Putting Kids Out of Work: Unintended Consequences of Child Labor Legislation in Bolivia*

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Abstract

We study the effects of a Bolivian law that introduced benefits and protections for child workers and lowered the de facto legal working age from 14 to 10. We employ a difference-in-discontinuity approach that exploits the variation in the law's application to different age groups. Work decreased for children under 14, whose work was newly legalized and regulated under the law. We provide evidence that this was the result of increased perceived costs of employing young children. Further, we show that the law had no statistically significant impacts on the riskiness of child work, hazardous work, or job-related injuries.

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1 Introduction

There is a long-standing debate over the policies to reduce child labor. On the one hand, most national governments and international organizations consider bans as necessary to decrease (and eventually abolish) child labor. By 2018, 171 countries (covering 93% of the world's children) had ratified Convention 138 of the International Labor Organization, the primary goal of which is to establish a minimum age for entry into work and employment (International Labor Organization (2018)). On the other hand, child labor unions and child rights NGOs[1] advocate legalizing child labor. They argue that legal recognition of child work will enable resource-strapped government agencies to focus on implementing regulations intended to make child work safer.

Though many studies examine the effectiveness of bans and minimum working age laws on child labor,[2] very little is known about the effects of other policy alternatives such as recognizing and regulating child labor. In the case of bans, enforcement is centered on ensuring compliance with the minimum working age, usually in the context where existing (illegal) child work is de facto unregulated. In contrast, child labor regulation prioritizes enforcement of protections for working children. The net effect of regulation on the prevalence of child labor is not clear ex ante. Enabling younger children to work subject to regulations can draw more children into the labor force and increase safety at work. However, more regulations may change the relative cost of employing

[1]Working children in Latin American, Africa, and Asia first began to organize the late 1970s and many child labor unions formalized in the early 1990s (Liebel and Invernizzi (2019)).

[2]Theoretical work (starting with Basu and Van (1998)) highlights the possibility that bans may either increase or decrease child labor; empirically, while some recent work has found a Brazilian ban was successful in reducing work for boys but not girls (Piza and Souza (2017, 2016)), others have found that bans are either ineffective (Bargain and Boutin (2021); Edmonds and Shrestha (2012); Boockmann (2010); Moehling (1999)) or even lead to perverse effects legally (Bharadwaj et al. (2020)).

younger children.[3]

In 2014, Bolivia passed one of the first laws in modern history that effectively reduced the minimum working age, from 14 to 10.[4] Specifically, the law allowed children older than 12 to work for others, provided that they acquire a work permit, while simultaneously extending benefits and protections to child workers. For example, the law entitled working children to the adult minimum wages and to 2 paid hours per day to devote to school or study. The law also allowed children between the ages of 10 and 12 to work as own-account (self-employed) workers. To ensure enforcement, the government tasked local child advocacy offices (Defensorias de la Ninez y Adolescencia, DNAs) with issuing working permits and local offices of the Ministry of Labor with adding child labor inspections to their regular labor and workplace inspections.

We use a difference-in-discontinuity approach that exploits the variation in the 2014 law's application to different age groups and accounts for any preexisting discontinuities in outcomes prior to 2014. Thus, we examine differences in work outcomes for children just above and below the set of thresholds issued by the law –at ages 10, 12, and 14– and we study how those differences changed after 2014. Comparing children just above and below the 14-year-old threshold identifies the combined effect of allowing both self-employment and work for others, as children over 14 were permitted to work for themselves and others even prior to 2014. Similarly, comparing children just above and below the 10- and 12-year-old threshold identifies the effect of separately allowing work in self-employment and for others, respectively.

[3]There is evidence that worker entitlements and protections can often have perverse effects on employment. See for instance Besley and Burgess (2004) for evidence from developing countries and Gruber (1994); Lahey (2012); DeLeire (2000); Gruber and Krueger (1991) for evidence from the U.S.

[4]As detailed in Section 2, the law maintained the official minimum working age of 14 but introduced exceptions so that children as young as 10 could work legally.

We estimate the effects of the law on the prevalence of child labor (likelihood and hours of work) and the type of work (self-employment, work for others, allowed and prohibited sectors, and overtime work) using repeated cross-sectional household surveys from 2012 to 2017. The law decreased the likelihood of work for children under 14 (who were newly able to work legally) by nearly 4 percentage points (roughly 18% of the pre-law mean), especially in allowed/regulated tasks and sectors. We find small, negative, and non-significant effects at younger thresholds.[5] Moreover, we find no evidence that the law shifted child labor across allowed and prohibited work, both in terms of self versus external employment and in terms of permitted tasks and sectors. This suggests that the overall null effect does not mask a reallocation of children across types of work in response to the law. We also examine the effects of the law on schooling and household outcomes but find no effects.

To bolster the credibility of our estimating strategy, we show that after the 2018 repeal of the articles in the 2014 law that extended rights and protections to working children under 14, these children became more likely to work and worked more hours. These results are consistent with evidence from India suggesting that banning children's work outright can actually increase in child labor (Bharadwaj et al., 2020).

We then study the impacts of the law on the safety of child work - particularly, work in risky conditions, hazardous work (according to the International Labor Organization definition), and on-the-job injuries. We use two surveys that focused specifically on the characteristics of child work, which enables us to study the effects of child labor legislation on novel dimensions of child

[5] Our estimates are precise, so we can rule out meaningful increases in child work for younger age groups. For example, for 10-year-olds, we can rule out increases in the probability of work larger than 1.1 percentage points and increases in hours of work larger than 0.44 hours in the previous week with 95% confidence.

work rarely explored in economics. We find that the law had no statistically significant impacts on the riskiness of child work, the likelihood of performing hazardous work, or injuries sustained while at work. These findings are important because one of the main arguments for the 2014 law was that legalizing and regulating child work would make it safer, yet we find no evidence consistent with this policy aim. Conversely, we also find no evidence that allowing young children to work increased exposure to harmful or hazardous work conditions.

We show that our results are not driven by standard concerns for difference-in-discontinuity designs, such as manipulation of the running variable, changes in sample composition and balance across age thresholds, bandwidth selection, inclusion of controls, and functional form specifications for the running variable. We also estimate alternative specifications that help to improve precision and illustrate that our results are robust to other identification strategies, including difference-in-difference methods.

We provide suggestive evidence that the decline in work for children under 14 is explained by an increase in the perceived costs of employing young children under the new regulation. First, the law simultaneously increased the resources devoted to inspections and the scrutiny of firms hiring younger children, which led to widespread awareness of the law. Second, we find that the effects of the law were strongest in areas with higher probability of inspection, proxied by the distance to the closest regional offices of the Ministry of Labor, Employment, and Social Protection —the government agency in charge of conducting labor inspections. For the mostly informal firms that tend to employ children, the 2014 law may have increased the perceived threat of general labor inspections, incentivizing firms to remain "under the radar" by not hiring younger children. Indeed, we find suggestive evidence that the law reduced the size of the firms in which children work.[6]

Our results are also consistent with the possibility that the cost and complexity of the process to obtain a work authorization (required under the new law) deterred some children from acquiring a permit, especially children from the most disadvantaged households. Overall, the low take-up of work permits and the increased threat of labor inspections for employers are consistent with the decline in child labor amid the law.

This paper provides novel insights to the literature evaluating the effects of child labor legislation. Previous studies analyzed the effects of child labor bans (Bharadwaj et al., 2020; Piza and Souza, 2016, 2017; Bargain and Boutin, 2021; Edmonds and Shrestha, 2012). In contrast, the unique Bolivian context enables us to study the interaction of the legalization of child work with regulations protecting child workers; this is especially important as these two dimensions may generate opposing effects. We also make two additional contributions relative to the existing literature, including Kamei (2020), who also studies the Bolivian case, albeit with a different empirical approach and a shorter time span.[7] We provide novel evidence on the effects of child labor legislation on job safety, a critical dimension of child work and oft-cited rationale for child labor legislation. In addition, we can exploit variation in both the initial policy change and the later reversal to test whether the effects

[6]For formal firms, the new law may have directly increased the costs of hiring younger children as a consequence of the protections and benefits newly extended to workers under 14 by the 2014 law, such as the right to the adult minimum wage. However, we find small and statistically insignificant increases in wages due to the law, suggesting that this mechanism is unlikely to explain our results, which coincides with the setting of high informality in Bolivia.

[7]Using a difference-in-difference strategy that compares 12-13 year old boys to a control group of boys aged 7-9 and 14-16, Kamei (2020) finds that the probability that 12-13-year-old boys increase work for their families in 2014 relative to the pre-law period. We study the impact of the law over a longer horizon (4 years), across different outcomes (namely, job safety), and with a different empirical strategy; we also evaluate the policy reversal. We discuss Kamei (2020) in more detail in Section 5.1.1.

of this highly scrutinized legislation are a function of its enforcement or of longer-term changes in behavior that persist despite its reversal.

Importantly, our findings also highlight an important facet of child labor legislation: its impact on employers. Child labor laws rarely address what many regard as a root cause of child labor: poverty (Basu and Van (1998); Edmonds and Schady (2012); Edmonds and Pavcnik (2005); Edmonds (2005)). Instead, bans and other regulations more often impact the demand for child work by altering the costs of child workers. Imperfectly enforced bans can impose costs associated with hiring children, which can then be passed through to children in the form of lower wages (Bharadwaj et al. (2020)). In our context, recognizing and regulating child labor appears to have increased the perceived risk of labor inspections and thus the cost of hiring child workers. This aligns with a long line of existing work that finds that mandated benefits and worker protections can have adverse consequences for workers, particularly along the lines of employment (for a review of the effects of protections and mandated benefits in developing countries, see Freeman (2010)). Thus, regulation — whether in the form of worker protections or in outright bans — increases the cost of hiring children, which ultimately affects child work in ways that can contradict the policymakers' intentions.

Finally, our findings also relate to the literature on regulating illicit markets. A common argument for legalizing and regulating illicit markets is that it should decrease the risk and improve the safety of engaging in the market. Our results are consistent with evidence in the context of sex workers showing that regulation can often lead to zero or even unintended consequences (Manian (2021); Gertler and Shah (2011); Ito et al. (2018); Cameron et al. (2021)). Our contribution is to unravel evidence from one of the most vulnerable groups of workers: children.

2 Child labor legislation in Bolivia

Child work is relatively common in Bolivia. From 2012 to 2013, roughly one in five children between the age of 10 and 14 worked despite being younger than the minimum working age of 14 years old.[8] The conditions under which children work are also striking. Based on the 2008 Survey of Child Work (Encuesta Nacional sobre Trabajo Infantil, ENTI), more than 65% of child workers worked in occupations that are classified as hazardous by the International Labor Organization and more than one third of working children reported suffering an injury at work. These dramatic patterns were similar even among the 16.5% of children who work for their families.[9] In comparison, roughly half of working children are engaged in hazardous work worldwide (International Labour Organization, 2021).

Despite consensus on the importance of protecting the integrity of children, Bolivia has experienced important tensions between policymakers and working children themselves. Setting and enforcing minimum working age requirements that align with compulsory schooling ages are popular policy guidelines recommended by international organizations. However, these policies are often criticized as being at odds with the reality of child work; many argue that child work is often necessary in the face of poverty and that policy should instead focus on regulating child work to ensure safe working conditions and the protection of child rights. In Bolivia, grassroots organizations such as the National Union of Working Children's (*Union Nacional de Niños, Niñas y Adolescentes Trabajadores de Bolivia*, UNATSBO) have been at the forefront

[8] Authors' calculations of weighted means based on the 2012-2013 Encuesta de Hogares. This definition does not include participation in household chores.

[9] Specifically, 63% of children working for their families are engaged in hazardous work while 31% reported suffering an injury at work. Authors' calculations using the 2008 ENTI.

of such policy suggestions, demanding the recognition of labor as an integral and unavoidable part of children's development.[10]

This tension is empirically salient: 66.5% of working children report working to support their families[11] and both child labor and the incidence of injuries at work are strongly correlated with household per-capita income (see Appendix Figure A.1), suggesting that children from the poorest households are the most likley to work and also the most exposed to worplace hazards. In part as a response to this tension, the Child and Adolescents Code of 2014 was implemented to legally recognize some forms of child labor and thus guarantee protections to working children. We describe the main changes induced by the law in the following sections 2.1 and 2.2.

2.1 Child labor legislation prior to 2014

Before 2014, two laws regulated the engagement of children in labor markets: the Child and Adolescents Code (law 206 of 1999), which provided general guidelines about the rights of youths, and the General Labor Law (law 224 of 1943), which regulates overall participation in labor markets.

Title VI of the 1999 Child and Adolescents Code describes the legal framework related to the protection of working children. There are three important dimensions for our analysis. First, the code set a minimum working age of 14 years old (Article 126). Second, the 1999 code put forth regulations for working children between the age of 14 to 18 but did not specify protections for younger children. Third, the code established that the work of adolescents (14 years and older) was regulated by the General Labor Law of 1953. Thus,

[10] See Chapter 4 in Unión de Niños Niñas y Adolescentes Trabajadores de Bolivia (2010).

[11] Calculated using the main reported reason for working among working children 7-17 years of age. Most of the remaining children (22%) report working to acquire skills. Authors' calculations using the 2008 ETI.

working adolescents were entitled with the same rights and obligations of adult workers. Specifically, working children were to be paid at least the adult minimum wage and they were to be enrolled in the social security system by their employers. In addition, the 1999 code mandated that employers or parents (in the case of family businesses) offer flexible schedules to working adolescents so that they could attend school and that daily shifts not exceed 8 hours (not more than 40 hours per week). The 1999 code also prohibited child work in occupations deemed hazardous and those that potentially compromised the dignity of working children.[12]

2.2 The 2014 law

Law No. 548 of 2014 addressed the general welfare and rights of children and expanded workplace protections to younger children. Specifically, it stated that its objective was "... to recognize, develop, and regulate the exercise of child and adolescent rights ..." (Article 1). Under these broad objectives, the new law changed preexisting child labor regulations in two core dimensions: exceptions that lowered the de facto minimum working age and expansions of worker protections to younger workers.

Appendix Table A.1 summarizes the key changes induced by the law for each age group. The new law confirmed the minimum working age of 14 years, but it also introduced exceptions that allowed children aged 10 to 13 years to work legally, subject to additional restrictions. Before 2014, no children younger than 14 were allowed to work legally. Under the new law, children aged 10 to 11 were allowed to work as self-employed (own-account) workers, while children aged 12 to 13 were permitted work as both self-employed workers

[12] Appendix Section A provides a list of all forbidden activities under Articles 134-135 of Title VI of the 1999 code.

and to work for others. For both age groups, children were required to obtain work authorizations from local child protection offices (Defensoría de la Niñez y Adolescencia). This authorization required parental consent and a medical examination of applicants.

By recognizing the work of younger children, the new law also charged the government with regulating work and establishing protections for younger working children that were not accounted for in the previous law. The law explicitly stated "The State at all levels will guarantee the exercise or work performance of adolescents over fourteen (14) years of age, with the same rights enjoyed by adult workers. The protection and guarantees for working adolescents over fourteen (14) years of age is extended to adolescents under fourteen (14) years of age" (Law 548, Article 130).[13] Thus, beginning in 2014, working children aged 12 and 13 were entitled to the same benefits and entitlements of adult workers, such as minimum wages and social security. Additionally, the 2014 law required that employers give child employees (age 12 to 17) flexible schedules and at least two paid hours per day to perform their schooling obligations.[14] It also set a maximum of 30 hours of work per week (6 hours per day) for children between 10 and 14 years old. As was the case prior to 2014, children 14 to 18 years old were allowed to work up to 40 hours per week, with a maximum of 8 hours per day. Finally, the list of prohibited tasks and jobs was updated to include agricultural work occurring outside of family and communal work.

[13]Authors' translation of original document in Spanish.

[14]In the case of self-employed children, the 2014 law required that parents ensure that children can attend school even while working.

2.3 Enforcement and awareness

The law tasked the regional offices of the Ministry of Labor and Social Protection (Ministerio de Trabajo, Empleo y Proteccion Social, MTEPS) with carrying out inspections and permanent supervision of workplaces to ensure that employers were complying with the regulations under the law (Article 139).[15] If any party were found to be in violation of the rights and protections under the law, the MTEPS would turn the case over to the Defensoría de la Niñez y Adolescencia (DNA) for legal restitution. Under the 2014 Law, the DNA was allowed to impose penalties such as warnings and reprimands, fines, the removal of children from work, and temporary suspension of business activities.[16] Parents in violation of the code (for example, as employers of their children in family work, but also as guardians of their children more broadly) were also subject to measures, ranging from warnings, to required attendance of courses and programs, to (at the extreme) separation from their children. In the case of repeat offenders, the DNA had the authority to send the proceedings into criminal court.

It is worth noting that under the law, the local DNAs were primarily responsible for processing child work permits and following up any violations brought to light by MTEPS inspections. On the other hand, the responsibility of inspecting workplaces was given to the regional MTEPS offices, which were already in charge of conducting general labor and technical inspections to firms and inspections related to preventing forced labor.[17] This distinction

[15] Article 46 of Executive Order 2377 provides implementation rules related to inspections.

[16] As stated in Article 169 of Law 548 and Article 219 of the 1999 code.

[17] Labor inspections verify the compliance with national regulations, including being part of the mandatory employer registry (Registro Obligatorio de Empleadores), contributions to social security and health insurance, and the compliance with worker protections established in the Labor Law. Technical inspections verify that work facilities comply with safety and sanitary standards.

is important for understanding potential behavioral responses to the new law. The DNAs offer children the opportunity to apply for work authorizations which may increase the supply of child labor. In contrast, the threat of a inspection by the MTEPS is likely to affect employers compliance with the newer regulations and their demand for child labor. Employers may increase worker protections to avoid sanctions or reduce the demand for younger child workers as they become relatively more expensive to hire legally.

