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Evidence from Child Labor Legislation

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Abstract*

We study the effects of a Bolivian law that introduced benefits and protections for child workers (who are overwhelmingly informal workers) and lowered the de facto legal working age from 14 to 10. We employ a difference-in-discontinuity approach that exploits the variation in the law's application to different age groups. Work decreased for children under 14, whose work was newly legalized and regulated under the law, particularly in areas with a higher threat of inspections. The effects appear to be driven by a reduction in the most visible forms of child work, suggesting that firms may have reduced employment of young children to minimize the risk of being inspected. In contrast, we find that more formal channels of adjustments - such as increased costs of hiring due to the costs of complying with the new law - are unlikely to explain the overall decline in the work of young children.

JEL classifications: J08, O12, K3

Keywords: Child labor, Informality, Worker protection

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1 Introduction

Over 60% of the global workforce are hired off-the-books (Bonnet et al., 2019), often under precarious working conditions. Improving the working conditions of these workers is challenging: most policy responses focus on introducing protections or mandated benefits for incumbent workers employed by formal firms (Lazear, 1990; Freeman, 2010). While such policies can improve some workers' conditions by encouraging informal workers to move to the formal sector (Almeida and Carneiro, 2012), they are likely to exclude the most vulnerable workers whose work is not legally recognized (such as undocumented immigrants, sex workers, and children) and who are forced to remain in the informal sector. Despite the high vulnerability of these workers, there is little evidence on the effect of policies aiming to improve their working conditions. Furthermore, the implementation and enforcement of policies intended to protect marginalized and vulnerable workers may alter incentives for employers in ways that run contrary to policy goals.

We study the effects of the introduction of worker's rights and protections for informal workers by leveraging a unique 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. Specifically, we study a 2014 Bolivian law 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 law tasked local offices of the Ministry of Labor and Social Protection (MTEPS) with adding child labor inspections to their regular labor 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

¹The 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.

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.

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. We begin with a simple difference-in-difference framework in a one-year bandwidth around the pre-existing minimum working age that compares outcomes for children under age 14 (those targeted by the law) to those over age 14, 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). Moreover, to account for unobserved characteristics of children that vary systematically with age, we employ a difference-in-discontinuity approach as our preferred estimating strategy based on data on year and month of birth. Thus, we examine differences in work outcomes for children just above and below age thresholds issued by the law and we study how those differences changed over the periods when the law was implemented and then later reversed. 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.

We find that recognizing child work and entitling child workers to basic rights and protections decreases the prevalence of child labor in terms of the likelihood and hours of work using repeated cross-sectional household surveys. 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). As expected, the effects of the law dissipate after 2018, when key components of the law were repealed. We find no evidence that the law shifted child labor across allowed and prohibited work, both in terms of self-employment versus external employment and in terms of permitted tasks and sectors. We also examine the effects of the law on other measures of child time allocation (schooling and chores) and household outcomes (adult labor supply and household income) but find no effects.

We also estimate the impacts of increased protections among younger child workers

by comparing 11-year-old children who were allowed to work only for themselves (self-employment) to 12-year-old children who were also allowed to work for others, and by comparing 9-year-olds who were not legally allowed in any capacity to work to 10-year-olds who were allowed to work only in self-employment activities. However, we find no substantial impacts at these age thresholds, likely because the incidence of child labor was relatively low among these age groups. Our results are not driven by standard concerns for difference-in-discontinuity designs, such as manipulation of the running variable, changes in sample composition and balance across age thresholds, bandwidth selection, inclusion of controls, and functional form specifications for the running variable.

Enforcement and the threat of inspection appear to have been key drivers of the law's impacts. We find that the effects of the law were strongest in areas with higher probability of inspection, proxied by the (driving) distance to the closest regional offices of the MTEPS — the public agency in charge of conducting labor inspections. 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 two potential mechanisms behind the declines in child work. First, several studies suggest that the costs of complying with new worker protections may increase hiring costs and reduce the demand for labor (Lazear, 1990; Autor et al., 2007). To explore this possibility, we study the impacts 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. Interestingly, we find that the law had no statistically significant impacts on the riskiness of child work. Correspondingly, we find no evidence on impacts on injuries sustained while at work. We observe non-statistically significant increases in wages among children who remained employed. However, as few children work formally, we believe that the increases in direct costs of complying with the law are not likely to fully explain the overall decline in child work. Overall, the law does not appear to have improved the working conditions of children, likely because most children were employed by informal employers with fewer incentives to comply with the new worker protections.

Alternatively, for informal firms that employ children, 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 regulator agency (MTEPS), incentivizing firms to remain “under the radar” by not hiring younger children who were visible targets of the new legislation. Indeed, we find that 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. We also find suggestive evidence that the law reduced the size of the firms in which children work. This is consistent with the notion that larger firms, which are more likely to be inspected (Almeida and Carneiro, 2009), find it more costly to hire younger workers after the 2014 law and that, consequently, these younger children end up working for smaller firms. Our finding that firms located nearer to MTEPS offices (where visibility to inspectors is particularly relevant) are less likely to hire younger workers targeted by the law is also consistent with the explanation that firms want to avoid drawing attention from MTEPS inspectors, who are also responsible for conducting both child labor and general inspections.

These findings contribute to the literature studying the impacts of labor regulation on employment. Previous studies analyze the impacts of legislation aimed at protecting the employment of current workers such as severance pay (Lazear, 1990; Kugler, 2005; Butscheck and Sauermann, 2022; Autor et al., 2007) while others have studied the effects of increased worker benefits on formal firms and workers (Gruber, 1994). A common finding is that these legislations can increase workers’ protection at the cost of declines in employment, productivity, or in wages among incumbent workers (see Freeman (2010) and Heckman and Pages (2003) for a review in developing countries and Latin America in particular). We contribute by analyzing the effects of legislation that recognizes the work of informally hired workers, as opposed to expanding protections for formally hired workers. Our results suggest that such policies can induce a decline in employment for the workers whom they intended to protect, without necessarily improving working conditions. In particular, we find that worker conditions appear unresponsive to regulation for the vast majority of child workers, while informal firms respond to the regulation by reducing their demand of child workers.

Our findings also complement studies that analyze how imposing mandated benefits or pro-worker protections to formal firms can generate worker transitions from informality to formality (Almeida and Carneiro, 2012) or alter the relative size of the informal sector (Besley and Burgess, 2004). Our results suggest a novel mechanism: in the informal sector, employment can negatively respond to worker protections – not because complying with such protections increases hiring costs but because informal firms reduce their demand for newly protected workers to avoid drawing the attention of regulators. This margin appears particularly salient in markets with widespread informal employment, where firms survive in part by remaining under the radar of inspectors and outside of the purview of the costs of regulatory compliance, and in settings where institutional constraints limit formal employment opportunities, as is the case for child workers. Indeed, our results are consistent with evidence from other illicit markets, which share many of these characteristics. In the context of sex workers, regulations appear to fail to improve worker safety, often leading to unintended consequences due to worker responses to the regulations (Manian (2021); Gertler and Shah (2011); Ito et al. (2018)). In our setting, the unintended consequences appear to be driven by employers.

Our results also provide novel insights to the literature evaluating the effects of child labor legislation. Previous studies analyzed the effects of child labor bans (Bharadwaj et al. (2020) in India, Piza and Souza (2016, 2017) and Bargain and Boutin (2021) in Brazil, Edmonds and Shrestha (2012) using a large cross-section of countries). In contrast, the unique Bolivian context enables us to study the interaction of the legalization of child work with regulations protecting child workers. Moreover, we provide 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. Importantly, our findings also highlight an important facet of child labor legislation: its impact on employers. Child labor laws rarely address what many regard as a root cause of child labor: poverty (Basu and Van (1998); Edmonds and Schady (2012); Edmonds and Pavcnik (2005); Edmonds (2005)). Instead, bans and other regulations more often impact the demand for child work by altering the costs of child workers. Imperfectly enforced bans can impose costs associated with hiring children, which can then be passed through to children in the form of lower wages (Bharadwaj et al.

(2020)). In our context, recognizing and regulating child labor appears to have increased the perceived risk of labor inspections and thus the cost of hiring child workers. Thus, regulation — whether in the form of worker protections or in outright bans — increases the cost of hiring children, which ultimately affects child work in ways that can contradict policymakers’ intentions.

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.² 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, mostly in informal firms which remain under the radar of workplace safety inspections.³ 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 (*Unión Nacional de Niños, Niñas y Adolescentes Trabajadores de Bolivia*, UNATSBO) have

²Authors’ calculations of weighted means based on the 2012-2013 Encuesta de Hogares. This definition does not include participation in household chores.

³Specifically, 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.

been at the forefront of such policy suggestions, demanding the recognition of labor as an integral and unavoidable part of children's development.⁴ In part as a response to this tension, the Child and Adolescents Code of 2014 was implemented to legally recognize some forms of child labor and thus guarantee protections to working children. We describe the main changes induced by the law in the following sections, 2.1 and 2.2.

2.1 Child Labor Legislation prior to 2014

Before 2014, two laws regulated the engagement of children in labor markets: the Child and Adolescents Code (law 206 of 1999), which provided general guidelines on 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 to the same rights and obligations as 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.⁵

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

⁵Appendix Section 2 provides a list of all forbidden activities under Articles 134-135 of Title VI of the 1999 code.

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

Appendix Table A.1 summarizes the key changes induced by the law for each age group. The new law confirmed the minimum working age of 14 years, but it also introduced exceptions that allowed children aged 10 to 13 years to work legally, subject to additional restrictions. Before 2014, no children younger than 14 were allowed to work legally. Under the new law, children aged 10 to 11 were allowed to work as self-employed (own-account) workers, while children aged 12 to 13 were permitted work as both self-employed workers 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 state with regulating work and establishing protections for younger working children who 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).⁶ 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

⁶Authors’ translation of original document in Spanish.

their schooling obligations.⁷ 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.

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 State’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 2018 amendment repealed paragraph IV of Article 132, which regulated weekly work hours for children between 10 and 14 years old.

Throughout the paper, we interpret the enactment of the 2014 law as a legal recognition of the work of younger children and an expansion of worker rights for this group. In contrast, we interpret the 2018 amendment as an abrupt decrease in the enforcement of worker protections for younger children.

2.3 Enforcement and Awareness

The law tasked the regional offices of the Ministry of Labor and Social Protection (Ministerio de Trabajo, Empleo y Protección Social, MTEPS) with carrying out inspections and permanent supervision of workplaces to ensure that employers were complying with the regulations under the law (Article 139).⁸ If any party were found to be in violation of the rights and protections under the law, the MTEPS would turn the case over to the Defensoría de la Niñez y Adolescencia (DNA) for legal restitution. Under the 2014 Law, the DNA was allowed to impose penalties such as warnings and reprimands, fines, the removal of children from work, and temporary suspension of business activities.⁹ Parents in violation of the code (for example, as employers of their children in family work, but also as guardians of their

⁷In the case of self-employed children, the 2014 law required that parents ensure that children can attend school even while working.

