The Effects of Expanding Worker Rights to Children*

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Abstract

One out two working children worldwide works in hazardous conditions. We study the effects of law that introduced benefits and protections for child workers and temporarily lowered the de facto legal working age from 14 to 10 in Bolivia. 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, particularly in areas with a higher threat of inspections. The effects disappear after the law is reversed. We do not find evidence of improvements in work safety. Thus, the effects do not appear to be driven by increased hiring costs to ensure worker safety. Instead, the effects appear to be driven by a reduction in the most visible forms of child work, suggesting that firms and parents (households) may have reduced employment of young children to minimize the risk of being subject to legal and social sanctions. **JEL Codes**: J08, O12, K3.

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1 Introduction

Over 160 million children worldwide are engaged in child labor and roughly 50% (79 million) perform hazardous work (International Labour Organization, 2021). Most working children are likely hired "off-the-books", in precarious conditions and under the radar of workplace regulations. Existing evidence focuses on policies to directly prevent children from working such as child labor bans (e.g, Bharadwaj et al. (2020); Basu and Van (1998); Bargain and Boutin (2021); Piza and Souza (2017, 2016) and Abman et al. (2023)). Much less attention has been paid to policies that aim at improving the working conditions of children or that try to bring child workers "out of the shadows". Moreover, little is known about the effectiveness of policies to protect workers that are typically hired informally, whose participation in labor markets is often socially condemned and rarely legally recognized. These key characteristics of the labor market for children may alter the incentives of households and employers in ways that run contrary to the policy goals, which could have implications for understanding the effects of regulations in other markets that share similar characteristics.

We leverage a unique setting to better understand the effects of policies regulating the working conditions of children and the mechanisms behind such effects. Specifically, we study a policy change that legally recognized child work with the aim of expanding benefits and protections to child workers, similar to those granted to adults working formally. In 2014, Bolivia passed legislation that recognized the work of children as young as 10 years old, whose age placed them below the official minimum working age of 14 years old. The law enabled young children aged 10 to 13 to work legally (subject to obtaining a work permit) while simultaneously extending benefits and protections to these workers.[1] For example, the law entitled working children to adult minimum wages and to 2 paid hours per day to devote to school or study; the law also required that employers guarantee safe working conditions for children.

To ensure enforcement, the government tasked local offices of the Ministry of Labor and Social Protection (MTEPS) with adding child labor inspections to their regular labor

[1]The law allowed children older than 12 to legally work for others and children between the ages of 10 and 12 to work as own-account (self-employed) workers. 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. Thus, the law lowered the de-facto minimum working age from 14 to as young as 10.

and workplace inspections. Nationally, child labor inspections doubled between 2013 (the year prior to the law) and 2017 (Ministerio de Trabajo, Empleo y Previsión Social, 2018; U.S. Department of Labor, 2019). Awareness of the policy change appears to have been widespread, as evidenced by coverage of the law in national and international news outlets and by recorded attendance of official workshops conducted by the MTEPS to educate children, parents, and employers about the law. Indeed, amid high levels of scrutiny, the key features of the law that recognized the work of younger children were reversed in 2018.

We exploit the timing of the changes in legislation and cross-individual variation in the exposure to such changes to empirically estimate the impacts of the law. Specifically, to account for unobserved characteristics of children that vary systematically with age, we employ a difference-in-discontinuity design based on a child's year and month of birth. Using a repeated cross-sectional household survey, we examine differences in work outcomes for children just above and below age thresholds issued by the law across the periods before the law was implemented (2012-2013), during the years in which the law was enforced (2014-2017), and after key components of the law that protected the rights of younger working children were reversed (2018-2019). This strategy allows other determinants of work to vary (smoothly) with age in months and accounts for any preexisting discontinuities in outcomes prior to 2014.

Even though the law legalized child work and entitled child workers to basic rights and protections, its enactment decreased the prevalence of child labor in terms of the likelihood and hours of work. We find that children under 14 (who were newly able to work legally) were nearly 4 percentage points less likely to work when the law was in effect (roughly 16% of the pre-law mean), relative to children above age 14 (who were always allowed to work legally and whose workers' rights were guaranteed prior to and following the law). These effects dissipate after 2018, when key components of the law were repealed. We find no evidence that the law shifted child labor across regulated and unregulated (prohibited) work or in terms of self versus external employment. Despite the decline in child employment, we find no effects on other measures of child time allocation (schooling and chores) or on household outcomes (adult labor supply and household income).

We find that the effects of the law were strongest in areas with higher probability of inspection by regulators, proxied by the distance to the closest regional offices of the MTEPS — the government agency in charge of conducting labor inspections. These results are robust to excluding the largest urban centers, which suggests that they do not simply capture urban-rural heterogeneity. They also suggests that enforcement, although imperfect, may have increased the perceived threat of inspection. This result is consistent with other studies that analyze how firms respond to regulations and tax-compliance efforts using distance to the regulator as a proxy for enforcement Almeida and Ronconi (2016); McKenzie and Seynabou Sakho (2010).

We analyze several potential mechanisms behind the declines in child work. On the labor-demand side, the law may have increased the relative costs of legally employing younger children among compliant firms, which may have reduced the demand for labor of young workers (Lazear, 1990; Autor et al., 2007). Thus, one should expect the working conditions for young children to improve. To this end, we study the effects of the law on job characteristics of child workers: namely, job safety and pay, two job attributes specifically targeted by the law. For job safety, we use two surveys that focused specifically on the nature of child work. We find that the law had no statistically significant impacts on the riskiness of child work or on injuries sustained while at work. We do observe non-statistically significant increases in wages among children that remained employed. However, as few children work for likely compliant, formal firms (3.2%), we believe that the increases in direct costs of complying with the law are unlikely to fully explain the overall decline in child work. Contrary to its key objective, the law does not appear to have improved the working conditions of child workers. Thus, there appears to be a net loss of of worker welfare; some child workers lose their jobs and yet this does not lead to increased benefits for child workers who retain their jobs.

Alternatively, the enactment of the law and the high level of scrutiny around it may have induced avoidance behavior from both sides of the market. Among informal employers, the 2014 law may have increased the perceived threat of general labor inspections as both child labor and general labor inspections were carried out by the same government agency (MTEPS), incentivizing them to not hire children who were visible targets of the new legislation in order to remain "under the radar" of regulators. Among parents (households), the law may have intensified scrutiny surrounding child work and thus increased the risk of legal and social sanctions to parents of younger child workers, reducing the supply of child labor. Several pieces of evidence support this mechanism. First, the law's effects are larger in areas that are closer to regional offices of the enforcement agency, where the threat of inspections and social stigma are likely to be higher. Second, the declines in employment due to the law are driven by declines in the probability of working outside home at fixed establishments, which are more visible and traceable by inspectors; in contrast, we find no changes in employment in less conspicuous and trackable modes, such as at work occurring within the home or in mobile locations. In addition, we find suggestive evidence of substitution from visible to less visible work locations among the children who remained employed. Finally, the fact that the effects dissipate after the law is reversed suggest that the avoidance behavior was driven by employers (as opposed to parents), as the threat of social sanctions and social stigma would have implied a more persistent pattern of avoidance behavior.

Our results are robust to a battery of robustness checks. First, we show that they 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, and functional form specifications for the running variable. Second, we show that our results are robust to using an alternative difference-in-difference research design to relax the role of smoothness for identification imposed by our difference-in-discontinuity design. Third, we show that our results are not explained by changes in employment in areas with a higher presence of indigenous communities for which child employment (either for the family or the community) is often conceived as an integral part of engagement with the community and traditions.

Our results also provide novel insights to the literature evaluating the effects of child labor legislation. Previous studies have focused on the effects of child labor bans on work outcomes. We contribute to this literature in two ways. First, we leverage a unique policy change that instead of banning child labor, legally recognizes and regulates the work of children. While some studies find that child labor bans little overall effect on child work (Edmonds and Shrestha, 2012; Bargain and Boutin, 2021)or induce a decline in child work (Piza and Souza, 2016, 2017), our results suggest that legally recognizing the work of younger children does not increase child labor. Instead, our results are consistent with evidence of unintended consequences of child labor bans.[2] The declines in child labor disappear when the legislation is reversed — which essentially amounts to reinstating the ban on legal work under the age of 14. This finding is consistent with the effects to those in Bharadwaj et al. (2020) and Abman et al. (2023); removing legal status and worker protections for younger children actually increases the likelihood that they work.

Second, previous studies of the impacts of child labor legislation have mostly focused on the effects on employment and provided little evidence on working conditions for children. Leveraging novel data, we contribute by providing new evidence on the effects (or lack thereof) of child labor legislation on job safety, a critical dimension of child work and oft-cited rationale for child labor legislation. The results demonstrate that recognizing and regulating child labor does not yield improvements in children's working conditions and instead appears to have increased the perceived risk of labor inspections and thus the cost of hiring child workers, which ultimately affects child work in ways that can contradict policymakers' intentions.

Finally, beyond the labor market for children, our results contribute to our understanding of how policy affects the labor market outcomes of vulnerable workers hired informally. In the context of sex workers, legalization and regulation can fail to improve worker safety and well-being (Gertler and Shah, 2011; Ito et al., 2018); some evidence suggests that this could be due to workers' reluctance to visibly identify themselves as sex workers by obtaining the necessary certification to work legally (Manian, 2021). Our contribution is to document a novel channel through which regulation can hamper employment of targeted workers: avoidance behavior driven mostly by informal employers.

[2] Other studies explore the impacts of minimum working age laws in the U.S. in the early 20th century. For example, Moehling (1999) finds little effect of minimum ages laws; Manacorda (2006) demonstrates that though minimum age laws reduce work for targeted children, this is often compensated by an increase in the labor supply of siblings such that the overall effect on child work in the household is negligible.

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.[3] 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.[4] 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 of such policy suggestions, demanding the recognition of labor as an integral and unavoidable part of children's development.[5] 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.

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

[4]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.

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

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, 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.[6]

2.2 Changes in legislation after 2014

We exploit the enactment of new child labor legislation in 2014 and its subsequent reversal in 2018 as sources of plausibly exogenous variation to estimate the impact of legalizing the work of younger children and increasing worker protections. 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

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

lowered the de facto minimum working age and expansions of worker protections to younger workers.

Table 1 summarizes the key changes induced by the law for each age group. The new law set a baseline minimum working age of 14 years, but it also allowed children aged 10 to 13 years to work legally in certain capacities and subject to obtaining work authorizations. Specifically, the new law permitted children aged 10 to 11 to work as self-employed workers[7] and children aged 12 to 13 to work as both self-employed workers 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).[8] 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.[9] 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. Communal work – culturally valued activities taking place in indigenous, Afro-Bolivian, and intercultural communities – was

[7]Self-employed – or independent – work is defined by the law as work that is carried out by the child without any employer relationship. It is distinct from work for the family. Examples include street vending and washing vehicle windows at traffic lights.

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

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

allowed and was subject to separate rules and procedures set and implemented by indigenous jurisdictions. However, children that engaged in family and communal work were still granted the same rights and protections as all child workers.[10]

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. Thus, starting in 2018, the government no longer issued or implemented a program for protecting the rights of working children under the age of 14 (Defensoría del Pueblo, 2022).

2.3 Enforcement and awareness

The 2014 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 2014 law (Article 139). Even prior to the 2014 law, MTEPS offices were in charge of verifying the ownership of valid business registrations, conducting general labor and technical inspections, and carrying out inspections related to preventing forced labor.[11] A key component of enforcement was age verification for children. In Bolivia, age verification is relatively straightforward and feasible, due to near universal birth registration and widespread identity cards. According to the 2012 Bolivian Census, 99% of children in the age range of 9-15 years old were registered at birth at the civil registry, and 72.5% of them owned ID cards. Moreover, ID cards are required to obtain government benefits from

[10]One unfortunately common example of prohibited exploitative family work relates to children being sent by their parents to beg in the streets. In some cases, parents were accused of family violence for forcing their children to beg(Los Tiempos, 2013a).

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

Bolivia's conditional cash transfer program for school-age children.

There are 25 regional Ministry of Labor and Social Protection offices located in the most populated municipalities of the country (see Appendix Figure A.1). Using data from annual MTEPS reports, Figure 1 shows that child labor inspections increased considerably in 2014 and rose thereafter. There were on average around 300 child labor-specific inspections per year conducted during the period following the law's enactment. The total number of inspections (labor and technical) conducted by the MTEPS also increased after 2014, suggesting that the increase in child labor inspections did not crowd out – and perhaps even crowded in – other inspections conducted by the MTEPS.

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. Inspections carried legitimate consequences for employers; in 2018, 17% of child labor inspections were turned over to the DNAs for resolution (Ministerio de Trabajo, Empleo y Previsión Social, 2018). 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.[12] 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 and (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.

The threat of an inspection by the MTEPS office is likely to affect employers' compliance with new regulations and their demand for child labor. Formal firms 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—the larger sector in the economy[13] — 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. The fact that the MTPES was in charge of both the child labor and regular registration inspections may have increased the perceived risk

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

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

of inspection among firms hiring young children. 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).[14] 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).

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, 2013b), which may have amplified awareness about the policy change.[15] In Appendix Figure A.2 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 (2018), suggesting that the general public was aware of the policy changes.[16] 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. 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).

Throughout the paper, we interpret the enactment of the 2014 law as both a legal recogni-

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

[15] 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).

[16] Appendix Figure A.2 suggests potential for anticipatory effects because the reversal was announced in February 2018 but not implemented until December 2018. However, this does not represent an issue for our analysis because the survey data we use (described further in Section 3) is collected in November and December of each year and we treat 2018 as a post-reversal year.

tion of the work of younger children and as an expansion of worker rights for this group. The descriptive evidence on enforcement and awareness suggests that these legislative changes were perceived as important and relevant for firms and families. Accordingly, we interpret the 2018 reversal as an abrupt cessation of both legal recognition and rights for younger working children.

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.[17]

As discussed in Section 2.2, exposure to different dimensions of the 2014 law (and its later amendment) 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.

Work is measured by an indicator of whether a child worked at least one hour during the week preceding the interview.[18] 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 (self-employment), work for others (family or external employer), prohibited work (employment in activities that are

[17] 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.

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

prohibited under the law for all children under age 18, such as mining), and allowed activities (those that are allowed and regulated under the law).[19] Finally, we also measure labor force participation, which includes both those who are working and those who are unemployed but actively searching for jobs. We examine the role of enforcement of the law using data on the locations of regional MTEPS offices, which we describe in more detail in Section 5.2 and Appendix D.