In the case of informal firms[18], the threat of inspection may operate through an additional channel: firms may decide to employ fewer young children in order to avoid being inspected by the Ministry of Labor and continue operating informally; indeed, one of the purposes of MTEPS's labor inspections is to verify firm registration. A recent survey of Bolivian firms found that the overwhelming majority of firms — even among small and micro-enterprises — perceived costs associated with labor regulations as directly influencing their hiring decisions, suggesting that there is an advantage to remaining "under the radar" of labor inspectors (Muriel and Ferrufino, 2012).[19] Relatedly, prior work has found that firms tend to resist formalization, even when provided information about the registration process and when registration fees are waived, but that firms respond to the increased likelihood of inspections (De Andrade et al., 2016).

There are 25 regional Ministry of Labor and Social Protection offices located in the most populated municipalities of the country.[20] Annual reports from the MTEPS show that child labor inspections increased sharply in 2014

[18]Informal firms account for almost 80% of employment and 62% of GDP in Bolivia (Elgin et al., 2021).

[19]This behavioral response of firms to regulation has been discussed in other settings (see for example, Hsieh and Olken (2014); Tybout (2014)).

[20]The location of MTEPS offices is displayed in Appendix Figure A.2.

and rose thereafter, possibly reflecting an increase in resources devoted to enforcing the 2014 law; the number of dedicated child labor inspectors rises sharply in 2014 (Appendix Figures A.3 and A.4). There were on average around 300 child labor-specific inspections per year conducted during the period following the law's enactment; in 2018, 17% of such inspections were turned over to the DNAs for resolution (Ministerio de Trabajo, Empleo y Previsión Social, 2018). The total number of inspections (labor and technical) conducted by the MTEPS also increased after 2014 (Appendix Figure A.3), suggesting that the increase in child labor inspections did not crowd out other inspections conducted by the MTEPS.

The initial enactment of the law was very controversial and highly scrutinized by NGOs, international organizations, and authorities. Several press articles highlight the public support of the legislation by the then-president (Pagina Siete, 2013; Los Tiempos, 2013), which may have amplified awareness about the policy change.[21] In Appendix Figure A.5 we track articles that mention the 2014 law over time across national and regional Bolivian newspapers. There are clear spikes in the number of published articles around the time that the initial 2014 law was implemented and in the years in which the law amendment was announced and eventually implemented, suggesting that the general public was aware of the policy changes. We also observe coverage of the law in the intervening years – particularly in 2016 and 2017 – indicating that the issue continued to be relevant throughout the period. In addition, the enactment of the 2014 law was coupled with workshops on worker's rights and protections, delivered by the MTEPS and targeted to employers and children.

[21] There is a growing literature documenting how information provided by political leaders can modify citizen's attitudes and behavior through different media (Ajzenman et al., 2020; Pedemonte, 2020; Jetter and Molina, 2022).

Over 11,000 workers and employers attended to these child labor workshops between 2015 and 2018, according to MTEPS Annual Reports (Ministerio de Trabajo, Empleo y Previsión Social, 2018).

2.4 Amendments to the 2014 law

Amid intense debate and scrutiny, some key articles of the law — namely those granting children below the age of 14 the ability to work legally and benefit from the same protections and guarantees as older workers — were reversed in 2018. The 2018 amendment to Article 130 explicitly states the government's duty to ensure the rights of workers between the ages of 14 to 18 years old and does not establish rights of younger working children, in contrast to the 2014 law. Additionally, the government repealed paragraph IV of Article 132, which regulated weekly work hours for children between 10 and 14 years old. We interpret the 2018 amendment as an abrupt decrease in the enforcement of worker protections for younger children and analyze the effects of this amendment in Section 5.1. In Appendix Figure A.5, we observe spikes in press coverage of the law around the time the amendment was announced and when it was ultimately passed, suggesting that there was broad awareness of the reversal.

3 Data

To measure the effects of the policy change on employment and work hours, we leverage data corresponding to 8 waves of Bolivia's annual household surveys (*Encuesta de Hogares*, henceforth referred to as the household data). Each survey wave contains data from a nationally representative sample of households in Bolivia. We pool survey waves to construct a repeated cross-section covering two years before the policy change (2012 and 2013), 4 post-law years (2014-2017), and 2 post-reversal years (2018-2019). We exclude data preceding 2012 to minimize the potential effects of the rollout of Bolivia's conditional cash transfer (CCT) program targeted at school-age children.[22]

Economic activity is measured by an indicator of whether a child worked at least one hour during the week preceding the interview.[23] We also compute weekly work hours and construct an indicator for overtime work (defined by the 2014 law as working more than 30 hours for children under 14). Further, we separately measure work for self, work for others, employment in activities that are prohibited under the law for all children under age 18 (such as mining), and participation in allowed activities.[24] Self-employment is somewhat rare; less than 2% of working children worked for themselves prior to the 2014 law (see Appendix Table A.2). Work for others is largely made up of work for a family employer. However, work for a family employer and work for an external employer are similar along many critical dimensions. For example, most employers operate informal firms, regardless of whether they are family operated (97% informal) or not (90% informal); the median firm size (4 workers) is the same across family employers and non-family employers; virtually all jobs are performed outside the household (97%) even in family-operated firms; and children's exposure to risk and injury are high in both work for family (53% exposed to risk, 31% injured) and work for employers (65% exposed to risk,

[22]The *Bono Juancito Pinto* program was initially delivered to children enrolled in grades 1 to 5 in 2006 and expanded to include children in 8th grade in 2009. In 2012, it was announced that children in 9th grade would also be covered. See Vera-Cossio (2021) for details about the policy. We discuss a further expansion of the program to older children in Section 4.

[23]This definition does not include unpaid participation in household chores.

[24]See Appendix Sections B and C for a full list of prohibited activities and more detailed variable definitions.

41% injured).[25] There are some differences: family work is largely driven by agriculture and retail, while work for others is more diversified, although still dominated by retail and agriculture.

As discussed in Section 2.2, exposure to different dimensions of the 2014 law is a function of age. Our dataset includes the exact birth date of each household member, which enables us to calculate age at the time of the survey. We compute the number of months elapsed between a child's birth date and the month in which fieldwork of each survey started; typically, November of each year. We then normalize age in months relative to the cutoff of interest—age 10, 12 and 14. For survey waves 2013, 2014 and 2016, the (household-specific) exact date of survey interview is also available. Thus, we are able to compute age in days based on the exact birth and interview dates of each child. We report robustness analyses using this more granular measure of age in days in Section 5.1.1.

To measure the impacts of the law on the nature of child work, we leverage detailed information on risky tasks, hazardous work, and injuries at work from the 2016 Survey of Children and Adolescents (Encuesta Niño, Niña y Adolescente, ENNA) and the 2008 survey on working children (Encuesta Nacional sobre Trabajo Infantil (ENTI) 2008). The sampling frame differs across the two surveys; while the 2016 ENNA is nationally representative, the 2008 ENTI focuses on children who are likely to work. Therefore, in order to pool the two datasets, we reweight the observations in each survey.[26]

We measure engagement in risky activities as an indicator capturing whether the activities performed by working children involved working in conditions

[25] Authors' calculations using the sample of working children age 9-15 from the 2008 ENTI. Observations are weighted using the procedure described later in Section 4.

[26] We discuss this reweighting method in more detail in Section 4.

that may compromise their health or physical integrity, such as working under extreme temperatures or working in an area exposed to fire, flames, or contaminated dirt and dust. Similarly, we construct an indicator of whether a child reports having experienced any job-related injuries in the past year, such as skin injuries, fractures, or respiratory complications. Finally, we compute engagement in hazardous occupations following the International Labor Organization's definition of hazardous work (International Labor Organization (2011)). Specifically, we create an indicator of whether a child reports performing any work involving risky tasks (as defined above), heavy lifting, use of heavy equipment, night shifts, or mining. See Appendix Section C for more detailed variable descriptions.

Appendix Figure A.6 illustrates that the gradient in work probability is very steep; for example, 17-year-olds are more than twice as likely to work as 10 year-olds. This steep gradient highlights the potentially stark differences in the labor supply of older and younger children and motivates our empirical strategy, which focuses on local comparisons around the age thresholds specified by the 2014 law. Among working children, the average number of weekly work hours is 24.8 and over 27% of working children worked more than 30 hours per week. As mentioned in Section 2, child work in Bolivia is dangerous for many children; roughly 56 percent of working children are engaged in risky activities and 34% of working children report having experienced a job-related injury in 2008. These and other summary statistics for our sample are displayed in Appendix Table A.2.

4 Empirical approach

4.1 Identification

To identify the causal effects of the exposure to the law, we exploit two sources of variation. First, the 2014 law applied differently to children depending on their age. Whether and which type of jobs children were allowed to work changed discontinuously at three age thresholds: 10, 12, and 14. Thus, our empirical design compares the work outcomes of children who – based on their age as of data collection – just became eligible to work to the outcomes of children who were only months away from being eligible under the law. Second, we use data spanning both the pre- and post-law periods (before and after 2014) to net out any preexisting discontinuities under laws prior to 2014, which dictated a minimum working age of 14.

We combine these sources of variation and in a difference-in-discontinuity approach. In particular, we locally estimate differences in work outcomes on either side of the age cutoffs imposed by the 2014 law and remove any preexisting differences prior to 2014. As there is a steep gradient between work and child age (see Appendix Figure A.6), these local comparisons help minimize concerns about time-varying shocks that differentially affect work outcomes by age that would invalidate standard difference-in-difference strategies.

More formally, we model the effect of being exposed to the law on outcome $Y_{i,t}$ corresponding to child i observed in survey wave t as:

$$\begin{aligned} Y_{i,t} =& \beta_0 + \beta_1 T_i \times Post_t + \beta_2 T_i + \beta_3 Post_t + \theta_1 (Age_{i,t} - c) \\ &+ \theta_2 T_i \times (Age_{i,t} - c) + \gamma x_{i,t} + \epsilon_{i,t} \end{aligned} \tag{1}$$

where $Age_{i,t}$ is the age of child i in months at the beginning of the relevant recall period (which differs by outcome) for survey wave t[27]; and c is the relevant cutoff age related to the key policy changes induced by the new law (at ages 10, 12, and 14). $\epsilon_{i,t}$ is an error term.

T_i is an indicator of whether child i is exposed to the policy change associated to each cutoff. For the 10- and 12-year-old cutoff, it is defined as an indicator of whether a child is 10 years old or older and 12 years old or older, respectively. We define the treatment indicators in this way because at the age 10 threshold, the 2014 law grants children just above the threshold the ability to work legally as self-employed and, at the age 12 threshold, the 2014 law further allows them to work for others. In the case of the cutoff at 14 years old, exposure to the law (T_i) is an indicator of whether a child is *younger* than 14 years old. This is because the 2014 law newly allowed children under age 14 to work; children aged 14 and older were legally allowed to work even under the preexisting law. Defining the treatment indicators in this way makes the interpretation of T_i consistent across all thresholds, in that all treated children have newly expanded working rights under the 2014 law relative to control children. For all thresholds, the parameter of interest, β_1, captures the effect of being exposed to each dimension of the law, i.e., the changes in work outcomes for those just on the treated side of the cutoff, relative to those just on the control side.

$x_{i,t}$ is a vector of demographic household and child characteristics that are unlikely to vary due to the program. These include household head characteristics such as schooling, gender, age, and ethnicity; household characteristics such as number of children in the household in following age categories: 0-

[27]For example, the recall period for employment is the week prior to the survey, so $Age_{i,t}$ reflects the age of the child at the beginning of the prior week when considering employment outcomes.

6, 7-9, 10-13, and 14-17 and number of adult men and women; whether the household is located in an urban area; the child's gender; and departamento-by-year fixed effects.[28] The coverage of Bolivia's flagship CCT program was expanded in 2014 to include children enrolled in grades 9 to 12 (regardless of age). Given that some children in grade 9 are 14 years old, we also control for grade-for-age fixed effects and their interactions with a post-2014 indicator when we estimate equation 1 for the 14-year-old cutoff. This helps account for the potential impacts of the CCT on child labor that may also differ for children above and below age 14.[29]

We use a linear specification of the running variable around the cutoff and allow for different slopes on either side of the cutoff. We show that our results are unchanged when we instead use a second-order polynomial and when we allow the slopes to vary before and after the policy change in Section 5.1.1. We estimate equation (1) using triangular kernels that assign a higher weight to observations closer to the eligibility cutoff and conduct inference using standard errors clustered at the household level to account for correlated error terms across siblings.

Our preferred specification uses a 12-month bandwidth on either side of the cutoff. Doing avoids classifying observations as part of the treatment group when we analyze one cutoff and as part of the control group in a different cutoff. For example, a child who is 11.5 years old would be in the treatment group relative to the 10-year-old cutoff, but the same child would be in the

[28]We include covariates to increase precision, though we show that our results are robust to specifications without controls in Section 5.1.1. Departamento is an administrative/geographic unit roughly comparable to a U.S. state.

[29]Controlling for CCT exposure is not necessary for younger children (those around the 10- and 12-year-old cutoffs) because by 2009 all children in these age groups were eligible to receive the CCT (regardless of being above or below the thresholds defined in the 2014 law).

control group relative to the 12-year-old cutoff. Thus, we compare 9-year-old to 10-year-old children around the 10-year-old cutoff, 11-year-old to 12-year-old children around the 12-year-old cutoff, and 13- to 14-year-old children around the 14-year-old cutoff[30]. In Section 5.1.1, we show that our results are robust to using narrower and wider bandwidths.

Finally, to estimate the impacts of the enactment of the law, we estimate equation (1) using 2 pre-law (2012-2013) and 4 post-law survey waves (2014-2017). To estimate the impacts of the reversal of key regulations, we use the 4 post-law survey waves as pre-reversal period and the 2 post-reversal waves (2018-2019) as the post-reversal period.

4.2 Pooling the 2016 ENNA and 2008 ENTI surveys

To estimate the causal impact of the 2014 law on the nature of child work, we turn to the 2016 ENNA and 2008 ENTI data. Because the 2016 ENNA is nationally representative whereas the 2008 ENTI samples children likely to work, we combine the two surveys by reweighting the data. To calculate the weights, we pool the observations from a randomly chosen 70% subsample from each survey and then predict the likelihood of appearing in the 2016 nationally representative ENNA using a Probit model based on demographic characteristics of children and their households. We then use these predicted probabilities to construct weights so that observations that are similar (based

[30]The mean squared error (MSE) optimal bandwidth proposed by Imbens and Kalyanaraman (2012) ranges from 13 to 25 months for all our main outcomes. As explained above, these bandwidths are too wide for our context because they would yield overlapping treatment and control groups. Since the bandwidth we selected is more narrow than the MSE optimal bandwidth, the choice of a 12-month bandwidth is not inducing bias in our estimates though it affects the power of our regressions. We show in Section 5.1.1, that our results are robust to widening the bandwidth.

on observables) across survey waves are given higher weight.[31] In Table A.3, we show balance on these characteristics across the age thresholds and survey rounds (after reweighting) for the remaining 30% random subsamples that were not used in calculating the weights.[32] In estimating equation 1, we use combined weights that reflect both the triangular weights and the constructed sampling weights. For the pre-period (2008), we divide the triangular kernel weights by one minus the inverse probability of being in the post sample in 2016. For the post-period (2016), we divide the triangular kernel weights by the inverse probability of being in the post sample in 2016.

4.3 Threats to identification

Manipulation. The validity of our empirical design requires that individuals cannot perfectly manipulate the assignment variable, which in our setting is the age (in months) at the time of data collection. There are two reasons why manipulation is unlikely. First, we study the impact of a law using data that is regularly collected by the government and which was not designed or framed as tool to measure the impacts of the law; ex ante there was no incentive to manipulate child age in order to appear compliant in our analysis. Second, even though age heaping is common, interviewees are asked for the birth date of each household member as opposed to their age.

As we rely on self-reported data, a similar threat to validity is that becoming eligible to work under the law may have caused differential survey response

[31]Specifically, observations from the 2016 survey receive a weight of $\frac{1}{p}$, where p is the predicted probability of being in the 2016 survey. Observations from the 2008 survey receive a weight of $\frac{1}{1-p}$. This reweighting procedure is similar in spirit to the one proposed in Abadie (2005), which aims to minimize bias and maximize balance across the samples.

[32]We follow this approach to ensure that balance on targeted variables is not simply a consequence of overfitting.

rates of children around each cutoff. Appendix Figure A.7 reports the distribution of observations around the cutoffs, focusing on children with birth dates within a year of each cutoffs (the bandwidth of our baseline specifications). It shows no evidence of discontinuous changes at the cutoff; this is corroborated when we conduct the McCrary (2008) test for manipulation in the pre- and post-2014 periods (see Appendix Table A.4). We discuss additional checks for measurement error in Section 5.1.1.

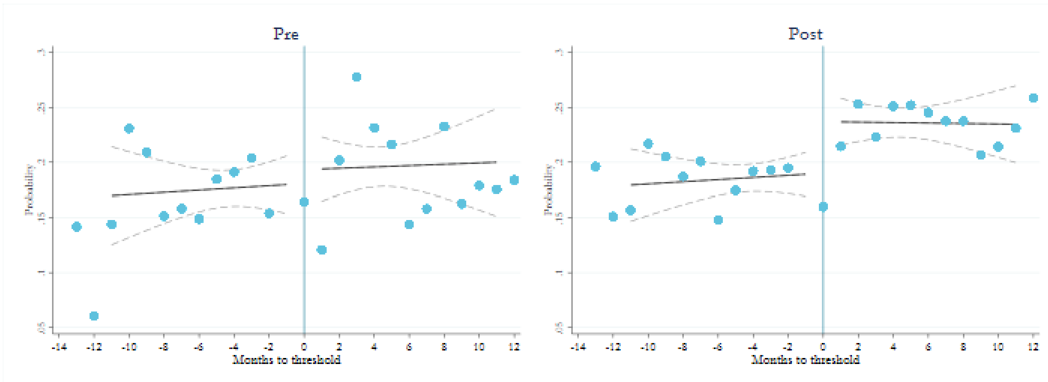
Changes in sample composition and balance. We test for changes in demographic characteristics around the cutoff before and after the policy change. For this, we estimate (1) using demographic characteristics as dependent variables. Appendix Table A.5 shows that, at a 5% significance level, there are no differences across each cutoff. While 2 out of 18 differences are significant at 10% level for the household data, these differences do not reflect a systematic pattern across cutoffs. In addition, for each cutoff, we are unable to reject the null hypothesis that the coefficients in each column are jointly zero. There is no evidence of differential changes in sample composition across any of the age cutoffs in the child labor survey (Appendix Table A.6).