⁸Article 46 of Executive Order 2377 provides implementation rules related to inspections.

⁹As stated in Article 169 of Law 548 and Article 219 of the 1999 code.

children more broadly) were also subject to measures, ranging from warnings, to required attendance of courses and programs, to (at the extreme) separation from their children. In the case of repeat offenders, the DNA had the authority to send the proceedings to criminal court.

It is worth noting that under the law, the local DNAs were primarily responsible for processing child work permits and following up any violations brought to light by MTEPS inspections. On the other hand, the responsibility of inspecting workplaces was given to the regional MTEPS offices, which were already in charge of verifying the ownership of valid business registrations, conducting general labor and technical inspections, and carrying out inspections related to preventing forced labor.¹⁰ Thus, the threat of an inspection by the MTEPS office is likely to affect employers' compliance with the newer 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 economy¹¹—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. 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).¹² Relatedly, prior work has found that firms tend to resist formalization, even when provided information about the registration process and when registration fees are waived, but that firms respond to the increased likelihood of inspections (De Andrade et al., 2016).

There are 25 regional Ministry of Labor and Social Protection offices located in the most

¹⁰Labor 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.

¹¹Informal firms account for almost 80% of employment and 62% of GDP in Bolivia (Elgin et al., 2021).

¹²This behavioral response of firms to regulation has been discussed in other settings (see for example, Hsieh and Olken (2014); Tybout (2014)).

populated municipalities of the country.¹³ Using data from annual MTEPS reports, Figure 1 shows that child labor inspections and the number of dedicated child labor inspectors increased considerably in 2014 and rose thereafter, possibly reflecting an increase in resources devoted to enforcing the 2014 law. There were on average around 300 child labor-specific inspections per year conducted during the period following the law’s enactment; in 2018, 17% of such inspections were turned over to the DNAs for resolution (Ministerio de Trabajo, Empleo y Previsión Social, 2018). The total number of inspections (labor and technical) conducted by the MTEPS also increased after 2014, suggesting that the increase in child labor inspections did not crowd out other inspections conducted by the MTEPS.

The initial enactment of the law was very controversial and highly scrutinized by NGOs, international organizations, and authorities. Several press articles highlight the public support of the legislation by the then-president (Pagina Siete, 2013; Los Tiempos, 2013), which may have amplified awareness about the policy change.¹⁴ 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. 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 workers’ rights and protections, delivered by the MTEPS and targeted to employers and children. Over 11,000 workers and employers attended these child labor workshops between 2015 and 2018, according to MTEPS Annual Reports (Ministerio de Trabajo, Empleo y Previsión Social, 2018).

¹³The location of MTEPS offices is displayed in Appendix Figure A.1.

¹⁴There is a growing literature documenting how information provided by political leaders can modify citizens’ attitudes and behavior through different media (Ajzenman et al., 2020; Pedemonte, 2020; Jetter and Molina, 2022).

3 Data

To measure the effects of the policy change on employment and work hours, we leverage data corresponding to eight 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), four post-law years (2014-2017), and two 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.¹⁵

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 14.¹⁶

Economic activity is measured by an indicator of whether a child worked at least one hour during the week preceding the interview.¹⁷ We also compute weekly work hours and construct an indicator for overtime work (defined by the 2014 law as working more than 30 hours for children under 14). Further, we separately measure work for self, work for others, employment in activities that are prohibited under the law for all children under age 18 (such as mining), and participation in allowed activities.¹⁸ 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 Section 4

To better understand the mechanisms behind the main results, we use information from

¹⁵The *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.

¹⁶For survey waves 2013, 2014 and 2016, the (household-specific) exact date of survey interview is also available. We report robustness analyses restricting the sample to those survey waves with exact date of interview (and thus exact age at the time of survey) in Section 5.1.1.

¹⁷This definition does not include unpaid participation in household chores.

¹⁸See Appendix Sections 2 and 3 for a full list of prohibited activities and more detailed variable definitions.

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 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).¹⁹

Panel A of Table 1 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 firms²⁰, regardless of whether they are family operated or not (see Panel A of Table 2); 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 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 2), although children working for external employers are more likely to work in mobile locations.

Panel B of Table 1 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 2).

¹⁹The 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 3.

²⁰Formality is defined by whether the firm is formally registered with the national tax authority.

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.

We begin with a difference-in-difference specification that compares changes in outcomes of children across age cutoffs between three periods: before the law (2012-2013), during the period in which the law was enforced (2014-2017), and after the reversal of the law (2018-2019). Specifically, we estimate the following flexible difference-in-differences specification:

$$Y_{i,t} = \alpha_0 + \alpha_1 T_i \times Law_t + \alpha_2 T_i \times Reversal_t + \alpha_3 T_i + \gamma x_{i,t} + \delta_{d,t} + \epsilon_{i,t} \quad (1)$$

Here, $Y_{i,t}$ is a work outcome for child i in survey year t . 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, 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).²¹

Our primary parameters of interest are α_1 and α_2 , which estimate the change in outcomes of the treated group (with expanded workers' rights) relative to the control group in the periods during which the law was implemented and then reversed, relative to the pre-law period (2012-2013). In particular, α_1 captures the effect of the law on treated children relative to the pre-law period. Given that the law simultaneously legalizes work for treated children and expands their workers' rights and protections, the expected sign of α_1 is ambiguous. Assuming that the 2018 amendment effectively reverses the law, we expect α_2 to be zero, i.e., for the effects of the 2014 law to disappear and for work outcomes to return to pre-law levels.

We also include a vector of demographic household and child characteristics that are unlikely to vary due to the program ($x_{i,t}$). 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.²²

We estimate equation (1) using a one-year bandwidth from each age cutoff. Doing so 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.²³ 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.

This difference-in-difference specification captures the causal effects of the law under the assumption that unobserved time varying shocks do not differentially affect work outcomes of children based on their age. This assumption is very strict and may not be satisfied, 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

²¹The then-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.

²²Departamento is an administrative/geographic unit roughly comparable to a U.S. state.

²³For 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.

continuously increases by age in months. To address this concern, we propose an alternative 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 alternative empirical strategy helps control for potential time varying shocks with differential effects based on age.

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:

$$Y_{i,t} = \beta_0 + \beta_1 T_i \times Law_t + \beta_2 T_i \times Reversal_t + \beta_3 T_i + \theta_1 (Age_{i,t} - c) + \theta_2 T_i \times (Age_{i,t} - c) + \gamma x_{i,t} + \delta_{d,t} + \epsilon_{i,t} \quad (2)$$

where $Age_{i,t}$ is the age of child i in months at the beginning of the relevant recall period (which differs by outcome) for survey wave t ²⁴; and c is the relevant cutoff age related to the key policy changes induced by the new law (at ages 10, 12, and 14). $\epsilon_{i,t}$ is an error term.

As in the case of equation (1), T_i is an indicator of whether child i is exposed to the policy change associated to each cutoff. 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.

As before, we control for a vector of demographic characteristics $x_{i,t}$ and for departamento-by-year fixed effects ($\delta_{d,t}$).²⁵ The coverage of Bolivia’s flagship CCT program was expanded

²⁴For 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.

²⁵We include covariates to increase precision, though we show that our results are robust to specifications

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 2 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 5.1.1. We estimate equation (2) using triangular kernels that assign a higher weight to observations closer to the eligibility cutoff and conduct inference using standard errors clustered at the household level to account for correlated error terms across siblings. Our preferred specification uses a 12-month bandwidth on either side of the cutoff.²⁷ In Section 5.1.1, we show that our results are robust to using narrower and wider bandwidths.

4.2 Threats to Identification

Manipulation. The validity of our difference-in-discontinuity 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 national bureau of statistics and which was not designed or framed as tool to measure the impacts of the law; ex ante there was no incentive to manipulate child age in order to appear compliant in our analysis. Second, even though age heaping is common, interviewees are asked for the birth date of each household member as opposed to their age.

without controls in Section 5.1.1.

²⁶Controlling 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).

²⁷The mean squared error (MSE) optimal bandwidth proposed by Imbens and Kalyanaraman (2012) ranges from 13 to 25 months for all our main outcomes. As explained above, these bandwidths are too wide for our context because they would yield overlapping treatment and control groups. Since the bandwidth we selected is narrower than the MSE optimal bandwidth, the choice of a 12-month bandwidth is not inducing bias in our estimates, though it affects the power of our regressions. We show in Section 5.1.1, that our results are robust to widening the bandwidth.

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. Appendix Figure A.4 reports the distribution of observations around the cutoffs, focusing on children with birth dates within a year of each cutoffs (the bandwidth of our baseline specifications). It shows no evidence of discontinuous changes at the cutoff; this is corroborated when we conduct the McCrary (2008) test for manipulation in the pre- and post-2014 periods (see Appendix Table A.2). We discuss additional checks for measurement error in Section 5.1.1.

Changes in sample composition and balance. We test for changes in demographic characteristics around the cutoff before and after the policy change. For this, we estimate (2) using demographic characteristics as dependent variables. Appendix Table A.3 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.

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 based on the simple difference-in-difference design. We focus on the impacts around the 14-year-old threshold, which speak to the combined effects of regulating self-employment and work for others, because there is a substantially higher rate of working children around this cutoff.

Figure 2 reports flexible difference-in-difference (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.²⁸ 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

²⁸We group observations in two-year bins to gain precision amid the reduced number of observations per survey wave.

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.5.

We further confirm the graphical evidence with the regression estimates from equation 1. In Table 3, see that the simple difference-in-difference method estimates that the 2014 law reduced the work probability of children under 14 by little under 4 percentage points (significant at the 5% level, column 2). In contrast, we find that the differences in work probabilities between treated and control children dissipate in the post-reversal period; in essence, they return to the pre-2014 levels. The results are not affected by the inclusion of covariates (column 1 versus column 2), though the latter increases precision. We find no statistically significant effects at the 10- and 12-year-old cutoffs (see Appendix Table A.4).

We next show that these declines in work probabilities are also observed when we conduct local comparisons between children in the margin of exposure to the law using our preferred difference-in-discontinuity strategy. 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. Recall that throughout the entire sample period, the work of children aged 14 and older was regulated. The 2014 law enabled younger children to work legally but also imposed requirements related to worker protections. 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. 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 common), we observe no discontinuities around the cutoffs during the implementation of the law (see Appendix Figures A.6 and A.7).

