The Role of Policy and Education Reforms in Economic Development: Microeconometric Analyses for Latin America

Inauguraldissertation

zur Erlangung der Würde

eines Doktors der Wirtschaftswissenschaft

(doctor rerum oeconomicarum)

an der Schumpeter School of Business and Economics

der Bergischen Universität Wuppertal

Dissertation

vorgelegt von

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aus Beirut

Wuppertal, im November 2022

Acknowledgments

This dissertation would not have been possible without the support and motivation of my advisors, colleagues, family, and friends.

First, I would like to thank my supervisor, Kerstin Schneider, for her encouragement, academic advice, and comments on each chapter in this dissertation. I also thank my second supervisor Werner Bönte for providing helpful comments. I would like to express my deepest thanks for my colleague and friend, Fernanda Martínez Flores, for teaching me how to work independently, always being there for me, and for co-authoring two chapters of this dissertation. Special thanks for Franz Westermaier for his insights and valuable feedback on each chapter.

I would like to thank further Christian Bredemeier for his valuable guidance and comments. Many thanks to Hans Frambach, working with you is my pleasure. During my work at the University of Wuppertal, I have gained a lot of experience by giving lectures, attending doctoral seminars, internal paper reading sessions, and lively discussions with different colleagues at the chair. I am grateful to have shared my office with Anna Makles. Thank you Anna for the nice environment at work and for having a sympathetic ear.

In addition, I would also like to thank the modest group of friends here in Germany. Thank you for making me feel home. A big thanks for Elfi and Horst Jentsch for being my family in Germany and for being the nicest grandparents to my children and jumping in to support me with my work whenever needed.

My biggest thanks goes to my parents, Elias and Viviane, without you none of this would have been possible. Thank you for believing in me and supporting me to finish the dissertation. Mom thank you for all the cooking, cleaning, and taking care of the children while I was writing my dissertation. Dad thank you for making sure that I go to school, because of you I am proud of the woman I am today. Many thanks also for the support and encouragement of my brother David and my sisters Carla and Mandy. Finally, I could not have completed this dissertation without the support and patience of my husband Elie and my children John Paul, Matthias, and Elena who provided a happy distraction to rest my mind away from my research.

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1 Introduction

In developing countries, poverty is considered as the main trigger of child labor especially in low income countries because it drives families to push their children to work when they face financial challenges or uncertainties (Edmonds and Pavcnik, 2005). The main poverty reduction strategies have mostly focused on improving human capital accumulation and increasing the labor force participation of vulnerable groups, in particular, by focusing on women and the youth. From 2000 to 2017, the number of people living in extreme poverty, that is, those who live on $1.9 or less a day, has fallen from 1.7 billion to 689 million (The World Bank, 2019). Simultaneously, child labor rates have declined by 94 million from 2000 to 2016 (ILO, 2017). Despite the improvement in child labor rates, this remains a problem. The ILO global estimates in 2020 show that 160 million children aged 5 to 17 years, out of which 63 million are girls and 97 million are boys, are still in child labor. As for the labor force participation of women, despite the commitments made to improve women's chances in the labor market, their labor force participation has been declining from 51% in 2000 to to 48% in 2017 (The World Bank, 2022a).

Unfortunately, the goal to end poverty has been interrupted by the COVID-19 pandemic (Coronavirus disease). This might push around 8.9 million children into child labor by the year 2022 (ILO, 2020a) because of the measures taken to deal with this disease, such as school closures. Moreover, due to lack of childcare support systems during the pandemic and the fact that women work mostly in the informal and services sector, the sectors mostly affected by the COVID-19 pandemic (ILO, 2022a), the labor force participation of those women decreased further to 46% in 2020 (The World Bank, 2022a).

Many international organizations, like the United Nations (UN), United Nations Children's Fund (UNICEF), and the International Labor Organization (ILO), have been trying to find effective policy measures to combat poverty; for example, through the implementation of Sustainable Development Goals (SDGs) to help guide the work of those institutions. Therefore, policies such as reducing poverty by educating children, raising public awareness, providing support services for working children, and inducing proper legislation and regulation for work have been implemented to eradicate child labor and to increase the labor force participation of women (ECLAC and International Labor Organization ILO, 2019).

Figure I shows the percentage of individuals in child labor in Latin America and the Caribbean, Asia and the Pacific, and Sub-Saharan Africa. Child labor is defined as work that deprives children from their childhood and is harmful for their mental and physical development (ILO, 2022).

As Figure I shows, child labor is decreasing through the years. By focusing on Sub-Saharan Africa, the figure shows that this region witnessed an increase in child labor rate since 2012. Yet, focusing on both Asia and the Pacific and on Latin America and the Caribbean, Figure I indicates that from 2008 to 2020, child labor rates have been decreasing. More precisely, in Sub-Saharan Africa, child labor rate in 2008 is almost the same as in 2020, 25.3% and 23.9%. In Latin America, child labor rate has decreased from almost 10% in 2008 to 6% in 2020, and in Asia and the pacific the decrease is from 13.3% in 2008 to almost 5.6% in 2020. This raises the question of which policies and regulations implemented to combat child labor have been the most effective. Policies to regulate or forbid child labor usually tackle the problem directly by establishing concrete bans such as introducing the minimum

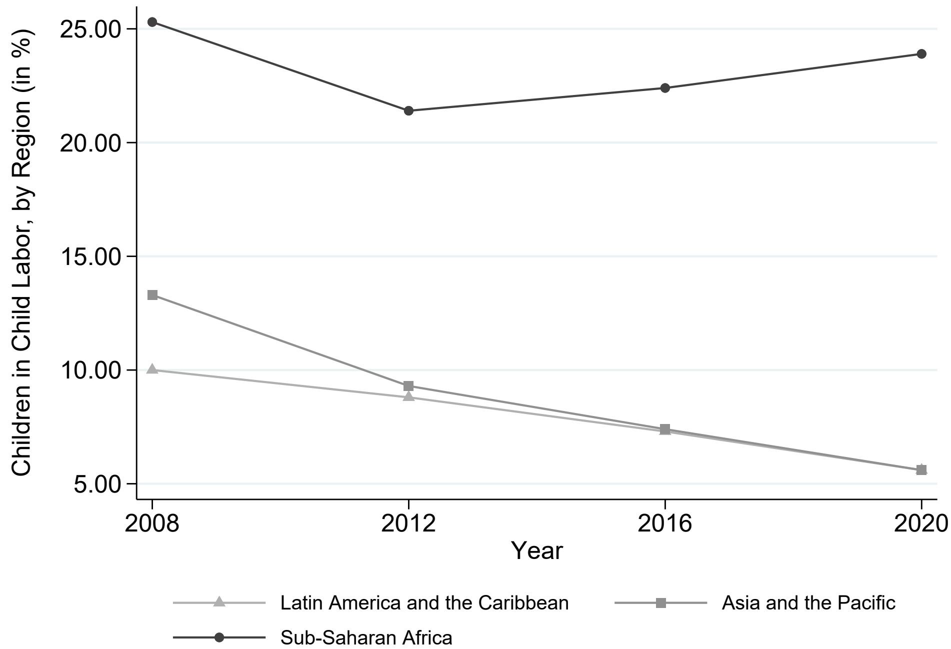

Figure I: Proportion of Children in Child Labor
Source: Author's analysis using data from ILO 2020 global estimates (ILO, 2020a).
Notes: – The figure illustrates the percentage of children in child labor by developing region according to the ILO 2020 global estimates. Data for other regions prior to the year 2016 are not available to be able to compare the regions.

working age (ILO, 2017), or indirectly by fostering school attendance and enrollment such as increasing the instruction time at school (UNESCO, 2015).

Therefore in this dissertation I focus on three important aspects on the role of policy and education reforms that led to the reduction in child labor rates through out the years and improved the labor force participation of women, for Latin America, taking the country Mexico in particular. First, I focus on evaluating the impact of a policy that targeted at increasing school enrollment and decreasing child labor rates both directly and indirectly. Second, I evaluate the importance of schooling coverage not only to decrease child labor, but also to increase the labor force participation of women with young children. Third, I examine the impacts in the context of the economics crises of the COVID-19 pandemic and the extent to which school closures affected the labor force participation of women with young children. Despite the improvements done in Mexico in school enrollment and child labor rates, 11% (3.2 million) of children aged 5 to 17 years old still engage in child labor. 6.4% of those children were involved in market work that is under the minimum age regulation, 4% performed domestic work in unsuitable conditions, and 0.7% combined both market and domestic work (INEE, 2018a). And despite the increase in the labor force participation of women in Mexico over the past 15 years, in 2019 45% of women compared to 78.5% of men are in the labor force (The World Bank, 2022b). Therefore, in spite of the improvements and the policy measures implemented a lot of work is still to be done to achieve the SDG goals.

This dissertation contributes to previous studies focusing on the impacts of direct and indirect child labor regulation. Direct regulation most commonly refers to employment bans that set a minimum working age for the admission of children to work at 15 (ILO, 2018). Studies have found that child labor bans are used to reduce poverty and alleviate child labor (Piza and Souza, 2017; Del Rey *et al.*, 2018). Therefore, most of the countries have now implemented labor laws that prohibit children under the age of 15 to work and regulate the work of those children until they reach 18 (Edmonds and Shrestha, 2012; ILO, 2018). If properly enforced those bans can change the age distribution and type of work children are doing (Edmonds, 2014). However, it is still not clear whether such bans will actually lead to the reduction of child labor. If poor families rely on child labor to obtain income and avoid hunger, introducing a child labor ban would not be effective (Basu, 1999). This would cause poor families to go below their subsistence level.

The empirical literature for developing countries on child labor shows that by implementing a child labor ban only in certain sectors, simply shifts children to work in other sectors or shifts work to slightly older children. This implies that well-intended regulations, if not implemented successfully, could easily backfire (Bharadwaj *et al.*, 2020). On the other hand, recent studies find that child labor bans decrease child labor, the decrease is mainly driven by boys (Piza and Souza, 2017). Others argue, that child labor bans have a small impact on child labor because the law was not enforced properly (Moehling, 1999). Therefore, a child labor ban becomes efficient when law enforcement is taken into account, through for example inspections (Bargain and Boutin, 2021).

For child labor bans to work, not only enforcement of the law is important but schooling should be made also compulsory because monitoring children at school is easier than monitoring the absence of children from work (Basu and Van, 1998). Of course, schooling and work are not mutually exclusive, that is, children can still go to school and work, but this will prevent the full-time employment of those children.

Next, policies indirectly targeting child labor, aim at decreasing poverty constraints, increasing schooling enrollment, or both. Previous studies have shown that programs such as elimination of school fees, making textbooks and uniforms for free, and introducing conditional cash transfers or in-kind transfers have proven to contribute to the decrease in child labor (Skoufias *et al.*, 2001; Maluccio, 2009; Peruffo and Ferreira, 2017; ILO, 2019b). Other policies aimed at increasing school enrollment have been implemented to fight against child labor and achieve the global goal of universal primary education (U.S. Department of Labor, 2019). Universal education is not only important to decrease child labor but is an important determinant for economic well-being (Hanushek and Woessmann, 2010). Studies analyzing the impact of education subsidies to increase school enrollment have shown large increases in school attendance but much lower decreases in child labor rates (Skoufias *et al.*, 2001; Ferro *et al.*, 2010; Arias *et al.*, 2010). Since several Latin American countries have achieved the goal of universal primary education, current education policies are shifting from increasing schooling access to improving schooling quality by increasing the time spent at school in order to fight child labor (UNESCO, 2015; ILO, 2019a). Studies have shown that by increasing the instruction time not only child labor decreases but also this has a positive impact on students achievements (Figlio *et al.*, 2018; Cabrera-Hernández, 2020; Thompson, 2021) and drop out rates and grade repetitions (García *et al.*, 2013). Yet, there are no studies done so far that check the impact of extending school time on child labor, which will be part of contribution of this dissertation.

In addition, education is not only important to decrease child labor but serves as a mean of support in child care for working women and thus also to decrease poverty. Therefore, studies have shown that increasing school access would increase maternal employment (Bick, 2016; Bauernschuster and Schlotter, 2015; Nollenberger and Rodríguez-Planas, 2015). Therefore, the absence of education for children such as school closures causes parents to suffer from the lack of child care and increases the need for home supervision, leading to the decrease in the labor force participation of those parents, especially mothers (Bick, 2016; Brilli *et al.*, 2016). Therefore, this pushes the families back to poverty and child labor (UNICEF, 2021b).

In this context, this dissertation covers three empirical analyses focusing on child labor bans, education, and mother's labor force participation in Latin America, specifically in Mexico. The first part of the dissertation contributes to the literature on child labor by examining the impact of policies aimed at decreasing child labor both directly and indirectly. The dissertation provides evidence on the efficacy of these policies and evaluates how they contribute to increasing school enrollment. Precisely, **Chapter 2** examines the impact of a complex child labor ban, coupled with minimum schooling requirements and concrete regulations for the work of individuals under the age of 18, on school enrollment and child labor. **Chapter 3** examines the importance of increasing quality education time at school on school enrollment and child labor. The second part of this dissertation contributes to the literature on labor force participation of mothers in developing countries. Therefore, **Chapter 4** examines the effect of school closures, in the context of the COVID-19 pandemic, on labor force participation of women. A summary of the research questions, main findings, contribution to the literature, and policy implication are summarized as follows:

Chapter 2 *(joint work with Fernanda Martínez. Data preparation, analysis, and text were equally distributed between co-authors.)* is motivated by the high number of working children aged 5 to 17 years old that engage in activities that are harmful to their development or that do not comply with the international standards of the minimum working age regulations (UNICEF, 2019). Policies such as child labor bans have been implemented as a mean to eradicate child labor through amending the minimum working age of children. Therefore, the ILO Convention No. 138 recommends the age of 15 for entry into the market (ILO, 2017). In this chapter, we investigate the impact of a child labor ban which was introduced in Mexico in 2015, on school enrollment and a number of child labor indicators.

While most of the studies in the literature find mixed evidence on the effect of bans on child labor, we contribute to the literature by focusing on a unique and more complex child labor ban that increases the minimum working age from 14 to 15 in all sectors and limits outside options for the affected children. To do so, we take advantage of the Constitutional Amendment in Mexico in 2014 that only announced the shift in the minimum working age and compare it to the more complex ban that was introduced by the reform to the Labor Law in 2015 where penalties, inspections, and regulations were introduced to prohibit the work of children under 15.