To better understand the mechanisms behind the main results, we use information from the household survey on job attributes (namely, wages and the size of firms children work for). Additionally, we leverage detailed information on the locations where child work takes place (e.g., at home, as a street vendor, or at an establishment with a fixed location) as well as involvement with risky tasks (including, among others, working under extreme temperatures or working in an area exposed to fire, flames, or contaminated dirt and dust) and injuries at work (such as skin injuries, fractures, and respiratory complications) 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).[20]

Panel A of Table 2 reports summary statistics for children age 9 to 15 years old during the pre-law period (2012-2013). Before the policy change, 14% of children in the sample worked. Among working children, the average number of weekly work hours is 21 and over 19% of working children worked more than 30 hours per week. Self-employment is somewhat rare; less than 2% of working children worked for themselves prior to the 2014 law. Work for others is largely made up of work for a family employer (88%). 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,[21] regardless of whether they are family operated or not (see Panel A of Table 3); 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. Family work is largely driven by agriculture

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

[20]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. We discuss this reweighting method in more detail in Section 6.1. We also give more detailed descriptions of variables in Appendix Section C.

[21]Formality is defined by whether the firm is formally registered with the national tax authority.

and retail, while work for others is more diversified, although still dominated by retail and agriculture. Children tend to work outside home, mostly in fixed establishments, regardless of whether their employer is a household member or not (see Panel A of Table 3), although children working for external employers are more likely to work in mobile locations.

Panel B of Table 2 shows that roughly 56% of working children are engaged in risky activities and 34% of working children report having experienced a job-related injury in 2008. Children's exposure to risk and injury are high in both work for family and work for employers (Panel B of Table 3).

4 Empirical approach

4.1 Identification

To identify the causal effects of the exposure to the law, we exploit two sources of variation. First, under the 2014 law, whether and which type of jobs children were allowed to work changed discontinuously at three age thresholds: 10, 12, and 14. Second, we exploit the variation in the timing of the law and its reversal to net out preexisting differences in outcomes across children of different age groups potentially related to the pre-2014 minimum working age of 14.

One key concern is that time varying shocks can differentially affect work outcomes of children based on their age, as there is a steep age-gradient in work probability (see Appendix Figure A.3); for example, 17-year-olds are more than twice as likely to work as 10 year-olds and the probability of working continuously increases by age in months. Thus, it is likely that an aggregate shock to labor markets disproportionately affects the children that are more likely to work. To address this concern, we propose an empirical design that exploits the discontinuous changes in exposure to the law at each age threshold, while allowing for a (continuous) age gradient in outcomes. This strategy 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.

We combine identification at thresholds with temporal variation in the enforcement of the law to account for any preexisting differences in work outcomes that predated the law's implementation. By relying on *local* comparisons around age thresholds, our empirical strategy helps control for potential time varying shocks with differential effects based on age. One limitation of this approach is that it enables us to estimate only short-term responses around a narrow time window after children change exposure status. We discuss alternative specifications to circumvent this issue in Section 5.3.

More formally, we use a difference-in-discontinuity specification. 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:

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

where $Y_{i,t}$ is a work outcome for child i in survey year t, $Age_{i,t}$ is the age of child i in months at the beginning of the relevant recall period (which differs by outcome) for survey wave t.[22] We define age relative to the start of the survey recall period because we need to capture the age eligibility for legal work (and thus worker protections) that is relevant for the work outcomes reported in the survey. c is the relevant cutoff age related to the key policy changes induced by the new law (at ages 10, 12, and 14). T_i is an indicator of whether child i is exposed to the policy change associated to each cutoff. 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 and do so with protections and benefits; children aged 14 and older were legally allowed to work even under the preexisting law. For the 10- and 12-year-old cutoff, T_i 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. With these definitions,

[22] 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.

the interpretation of T_i is consistent across all thresholds, in that all treated children have newly expanded working rights under the 2014 law relative to control children. Law_t is an indicator identifying the years in which the law was enforced (2014-2017), while $Reversal_t$ identifies the years after the reversal of the law (2018-2019).[23] Finally, $\epsilon_{i,t}$ is an error term.

For all thresholds, the parameter of interest are β_1 and β_2, which captures changes in work outcomes of children marginally exposed to each dimension of the law, relative to those just on the control side, between the periods in which the law was enforced and repealed with respect to the pre-law period.

We also include a vector of demographic household and child characteristics that are unlikely to vary due to the program ($x_{i,t}$).[24] 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: 0-6, 7-9, 10-13, and 14-17 and number of adult men and women; whether the household is located in an urban area; and the child's gender. We also include a full set of departamento-by-year fixed effects ($\delta_{d,t}$) to flexibly account for regional time-varying shocks.[25]

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.[26]

To account for the age gradient in work outcomes, we use a linear specification of the running variable 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 5.3. We estimate equation (1)

[23]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.

[24]We include covariates to increase precision, though we show that our results are robust to specifications without controls in Section 5.3.

[25]Departamento is an administrative/geographic unit roughly comparable to a U.S. state.

[26]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).

using triangular kernels that assign a higher weight to observations closer to the eligibility cutoff and conduct inference using standard errors clustered at the age-in-months level (the level at which treatment varies) to account for correlated shocks within age groups.

We estimate equation (1) using a twelve-month bandwidth from each age cutoff. This bandwidth is narrower than the mean squared error (MSE) optimal bandwidth proposed by Imbens and Kalyanaraman (2012), which ranges from 13 to 25 months for all our main outcomes. However, selecting a narrower bandwidth in our setting 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.[27] 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. In Section 5.3, we show that our results are robust to using narrower and wider bandwidths.

4.2 Threats to identification

Manipulation. The validity of our diff-in-disc 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 a 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 rates of children around each cutoff. Specifically, the concern would be that survey respondents are less likely to truthfully report the age of children younger than 14 years old. Appendix Figure A.4 plots the distribution of observations around the cutoffs, focusing on children with birth dates within a year of each cutoff (the bandwidth of our baseline specifications). It shows no

[27] 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 control group relative to the 12-year-old cutoff.

evidence of discontinuous changes at the cutoff either when we compare pre- to post-law periods and post-law to reversal periods.

We more formally test for discontinuous changes at each cutoff in the *differences* in densities between the pre-and post-law periods, and the post-law and reversal periods—i.e., the difference-in-discontinuities analogue of the traditional manipulation test in regression discontinuity designs. Appendix Figures A.5 - A.7 plot the differences in densities between periods as a function of the running variable, for each cutoff using household survey data.[28] We find no evidence of discontinuous changes in the differences in densities across all cutoffs at a 5% confidence level. In the case of the 14-year old cutoff, there appears to be a significant difference in densities between the post-law and the law reversal period at 10% (p-value=0.098). Such difference is small (-0.0007) and implies that, per 10,000 observations, there were 7 fewer observations corresponding to children just above 14 years old. In Appendix Section 5.3, we show that our results are robust to excluding observations very close to the cutoff (and where this small discontinuity could be relevant). In addition, we discuss additional checks for measurement error in Appendix Section 5.3.

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.1 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.

[28] Specifically, we follow Grembi et al. (2016) and, for each bin around each cutoff, compute the differences in densities before and after the policy change. We then fit a linear polynomial at each side of the cutoff and compute the difference at each cutoff point.

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. We focus on the impacts around the 14-year-old threshold, which speak to the combined effects of legalizing and regulating both self-employment and work for others, because there is a substantially higher rate of working children around this cutoff. Figure 2 reports flexible difference-in-discontinuity (i.e., event study-style) estimates of the effect of the law around the 14-year-old cutoff using a variation of equation (1) that allows the effects of the law to vary over time by grouping observations in two-year bins.[29] The work probabilities of 13-year-old (treated) children —whose work was newly regulated by the 2014 law— decline with respect to that of 14-year-old (control) children after 2014. These differences disappear after the law was reversed (2018). Overall, the results suggest that the 2014 law reduced employment for children around the 14-year-old cutoff. We do not observe substantial differences for children around the 12- and 10-year-old cutoffs in Appendix Figure A.8.

Figure 3 plots work probabilities as a function of age (in months) relative to the 14-year-old cutoff before, during, and after the implementation of the 2014 law change. During the pre-law period, there is no discontinuous change on work outcomes around the cutoff. This suggests that the preexisting minimum working age was not a binding constraint to child labor. In contrast, we find a discontinuous change around the cutoff after the policy change. Relative to 14-year-old (control) children, marginally younger (treated) children were less likely to work while the 2014 law was in effect.[30] This difference disappears after the key components of the law recognizing and regulating the work of younger children are reversed in 2018. For children around the 12- and 10-year-old cutoffs (for whom child work is less

[29] We group observations in two-year bins to gain precision amid the reduced number of observations per survey wave.

[30] In Appendix Table A.2, we test the extent to which the changes in work probabilities during the implementation of the law (middle panel in Figure 3 are explained by a discontinuous increase in working probabilities among 14-year-old (control) children instead of declines in work probabilities among treated children. Specifically, we compare changes in work outcomes before and after the implementation of the law among 14-year-olds and 15-year-olds. If the effects were driven by increased work by 14 year-olds – for example, if the 2014 law simply made clear that age 14 was the acceptable age for work – we would expect to find an increase in work for this group relative to 15-year-olds during the implementation of the law. Column 2 of Appendix Table A.2 shows no evidence that this is the case.

common), we observe no discontinuities around the cutoffs during the implementation of the law (see Appendix Figures A.9 and A.10).

We now turn to the regression-based evidence. Table 4 reports the effect of the law on work outcomes around the 14-year-old cutoff. We find that the probability of work declines by 3.94 percentage points for 13-year-old children (a 16% 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. Finally, these declines in employment translate into similar declines in labor force participation (column 7). We discuss potential mechanisms in detail in section 6.

The coefficients associated with the periods following the 2018 reversal of key protections for younger workers under the law validate our empirical approach. Relative to pre-implementation period, there are no substantial differences between marginally exposed and unexposed children when the key protections regulating the work of children under the age of 14 are no longer enforced. The magnitudes of the coefficients associated with the post-reversal period are small and suggest that the changes in work outcomes induced by the enactment of the law fully dissipate after the reversal.

Interestingly, the estimated effects of the reversal of the law (i.e., the removal of legal work status and worker rights) are similar in magnitude to those found in Bharadwaj et al. (2020), who study the effects of a child labor ban in India. 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[31], whereas Bharadwaj et al. (2020) find that the ban results in a 22% increase in

[31] In column 1, the difference between the Post-law and Post-reversal coefficients suggest that the reversal increased the work probability of 13-year-olds by 3.5 percentage points, relative to the periods in which the law was enforced.

work for children under 14 relative to the pre-ban mean. They are also qualitatively similar to estimates of *increases* in child labor when regional trade agreements include child labor bans and *declines* in child labor when regional trade agreements do not include child-labor bans (Abman et al., 2023). In contrast, our results suggest different effects from a Brazilian law that increased the legal working age from 14 to 16; studies of the Brazilian law found no overall effects (Bargain and Boutin, 2021) or declines in child work (Piza and Souza, 2017).

Appendix Table A.3 corroborates 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. 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. Panel A 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. Similarly, we find no statistically significant effects of the 2014 law on work probabilities at the 10-year-old cutoff (Panel B, column 1). 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 also examine the impact of the law on schooling but find no statistically significant effects (see column 1 of Appendix Table A.4).[32] 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. This finding is bolstered by qualitative evidence that finds that for many working children, work and study are complements; and in many cases, work provides the means to pay for schooling-related expenses (Defensoría del Pueblo, 2022). Additionally, we estimate the effects of the law on the time children spend performing household chores (in the past week) but we find no evidence that the law impacted children's time allocation along this dimension; the estimated effect is small and statistically insignificant (column

[32]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.

2).[33] We also find that the 2014 law had no significant effects on the labor supply of other household members or on household income per capita (see Appendix Table A.5)

5.2 The role of enforcement

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 through inspections. These inspections complemented the labor and workplace safety inspections already being conducted by the MTEPS before the law, which verify firms' formal registration and compliance with general worker regulations. As discussed in Section 2.3, MTEPS inspections — both generally, and specifically for child labor — increased after the enactment of the law (see Figure 1). However, the threat of enforcement varies across localities; there is substantial variation in a locality's proximity to the nearest regional MTEPS office (see Appendix Figure A.1).

We exploit this cross-locality variation to verify whether the effects that we document are driven by children working in areas where inspections are more likely. 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 4 illustrates that adult 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, even after controlling for job and worker characteristics that are likely correlated with distance to MTEPS offices (such as worker education, sector of work, and firm tax registration – a marker of firm formality).

Accordingly, we exploit cross-municipality variation in the driving time to regional MTEPS offices 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"

[33] Note that the data on participation in domestic chores comes from the ENTI 2008 and the ENNA 2016, described in more detail in Section6.1. As there is no data beyond 2016, we cannot estimate a post-reversal coefficient for this outcome.

from the nearest regional Ministry of Labor (MTEPS) office, where "far" is defined as above the median driving time.[34] 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 and later. Thus, the sample for Table 5 does not include data past 2016 and we cannot estimate a "Post-reversal" coefficient. This exercise is similar in spirit to that in Bargain and Boutin (2021), who study heterogeneity in a Brazilian law's effect using state-level variation in labor inspection rates.

Panel A in Table 5 illustrates that the law appears to decrease 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. This remains true when we further restrict the sample to municipalities that do not contain an MTEPS office (column 2), illustrating that the result is not being driven only by large, mostly urban municipalities.[35] 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), and are estimated with greater precision. 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.[36] These results are consistent with those in Bargain and Boutin (2021), who find that the effects of a Brazilian child labor law are detectable only in states with a high potential threat of inspection. 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.6), likely due to the low incidence of overall child labor among younger children. Overall, the results suggest that enforcement was a key driver of the decline in child work due to the law. We discuss the mechanisms behind these results in Section 6.

[34]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.

[35]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.

[36]To show that these results are not driven by differences across urban and rural areas or by geographical variation in baseline child labor rates, in Appendix Table A.7, we also report the results after additionally controlling for all possible interactions between the treatment variables, the post-law indicator, and urban status/district-level baseline child labor rates (as measured in 2012). Adding these controls does not change the results in a meaningful way; the estimated effects in areas near and far from MTEPS offices are very similar to those reported in Table 5.

5.3 Robustness

Alternative Specifications. We show that our results are robust to alternative specification choices that are common in Regression Discontinuity designs. First, we show that our results are robust to different analysis bandwidths. 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.8 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 the latter case. Second, our point estimates are robust to excluding demographic controls from our main specification (see column 4). Third, our results are robust 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 (columns 5-7).