We now turn to the regression-based evidence using our preferred difference-in-discontinuity strategy. Table 4 reports the effect of the law on work outcomes around the 14-year-old cut-

off. 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. There are no statistically significant effects of the law on 13-year-old children’s overtime work (column 7). The results suggest a decline in the demand for young workers amid the perceived increased costs of hiring young workers due to the regulation. We discuss this argument in more 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. 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. Interestingly, these estimates are similar in magnitude to those found in Bharadwaj et al. (2020), who study the effects of a child labor ban in India.²⁹

Appendix Table A.5 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-

²⁹We find a 21% increase in the probability of working for those under 14 relative to the pre-reversal average work probability for 13-year-olds. Bharadwaj et al. (2020) find that the ban results in a 22% increase in work for children under 14 relative to the pre-ban mean. Studies of a Brazilian law that increased the legal working age from 14 to 16 found no effects (Bargain and Boutin, 2021) or declines in child work (Piza and Souza, 2017).

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.6).³⁰ 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. 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 2).³¹ 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.7)

5.1.1 Robustness

We show that our results are robust to alternative specifications. Our main results on work probabilities are based on estimates of equation (2) using a 12-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. In addition,

³⁰Since 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.

³¹Note that the data on participation in domestic chores comes from the ENTI 2008 and the ENNA 2016, described in more detail in Section 6.1. As there are no data beyond 2016, we cannot estimate a post-reversal coefficient for this outcome.

columns 4-7 show that the results are robust to excluding demographic controls from our main specification, to using a second-order polynomial on each side of the cutoff to flexibly control for the running variable, and to allowing the slopes to vary before and after the policy change on either side of the cutoff, respectively.

In our main specification we use age in months to determine exposure to the law. However, because we do not have the exact survey interview date, among children born in the same month, there might be children who were exposed to the law at the time of data collection and others who were not. To ensure that measurement error is not biasing our results towards zero, column 8 of Appendix Table A.8 reports results from a specification that uses exact birth and survey interview dates to determine exposure. Even though precision is reduced because exact interview dates are only available for three survey waves (2013, 2014 and 2016), the point estimates are very similar. Further, we also find similar results when we exclude observations of children that, according to their age in months, are within a month of exposure and who are more prone to misclassification (column 9).³²

Another potential source of measurement error stems from social desirability bias.³³ In particular, one might worry that the law changed the stigma surrounding child labor and affected the accuracy of parents' reports of their children's work. However, we think that this is unlikely for several reasons. First, we observe no discontinuities in either survey responses (Appendix 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

³²This, in turn, attenuates potential concerns related to the reference period of the questions about employment—the week prior to the interview—which can result in misclassification among children born within a month of each cut-off.

³³The 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).

after the 2014 law, suggesting that our results may underestimate the true labor-reducing effects of the law.

Alternatively, one might think that the 2014 law increased the salience of the harm caused by work for young children and made parents more reluctant to admit their children were working. If this were the case, the reduction in child work that we document could simply reflect reduced parental reporting of work for children under 14 rather than an effect of the law on work. In this scenario, we would expect the stigma surrounding child work to be especially strong for younger children; however, we find no statistically significant effects at the younger thresholds (see Panels B and C of Table 4). This suggests that this type of systematic misreporting is unlikely to explain our results at the 14-year-old threshold. Recall that we define treated children as the *older* children at the younger thresholds (because they receive expanded workers' rights following the 2014 law relative to younger children); thus the results in Panels B and C indicate that, if anything, older children (who should be less subject to stigmas surrounding work, relative to younger children) were less likely to be reported as working after 2014. Overall, we find little reason to believe that social desirability bias plays a role in our estimates.

The 2014 law allowed the participation of children in family and community activities without age restrictions as long as the activities contribute to children's integration into the community or to the development of skills and did not represent exploitation, interfere with a child's education, or entail potentially risky activities. Examples include working in a communal farm or working for community organizations. While our data do not allow us to identify specific types of family or community labor (to which these exceptions apply), work for this purpose is rare; in 2016, only 6% of working children report maintaining family or community customs as the main reason for working.³⁴ Furthermore, 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 who identify as indigenous (defined as municipalities with an above-median share of indigenous residents), where communal work and family work related to cultural traditions are likely

³⁴For children ages 7-17. Authors' calculations using the 2016 ENNA.

more prevalent and where these exceptions to the law are more likely to apply.³⁵

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

Second, Kamei (2020) uses a difference-in-difference strategy that compares 12-13-year-old boys to a pooled control group of boys aged 7-9 and 14-16. In contrast, given the steep age gradient in work probabilities observed in the data, we employ a difference-in-discontinuity strategy that compares children just above and below the age thresholds, before and after the law. This enables us to attenuate potential confounders related to labor market shocks that can differently affect younger children (7-9) and older children (15-16). Nonetheless, we examine the robustness of our results to an alternate difference-in-difference specification similar to Kamei (2020). Specifically, we pool 9- and 14-year-old children into a single control group and estimate separate treatment effects for a younger treatment group (children that are at least 10 but younger than 12) and an older treatment group (who are at least 12 but younger than 14). We find that the point estimates of the effect of the law are negative for both the younger and older treatment groups, though it is only significant for the older treatment group (Appendix Table A.9, column 1), which is consistent with our main results around the 14-year-old cutoff.³⁶ However, when we expand our pooled control group to include children as young as 7 and as old as 16 as in Kamei (2020), the coefficients drop

³⁵Municipalities 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.

³⁶It is important to note that this alternate specification does *not* identify the same effects as our main specification because the treatment and control groups are not the same.

in magnitude and are not statistically significant, although they remain negative. These changes may reflect potential violations to the identification assumption for this pooled difference-in-difference specification —that in the absence of the policy change, the work outcomes would have evolved similarly for the 7- and 16-year-old control groups and the younger and older treatment groups.

5.2 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 (in addition to enforcing existing labor regulations) 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 5.2, MTEPS inspector and 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 located close 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 and work for a firm with a national tax registration, even after controlling for job and worker characteristics that are likely correlated with distance to MTEPS offices (such as education and sector of work).

Accordingly, we exploit cross-municipality variation in the driving time to regional MTEPS offices (those in charge of conducting inspections) to proxy for variation in the probability of workplace inspections. We compare the effects of the law on work probabilities between mu-

nicipalities that are “far” and “near” from the nearest regional Ministry of Labor (MTEPS) office, where “far” is defined as above the median driving time.³⁷ 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.

Panel A in Table 5 illustrates that the law significantly decreases the likelihood of allowed/regulated work for 13-year-olds relative to 14-year-olds, but only in areas that are located near MTEPS offices, where there was likely to be stronger enforcement (column 1). This remains true when we further restrict the sample to municipalities that do not contain an MTEPS office (column 1), illustrating that the result is not being driven only by large, mostly urban municipalities.³⁸ These results are robust to using straight line or “as the crow flies” distance as an alternative measure of distance to MTEPS offices (see Panel B). While the effects are not statistically distinguishable across areas near and far from MTEPS offices, the point estimates suggest that the overall declines in child labor are almost exclusively driven by children in localities closer to enforcement offices.³⁹ 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.10), 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 declines in child work due to the law. We discuss the mechanisms behind these results below.

6 Mechanisms

The impact of the law was not *ex ante* obvious. On the one hand, enabling young children to obtain work permits should draw more children into the labor force, if there was indeed demand for such permits. On the other hand, the increased threat of inspections by the

³⁷We measure the driving time from the municipality capital, typically the most populated locality in the municipality, to the nearest MTEPS office. See Appendix Section 4 for details.

³⁸These 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.

³⁹To show that our results are not driven by differences across urban and rural areas, we also report the p-value for the difference when additionally controlling for all possible interactions between the treatment variables, the post-law indicator, and urban status. These additional controls do not change the results.

Ministry of Labor, Employment, and Social Protection (MTEPS) may have discouraged the hiring of younger children. The fact that we observe a *decline* in child labor due to the law is consistent with the idea that the new legislation increased the perceived costs of employing 13-year-old children, relative to 14-year-olds.

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. We discuss these mechanisms below.

6.1 Worker Safety and Hiring 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.⁴⁰ In Appendix Table A.11, we show balance on these characteristics across the age thresholds and survey rounds (after re-weighting) using random subsamples that were not used in calculating the weights.⁴¹ Second, with only

⁴⁰To calculate the weights, we pool the observations from a randomly chosen 70% subsample from each survey and then predict the likelihood of appearing in the 2016 nationally representative ENNA using a Probit model based on demographic characteristics of children and their households. We then use these predicted probabilities (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.

⁴¹We 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

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.⁴² We additionally include cutoff fixed effects, which ensures that our estimates continue to be based on local comparisons around each age cutoff.⁴³ 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.

In the pre-law data (upper panel of Figure 5), we observe no discontinuity across the stacked thresholds for facing risks at work or having been injured on the job. In the post-law data (lower panel of Figure 5), treated children face slightly less risk at work following the 2014 law, but this difference does not appear to be substantial.

Turning to the regression-based evidence, 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).⁴⁴ 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. One concern is that because the law reduced child work around the 14-year-old cutoff, our reduced-form results do not accurately capture the true impacts of the law on risks outcomes among children who remain working. We offer two pieces of evidence to rule out this concern. Columns 2 and 4 in Table 6 show that we are unable to detect significant differences in risk exposure and injuries when we

test balance.

⁴²Because 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.

⁴³Specifically, we estimate a slightly modified version of the specification in equation 2 that includes cutoff fixed effects. In estimating equation 2, 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.

⁴⁴There is no evidence of differential changes in sample composition across any of the age cutoffs in the child labor survey (Appendix Table A.12).

focus only on children who report working. While the point estimate on risk suggests a 3-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.13, we find relatively small, non-significant effects for younger children—those for which we found no effects of the law on work probabilities. The results are robust to alternative specifications.⁴⁵

The lack of substantial declines in risky activities suggest that compliance with costly safety regulations was not a key driver of the decline in employment among 13-year-olds. One alternative explanation 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 7, 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 9 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.⁴⁶ 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).

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. One explanation is that most employers in Bolivia are informal employers, which employ 85% of the adult workforce in Bolivia (Elgin et al., 2021) and the overwhelming majority of child workers (see Table 1). Thus, one should expect that the effects are explained by the responses of informal employers.

⁴⁵Appendix Table A.14 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).

⁴⁶To 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 The Threat of Workplace Inspections

Informal 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.

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 firms located nearer to MTEPS offices (where visibility to inspectors is particularly relevant) are less likely to hire younger workers (see Table 5).