Our findings show that a simple increase in the minimum working age as the change in the Constitutional Amendment in 2014 does not affect child labor, it only has a small positive impact on school enrollment. However, when the ban in 2015 is coupled with concrete regulations of underage employment and penalties for employers, school enrollment for children aged 14 increases by 2.2 percentage points and their probability to work decreases by 1.2 percentage points. This resembles a 16% reduction in child labor relative to the pre-ban mean. A back of the envelope calculation shows that due to the ban in 2015, 25 thousand teens who were in child labor stopped working, and 50 thousand who would have likely dropped out of school to enter the labor force, did not drop out of school. We also show that due to the ban the effect of the increase in school enrollment and decrease in child labor persists over time, even after reaching legal adulthood.

For policy makers, our study highlights two main important implications. First, that a mere shift in the minimum employment age will not be effective. The ban is only effective when coupled with a set of rules to employ adolescents such as minimum schooling requirements, and concrete penalties for employers to stop hiring children. Second, that child labor bans not only decrease child labor but also increase school enrollment.

Chapter 3 *(joint work with Fernanda Martínez. Data preparation, analysis, and text were equally distributed between co-authors.)* is also motivated by the high number of child labor because children are not only working in hazardous work but this work has long lasting negative effects, especially for children coming from low-income households, on children's development (Beegle *et al.*, 2009) and on their education and health (Emerson and Souza, 2011a). In this chapter, we examine how increasing school instruction time affects school enrollment and child labor.

Most of the studies in the literature have focused so far on interventions based on transfers of resources to reduce child labor such as unconditional, conditional, and in cash or in kind transfer programs (Skoufias *et al.*, 2001; Dammert, 2010; Covarrubias *et al.*, 2012). We contribute to the literature by being the first study to examine the impact of Full-Time Schooling (FTS) program on child labor and school attendance. To identify the effect, we take advantage of the exogenous roll-out of the FTS-program that increases schooling hours from 20 to either 30 or 40 hours in Mexico from 2009 to 2018.

Our findings show that increasing the instruction time does not affect school attendance which alleviates the concern that parents who rely on child labor will take their children out of school because of longer time spent in schooling. When focusing on child labor, the findings show that the increase in the share of FTS at the municipality level leads to a 12%

reduction in child labor, the decrease is driven by children living in extreme poverty. A back of the envelope calculation shows that almost 158 thousand children aged 7-14 stopped working because of the FTS-program. When focusing on the labor force participation of other household members, we find that increasing the instruction time increases the labor force participation of mothers of children aged 7 to 14 years that were affected by the program.

The main policy implication of this chapter is that a shift from a part-time to a full-time school day has the potential to decrease child labor without increasing the risk of children dropping out of school. In addition, a more compatible school day, with the traditional working day also pushes mothers to increase their labor force participation.

Chapter 4 *(single-authored.)* is motivated by the labor force participation rates of women with children. Due to the COVID-19 pandemic, schools were closed for about 158 days in Latin America and therefore women's labor force participation was affected negatively (ILO, 2022a). In the second quarter of 2020, almost 23.6 million women lost their jobs and only 19.3% of the jobs were recovered in 2021. This translates to 4 million women who were not able to join the labor market or return to their work (ILO, 2022a). In this chapter, I estimate the effect of school closures on labor outcomes of women with school-aged children 6 to 14.

The studies in the literature have analyzed so far the impact of child care subsidies on maternal employment and the effect of COVID-19 on labor force participation of women and men (Couch *et al.*, 2021; Yamamura and Tsustsui, 2021; Bundervoet *et al.*, 2022). I contribute to the literature by being the first to focus on the direct impact of school closure on the labor force participation of women with school-aged children. To identify the effect, I take advantage of the exogenous shock, that is school closure in the second quarter of 2020, to assign women with school-aged children 6 to 14 in the treatment group. Those women were directly affected by school closure. I define the control group as women with nursery-aged children 0 to 5 years old that were not directly affected by school closure.

My findings show that school closure decreases labor force participation of women with school-aged children by 1.7 percentage points and increases their domestic work. The increase in domestic work lowers simultaneously the hours spent on work in the market. A back of the envelope calculation shows that almost 750 thousand women with school-aged children stopped working because of the school closure. The results are mainly driven by work done in the informal sector, paid employment, and the services sector. The decrease is observed for all women irrespective of their age and income level. I find no additional decrease in the labor force participation for women that are single or that have access to informal child care.

The main policy conclusion from this chapter is that childcare access such as schooling is important to help women with children to stay in or join the labor force. Second, in developing countries we need systems that are able to cope with sanitary emergencies that allow children to go back to school faster in a safe and orderly manner. Finally, policy makers need to acknowledge the need for more flexible employment arrangements such as flexible working hours and the possibility to work from home, which is rather the exception and not the rule in developing countries.

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

Abstract: This paper investigates the effect of a unique child labor ban regulation on employment and school enrollment. The ban implemented in Mexico in 2015, increased the minimum working age from 14 to 15, introduced restrictions to employ underage individuals, and imposed stricter penalties for the violation of the law. Our identification strategy relies on a DiD approach that exploits the date of birth as a natural cutoff to assign individuals into treatment and control groups. The ban led to a decrease in the probability to work by 1.2 percentage points, resembling a 16% decrease in the probability to work relative to the pre-ban mean, and an increase in the probability of being enrolled in school by 2.2 percentage points for the treatment group. These results are driven by a reduction in employment in paid work, and in the manufacturing and services sectors. The effects are persistent several years after the ban.

*Co-authored with Fernanda Martínez Flores. It contains minor revisions of: Kozhaya M., Martinez Flores, F. (2022): Child Labor Bans, Employment, and School Attendance: Evidence from Changes in the Minimum Working Age. IZA DP No.15144. We thank Thomas K. Bauer, Ronald Bachmann, Christian Bredemeier, Julia Bredtmann, Caio Piza, Kerstin Schneider, Franz Westermaier, and David Zuchowski for their comments. Rachel Kuhn provided excellent research assistance. We also thank the participants of the internal research seminars at the University of Wuppertal, and the participants of the Annual Conference of the European Society for Population for their constructive comments. All remaining errors are our own.

2.1 Introduction

From 2012 to 2016, important reductions in child labor have been made worldwide, resulting in 134 million fewer children engaged in employment. Yet, in developing countries one out of every four children aged 5 to 17 is engaged in activities that are hazardous and risky, affect their development, or do not comply with the international minimum working age standards (UNICEF, 2019).[2] One of the main international initiatives to eradicate child labor are employment bans, through the implementation of a minimum working age and the prohibition to hire underage individuals in certain sectors (ILO, 2017). For instance, the ILO Convention No. 138 introduced in 1973 recommends a minimum age of 15 years to enter the labor force (ILO, 2018) and has been ratified by 131 developing countries.[3]

Despite the high number of countries that have ratified this convention, little is known about the effectiveness of these bans. Although these laws should lead to a decrease in child labor rates, weak enforcement of the law, the lack of punishment if employers do not abide by the law, or even imperfect monitoring, could limit their effectiveness. The few studies analyzing the impact of bans find contradicting results (see e.g., Piza and Souza, 2017; Bharadwaj *et al.*, 2020; Bargain and Boutin, 2021). This paper evaluates the impact of a complex child labor ban, which was introduced in Mexico, on school enrollment and a number of child labor indicators. The main results in this paper present new evidence on child labor bans and offer possible explanations for previous diverging findings.

Mexico presents a unique setting to analyze the impact of child labor bans. In 2014, Mexico introduced a Constitutional Amendment that increased the minimum working age from 14 to 15, without stating further regulation. A year later, in 2015, Mexico introduced

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

[3]79 out of 131 developing countries have banned children younger than 15 years from the labor market. 52 out of the 131 developing countries have set the minimum working age at 14 years.

an ambitious reform to the National Labor Law *"Ley Nacional de Trabajo"*. To date, this reform is one of the most extensive initiatives in Latin America to eradicate child labor. The reform not only shifted the working age from 14 to 15 years, but also coupled this shift with i) restrictions to hire underage individuals i.e., under 18 years of age, who had not completed basic education (primary and lower secondary), ii) regulations to hire individuals above 15 but younger than 18 with respect to the type of activities, sector, hours worked, and working schedule, and iii) much stricter penalties for employers that violate the regulations. This study evaluates the simple shift in 2014 in the minimum working age and the shift in 2015 coupled with additional employment restrictions and penalties.

Several studies show that the main reason for parents to rely on child labor is poverty, which leads parents to give priority to current consumption and thus trade-off between child labor and schooling, i.e., future earnings. The theoretical literature suggests that child labor bans have an ambiguous impact on household welfare. On the one hand, child labor bans could potentially improve welfare by encouraging the accumulation of human capital. On the other hand, in highly unequal economies child labor bans may affect the distribution of income if children of poor families accumulate human capital (Baland and Robinson, 2000). In some cases, these bans could backfire and contribute to the persistence of child labor. If unskilled wages increase as a response to the ban, unskilled workers will have less incentives to educate their children perpetuating the cycle (Doepke and Zilibotti, 2009). In very poor regions such bans may not be effective because children need to work in order to avoid hunger or if the household depends on the child's income (Basu, 1999).[4] Indeed, several studies show that the main reason for parents to rely on child labor is poverty, which leads parents to give priority to current consumption and trade-off between child labor and schooling, i.e., future earnings (Basu and Van, 1998; Baland and Robinson, 2000; Ranjan,

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

2001; Cigno *et al.*, 2002; Jafarey and Lahiri, 2002; Horowitz and Wang, 2004; Edmonds, 2007).

The empirical evidence on child labor bans in developing countries also shows this contradicting pattern.[5] Piza and Souza (2017) evaluate the impact of a shift in the minimum working age from 14 to 16 in Brazil. The study finds that in the short-run, 14 year old boys decrease their labor force participation, mainly in the informal sector, while girls do not respond to the ban. In the long-run, the authors find no impact on earnings and work, but they find that the affected cohort is less likely to have a formal occupation. In contrast, for the same ban, Bargain and Boutin (2021) find in the short-run no overall significant impacts,[6] but suggestive evidence that child labor decreases in areas where labor inspections were high. This finding is reinforced by Edmonds and Shrestha (2012), who find no influence of the minimum working age on child time allocation using micro-data from 59 low-income countries. Finally, Bharadwaj *et al.* (2020) analyze a landmark legislation against child labor in India. In contrast to the previous findings, the study shows that the ban led to an increase in child labor, due to a decrease in child wages relative to adult wages. The ban shifted child labor from banned sectors to other sectors, and shifted work from younger to older siblings.[7]

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

[6]Diverging findings could be explained by differences in the methodology used and in the time span included in the analyses.

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

We contribute to this literature in three different ways. First, our database allows us to identify a rich set of child labor indicators that goes beyond what previous studies have analyzed, i.e., we focus not only on the probability to work, (in-)formal work, (un-)paid work, and school enrollment, but also on weekly hours worked, part-time and full-time work, sector of employment, wages, access to a contract, and employment benefits. This larger range of indicators allows us to draw conclusions about the type of work and sectors that were most impacted by the child labor ban.

Second, this study reconciles previous opposing results on the impact of child labor bans. To do so, we examine the Constitutional Amendment which shifted the working age from 14 to 15 in 2014, but established no concrete penalties or further regulation to the employment of minors. We then compare the results to the more complex package introduced by the reform to the Labor Law in 2015. Specifically, we highlight the role of penalties, regulation of underage work, and requirements to complete basic education to access the labor market for the effectiveness of child labor bans.[8]

Third, we exploit survey data collected on a quarterly basis for several years pre- and post-ban. The database allows us to identify the individuals exact birth date, which we exploit as natural cutoffs to assign individuals into treatment and comparison groups. We identify short-run impacts and long-run impacts by following individuals born in the same cohort using cross-sectional variation. In addition, the data offers a limited panel dimension as individuals are followed for five quarters before dropping from the survey.

We further exploit this limited panel dimension not only to control for unobserved heterogeneity at the individual level when estimating short-run impacts, but also to investigate the impact of the ban for children with different initial work status. In addition, we exploit this panel dimension to provide descriptive evidence on individuals who either respond or

[8]The regulation does not establish penalties for minors working in activities for own-consumption if they are safe and do not interfere with schooling.

do not respond to the ban conditional on working pre-ban. This is an important addition to the literature, as no previous study on child labor bans has accounted for time invariant unobserved characteristics nor have the studies provided results that go beyond an intention-to-treat approach.

Our empirical strategy, exploits the shift in the minimum working age as a natural experiment. We use data from the Mexican Labor Force Survey (ENOE), for the years 2013 till 2017 for the short-run and for the years 2012 to 2019 for the long-run, which is collected on a quarterly basis, and contains rich information on schooling and employment. We implement a DiD design that exploits the date of birth to assign individuals into treatment and control groups.[9] To identify the effect, we focus on the cohort that was directly affected by the ban, and run further analysis with additional cohorts to test the sensitivity of our results.

Our within-birth-cohort approach assigns individuals born in the second half of 2000 to the treatment group. These individuals are 14 years old when the law is enacted, and therefore banned from the labor force. Individuals born in the first half of 2000 are assigned to the control group, as they are 15 years old when the law is enacted. We estimate both short-run and long-run impact of the ban using cross-sectional variation. For the short-run, we observe the affected cohort two years pre- and post-ban. We further narrow the time frame when exploiting the limited panel dimension by restricting the sample to individuals observed at least one quarter before and one quarter after the ban was introduced. This approach solves the concern that children in the affected cohort qualify to work once they turn 15. For the long-run, we extend our time period to observe treatment and control

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

groups shortly after reaching legal adulthood (at age 18). We deviate from Piza and Souza (2016), by estimating a dynamic DiD model. This allows us to rule out the existence of pre-trends, to evaluate when the impact of the reform kicks-in, and to analyze if the effects are persistent once the individuals in the treatment group are eligible to work.

The empirical approach presents two main challenges. First, individuals who are born in the same year, but at different times of the year, may not represent an ideal control group if the timing of birth is correlated with unobserved factors that determine both schooling and employment decisions e.g., through the age at school start (Fredriksson and Öckert, 2014). To address this concern we estimate across-cohort comparisons to compare individuals born in the same month of the year.

Second, our empirical strategy follows individuals born in the same cohort several quarters (years) after the ban is implemented. The definition of treatment and control groups implies that our estimates reflect the impact of being banned from the labor force for a few days up to a maximum of six months for the treatment relative to the control group. Some children banned only for e.g., one month, may not be affected as strongly as children who have to wait 6 months to qualify to work. Therefore, we exploit this and present additional sensitivity analysis where we use a continuous definition of the treatment i.e., the number of months a child is banned from the labor force in contrast to children who are not banned.