5 Effects of the 2014 Law

5.1 Effects of the law on the prevalence and sector of child work

We begin by discussing graphical evidence of the impacts of the law, which illustrate the intuition behind our regression-based estimates below. We focus on the impacts around the 14-year-old threshold, which speak to the combined effects of regulating self-employment and work for others, because there is a

Figure 1: Work Probability: 14-Year-Old Cutoff



The running variable is the difference between age in months and the age cutoff a week before the survey date. The pre sample includes 2012-2013 and the post sample includes 2014-2017. We use a triangular kernel.

substantially higher rate of working children around this cutoff.

Figure 1 plots work probabilities as a function of age (in months) relative to the 14-year-old cutoff, both before and after the 2014 law change. Recall that under both the new and preexisting laws, the work of children aged 14 or older was regulated. The new law enabled younger children to work but also provided a number of requirements related to worker protections. During the pre-period, there does not seem to be a discontinuous change on work outcomes around the cutoff. This suggests that the pre-existing minimum working age was not a binding constraint to child labor. However, there appears to be a discontinuous change around the cutoff after the policy change. Relative to 14-year-old children (who were allowed to work before and after the policy change), marginally younger children were less likely to work after 2014.

Figures A.8 and A.9 provide a similar comparison around the 12- and 10-year-old cutoffs. Again, prior to the 2014 law, neither age group was allowed to work. As expected, we find no discontinuity in work probabilities prior to the 2014 change (left panels). The new law enabled both 11- and 12-year-old children to work; however, only those 12 or older could work for others, subject to obtaining a work permit. After the policy change, there is no evidence of a discontinuous jump in the work probabilities at the 12-year-old cutoff in Figure A.8, suggesting no effects of the law around the 12-year-old cutoff (right panel). Likewise, in Figure A.9, there does not seem to be a discontinuity around the 10-year-old cutoff, either before or after the law, suggesting that the regulations preventing younger children age 10 years old or older to work as own-account workers (i.e., self-employed or independent contractors) were non-binding constraints.

We now turn to the regression-based evidence, using the difference-in-discontinuity strategy outlined in Section 4. Panel A of Table 1 reports the effect of the law on work outcomes around the 14-year-old cutoff. Relative to 14-year-olds (whose work was previously regulated), the 2014 law granted 13-year-olds the ability to work legally, both independently and for an outside employer. However, we find that the probability of work *declines* by 3.94 percentage points for 13-year-old children (an 18% decline relative to 14-year-old children; see column 1). Hours of work fall by about an hour per day, averaged across all children, including non-workers.

These effects appear to be driven by a decrease in the probability of work for others (3.9 percentage points, statistically significant at the 5% level; see column 4) as opposed to self-employment (0.2 percentage points, not statistically significant; see column 3). The decline in work is particularly pronounced in occupations that are legally allowed and regulated under the 2014 law (4.41 percentage points, statistically significant at the 5% level; see column 6). This decline does not coincide with a corresponding increase in work in prohibited occupations (column 5), suggesting that there was no reallocation of child labor across types of work. There are no statistically significant effects of the law on 13-year-old children's overtime work (column 7).

Table 1: Effects of the Law on the Work Probabilities, Hours, and Occupation

Panel A: 14-Year-Old Cutoff

	Any Work (1)	Hours Worked (2)	Work for Self (3)	Work for Others (4)	Prohibited Work (5)	Allowed Work (6)	Works more than 30 hrs. (7)
Post × $\mathbb{1}\{\text{Age} < 14\}$	-0.0394	-0.977	-0.00216	-0.0372	0.00468	-0.0441	-0.00429
	(0.0177)	(0.572)	(0.00466)	(0.0174)	(0.00550)	(0.0174)	(0.0111)
Obs.	9046	9046	9046	9046	9046	9046	9046
Mean	0.221	5.356	0.00838	0.213	0.0174	0.204	0.0576

Panel B: 12-Year-Old Cutoff

	Any Work (1)	Hours Worked (2)	Work for Self (3)	Work for Others (4)	Prohibited Work (5)	Allowed Work (6)	Works more than 30 hrs. (7)
Post × $\mathbb{1}\{\text{Age} \geq 12\}$	-0.0140	-0.351	-0.00128	-0.0128	-0.00388	-0.0102	0.00152
	(0.0161)	(0.407)	(0.00285)	(0.0160)	(0.00355)	(0.0159)	(0.00741)
Obs.	8731	8731	8731	8731	8731	8731	8731
Mean	0.151	2.826	0.00315	0.148	0.00533	0.146	0.0201

Panel C: 10-Year-Old Cutoff

	Any Work (1)	Hours Worked (2)	Work for Self (3)	Work for Others (4)	Prohibited Work (5)	Allowed Work (6)	Works more than 30 hrs. (7)
Post × $\mathbb{1}\{\text{Age} \geq 10\}$	-0.0169	-0.206	0.00151	-0.0184	-0.00262	-0.0143	0.00562
	(0.0144)	(0.331)	(0.00157)	(0.0143)	(0.00218)	(0.0143)	(0.00587)
Obs.	8636	8636	8636	8636	8636	8636	8636
Mean	0.0992	1.570	0.00145	0.0977	0.00194	0.0972	0.0104

Household-level clustered standard errors in parentheses. Control variables: CCT eligibility indicator (Panel C only), household head characteristics (schooling, gender, age, and indigenous indicator), gender, number of children in the following age categories: 0-6, 7-9, 10-13, and 14-17, number of adult men and women, an urban dummy, and departamento by year fixed effects. We also include linear splines of the running variable, defined as the difference between the cutoff age and age a week before the survey date in months. We use a bandwidth of 12 months and a triangular kernel. Survey years: 2012-2017.

Panels B and C of Table 1 corroborate the results from the graphical evidence for younger children; that the law had no statistically discernible effect on the work of 12- and 10-year-old children, respectively. Panel B shows that the point estimate of the effect on the likelihood of work for 12-year-old children (column 1) is negative, though not significant at conventional levels; we are able to rule out increases in work that are at least 1.8 percentage points (and 0.45 hours of work; column 2) with 95% confidence. Similarly, in Panel C we can rule out an increase in the probability of work larger than 1.1 percentage points (0.44 hours of work) with 95% confidence for 10-year-olds. We also find that the law does not lead to any changes in the type of work that 10- and 12-year-olds engage in, either in terms of sector of work (allowed versus prohibited), overtime work, self-employment or work for others.

We validate our empirical approach by exploiting the reversal of key protections that the 2014 law—namely those regulating the work of children under the age of 14—as a source of exogenous variation in the enforcement of the law's protections for younger working children. Table 2 reports coefficients from equation (1) using the 2014-2017 rounds of the household survey data as the pre-reversal period and the 2018 and 2019 rounds as the post-reversal period.[33] The results mirror-image those from our analysis of the effects of the introduction of the law. We find that 13-year-olds increase their work probabilities and hours worked when the labor regulations and protections for children under 13 are lifted by 3.52 percentage points and 1.65 hours, respectively (columns 1 and 2). Interestingly, these estimates are similar in magnitude to those found in Bharadwaj et al. (2020), who study the effects of a child labor ban in India.[34] The fact that the effects of the law disappeared after

[33]The government announced the reversal of the law in mid-2018 and the household surveys are conducted at the end of the year, so we consider 2018 as a post-reversal year.

[34]We find a 21% increase in the probability of working for those under 14 relative to the pre-reversal average work probability for 13-year-olds. Bharadwaj et al. (2020) find that the ban results in a 22% increase in work for children under 14 relative to the pre-ban mean. Studies of a Brazilian law that increased the legal working age from 14 to 16 found no effects (Bargain and Boutin, 2021) or declines in child work (Piza and Souza, 2017).

the reversal provides support to our identification strategy. We also find that the reversal lead to increases in work for 12-year-olds relative to 11-year-olds (Panel B); these also mirror the negative (though not statistically significant) negative effects of the initial implementation of the law in Table 1. The work of 10-year-olds remains unaffected by changes in legislation (Panel C).

We also examine the impact of the law on schooling but find no statistically significant effects (see In Appendix Table A.7).[35] One explanation is that the school day in Bolivia is limited to 4 hours which allows children to combine work and schooling; this aligns with the observation that the overwhelming majority of children in the sample attend school (for example, 93.7% of 13-year-olds attend school). Thus, even if the law had decreased child work (as our results around the 14-year-old cutoff suggest), we expect to find little impacts on school attendance. We also find that the 2014 law had no significant effects on the labor supply of other household members or on other measures of household wellbeing (see Appendix Table A.8).

5.1.1 Robustness

We show that our results are robust to alternative specifications. Our main results on work probabilities are based on estimates of equation (1) using a twelve-month bandwidth around each cutoff. Columns 1 and 3 of Appendix Table A.9 shows that the results are unchanged when we expand the estimation bandwidth to 24 months and when we reduce the bandwidth to six months, albeit with a substantial decline in precision. In addition, columns 4-7 show that the results are robust to excluding demographic controls from our main

[35] Since 2009, schooling has been compulsory for all primary and secondary levels, and free in public schools. Thus, our estimates do not confound any changes in compulsory schooling laws.

Table 2: Effects of the Law Reversal on the Work Probabilities, Hours, and Occupation

Panel A: 14-Year-Old Cutoff

	Any Work (1)	Hours Worked (2)	Work for Self (3)	Work for Others (4)	Prohibited Work (5)	Allowed Work (6)	Works more than 30 hrs. (7)
Post × $\mathbb{1}\{\text{Age} < 14\}$	0.0383	1.469	0.00145	0.0369	0.0136	0.0248	0.0221
	(0.0168)	(0.462)	(0.00437)	(0.0165)	(0.0106)	(0.0157)	(0.00856)
Obs.	8930	8930	8930	8930	8930	8930	8930
Mean	0.216	4.871	0.00892	0.207	0.0371	0.179	0.0477

Panel B: 12-Year-Old Cutoff

	Any Work (1)	Hours Worked (2)	Work for Self (3)	Work for Others (4)	Prohibited Work (5)	Allowed Work (6)	Works more than 30 hrs. (7)
Post × $\mathbb{1}\{\text{Age} \geq 12\}$	0.0279	0.557	-0.00393	0.0318	0.00548	0.0224	-0.000283
	(0.0154)	(0.358)	(0.00277)	(0.0153)	(0.00873)	(0.0140)	(0.00572)
Obs.	8853	8853	8853	8853	8853	8853	8853
Mean	0.140	2.384	0.00356	0.136	0.0224	0.117	0.0142

Panel C: 10-Year-Old Cutoff

	Any Work (1)	Hours Worked (2)	Work for Self (3)	Work for Others (4)	Prohibited Work (5)	Allowed Work (6)	Works more than 30 hrs. (7)
Post × $\mathbb{1}\{\text{Age} \geq 10\}$	0.00420	-0.110	-0.00104	0.00524	0.00364	0.000567	-0.00371
	(0.0131)	(0.247)	(0.00189)	(0.0130)	(0.00798)	(0.0116)	(0.00378)
Obs.	9127	9127	9127	9127	9127	9127	9127
Mean	0.0890	1.286	0.00125	0.0878	0.0135	0.0755	0.00723

Household-level clustered standard errors in parentheses. Control variables: CCT eligibility indicator (Panel C only), household head characteristics (schooling, gender, age, and indigenous indicator), gender, number of children in the following age categories: 0-6, 7-9, 10-13, and 14-17, number of adult men and women, an urban dummy, and departamento by year fixed effects. We also include linear splines of the running variable, defined as the difference between the cutoff age and age a week before the survey date in months. We use a bandwidth of 12 months and a triangular kernel. Survey years: 2014-2019.

specification, to using a second-order polynomial on each side of the cutoff to flexibly control for the running variable, and to allowing the slopes to vary before and after the policy change on either side of the cutoff, respectively.

In our main specification we use age in months to determine exposure to the law. However, because we do not have the exact survey interview date, among children born in the same month, there might be children who were exposed to the law at the moment of data collection and others who were not. To ensure that measurement error is not biasing our results towards zero, column 8 of Appendix Table A.9 reports results from an specification that uses exact birth and survey interview dates to determine exposure. Even though precision is reduced because exact interview dates are only available for 3 survey waves (2013, 2014 and 2016), the point estimates are very similar. Further, we also find similar results when we exclude observations of children that, according to their age in months, are within a month of exposure and who are more prone to misclassification (column 9).[36]

Another potential source of measurement error stems social desirability bias.[37] In particular, one might worry that the law changed the stigma surrounding child labor and affected the accuracy of parents' reports of their children's work. However, we think that this is unlikely for several reasons. First, we observe no discontinuities in either survey responses (Appendix Fig-

[36]This, in turn, attenuates potential concerns related to the reference period of the questions about employment—the week prior to the interview—which can result of misclassification among children born within a month of each cut-off.

[37]The extent to which measurement error in child labor as reported by proxies (e.g., parents) plagues household survey data and whether it is related to social attitudes and norms is debated. Some find that there is no systematic differences across reports by children and proxies when concerning economic activity (Dillon et al., 2012; Dziadula and Guzmán, 2020) while others find differences but no relation to social norms (Dammert and Galdo, 2013). A recent study from the cocoa industry in Cote d'Ivoire finds that proxies severely under-report work of children attending school and that under-reporting responded to an intervention that potentially signaled support (rather than punishment) for farmers with working children (Lichand and Wolf, 2022).

ure A.7) or in reported work probabilities in the pre-law period (Figure 1), when work under 14 was illegal. Second, the 2014 law legalized and legitimized work for those under 14. If anything, we expect that the law reduced pressure for parents to under-report their children's work (i.e., be more likely to report that their children work) after the 2014 law. However, we find that children under 14 become less significantly likely to work after the 2014 law, suggesting that our results may underestimate the true labor-reducing effects of the law.

Alternatively, one might think that the 2014 law increased the salience of the harm caused by work for young children and made parents more reluctant to admit their children were working. If this were the case, the reduction in child work that we document could simply reflect reduced parental reporting of work for children under 14 rather than an effect of the law on work. In this scenario, we would expect the stigma surrounding child work to be especially strong for younger children; however, we find no statistically significant effects at the younger thresholds (see Panels B and C of Table 1). This suggests that this type of systematic misreporting is unlikely to explain our results at the 14-year-old threshold. Recall that we define treated children as the *older* children at the younger thresholds (because they receive expanded workers' rights following the 2014 law relative to younger children); thus the results in Panels B and C indicate that if anything, older children (who should be less subject to stigmas surrounding work, relative to younger children) were less likely to be reported as working after 2014. Overall, we find little reason to believe that social desirability bias plays a role in our estimates.

The 2014 law allowed the participation of children in family and community activities without age restrictions as long as the activities contribute to children's integration into the community or to the development of skills and did not represent exploitation, interfere with a child's education, or entail potentially risky activities. Examples include working in a communal farm or working for community organizations. While our data do not allow us to identify specific types of family or community labor (to which these exceptions apply), work for this purpose is rare; in 2016, only 6% of working children report maintaining family or community customs as the main reason for working.[38] Furthermore, in column 11 of Appendix Table A.9 we provide evidence that our main results are not driven by changes in these types of activities by excluding municipalities with a high share of residents that identify as indigenous (defined as municipalities with an above-median share of indigenous residents), where these exceptions to the law are more likely to apply.[39]

Our results are also robust to alternative specifications that rely on different identification assumptions. We estimate difference-in-difference specifications comparing outcomes of children one year above each cutoff to those one year below the each cutoff, before and after the policy change. Unlike our main specification, which identifies responses among children who were just affected by the policy change (based on their date of birth), the difference-in-difference specification allows for lags in responses to the law of up to a year. However, this alternative methods assumes that there were not economic shocks differentially affecting the work outcomes of older versus younger children. Reassuringly, Appendix Table A.10, shows that the results are nearly identical to those from our main specification. Finally, in light of the results demonstrating no evidence of discontinuities around the relevant cutoffs before the law (see

[38]For children ages 7-17. Authors' calculations using the 2016 ENNA.

[39]Municipalities are classified according to the 2012 Census data. Note that municipality codes are anonymized in the household data starting in 2017, meaning that we cannot link the data to other sources using municipality codes in 2017. Thus, the sample for column 10 of Appendix Table A.9 does not include data from 2017.

Figure 1), we also report estimates corresponding to a regression discontinuity specification using only data after the enactment of the law. Column 10 in Appendix Table A.9 shows that the point estimates are very similar to those related to our main difference-in-discontinuity specification.

Finally, our results on child employment are at odds with those of Kamei (2020) who studies the impact of the 2014 Bolivian law and finds that the probability that boys age 12-13 work for their families increases in 2014 relative to the pre-law period. We believe that the differences with our results arise largely from differences along two important dimensions: data and empirical approach. First, we study the effects of the law over a longer horizon (up to 4 years after the introduction of the law), whereas Kamei (2020) restricts attention to the 6 months after the introduction. This longer time span is important if the law's effects take time to surface — for example, if employers take time to adjust to the new regulations. Moreover, we use additional survey waves after the de facto reversal of the law in 2018 to validate our estimates.

Second, Kamei (2020) uses a difference-in-difference strategy that compares 12-13 year old boys to a pooled control group of boys age 7-9 and 14-16. In contrast, given the steep age gradient in work probabilities observed in the data, we employ a difference-in-discontinuity strategy that compares children just above and below the age thresholds, before and after the law. This enables us to attenuate potential confounders related to labor market shocks that can differently affect younger children (7-9) and older children (15-16). Nonetheless, we examine the robustness of our results to an alternate difference-in-difference specification similar to Kamei (2020). Specifically, we pool 9- and 14-year-old children into a single control group and estimate separate treatment effects for a younger treatment group (children that are at least 10 but younger than 12) and an older treatment group (who are at least 12 but younger than 14). We find that the point estimates of the effect of the law are negative for both the younger and older treatment groups, though it is only significant for the older treatment group (Appendix Table A.11, column 1), which is consistent with our main results around the 14-year-old cutoff.[40] However, when we expand our pooled control group to include children as young as 7 and as old as 16 as in Kamei (2020), the coefficients drop in magnitude and are not statistically significant, although they remain negative. These changes may reflect potential violations to the identification assumption for this pooled difference-in-difference specification —that in the absence of the policy change, the work outcomes would have evolved similarly for the 7- and 16-year-old control groups and the younger and older treatment groups.