One empirical implication of this mechanism is that the declines in child work should be driven by firms with greater visibility to inspectors. 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 significant decline in the probability of working at a fixed location but neither substantial nor significant effects on the probability of working at home or in a mobile location. This suggests that the decline in overall employment among 13-year-olds due to the law is largely explained by a contraction in the employment of children who worked in more traceable and visible locations.

In Panel B, 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 9 percentage points when the law was enforced, relative to 14-year-old workers. In contrast, we observe a 7-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.

We also provide an additional piece of evidence consistent with the idea that the law deterred the hiring of younger children. Based on the idea that larger firms are more likely to be targeted by regulators while small firms may be more likely to remain under the radar (Almeida and Carneiro, 2009)⁴⁷, we test the extent to which the size of firms that hire children differentially declines for younger children, who were explicitly targeted by the law, relative to older children. In the face of the changes in regulation induced by the 2014 law, larger firms may decide to stop hiring workers targeted by the legislation in order to avoid unwanted attention from inspectors.

Specifically, we estimate the effects of the 2014 law on the size of firms that children work for, measured as the number of employees at the firm. Overall, we find evidence that younger children — for whom the 2014 legislation makes hiring particularly costly — tend to work for smaller firms following the passage of the 2014 law. Column 1 of Panel A in Table 7 shows that this is true when considering the sample of all children (where firm size is coded as zero for non-working children) and, more importantly, column 1 in Panel B illustrates that this effect is even larger for the sample of working children (though it is not precisely estimated). This finding is consistent with the notion that larger firms (which are more likely to be inspected) find it more costly to hire younger workers after the 2014 law and that, consequently, these younger children end up working for smaller firms.

The results suggest a novel mechanism through which the increase of regulations aiming at protecting workers may reduce employment. When regulation provides protections for workers whose work was previously not legally recognized and who were mostly hired infor-

⁴⁷Almeida 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.

mally (as in the case of child workers), the enforcement of such laws may increase the risk of inspections for employers and thus induce informal employers to rely less on the targeted workers so that they remain under the radar of regulation. Thus, the declines in employment often associated with increased regulations may not always relate to increased hiring costs for complying formal firms. Instead, they may reflect the optimal responses of informal firms.

6.3 Other Mechanisms

The lack of employment effects at the 10- and 12-year-old thresholds may be explained by two non-exclusive mechanisms. First, younger children—i.e., those below the age of 12 years old—are simply less likely to work, suggesting that the prior legislation was not a binding constraint for them.⁴⁸ Second, the costs and complexity of the application process may have lowered the demand for permits. To qualify for a permit, children first had to be declared fit to work by a doctor following a medical exam, and then visit the closest Child Advocacy office (DNA), often in a different locality. These transaction costs may deter children from legally entering 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.8), who are least able to pay the costs of obtaining a permit.

The high transaction costs of obtaining a working permit stem from low investments in DNA offices. 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 (*Defensora del Pueblo*) found only 12% of surveyed municipalities kept records of child and adolescent labor (Defensoría del Pueblo, 2021). As a result, the main mechanisms under which the law could have induced higher legal child labor were shut down.

⁴⁸We observe very low levels of employment for this age group (9.7%) before 2014 (see Appendix Figure A.3).

7 Conclusion

Overall, we find no evidence that the 2014 law increased child work in Bolivia. In fact, we find that children under age 14 were less likely to work in permitted and regulated activities after the passage of the law (relative to children over age 14). We posit that this is primarily due to the increased perceived costs of hiring younger children that the 2014 law imposed on employers — both through increased scrutiny and threat of inspections for firms hiring young children and through the new regulations that granted rights and protections to working children under 14. As some have claimed, “For adolescents, the code frequently had the effect that companies preferred to hire adults rather than jump over bureaucratic hurdles” (Liebel (2019)). Indeed, we find that after the key child labor components of the law (those granting rights and protections to workers under the age of 14) were repealed in 2018, work probabilities and hours of work 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.

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

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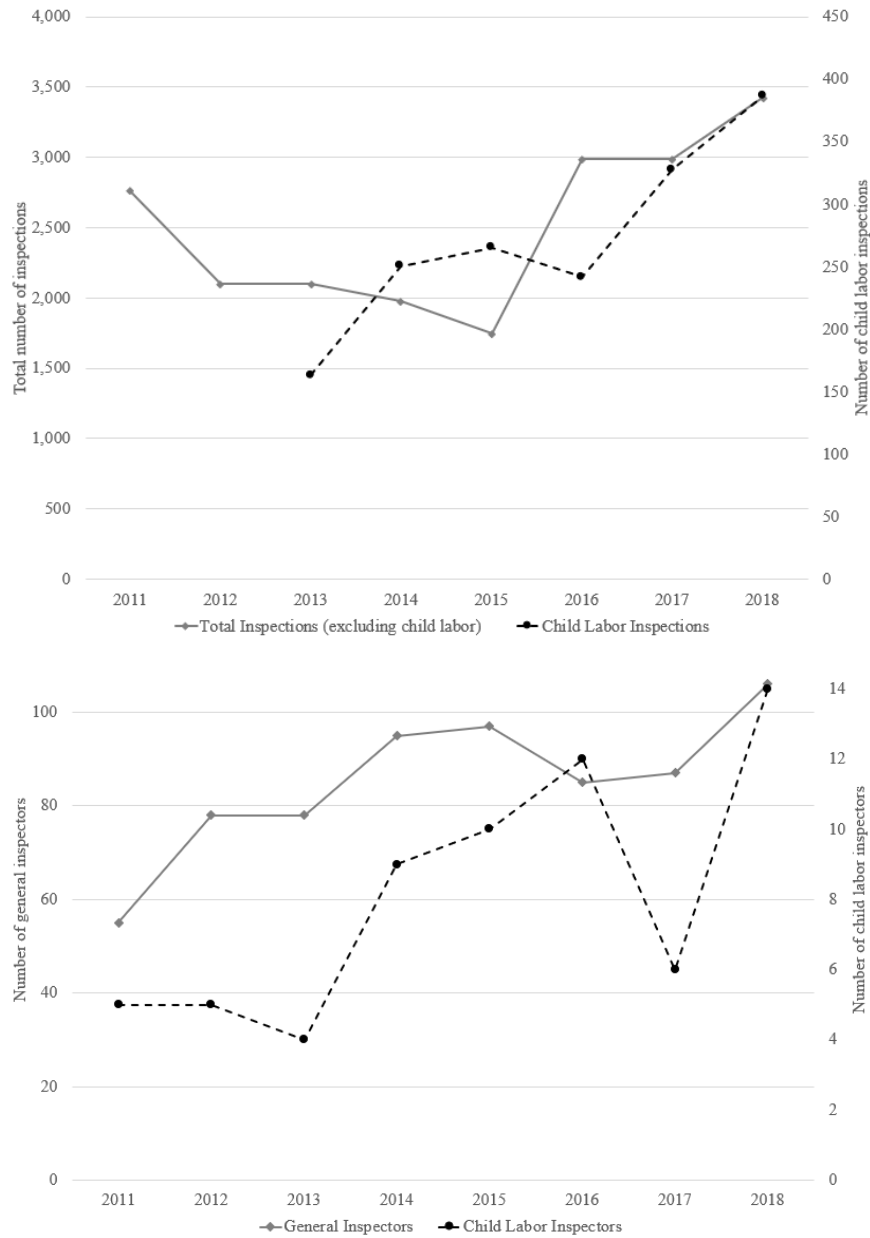
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Figure 1: Ministry of Labor Inspections and Inspectors over Time



Note: Data on inspections are 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). Data on the number of inspectors is obtained from the annual reports published by the US Department of Labor (U.S. Department of Labor, 2019). The total number of child labor inspections and inspectors are measured in the secondary axes.

Table 1: Descriptive Statistics (Pre-Law)

Panel A: Household Data		
	All Children (1)	Working Children (2)
Any work	.143	-
Hours worked	3.049	21.321
Work for self	.003	.018
Work for others	.14	.982
Work for external employer	.014	.1
Work for family employer	.126	.881
Prohibited work	.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
Works for a Formal Firm	.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 (1)	Working Children (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 3. The list of prohibited tasks appears in Appendix 2. 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.

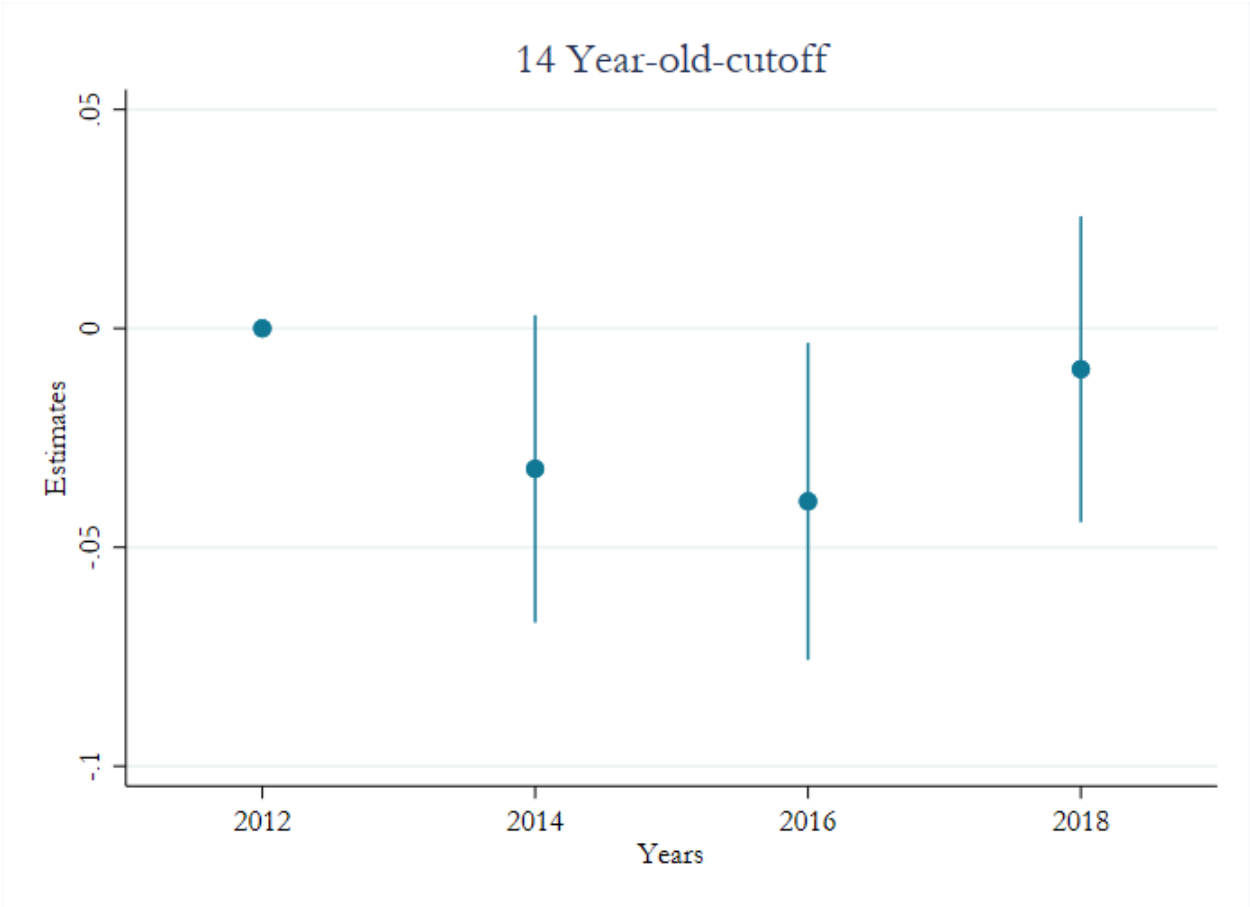
Table 2: Descriptive Statistics by Employer Type (Pre-Law)