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

The media coverage of these reforms could partially explain these results. For the Constitutional Amendment in 2014, the media made a strong emphasis on schooling: *"...kids and teenagers should remain in school, to improve their quality of life, (...) and increase their likelihood of having a better job and higher wages..."* (Senado de la República, 2014). In contrast, the media coverage of the Labor Law reform in 2015, highlighted the restrictions and penalties imposed for potential violations to the law. *"...failure to comply with obligations regarding minors is punishable by imprisonment for one to four years and a fine of 250 (17,525.00 MXN) to 5000 (350,500.00 MXN) times the general daily minimum wage."* (Martínez, 2015).

Our findings are in line with the findings by Piza and Souza (2016) for Brazil. Yet, this is only true when the corresponding regulation is further coupled with stricter penalties and there is general awareness of this penalties, which is in line with, Bargain and Boutin (2021). Finally, in contrast with the framework analyzed for India (Bharadwaj *et al.*, 2020), the regulation in Mexico did not simply shift the demand from 14 years old to slightly older individuals or to other sectors. The regulation made it much costlier for potential employers to hire minors, which limited the substitution between 14 years old and individuals aged 15-17 due to: the number of sectors that restricted employment for all minors, the regulation concerning working hours and benefits, and the prohibition to hire minors who have not completed their basic education.

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

This paper is structured as follows: the following section presents the background and provides additional information on the reform. Sections 2.3 and 2.4 present our empirical strategy and data, Section 2.5 results, and Section 2.6 concludes.

2.2 Background

2.2.1 Child Labor Regulation Pre-2015 and Statistics

In 1917, the Article 123 of the Mexican Constitution set the minimum age for admission to work at 12 years of age. Subsequently, with the constitutional reform in 1962, the minimum working age was set at 14 years, establishing the length of the workday at six hours for individuals aged 14 to 16, prohibiting night shifts and overtime. This regulation remained unchanged for several years. Only after 1989, when the "Convención de los Derechos de Los Niños" was signed, several public policies aimed at recognizing children as subjects of rights and legal protection according to their development and age. A decade later, in 2000, Mexico ratified the ILO Convention 182 concerning the elimination of the worse forms of child labor (ILO, 2020b).

Since that date, the Mexican Ministry of Labor and Social Welfare has increased its efforts to prevent and eradicate the all forms of child labor. During 2007 to 2012, the Ministry of Labor and Social Welfare implemented an inter-institutional strategy to increase the commitment of different sectors to reduce child labor among minors under age 14, increase

[10] 23% of children coming from high income level were working before the ban vs. 77% of the children that come from low and extreme poor income levels (Table A10).

the protection of adolescent workers (above the legal working age), and increase compliance with the national and international legal framework for minors. The main objectives in this strategy include: generating periodic statistical information on child labor indicators, prevent and eradicate child labor in the agricultural sector, promote labor rights and strengthen the legal framework.[11]

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

In recent years, initiatives that directly target child labor have gained importance given that Mexico, similar to other countries in Latin America, has achieved the goal of universal primary coverage and has shown important increases in secondary enrollment rates. From 1990 to 2015, school enrollment increased from 89% to 98% for children aged 6 to 11, from 79% to 93% for children aged 12 to 14, and from 47% to 73% for individuals aged 15 to 17 (INEE, 2018a). As of 2012, upper secondary education became also compulsory (OECD, 2018). However, the compulsory schooling regulation is not based on age but on the school level.[12] Therefore, the opportunities of decreasing child labor through increasing school enrollment are limited.

[11]See (STPS , 2014) for additional information about the historical developments of the child labor regulation in Mexico.

[12]It is important to note that the school calendar year in Mexico begins late August to early July.

From 2007 to 2017, Mexico witnessed a decrease in dangerous employment from 6.9% to 3.6% for children aged 5 to 14 years old and from 26.6% to 18.2% for children aged 15 to 17 years old (INEGI, 2018a). From 2015 to 2019, the child labor rate decreased from 9.8% to 7.1%. Although the reduction is considerable, 2 million children continue to be engaged in work.[13] For children aged 5 to 15, who are banned from the labor force, the child labor rate decreased from 6.9% to 4.1% from 2007 to 2019.

2.2.2 Constitutional Amendment and Labor Law Reform

The legal framework in 2012 – before the changes analyzed in this paper –, thus, banned all children under the age of 14 from the labor market (excluding work for family members). For individuals below the age of 16, the regulation prohibited night-work, dangerous work (without listing specific prohibited activities), established a maximum of 6 daily working hours, prohibited overtime, work during festive days and weekends, and established a minimum of 16 days of holidays. Penalties for employers who violate this framework were established at 3 to 155 times the minimum wage.

Before 2015, Mexico was one of the last countries in Latin America that had not ratified the ILO Convention No. 138. The convention establishes a "Minimum Age for Admission to Employment" requiring countries to set the minimum age at 15 for entry into the labor force which is in accordance with the age at which a child leaves compulsory schooling, and to create national policies to eradicate child labor.[14] In order to ratify the convention, two main steps were implemented: First, Mexico amended the Article 123 of its Federal Constitution on **June 17, 2014** shifting the minimum working age from 14 to 15. In this phase, no

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

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

additional regulation was introduced except for the change in the minimum working age (DOF, 2014).

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

– The Labor Law prohibits all children younger than 15 to work.

– Individuals who are under age 18 and did not complete compulsory schooling are prohibited from working, unless approved by the corresponding labor authority.

– All activities which are dangerous, risky, or morally hazardous of individuals under age 18 – for family members – are also prohibited. Activities with the purpose of own-consumption are excluded.

– Individuals under age 18 shall receive an annual paid vacation period of at least eighteen working days.

– It is forbidden to rely on the work of individuals under the age 18 for night-workt, extra-hours, work on Sundays or on official holidays.

– All types of work that are hazardous, risky, or morally damaging are prohibited for individuals who are under age 18. This includes activities with exposure to: noise, dangerous substances, heights, narrow spaces, heavy equipment, extreme conditions, vehicular transit; as well as industries such as: agriculture, mining, construction, care, petroleum and nuclear energy.[15]

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

Most importantly, the reform to the the Labor Law in 2015 establishes concrete penalties for employers hiring individuals under 15. If the labor authorities identify violations to

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

this regulation, the work of the underage individual shall be immediately terminated and the employers shall be punished with a prison term of 1 to 4 years and/or a fine of 250 to 5000 times the minimum wage (DOF, 2015). The same penalty can be applied for parents (mothers, fathers, or guardians) that allow the employment of children in work that affects their physical, mental, or emotional development, i.e., hazardous work.

Therefore, after the reform to the Labor Law, the Secretary of Labor and Social Security (STPS) increased child labor inspections mainly in industries. For the period between June 1, 2015 and June 20, 2017, the General Directorate of the Federal Labor Inspectorate (DGIFT) conducted 245,019 inspections that covered 9,982,393 workers (ILO, 2019a).[16] In 7,748 of the cases children under the age of 15 were engaged in child labor and were immediately detached from the working environment.

The limited efficiency of inspections in Mexico's setting should also be acknowledged, even after the increase in efforts after 2015. The STPS is more likely to carry out an inspection after a formal complaint i.e., inspections are not a routine procedure that occur on a regular basis. In addition, the STPS does not have an internal system to track child labor violations, and state-level efforts to increase child labor inspections are not well-documented for all states (see e.g., U.S. Department of Labor, 2020).[17]

2.3 Identification Strategy

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

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

[17] Unfortunately, we were not able to obtain high-quality data on inspections. We provide the results focusing on areas where inspections are more likely to occur e.g., areas with different urbanization levels.

them to treatment and control groups according to their month of birth. To do so, we focus on the cohort of children who were born in the year 2000.

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

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

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

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

$$Y_{imt} = \alpha_0 + \beta(Treated_i \times Post{-}ban_t) + \theta'\mathbf{X}_i + \mu'\mathbf{P}_i + \delta_m + \gamma_s + \alpha_t + \mathbf{t}\lambda_s + \epsilon_{imt} \quad (2.1)$$

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

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

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

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

We also show that the results are robust to other clustering levels that follow a more liberal approach such as the birthday-survey-year level or a more conservative approach such as state-by-month-of-birth or birth-month level.

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

The main identifying assumptions of our DiD design is that in the absence of the child labor ban, both groups would have followed the same trajectory. Thus, the main threat to our identification strategy is that the change in the law could shift the labor demand for 15 year old individuals to replace the labor of 14 year old individuals. To show that this is not the case, we provide graphical evidence on the parallel trends and estimates on employment rates (see Figure 2.1 and Table 2.9).

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

$$Y_{it} = \alpha_1 + \beta_1(Treated_i \times Post{-}ban_t) + \eta'\mathbf{Z}_{it} + \rho_i + \alpha_t + \mathbf{t}\kappa_s + \upsilon_{it} \tag{2.2}$$

where Y_{it}, denotes either child labor or school enrollment for child i, at survey time t. $Treated_i$ is a dummy variable that takes the value one for individuals born between June 13th and December 31st, 2000. $Post{-}ban_t$ is a dummy variable that takes the value one after June 2015, when the ban was introduced. Z_{imt} is a vector of children time varying characteristics such as age and aged squared. ρ_i captures individual fixed effects, α_t, quarter-by-year fixed effects, $\mathbf{t}\kappa_s$ captures state linear time trends and υ_{it} the error term. This specification allows us to estimate the within individual impact of the ban and account for unobserved time invariant characteristics at the individual level. We refrain from using this as the baseline because i) individuals are only followed for five quarters and there is attrition in the sample, which decreases considerably the number of observations; and ii) to facilitate the comparison between the short-run and long-run results.

Second, following Eq. 2.1 we implement an across-cohort comparison and use the cohort born in 1999 as a control group to estimate the effect of the ban. For this, we construct two definitions for the treatment and control groups: i) the treatment group are individuals born in 2000 and the control group individuals born in 1999; ii) the treatment are individuals born in the second half of 2000 compared to the control who are defined as individuals born in the second half of 1999. In addition, we provide a number of placebo tests focusing on non-affected cohorts to show that our estimates are not driven by age differences. Table A1 in the Appendix summarizes the relevant dates and definitions for the treatment and control groups for the within-cohort and across-cohort comparisons.

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

2.4 Data and Descriptive Statistics

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

The ENOE provides rich information on schooling and employment[18], as well as demographic characteristics of the child, the parents, and the place of residence. We further complement this database using the marginalization level data obtained from the *Consejo Nacional de Población* (CONAPO) for the year 2010 at the municipality level. The marginalization index is a multidimensional poverty measure which takes into account education, dwelling characteristics, population geographical distribution, and income level (CONAPO, 2019).

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

[18]That is employment status, whether active in the labor market, earnings, and number of hours worked. Here it is worth mentioning that school enrollment variable that is reported does not take into account school attendance and school attainment. As of 2018 the survey does not report employment information for individuals younger than 14, because of the change in the minimum working age. This does not affect our estimates because we focus on the sample of 14 years and older starting the year 2013 of the ENOE data.

Additionally, we use data for the cohorts born in 1997, 1998, and 1999 for the across-cohort comparison and placebo tests.

Table 2.1 provides descriptive statistics for the treatment and control group before the ban was implemented. The final column provides the t-test indicating if the difference in means between groups is significant. The table shows that 95% of children are enrolled in school, 8% are engaged in child labor, and conditional on working, they work 21 hours per week. Almost all working children are in the informal sector and only 45% receive compensation for their work. 30% of working children work in the primary (agriculture) sector, 18% in the secondary sector (manufacturing), and 52% in the tertiary sector (services). The majority of them do not have either a written contract nor access to benefits. 15% of them work full-time and on average they earn 8 Pesos per hour, which equals the Mexican hourly minimum wage in 2015. The children in our sample live on average in households with 5 members, and 42% of them are the first-born. 80% of children live with both parents. With respect to parental education, 68% (72.2%) of children have mothers (fathers) who have at most secondary education or higher.[19] Finally, 53% of them live in localities that are highly urbanized areas i.e., localities with more than 100,000 inhabitants

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

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

Table 2.1: Pre-Ban Descriptive Statistics

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

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

2.4.1 Descriptive Analysis

We start by providing graphical evidence on the evolution of school enrollment and employment rates for the treatment and control groups. Figure 2.1 shows that before the ban,

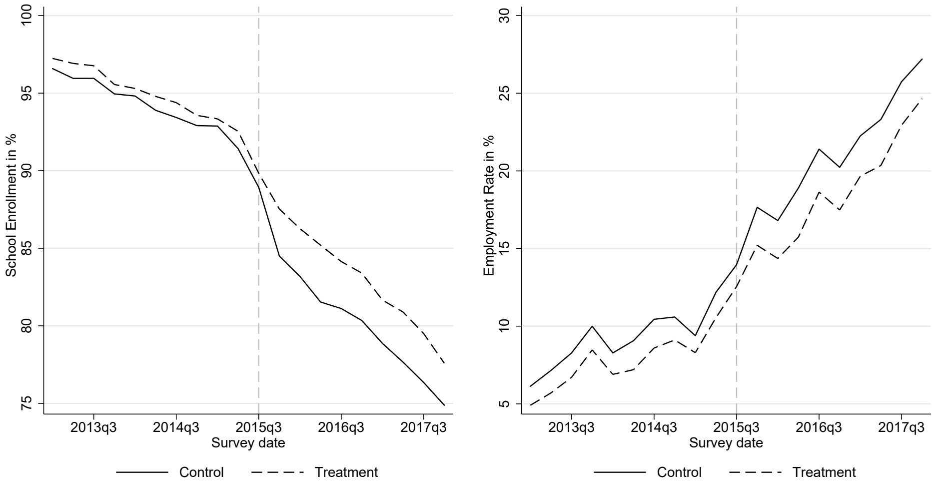

Figure 2.1: Parallel Trends by Treatment and Control Group
Source: ENOE, authors' analysis.
Notes: – The figure illustrates the shares which are calculated predicting both school attendance and the probability to work controlling for the full set of observable characteristics.All children are born in the year 2000. Children in the control group are born between January 1 and June 12. Children in the treatment group are born between June 13 and December 31. Figure 1 shows that before the ban, schooling and employment followed a similar trend with a minor level difference between groups. Schooling is decreasing after the second quarter of 2015 for the control group because lower secondary education is completed at age 15.19. For the treatment group, school enrollment also decreases, but not as steep as in the control group. The size of the gap between both groups increases considerably after 2015. For Employment rate we observe a similar pattern, i.e. a small level difference and a gap that opens up after the third quarter of 2015.

schooling and employment followed a similar trend with a minor level difference between groups. Focusing on schooling, the figure shows a sharp drop in school enrollment after the second quarter of 2015 for the control group. This drop is not surprising because usually lower secondary is completed at age 15.[20] For the treatment group, we also observe a drop in school enrollment, but not as steep as in the control group. The size of the gap between

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

both groups increases considerably after 2015. By the end of 2017, the school enrollment rate of the treatment group remains higher than that of the control group. Focusing on employment rates, we observe a similar pattern, i.e., a common trend before the ban and a gap that opens up after 2015. The empirical analyses in the long-run further allows us to rule out the existence of diverging pre-trends.