Measurement error. 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, we show that our results are very similar 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). It is worth noting that the fact that the results are robust to this alternative specification also attenuates the concerns that the small difference in densities between the post-law and reversal periods at the 14 year-old cutoff reported in Appendix Figure A.5.

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 around the cutoffs. However, we

[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).

think that this is unlikely for several reasons. First, we observe no discontinuities in either survey responses (Appendix Figure A.4) or in reported work probabilities in the pre-law period (Figure 3), 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 evidence consistent with this hypothesis. In column 1 of Appendix Table A.2, we compare 12- to 13-year-olds. Both of these age groups are subject to the same legal status and worker protections, but if the law underscored the harm work causes younger children, we should expect work to decline even more for 12-year-olds relative to 13-year-olds; yet, the results indicate that there are no differences in the responses of the two age groups.

Difference in differences approach. As discussed in Section 4.1, one limitation of our difference in discontinuities design is that it only enables us to make local comparisons just when children change their treatment status based on their age. If the enforcement of the law varied with how far children are from the cutoff, then our main estimates would be capturing lower bounds. Appendix Table A.9 reports results from three difference-in-difference specifications that allow for comparisons of work outcomes of treated and control children over time, regardless of their proximity to the cutoffs. Column 1 shows results for a simple diff-in-diff model that uses 14-year-old children as controls for 13-year-old children, controlling for a set of demographic attributes as in our main specification. The point estimates are remarkably similar to those in our main specification. Columns 2 and 3 show results of alternative specifications that use two definitions of control groups made up of younger and older children (9 and 14 year old children, and 7-9 and 14-16 year old children, respectively). Column 2 yields qualitatively similar results than our main specification. However, when we expand our pooled control group to include children as young as 7 and as old as 16 as in Kamei (2020), who analyzes the impact of this law but only using one post-period survey wave, 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.[38]

Accounting for communal work. The 2014 law allowed the participation of children in community activities – culturally valued activities taking place in indigenous, Afro-Bolivian, and intercultural communities – without age restrictions as long as the activities contribute to children's integration into the community or to the development of skills and only if it did not represent exploitation, contradict a child's rights, or entail potentially risky activities. Examples of these include working in a communal farm or working for community organizations. While our data do not allow us to identify specific types of community labor (to which the law exceptions apply), work for this purpose appears to be rare; in 2016, only 6% of working children report maintaining family or community customs as the main reason for working.[39]

In column 11 of Appendix Table A.8 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 communal work related to cultural traditions are more prevalent

[38]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. Second, using children that are much younger and older than the treatment group is prone to violations to the parallel trends assumption stemming from differential responses to labor-market shocks by age.

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

and where these exceptions to the law are more likely to apply.[40] The point estimates are remarkably similar to those from our main specification, despite excluding 45% of the sample.

6 Mechanisms

The mechanisms behind the negative effects of recognizing work for younger children and extending worker protections to such workers are not ex ante obvious. Traditionally, the trade-off between increased worker protections and reductions in labor demand is linked to the idea that as firms comply with new regulations, the cost of hiring increases, which in turn depresses the demand for labor (Lazear, 1990). However, in markets where most employers are informal and operate under the radar of regulation, firms may also reduce the demand for newly entitled workers to continue avoiding attention from inspectors and regulators. Likewise, in markets associated with a large degree of social stigma, the increased scrutiny amid new regulations may also deter labor supply. We discuss these mechanisms below.

6.1 Compliance costs

One key objective of the new law was to improve the working conditions of children. One possible explanation for the overall declines in employment among younger children is that the law increased the safety of child work (at a cost to employers) and subsequently reduced the demand for child workers. We explore this hypothesis by analyzing two child labor surveys on risky tasks and injuries at work: the ETI 2008 and the ENNA 2016.

There are some empirical challenges related to these data. First, the surveys come from different sampling frames. The ETI 2008 samples children that are likely to work while the ENNA 2016 is nationally representative of all children. We combine the two surveys by reweighting the data so that observations that are similar (based on observables) across survey waves are given higher weight.[41] In Appendix Table A.10, we show balance on

[40] 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.8 does not include data from 2017.

[41] 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

these characteristics across the age thresholds and survey rounds (after re-weighting) using random subsamples that were not used in calculating the weights.[42] Second, with only two survey waves of these data, we have much smaller samples to assess the effects of the law on job safety outcomes separately at each age threshold. To 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 relative to each specific threshold.[43] We additionally include cutoff fixed effects, which ensures that our estimates continue to be based on local comparisons around each age cutoff.[44] Finally, there are no surveys on risky tasks and work injuries after 2016, so we cannot study the effects of the 2018 reversal on these outcomes.

We find neither significant or substantial declines in the incidence of risk (column 1) and injuries at work among treated children (column 3)— who are newly granted worker protections under the 2014 law (see Table 6).[45] We are able 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. The results are robust to alternative specifications.[46] The lack of substantial declines in risky activities suggest that compliance with costly safety regulations was not a

Probit model based on demographic characteristics of children and their households. We then use these predicted probabilities (propensity scores) to construct weights. 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.

[42]We follow this approach to ensure that balance on targeted variables is not simply a consequence of overfitting. We used 70% of the observations to estimate the propensity score p and the remaining 30% to test balance.

[43]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.

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

[45]We display graphical evidence in Appendix Figure A.11. There is no evidence of differential changes in sample composition across any of the age cutoffs in the child labor survey (Appendix Table A.11).

[46]Appendix Table A.13 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).

key driver of the decline in employment among 13-year-olds.

One concern with our empirical approach 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. Columns 2 and 4 in Table 6 show that we are unable to detect significant differences in risk exposure and injuries when we focus only on children who report working. While the point estimate on risk suggests a three percentage point decline (column 2), the point estimate on the probability of suffering an injury at work remains unchanged (column 4). Second, when we replicate our analysis around each age cutoff in Appendix Table A.12, we find relatively small, non-significant effects for younger children—those for which we found no effects of the law on work probabilities. Reassuringly, we do not find neither substantial nor significant effects around the 14-years-old cutoff either.

Another possibility is that the law directly increased the costs of hiring younger children relative to children age 14 or older, as it established that even children age 13 or younger were entitled to receive the minimum monthly salary. In column 5 of Table 6, we use the subsample of working children who report wages to estimate differences in wages induced by the policy change. We find that the hourly wages of working children just under the age of 14 are ten percent larger than those just above age 14 during the period in which the law was enforced, and that these differences vanish when the 2014 worker protections were no longer enforced.[47] However, these differences are not significant at conventional levels. Moreover, even taking this difference at face value, the potential increase in wages is unlikely to account for the negative effect of the law on child work, as the subset of working children that report receiving a salary is very small (712 children out of roughly 3,600 working children and 18,000 children overall in the sample around the 14-year-old cutoff).

Thus, contrary to previous studies analyzing the impact of increased worker protections (Lazear, 1990; Autor et al., 2007; Almeida and Carneiro, 2012), the evidence from Bolivia suggests that the effects of extending rights to child workers does not seem to be explained by increased worker benefits and hiring costs for complying firms.

[47] To increase sample size, given the low survey response rates related to child earnings, we estimated the wage equation around a wider bandwidth (18 months) around the 14-year-old cutoff.

6.2 Avoidance behavior

Most children work for informal firms. Such firms — by virtue of hiring "off the books" — face different incentives after the introduction of new regulations recognizing the work of younger child workers, who before the policy change were hired illegally. Given the context of high public scrutiny of the 2014 law, hiring younger children — a demographically distinguishable group — may increase the visibility of firms and thus the risk of labor inspections. To the extent that firms internalize this increased risk, they may choose to avoid hiring younger children in order to remain under the radar of regulation. Likewise, as most informal firms are family-owned, the new regulations may have deterred parents from employing their children in their firms. In addition, the high level of scrutiny and social stigma around the new regulations may have deterred other parents from allowing their children to work from others. In equilibrium, with higher perceived risks of regulatory and social sanctions, the new regulations may have triggered avoidance behavior on both sides of the market.

These incentives are consistent with the institutional setting in Bolivia: The entity in charge of child labor inspections (MTEPS) is also in charge of general labor and workplace inspections, and thus firms that draw attention from child labor inspectors will also likely be subject to general inspections. Indeed, as discussed in Section 5.2, we find that child employment declined in areas located nearer to MTEPS offices where visibility to inspectors is particularly relevant (see Table 5).

One empirical implication of this mechanism is that the declines in child work should be driven by firms with greater visibility. We thus distinguish between children who work outside the home at a fixed establishment and children who work either at home or outside home in non-fixed, mobile locations. The intuition is that inspectors may be better able to track firms operating at fixed external establishments (e.g., a factory, or a shop) as opposed to those operating inside the owner's home with no external visibility or those that frequently change locations and are less traceable (e.g., family farms or street vendors). Panel A in Table 7 reports treatment effects of the law on the probability of working at a fixed establishment, on the probability of working at home, and on the probability of working at a mobile work location, around the 14-year-old cutoff. We observe a 5 percentage point decline in the probability of working at a fixed location (column 1); this effect is statistically significant and meaningful in magnitude (about a 33% decline). In contrast, we find no effects on the probability of working at home or in a mobile location (columns 2 and 3). This suggests that the decline in overall employment among 13-year-olds is largely explained by a contraction in the employment of children who worked in more traceable and visible locations.[48]

In Panel B of Table 7, we examine how the law affected the composition of employment. For this, we focus on the subsample of employed children before and after the policy change. We find that relative to the pre-law periods, the share of 13 year old children working in fixed, high-visibility establishments declines by 10 percentage points when the law was enforced, relative to 14 year old workers (column 1). In contrast, we observe a 8 percentage point increase in the share of younger children (under 13 years old) who are employed in mobile, less traceable locations. Even though these estimates are not causal, they suggest a change in the composition of employment that is consistent with younger children moving to less visible forms of employment.

Another dimension of visibility of work is firm size. Previous studies find that larger firms are more likely to be targeted by regulators than small firms (Almeida and Carneiro, 2009).[49] This implies that larger firms have a greater incentive to reduce hiring of young workers targeted by the legislation because of the higher threat of inspection. As a result, young children may end up working for smaller (i.e., less visible) firms; indeed, in column 4, we find that children under 14 work for smaller firms while the law is in place.

The results in Table 7 are also consistent with avoidance behavior on the part of families. One possibility is that the law brought more awareness to the potential adverse consequences of work for young children. If the law resulted in a greater perceived stigma related to child work — particularly for those under 14 — then reductions in visible forms of work may also reflect parents' unwillingness to work their children in ways that are visible to others in the community.

[48] These results complement and strengthen those in Bargain and Boutin (2021), who find "very mild evidence" that the effects of a Brazilian child labor law were concentrated in activities that were more "inspectable."

[49] 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.

To test if the declines in employment are exclusively explained by firm adjustments or also driven by household decisions, we analyze the difference between the effects of the 2014 change in regulation on employment and labor force participation. If the effects were exclusively driven by firm's avoidance behavior, then one should observe more muted effects on labor force participation as laid-off children look for new jobs. In contrast, if both firms and parents react to the new regulations, the effects on employment and on labor force participation should be quantitatively similar. Indeed, in Table 4, we see that the effects of the law on work status (column 1) are virtually identical to those on labor force participation (column 7). Combined with the results in Table 7, suggests that the reduction in employment we find is consistent with parents removing their children from forms of work that are visible to the community, inspectors, or both.

6.3 Costs of work permits

An alternative explanation for our central finding that younger children are less likely to work during years the law was in place is that 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. This process has been described as "highly tedious", characterized by long waits at DNAs and hospitals and requiring considerable time and effort on the part of administrators, parents, and employers (Defensoría del Pueblo, 2022).[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.12), who are least able to pay the costs of the obtaining a permit. Moreover, the process may discourage employers from hiring children

[50] Low investments in and lack of easily accessible DNA offices exacerbated the cost of obtaining permits. 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). Likewise, in a recent report from a survey to 59 out of 339 municipalities, the People's Advocate Office (*Defensoría del Pueblo*) found only 12% of surveyed municipalities kept records of child and adolescent labor (Defensoría del Pueblo, 2021).

under the age of 14 (who were required to obtain work authorization from DNAs). As one child states, the authorization process is one reason "why they don't give young people so much work" (Defensoría del Pueblo, 2022, p. 103).

7 Conclusion

Overall, we find no evidence that recognizing the work of young children and extending worker protections to them increased child work in Bolivia. In fact, we find that children under age 14 were less likely to work in permitted and regulated activities after the passage of the law (relative to children over age 14). We posit that this is primarily due to the new regulation inducing avoidance behavior by firms and parents. The 2014 new regulations increased the perceived costs of employing younger children— 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 returned to pre-law levels for children under the age of 14.

Importantly, we find that the law did not significantly affect children's riskiness of work or injuries on the job. This stands in contrast to one of the purported aims of the policy to make child work safer. Together with the observed decline on employment among younger children due to the law, these results suggests an overall worker welfare loss: the law appears to have reduced employment among the children that it intended to protect without improving the worker conditions of the children who kept their jobs.

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.

Finally, perhaps a silver lining of the Bolivian case is the powerful role of public scrutiny, which appears to have led to avoidance behavior. Increasing the salience of social issues that are very quite sensitive may be able to achieve what seems challenging for regulation in settings with limited state capacity.