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 3. 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



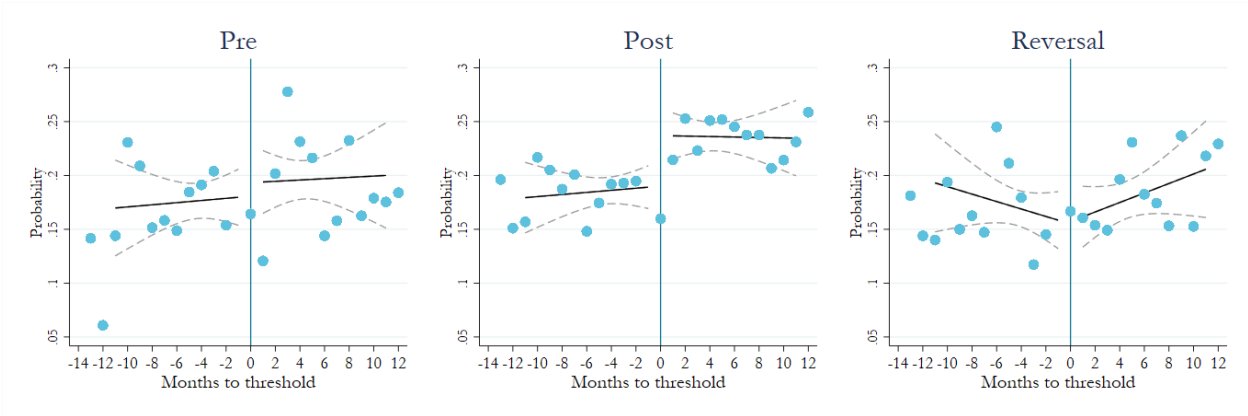
Note: The figure reports changes in work probabilities for 13 year olds relative to 14 year olds over time (grouped in two-year bins), with respect to the years preceding the policy change (2012-2013). Control variables: CCT eligibility indicator, household head characteristics (schooling, gender, age, 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 household level.

Table 3: Effects of the Law and Reversal on Work Probability for the 14-year-old cutoff
(Difference-in-Difference)

	Without Controls (1)	With Controls (2)
Post Law $\times \mathbb{1}\{\text{Age} < 14\}$	-0.0293* (0.0173)	-0.0375** (0.0154)
Post Reversal $\times \mathbb{1}\{\text{Age} < 14\}$	0.00723 (0.0197)	-0.0110 (0.0175)
Obs.	11991	11991
Mean	0.180	0.180

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: in grade for CCT, 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.

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.

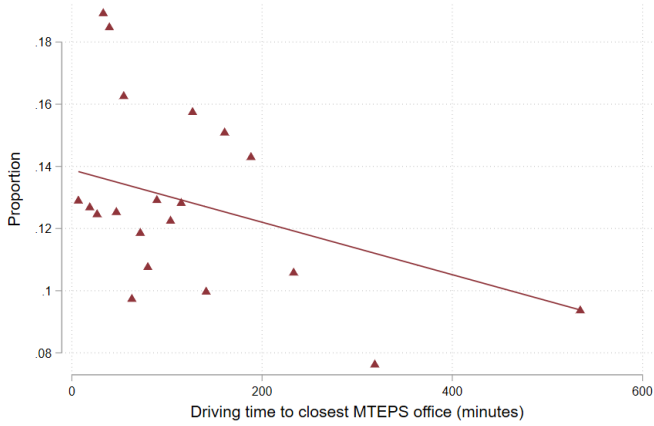
Table 4: Difference in Discontinuity Effects of the Law on the Work Probabilities, Hours, and Occupation for the 14-Year-Old Cutoff

	Any Work (1)	Hours Worked (2)	Work for Self (3)	Work for Others (4)	Prohibited Work (5)	Allowed Work (6)
Post Law $\times \mathbb{1}\{\text{Age} < 14\}$	-0.0394** (0.0177)	-0.969* (0.572)	-0.00194 (0.00466)	-0.0374** (0.0174)	0.00366 (0.00571)	-0.0430** (0.0174)
Post Reversal $\times \mathbb{1}\{\text{Age} < 14\}$	-0.000471 (0.0197)	0.508 (0.577)	-0.000448 (0.00503)	-0.0000226 (0.0193)	0.0183* (0.0110)	-0.0187 (0.0185)
Obs.	11991	11991	11991	11991	11991	11991
Mean	0.180	4.397	0.00490	0.175	0.0114	0.169

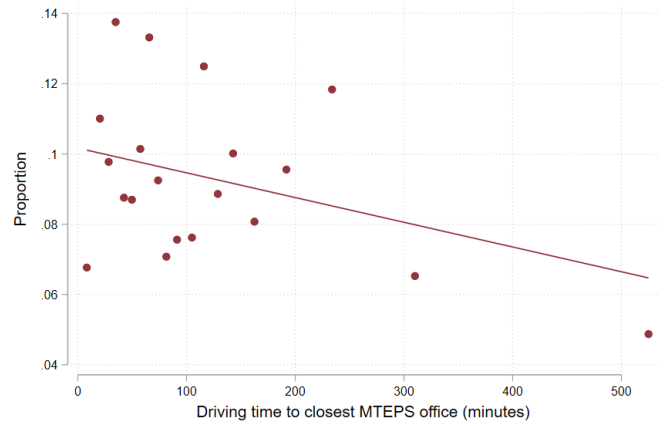
Notes: Household-level clustered standard errors in parentheses. Significance levels denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Control variables: CCT eligibility indicator, household head characteristics (schooling, gender, age, and indigenous indicator), gender, number of children in the 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: 2012-2019.

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

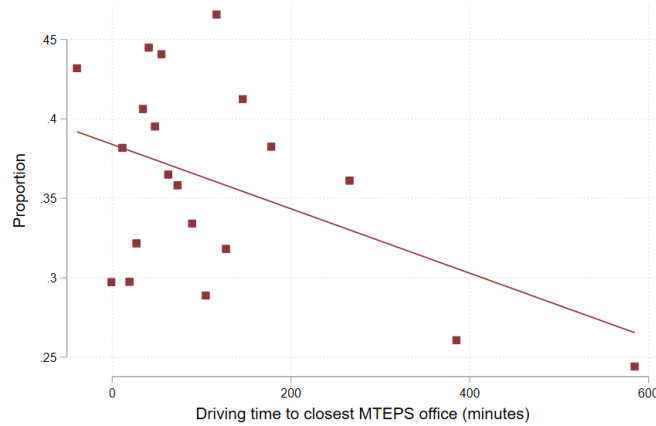
(a) Formal Labor Contracts



(b) Formal Registration



(c) Health Insurance



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

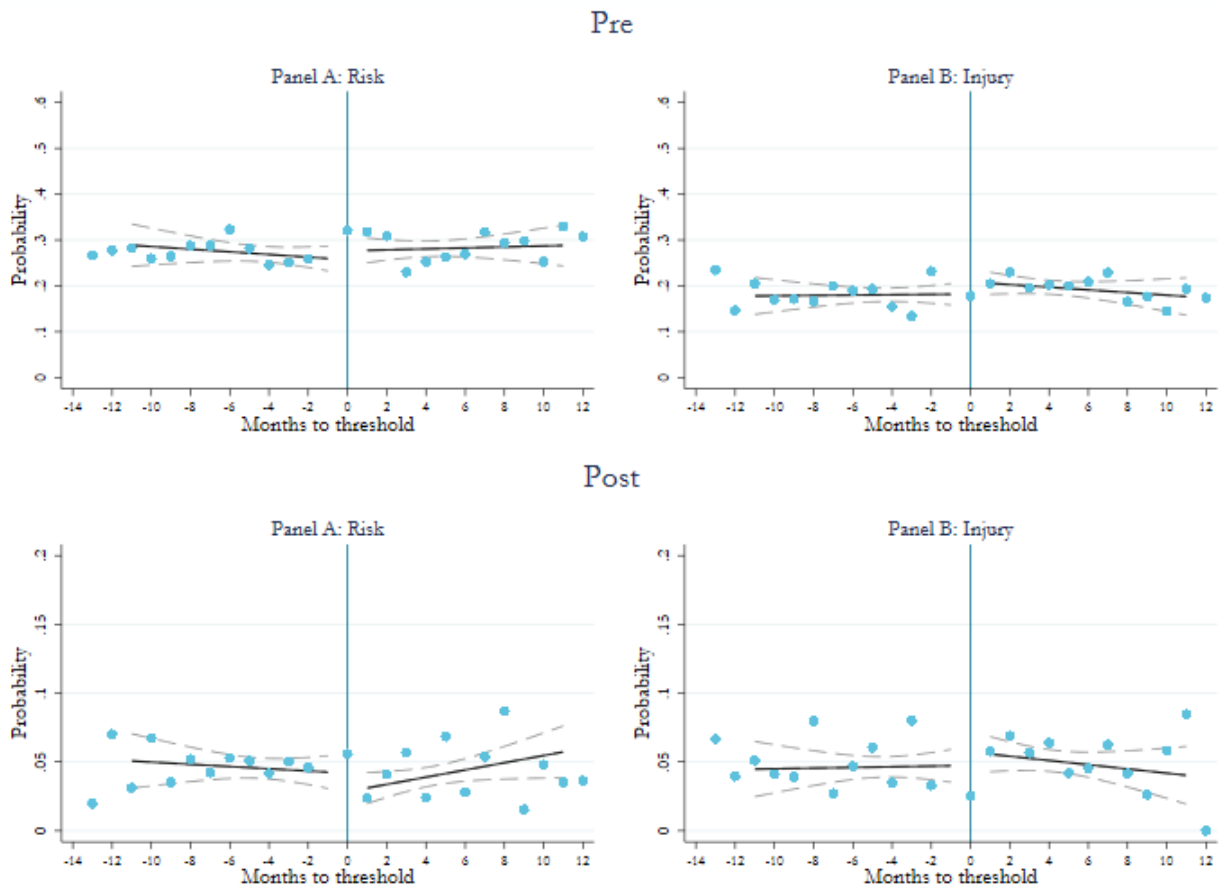
Table 5: Heterogeneous Effects of the Law by Distance from MTEPS Offices
(Difference-in-Discontinuity)