5.2 Effects of the law on the nature of child work

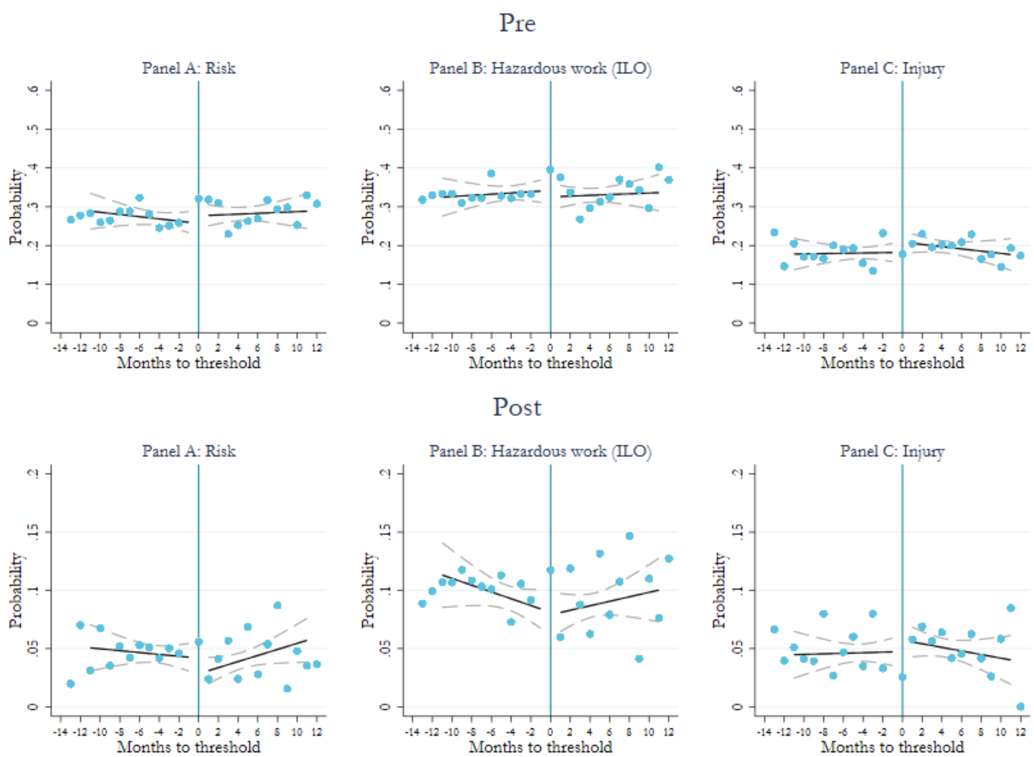
One key objective of the new law was to improve the working conditions of children. Though the new law did not increase the likelihood of child work, it is possible that the law changed the nature of jobs that children engage in.

As mentioned in Section 3, we have only two rounds of child labor surveys. This means that we have much smaller samples to assess the effects of the law on job safety outcomes and that identifying the effects separately at each age threshold will likely lead to noisy estimates. To help improve the precision of our estimates, we estimate a stacked difference-in-discontinuity specification, an often-used approach to estimating a common treatment effect across multiple cutoffs (see, for example, Beuermann and Jackson (2020); Pop-Eleches and Urquiola (2013)). Specifically, we pool the samples across age groups but maintain the definitions of treatment variables and running variables to be rel-

[40]It is important to note that this alternate specification does *not* identify the same effects as our main specification because the treatment and control groups are not the same.

ative to each specific threshold.[41] We additionally include cutoff fixed effects, which ensures that our estimates continue to be based on local comparisons around each age cutoff.[42]

Figure 2: Job Risks, Hazardous Work, & Work Injuries: Stacked Data



The running variable is the difference between age in months and the age cutoff a week before the survey date, defined separately for each age threshold. We use a triangular kernel and we reweight the observations as described in Section 4.

We first turn to the graphical evidence, presented in Figure 2. In the pre-law data, we observe no discontinuity across the stacked thresholds for any of the three outcomes—facing risks at work, performing hazardous work, or

[41]Because the treated group are those over the threshold at the 10- and 12-year-old cutoffs but below the threshold at the 14-year-old cutoff, we multiply the running variable by -1 for the observations around the 14-year-old cutoff to maintain consistency across thresholds.

[42]Specifically, we estimate a slightly modified version of the specification in equation 1 that includes cutoff fixed effects.

having been injured on the job. In the post-law data, it appears that treated children face slightly less risk at work following the 2014 law, but this difference does not appear to be statistically significant. Overall, the data in Figure 2 suggest that there were no impacts of the 2014 law on these outcomes.

Table 3: Effects of the Law on Job Risks, Hazardous Work, and Work Injuries (Stacked Difference in Discontinuity)

Panel A: Without Controlling for Work Indicator

	Faces Risks at Work	Performs Hazardous Work	Has Been Injured at Work
	(1)	(2)	(3)
Post × Treated	-0.00777	0.0210	-0.0145
	(0.0171)	(0.0189)	(0.0151)
Obs.	8372	8372	8411
Mean	0.157	0.214	0.114

Panel B: Controlling for Work Indicator

	Faces Risks at Work	Performs Hazardous Work	Has Been Injured at Work
	(1)	(2)	(3)
Post × Treated	-0.0108	0.0167	-0.0109
	(0.0154)	(0.0153)	(0.0145)
Obs.	8372	8372	8411
Mean	0.157	0.214	0.114

Household level clustered standard errors in parentheses. Control variables: gender, working indicator (Panel B only), urban indicator, age group fixed effects, household head characteristics (schooling, gender, age, and indigenous indicator), gender, number of children in the following age categories: 0-6, 7-9, 10-13, and 14-17, number of adult men and women, and departamento by year fixed effects. For the risk index and hazardous work regressions, the running variable is the difference between age in months and the age cutoff at the survey date. For the injury index, the running variable is the difference between age in months and the age cutoff a year before the survey date. The specification includes linear splines of the running variable. The running variable is multiplied by -1 for the 13 and 14 year-olds age group for interpretability. The bandwidth for all specifications is 12 months. We use a triangular kernel. Survey years: 2008 and 2016. We use a reweighting method described in Section 4.

Turning to the regression-based evidence, we find that treated children — who are newly granted worker protections under the 2014 law — are generally less likely to face risks at work. Consequently, they are less likely to have been injured at work, but these estimated effects are not statistically significant from zero (see columns 1 and 3 in Table 3). These results are precise enough to allow us to rule out declines in risk larger than 4.3 percentage points and declines in injuries larger than 4.0 percentage points with 95% confidence.

One concern is that because the law reduced child work around the 14-year-old cutoff, our reduced-form results do not accurately capture the true impacts of the law on risks outcomes among children who remain working. We offer two pieces of evidence to rule out this concern. First, Panel B in Table 3 shows that we obtain similar point estimates when we control for work probabilities. Second, we find relatively small, non-significant effects for younger children—those for which we found no effects of the law on work probabilities—when we replicate our analysis around each age cutoff in Appendix Table A.12.

5.2.1 Robustness

The results regarding risk outcomes are robust to alternative specifications. Appendix Table A.12 reports estimates of the effects of the law around each age threshold, as in equation (1). The results show the point estimates are consistent with the stacked results in Table 3 and very similar across age thresholds.[43] In addition, Appendix Table A.16 shows that our results are also robust to changes in bandwidth, excluding controls, including a quadratic polynomial in the running variable, and excluding children within 1 month of the cutoffs (donut-style regressions).

[43] We display the graphical results for the individual age thresholds in Appendix Figures A.11, A.12, and A.13

6 Mechanisms

The impact of the law was not ex ante obvious. On the one hand, enabling children to obtain work permits should draw more children to the labor force, if there was indeed demand for such permits. On the other hand, the threat of inspections by the Ministry of Labor, Employment, and Social Protection (MTEPS) may discourage the hiring of younger children either because employing children increases the likelihood of labor inspections, which can be costly for informal firms, or because complying with regulations can be costly for formal firms. The fact that we observe a *decline* in child labor due to the law is consistent with the idea that the new legislation increased the costs of employing 13 year-old children, relative to 14 year-olds. We discuss this and other mechanisms below.

6.1 The threat of workplace inspections

The 2014 law highlighted the protections and benefits newly granted to workers under the age of 14 and tasked the MTEPS with ensuring compliance with the law (in addition to enforcing existing labor regulations). One potential explanation for our findings is that the 2014 law increased the perceived scrutiny of employers hiring younger children, increasing the relative costs of hiring younger children and decreasing the demand for their labor. Inspections can be particularly costly for informal firms; MTEPS inspections could investigate them for their compliance with child-worker protections but also for their registration with the tax authorities (see Section 2.3), and compliance with other labor regulations. This mechanism relies on two premises: that employers were aware of the regulations and that workplace inspections after the law were indeed a credible threat. We examine both premises below.

First, recall from Section 2.3 that the law brought up intense scrutiny from the local and international community and support from the then president, which is reflected in the spikes on press articles about the law published by Bolivian Media (see Appendix Figure A.5). Moreover, between 2015 and 2018, the MTEPS carried out a series of workshops to inform employers about the regulations regarding child labor and to educate child workers and parents about workers' rights; these workshops reached over 11,000 workers and employers, suggesting that there was also a formal effort to increase awareness.

Second, the 2014 law required that the regional MTEPS offices conducted child labor inspections to verify compliance. These inspections complemented the labor and workplace safety inspections already being conducted by the MTEPS before the law, which verify compliance of a firm's formal registration and compliance of the general worker protections. We observe increase in MTEPS inspections — both generally, and specifically for child labor — in the period after the enactment of the law (see Appendix Figure A.3), likely due to an increase in resources devoted to this cause (see for example, the stark increase in child labor inspectors in 2014 in Appendix Figure A.4).

We also highlight that the effects of the law are largest for work that is allowed and regulated under the law (Table 1, Panel A, column 6). The effects are also driven by work for others, which is more likely to be scrutinized than self-employment. While most of work for others occurs in family-owned businesses, children's work for family and for external employers appears similar on important observable dimensions (see Section 3). Critically, family-owned firms and externally-owned firms are the same size (as captured by the median number of employees) and exhibit similar rates of formalization. This suggests that the sanctions for lack of enforcement and the probability of inspections are likely to bind similarly for family and non-family employers.

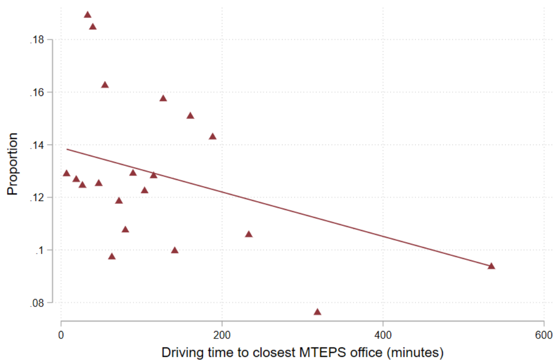
We complement this descriptive evidence with an empirical test for this mechanism. If the perceived threat of being subject to a MTEPS inspection is an important determinant of the law's effects, it should be the case that the declines in child work are driven by localities that are more likely to be subject to inspections. Previous work finds that distance acts as a deterrent to enforcement of labor regulations (Almeida and Carneiro, 2012; Ponczek and Ulyssea, 2021), and evidence from Bolivia suggests that compliance with tax registration is higher among firms closely located to the tax authority (McKenzie and Seynabou Sakho, 2010). We find corroborating evidence in our data; Figure 3 illustrates that workers in areas closer to MTEPS offices, based on driving routes (optimized to minimize travel time), are more likely to have formal labor contracts and employer-provided health insurance and work for a firm with a national tax registration, even after controlling for job and worker characteristics that are likely correlated with distance to MTEPS offices (such as education and sector of work).

Accordingly, we exploit cross-municipality variation in the driving time to regional MTEPS offices (those in charge of conducting inspections) to proxy for variation in the probability of workplace inspections. We compare the effects of the law on work probabilities between municipalities that are "far" and "near" from the nearest regional Ministry of Labor (MTEPS) offic, where "far" is defined as above the median driving time.[44]

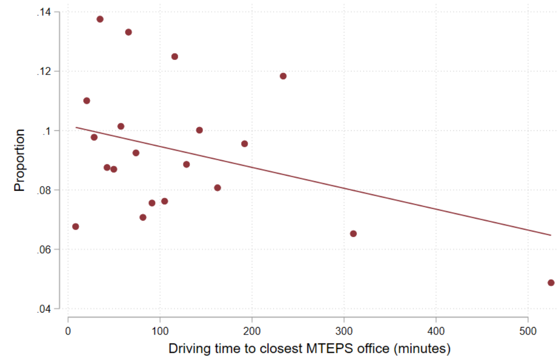
[44] We measure the driving time from the municipality capital, typically the most populated locality in the municipality, to the nearest MTEPS office. See Appendix Section D for details. Note that municipality codes are anonymized in the household data starting in 2017, meaning that we cannot link the data to other sources using municipality codes in 2017. Thus, the sample for Table 4 does not include data from 2017.

Figure 3: Compliance with labor regulations and travel time to inspectors (Pre-Law)

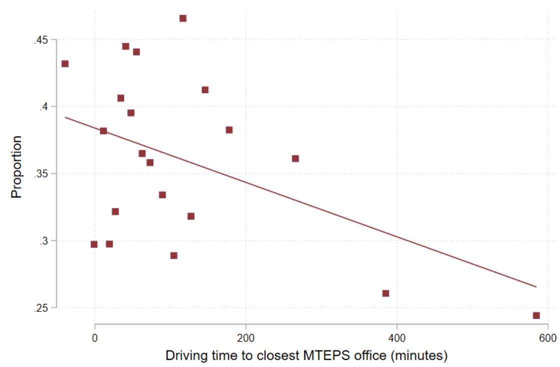
(a) Formal Labor Contracts



(b) Tax Registration



(c) Health Insurance



This figure presents the proportion of adult workers (age 18+) that have a formal work contract (panel a), work for a firm with a national tax ID (panel b), and have health insurance through their employer (panel c), by quantiles of driving time to the nearest MTEPS office (20 quantiles) using the 2012-2013 Encuesta de Hogares. The data are residuals after removing variation due to the following controls: age, gender, years of schooling, an urban dummy, a dummy variable denoting department capitals, and sector of work fixed effects.

Table 4: Heterogeneity by Distance to MTEPS Officess: Likelihood of Allowed Work (14-Year-Old Cutoff)

Panel A: Driving Time

	All (1)	No Capitals (2)	No MTEPS (3)
Post × $\mathbb{1}\{\text{Age}< 14\}$ for Far (Driving Time)	0.0114	0.0159	0.00469
	(0.0541)	(0.0527)	(0.0532)
Post × $\mathbb{1}\{\text{Age}< 14\}$ for Near (Driving Time)	-0.0368	-0.0544	-0.100
	(0.0192)	(0.0298)	(0.0441)
Obs.	7650	4526	2964
Mean	0.207	0.285	0.366
P-value of difference	0.401	0.244	0.127
P-value of difference (urban controls)	0.145	0.182	0.114
Panel B: Straight-line Distance (as the crow flies)			
Post × $\mathbb{1}\{\text{Age}< 14\}$ for Far (Direct Distance)	0.000968	-0.0172	-0.0190
	(0.0377)	(0.0397)	(0.0429)
Post × $\mathbb{1}\{\text{Age}< 14\}$ for Near (Direct Distance)	-0.0354	-0.0429	-0.0829
	(0.0187)	(0.0276)	(0.0378)
Obs.	7650	4526	2964
Mean	0.207	0.285	0.366
P-value of difference	0.326	0.510	0.132
P-value of difference (urban controls)	0.276	0.451	0.136

Household-level clustered standard errors in parentheses. Municipalities that are classified as Far are above the median driving time from a MTEPS office (see Appendix D for more details). Control variables: CCT eligibility indicator, urban, HH head characteristics (schooling, gender, age, indigenous indicator), gender, no. of children aged 0-6, 7-9, 10-13, and 14-17, no. of adult men and women, and departamento by year FE. We also include linear splines of the running variable (difference between the cutoff age and age a week before the survey date in months). The specification for the p-value with urban controls additionally includes: post × urban, treatment × urban, post × distance × urban, and treatment × distance × urban. We use a bandwidth of 12 months and a triangular kernel. Survey years: 2012-2016. We also report the mean of the dependent variable for the control group.

Panel A in Table 4 illustrates that the law significantly decreases the likelihood of allowed/regulated work for 13-year-olds relative to 14-year-olds, but only in areas that are located near MTEPS offices, where there was likely to be stronger enforcement (column 1). This remains true when we further restrict the sample to municipalities that are not also department capitals (column 2), and to municipalities that do not contain an MTEPS office (column 3), illustrating that the result is not being driven only by large, mostly urban municipalities.[45] These results are robust to using straight line or "as the crow flies" distance as an alternative measure of distance to MTEPS offices (see Panel B). While the effects are not statistically distinguishable across areas near and far from MTEPS offices, the point estimates suggest that the overall declines in child labor are almost exclusively driven by children in localities closer to enforcement offices.[46] We do not find substantially different effects between municipalities that are near and far from the MTEPS regional offices for younger children (see Appendix Table A.13), likely due to the low incidence of overall child labor among younger children.

As a final piece of evidence for perceived increases in the probability of inspection, we examine the shifts in child work across different firm sizes. This is based on prior studies which find that larger firms are more likely to be targeted by regulators while small firms may be more likely to remain "under the radar" (Almeida and Ronconi, 2016).[47] Specifically, we estimate the effects of the 2014 law on the size of firms that children work for, measured as the number of employees at the firm. Overall, we find evidence that younger children — for whom the 2014 legislation makes hiring particularly costly — tend to work for smaller firms following the passage of the 2014 law. Column

[45] These results also help to rule out the concern that the results are driven by family work in subsistence farming, which is more prominent in isolated areas far from MTEPS offices.