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Table 1: Key Dimensions of Child Labor Legislation

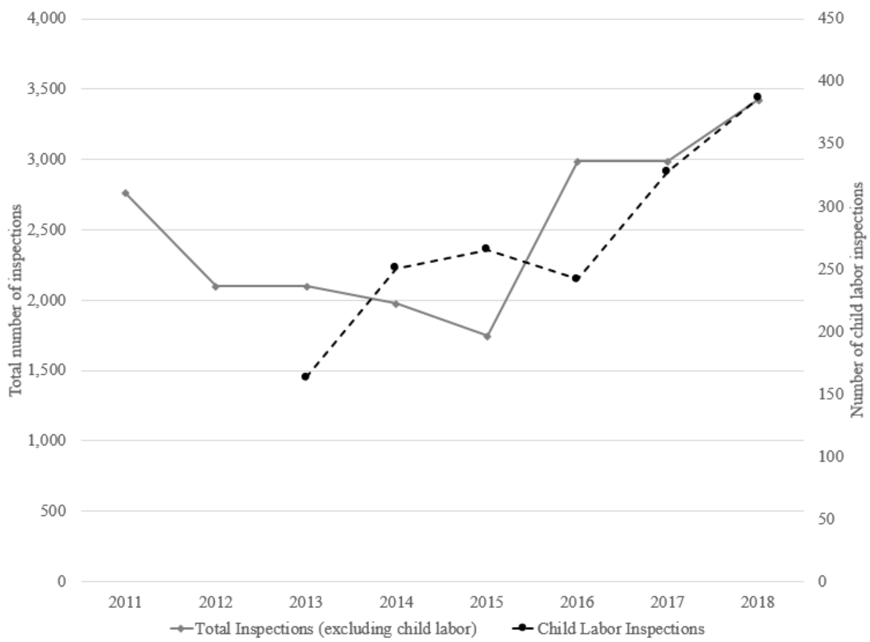
	Before 2014 (Pre-Law)	2014-2018 (During Law)	After 2018 (Post-Reversal)
Age<10	No legal work	No legal work[1]	No legal work[1]
$10 \leq$Age< 12	No legal work	Legal to engage in independent work[2]	No legal work[1]
$12 \leq$Age< 14	No legal work	Legal to engage in independent work or work for others[2], with worker benefits and protections[3]	No legal work[1]
Age≥ 14	Legal to engage in independent work or work for others[2], with worker benefits and protections[3]		

[1] Starting in 2014, children of all ages were allowed to engage in communal work – culturally valued activities taking place in indigenous, Afro-Bolivian, and intercultural communities – as long as it did not infringe on their rights and protections as guaranteed by law.

[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 1: Ministry of Labor Inspections over Time



Note: Data on inspections is obtained from 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).

Table 2: Descriptive Statistics (Pre-Law)

Panel A: Household Data

	All Children	Working Children
	(1)	(2)
Household & Child Characteristics		
HH Head Years of Schooling	8.632	-
HH Head is Male	.787	-
HH Head Age	44.126	-
HH Head is Indigenous	.358	-
Household Size	5.603	-
Child is Male	.504	-
Child Work & Schooling Outcomes		
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	.006	.039
Allowed work	.137	.961
Work $\geq$ 30 hrs/week	.027	.192
Attends school	.972	.913
Observations	8699	1244

Panel B: Job Attributes (Household Survey)

	Working Children
	(1)
Firm size (median)	4
Hourly wage (Bolivianos)	7.025
Firm pays taxes	.032
Works Outside of Home in Fixed Location	.864
Works Outside of Home in Mobile Location	.107
Works at Home	.029
Observations	1230

Panel C: Job Attributes (Child Labor Survey)

	All Children	Working Children
	(1)	(2)
Risk at work	.28	.548
Injured at work	.168	.325
Observations	4159	1984

Notes: The table shows the mean of the variables, except for firm size, where the median is displayed. Definitions of the variables appear in 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 Panels A and B, and 2008 in Panel C. Observations of the child labor survey are reweighted using the method described in Section 6.1.

Table 3: Descriptive Statistics by Employer Type (Pre-Law)

Panel A: Household Data

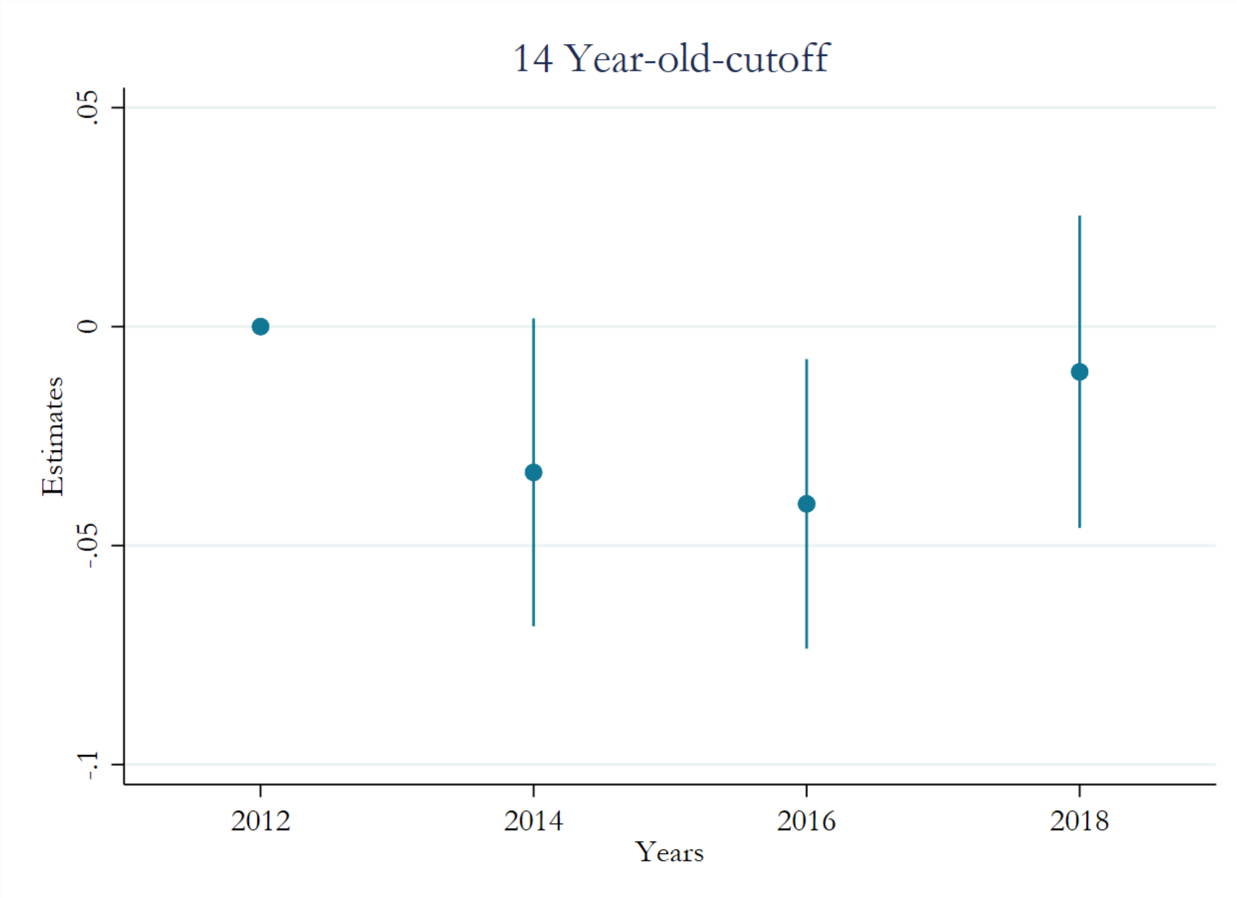
	Work for External Employer (1)	Work for Family Employer (2)
Firm size (median)	4	4
Hourly wage (Bolivianos)	6.291	18.557
Formal Firm	.098	.026
Works Outside of Home in Fixed Location	.64	.899
Works Outside of Home in Mobile Location	.36	.071
Works at Home	0	.03
Sector		
Agriculture	.144	.772
Sales and retail	.232	.101
Other	.624	.127
Observations	113	1094

Panel B: Child Labor Survey Data

	Work for External Employer (1)	Work for Family Employer (2)
Risk at work	.679	.537
Injured at work	.447	.314
Observations	186	1741

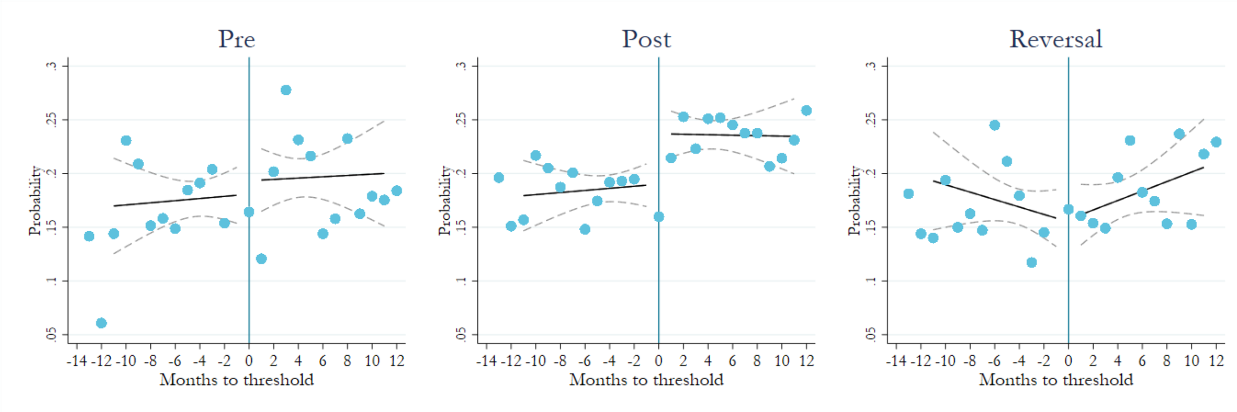
Notes: The table shows the mean of the variables, except for firm size, where the median is displayed. Definitions of the variables appear in Appendix C. The sample in both panels includes children from ages 9 to 15. The survey years are 2012-2013 in Panel A, and 2008 in Panel B. Observations of the child labor survey are reweighted using the method described in Section 6.1.

Figure 2: Changes in Work Probability relative to Pre-law Periods at the 14-Year-Old Cutoff



Note: The figure reports changes in work probabilities for 13-year-olds relative to 14-year-olds over time (grouped in two-year bins), with respect to the years preceding the policy change (2012-2013). The specification includes linear splines of the running variable, defined as the difference between the cutoff age and age a week before the survey date in months. Control variables: CCT eligibility indicator, 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. The sample includes 2012-2019. The 95% confidence intervals are based on standard errors clustered at the age in months level.

Figure 3: Work Probabilities at the 14-Year-Old Cutoff (Before, During, and After the Law)



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, the post sample includes 2014-2017, and the reversal sample includes 2018-2019. We use a triangular kernel.

Table 4: Difference in Discontinuity Effects of the Law on the Work Probabilities, Hours, and Occupation for the 14-Year-Old Cutoff

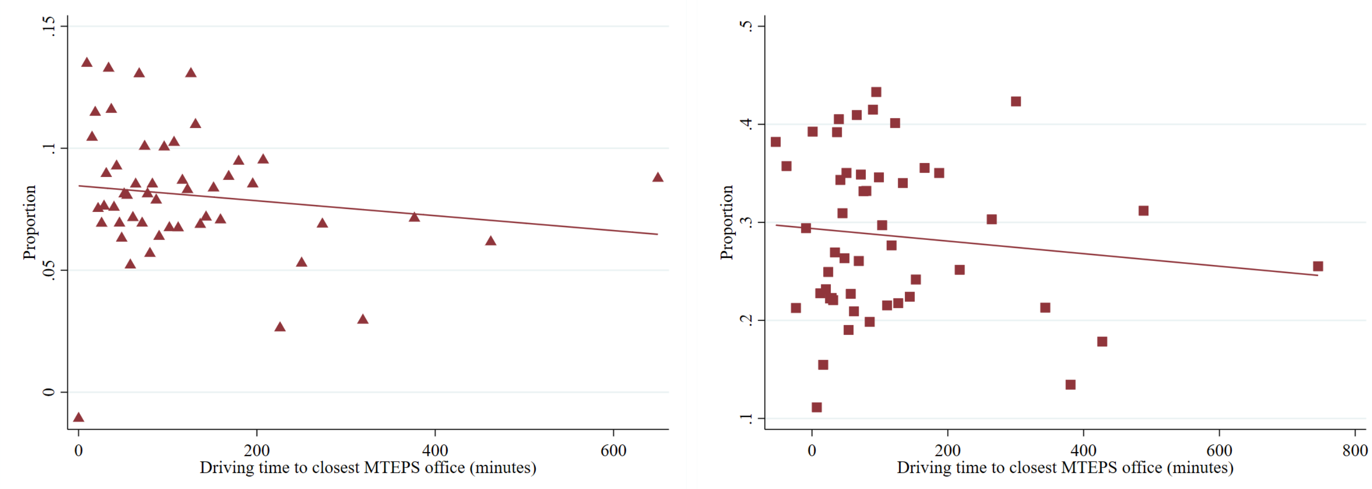
	Any Work	Hours Worked	Work for Self	Work for Others	Prohibited Work	Allowed Work	Labor Force Participation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post Law $\times$ $\mathbb{1}\{\text{Age}< 14\}$	-0.039**	-0.969*	-0.002	-0.037**	0.004	-0.043***	-0.040**
	(0.017)	(0.526)	(0.004)	(0.017)	(0.006)	(0.015)	(0.017)
Post Reversal $\times$ $\mathbb{1}\{\text{Age}< 14\}$	-0.000	0.508	-0.000	-0.000	0.018	-0.019	0.002
	(0.019)	(0.562)	(0.005)	(0.019)	(0.012)	(0.018)	(0.019)
Obs.	11991	11991	11991	11991	11991	11991	11991
Mean	0.180	4.397	0.00490	0.175	0.0114	0.169	0.185

Notes: Age in months by year clustered standard errors in parentheses. Significance levels denoted by: *** p<0.01, ** p<0.05, * p<0.1. Control variables: CCT eligibility indicator, 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.

Figure 4: Compliance with Labor Regulations and Travel Time to Inspectors (Pre-Law)

(a) Formal Labor Contracts for Workers

(b) Health Insurance for Workers



This figure presents the proportion of adult workers (age 18+) that have a formal work contract (panel a) and have health insurance through their employer (panel b), by quantiles of driving time to the nearest MTEPS office (50 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, sector of work fixed effects, and a dummy for firm tax registration.

Table 5: Heterogeneous Effects of the Law by Distance from MTEPS Offices
(Difference-in-Discontinuity)

Panel A: Driving Time

	Dependent Variable: Works	
	All	No MTEPS Offices
	(1)	(2)
Post × $\mathbb{1}\{\text{Age}< 14\}$ for Far	0.002	0.002
	(0.061)	(0.058)
Post × $\mathbb{1}\{\text{Age}< 14\}$ for Near	-0.030	-0.074*
	(0.021)	(0.043)
Obs.	7650	2984
Mean	0.180	0.317
P-value of difference	0.644	0.338

Panel B: Direct Distance ("as the crow flies")

	Dependent Variable: Works	
	All	No MTEPS Offices
	(1)	(2)
Post × $\mathbb{1}\{\text{Age}< 14\}$ for Far	0.009	0.007
	(0.060)	(0.056)
Post × $\mathbb{1}\{\text{Age}< 14\}$ for Near	-0.037*	-0.103**
	(0.021)	(0.044)
Obs.	7650	2984
Mean	0.180	0.317
P-value of difference	0.496	0.175

Notes: Age in months by year 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 distance from a MTEPS office. 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). 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 pre-law period.