2.5 Results

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

2.5.1 Baseline Results

We estimate the effect of the child labor ban on the probability of being enrolled in school and on the probability of being employed and report the results in Table 2.2 following our specification in Eq.(2.1). Columns I and II report the results focusing on school enrollment and columns III and IV focusing on employment. For each outcome variable, we provide a specification controlling for the full set of control variables, but excluding month of birth fixed effects (column I and III). In addition, we provide a specification including month of birth fixed effects (column II and IV), which should capture the age difference between treatment and control groups.

Table 2.2: Effect of Labor Law Reform in 2015 on School Enrollment and Employment

	School enrollment	School enrollment	Employed	Employed
	I	II	III	IV
Treated x Post-ban	0.027***	0.022***	−0.025***	−0.012**
	(0.005)	(0.004)	(0.006)	(0.005)
Male	−0.024***	−0.024***	0.102***	0.102***
	(0.004)	(0.004)	(0.006)	(0.006)
HH size	−0.016***	−0.016***	0.009***	0.009***
	(0.002)	(0.002)	(0.001)	(0.001)
Birth rank: Ref.: First-born				
Middle-born	0.007**	0.008**	0.002	0.001
	(0.003)	(0.003)	(0.003)	(0.003)
Last-born	0.005**	0.005**	−0.018***	−0.018***
	(0.002)	(0.002)	(0.002)	(0.002)
Both parents present	−0.054***	−0.052***	0.036***	0.035***
	(0.014)	(0.014)	(0.011)	(0.011)
Mother's education level: Ref.: None				
Primary education	0.084***	0.084***	−0.053***	−0.052***
	(0.014)	(0.013)	(0.006)	(0.006)
Secondary education	0.148***	0.148***	−0.080***	−0.080***
	(0.015)	(0.015)	(0.007)	(0.007)
High-school	0.190***	0.189***	−0.101***	−0.101***
	(0.017)	(0.017)	(0.009)	(0.009)
Vocational training	0.187***	0.187***	−0.111***	−0.111***
	(0.018)	(0.018)	(0.010)	(0.010)
University degree	0.198***	0.198***	−0.132***	−0.132***
	(0.018)	(0.018)	(0.010)	(0.010)
Father's education level: Ref.: None/Father not present				
Primary education	0.052***	0.051***	−0.031***	−0.030***
	(0.015)	(0.015)	(0.011)	(0.011)
Secondary education	0.101***	0.100***	−0.066***	−0.065***
	(0.014)	(0.014)	(0.012)	(0.012)
High-school	0.123***	0.122***	−0.082***	−0.081***
	(0.015)	(0.015)	(0.012)	(0.012)
Vocational training	0.123***	0.122***	−0.086***	−0.085***
	(0.015)	(0.014)	(0.013)	(0.013)
University degree	0.115***	0.114***	−0.091***	−0.091***
	(0.014)	(0.014)	(0.012)	(0.012)
Locality size: Ref.: > 100,000 inhabitants				
15,000-99,999 inhabitants	−0.005	−0.005	0.023***	0.023***
	(0.004)	(0.004)	(0.003)	(0.003)
2,500-14,999 inhabitants	−0.005	−0.005	0.029***	0.029***
	(0.004)	(0.004)	(0.004)	(0.004)
Less than 2,500 inhabitants	−0.021***	−0.021***	0.060***	0.061***
	(0.005)	(0.005)	(0.004)	(0.004)
Constant	0.895***	0.915***	−0.003	0.012
	(0.016)	(0.015)	(0.009)	(0.010)
State FE	yes	yes	yes	yes
Quarter-by-year FE	yes	yes	yes	yes
Month of birth FE	no	yes	no	yes
State-specific trend	yes	yes	yes	yes
Observations	123,487	123,487	123,487	123,487
R^2	0.131	0.135	0.109	0.112

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

The estimated coefficients show that the ban led to an increase in school enrollment by 2.2 percentage points for children in the treatment group relative to children in the control group (column I). The coefficient remains stable after taking into account month of birth fixed effects (column II). By partialling out the month of birth fixed effects (column III), the results further indicate that the ban lead to a decrease in the probability of being employed by 1.8 percentage points, but this coefficient drops slightly, to 1.2 percentage points, after taking into account the month of birth fixed effects (column IV).[21]

Relative to the pre-ban mean, these coefficients translate to an increase in school enrollment by 2% and a decrease in child labor rates by 16%. This indicates that the enforcement of the law and penalties were still relatively weak after the 2015 ban; however, our estimates support that legislated bans coupled with regulations and penalties lead to a reduction in child labor compared to other studies in developing countries. Our results contradict Edmonds and Shrestha (2012) who find no effect of the minimum working age in 59 low-income countries. Unlike Bharadwaj *et al.* (2020), we find that the probability of work decreases (not increases) after the ban. The results, however, are in line with the findings in Piza and Souza (2016) and are similar in magnitude as in Bargain and Boutin (2021), except that the latter do not find significant effects.

Table A3 in the Appendix further shows the results focusing on total weekly hours worked and hours worked conditional on employment. We observe a decrease in the number of weekly hours worked by about 0.75 hours (45 minutes). The estimated coefficient for conditional hours worked is negative but not statistically significant. The latter suggests that the reduction in hours worked is mainly driven by the extensive margin rather than the intensive margin. Table A5 in the Appendix further shows that the results are robust to alternative ways of clustering the standard errors.

[21]Table A6 in the Appendix provides the baseline results showing them without the state-specific time trend.

Table 2.3: Effect of Ban in 2015 on School Enrollment and Employment: Individual fixed effects Approach

Dependent variable:	**School enrollment** I	**Hours worked** II	**Extensive margin** III	**Intensive margin** IV
A. Full-sample				
Treated x Post-ban	0.028***	−0.733***	−0.020**	−0.486
	(0.005)	(0.267)	(0.009)	(1.519)
Observations	23,562	23,562	23,562	3,035
B. Children working pre-ban				
Treated x Post-ban	0.047***	−1.713	−0.075**	−0.471
	(0.016)	(1.110)	(0.036)	(1.534)
Observations	4,155	4,155	4,155	2,121
C. Children not working pre-ban				
Treated x Post-ban	0.022***	−0.721***	−0.017**	–
	(0.005)	(0.213)	(0.007)	–
Observations	19,407	19,407	19,407	–

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

Next, we estimate the impact of the ban exploiting within individual variation. In this case, we restrict the sample to individuals who are observed at least once before and after ban. As individuals are only observed for a maximum of five quarters, this limits the time frame to shortly before and after the ban. The main advantage of this strategy is that we are able to control for all unobserved heterogeneity at the individual level. In addition, this allows us to identify the effect of children with different work status pre-ban.

The results presented in Table 2.3 show similar findings as in the baseline results. Panel A, shows the results for the full sample of individuals. We find an increase in the probability of being enrolled in school as well as a decrease in the probability of being employed.[22] Next, panels B and C show the results for children who were working and not working before the ban, respectively. The results are consistent with panel A, but the coefficients are much larger for those children who were working before the ban was introduced. Taken together, the results suggest that the ban is effective in decreasing child labor in two different ways: the ban decreased the proportion of working children and simultaneously decreased

[22]We further estimate the baseline results, but restricting the sample to the year 2015, i.e, shortly before and after the ban. The results in Table A4 in the Appendix show a similar pattern.

the proportion of children who would have otherwise (in the absence of the ban) entered employment.

The limited panel dimension of the data allows us to descriptively investigate differences between children who complied vs. children who did not comply with the ban. Table A7 in the Appendix shows the pre-ban descriptive characteristics. Children who complied with the ban are more likely to be enrolled in school, to work fewer hours, and are less likely to work in paid-employment than non-compliers. Compliers are less likely to work in the agricultural sector and more likely to work in the services sector. They come from households with fewer members, have slightly more educated parents, and are more likely to reside in larger localities i.e., more urbanized areas.

Finally, in Figure 2.A2, we show further descriptive statistics for household income and secondary completion rates. We focus on three groups of individuals in the treatment group i) individuals who were not employed pre-ban, ii) individuals who were employed pre-ban and complied with the ban, and iii) individuals who were employed pre-ban and did not comply with the regulation. The figure shows, that the share of individuals who complete secondary level, steadily increases for the first two groups. About 40 percent of individuals who were not employed pre-ban or who complied with the ban report having completed secondary school at age 16. The share of secondary school completion is much lower for individuals who did not comply with the ban. While this could be fully attributed to income differences, the graph below shows household income per capita for these three groups. Household income per capita for children who work pre-ban is very similar. After the ban, income per capita increases slightly for the group of non-compliers and decreases for the group of compliers. The graph shows that removing a child from employment could have an impact on household income in the short-run. This decrease however, may be compensated in the future due to higher completion schooling completion rates for the group of compliers.

2.5.2 Constitutional Amendment vs. Labor Law Reform

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

Table 2.4: Effect of the Child Labor Ban: Placebo, Constitution Amendment, and Labor Law Reform

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

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

Table 2.4 reports the results focusing on school enrollment (panel A) and employment (panel B). The results of the placebo ban reported in column I are not statistically significant for schooling nor for employment, reducing the concern that our findings are driven by underlying group-specific trends.

Turning to the results of the Constitutional Amendment in 2014 reported in column II, we observe a statistically significant increase in the probability to be enrolled in school; however this coefficient represents a small increase in schooling in comparison to the pre-ban mean. We further observe no impact on the probability of being employed. The estimated coefficient is close to zero and not statistically significant. In contrast, the estimated coefficients for the reform to the Labor Law in 2015 in column III, are larger in magnitude and are both statistically significant.[23]

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

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

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

2.5.3 Long-Run Results

Next, we estimate if the labor force reform in 2015 had persistent effects over time. We extend the time frame for the analysis from 2012 to 2019 and follow the cohort born in 2000 until they reach adulthood. The empirical strategy follows the same logic as in Eq. (2.1), but we estimate the effect by survey-year to observe i) differences between treatment and control groups in the pre-treatment period, ii) differences in the period after the reform, and iii) if these differences are persistent over time.

For this analysis, we focus on the same outcome variables: school enrollment and the probability of being employed.[24] However, as working may not be a disadvantage as the cohort gets older and is permitted to work after individuals turn 15; therefore, we investigate the impact on other employment variables that may hinder education i.e., being employed full-time or being employed conditional on not being in education.

Figure 2.2 reports the point estimates and the confidence interval at the 95% level by survey-year. The reference year is 2012. All graphs show no significant differences between treatment and control group in the pre-treatment period. For the post-treatment period, we observe a significant increase in school enrollment and a decrease in employment mainly driven by: a decrease in the probability of working full-time and/or in the probability of working and being out of school (lower panel). Similar to the findings for Brazil (Piza and Souza, 2016), these effects seem to last until at least the age of 18, once the individual reaches adulthood. After 2018, individuals in the treatment group reach adulthood and all previous restrictions to enter the labor force do not apply anymore. In this year, there is a spike in school enrollment and employment for the treatment group, which is most likely driven by the slight age difference between groups. The control group reaches adulthood sooner than the treatment group, which leads to a decrease in their school enrollment and

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

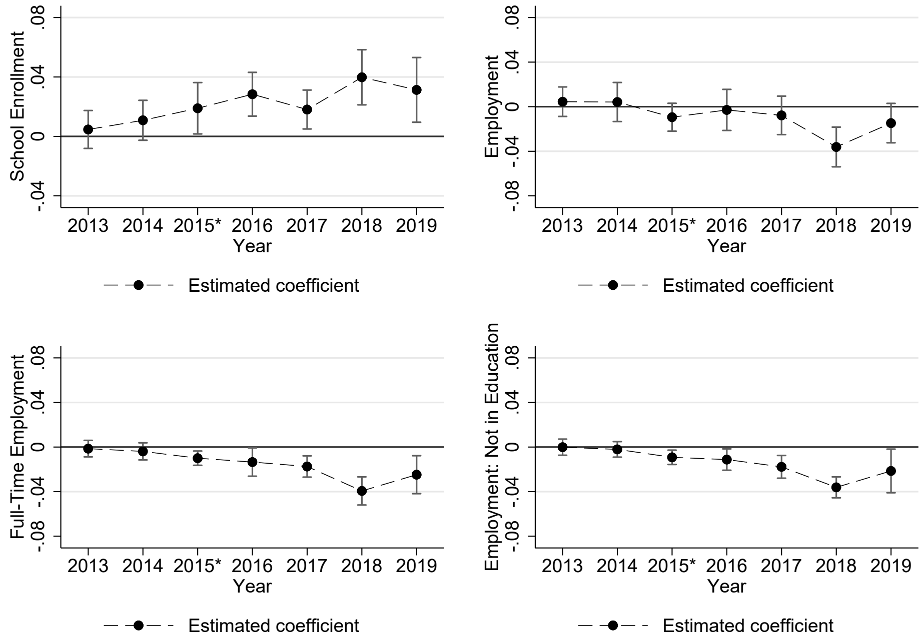

Figure 2.2: Impact of Ban by Year - Long Run
Source: ENOE , authors' analysis.
Notes: – The results are obtained from linear regression models including the full set of controls, fixed effects, and a state-specific linear time trend. Confidence intervals are reported at the 95% level and the standard errors are calculated at the month of birth-survey year level. The results focus on the cohorts born in 2000. The treatment takes the value 1 if the individuals were born after June 13th. The ban was officially enacted on the third quarter of 2015.

an increase in their labor force participation relative to the treatment group. In 2019 when both groups have reached adulthood this spike disappears; however, we continue to observe significant differences for the treatment group, but the differences are smaller.