Panel A: Driving Time		
	Dependent Variable: Works	
	All (1)	No MTEPS Offices (2)
Post \times $\mathbb{1}\{\text{Age} < 14\}$ for Far	0.000737 (0.0543)	-0.00695 (0.0535)
Post \times $\mathbb{1}\{\text{Age} < 14\}$ for Near	-0.0325* (0.0197)	-0.0867** (0.0439)
Obs.	7650	2984
Mean	0.180	0.317
P-value of difference	0.565	0.247
P-value of difference (urban controls)	0.330	0.221

Panel B: Direct Distance (“as the crow flies”)		
	Dependent Variable: Works	
	All (1)	No MTEPS Offices (2)
Post \times $\mathbb{1}\{\text{Age} < 14\}$ for Far	0.0109 (0.0562)	0.0165 (0.0549)
Post \times $\mathbb{1}\{\text{Age} < 14\}$ for Near	-0.0346* (0.0193)	-0.0912** (0.0438)
Obs.	7650	2984
Mean	0.180	0.317
P-value of difference	0.444	0.125
P-value of difference (urban controls)	0.254	0.107

Notes: Household-level clustered standard errors in parentheses. Significance levels denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Municipalities that are classified as Far are above the median 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 pre-law period.

Figure 5: 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 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 \times Treated	-0.00777 (0.0171)	-0.0377 (0.0383)	-0.0145 (0.0151)	-0.0153 (0.0342)	0.103 (0.157)
Post Reversal \times $1\{\text{Age} < 14\}$					-0.0118 (0.168)
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: Household level 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 discontinuity 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. Samples: 2008 ENTI and 2016 ENNA in columns 1 to 4 and 2012-2019 household surveys 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 (1)	Works in Fixed Location Out of Home (2)	Works in Mobile Location Out of Home (3)	Works at Home (4)
Post Law $\times \mathbb{1}\{\text{Age} < 14\}$	-0.267** (0.122)	-0.0506*** (0.0161)	0.00855 (0.00832)	0.00266 (0.00425)
Post Reversal $\times \mathbb{1}\{\text{Age} < 14\}$	-0.0332 (0.107)	-0.00862 (0.0168)	0.00482 (0.0131)	0.00332 (0.00386)
Obs.	11918	11991	11991	11991
Mean	0.853	0.149	0.0248	0.00588

Panel B: Working Children

	Firm Size (1)	Works in Fixed Location Out of Home (2)	Works in Mobile Location Out of Home (3)	Works at Home (4)
Post Law $\times \mathbb{1}\{\text{Age} < 14\}$	-0.726 (0.496)	-0.0982** (0.0398)	0.0777** (0.0374)	0.0206 (0.0208)
Post Reversal $\times \mathbb{1}\{\text{Age} < 14\}$	-0.359 (0.405)	-0.0432 (0.0514)	0.0219 (0.0501)	0.0213 (0.0213)
Obs.	2250	2323	2323	2323
Mean	4.796	0.829	0.138	0.0327

Notes: Household-level 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 for “The Impact of
Expanding Worker Rights to Informal Workers:
Evidence from Child Labor Legislation”

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1 Appendix Figures and Tables

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

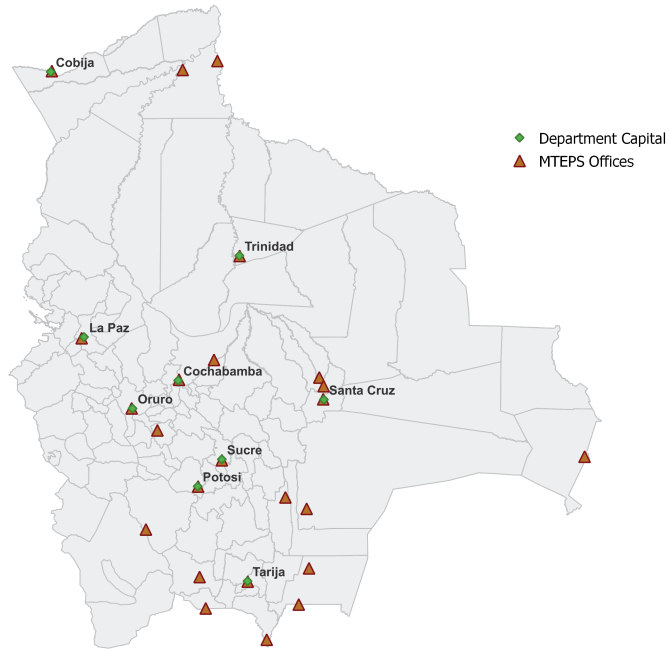
	Before 2014	After 2014
Age < 10	No legal work	No legal work ¹
10 ≤ Age < 12	No legal work	Legal to engage in independent work ²
12 ≤ Age < 14	No legal work	Legal to engage in independent work or work for others ² , with worker benefits and protections ³
Age ≥ 14	Legal to engage in independent work or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

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

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

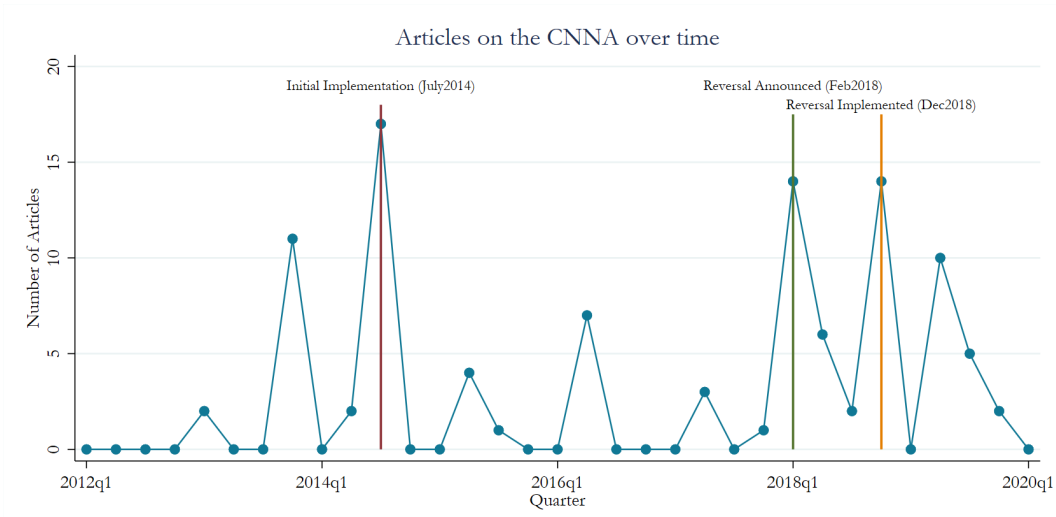
³Prior to 2014, only children age 14 and over were entitled to the same workers' rights as adults, including minimum wages and social security. After 2014, these rights were extended to working children age 12 and older and the benefits were expanded (for example, to include two paid study hours per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

Figure A.1: Ministry of Labor Offices



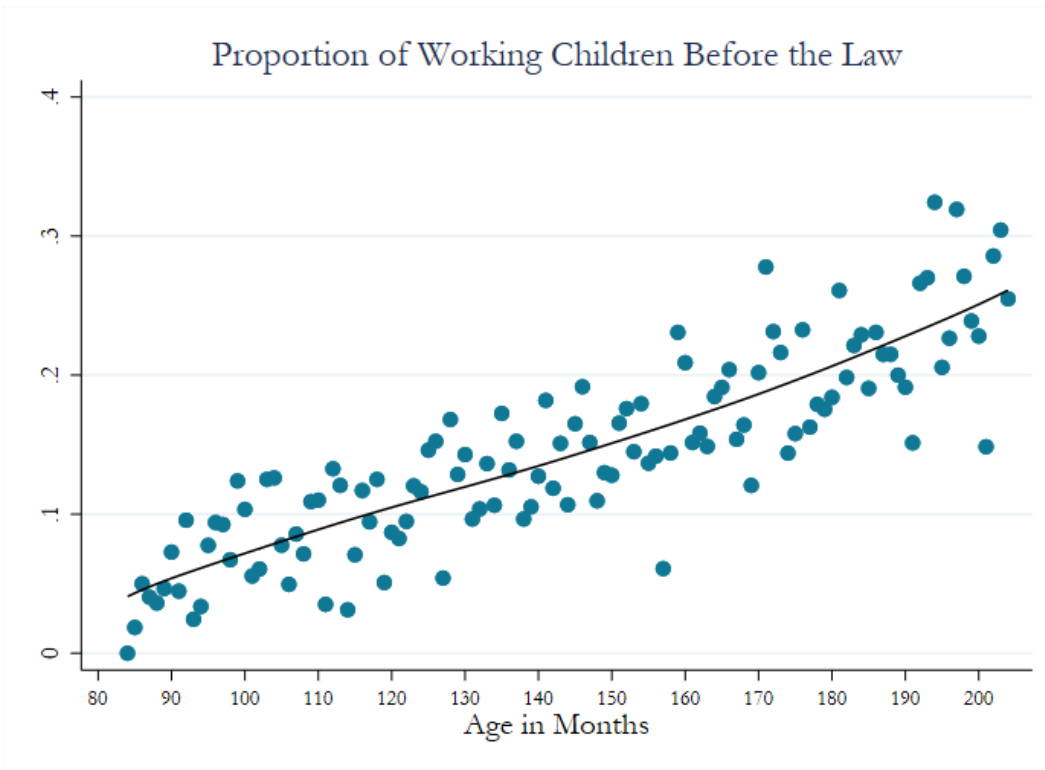
The addresses of permanent MTEPS offices can be found here: https://www.mintrabajo.gob.bo/?page_id=2626.