Table 6: Effects of the Law on Risk, Injuries at Work and Wages

	Faces Risks at Work (1)	Faces Risks at Work (2)	Has Been Injured at Work (3)	Has Been Injured at Work (4)	Log Hourly Wage (5)
Post Law × Treated	-0.008	-0.038	-0.015	-0.015	0.103
	(0.017)	(0.035)	(0.014)	(0.029)	(0.180)
Post Reversal × $\mathbb{1}\{\text{Age}< 14\}$					-0.012
					(0.180)
Obs.	8372	2914	8411	3208	712
Mean	0.281	0.536	0.188	0.327	6.656
Sample	All Children	Working Children	All Children	Working Children	Paid Workers

Notes: Age in months by year clustered standard errors in parentheses. Significance levels denoted by: *** p<0.01, ** p<0.05, * p<0.1. The sample in columns 1 to 4 comes from the child labor survey, and the sample in column 5 comes from the household survey. 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 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. In columns 1 to 4, we do a stacked difference in disconinuity by multiplying the running variable by -1 for the 13 and 14 year-olds age group for interpretability. For column 5, we do a difference in discontinuity in which 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. The bandwidth for all specifications is 12 months. We use a triangular kernel. Survey years: 2008 and 2016 in columns 1 to 4 and 2012-2019 in column 5. We use a reweighting method for columns 1 to 4 described in Section 4.

Table 7: Effects of the Law on Job Location and Firm Size

Panel A: All Children

	Firm Size	Works in Fixed Location Out of Home	Works in Mobile Location Out of Home	Works at Home
	(1)	(2)	(3)	(4)
Post Law × $\mathbb{1}\{Age< 14\}$	-0.267**	-0.051***	0.009	0.003
	(0.121)	(0.014)	(0.008)	(0.004)
Post Reversal × $\mathbb{1}\{Age< 14\}$	-0.033	-0.009	0.005	0.003
	(0.112)	(0.016)	(0.013)	(0.003)
Obs.	11918	11991	11991	11991
Mean	0.853	0.149	0.0248	0.00588

Panel B: Working Children

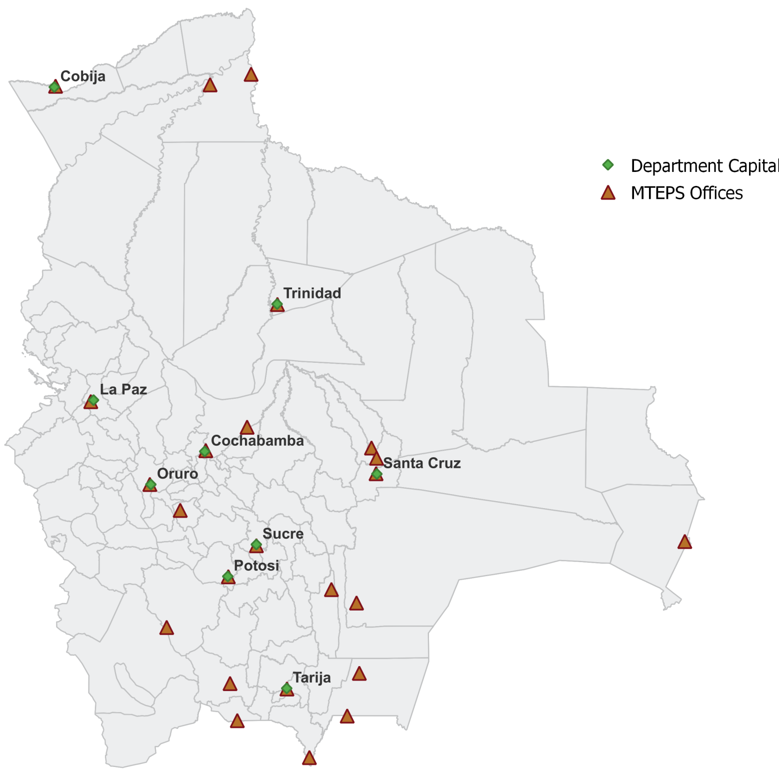
	Firm Size	Works in Fixed Location Out of Home	Works in Mobile Location Out of Home	Works at Home
	(1)	(2)	(3)	(4)
Post Law × $\mathbb{1}\{Age< 14\}$	-0.726	-0.098***	0.078**	0.021
	(0.473)	(0.035)	(0.034)	(0.018)
Post Reversal × $\mathbb{1}\{Age< 14\}$	-0.359	-0.043	0.022	0.021
	(0.383)	(0.050)	(0.054)	(0.018)
Obs.	2250	2323	2323	2323
Mean	4.796	0.829	0.138	0.0327

Notes: Age in months by year clustered standard errors in parentheses. Significance levels denoted by: *** p<0.01, ** p<0.05, * p<0.1. The sample in Panel A includes all children, while the sample in Panel B is restricted to working children only. Control variables: CCT eligibility indicator, 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: 2012-2019.

ONLINE APPENDIX

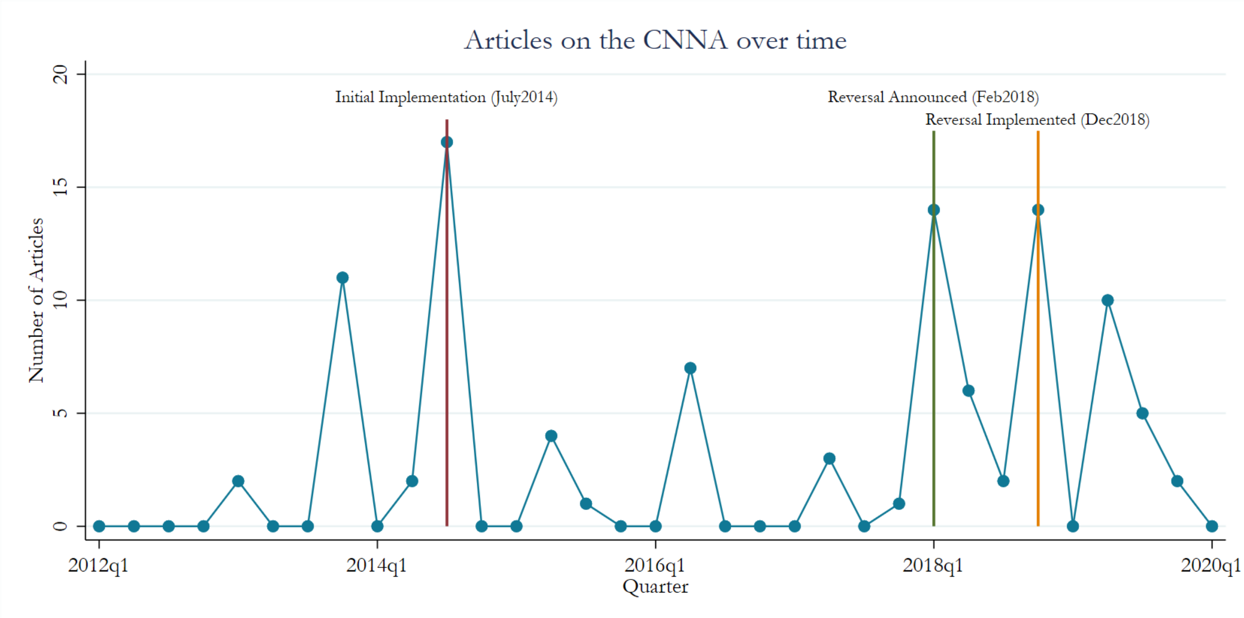
A Appendix Figures and Tables

Figure A.1: Ministry of Labor Offices



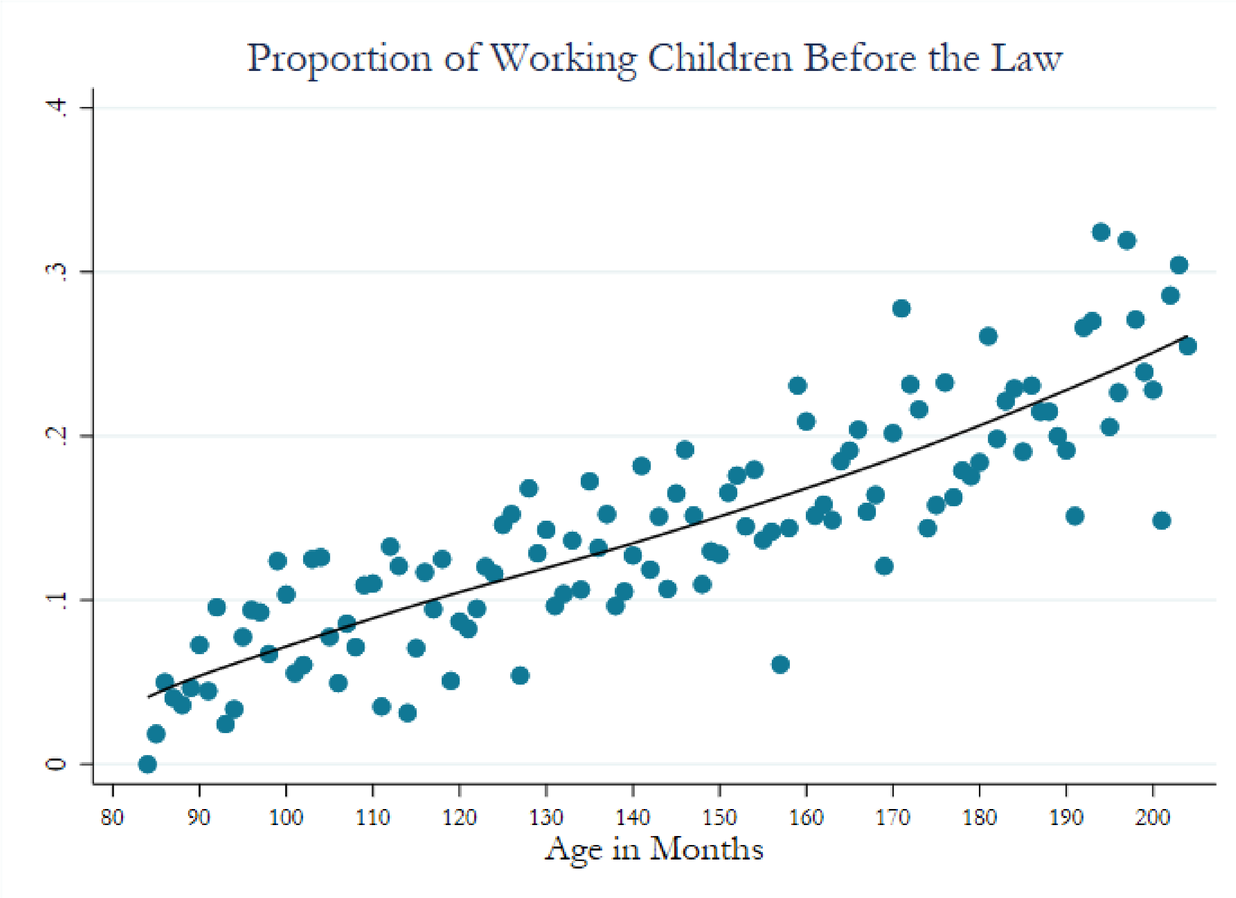
The addresses of permanent MTEPS offices can be found here: `https://www.mintrabajo.gob.bo/?page_id=2626`.

Figure A.2: Articles on the 2014 Law over Time



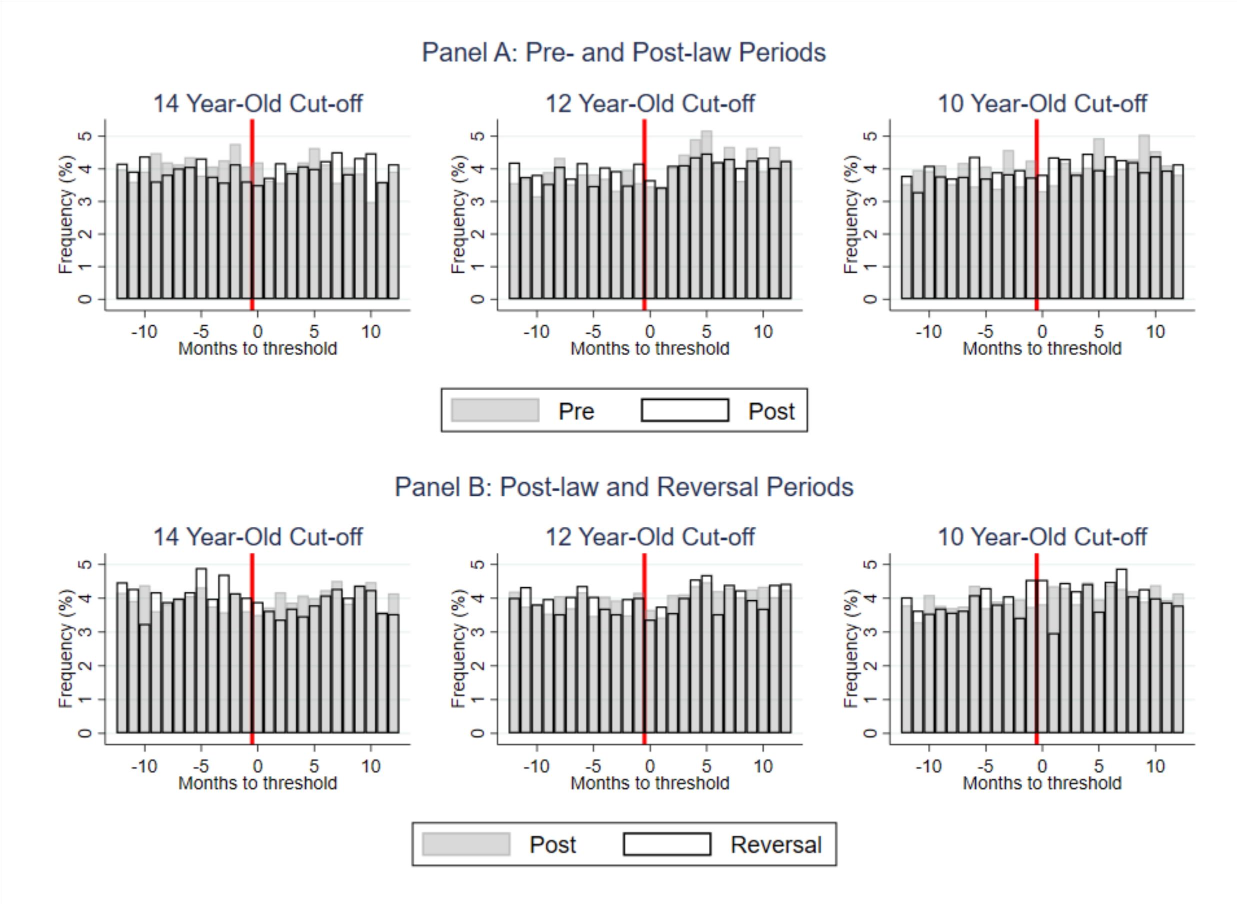
This figure tracks the number of articles concerning the 2014 law scraped from 43 national and regional Bolivian newspapers between 2012 and 2020. Articles that both mentioned the 2014 law and child labor were included.