Finally, we estimate the results of a placebo ban in 2013, for the unaffected cohort born in 1998 and report the results in Figure 2.3. The results of the placebo ban show no statistically significant differences in the probability of being enrolled in school or working between treatment and control groups for the pre- and post-treatment periods, nor for the probability of working full-time or working and being out of school. These results further

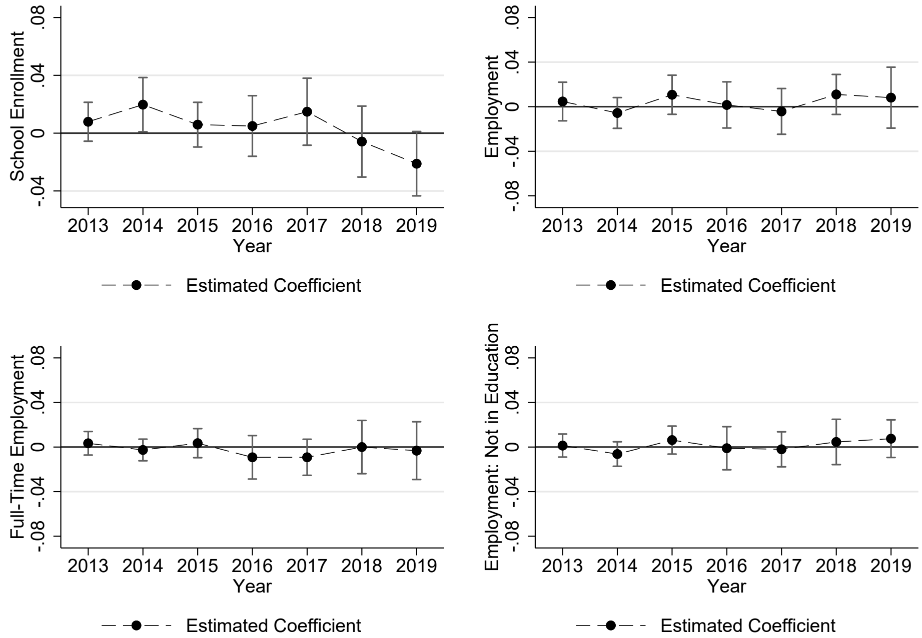

Figure 2.3: Impact of Ban by Year - Long Run: Placebo Ban in 2013

Source: ENOE , authors' analysis.

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

support the findings in Figure 2.2 showing that the effect of the ban on school enrollment and employment is a causal estimate and not mere correlations.

2.5.4 Heterogeneous Effects

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

We start by analyzing differential impacts by gender and present the results for the interaction term in Table 2.5. Looking at the impact on school enrollment, the table shows

Table 2.5: Effect of Ban in 2015 by Gender

Dependent variable:	**School enrollment** I	**Hours worked** II	**Extensive margin** III	**Intensive margin** IV
Effect of ban	0.036***	−1.865***	−0.036***	−1.938***
	(0.006)	(0.251)	(0.007)	(0.725)
Male	−0.017***	2.630***	0.090***	3.283***
	(0.004)	(0.294)	(0.007)	(0.432)
Male x Effect of ban	−0.027***	2.167***	0.047***	1.478**
	(0.008)	(0.416)	(0.009)	(0.658)
Observations	123,487	123,487	123,487	15,911
R^2	0.136	0.115	0.113	0.182

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

that after the ban girls increase their school enrollment by 3.6 percentage points. For boys, the effect is smaller at 0.9 percentage points. Turning to the child labor results, we find larger impacts for girls. Unlike Piza and Souza (2016), our results indicate that girls decrease their labor force participation to a larger extent than boys because girls are more likely to work in the secondary and tertiary sectors.[25] Column II shows that although boys tend to work more hours, girls are the ones who respond more strongly to the ban. Girls decrease total hours worked by almost 1.9 hours. The extensive margin (column III) and intensive margin (column IV) show a similar pattern.

Although these results may seem surprising, we provide additional descriptive statistics in Table A9 by gender. The table shows that indeed fewer girls work in comparison to boys, and on average, girls work less hours per week than boys. However, the largest differences are found in the sector of work: the majority of girls work in the tertiary sector (74% in

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

contrast to 50% of boys), followed by the secondary sector (16% in contrast to 18% for boys), and the primary sector (10% in contrast to 39% for boys).[26]

To further examine the heterogeneous effect for the type of employment, we test how the ban affects formal vs. informal work, paid vs. unpaid work, and sector of employment and report the results in Table 2.6.[27] The results show a decrease in the probability of being employed in the formal and informal sectors (column I and II). Yet, when examining paid and unpaid work (columns III and IV), we observe that the ban had a stronger negative impact on paid activities. The coefficient for unpaid work is close to zero and not statistically significant. The impact of the ban by sector of employment (columns V-VII) shows no significant impact on agricultural work (primary), but a reduction on employment in manufactures (secondary) and services (tertiary).

Table 2.6: Effect of Ban in 2015: Formal, Paid Employment, and Sector

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

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

This is consistent with the idea that subsistence work (which usually takes the form of unpaid agricultural activities) will remain unaltered because the family depends on it to cover their basic needs (see e.g., Basu *et al.*, 2010). These results also explain the larger decrease in employment for girls, who tend to work in the secondary and tertiary sectors,

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

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

while boys concentrate in the primary sector. The decrease in paid work as well as work in the secondary and tertiary sectors can also be explained by a higher probability of the employer being subject to penalties.

Most of the penalties e.g., through inspections, take place in urban areas for the services and manufacture industries. For potential employers it is costlier to hire underage individuals because of the new set of regulations to hire individuals under the age of 18, and in case of violations they are more likely to be subject to a penalty. Although these restrictions could be overseen for employers in the informal sector, if child labor is visible[28] they could also be subject to penalties.

Table 2.7: Effect of Ban in 2015 on Child Labor: Conditional on being Employed

Dependent variable:	**Full-time** I	**Contract** II	**Benefits** III	**Ln(wage)**^{a} IV
Treated x Post-ban	-0.043^{***}	-0.014^{***}	-0.008^{*}	0.048
	(0.016)	(0.004)	(0.004)	(0.033)
Mean	.293	.0345	.028	2.863
Observations	15,832	15,832	15,832	8,928
R^2	0.151	0.076	0.078	0.145

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

Next, we examine specifically what happens for those children who continue to work i.e., at the sample conditional on being employed. Table 2.7 shows the results focusing on full-time work, access to a written contract, to social security benefits, and wages. After the ban, children in the treatment group are less likely to work full-time which is consistent with the new regulations established by the reform to the law. However, we also observe that those children who continue to work are less likely to have a written contract or access to benefits. Previous studies have found that enforcement of labor regulation can push workers

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

to informality (Almeida and Carneiro, 2012). While we find an overall decrease in informal and formal employment, we observe that conditional on being employed, the ban decreases the work of children aged 14 leading the treatment group to decrease the probability of having a contract or access to benefits. We find no significant impact on wages.

We also explore if the results are heterogeneous using different definitions to proxy the poverty level of the household. The results are reported in Figure 2.4, which show the point estimates and confidence intervals of the effect of the ban interacted with the respective income (regional) classification. The figure reports marginal effects.

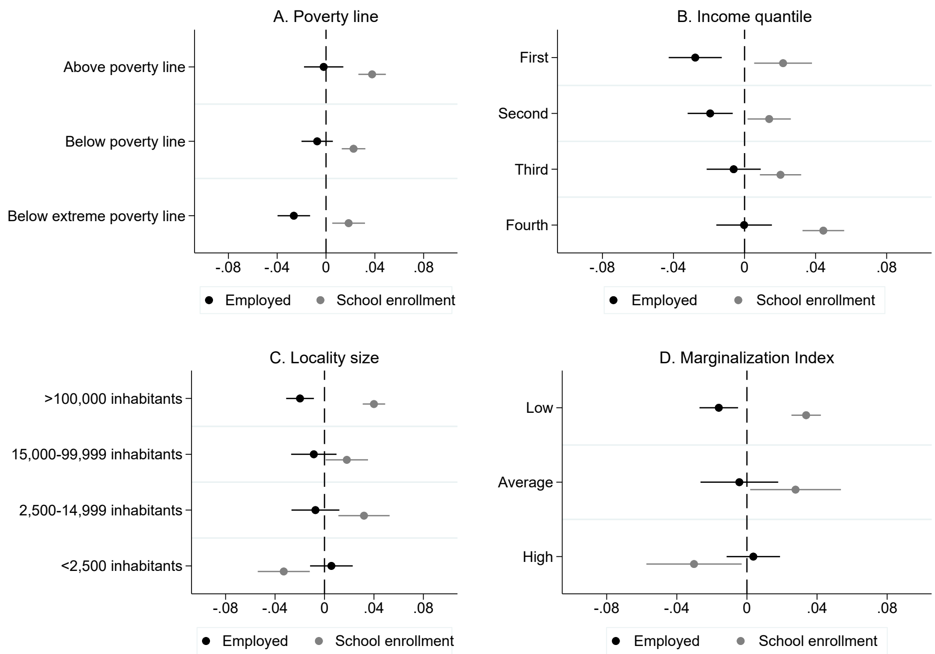

Figure 2.4: Heterogeneous Impacts of the Child Labor Ban
Source: ENOE , authors' analysis.
Notes: – Each panel shows for the years 2013 till 2017 the marginal effects of interacting the "Treated x Post-ban" indicator with the respective categorical variable i.e., poverty level, marginalization index, locality size, and income quantile. The results are calculated using as the dependent variable a binary variable indicating if the child i) is employed and ii) is enrolled in school. The regressions include the full set of control variables, time fixed effects, and a state-specific time trend.

Panel A shows the results of an interaction between the effect of the ban and a poverty indicator. This poverty variable indicates if the household lives in i) extreme poverty, ii) moderate poverty, or iii) above the poverty line.[29] Second, in Panel B, we show the results interacting the effect of the ban with the household income per person in quantiles.

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

Accordingly, Figure 2.4 shows that the probability of being employed decreases for children who live in poor households (panels A and B). The effect is concentrated for children living below the extreme poverty line and for the lowest income quantiles. The results on school enrollment are positive and significant for all poverty categories and income quantiles. In contrast, the decrease in employment mainly happens in areas with a low marginalization level, which are mostly urban areas (panels C and D). The increase in the probability to attend school, is also driven by children living in these areas.

Taken together, the results in Figure 2.4 suggest that children who are poor, but who live in urban areas are the ones that respond more to the ban. If child labor would only be present in rural areas or only in very low income families, then the results in this section may not represent a large reduction in employment. However, the descriptive statistics in Table A10 show that 34% of children who work aged 14-17 live in urban areas, and that 77% of children who work live in households that are extremely poor, or poor, and 23% of the working children live in households above the poverty line.

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

Table 2.8: Effect of the Labor Law Reform in 2015 on School Enrollment and Labor Outcomes: Continuous Treatment

	School enrollment I	Employment II	Extensive margin III	Intensive margin IV
Banned 1 month x Post-ban	0.018***	−0.021	0.005	0.351
	(0.004)	(0.213)	(0.006)	(0.687)
Banned 2 months x Post-ban	0.003	−0.544***	−0.012**	−0.638
	(0.006)	(0.174)	(0.006)	(0.762)
Banned 3 months x Post-ban	0.025***	−0.779***	−0.016**	−0.642
	(0.006)	(0.268)	(0.006)	(0.970)
Banned 4 months x Post-ban	0.019**	−0.713***	−0.013**	−0.048
	(0.008)	(0.205)	(0.006)	(0.934)
Banned 5 months x Post-ban	0.021***	−1.160***	−0.018***	−1.127
	(0.006)	(0.196)	(0.005)	(1.000)
Banned 6 months x Post-ban	0.029***	−1.563***	−0.028***	−1.453
	(0.006)	(0.182)	(0.006)	(1.113)
Observations	123,487	123,487	123,487	15,911
R^2	0.136	0.114	0.112	0.182

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

Finally, Table 2.8 shows the results focusing on a categorical definition of the treatment (instead of a binary cutoff). In this specification, we define the treatment based on the number of months the child has to wait in order to qualify to work and estimate the results. We observe consistent results as in the baseline. However, the results are stronger for children who are further away from the cutoff meaning that if children have to wait for longer months in order to qualify to work, they will be more likely to attend school and less likely to work. For children who are very close to the cutoff, i.e., who only have to wait one month in order to qualify, the impacts on employment vanish.

2.5.5 Robustness Checks

A potential concern is that employment is simply shifted from younger siblings to older siblings. We show this empirically, by estimating the impact of the reform on individuals who have a younger sibling affected by the reform. The same logic applies as in Eq.(2.1); however, we define the treatment as individuals aged 15 to 17 years old who have a younger sibling aged 7 to 14 years old and, thus, banned from the labor force. For the comparison group, we focus on individuals aged 15 to 17 years old that have no younger siblings aged 7 to 14. Table 2.9 shows no significant effects on the ban on the labor force participation of individuals who have a younger sibling affected by the ban.

Table 2.9: Effect of Ban in 2015 for Older Siblings

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

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

Next, we address the main concern that our estimates could be partially driven by the age difference between our treatment and control group. In Table 2.10 we test the sensitivity of our results implementing across-cohort comparisons. We provide the results focusing on a

Table 2.10: Effect of the Child Labor Ban: Placebo, Constitution Amendment, and Labor Law Reform - Two Cohort Definition

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

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

placebo ban in 2013 (column I), on the Constitutional Amendment in 2014 (column II), and on the shift to the Labor Law in 2015 (column III).

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

In Table A8 in the Appendix we further refine the definition of the treatment and control group of the across-cohort comparison, by restricting the sample to individuals who are born in the second half of the year e.g., for the baseline estimates we define the treatment group as individuals born between June and December of 2000, and the control group as individuals born between June and December of 1999. Similarly, for the Constitutional Amendment and placebo reform we focus on the cohorts 1997-1998 and 1998-1999, respectively. The estimates remain robust. We observe an increase in schooling and a decrease in employment when focusing on the Labor Law reform in 2015.

2.6 Conclusion

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

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

Using data from the Mexican Labor Force Survey (ENOE), we implement a DiD approach that exploits the date of birth as a natural cutoff to assign individuals into treatment and control groups. Unlike child labor ban studies in India and Brazil (Bharadwaj *et al.*, 2020;

Bargain and Boutin, 2021), we find that the ban indeed led to a decrease in child labor. However, this decrease is only observed after the reform of the Labor Law in 2015 included a more complex package of regulations to eradicate child labor.

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

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

The results in this study are of particular relevance given the current initiative to decrease the minimum working age in agriculture in Mexico from 18 to 16, which is currently being discussed in the Senate (Cantú, 2022). Agriculture is classified as a hazardous activity given that it often involves heavy physical work, the use of heavy equipment, and handling of toxic substances. If the shift is approved, policymakers should guarantee that young people in rural areas have appropriate working conditions and the corresponding opportunities to remain in school.