[46] To show that our results are not driven by differences across urban and rural areas, we also report the p-value for the difference when additionally controlling for all possible interactions between the treatment variables, the post-law indicator, and urban status. These additional controls do not change the results.

[47] Almeida and Ronconi (2016) outline a number of reasons why enforcement agencies may target larger firms; for example, larger firms may be less costly to inspect; they may be more visible to media and the public; and they may have more rents to extract if inspectors are corrupt.

1 of Table A.14 shows that this is true when considering the sample of all children (where firm size is coded as zero for non-working children) and, more importantly, column 2 illustrates that this effect is even stronger for the sample of working children (though it is not precisely estimated, given the small sample size of working children). This finding is consistent with the notion that larger firms (which are more likely to be inspected) find it differentially more costly to hire younger workers after the 2014 law and that, consequently, these younger children end up working for smaller firms.

6.2 Alternative mechanisms

Direct increases in wages. The 2014 law established that even children age 13 or younger were entitled to receive wages in line with the minimum monthly salary. One alternative explanation is that the law directly increased the costs of hiring younger children, relative to children age 14 or older. We provide suggestive evidence that the hourly wages of workers just under age 14 rose by more than wages of workers just over age 14 in column 1 of Appendix Table A.15. We also observe that total monthly earnings rise (column 2) and that the probability of earning above the minimum salary increases by about 6.3 percentage points (column 3), though the estimates are imprecisely estimated.[48] Despite the suggestive evidence of increased costs of hiring younger children, this mechanism is unlikely to account for the overall declines in child work that we observe as the subset of working children that report receiving a salary is very small (589 children out of 9000 children in the sample around the 14-years-old cutoff). In addition, this mechanism should affect

[48] We recognize that the estimates in Appendix Table A.15 may not represent causal effects of the law on wages as we estimate them on a selected sample: children who work and report wages. Both criteria can be endogenous to the policy change.

employment mostly in the formal sector, which is unlikely as most workers work informally (Elgin et al., 2021).

Low take up of work permits. The lack of employment effects for younger children may be explained by two non-exclusive mechanisms. First, younger children—i.e., those below the age of 12 years old—are simply less likely to work, suggesting that the prior legislation was not a binding constraint for them.[49] Second, the costs and complexity of the application process may have lowered the demand for permits. To qualify for a permit, children first had to be declared fit to work by a doctor following a medical exam, and then visit the closest Child Advocacy office (DNA), often in a different locality.[50] These transaction costs may deter children from legally entering into the workforce, even when they have the option to do so. Consistent with evidence showing that the complexity of application processes for public services reduces takeup (Banerjee et al., 2021), the probability of having a permit is substantially lower among the children from the poorest households (see Appendix Figure A.14), who are least able to pay the costs of the obtaining a permit.

7 Conclusion

Overall, we find no evidence that the 2014 law increased child work in Bolivia. In fact, we find that children under age 14 were less likely to work in permitted

[49]We observe very low levels of employment for this age group (9.7%) before 2014 (see Appendix Figure A.6).

[50]Though the 2014 law mandates that every municipality in the country have a dedicated Child Advocate Office, as of 2016, 20% did not have one and many lack funding, personnel, and materials (U.S. Department of Labor (2019)). In a recent report, the People's Advocate Office (*Defensora del Pueblo*) found that only 12% of surveyed municipalities kept records of child and adolescent labor (Defensoría del Pueblo, 2021), though only 59 out of 339 municipalities were surveyed and it is not clear whether the surveyed municipalities were representative.

and regulated activities after the passage of the law (relative to children over age 14). We posit that this is primarily due to the increased perceived costs of hiring younger children that the 2014 law imposed on employers — both through increased scrutiny and threat of inspections for firms hiring young children and through the new regulations that granted rights and protections to working children under 14. As some have claimed, "For adolescents, the code frequently had the effect that companies preferred to hire adults rather than jump over bureaucratic hurdles" (Liebel (2019)). Indeed, we find that after the key child labor components of the law (those granting rights and protections to workers under the age of 14) were repealed in 2018, work probabilities and hours of work *increased* for children under the age of 14.

Importantly, we find that the law did not significantly affect children's riskiness of work, hazardous work, or injuries on the job. This stands in contrast to one of the purported aims of the policy to make child work safer. That said, we also find no evidence that allowing young children to work increased exposure to harmful or hazardous work conditions.

The findings are important to the broader discussion of optimal child labor policy. While previous work finds that outright bans are not able to eradicate child labor, our results illustrate that a natural alternative — legal recognition and regulation of child labor — does not necessarily make child work safer. Both bans and legalization/regulation do not address what many consider the root cause of child labor: poverty. Instead, these policies affect employers' costs of hiring children, and thus affect child labor in nuanced ways that can run contrary to policy aims.

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A Appendix Figures and Tables

Table A.1: Key Dimensions of Child Labor Legislation before and after 2014

	Before 2014	After 2014
Age<10	No legal work	No legal work[1]
$10 \leq$Age<12	No legal work	Legal to engage in independent work[2]
$12 \leq$Age<14	No legal work	Legal to engage in independent work or work for others[2], with worker benefits and protections[3]
Age≥ 14	Legal to engage in independent work or work for others[2], with worker benefits and protections[3]	

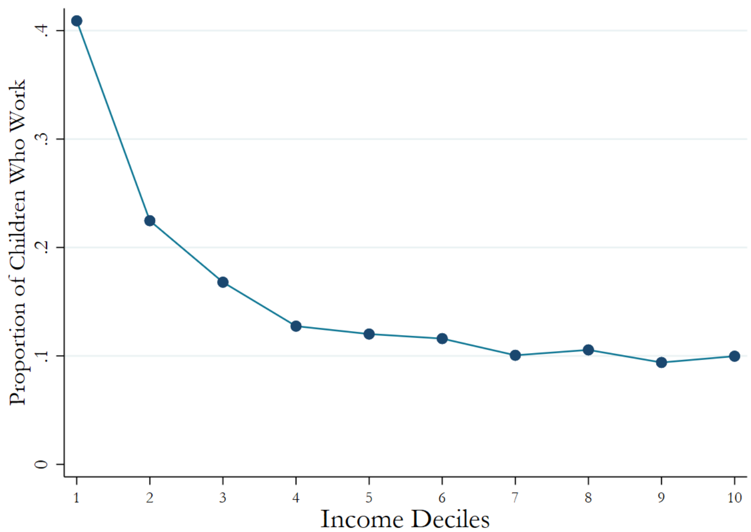
[1]In 2014, children of all ages were allowed to engage in communal work as long as it did not infringe on their basic rights (e.g., to education and health).

[2]In 2014, the list of permitted tasks and sectors for child work was revised to exclude agricultural work for an employer.

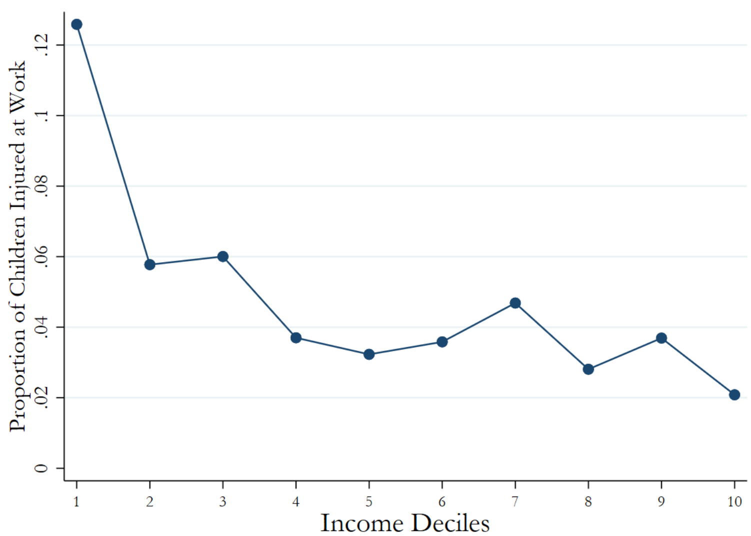
[3]Prior to 2014, only children age 14 and over were entitled to the same workers' rights as adults, including minimum wages and social security. After 2014, these rights were extended to working children age 12 and older and the benefits were expanded (for example, to include two paid study hours per day).

Figure A.1: Probability of Work and Work Injuries by Income

(a) Probability of Work by Income Decile

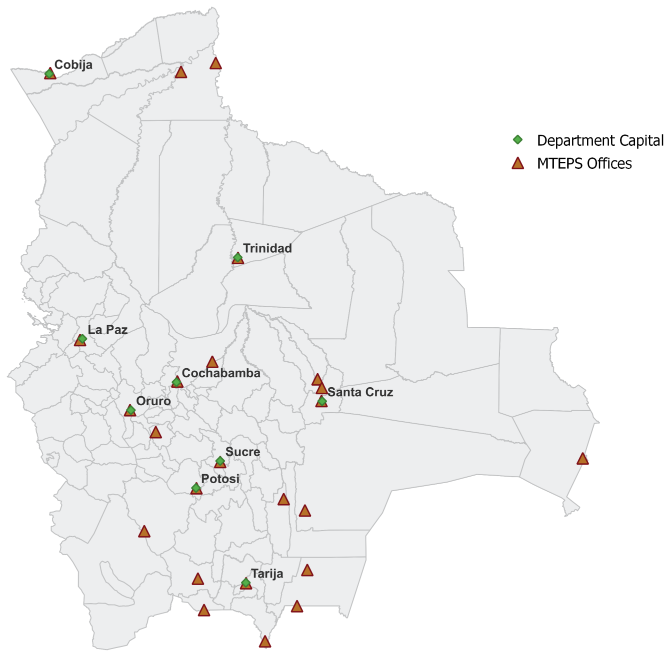


(b) Probability of Work Injury by Income Decile



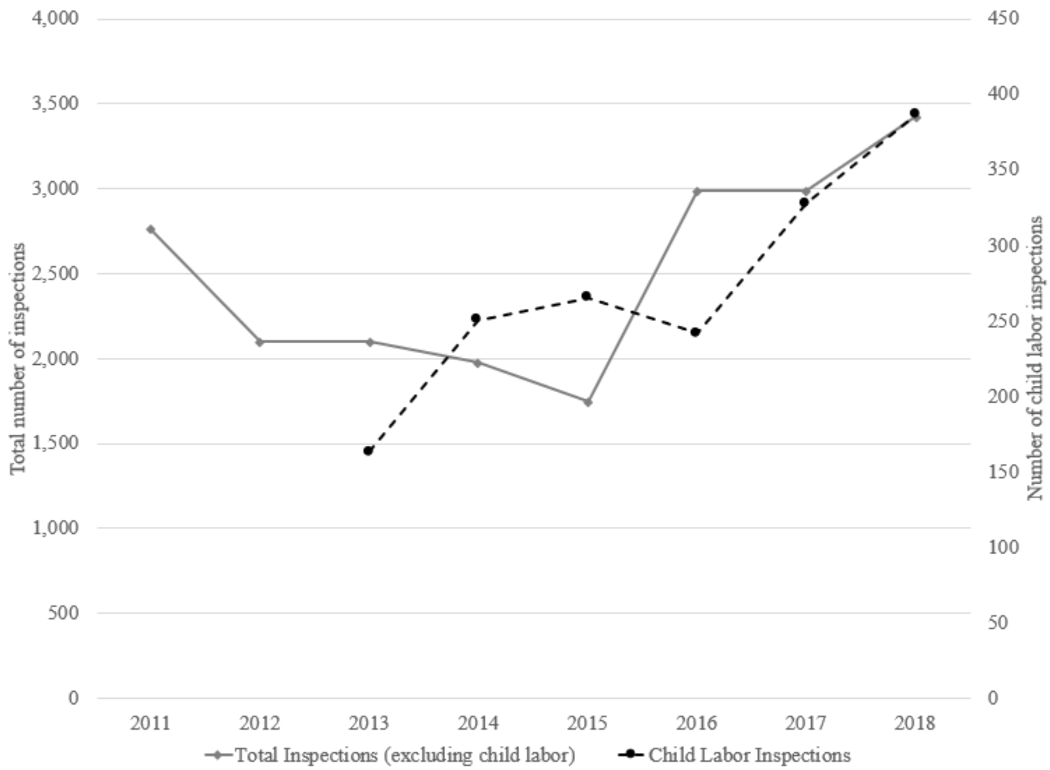
(a) Sample: children age 7 to 17 in the 2012-2013 rounds of the Encuesta de Hogares. (b) Sample: children age 7 to 17 in the 2016 Survey of Children and Adolescents. Note that household income is not available in the 2008 Child Labor Survey.

Figure A.2: Ministry of Labor Offices



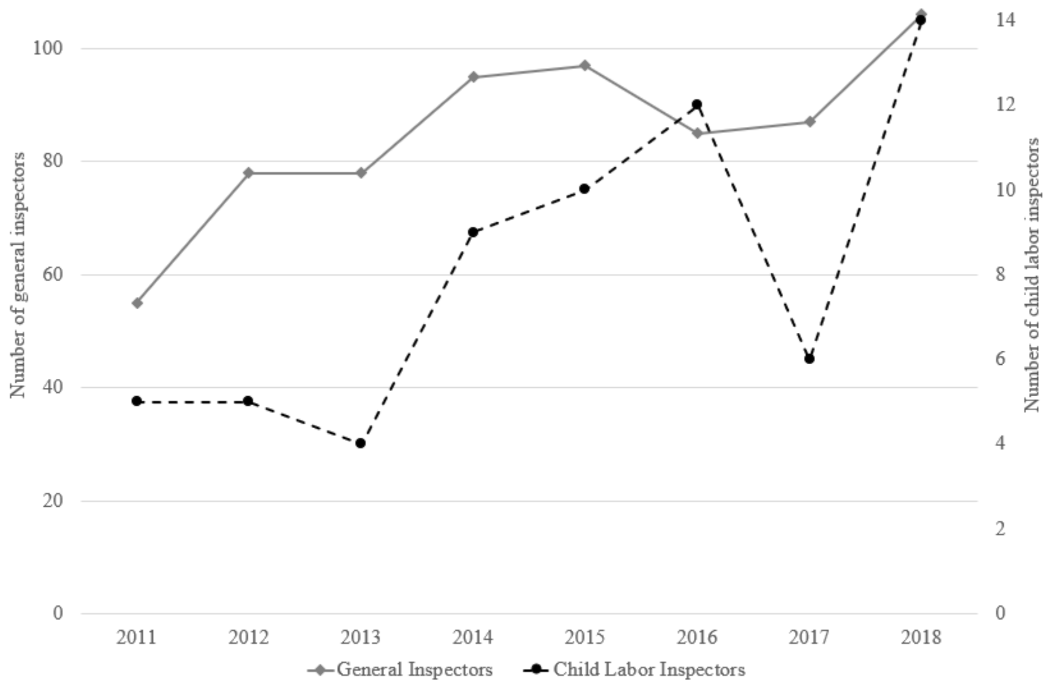
The addresses of permanent MTEPS offices can be found here: `https://www.mintrabajo.gob.bo/?page_id=2626`.

Figure A.3: Ministry of Labor Inspections over Time



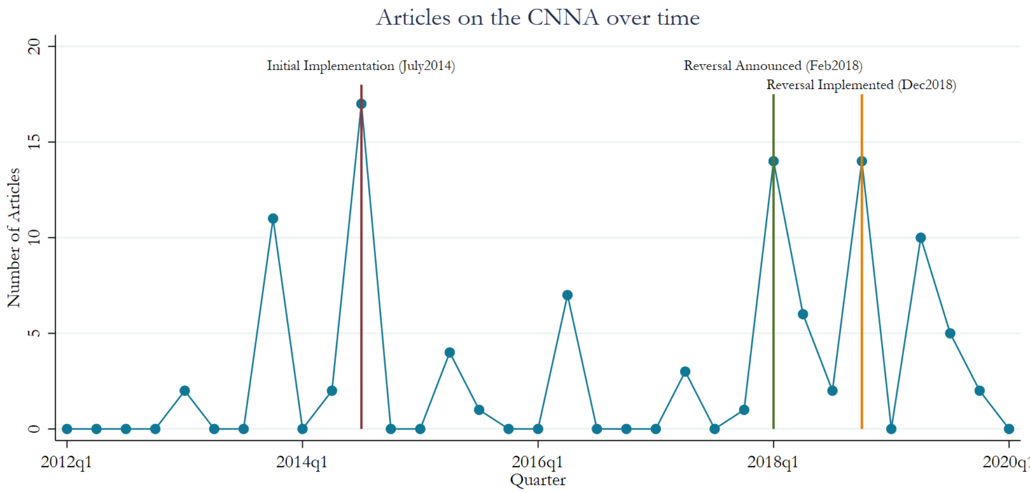
As reported in the annual reports by the Ministry of Labor (Ministerio de Trabajo, Empleo y Previsión Social, 2018). Child labor inspections prior to 2015 are as reported in the US Department of Labor reports (U.S. Department of Labor, 2019).