Figure A.3: 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.

Figure A.4: 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 post sample includes 2014-2017 and the reversal sample includes 2018-2019. Both panels use data from multiple rounds of hosuehold surveys.

Table A.1: Balance Table: Difference in Discontinuity - Household Survey

Panel A: 14-Year-Old Cutoff

	Schooling (HH head)	Male (HH head)	Age (HH head)	Indigenous (HH head)	Male (child)	HH size
	(1)	(2)	(3)	(4)	(5)	(6)
Post Law × $\mathbb{1}\{\text{Age}< 14\}$	0.197	-0.020	-0.412	0.027	-0.035	-0.072
	(0.308)	(0.020)	(0.530)	(0.022)	(0.028)	(0.094)
Post Reversal × $\mathbb{1}\{\text{Age}< 14\}$	0.310	-0.011	0.370	0.032	-0.009	-0.111
	(0.345)	(0.025)	(0.572)	(0.026)	(0.034)	(0.088)
Obs.	11498	11498	11498	11498	11498	11498
Mean Control	8.509	0.798	45.16	0.347	0.499	5.562
Mean Treated	8.595	0.760	45.49	0.366	0.484	5.532
Joint test P-value = .632						

Panel B: 12-Year-Old Cutoff

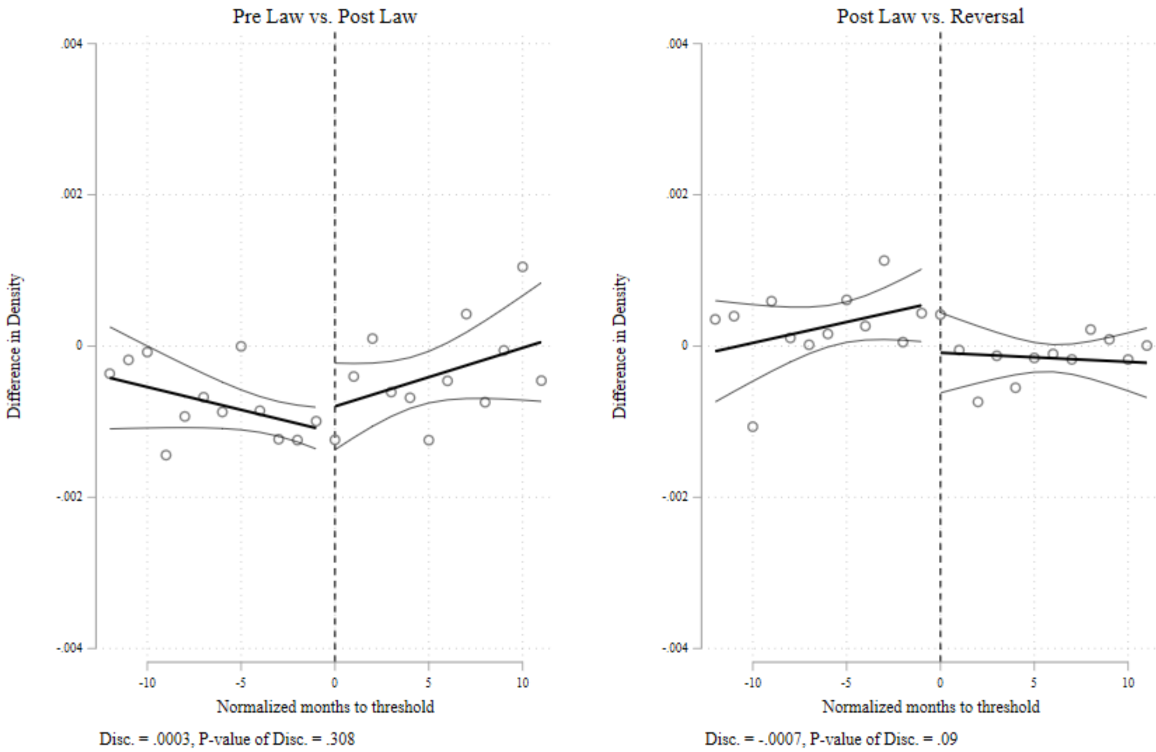
	Schooling (HH head)	Male (HH head)	Age (HH head)	Indigenous (HH head)	Male (child)	HH size
	(1)	(2)	(3)	(4)	(5)	(6)
Post Law × $\mathbb{1}\{\text{Age}\geq 12\}$	-0.184	-0.028	0.064	-0.020	-0.050*	0.093
	(0.322)	(0.019)	(0.571)	(0.027)	(0.030)	(0.109)
Post Reversal × $\mathbb{1}\{\text{Age}\geq 12\}$	-0.295	-0.011	-0.133	0.013	-0.043	-0.002
	(0.357)	(0.021)	(0.570)	(0.029)	(0.037)	(0.111)
Obs.	11194	11194	11194	11194	11194	11194
Mean Control	8.653	0.790	44.26	0.356	0.522	5.619
Mean Treated	8.574	0.776	43.75	0.354	0.486	5.657
Joint test P-value = .514						

Panel C: 10-Year-Old Cutoff

	Schooling (HH head)	Male (HH head)	Age (HH head)	Indigenous (HH head)	Male (child)	HH size
	(1)	(2)	(3)	(4)	(5)	(6)
Post Law × $\mathbb{1}\{\text{Age}\geq 10\}$	-0.115	-0.036*	0.577	-0.003	0.036	0.056
	(0.344)	(0.018)	(0.571)	(0.033)	(0.027)	(0.121)
Post Reversal × $\mathbb{1}\{\text{Age}\geq 1\}$	0.129	-0.043**	-0.159	0.018	0.022	-0.008
	(0.383)	(0.022)	(0.628)	(0.031)	(0.027)	(0.124)
Obs.	11313	11313	11313	11313	11313	11313
Mean Control	8.729	0.813	43.07	0.357	0.504	5.609
Mean Treated	8.848	0.777	42.59	0.369	0.525	5.669
Joint test P-value = .595						

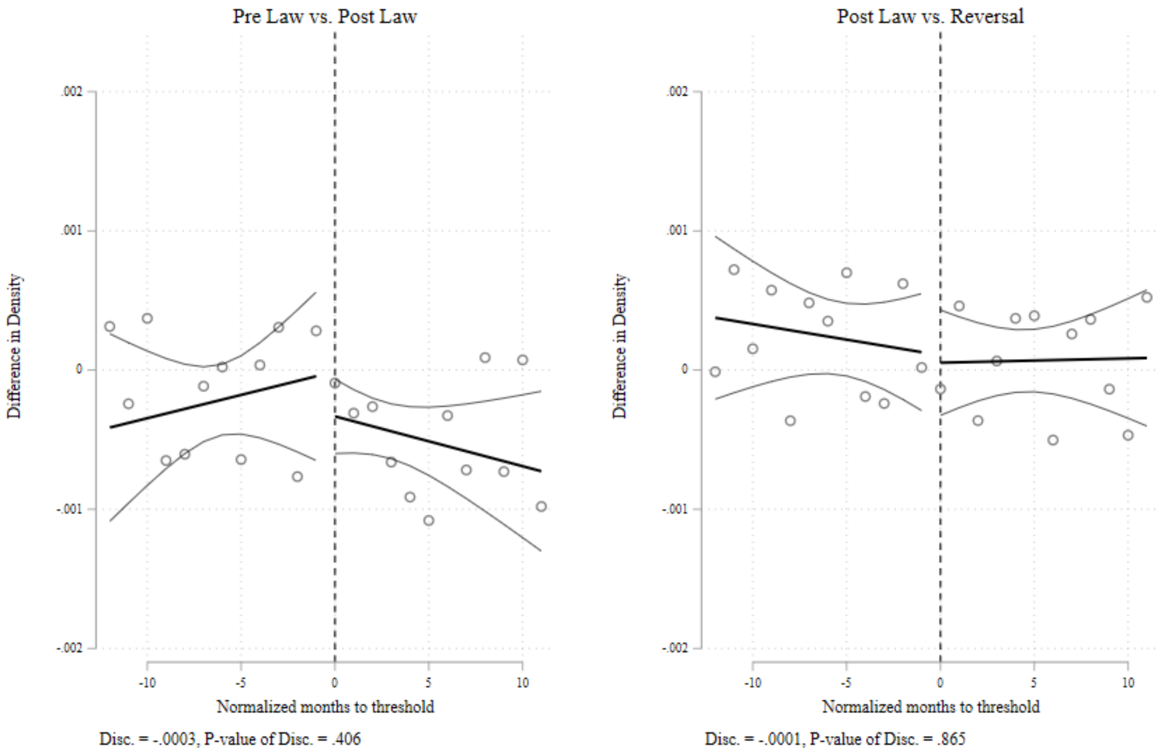
Notes: Age in months by year clustered standard errors in parentheses. Significance levels denoted by: *** p<0.01, ** p<0.05, * p<0.1. The running variable is the difference between age in months and the age cut-off at the survey date. The specification includes linear splines of the running variable, an indicator that is one from 2014 to 2017, an indicator equal to one on 2018 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 2012-2019.

Figure A.5: Differences in densities: 14 year-old cutoff



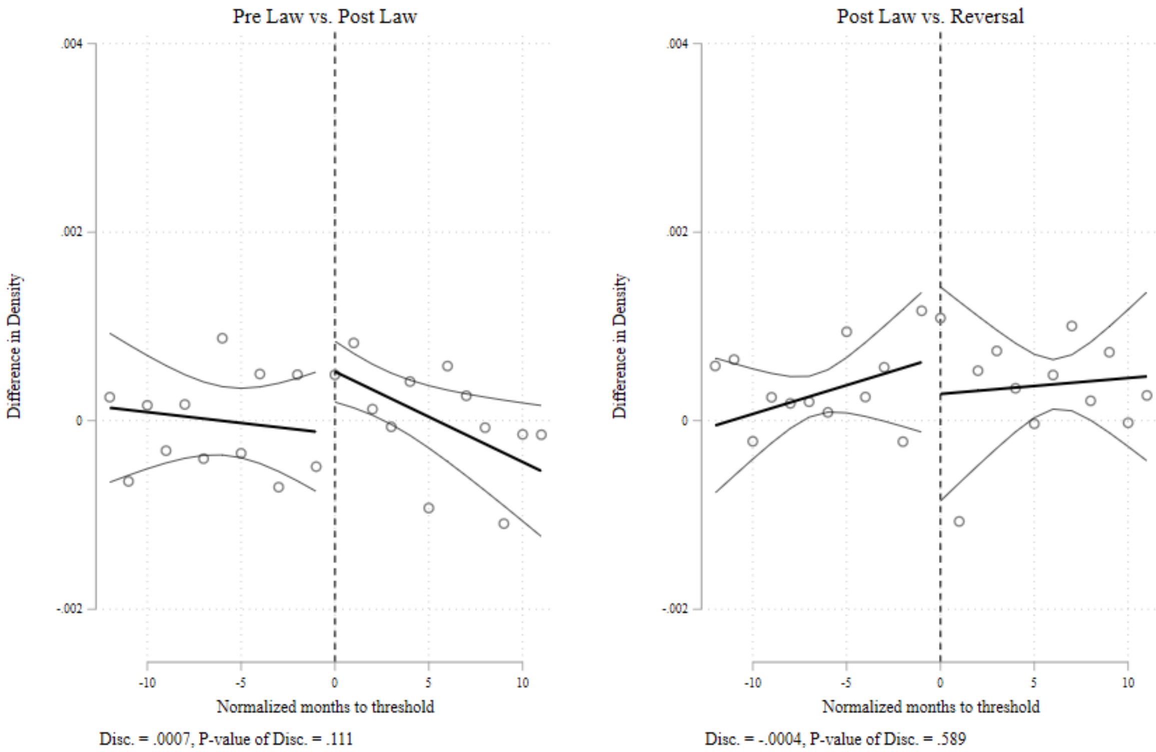
We use linear splines on each side of the potential density discontinuity to graphically approximate the density of the running variable, age in months, around a 12-month bandwidth. We also use a 12-month bandwidth and a linear polynomial specification interacted with a dummy equal to 1 at the right side of the cutoff to test for manipulation in the running variable. We report the discontinuity and its p-value below the graph. We use robust standard errors for both the graph and the estimate of the potential discontinuity.