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

Appendix Chapter 2

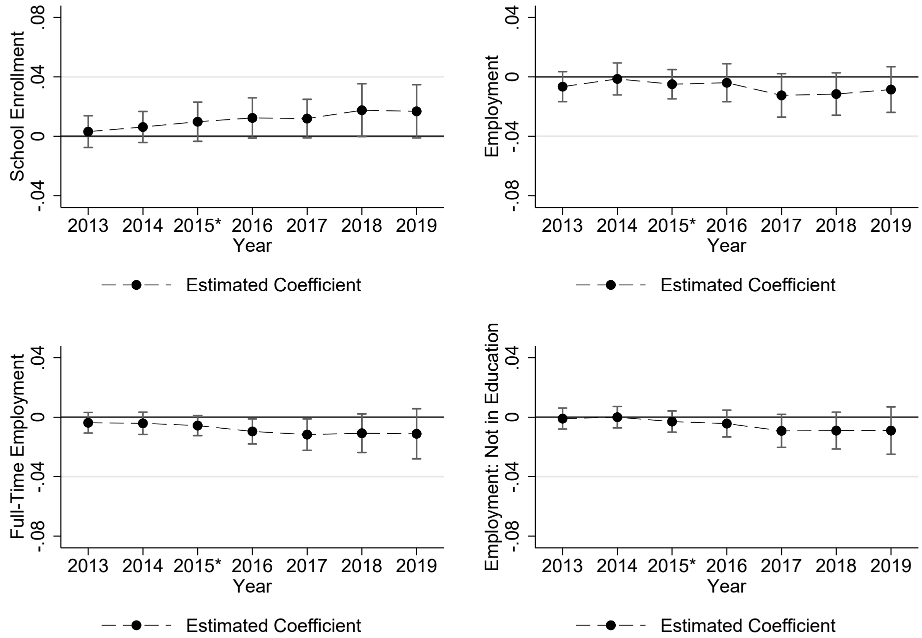

Figure 2.A1: Impact of Ban by Year: Two-Cohort Definition

Source: ENOE , authors' analysis.

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

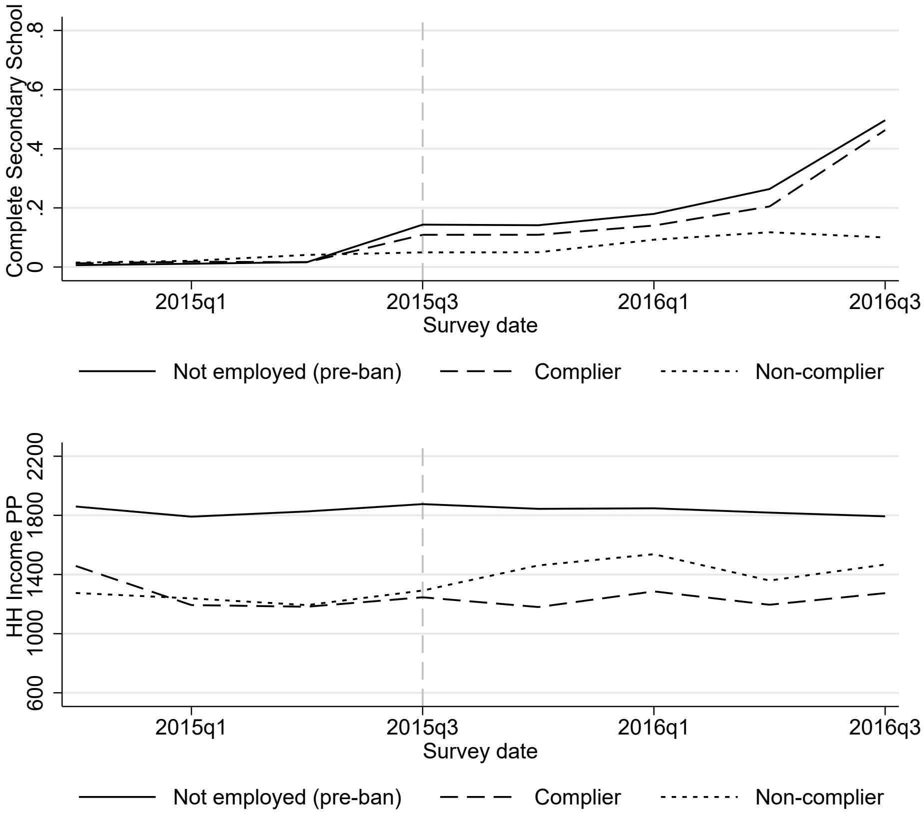

Figure 2.A2: Compliers vs Non-Compliers: HH Income and Secondary Completion

Source: ENOE , authors' analysis.

Notes: – The sample is restricted to working children born between June 13 and December 31 who were banned from the labor force, and are surveyed both before and after the ban. The group "not employed" are children who were not employed pre-ban. The group "complier" refers to children who were employed pre-ban but dropped from the labor force post-ban. The group "non-complier" are children who were employed pre-ban, but did not drop from the labor force post-ban.

Table A1: Summary of Treatment and Control Groups

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

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

Table A2: Post-Ban Descriptive Statistics

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

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

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

	Hours worked I	Hours worked II	Intensive margin III	Intensive margin IV
Treated x Post-ban	−1.094***	−0.745***	−1.691***	−0.900
	(0.199)	(0.157)	(0.487)	(0.592)
Male	3.177***	3.171***	3.879***	3.806***
	(0.266)	(0.267)	(0.334)	(0.334)
HH size	0.409***	0.409***	0.847***	0.833***
	(0.044)	(0.045)	(0.106)	(0.105)
Birth rank: Ref.: First-born				
Middle-born	−0.023	−0.035	0.266	0.173
	(0.104)	(0.105)	(0.416)	(0.412)
Last-born	−0.467***	−0.463***	0.117	0.077
	(0.078)	(0.078)	(0.388)	(0.375)
Both parents present	1.249***	1.232***	0.012	0.090
	(0.367)	(0.362)	(0.914)	(0.918)
Mother's education level: Ref.: None				
Primary education	−2.266***	−2.256***	−2.729***	−2.734***
	(0.297)	(0.296)	(0.656)	(0.631)
Secondary education	−3.409***	−3.396***	−4.987***	−5.062***
	(0.333)	(0.333)	(0.702)	(0.687)
High-school	−4.350***	−4.331***	−8.494***	−8.574***
	(0.416)	(0.417)	(0.873)	(0.862)
Vocational training	−4.434***	−4.440***	−7.984***	−8.036***
	(0.441)	(0.441)	(0.958)	(0.951)
University degree	−5.055***	−5.044***	−10.919***	−10.951***
	(0.475)	(0.478)	(0.879)	(0.880)
Father's education level: Ref.: None/Father not present				
Primary education	−1.186***	−1.160***	−0.854	−0.961
	(0.388)	(0.386)	(0.846)	(0.834)
Secondary education	−2.449***	−2.419***	−2.768***	−2.785***
	(0.412)	(0.408)	(0.925)	(0.919)
High-school	−3.013***	−2.997***	−4.696***	−4.770***
	(0.420)	(0.416)	(0.919)	(0.908)
Vocational training	−3.159***	−3.141***	−7.029***	−6.886***
	(0.424)	(0.418)	(1.391)	(1.405)
University degree	−3.083***	−3.069***	−5.800***	−5.779***
	(0.415)	(0.409)	(0.933)	(0.940)
Locality size: Ref.: > 100,000 inhabitants				
15,000-99,999 inhabitants	0.594***	0.593***	0.539	0.613
	(0.123)	(0.124)	(0.586)	(0.584)
2,500-14,999 inhabitants	0.436***	0.424***	−1.547***	−1.480***
	(0.144)	(0.145)	(0.491)	(0.511)
Less than 2,500 inhabitants	0.958***	0.972***	−3.059***	−2.983***
	(0.130)	(0.128)	(0.326)	(0.338)
Constant	0.530	0.629*	21.971***	21.938***
	(0.367)	(0.352)	(1.556)	(2.373)
State FE	yes	yes	yes	yes
Quarter-by-year FE	yes	yes	yes	yes
Month of birth FE	no	yes	no	yes
State-specific trend	yes	yes	yes	yes
Observations	123,487	123,487	15,911	15,911
R^2	0.110	0.114	0.174	0.181

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

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

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

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

Table A5: Effect of Ban in 2015 on Child Labor: Alternative Std. Error Clustering

Dependent variable:	**School enrollment** I	**Hours worked** II	**Extensive margin** III	**Intensive margin** IV
A. Baseline: Birth Month and Survey Year				
Treated x Post-ban	0.022***	−0.755***	−0.012**	−0.915
	(0.004)	(0.157)	(0.005)	(0.598)
Observations	123,487	123,487	123,487	15,911
B. Birth Month and State				
Treated x Post-ban	0.022***	−0.755***	−0.012**	−0.915
	(0.006)	(0.210)	(0.006)	(0.608)
Observations	123,487	123,487	123,487	15,911
C. Birthday (day, month)				
Treated x Post-ban	0.022***	−0.755***	−0.012**	−0.915
	(0.006)	(0.197)	(0.006)	(0.608)
Observations	123,487	123,487	123,487	15,911
D. Birth month				
Treated x Post-ban	0.022***	−0.755**	−0.012	−0.915
	(0.005)	(0.272)	(0.009)	(0.588)
Observations	123,487	123,487	123,487	15,911

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

Table A6: Effect of the Labor Law Reform in 2015 on School Enrollment and Labor Outcomes: Excluding State-Trends

	School enrollment I	Employment II	Extensive margin III	Intensive margin IV
Treated x Post-ban	0.022^{***}	-0.012^{**}	-0.745^{***}	-0.900
	(0.004)	(0.005)	(0.157)	(0.592)
Individual FE	yes	yes	yes	yes
Quarter-by-year FE	yes	yes	yes	yes
State-specific trend	no	no	no	no
Observations	123,487	123,487	123,487	15,911
R^2	0.135	0.112	0.114	0.181

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

Table A7: Descriptive Statistics Pre-Ban: Working Children Banned – Compliers and Non-Compliers

	All		Compliers		Non-Compliers		T-test
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Δ Mean[a]
Dependent variables							
Attends school	0.817	0.387	0.884	0.320	0.689	0.463	0.195***
Conditional hours worked	22.802	16.796	20.323	15.715	26.506	17.685	−6.183***
Cond. Dependent variables							
Informal work	0.994	0.079	0.995	0.072	0.992	0.088	0.003
Paid employment	0.360	0.480	0.321	0.467	0.440	0.497	−0.119***
Sector							
Primary	0.312	0.463	0.258	0.438	0.392	0.489	−0.134***
Secondary	0.193	0.395	0.191	0.394	0.196	0.398	−0.005
Tertiary	0.495	0.500	0.551	0.498	0.412	0.493	0.139***
Contract	0.008	0.088	0.010	0.102	0.004	0.062	0.007
Benefits	0.008	0.088	0.005	0.072	0.012	0.108	−0.006
Part-time	0.369	0.483	0.364	0.481	0.379	0.486	−0.015
Hourly wage	9.548	15.741	9.889	17.553	9.039	12.571	0.850
Control variables							
Treatment	1.000	0.000	1.000	0.000	1.000	0.000	0.000
Post-ban	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Male	0.738	0.440	0.686	0.465	0.838	0.369	−0.152***
Household size	5.433	1.870	5.365	1.810	5.560	1.974	−0.195*
Month of birth	9.006	1.871	9.098	1.859	8.831	1.882	0.268**
Both parents present	0.795	0.404	0.783	0.413	0.819	0.386	−0.036
Family order							
First-born	0.424	0.494	0.436	0.496	0.402	0.491	0.034
Second-born	0.344	0.475	0.335	0.472	0.360	0.481	−0.025
Last-born	0.232	0.422	0.229	0.420	0.238	0.426	−0.009
Mother's education level							
No education	0.080	0.272	0.056	0.230	0.127	0.333	−0.071***
Primary education	0.415	0.493	0.399	0.490	0.445	0.498	−0.046
Secondary education	0.357	0.479	0.386	0.487	0.301	0.459	0.085***
High-school	0.072	0.258	0.068	0.252	0.078	0.268	−0.009
Vocational training	0.037	0.188	0.051	0.220	0.009	0.097	0.042***
University degree	0.040	0.196	0.040	0.195	0.040	0.196	−0.000
Father's education level							
No Education	0.258	0.438	0.272	0.445	0.231	0.422	0.041
Primary education	0.341	0.474	0.307	0.461	0.405	0.491	−0.098***
Secondary education	0.270	0.444	0.280	0.449	0.252	0.435	0.028
High-school	0.076	0.266	0.087	0.282	0.056	0.231	0.030*
Vocational training	0.022	0.147	0.021	0.144	0.024	0.152	−0.002
University degree	0.033	0.180	0.034	0.180	0.033	0.179	0.001
Locality size							
More than 100,000 inhabitants	0.320	0.467	0.363	0.481	0.240	0.428	0.123***
15,000-99,999 inhabitants	0.124	0.330	0.139	0.346	0.096	0.296	0.043**
2,500-14,999 inhabitants	0.172	0.377	0.178	0.382	0.160	0.367	0.018
Less than 2,500 inhabitants	0.384	0.486	0.320	0.467	0.504	0.501	−0.183***
Observations	1,230		805		425		

Notes: – The table presents descriptive statistics focusing on a limited panel dimension. The sample is restricted to working children born between June 13 and December 31 who were banned from the labor force, and are surveyed before and after the ban. The column compliers identifies children who stopped working after the ban. The column non-compliers includes children who did not stop working after the ban.

Table A8: Effect of the Child Labor Ban: Placebo, Constitution Amendment, and Labor Law Reform - Two Cohorts (Born between July and December)

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

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

Table A9: Pre-Ban Descriptive Statistics by Gender: Conditional on Working

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

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

Table A10: Pre-Ban Descriptive Statistics: Working vs. Non-Working Children

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

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

Table A11: Post-Ban Descriptive Statistics: Working vs. Non-Working Children

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

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

Table A12: Post-Ban Descriptive Statistics for 2018 and 2019

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

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

3 School Attendance and Child Labor: Evidence from Mexico's Full-Time School Program*

Abstract: This paper examines the effect of a program that extended the length of the school day from part-time to full-time in Mexico, on school enrollment, time spent on schooling activities, as well as market and excessive domestic work of children aged 7 to 14. We further analyze possible spillover effects within the household focusing on older siblings and parents. To identify the effect, we take advantage of the staggered implementation of the Full-Time Schools (FTS) program across municipalities from 2009 to 2018. The results show that the FTS program has no impact on school enrollment, but increases the weekly hours allocated to schooling activities, and at the same time reduces child labor hours. A one standard deviation increase in the share of FTS reduces the probability of engaging in child labor by 0.9 percentage points, which implies a 12% reduction in child labor.