Figure A.4: Number of Labor Inspectors over Time



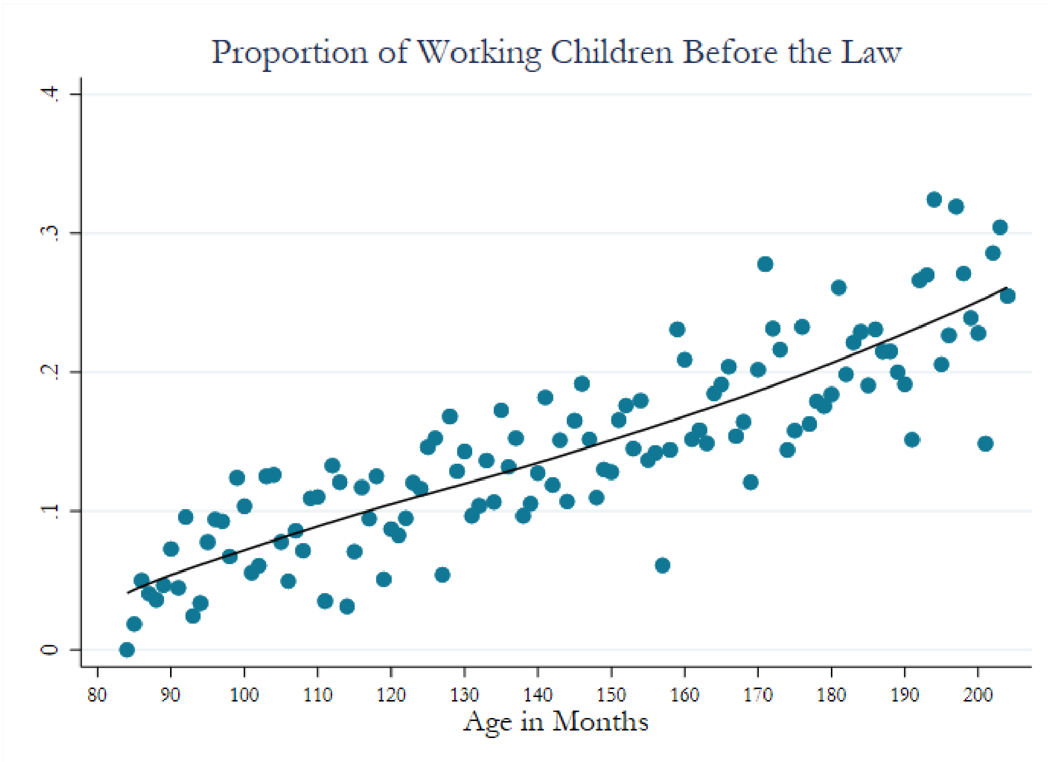
As reported in the annual reports published by the US Department of Labor (U.S. Department of Labor, 2019).

Figure A.5: Articles on the 2014 Law over Time



This figure tracks the number of articles concerning the 2014 law scraped from 43 national and regional Bolivian newspapers between 2012 and 2020. Articles that both mentioned the 2014 law and child labor were included.

Figure A.6: Work Probabilities by Age (Pre-law)



This figure plots the average raw work probability by age (in months) as well as a smoothed line for children between the ages of 7 and 17 prior to 2014. Data source: Encuesta de Hogares. Survey years: 2012-13.

Table A.2: Descriptive Statistics (Pre-Law)

Panel A: Household Data

	All Children (1)	Working Children (2)
Any work	.143	-
Hours worked	3.049	21.321
Work for self	.003	.018
Work for others	.14	.982
Work for external employer	.014	.1
Work for family employer	.126	.881
Prohibited work	.137	.961
Allowed work	.006	.039
Work $\geq$ 30 hrs/week	.027	.192
Attends school	.972	.913
Observations	8699	1244

Panel B: Child Labor Survey Data

	All Children (1)	Working Children (2)
Risk at work	.28	.548
Hazardous activities	.336	.652
Injured at work	.168	.325
Observations	4159	1988

The table shows the mean of the variables. Definitions of the variables appear in Section 3 and Appendix C. The list of prohibited tasks appears in Appendix B. The sample in both panels includes children from ages 9 to 15. The survey years are 2012-2013 in Panel A, and 2008 in Panel B. Observations of the child labor survey are reweighted using the method described in Section 4.

Table A.3: Balance for 30% of Child Labor Survey Data

Panel A: 14-Year-Old Cutoff

	Male	HH Size	Age HH Head	Education HH Head	Male HH Head	Indigenous HH Head	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post	-0.0177	0.322	0.524	-0.181	0.0546	0.0870	-0.00237
	(0.0349)	(0.168)	(0.773)	(0.359)	(0.0310)	(0.0350)	(0.0319)
Obs.	889	889	889	889	889	889	889
R-squared	0.000314	0.00572	0.000614	0.000336	0.00392	0.00794	0.00000700
Mean	0.493	5.625	43.79	7.904	0.738	0.382	0.723
Joint test P-value = .085							

Panel B: 12-Year-Old Cutoff

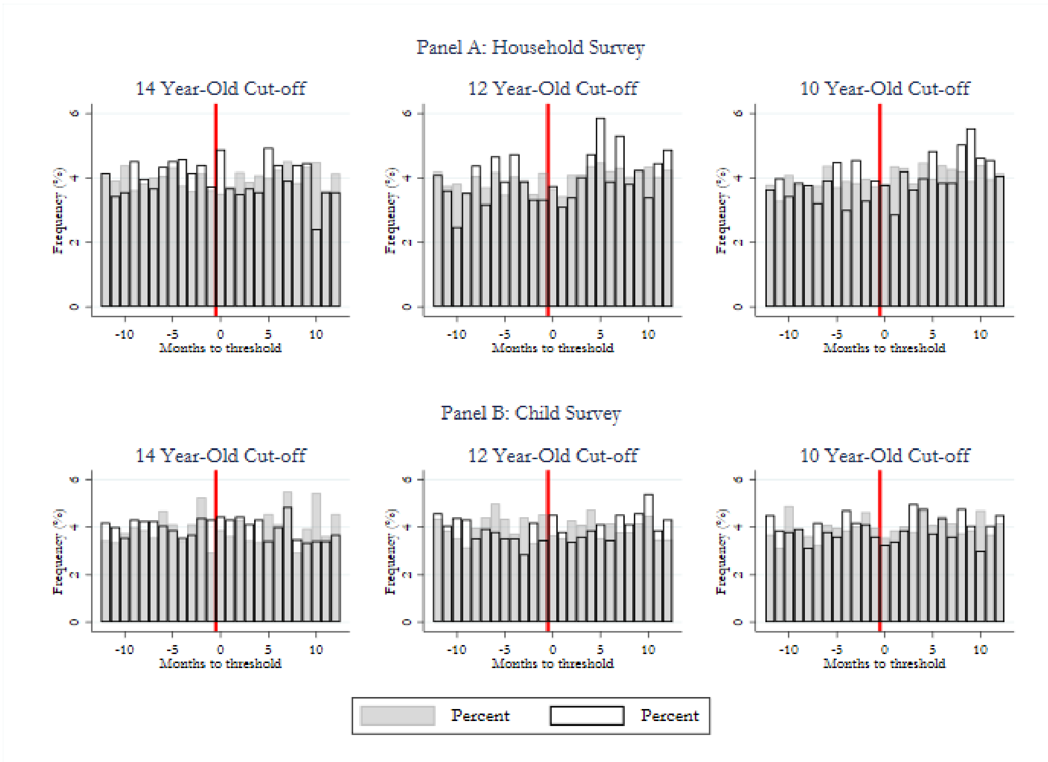
	Male	HH Size	Age HH Head	Education HH Head	Male HH Head	Indigenous HH Head	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post	-0.0369	-0.0241	0.885	-0.236	-0.0115	0.0369	0.0164
	(0.0345)	(0.142)	(0.761)	(0.374)	(0.0285)	(0.0350)	(0.0323)
Obs.	904	904	904	904	904	904	904
R-squared	0.00136	0.0000405	0.00170	0.000528	0.000205	0.00141	0.000335
Mean	0.472	5.522	43.53	8.326	0.786	0.392	0.708
Joint test P-value = .674							

Panel C: 10-Year-Old Cutoff

	Male	HH Size	Age HH Head	Education HH Head	Male HH Head	Indigenous HH Head	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post	-0.0247	0.0407	0.572	0.236	-0.00290	-0.0228	0.0204
	(0.0353)	(0.163)	(0.728)	(0.371)	(0.0289)	(0.0349)	(0.0307)
Obs.	875	875	875	875	875	875	875
R-squared	0.000613	0.0000992	0.000774	0.000553	0.0000133	0.000539	0.000550
Mean	0.465	5.458	41.06	8.591	0.779	0.390	0.728
Joint test P-value = .913							

Household level clustered standard errors in parentheses. The specification includes an indicator that is one in 2016. The bandwidth for all specifications is 24 months. The sample is 30% of the 2008 and 2016 observations that were not used in the reweighting exercise.

Figure A.7: Manipulation Test: Histograms



The running variable in both panels is the difference between age in months and the age cutoff at the survey date. In Panel A the pre sample includes 2012-2013 and the post sample includes 2014-2017. In Panel B the pre sample includes 2008 and the post sample includes 2016.

Table A.4: McCrary Tests for Age at Survey Date

Panel A: Household Survey Data

	Density test	
	Pre	Post
	(1)	(2)
$\mathbb{1}\{\text{Age} < 14\}$	.097	-.069
	(.065)	(.058)
$\mathbb{1}\{\text{Age} \geq 12\}$	.062	-.059
	(.075)	(.059)
$\mathbb{1}\{\text{Age} \geq 10\}$	-.077	.051
	(.075)	(.057)

Panel B: Child Survey Data

	Density test	
	Pre	Post
	(1)	(2)
$\mathbb{1}\{\text{Age} < 14\}$	.0943	-.0716
	(.1096)	(.1141)
$\mathbb{1}\{\text{Age} \geq 12\}$	.1159	-.0949
	(.1214)	(.1123)
$\mathbb{1}\{\text{Age} \geq 10\}$	-.0972	-.1118
	(.1169)	(.11)

The running variable in both panels is the difference between age in months and the age cut-off at the survey date. We use the DCdensity Stata command to implement the McCrary test, with a bandwidth of 12 months and a bin size of one month. In Panel A the pre sample includes 2012-2013 and the post sample includes 2014-2017. In Panel B the pre sample includes 2008 and the post sample includes 2016.

Table A.5: Balance Table: Difference in Discontinuity - Household Survey

Panel A: 14-Year-Old Cutoff

	Schooling (HH head) (1)	Male (HH head) (2)	Age (HH head) (3)	Indigenous (HH head) (4)	Male (child) (5)	HH size (6)
Post $\times$ $\mathbb{1}\{\text{Age}< 14\}$	0.192	-0.0199	-0.415	0.0269	-0.0350	-0.0717
	(0.265)	(0.0212)	(0.565)	(0.0244)	(0.0256)	(0.0981)
Obs.	8662	8662	8662	8662	8662	8662
Mean Control	8.462	0.783	44.40	0.381	0.494	5.490
Mean Treated	8.499	0.759	45.16	0.377	0.505	5.481
Joint test P-value = .453						

Panel B: 12-Year-Old Cutoff

	Schooling (HH head) (1)	Male (HH head) (2)	Age (HH head) (3)	Indigenous (HH head) (4)	Male (child) (5)	HH size (6)
Post $\times$ $\mathbb{1}\{\text{Age}\geq 12\}$	-0.182	-0.0283	0.0624	-0.0203	-0.0503	0.0933
	(0.279)	(0.0218)	(0.561)	(0.0250)	(0.0264)	(0.101)
Obs.	8344	8344	8344	8344	8344	8344
Mean Control	8.741	0.776	43.85	0.375	0.507	5.475
Mean Treated	8.672	0.784	43.15	0.389	0.490	5.491
Joint test P-value = .253						

Panel C: 10-Year-Old Cutoff

	Schooling (HH head) (1)	Male (HH head) (2)	Age (HH head) (3)	Indigenous (HH head) (4)	Male (child) (5)	HH size (6)
Post $\times$ $\mathbb{1}\{\text{Age}\geq 10\}$	-0.118	-0.0364	0.581	-0.00293	0.0363	0.0545
	(0.284)	(0.0215)	(0.586)	(0.0258)	(0.0270)	(0.101)
Obs.	8273	8273	8273	8273	8273	8273
Mean Control	8.805	0.788	42.40	0.389	0.513	5.457
Mean Treated	8.952	0.787	41.61	0.385	0.495	5.445
Joint test P-value = .401						

Household level clustered standard errors in parentheses. We control for eligibility to CCT in the 14-year-old cut-off. The running variable is the difference between age in months and the age cut-off at the survey date. The specification includes linear splines of the running variable, an indicator that is one in 2014 and after, and an indicator that is one for the children in the corresponding age group. The bandwidth for all specifications is 12 months. We use a triangular kernel. The sample includes 2012-2017.

Table A.6: Balance for Reweighted Child Labor Survey Data - Full sample

Panel A: 14-Year-Old Cutoff

	Male	HH Size	Age HH Head	Education HH Head	Male HH Head	Indigenous HH Head	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × $\mathbb{1}\{\text{Age}< 14\}$	-0.0238	-0.0723	-1.410	0.377	-0.0477	-0.0212	0.0214
	(0.0440)	(0.177)	(0.915)	(0.467)	(0.0374)	(0.0427)	(0.0383)
Obs.	2808	2808	2808	2808	2808	2808	2808
R-squared	0.00110	0.00730	0.00432	0.00172	0.00430	0.00392	0.00219
Mean	0.524	5.673	44.47	8.158	0.774	0.380	0.755
Joint test P-value = .6970000000000001							

Panel B: 12-Year-Old Cutoff

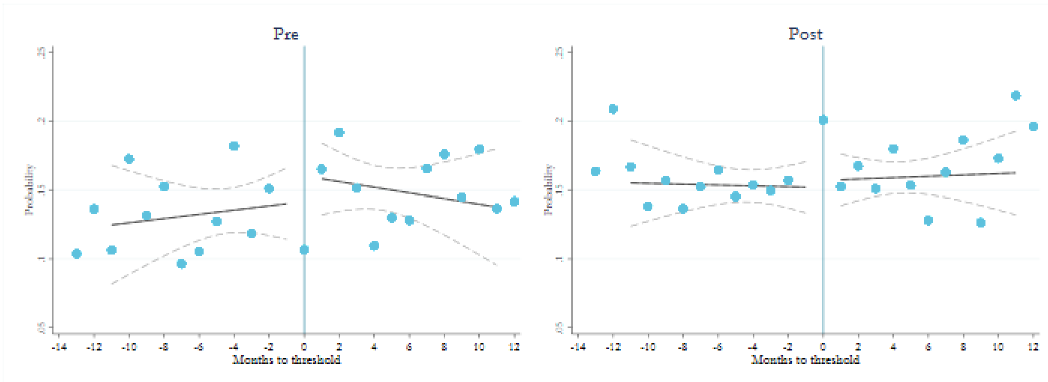
	Male	HH Size	Age HH Head	Education HH Head	Male HH Head	Indigenous HH Head	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × $\mathbb{1}\{\text{Age}\geq 12\}$	-0.0705	0.0490	-1.055	0.433	0.0298	0.0312	-0.00689
	(0.0452)	(0.172)	(0.969)	(0.474)	(0.0388)	(0.0445)	(0.0407)
Obs.	2733	2733	2733	2733	2733	2733	2733
R-squared	0.00213	0.00201	0.00221	0.00507	0.000808	0.00211	0.00151
Mean	0.501	5.550	42.77	8.271	0.782	0.390	0.716
Joint test P-value = .543							

Panel C: 10-Year-Old Cutoff

	Male	HH Size	Age HH Head	Education HH Head	Male HH Head	Indigenous HH Head	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × $\mathbb{1}\{\text{Age}\geq 10\}$	-0.0341	-0.0788	-0.0256	-0.0492	0.0311	0.0129	0.00831
	(0.0438)	(0.178)	(0.972)	(0.480)	(0.0355)	(0.0434)	(0.0395)
Obs.	2831	2831	2831	2831	2831	2831	2831
R-squared	0.00401	0.00130	0.00149	0.00112	0.00108	0.00192	0.00206
Mean	0.503	5.477	41.33	8.442	0.786	0.401	0.721
Joint test P-value = .951							

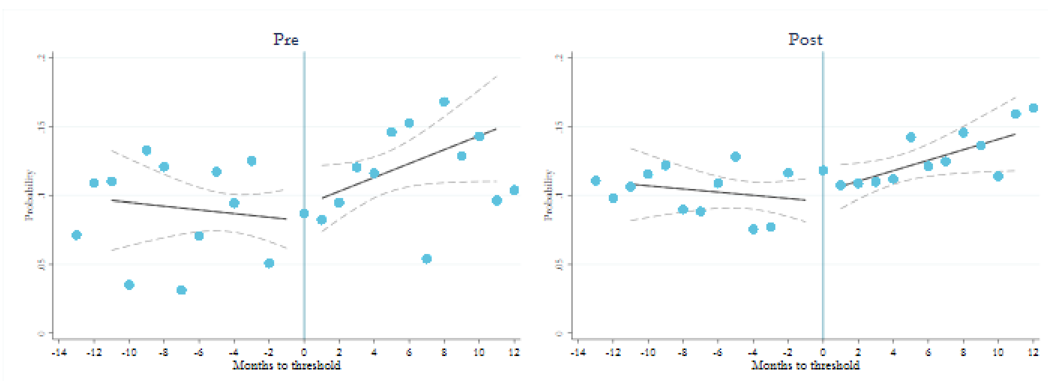
Household level clustered standard errors in parentheses. The control variables are: gender, working indicator, and departamento fixed effects. The running variable is the difference between age in months and the age cut-off at the survey date. The specification includes linear splines of the running variable, an indicator that is one in 2016, and an indicator that is one for the children in the corresponding age group. The bandwidth for all specifications is 24 months. We use a triangular kernel. The sample includes 2008 and 2016.

Figure A.8: Work Probability: 12-Year-Old Cutoff



The running variable is the difference between age in months and the age cutoff a week before the survey date. The pre-law sample includes 2012-2013 and the post sample includes 2014-2017. We use a triangular kernel.

Figure A.9: Work Probability: 10-Year-Old Cutoff



The running variable is the difference between age in months and the age cutoff a week before the survey date. The pre-law sample includes 2012-2013 and the post sample includes 2014-2017. We use a triangular kernel.

Table A.7: Effect of the Law on Schooling Attendance

Panel A: 14-Year-Old Cutoff

	Attends School
	(1)
Post $\times$ $\mathbb{1}\{\text{Age} < 14\}$	0.0135
	(0.0113)
Obs.	8662
R-squared	0.0672
Mean	0.937

Panel B: 12-Year-Old Cutoff

	Attends School
	(1)
Post $\times$ $\mathbb{1}\{\text{Age} \geq 12\}$	0.0101
	(0.00796)
Obs.	8344
R-squared	0.0218
Mean	0.979

Panel C: 10-Year-Old Cutoff

	Attends School
	(1)
Post $\times$ $\mathbb{1}\{\text{Age} \geq 10\}$	-0.00381
	(0.00682)
Obs.	8273
R-squared	0.0186
Mean	0.981

Household-level clustered standard errors in parentheses. Control variables: CCT eligibility indicator (Panel C only), household head characteristics (schooling, gender, age, and indigenous indicator), gender, number of children in the following age categories: 0-6, 7-9, 10-13, and 14-17, number of adult men and women, an indicator for urban, and departamento by year fixed effects. We also include linear splines of the running variable, defined as the difference between the cutoff age and age at the survey in months. We use a bandwidth of 12 months and a triangular kernel. Survey years: 2012-2017. We also report the mean of the dependent variable for the control group in the pre-law period.