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



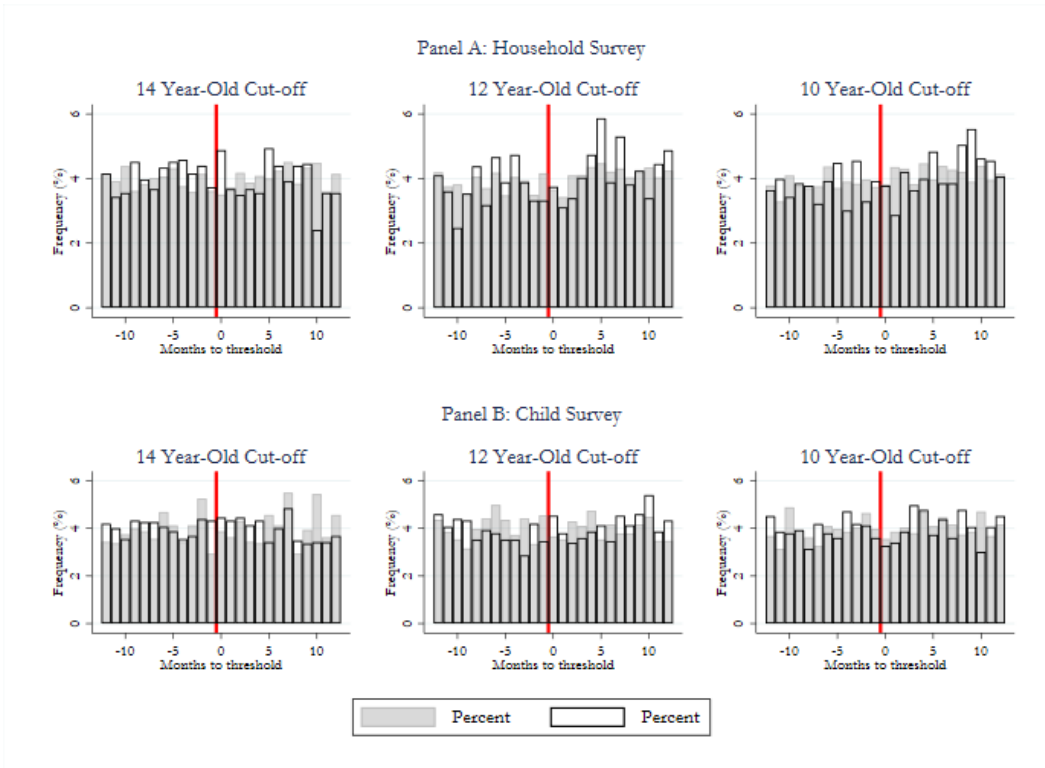
This figure tracks the number of articles concerning the 2014 law scraped from 43 national and regional Bolivian newspapers between 2012 and 2020. Articles that both mentioned the 2014 law and child labor were included.

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 pre sample includes 2008 and the post sample includes 2016.

Table A.2: McCrary Tests for Age at Survey Date

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

Panel B: Child Survey Data		
	Density test	
	Pre	Post
	(1)	(2)
$\mathbb{1}\{\text{Age} < 14\}$	-.0364 (.1088)	.0514 (.1147)
$\mathbb{1}\{\text{Age} \geq 12\}$.1159 (.1214)	-.0949 (.1123)
$\mathbb{1}\{\text{Age} \geq 10\}$	-.0972 (.1169)	-.1118 (.11)

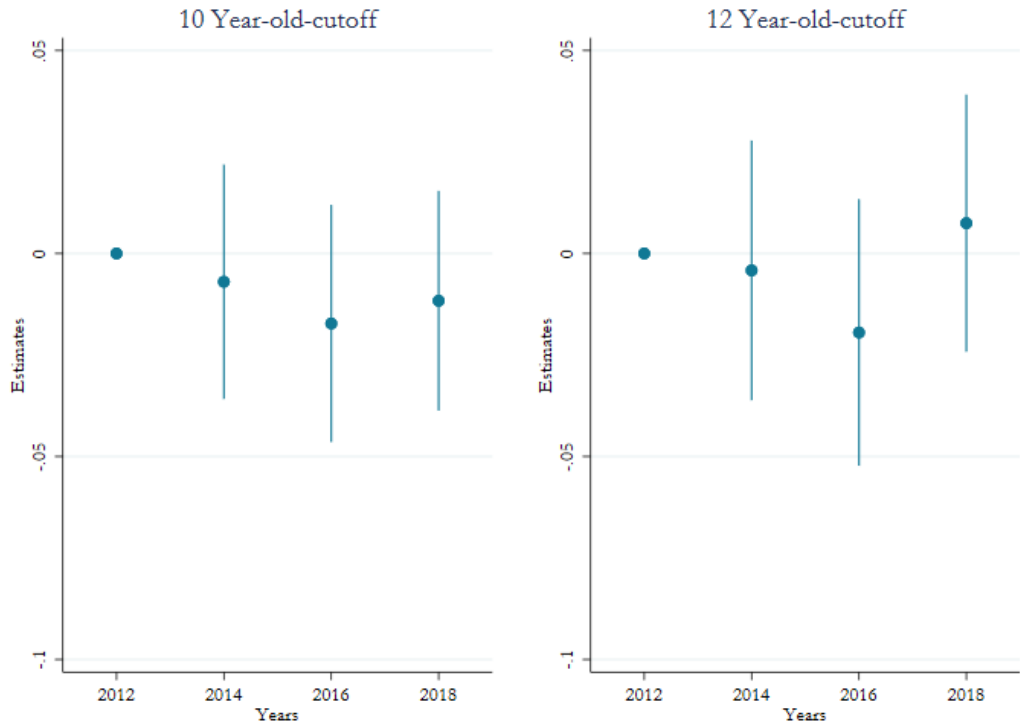
Notes: Significance levels denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The running variable in both panels is the difference between age in months and the age cut-off at the survey date. We use the DCdensity Stata command to implement the McCrary test, with a bandwidth of 12 months and a bin size of one month. In Panel A the pre sample includes 2012-2013 and the post sample includes 2014-2017. In Panel B the pre sample includes 2008 and the post sample includes 2016.

Table A.3: 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 $\times \mathbb{1}\{\text{Age} < 14\}$	0.197 (0.265)	-0.0197 (0.0212)	-0.412 (0.565)	0.0270 (0.0244)	-0.0346 (0.0256)	-0.0724 (0.0982)
Post Reversal $\times \mathbb{1}\{\text{Age} < 14\}$	0.310 (0.300)	-0.0108 (0.0253)	0.370 (0.636)	0.0322 (0.0281)	-0.00888 (0.0297)	-0.111 (0.107)
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 $\times \mathbb{1}\{\text{Age} \geq 12\}$	-0.184 (0.279)	-0.0283 (0.0218)	0.0642 (0.561)	-0.0203 (0.0251)	-0.0502* (0.0264)	0.0933 (0.101)
Post Reversal $\times \mathbb{1}\{\text{Age} \geq 12\}$	-0.295 (0.310)	-0.0110 (0.0251)	-0.133 (0.642)	0.0126 (0.0284)	-0.0429 (0.0304)	-0.00202 (0.110)
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 $\times \mathbb{1}\{\text{Age} \geq 10\}$	-0.115 (0.284)	-0.0358* (0.0215)	0.577 (0.586)	-0.00306 (0.0258)	0.0361 (0.0270)	0.0560 (0.101)
Post Reversal $\times \mathbb{1}\{\text{Age} \geq 1\}$	0.129 (0.313)	-0.0434* (0.0249)	-0.159 (0.640)	0.0182 (0.0286)	0.0220 (0.0305)	-0.00759 (0.107)
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: Household level 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: 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.

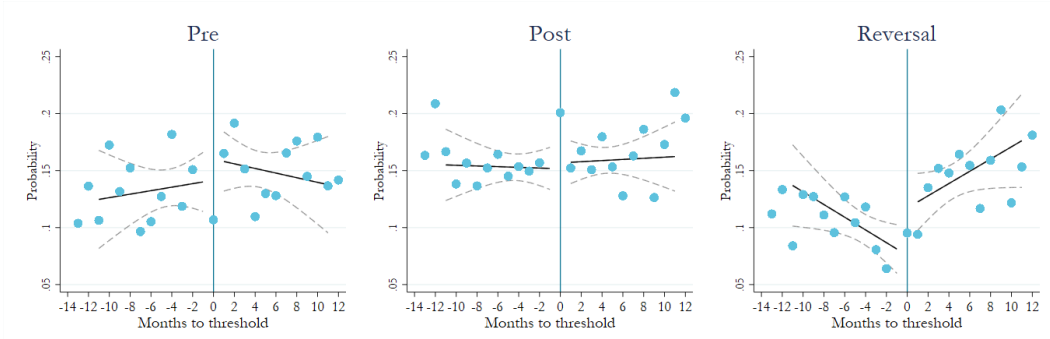
Table A.4: Difference in Difference - Work Probability

Panel A: 12-Year-Old Cutoff		
	Without Controls (1)	With Controls (2)
Post Law $\times \mathbb{1}\{\text{Age} \geq 12\}$	-0.00931 (0.0152)	-0.00910 (0.0140)
Post Reversal $\times \mathbb{1}\{\text{Age} \geq 12\}$	0.0223 (0.0169)	0.00954 (0.0157)
Obs.	11719	11719
Mean	0.142	0.142

Panel B: 10-Year-Old Cutoff		
	Without Controls (1)	With Controls (2)
Post Law $\times \mathbb{1}\{\text{Age} \geq 10\}$	-0.00707 (0.0136)	-0.00796 (0.0126)
Post Reversal $\times \mathbb{1}\{\text{Age} \geq 10\}$	-0.00861 (0.0143)	-0.00820 (0.0135)
Obs.	11801	11801
Mean	0.105	0.105

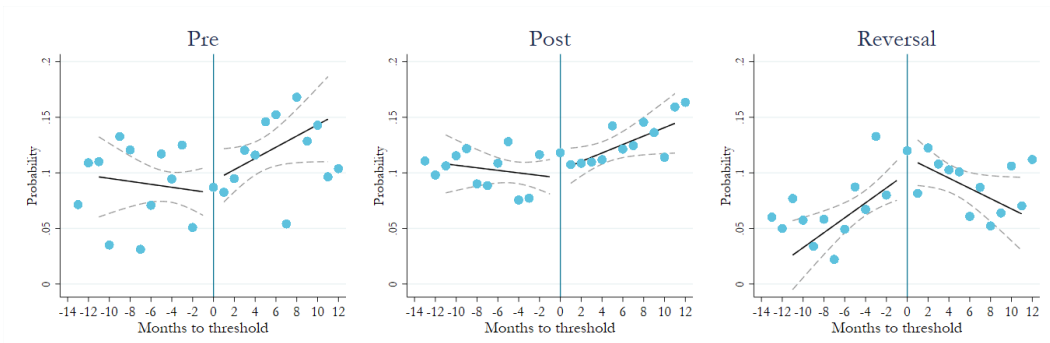
Notes: Household level clustered standard errors in parentheses. Significance levels denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The control variables are: an indicator for urban areas, household head characteristics (schooling, gender, age, and indigenous indicator), gender, number of children 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.