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*Co-authored with Fernanda Martínez Flores. This chapter contains minor revisions on a previous version published as: Kozhaya, M., & Martinez Flores, F. (2022). School attendance and child labor: Evidence from Mexico's Full-Time School program. *Economics of Education Review*, 90, 102294. We thank two anonymous referees, Thomas K. Bauer, Julia Bredtmann, Ira N. Gang, Melanie Khamis, Arndt Reichert, Kerstin Schneider, Anna Makles, Franz Westermaier for their constructive comments. We also thank the participants of the internal research seminars at Wesleyan University, the World Bank, University of Wuppertal, and RWI for helpful comments and suggestions. Janin Marquardt, Johanna Muffert, and Dominik Paluch provided excellent research assistance. All remaining errors are our own.

3.1 Introduction

In developing countries, one out of every four children is engaged in child labor (ILO, 2017). Child labor affects child's development negatively (Beegle *et al.*, 2009; Gunnarsson *et al.*, 2006; Holgado *et al.*, 2014) and has long-lasting consequences with respect to health, education, productivity, and wages later in life (Emerson and Souza, 2011b; O'Donnell *et al.*, 2005). In Latin America and the Caribbean, more than 10 million children are involved in forms of employment that do not comply with minimum age requirements and are hazardous or exploitative. Therefore, increasing school enrollment has been the centerpiece of global anti-child labor policies (U.S. Department of Labor, 2019) and several countries in Latin America have made important progress towards advancing the goal of achieving universal primary coverage.[2] Studies analyzing the impact of these initiatives targeted at increasing school enrollment and attendance find large increases in school attendance and much smaller decreases in child labor (see e.g., Skoufias *et al.*, 2001; Glewwe and Olinto, 2004; Maluccio and Flores, 2005; Ferro *et al.*, 2010; Attanasio *et al.*, 2010) because school attendance and child labor are not mutually exclusive (de Hoop and Rosati, 2014), and the trade-off between work and schooling is not clear-cut (Kondylis and Manacorda, 2012).[3]

In settings where primary coverage is almost universal, increasing school enrollment as an avenue for reducing child labor is exhausted. Therefore, current education policies are shifting from increasing schooling access to improving learning opportunities e.g., through the extension of the school day and Full-Time School programs (FTS) (UNESCO, 2015).[4]

[2] In Mexico, for example, from 1990 to 2015, school enrollment increased from 89.4% to 97.7% for children aged 6-11 and from 78.6% to 93.3% for children aged 12-14 (INEE, 2018a). Yet, in 2017 Mexico continued to account for over 30% (3.2 million) children engaged in child labor (INEGI, 2018a). In absolute terms, Brazil, Mexico, and Peru have the highest number of working children. As percentage of the population, Bolivia, Paraguay, and Peru have the highest child labor rates (CEPAL, 2019).

[3] See de Hoop and Rosati (2014) for an extensive literature review of the evidence on the impact of initiatives to increase school enrollment and attendance, in particular, conditional in-kind or cash transfer, on schooling and child labor.

[4] Longer school days have been implemented in Chile, Uruguay, Colombia, Mexico, Brazil, among others.

Yet, little is known about how increasing school instruction time has an impact on both school enrollment and child labor.[5] Our paper aims at filling this gap. On the one hand, increasing school instruction time could lead to a direct impact on child labor by increasing the marginal returns to education, which could lead to higher wages for the child in the future, thereby compensating the household for the loss of child's income today (Edmonds, 2007). On the other hand, FTS indirectly subsidize childcare, freeing the time of other household members to increase their labor supply and substitute child labor for adult labor (Basu and Van, 1998). This study is the first to investigate the causal effect of a FTS program on child labor. We focus on a range of child labor indicators that cover market work, excessive domestic work, weekly hours worked, as well as extensive and intensive margins.[6] We further elaborate on different sources of heterogeneity to explain our results such as poverty level at the regional and household level, as well as child-specific characteristics and elaborate possible mechanisms that explain our findings.

A second contribution of this paper is to the studies analyzing the impact of FTS programs. FTS programs have been shown to have a positive impact on academic outcomes, leading to modest improvements in test scores (see e.g., Bellei, 2009; Agüero, 2016; Hincapie, 2016; Figlio *et al.*, 2018; Cabrera-Hernández, 2020; Padilla-Romo, 2022; Thompson, 2021), and to a decrease in the probability of early dropout and grade repetition (García *et al.*, 2013). Only few studies focus on non-academic outcomes. These studies find that longer-school days decrease the involvement of children in risky activities such as crime and early pregnancy (Berthelon and Kruger, 2011). We contribute to the literature by focusing on academic and non-academic outcomes that have not been analyzed before i.e., school enrollment[7], time

[5] An exception is the study by Tang *et al.* (2020) who analyze the effect of education subsidies by analyzing compulsory education (and not increasing instruction time at school), in rural China. The authors find that free compulsory education reduced the incidence of child labor for boys, but not for girls.

[6] Market work refers to income generating activities inside or outside the household. Excessive domestic work refers to activities and services for consumption within the household. The exact definitions of both activities are provided in Section 3.3.

[7] An exception is the study by García *et al.* (2013), who evaluate the impact of full-time schools on dropout rates in Colombia. Yet, there is no evidence for the FTS in Mexico.

spent on schooling activities, and child labor indicators. We also explore the role of school meals as one of the channels through which FTS could impact school enrollment and child labor.

Finally, we contribute to the literature analyzing spill over effects of FTS to other family members. Previous studies find that FTS increase the labor force participation of mothers (Contreras and Sepúlveda, 2016; Padilla-Romo and Cabrera-Hernández, 2019) and grandmothers (Padilla-Romo and Cabrera-Hernández, 2020) because these programs entail subsidized childcare and a school day which is more compatible with the traditional working day. We extend this analysis by focusing not only on parents, but also older siblings. This is of particular relevance because within household substitution effects are likely to determine –to a large extent– the impact that FTS have on child labor. As young children spend more time in school, this may decrease the need for child supervision at home. Therefore, besides analyzing the decision to work and hours worked, we also evaluate if the time spent on domestic work changes as a response to the extension of the school day.

To identify the effect, we take advantage of the roll-out of the Full-Time Schools (FTS) program,[8] implemented in Mexico from 2009 to 2018. The FTS program is a national initiative that extended daily school hours from part-time (four hours) to full-time (six or eight hours) in primary and secondary schools[9] and covered more than 3 million children during the school year 2017/2018. The FTS program increases the weekly instruction time from 20 to either 30 or 40 hours i.e., an increase in schooling hours by 50% or even 100%. This study combines administrative school data with data from the Mexican National Labor Force Survey (ENOE) and the *"Módulo de trabajo Infantil"* (MTI), a nationally representative survey designed to collect information on economic, domestic, and schooling activities carried out by children in Mexico.

[8] *Programa Escuela de Tiempo Completo (PETC).*

[9] Schools operating on an eight hours basis had to offer a warm meal which was highly subsidized by the program.

Our empirical strategy exploits the staggered implementation of the FTS program at the municipality level to identify the causal effect of a longer school day. We examine the impact of changes in the share of FTS at the municipality level on a yearly basis on (i) the probability of attending school, (ii) weekly hours spent on schooling activities, and (iii) a range of child labor indicators for children aged 7 to 14 years. In addition, we investigate if the program has an impact on the labor force participation (LFP) and domestic work of other household members.

The program's guidelines establish that priority should be given to schools located in disadvantaged areas. Therefore, we address the concern that the roll-out of the FTS program may be endogenous to municipality characteristics by following three main steps. First, our empirical specification controls for municipality and time fixed effects, as well as a vector of time-varying municipality characteristics which capture local changing economic conditions which may be correlated with the roll-out of the program. We provide as well several robustness tests based on our baseline specification.

Second, we present descriptive and empirical evidence to rule out pre-existing trends and differences of children living in municipalities with different coverage of the program. To do so, we present the results of dynamic models interacting the share of FTS with a yearly dummy at the individual and municipality level. Third, we present the results of alternative specifications such as i) an IV approach that instruments the share of FTS with an interaction between the share of eligible schools at the municipality level and the yearly budget allocated to the program at the state level, ii) simple DiD estimates focusing on low-intensity and high-intensity municipalities, and iii) an event-study design which allows us to rule out pre-trends and evaluate if effects are persistent several periods after the municipality takes up the program.

Our findings can be summarized as follows: When focusing on schooling, we find no impact that an increase in the share of FTS in the municipality has an impact on the probability of being enrolled in school, but a larger coverage of the FTS program leads to an increase in the weekly hours allocated to schooling activities. This finding alleviates the concern that parents, who rely more strongly on child labor, will take their children out of school due to the increase in daily schooling hours. When focusing on child labor, we find that an increase in the share of FTS decreases the probability that children work. A standard deviation increase in the share of FTS at the municipality level leads to a 0.9 percentage point reduction in the probability to engage in child labor. Boys are less likely to engage in market work; and girls less likely to engage in excessive domestic work. We find that the reduction in child labor is smaller for children living in extreme poverty.

When analyzing the response of other household members to the FTS program, we find no impact for older siblings. This finding supports that the reduction in child labor for children aged 7-14 is not substituted by an increase in the probability to work by individuals aged 15-17. In line with Padilla-Romo and Cabrera-Hernández (2019), we find that mothers of children aged 7-14 increase their labor force participation. This increase in LFP is driven by mothers with low levels of education, in the lowest income quintiles, and is similar for mothers who face higher or lower childcare costs. Therefore, we argue that spillover effects within the household are not only driven by the indirect subsidy to childcare, but also due to a substitution effect between child and adult work. Finally, we find that fathers do not adjust their labor force participation, but they increase slightly the weekly hours allocated to domestic work.

The following section provides a general overview of the education system in Mexico, and describes the FTS program. Section 3.3 presents the data and Section 3.4 the empirical strategy. Section 3.5 shows the results and Section 3.6 concludes.

3.2 Background

3.2.1 Education and Child Labor in Mexico

The structure of the basic education system in Mexico is divided in three levels: primary education (grades 1-6), lower secondary education (grades 7-9), and upper secondary education (grades 10-12). Primary education starts at the age of 6 and all basic education levels are compulsory. As of 2012, upper secondary education became also compulsory (OECD, 2018). The compulsory schooling regulation is not based on age but on the school level. The minimum working age is set at 15.[10] School choice is free and most of the students attend public schools. During the school year 2016/2017, 90% of students enrolled in basic education attended a public school (INEE, 2018a).

In 2017, despite important improvements in school enrollment in Mexico, 11% (3.2 million) of minors aged 5 to 17 years were still involved in child labor. 6.4% of minors were involved in market work under the minimum age regulation, 4% performed domestic work in unsuitable conditions, and 0.7% combined both market work and domestic work. For the same year, the child labor rate was higher in rural areas (localities of less than 100 thousand inhabitants) with 13.6% as opposed to urban areas (localities of 100 thousand and more inhabitants), where child labor reached 7.6%. The agricultural sector accounts for more than 34% of child laborers, followed by the service (22%), and the trade sector (20.3%). Among children engaged in market work, 58.3% work for a family member, 39% are unpaid, and 31.3% receive only the minimum wage (INEGI, 2018a).

Figure 3.1 illustrates the share of children in child labor taking into account only market work, and the share of children out of school for the period 2009 to 2017. For all age groups, the child labor rate exceeds the rate of children out of school and the differences are larger

[10] In July 2015, the minimum working age was shifted from 14 to 15. We provide the results by different age groups in the robustness section to show that this change is not driving the results.

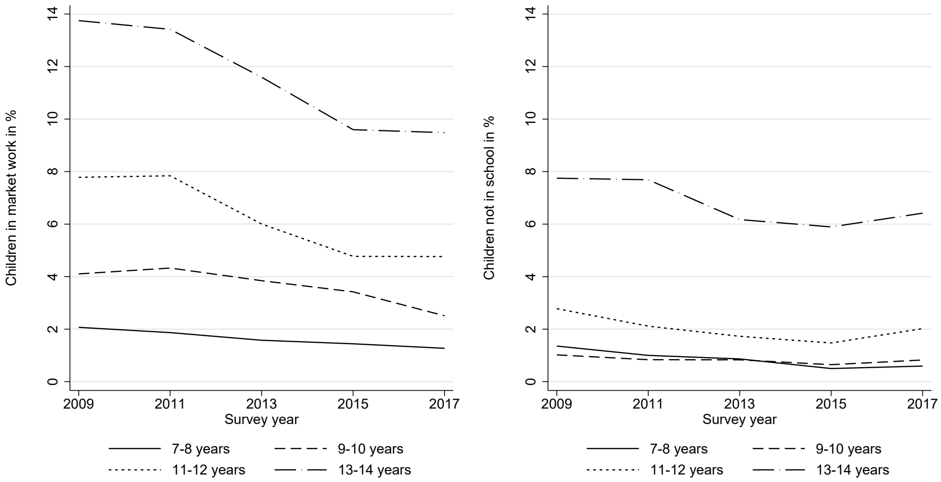

Figure 3.1: Schooling and Market Work by Age Group
Source: Authors' analysis using data from ENOE – Módulo de Trabajo Infantil (MTI).
Notes: – The shares are calculated using the MTI databases available biennially from 2009 to 2017.

for older children. In 2009, the child labor rate was 2% for children aged 7-8, 4% for children aged 9-10, and 8% for children aged 11-12. Yet, schooling was almost universal for children in these age groups, with only 2% of them out of school. For children aged 13-14, the child labor rate was 14% and the out of school rate 8%. In 2017, the share of children out of school remained stable for all age groups, except for the group of children aged 13-14, who experienced a decrease of 2 percentage points. The share of children working decreased for all groups, with the largest drop for children aged 11-12 (3 percentage points) and aged 13-14 (4 percentage points).

3.2.2 The Full-Time School Program

The FTS program is a federal program, the main objective of which is improving the quality of public basic education in Mexico through the extension of the school day. The program entails an increase in the number of daily school hours from four to either six or eight hours. The additional hours are dedicated to academic activities, cultural activities, and sports.

The FTS guidelines establish how additional time at school should be distributed across different activities. However, the schools are flexible to implement their schedule following these guidelines. In addition, the education authorities at the local level assign a supervisor for different FTS in the same area to monitor the technical and financial implementation of the program.

On a regular school day, primary schools operate from 8:00-12:30 and secondary schools from 7:30-13:40. If the school is part of the program and operates on an eight hour basis, the schedule is extended as follows: 08:00-16:00 for primary schools and 07:00-16:00 for secondary schools.