Table A.8: Difference in Discontinuity: Household Outcomes

Panel A: 14-year-old Cut-off

	Any Adult in HH Works	Total Hours Worked by Adults	Asset Index	Per Capita Income
	(1)	(2)	(3)	(4)
Post $\times \mathbb{1}\{\text{Age}< 14\}$	-0.00104	-3.885	-0.0195	-16.52
	(0.00955)	(2.766)	(0.0676)	(38.87)
Obs.	8086	8086	6662	8086
R-squared	0.0646	0.314	0.580	0.246
Mean	0.966	93.55	-0.0787	996.3

Panel B: 12-year-old Cut-off

	Any Adult in HH Works	Total Hours Worked by Adults	Asset Index	Per Capita Income
	(1)	(2)	(3)	(4)
Post $\times \mathbb{1}\{\text{Age}\geq 12\}$	0.00530	1.520	0.0957	14.74
	(0.00857)	(2.619)	(0.0683)	(37.73)
Obs.	7904	7904	6493	7902
R-squared	0.0893	0.321	0.592	0.253
Mean	0.967	88.87	-0.186	928.9

Panel C: 10-year-old Cut-off

	Any Adult in HH Works	Total Hours Worked by Adults	Asset Index	Per Capita Income
	(1)	(2)	(3)	(4)
Post $\times \mathbb{1}\{\text{Age}\geq 10\}$	-0.00172	1.389	0.126	24.39
	(0.00922)	(2.635)	(0.0701)	(39.57)
Obs.	7879	7879	6360	7878
R-squared	0.0765	0.324	0.585	0.259
Mean	0.967	85.40	-0.148	921.0

Household level clustered standard errors in parentheses. The control variables are: an indicator that is one if child in HH is in grade for CCT (only for 14-year-old cut-off), an indicator for urban, household head characteristics (schooling, gender, age, and indigenous indicator), number of children in the household in following age categories: 0-6, 7-9, 10-13, and 14-17, number of adult men and women, and departamento by year fixed effects. The running variable is the difference between age in months of the child in the household and the age cut-off a week before the survey date. Hence, we only include households that have only a single child in the corresponding age range. The specification includes linear splines of the running variable, an indicator that is one in 2014 and after, and interaction between the running variable and the indicator for 2014 and after, and an indicator that is one for the children in the corresponding age group. The bandwidth is 12 months. We use a triangular kernel. The sample includes 2012-2017.

Table A.9: Robustness Checks: Difference in Discontinuity for Work Probability

Panel A: 14-Year-Old Cutoff

	Bandwidth (months) Baseline			No Controls	Quadratic	Polynomials Pre-Post Linear	Polynomials Pre-Post Quadratic	Exact Age	Donut	RD	Excl. Indig.
	6	12	24								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Post $\times \mathbb{1}\{\text{Age} < 14\}$	-0.0336	-0.0394	-0.0271	-0.0312	-0.0399	-0.0321	-0.0396	-0.00186	-0.0299	-0.0383	-0.0316
	(0.0248)	(0.0177)	(0.0124)	(0.0202)	(0.0177)	(0.0334)	(0.0177)	(0.0269)	(0.0190)	(0.0198)	(0.0186)
Obs.	4512	9046	18408	9046	9046	9046	9046	4812	8344	5985	6408
R-squared	0.274	0.263	0.248	0.00410	0.264	0.263	0.264	0.268	0.262	0.286	0.138
Mean	0.227	0.221	0.232	0.221	0.221	0.221	0.221	0.269	0.224	0.235	0.157

Panel B: 12-Year-Old Cutoff

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	
Post $\times \mathbb{1}\{\text{Age} \geq 12\}$	-0.0201	-0.0140	-0.00980	-0.0206	-0.0143	-0.0294	-0.0147	0.00866	-0.00347	0.00307	-0.00321
	(0.0233)	(0.0161)	(0.0109)	(0.0184)	(0.0161)	(0.0318)	(0.0161)	(0.0236)	(0.0167)	(0.0177)	(0.0159)
Obs.	4337	8731	17746	8731	8731	8731	8731	4449	8091	5865	6041
R-squared	0.307	0.276	0.264	0.000942	0.276	0.276	0.276	0.291	0.268	0.307	0.124
Mean	0.152	0.151	0.138	0.151	0.151	0.151	0.151	0.186	0.150	0.161	0.0858

Panel C: 10-Year-Old Cutoff

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)		
Post $\times \mathbb{1}\{\text{Age} \geq 10\}$	-0.0188	-0.0169	-0.00114	-0.0112	-0.0168	-0.0280	-0.0170	0.00188	-0.0136	-0.00617	-0.00682
	(0.0200)	(0.0144)	(0.00991)	(0.0163)	(0.0144)	(0.0270)	(0.0144)	(0.0205)	(0.0150)	(0.0151)	(0.0127)
Obs.	4309	8636	17131	8636	8636	8636	8636	4433	7940	5962	5939
R-squared	0.257	0.253	0.256	0.00201	0.253	0.253	0.253	0.296	0.251	0.271	0.121
Mean	0.0993	0.0992	0.0921	0.0992	0.0992	0.0992	0.0992	0.130	0.0983	0.103	0.0526

Household level clustered standard errors in parentheses. Controls: in grade for CCT (only for 14-year-old cut-off), an indicator for urban, household head characteristics (schooling, gender, age, and indigenous indicator), gender, number of children in the household in following age categories: 0-6, 7-9, 10-13, and 14-17, number of adult men and women, and departamento by year fixed effects. The running variable is the difference between age in months (days in column 8) and the age cut-off a week before the survey date. We include linear splines of the running variable, an indicator for 2014 and after, and an indicator that is one for the children in the corresponding age group. Column 5 also includes quadratic splines of the running variable. Column 6 includes linear splines that that vary across both sides of the cut-off and before and after the law. Column 7 has linear and quadratic splines that vary across both sides of the cut-off and before and after the law. Column 8 uses exact interview date to calculate age at survey. Column 9 omits children within 1 month of the age threshold. Column 10 displays results of an RD on post-law data only. Column 11 excludes municipalities with above median shares of indigenous residents. We use a triangular kernel. The sample includes 2013, 2014, and 2016 for column 8; 2012-2016 for column 11; and 2012-2017 for all other columns.

Table A.10: Difference in Difference - Work Outcomes

Panel A: 14-Year-Old Cutoff

	Work Probability
	(1)
Post × $\mathbb{1}\{\text{Age} \geq 14\}$	-0.0378
	(0.0178)
Obs.	9046
R-squared	0.264
Mean	0.221

Panel B: 12-Year-Old Cutoff

	Work Probability
	(1)
Post × $\mathbb{1}\{\text{Age} \geq 12\}$	-0.0141
	(0.0161)
Obs.	8731
R-squared	0.275
Mean	0.151

Panel C: 10-Year-Old Cutoff

	Work Probability
	(1)
Post × $\mathbb{1}\{\text{Age} \geq 10\}$	-0.0180
	(0.0144)
Obs.	8636
R-squared	0.252
Mean	0.0992

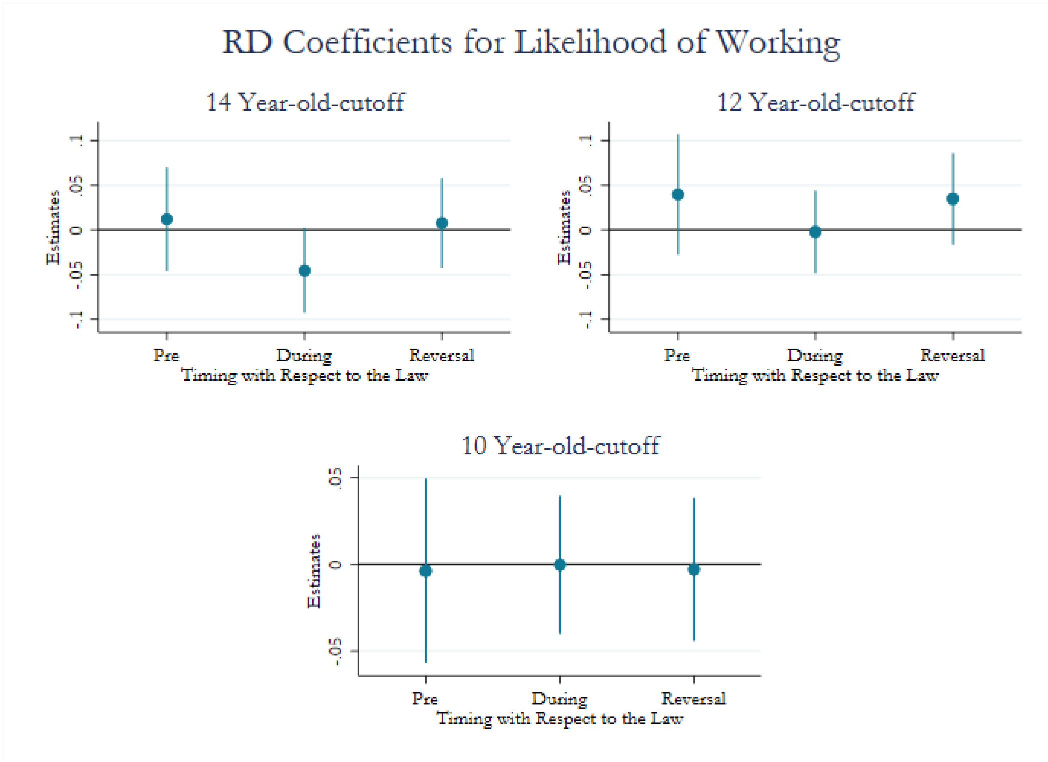
Household level clustered standard errors in parentheses. The control variables are: in grade for CCT (only for 14-year-old cut-off), an indicator for urban areas, household head characteristics (schooling, gender, age, and indigenous indicator), gender, number of children in the household in following age categories: 0-6, 7-9, 10-13, and 14-17, number of adult men and women, and departamento by year fixed effects. The specification includes an indicator for the corresponding age group, an indicator equal to one after the law was established, and an interaction between those two indicators. We use a triangular kernel. The sample includes 2012-2017.

Table A.11: Difference in Difference: Work Probability

	Control: 9 and 14 year olds Work Probability (1)	Control: 7-9 and 14-16 year olds Work Probability (2)
Post $\times$ $\mathbb{1}\{10 \leq \text{Age} < 12\}$	-0.0127	-0.00264
	(0.00944)	(0.00750)
Post $\times$ $\mathbb{1}\{12 \leq \text{Age} < 14\}$	-0.0185	-0.00857
	(0.00945)	(0.00766)
Obs.	26413	39766
R-squared	0.258	0.251
Mean	0.163	0.144

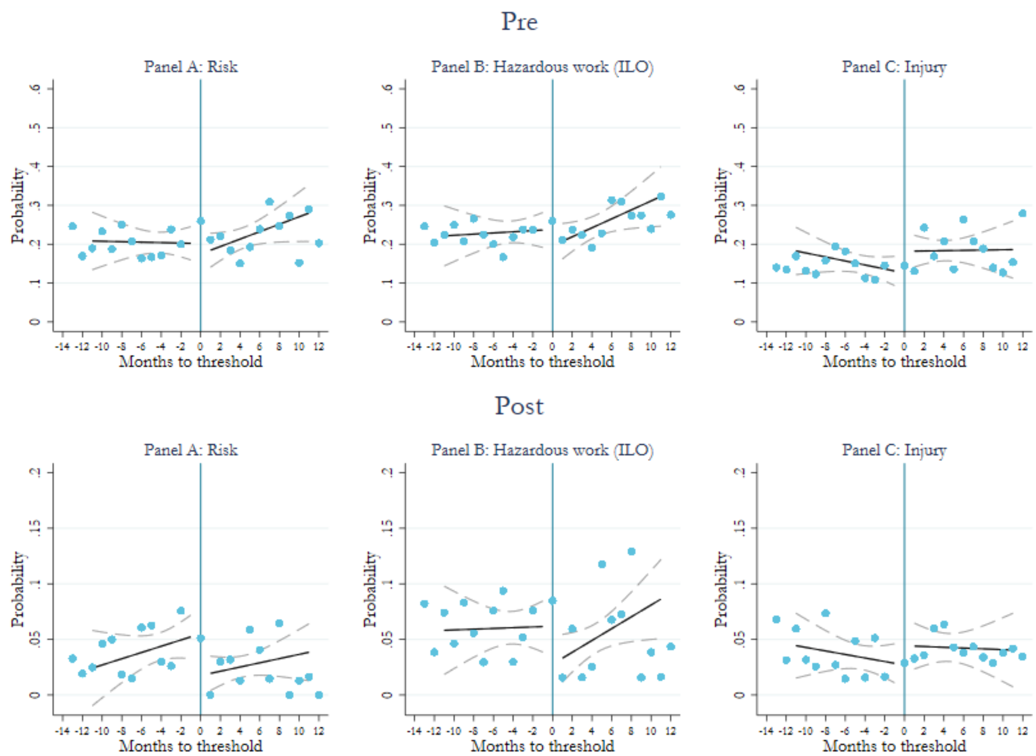
Household level clustered standard errors in parentheses. The control variables are: in grade for CCT (only for 14-year-old cut-off), an indicator for urban areas, household head characteristics (schooling, gender, age, and indigenous indicator), gender, number of children in the household in following age categories: 0-6, 7-9, 10-13, and 14-17, number of adult men and women, and departamento by year fixed effects. The specification includes an indicator for the corresponding age group, an indicator equal to one after the law was established, and an interaction between those two indicators. The sample includes 2012-2017.

Figure A.10: RD Estimates over Time



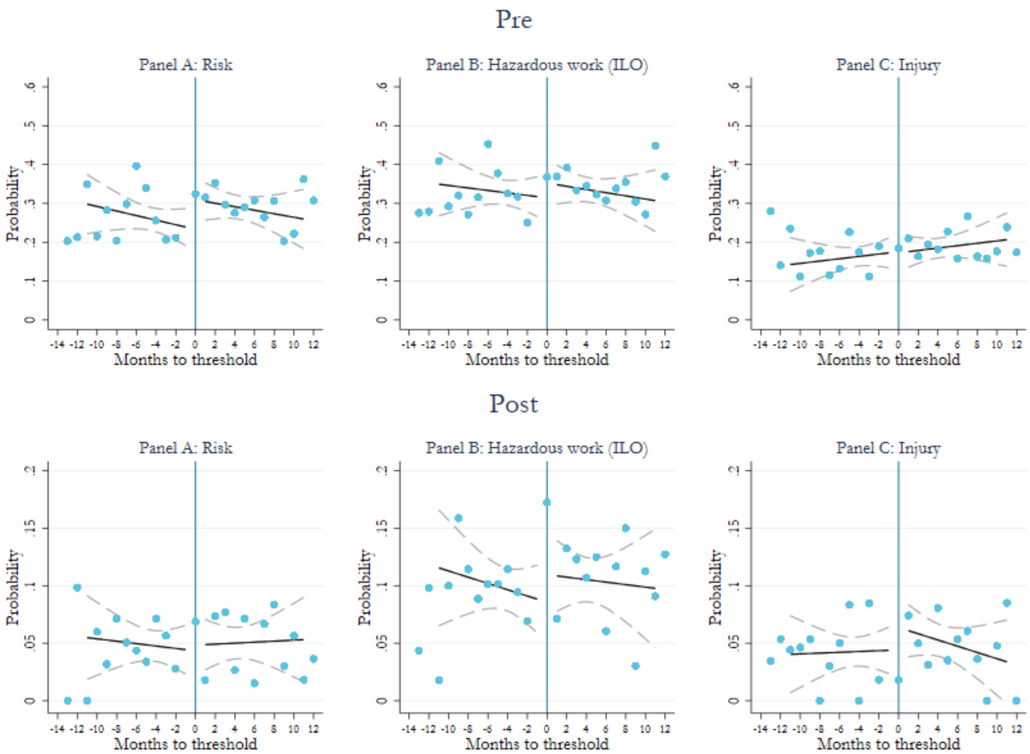
Each coefficient is obtained from a regression discontinuity regression on a sample that pools the rounds of the household survey that correspond to each period: pre-law (2012-13), during the law (2014-2017), and after the reversal (2018-19).

Figure A.11: Job Risks, Hazardous Work, & Work Injuries: 10-Year-Old Cutoff



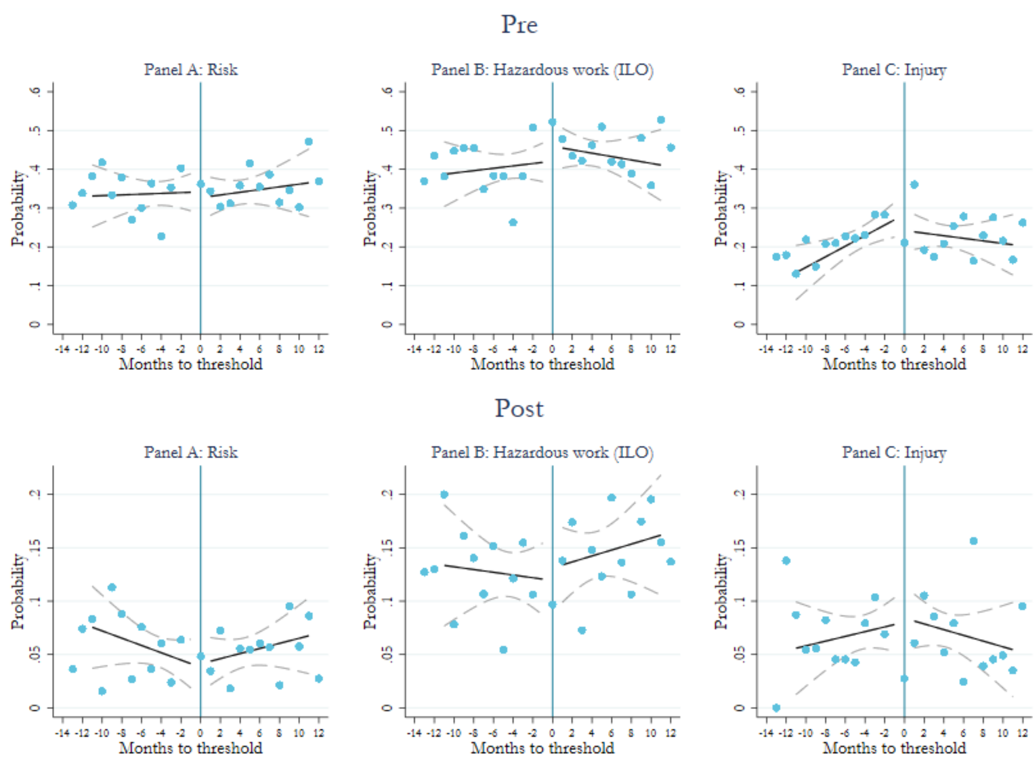
The running variable is the difference between age in months and the age cutoff a week before the survey date. We use a triangular kernel and we reweight the observations as described in Section 4.