Figure A.6: Differences in densities: 12 year-old cutoff



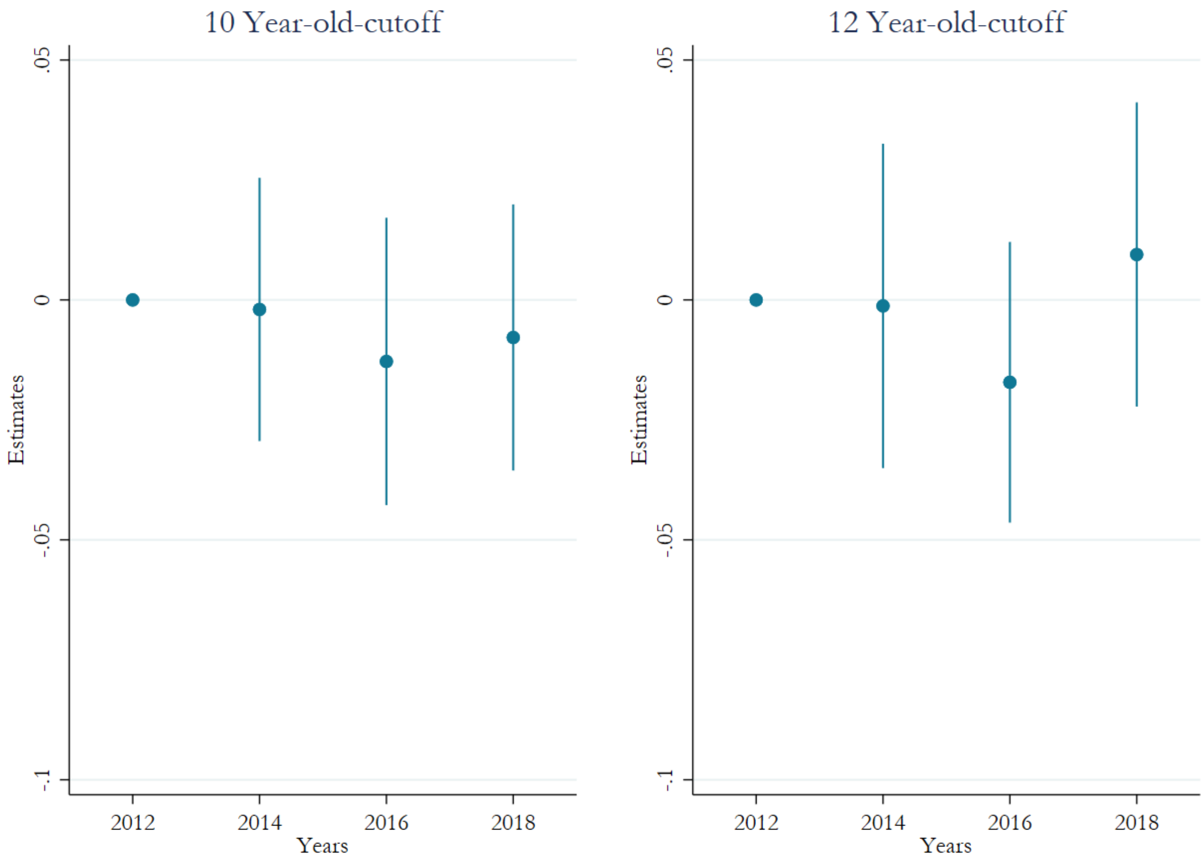
We use linear splines on each side of the potential density discontinuity to graphically approximate the density of the running variable, age in months, around a 12-month bandwidth. We also use a 12-month bandwidth and a linear polynomial specification interacted with a dummy equal to 1 at the right side of the cutoff to test for manipulation in the running variable. We report the discontinuity and its p-value below the graph. We use robust standard errors for both the graph and the estimate of the potential discontinuity.

Figure A.7: Differences in densities: 10 year-old cutoff



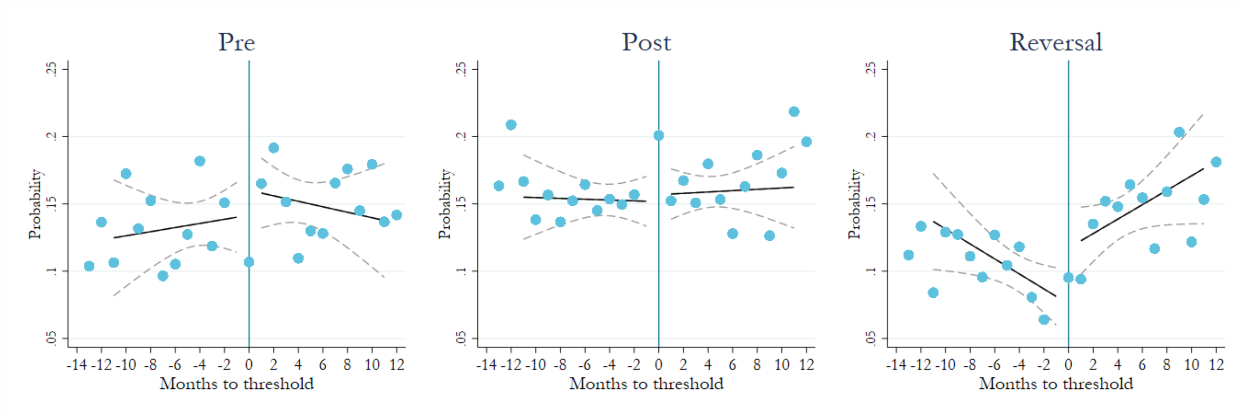
We use linear splines on each side of the potential density discontinuity to graphically approximate the density of the running variable, age in months, around a 12-month bandwidth. We also use a 12-month bandwidth and a linear polynomial specification interacted with a dummy equal to 1 at the right side of the cutoff to test for manipulation in the running variable. We report the discontinuity and its p-value below the graph. We use robust standard errors for both the graph and the estimate of the potential discontinuity.

Figure A.8: Difference in Discontinuity Event Study-style Estimates: Work Probability (12- and 10-Year-Old Cutoffs)



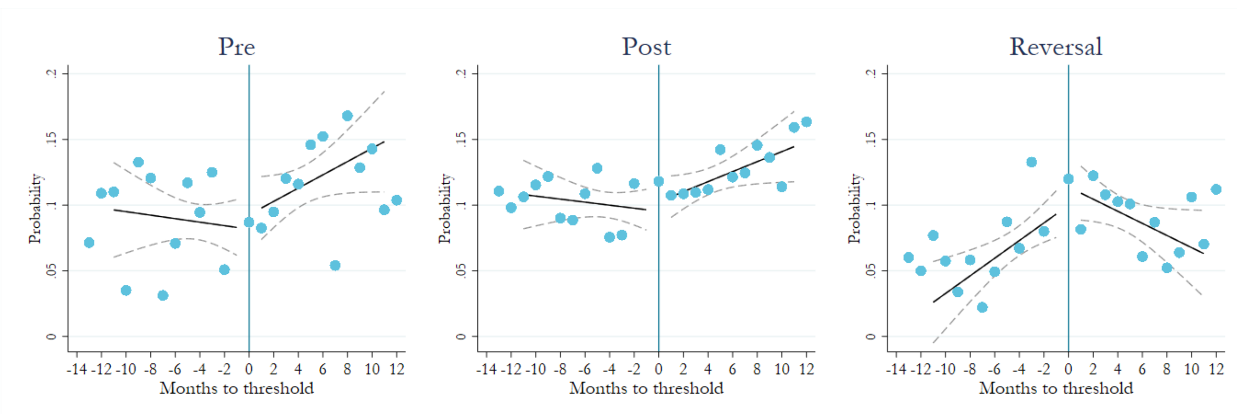
Household-level clustered standard errors in parentheses. Control variables: household head characteristics (schooling, gender, age, and indigenous indicator), gender, number of children in the following age categories: 0-6, 7-9, 10-13, and 14-17, number of adult men and women, an urban dummy, and departamento by year fixed effects. The sample includes 2012-2019.

Figure A.9: Work Probabilities at the 12-Year-Old Cutoff (Before, During, and After the Law)



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.10: Work Probabilities at the 10-Year-Old Cutoff (Before, During, and After the Law)



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.2: Effects of the Law and Reversal on Work Probability for 12 and 14 year-olds (Difference-in-Difference)

	Ages 12 vs. 13 (1)	Ages 14 vs. 15 (2)
Post Law × $\mathbb{1}\{\text{Treated}\}$	0.003	-0.005
	(0.014)	(0.016)
Post Reversal × $\mathbb{1}\{\text{Treated}\}$	-0.001	-0.005
	(0.018)	(0.019)
Obs.	12175	12165
Mean	0.160	0.203

Notes: Age in months by year clustered standard errors in parentheses. Significance levels denoted by: *** p<0.01, ** p<0.05, * p<0.1. For Column 1, Treated=1 for 12 year-olds, and =0 for 13 year-olds. For Column 2, Treated=1 for 14 year-olds, and =0 for 15 year-olds. The control variables are: in grade for CCT (only for Column 2), 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 one equal to one after the law was reversed, and an interaction between the age group indicator and the two indicators post law and reversal. The sample includes 2012-2019.

Table A.3: Effects of the Law on the Work Probabilities, Hours, and Occupation

Panel A: 12-Year-Old Cutoff

	Any Work (1)	Hours Worked (2)	Work for Self (3)	Work for Others (4)	Prohibited Work (5)	Allowed Work (6)
Post Law × $\mathbb{1}\{\text{Age} \geq 12\}$	-0.014	-0.339	-0.001	-0.012	-0.003	-0.010
	(0.015)	(0.339)	(0.003)	(0.016)	(0.004)	(0.014)
Post Reversal × $\mathbb{1}\{\text{Age} \geq 12\}$	0.015	0.231	-0.005**	0.020	0.003	0.012
	(0.019)	(0.420)	(0.003)	(0.019)	(0.008)	(0.016)
Obs.	11719	11719	11719	11719	11719	11719
Mean	0.142	2.846	0.00209	0.140	0.00349	0.138

Panel B: 10-Year-Old Cutoff

	Any Work (1)	Hours Worked (2)	Work for Self (3)	Work for Others (4)	Prohibited Work (5)	Allowed Work (6)
Post Law × $\mathbb{1}\{\text{Age} \geq 10\}$	-0.017	-0.199	0.002	-0.018	-0.003	-0.014
	(0.014)	(0.300)	(0.002)	(0.014)	(0.002)	(0.014)
Post Reversal × $\mathbb{1}\{\text{Age} \geq 10\}$	-0.013	-0.316	0.000	-0.014	0.001	-0.014
	(0.015)	(0.277)	(0.002)	(0.015)	(0.008)	(0.014)
Obs.	11801	11801	11801	11801	11801	11801
Mean	0.105	1.788	0.000748	0.104	0.00150	0.103

Notes: Age in months by year clustered standard errors in parentheses. Significance levels denoted by: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables: 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: 2012-2019.

Table A.4: Effect of the Law on Time Use

	Attends School (1)	Minutes Spent on Chores (2)
Post law × Treated	0.013	-13.843
	(0.012)	(18.363)
Post reversal × Treated	-0.010	
	(0.013)	
Obs.	11498	8372
Mean	0.955	407.0

Notes: Age in months by year clustered standard errors in parentheses. Significance levels denoted by: *** p<0.01, ** p<0.05, * p<0.1. Control variables: CCT eligibility indicator (Column 1 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. For Column 1, we include linear splines of the running variable, defined as the difference between the cutoff age and age at the survey in months. For Column 2, we do a stacked difference in disconinuity by multiplying the running variable by -1 for the 13 and 14 year-olds age group for interpretability. The running variable is the stacked difference between age in months and the age cutoff at the survey date, and the specification includes linear splines of the running variable. We use a bandwidth of 12 months and a triangular kernel for all specifications. Survey years for Column 1: 2012-2019. Survey years for Column 2: 2008 and 2016. We also report the mean of the dependent variable in the pre-law period.

Table A.5: Difference in Discontinuity: Household Outcomes for the 14-year-old Cut-off

	Any Adult in HH Works (1)	Total Hours Worked by Adults (2)	Per Capita Income (3)
Post law $\times \mathbb{1}\{\text{Age} < 14\}$	-0.001	-3.842*	-18.204
	(0.009)	(2.317)	(35.137)
Post reversal $\times \mathbb{1}\{\text{Age} < 14\}$	-0.007	2.444	-36.341
	(0.009)	(2.524)	(38.409)
Obs.	10788	10788	10788
Mean	0.969	94.26	908.6

Notes: Age in months by year clustered standard errors in parentheses. Significance levels denoted by: *** p<0.01, ** p<0.05, * p<0.1. The control variables are: an indicator that is one if child in HH is in grade for CCT, 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 income per capita variable in Column 3 is winsorized at the 99th percentile. 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 between 2014 and 2018, an indicator equal to one in 2018 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 2012-2019.

Table A.6: Heterogeneous Effects of the Law by Driving Time from MTEPS Offices (Difference-in-Discontinuity)

Panel A: 12-Year-Old Cutoff

	Dependent Variable: Works	
	All	No MTEPS Offices
	(1)	(2)
Post $\times$ $\mathbb{1}\{\text{Age} \geq 12\}$ for Far	0.038	0.019
	(0.037)	(0.045)
Post $\times$ $\mathbb{1}\{\text{Age} \geq 12\}$ for Near	-0.021	-0.054
	(0.016)	(0.037)
Obs.	7313	2938
Mean	0.142	0.257
P-value of difference	0.124	0.128
P-value of difference (urban controls)	0.342	0.180

Panel B: 10-Year-Old Cutoff

	Dependent Variable: Works	
	All	No MTEPS Offices
	(1)	(2)
Post $\times$ $\mathbb{1}\{\text{Age} \geq 10\}$ for Far	0.046	0.012
	(0.031)	(0.038)
Post $\times$ $\mathbb{1}\{\text{Age} \geq 10\}$ for Near	-0.024	-0.052
	(0.016)	(0.037)
Obs.	7148	2889
Mean	0.105	0.217
P-value of difference	0.0344	0.146
P-value of difference (urban controls)	0.312	0.627

Notes: Age in months by year 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 distance from a MTEPS office. 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.7: Heterogeneous Effects by Distance from MTEPS Offices, Allowing for Heterogeneity by Urban and Baseline Child Labor Rates

Panel A: Allowing for Heterogeneity by Urban

	Dependent Variable: Works	
	All	No MTEPS Offices
	(1)	(2)
Post × $\mathbb{1}\{\text{Age}< 14\}$ for Far	0.025	-0.016
	(0.069)	(0.070)
Post × $\mathbb{1}\{\text{Age}< 14\}$ for Near	-0.036*	-0.094**
	(0.021)	(0.048)
Obs.	7650	2984
Mean	0.180	0.317
P-value of difference	0.448	0.339

Panel B: Allowing for Heterogeneity by Baseline Child Labor Rates

	Dependent Variable: Works	
	All	No MTEPS Offices
	(1)	(2)
Post × $\mathbb{1}\{\text{Age}< 14\}$ for Far	-0.008	-0.019
	(0.066)	(0.064)
Post × $\mathbb{1}\{\text{Age}< 14\}$ for Near	-0.105***	-0.071
	(0.029)	(0.052)
Obs.	6874	2210
Mean	0.169	0.308
P-value of difference	0.199	0.565

Notes: Age in months by year 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 distance from a MTEPS office, where distance is calculated as the driving time from the municipality centroid to the nearest MTEPS office. 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 Panel A additionally includes: post × urban, treatment × urban, post × distance × urban, and treatment × distance × urban, where urban is normalized to the sample mean. The specification for Panel B additionally includes: post × baseline CL rates, treatment × baseline CL rates, post × distance × baseline CL rates, and treatment × distance × baseline CL rates, where baseline CL rates are defined at the municipality level, are calculated using data from only 2012 (pre-law), and are normalized to the municipality mean. 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 pre-law period.