Figure A.6: 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.7: 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.5: 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 $\times \mathbb{1}\{\text{Age} \geq 12\}$	-0.0136 (0.0161)	-0.339 (0.407)	-0.00130 (0.00285)	-0.0123 (0.0160)	-0.00334 (0.00371)	-0.0103 (0.0159)
Post Reversal $\times \mathbb{1}\{\text{Age} \geq 12\}$	0.0148 (0.0180)	0.231 (0.428)	-0.00526* (0.00279)	0.0201 (0.0179)	0.00267 (0.00891)	0.0122 (0.0168)
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 $\times \mathbb{1}\{\text{Age} \geq 10\}$	-0.0169 (0.0143)	-0.199 (0.331)	0.00152 (0.00157)	-0.0184 (0.0143)	-0.00283 (0.00239)	-0.0140 (0.0143)
Post Reversal $\times \mathbb{1}\{\text{Age} \geq 10\}$	-0.0132 (0.0158)	-0.316 (0.320)	0.000421 (0.00184)	-0.0136 (0.0158)	0.000619 (0.00807)	-0.0138 (0.0145)
Obs.	11801	11801	11801	11801	11801	11801
Mean	0.105	1.788	0.000748	0.104	0.00150	0.103

Notes: Household-level 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.6: Effect of the Law on Schooling Attendance and Household Chores

	Attends School (1)	Minutes Spent on Chores (2)
Post law \times Treated	0.0135 (0.0113)	-13.79 (19.27)
Post reversal \times Treated	-0.0103 (0.0119)	
Obs.	11498	8372
Mean	0.955	407.0

Notes: Household-level 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 discontinuity 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.7: 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 $\times \mathbb{1}\{\text{Age} < 14\}$	-0.000662 (0.00956)	-3.842 (2.769)	-18.20 (38.85)
Post reversal $\times \mathbb{1}\{\text{Age} < 14\}$	-0.00717 (0.0108)	2.444 (2.946)	-36.34 (42.64)
Obs.	10788	10788	10788
Mean	0.969	94.26	908.6

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

	14-Year-Old Cutoff									
	Bandwidth (months)			No Controls	Quadratic	Polynomials Pre-Post		Exact Age	Donut	Excl. Indig.
	Baseline					Linear	Quadratic			
	6	12	24							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post law $\times \mathbb{1}\{\text{Age} < 14\}$	-0.0335 (0.0248)	-0.0394** (0.0177)	-0.0274** (0.0124)	-0.0313 (0.0201)	-0.0397** (0.0177)	-0.0322 (0.0334)	-0.0394** (0.0177)	-0.0410 (0.0260)	-0.0299 (0.0190)	-0.0338* (0.0186)
Post reversal $\times \mathbb{1}\{\text{Age} < 14\}$	0.00476 (0.0275)	-0.000471 (0.0197)	-0.000176 (0.0139)	0.0147 (0.0226)	-0.000959 (0.0196)	0.0167 (0.0371)	-0.00000930 (0.0197)		0.0124 (0.0209)	
Obs.	5983	11991	24340	11991	11991	11991	11991	4706	11057	6481
Mean	0.188	0.180	0.180	0.180	0.180	0.180	0.180	0.194	0.183	0.111

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 (days in column 8) and the age cut-off a week before the survey date. We include linear splines of the running variable, an indicator for 2014 and after, and an indicator that is one for the children in the corresponding age group. Column 5 also includes quadratic splines of the running variable. Column 6 includes linear splines that vary across both sides of the cut-off and before and after the law. Column 7 has linear and quadratic splines that vary across both sides of the cut-off and before and after the law. Column 8 uses exact interview date to calculate age at survey. Column 9 omits children within 1 month of the age threshold. Column 10 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. We use a triangular kernel. The sample includes 2013, 2014, and 2016 for column 8; 2012-2016 for column 10; and 2012-2019 for all other columns.

Table A.9: Difference in Difference: Work Probability

	Control: 9 and 14 year olds Work Probability (1)	Control: 7-9 and 14-16 year olds Work Probability (2)
Post Law $\times \mathbb{1}\{10 \leq \text{Age} < 12\}$	-0.0124 (0.00942)	-0.00246 (0.00749)
Post Law $\times \mathbb{1}\{12 \leq \text{Age} < 14\}$	-0.0184* (0.00944)	-0.00856 (0.00766)
Post Reversal $\times \mathbb{1}\{10 \leq \text{Age} < 12\}$	-0.00837 (0.0103)	-0.000928 (0.00820)
Post Reversal $\times \mathbb{1}\{12 \leq \text{Age} < 14\}$	-0.0000314 (0.0107)	0.00849 (0.00875)
Obs.	35511	53490
Mean	0.144	0.137

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

Panel A: 12-Year-Old Cutoff		
	Dependent Variable: Works	
	All (1)	No MTEPS Offices (2)
Post \times $\mathbb{1}\{\text{Age} \geq 12\}$ for Far	0.0375 (0.0368)	0.0191 (0.0419)
Post \times $\mathbb{1}\{\text{Age} \geq 12\}$ for Near	-0.0207 (0.0173)	-0.0545 (0.0372)
Obs.	7313	2938
Mean	0.142	0.257
P-value of difference	0.109	0.0806
P-value of difference (urban controls)	0.326	0.119

Panel B: 10-Year-Old Cutoff		
	Dependent Variable: Works	
	All (1)	No MTEPS Offices (2)
Post \times $\mathbb{1}\{\text{Age} \geq 10\}$ for Far	0.0462 (0.0346)	0.0119 (0.0395)
Post \times $\mathbb{1}\{\text{Age} \geq 10\}$ for Near	-0.0243 (0.0154)	-0.0519 (0.0354)
Obs.	7148	2889
Mean	0.105	0.217
P-value of difference	0.0407	0.113
P-value of difference (urban controls)	0.291	0.581

Notes: Household-level clustered standard errors in parentheses. Significance levels denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Municipalities that are classified as Far are above the median 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.11: 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	-0.00357 (0.0202)	0.130 (0.110)	0.690 (0.476)	-0.149 (0.236)	0.0126 (0.0186)	0.0363 (0.0226)	0.00784 (0.0206)
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 = .36							

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.12: 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	-0.0409 (0.0254)	-0.0390 (0.104)	-0.766 (0.557)	0.234 (0.275)	0.00482 (0.0215)	0.00413 (0.0254)	0.0120 (0.0229)
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 = .606							

Notes: Household level 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.

Table A.13: 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 $\times \mathbb{1}\{\text{Age} < 14\}$	-0.00657 (0.0283)	-0.00121 (0.0281)
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 $\times \mathbb{1}\{\text{Age} \geq 12\}$	-0.0207 (0.0279)	-0.0155 (0.0250)
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 $\times \mathbb{1}\{\text{Age} \geq 10\}$	-0.0179 (0.0228)	-0.0247 (0.0233)
Obs.	2831	2817
Mean	0.214	0.166

Notes: Household level 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 6.1.

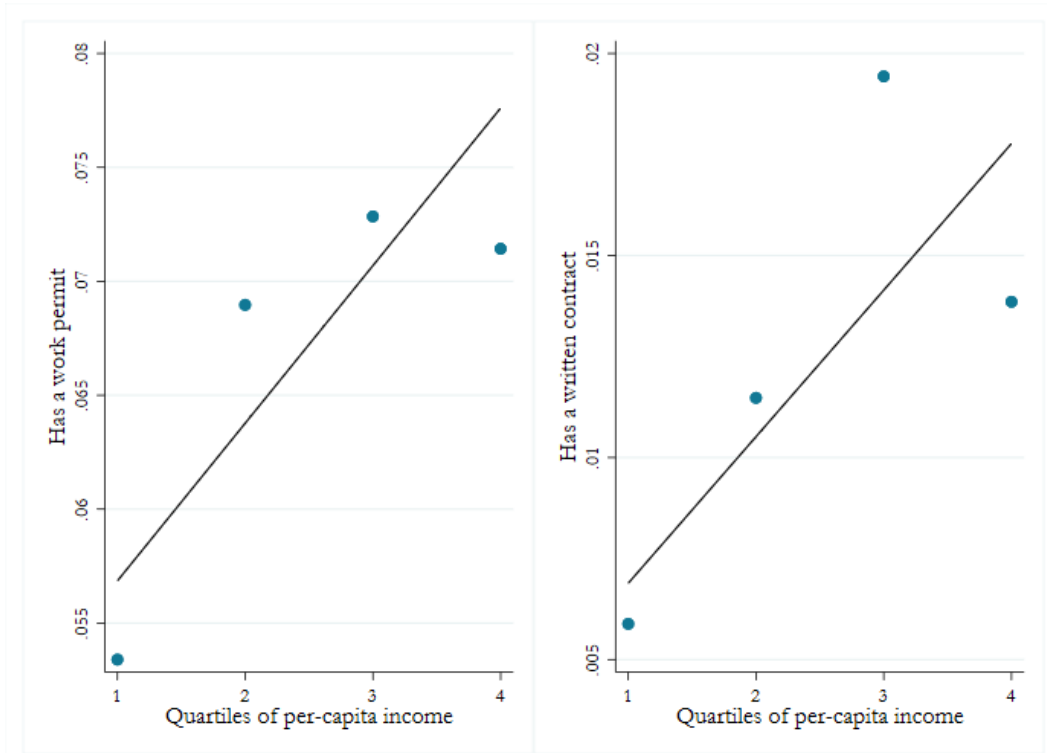
Table A.14: Robustness Checks: Difference in Discontinuity for Risk Outcomes

Panel A: Different Bandwidth Specifications						
	Risk Index			Injury Index		
	<i>Bandwidth (months)</i>					
	6	Baseline	24	6	Baseline	24
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated	-0.0121 (0.0243)	-0.00778 (0.0171)	-0.00922 (0.0149)	-0.00604 (0.0213)	-0.0146 (0.0151)	-0.00985 (0.0133)
Obs.	3981	8372	8872	4074	8411	8885
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	-0.0125 (0.0179)	-0.00698 (0.0171)	-0.00336 (0.0182)	-0.00753 (0.0157)	-0.0149 (0.0151)	-0.0363** (0.0166)
Obs.	8372	8372	7325	8411	8411	7351
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.8: 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.

2 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

3 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 2.
- Allowed work: Indicator equal to one if the child reports engaging in any other work that is not prohibited as detailed in Appendix 2.
- 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.

4 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 municipalities in two groups: Near (minimum distance below the cross-municipality median) and Far (minimum distance above the cross-municipality median).