Figure A.12: Job Risks, Hazardous Work, & Work Injuries: 12-Year-Old Cutoff



The running variable is the difference between age in months and the age cutoff a week before the survey date. We use a triangular kernel and we reweight the observations as described in Section 4.

Figure A.13: Job Risks, Hazardous Work, & Work Injuries: 14-Year-Old Cutoff



The running variable is the difference between age in months and the age cutoff a week before the survey date. We use a triangular kernel and we reweight the observations as described in Section 4.

Table A.12: Effects of the Law on Job Risks, Hazardous Work, and Work Injuries

Panel A: 14-Year-Old Cutoff

	Faces Risks at Work (1)	Performs Hazardous Work (2)	Has Been Injured at Work (3)
Post $\times \mathbb{1}\{\text{Age} < 14\}$	-0.00492	0.0182	-0.00314
	(0.0285)	(0.0282)	(0.0283)
Obs.	2808	2808	2827
R-squared	0.323	0.479	0.191
Mean	0.200	0.293	0.144

Panel B: 12-Year-old Cutoff

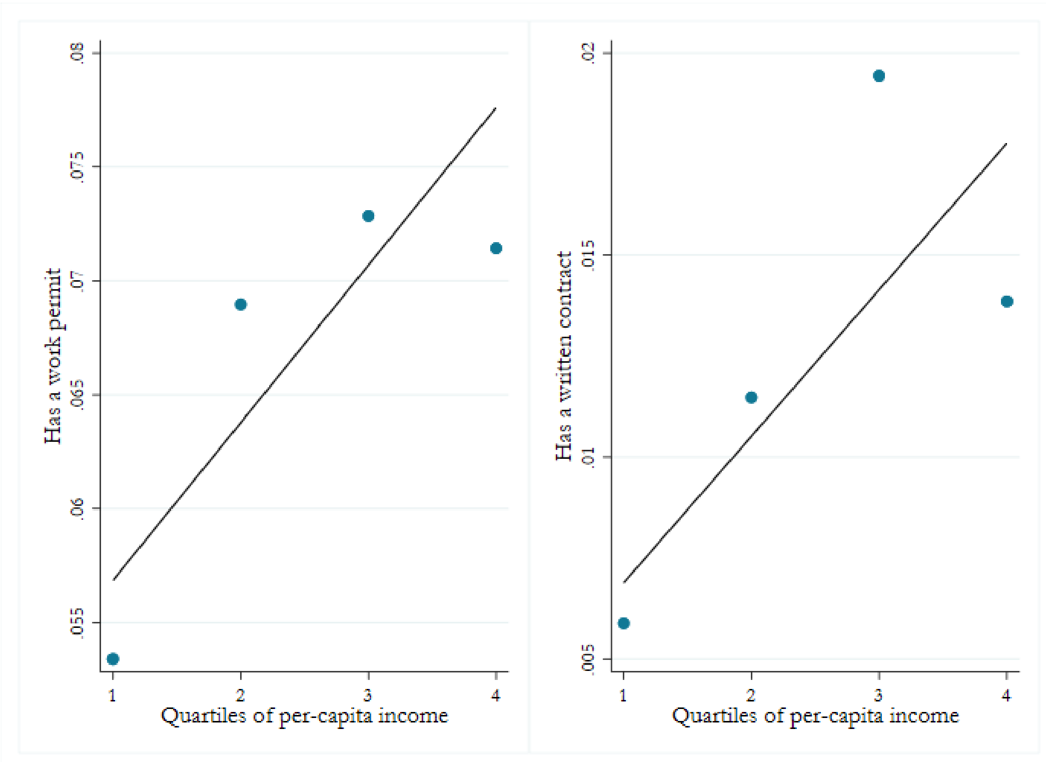
	Faces Risks at Work (1)	Performs Hazardous Work (2)	Has Been Injured at Work (3)
Post $\times \mathbb{1}\{\text{Age} \geq 12\}$	-0.0240	0.0166	-0.0144
	(0.0282)	(0.0283)	(0.0252)
Obs.	2733	2733	2767
R-squared	0.340	0.458	0.178
Mean	0.148	0.201	0.106

Panel C: 10-Year-old Cutoff

	Faces Risks at Work (1)	Performs Hazardous Work (2)	Has Been Injured at Work (3)
Post $\times \mathbb{1}\{\text{Age} \geq 10\}$	-0.0170	-0.000386	-0.0294
	(0.0230)	(0.0233)	(0.0235)
Obs.	2831	2831	2817
R-squared	0.371	0.440	0.214
Mean	0.118	0.139	0.0897

Household level clustered standard errors in parentheses. Control variables: gender, working indicator, urban indicator, and departamento fixed effects. For the risk index and hazardous work regressions, the running variable is the difference between age in months and the age cutoff at the survey date. For the injury index, the running variable is the difference between age in months and the age cutoff a year before the survey date. The specification includes linear splines of the running variable. The bandwidth for all specifications is 12 months. We use a triangular kernel. Survey years: 2008, 2016. We use a reweighting method described in Section 4.

Figure A.14: Work permits and written contracts by per-capita household income



The figures present means of the dependent variables by quartiles of per-capita household income using data on children aged 7 to 18 years old. The left hand side figure reports the probability of having a permit using data from the 2016 Child Labor Survey. The right hand side figure reports the probability of having a written contract with an employer on using data from the 2014-2017 household survey waves.

Table A.13: Heterogeneity by Driving Time from Capitals to MTEPS Offices: Likelihood of Allowed Work

Panel A: 12-Year-Old Cutoff

	All (1)	No Capitals (2)	No MTEPS (3)
Post $\times$ $\mathbb{1}\{\text{Age} \geq 12\}$ for Far	-0.00458	-0.00571	-0.0174
	(0.0505)	(0.0500)	(0.0504)
Post $\times$ $\mathbb{1}\{\text{Age} \geq 12\}$ for Near	-0.00523	0.0232	-0.0128
	(0.0175)	(0.0293)	(0.0446)
Obs.	7313	4325	2918
Mean	0.154	0.226	0.296
P-value of difference	0.990	0.617	0.946
P-value of difference (urban controls)	0.792	0.978	0.975

Panel B: 10-Year-Old Cutoff

	All (1)	No Capitals (2)	No MTEPS (3)
Post $\times$ $\mathbb{1}\{\text{Age} \geq 10\}$ for Far	-0.0765	-0.0765	-0.0710
	(0.0481)	(0.0477)	(0.0484)
Post $\times$ $\mathbb{1}\{\text{Age} \geq 10\}$ for Near	0.00591	0.00632	0.0152
	(0.0151)	(0.0262)	(0.0410)
Obs.	7148	4193	2872
Mean	0.106	0.159	0.213
P-value of difference	0.102	0.127	0.173
P-value of difference (urban controls)	0.118	0.102	0.0800

Household-level clustered standard errors in parentheses. Significance levels denoted by: *** p<0.01, ** p<0.05, * p<0.1. Municipalities that are classified as Far are above the median driving time from a MTEPS office (see Appendix D for details). Control variables: CCT eligibility indicator, urban, HH head characteristics (schooling, gender, age, indigenous indicator), gender, no. of children aged 0-6, 7-9, 10-13, and 14-17, no. of adult men and women, and departamento by year FE. We also include linear splines of the running variable (difference between the cutoff age and age a week before the survey date in months). The specification for the p-value with urban controls additionally includes: post $\times$ urban, treatment $\times$ urban, post $\times$ distance $\times$ urban, and treatment $\times$ distance $\times$ urban. We use a bandwidth of 12 months and a triangular kernel. Survey years: 2012-2016. We also report the mean of the dependent variable for the control group.

Table A.14: Difference in Discontinuity for Firm Size

	Firm Size (All) (1)	Firm Size (Workers) (2)
Post $\times$ $\mathbb{1}\{\text{Age}< 14\}$	-0.216	-0.740
	(0.126)	(0.505)
Obs.	8984	1739
R-squared	0.0879	0.118
Mean	0.966	4.492

Household level clustered standard errors in parentheses. The control variables are: in grade for CCT (only for 14-year-old cut-off), household head characteristics (schooling, gender, age, and indigenous indicator), gender, number of children in the household in following age categories: 0-6, 7-9, 10-13, and 14-17, number of adult men and women, and departamento by year fixed effects. The running variable is the difference between age in months and the age cut-off at the survey date. The specification includes linear splines of the running variable, an indicator that is one in 2014 and after, and an indicator that is one for the children in the corresponding age group. Firm size has been winsorized at the 95th percentile. The sample includes 2012-2017. We also report the mean for the control group.

Table A.15: Wage Difference in Discontinuity - 14-Year-Old Cutoff

	Hourly Wage (1)	Monthly Earnings (2)	$\geq$ Minimum Salary (3)
Post $\times$ $\mathbb{1}\{\text{Age}< 14\}$	0.566	11.99	0.0602
	(1.781)	(136.3)	(0.0925)
Obs.	589	592	592
R-squared	0.217	0.302	0.300
Mean	9.935	1052.5	0.374

Household level clustered standard errors in parentheses. The control variables are: in grade for CCT (only for 14-year-old cut-off), household head characteristics (schooling, gender, age, and indigenous indicator), gender, number of children in the household in following age categories: 0-6, 7-9, 10-13, and 14-17, number of adult men and women, and departamento by year fixed effects. The minimum salary threshold is the one established by the law in 2013, before the law. The running variable is the difference between age in months and the age cut-off a week before the survey date. The specification includes linear splines of the running variable, an indicator that is one in 2014 and after, and an indicator that is one for the children in the corresponding age group. The bandwidth is 18 months. The sample includes working children that report earnings in 2012-2017.

Table A.16: Robustness Checks: Difference in Discontinuity for Risk Outcomes

Panel A: Different Bandwidth Specifications

	Risk Index			Hazardous Work			Injury Index		
				Bandwidth (months)					
		Baseline			Baseline			Baseline	
	6	12	24	6	12	24	6	12	24
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post × Treated	-0.0119	-0.00777	-0.00923	0.0405	0.0210	0.0110	-0.00603	-0.0145	-0.00980
	(0.0243)	(0.0171)	(0.0149)	(0.0270)	(0.0189)	(0.0165)	(0.0213)	(0.0151)	(0.0133)
Obs.	3981	8372	8872	3981	8372	8872	4074	8411	8885
R-squared	0.186	0.179	0.182	0.220	0.203	0.200	0.110	0.107	0.103
Mean	0.158	0.157	0.157	0.217	0.214	0.214	0.113	0.114	0.114

Panel B: Without Controls, Quadratic Splines, and Donut Specification

	Risk Index			Hazardous Work			Injury Index		
	No Controls	Quadratic	Donut	No Controls	Quadratic	Donut	No Controls	Quadratic	Donut
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post × Treated	-0.0126	-0.00697	-0.00340	0.0135	0.0219	0.0162	-0.00749	-0.0149	-0.0363
	(0.0179)	(0.0171)	(0.0182)	(0.0199)	(0.0189)	(0.0199)	(0.0157)	(0.0151)	(0.0166)
Obs.	8372	8372	7325	8372	8372	7325	8411	8411	7351
R-squared	0.109	0.180	0.183	0.106	0.204	0.203	0.0509	0.107	0.109
Mean	0.157	0.157	0.156	0.214	0.214	0.211	0.114	0.114	0.113

Household level clustered standard errors in parentheses. The control variables are: gender, urban indicator, age group fixed effects, household head characteristics (schooling, gender, age, and indigenous indicator), gender, number of children in the following age categories: 0-6, 7-9, 10-13, and 14-17, number of adult men and women, and departamento by year fixed effects. The running variables are the difference between age in months and the age cut-off at the survey date for the risk and hazardous work indices, and the difference between age in months and the age cut-off a year before the survey date for the injury index. The specification includes linear splines of the running variable, an indicator that is one in 2016, and an indicator that is one for the children in the corresponding age group. We use a triangular kernel. The sample includes 2008 and 2016.

B List of Prohibited Tasks under the 2014 Law

Under the 2014 law, children were prohibited from engaging in the following tasks (Authors' translation of original Spanish document):

- Harvesting sugar cane
- Harvesting chestnuts (Brazil nuts)
- Mining
- Fishing in rivers and lakes (other than family or community work activities)
- Brickwork
- Selling alcoholic drinks
- Collecting waste that can affect children's health
- Cleaning hospitals
- Security services
- Live-in domestic work
- Plasterwork
- Agriculture (other than family or community work activities)
- Large livestock tending (other than family or community work activities)
- Work after hours
- Modeling that has an erotic connotation
- Attending to urinals after hours
- Stone cutting / masonry
- Sound amplification

- Handling heavy machinery
- Construction work (other than family or community work activities)
- Guarding cars after hours

C Variable Definitions

- Any work: Indicator equal to one if the child reports working (or temporarily taking time off from their usual job) in the week prior to the survey. Does not include any unpaid household chores, such as cooking, cleaning, or caring for family members.
- Hours worked: Reported hours worked during the week before the survey; takes the value of zero if children report not working. The survey contains data about the average number of days worked in a week and the average number of hours worked per day for each household member age 7 or older. We compute weekly work hours by multiplying the number of days worked per week by the number of daily hours.
- Prohibited work: Indicator equal to one if the child reports engaging in any work as listed in Appendix B.
- Allowed work: Indicator equal to one if the child reports engaging in any other work that is not prohibited as detailed in Appendix B.
- Works more than 30 hrs.: Indicator equal to one if the child reports working more than 30 hours in the week before the survey; takes the value of zero if children report not working.
- Work for self: Indicator equal to one if the child reports working as self-employed or as an unpaid business owner in the week before the survey; takes the value of zero if children report not working.
- Work for others: Indicator equal to one if the child reports working for an external employer or for a family employer in the week before the survey; takes the value of zero if children report not working.
- Faces risks at work: Indicator equal to one if the child reports facing any of the following at work in the week prior to the survey:

  - Dirt or contaminated dust
  - Fire, gas, flames
  - Loud noise or vibrations
  - Extreme heat or cold
  - Dangerous instruments (knives, explosives, etc.)
  - Underground work
  - Work at height
  - Work in water
  - Darkness, isolation, or without ventilation
  - Chemical products (e.g. pesticides, glue)
  - Other risks (given as an option in the survey)

  The indicator is zero if children report not working.

- Performs hazardous work: Indicator equal to one if the child reports facing any of the risks above (except "other") or if the child reports doing any heavy lifting, using heavy equipment, doing night shifts, or working in mining (based on definition by the International Labor Organization (International Labor Organization, 2011)) in the week prior to the survey. The indicator is zero if children report not working.

- Has been injured at work: Indicator equal to one if the child reports having experienced any of the following injuries at work in the year prior to the survey:

  - Superficial injuries or bites, blisters, etc.
  - Fractures or mutilations
  - Dislocation or distention
  - Burns, scalds, or freezing
  - Respiratory problems
  - Sight problems
  - Skin injuries
  - Stomach problems (diarrhea or chemical poisoning)
  - Exhaustion due to task intensity
  - Other injuries (given as an option in the survey)

The indicator is zero if children report not working.

- Attends school: Indicator equal to one if children report attending school regularly (or if they report being on vacation but are enrolled in school) at the date of the survey.

D Measuring driving time to MTEPS offices.

We describe the process for computing the driving time to the nearest MTEPS office below:

- We obtained addresses and coordinates for MTEPS offices from MTEPS's website `https://www.mintrabajo.gob.bo/?page_id=2626`.

- We obtained the coordinates (latitude and longitude) corresponding to the locality where the municipality government is located, typically the locality with the largest population in each municipality. To obtain this information we scraped data from `https://www.municipio.com.bo/`, a website with detailed descriptions of all municipalities in Bolivia. (See, for example, `https://www.municipio.com.bo/municipio-las-carreras.html`)

- For each point (centroid), the travel time to MTEPs offices in the record is calculated (about 8400+ combinations). Then for each municipality, we keep the travel information to the office with the fastest travel by car. Importantly, the algorithm is set to request the API to optimize travel time; therefore, the selected routes are the least time-consuming, although shorter routes (in terms of distance) may be possible. We use two measures to define the closest office to each municipality. First, we estimate the shortest possible distance between each municipality and each MTEPS office (straight line or "as the crow flies" distance). Second, we check for the fastest possible trip by driving. In some cases, where there was no existing network of routes connecting the points, we were not able to compute distance based on travel time. We avoid this problem by using geocoded centroids (Bing) when the issue arises. Specifically, we feed the algorithm a rough location, typically the name of the municipality (e.g., "Las Carreras, Chuquisaca, Bolivia"), from which we get a precise location that we later use to calculate travel routes.

- As a result, for each municipality, we are able to compute two measures of distance: travel time by road and "as the crow flies" distance.

- Based on each measure of distance, we split municipalites in two groups: Near (minimum distance below the cross-municipality median) and Far (minimum distance above the cross-municipality median).