Table A.8: Robustness Checks: Difference in Discontinuity for Work Probability (14-Year-Old Cutoff)

	Bandwidth (months)					Polynomials Pre-Post			Excl.	Excl.
	6	12	24	No Controls	Quadratic	Linear	Quadratic	Donut	Indig.	CCT control
		(Baseline)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post law $\times \mathbb{1}\{\text{Age} < 14\}$	-0.034	-0.039**	-0.027**	-0.031	-0.040**	-0.032	-0.039**	-0.030*	-0.034*	-0.037**
	(0.021)	(0.017)	(0.013)	(0.024)	(0.016)	(0.031)	(0.016)	(0.016)	(0.017)	(0.017)
Post reversal $\times \mathbb{1}\{\text{Age} < 14\}$	0.005	-0.000	-0.000	0.015	-0.001	0.017	-0.000	0.012		0.002
	(0.024)	(0.019)	(0.015)	(0.032)	(0.019)	(0.034)	(0.019)	(0.020)		(0.019)
Obs.	5983	11991	24340	11991	11991	11991	11991	11057	6481	11991
Mean	0.188	0.180	0.180	0.180	0.180	0.180	0.180	0.183	0.111	0.180

Notes: Household level clustered standard errors in parentheses. Controls: in grade for CCT, 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 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 omits children within 1 month of the age threshold. Column 9 excludes municipalities with above median shares of indigenous residents. Because municipality codes are anonymized in the household survey data starting in 2017, we cannot link the data to other sources using municipality codes for the periods after the law was reversed. Column 10 excludes the control that indicates whether the child is eligible for the CCT. We use a triangular kernel. The sample includes 2012-2016 for column 9 and 2012-2019 for all other columns.

Table A.9: Difference in Difference Specifications

	Dep. Var.: Any Work		
	Control: 14-year-olds	Control: 9- and 14-year-olds	Control: 7-9 and 14-16-year-olds
	(1)	(2)	(3)
Post Law × $\mathbb{1}\{\text{Age} < 14\}$	-0.038**		
	(0.015)		
Post Reversal × $\mathbb{1}\{\text{Age} < 14\}$	-0.011		
	(0.018)		
Post Law × $\mathbb{1}\{10 \leq \text{Age} < 12\}$		-0.012	-0.002
		(0.010)	(0.009)
Post Law × $\mathbb{1}\{12 \leq \text{Age} < 14\}$		-0.018*	-0.009
		(0.010)	(0.009)
Post Reversal × $\mathbb{1}\{10 \leq \text{Age} < 12\}$		-0.008	-0.001
		(0.011)	(0.009)
Post Reversal × $\mathbb{1}\{12 \leq \text{Age} < 14\}$		-0.000	0.008
		(0.012)	(0.010)
Obs.	11991	35511	53490
Mean	0.180	0.144	0.137

Notes: Age in months by year clustered standard errors in parentheses. Significance levels denoted by: *** p<0.01, ** p<0.05, * p<0.1. 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 before it was reversed, an indicator equal to one after the law was reversed, and interactions between the time and the age group indicators. The sample includes 2012-2019.

Table A.10: Balance for 30% of Child Labor Survey Data

	Male	HH Size	Age HH Head	Education HH Head	Male HH Head	Indigenous HH Head	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post	-0.023	0.129	0.696	-0.151	0.013	0.036*	0.008
	(0.020)	(0.088)	(0.481)	(0.211)	(0.017)	(0.020)	(0.018)
Obs.	2580	2580	2580	2580	2580	2580	2580
Mean	0.510	5.857	42.62	7.888	0.786	0.348	0.742
Joint test P-value = .262							

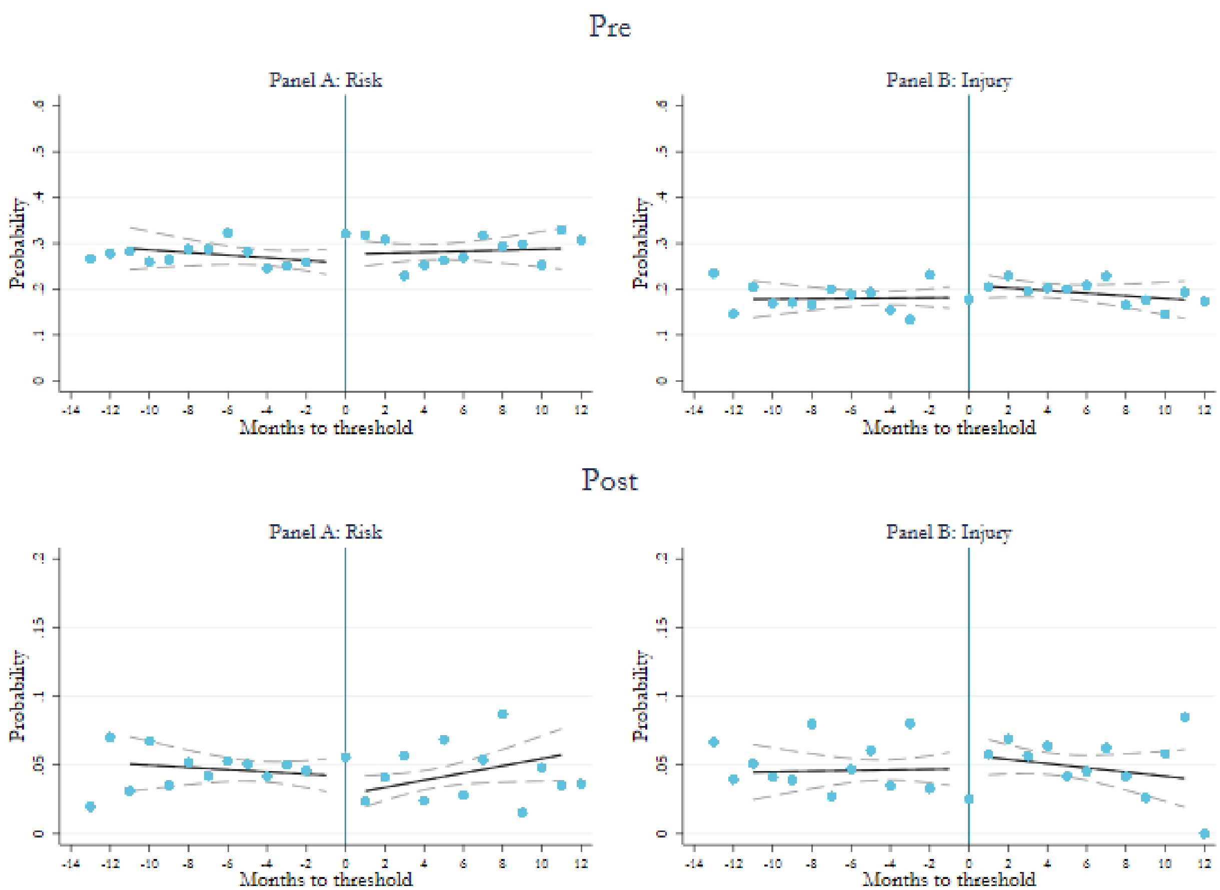
Notes: Household level clustered standard errors in parentheses. Significance levels denoted by: *** p<0.01, ** p<0.05, * p<0.1. The specification includes an indicator that is one in 2016. The running variable is multiplied by -1 for the 13 and 14 year-olds age group for interpretability. The bandwidth for all specifications is 12 months. The sample is 30% of the 2008 and 2016 observations that were not used in the reweighting exercise.

Table A.11: Balance for Reweighted Child Labor Survey Data - Full sample

	Male	HH Size	Age HH Head	Education HH Head	Male HH Head	Indigenous HH Head	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × Treated	-0.041*	-0.039	-0.764	0.237	0.005	0.004	0.012
	(0.022)	(0.099)	(0.546)	(0.267)	(0.019)	(0.025)	(0.024)
Obs.	8372	8372	8372	8372	8372	8372	8372
Mean	0.510	5.857	42.62	7.888	0.786	0.348	0.742
Joint test P-value = .604							

Notes: Age in months by year clustered standard errors in parentheses. Significance levels denoted by: *** p<0.01, ** p<0.05, * p<0.1. 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 12 months. We use a triangular kernel. The sample includes 2008 and 2016.

Figure A.11: Job Risks & Work Injuries (Before and During the Law): 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.

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

Panel A: 14-Year-Old Cutoff

	Faces Risks at Work (1)	Has Been Injured at Work (2)
Post $\times \mathbb{1}\{\text{Age} < 14\}$	-0.007	-0.001
	(0.019)	(0.026)
Obs.	2808	2827
Mean	0.349	0.219

Panel B: 12-Year-old Cutoff

	Faces Risks at Work (1)	Has Been Injured at Work (2)
Post $\times$ $\mathbb{1}\{\text{Age} \geq 12\}$	-0.021	-0.016
	(0.024)	(0.018)
Obs.	2733	2767
Mean	0.278	0.183

Panel C: 10-Year-old Cutoff

	Faces Risks at Work (1)	Has Been Injured at Work (2)
Post $\times \mathbb{1}\{\text{Age} \geq 10\}$	-0.018	-0.025
	(0.020)	(0.018)
Obs.	2831	2817
Mean	0.214	0.166

Notes: Age in months by year clustered standard errors in parentheses. Significance levels denoted by: *** p<0.01, ** p<0.05, * p<0.1. 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 regression, 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.

Table A.13: Robustness Checks: Difference in Discontinuity for Risk Outcomes

Panel A: Different Bandwidth Specifications

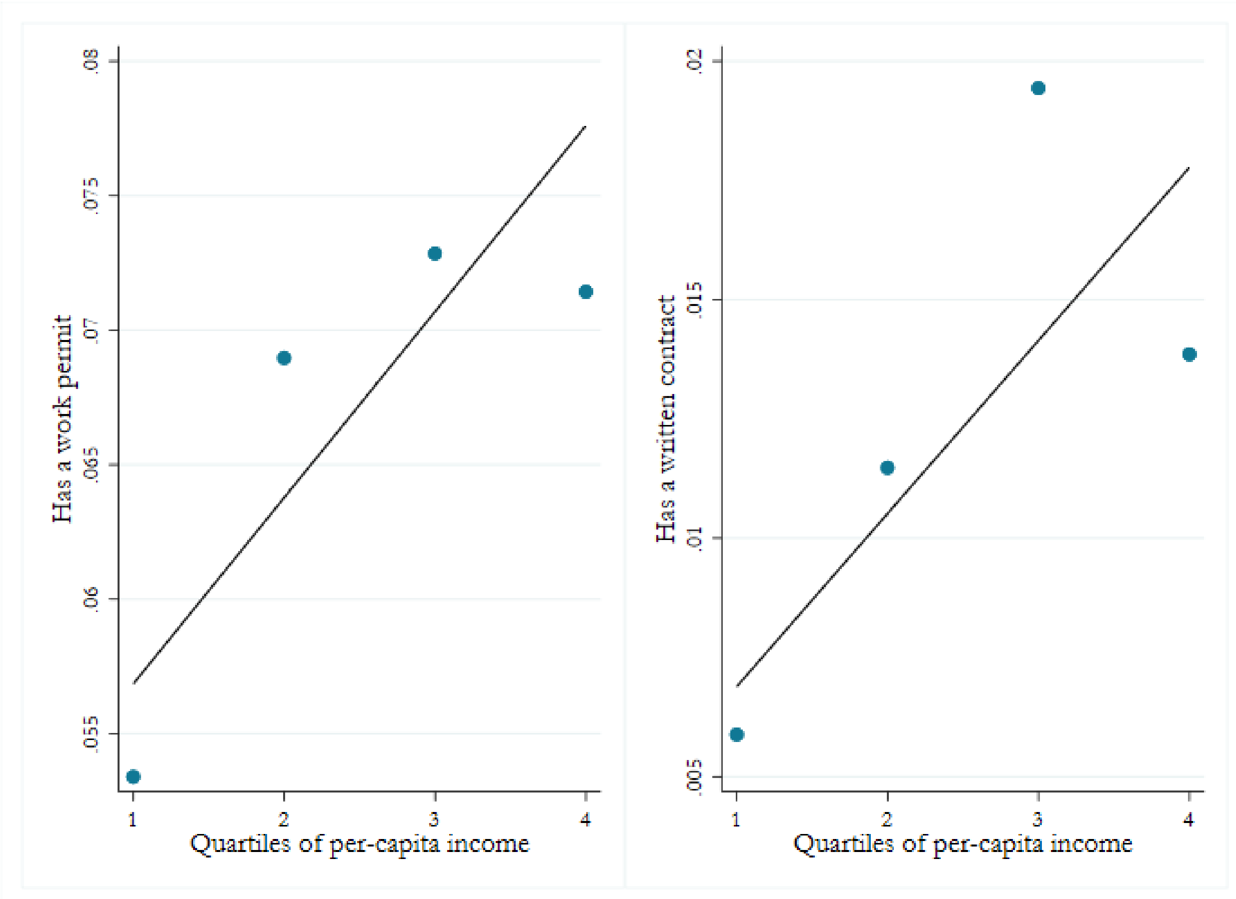
	Risk Index			Injury Index		
	Bandwidth (months)					
		Baseline			Baseline	
	6	12	24	6	12	24
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated	-0.012	-0.008	-0.009	-0.006	-0.015	-0.010
	(0.021)	(0.017)	(0.015)	(0.020)	(0.014)	(0.012)
Obs.	3981	8372	8872	4074	8411	8885
R-squared	0.186	0.179	0.182	0.110	0.107	0.103
Mean	0.277	0.281	0.281	0.194	0.188	0.188

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

	Risk Index			Injury Index		
	No Controls	Quadratic	Donut	No Controls	Quadratic	Donut
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated	-0.013	-0.007	-0.003	-0.007	-0.015	-0.036***
	(0.016)	(0.016)	(0.018)	(0.013)	(0.014)	(0.013)
Obs.	8372	8372	7325	8411	8411	7351
R-squared	0.109	0.180	0.183	0.0509	0.107	0.109
Mean	0.281	0.281	0.279	0.188	0.188	0.186

Notes: Household level clustered standard errors in parentheses. Significance levels denoted by: *** p<0.01, ** p<0.05, * p<0.1. 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.

Figure A.12: 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.

B List of Prohibited Tasks under the 1999 and 2014 Laws

Under the 1999 and 2014 laws, 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)* *This restriction was added in 2014.*
- 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.

- 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).