	0.160	0.366	0.121***
Observations	10,289		43,145		

Notes: – The table presents descriptive statistics for children aged 14 to 17 years old that are working vs. those children of the same age that are not working before the change in Labor Law in 2015 accounting for the years 2015-2017 taken from the ENOE data. [a] This column represents the difference between treatment and control and the respective p-value of the t-test.

Table A8: Post-Ban Descriptive Statistics for 2018 and 2019

	2018				2019			
	Treatment		Control		Treatment		Control	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Dependent variables								
Attends school	0.740	0.438	0.692	0.462	0.618	0.486	0.571	0.495
One Year of High School	0.680	0.466	0.726	0.446	0.755	0.430	0.768	0.422
Completed High School	0.074	0.262	0.133	0.340	0.355	0.479	0.502	0.500
Enrolled in University	0.002	0.049	0.010	0.100	0.055	0.229	0.093	0.290
Completed Secondary Education	0.912	0.283	0.919	0.273	0.936	0.245	0.935	0.246
Control variables								
Treatment	1.000	0.000	0.000	0.000	1.000	0.000	0.000	0.000
Observations	12,825		9,086		12,377		8,385	

Notes: – The table presents descriptive statistics after the change in the minimum working age in 2015 for the years 2018 and 2019, respectively. All children are born in the year 2000. Children in the control group are born between January 1 and June 12. Children in the treatment group are born between June 13 and December 31.