The FTS, an initiative of the Ministry of Education, was first introduced in 2007, during the administration of former President Felipe Calderón (2006-2012), as a small-scale program that intended to gradually extend the weekly school instruction time. By the end of his administration, 4,750 schools were operating on a full-time basis. In February 2013, a major education reform was announced by the administration of former President Enrique Peña Nieto (2012-2018), having one of its main components directed towards improving the quality of basic education in Mexico through the FTS program.[11] Therefore, the FTS program was given priority and was implemented on a national scale. The federal budget for the program doubled between 2012 and 2013, from 2.5 to 5.2 billion pesos. By the end of 2018, over 25 thousand schools were operating on a full-time basis, which is about 40% of schools that qualify for the program, covering over 3 million children (around 16% of students enrolled in primary or secondary education).[12] This program has become one of the largest and most relevant education interventions in Mexico (CONEVAL, 2018b).

[11]The results of the global ranking Program for International Student Assessment (PISA) in 2012 revealed that Mexico earned the lowest score out of all 34 OECD countries in Mathematics, Reading, and Science (OECD, 2013).

[12]During the 2017/2018 school calendar the total number of public primary and secondary schools was 87,756 and 34,293, respectively. The number of students enrolled in primary and secondary education was 18.9 and 6.1 million, respectively (INEE, 2018b).

A relevant aspect of the FTS worth clarifying is that neither schools nor parents can influence the selection of a school into the program. The selection of schools into the program is as follows: First, the yearly budget is assigned at the federal level. Second, participating schools are selected by educational authorities at the state level (Autoridad Educativa Local - AEL) before the start of the school year. Third, the schools chosen by the state authorities to implement the program should fulfill at least one of the following requirements: (i) cover all grades of the corresponding school level, (ii) offer only one shift[13], (iii) have an appropriate infrastructure for the extension of instruction time at school, and (iv) attend vulnerable population. Therefore, differences in how the budget is distributed across municipalities depends on the number of eligible schools. The Mexican Ministry of Education further reported giving priority to larger schools to cover the maximum number of students possible. The latter implies that the roll-out is not exogenous by construction. However, we address this concern by providing descriptive evidence on parallel trends, municipality characteristics of high and low intensity areas, and treatment estimates by year to rule out that pre-existing trends are driving the results.

Moreover, the subsidy is granted at the federal level and does not substitute other federal, state, or municipal funding. The program guidelines establish that the FTS funding will only be used for implementation purposes and not for infrastructure purposes. The states consider two different operation modes for the extension of the time spent in school (between 6 and 8 hours). Schools selected into the program operate on an eight-hour basis if they have the facilities to provide a warm meal per day, otherwise they operate on a six-hour basis. The guidelines establish that the full-time service has to be provided every day of the school calendar year, and that all students in the school must comply with the program, i.e., all students in the school should start and leave school at the same time of the day.

[13]Some schools in Mexico offer a morning and an afternoon shift. Students in the primary age attend the morning shift and students in the secondary age attend the afternoon shift. Schools offering two shifts are not eligible to participate in the program.

Schools participating in the program are supported in two different ways. First, they receive technical support to develop strategies to adapt the syllabus to the additional hours by assessing, orienting, and training the corresponding educational authorities. Second, they receive financial support to cover the costs of lengthening the school day which is intended to i) cover the payment of financial support to directors, teachers and supporting staff (up to 61% of the subsidy), ii) cover costs of technical assistance i.e., acquisition of materials/equipment for students, and iii) subsidize the food service in case the school was selected to operate on an 8 hours basis (15 pesos per student).[14] On average, the subsidy allocates 90 thousand pesos per school and year (CONEVAL, 2018b). In Section 3.3, after introducing the data used for this article, we provide additional descriptive statistics of the roll-out of the FTS program.

3.3 Data and Descriptive Statistics

3.3.1 Data

The data used for this study comes from three different sources. First, we use administrative data from the Ministry of Education on the universe of schools offering basic education in Mexico. The data consists of the lists of schools providing basic education by school calendar year spanning from 2009/2010 to 2017/2018. The lists include information on the total number of enrolled students, total number of teachers, and school location.

This data is complemented with the official lists of schools participating in the FTS program by school year. We calculate the share of FTS by municipality and school calendar year restricting the sample to public primary and secondary schools. As an alternative definition, we calculate the share of students in FTS. The lists not only allow us to identify

[14]For more information on the specific budget allocation see (DOF, 2013).

which schools are part of the program, but also if the schools offer the subsidized meal.[15] Based on this data, we calculate the share of schools that operate on an 8-hour basis vs those that operate on a 6-hour basis.

Second, we use survey data from the Mexican Labor Force Survey (ENOE). The ENOE data spans from the first quarter of 2009 to the fourth quarter of 2017. The ENOE is collected on a quarterly basis as a rotating panel with households surveyed for 5 quarters. The ENOE reports comprehensive information on demographic characteristics of the children (such as gender, age, and municipality of residence), parental demographic characteristics (education and marital status) and household characteristics (number of children, age of the children, household size, and household income). Information on employment is only available for individuals older than 15 (active on the labor force, employment status, hours worked, and earnings). In our baseline specification, we refrain from using income because for 20% of the sample income is missing or reported as zero. Yet, we use household income for our definition of household poverty in Section 3.5.2.

Third, we use data from the *Módulo de Trabajo Infantil* (MTI), a special module which is part of the ENOE. Since 2007, the MTI is conducted every two years at the national level during the fourth quarter of the year to collect information on child labor following international standards by the ILO and United Nations Fund for Children (UNICEF).[16] In contrast to the ENOE, this module is designed as cross-sectional surveys and does not allow tracking individuals over time. The MTI is collected in all households sampled in the ENOE (in the respective wave) that have at least one member aged 5-17 years (INEGI,

[15]This information is available only after the school year 2012/2013, once the program was rolled-out at the national level and the guidelines were officially established.

[16]Other databases that have been used to evaluate schooling and child labor in Mexico include the Survey of Household Socio-Economic Conditions (ENCASEH97) originally used to determine eligible communities for the Progresa/Oportunidades program and the follow-up evaluation surveys ENCEL (see e.g., Skoufias *et al.*, 2001; Behrman *et al.*, 2011). In contrast to these databases, the MTI is conducted at the national level with the specific purpose of collecting information on the type of economic, domestic, and schooling activities carried out by children/teenagers aged 5-17.

2018b). The guidelines of the survey establish that there is one main informant who provides the information: the individual is usually the household head or the spouse. However, if household members older than 12 are present at the time of the interview, they each provide their own information.

The data can be matched to the ENOE database and provides employment information on all children living in the household aged 5-17. Specifically, the MTI data reports information on school enrollment, a rich set of labor force statistics, information on working conditions, and time spent doing household activities. For the empirical analysis we use information on the MTI and ENOE starting 2009 given that the school data is only available from this year onwards. The national coverage of the surveys and information on the location of the household at the municipality level allows us to merge this information with the data on the FTS program.

We merge the ENOE and MTI databases using the household and individual identifiers. To merge these data with the administrative school data, we use the municipality identifier. All municipalities in Mexico (2,458) have at least one school offering basic education. We are able to merge 65% of the municipalities (1,574) given that the ENOE surveys do not sample all municipalities every quarter. The ENOE and MTI data were obtained from the National Institute of Statistics and Geography (INEGI).

We further complement our database using the marginalization level data obtained from the *Consejo Nacional de Población* (CONAPO) which are available for the years 2010 and 2015, at the municipality and locality level. The marginalization level is a multidimensional poverty measure which takes education, dwelling characteristics, population geographical distribution, and income level into account (CONAPO, 2019).[17]

[17] In this context low (high) marginalized areas means non-poor (extremely-poor) regions.

In the literature, the definition of child labor is broad and reflects between and within country differences in the types of activities that children engage in (Edmonds and Pavcnik, 2005). For our definition of market work, we use a pre-coded variable provided in the MTI database, which follows the international standards proposed by the UNICEF and the ILO, to identify child labor. Market work is the type of work that produces certain primary goods and services for the market, own production, and/or own consumption. This variable takes the value one if the child (i) is younger than 12 and is involved in light work, or (ii) is involved in regular work under the minimum legal working age which is 15, or (iii) is involved in hazardous work for children aged 7 to 14 years old. Hazardous work includes work that risks the child's safety, health, and morality and includes e.g., working at night, lifting heavy objects, or working with dangerous substances like chemicals and pesticides.

The definition of domestic work is less clear-cut in the literature. We follow a similar approach to Dammert (2010) to identify excessive unpaid household work. We aggregate the reported weekly hours spent (i) taking care of children or elderly people in the household, (ii) doing household chores, and (iii) renovating the house and fixing household appliances. While Dammert (2010) focuses on children who spent at least one hour per day on these activities, we focus on children who spend at least two hours per day for our definition of the extensive margin. For the intensive margin, we use the full distribution.

Table 3.1 shows the main descriptive statistics for the years 2009, 2013, 2017, and the average for all years. From 2009 to 2017 the share of FTS increased, on average, from 1.8% to 18%. Turning to the outcome variables, school attendance is almost universal and remains fairly constant during this period. Surprisingly, the same is true for the average weekly hours spent on schooling activities, which include time in school and time spent on homework, and amounts to 31 hours per week. Yet, the percentage of children working decreases by about 6 percentage points. This decrease can be observed for both domestic and market work, which decrease by 2.6 and 4.4 percentage points, respectively.

Table 3.1: DESCRIPTIVE STATISTICS

	2009		2013		2017		2009-2017	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Share of FTS	0.018	0.035	0.126	0.114	0.180	0.148	0.107	0.130
Dependent variables								
Attends school	0.968	0.177	0.978	0.146	0.976	0.153	0.974	0.158
Weekly hours spent on school activities	30.418	11.536	31.744	10.828	30.939	12.321	31.110	11.533
Child is working	0.170	0.376	0.119	0.323	0.106	0.307	0.136	0.343
Cond. weekly hours worked	19.867	11.719	19.496	12.284	19.568	11.650	19.630	11.766
Market work	0.070	0.254	0.055	0.229	0.044	0.204	0.058	0.233
Household work	0.110	0.313	0.069	0.253	0.066	0.248	0.085	0.279
Cond. weekly hours worked (market)	15.824	14.276	15.163	14.885	15.937	15.338	15.534	14.647
Cond. weekly hours worked (domestic)	18.516	7.286	18.875	7.619	18.346	6.946	18.436	7.154
Child characteristics								
Age	10.609	2.288	10.598	2.261	10.591	2.290	10.588	2.280
Male	0.510	0.500	0.510	0.500	0.507	0.500	0.509	0.500
Receives gov. support	0.244	0.429	0.253	0.434	0.264	0.441	0.266	0.442
Number of siblings	3.131	1.449	2.912	1.243	2.805	1.173	2.965	1.315
Birth order								
First born	0.348	0.476	0.369	0.482	0.375	0.484	0.364	0.481
Middle born	0.323	0.468	0.290	0.454	0.268	0.443	0.297	0.457
Last born	0.329	0.470	0.342	0.474	0.356	0.479	0.339	0.473
Both parents present	0.818	0.386	0.825	0.380	0.799	0.401	0.816	0.387
Mother's education level								
No education	0.075	0.263	0.043	0.202	0.035	0.184	0.052	0.223
Primary education	0.367	0.482	0.292	0.455	0.245	0.430	0.309	0.462
Secondary education	0.291	0.454	0.344	0.475	0.381	0.486	0.335	0.472
High-school	0.104	0.306	0.141	0.348	0.167	0.373	0.134	0.341
Vocational training	0.085	0.279	0.069	0.253	0.049	0.215	0.067	0.250
University degree	0.078	0.268	0.112	0.315	0.123	0.328	0.102	0.302
Father's education level								
No education	0.056	0.229	0.036	0.187	0.035	0.184	0.043	0.204
Primary education	0.348	0.476	0.282	0.450	0.262	0.440	0.303	0.460
Secondary education	0.291	0.454	0.327	0.469	0.338	0.473	0.320	0.467
High-school	0.135	0.342	0.172	0.378	0.186	0.389	0.160	0.367
Vocational training	0.041	0.198	0.038	0.192	0.026	0.159	0.035	0.183
University degree	0.129	0.335	0.144	0.351	0.152	0.359	0.139	0.346
Locality size								
More than 100,000 inhabitants	0.429	0.495	0.487	0.500	0.448	0.497	0.444	0.497
15,000-99,999 inhabitants	0.152	0.359	0.154	0.361	0.149	0.356	0.151	0.358
2,500-14,999 inhabitants	0.152	0.359	0.134	0.341	0.150	0.357	0.148	0.356
Less than 2,500 inhabitants	0.266	0.442	0.225	0.418	0.253	0.435	0.257	0.437
Municipality characteristics								
Share age 7-17 out of school	0.113	0.076	0.083	0.064	0.079	0.068	0.092	0.072
Share living in poverty	0.324	0.201	0.320	0.175	0.334	0.186	0.335	0.191
Share women in the LF	0.468	0.140	0.511	0.129	0.506	0.139	0.490	0.137
Observations	50,408		45,107		41,683		230,256	

Notes: – The table presents descriptive statistics for the years the MTI data is available. The last two columns report the mean and standard deviation for the full sample.

The table shows no decrease on conditional hours worked, with an average of 15.5 hours spent on market work and 18.4 hours on household work per week. The last two columns of Table 3.1 further show that, on average, the children in our sample are 10.6 years old and have 2.9 siblings. 51% are boys, 37% are the first borns, and 82% of the children live with both of their parents in the household. 26% receive support from the government, e.g., *Oportunidades*. Almost 56% of the children live in localities with less than 100 thousand people (rural areas). 70% (67%) of the children have mothers (fathers) with secondary or lower levels of education.

To control for time-varying municipality characteristics, we build population shares using the ENOE survey weighted by the expansion factor provided in the database. For these variables, we restrict the data to the fourth quarter of the year (to be consistent with the MTI database) and use lagged variables to account for the fact that the FTS could have an impact on e.g., employment rates or the number of children out of school. We construct three variables i) the share of the population aged 7-17 who are out of school, ii) the share of the population living below the poverty line, and iii) the share of women in working age i.e., age 25-65, who are active in the labor force.

3.3.2 The Roll-out of the FTS

The empirical strategy relies on the roll-out of the FTS program across time and municipalities. Given that the roll-out of the program was not homogeneous, in this section we explore if there are large differences between municipalities that witnessed a large expansion of the program and municipalities that did not.

In Figure 3.2 we show the staggered implementation of the program by municipality and school calendar year. The map illustrates the share of FTS at the municipality level, i.e., the number of FTS over the total number of schools in the municipality during the respective school calendar year. The first map shows that in the school year 2011/2012 most of the municipalities in Mexico had close to zero FTS. In contrast, in the school calendar year

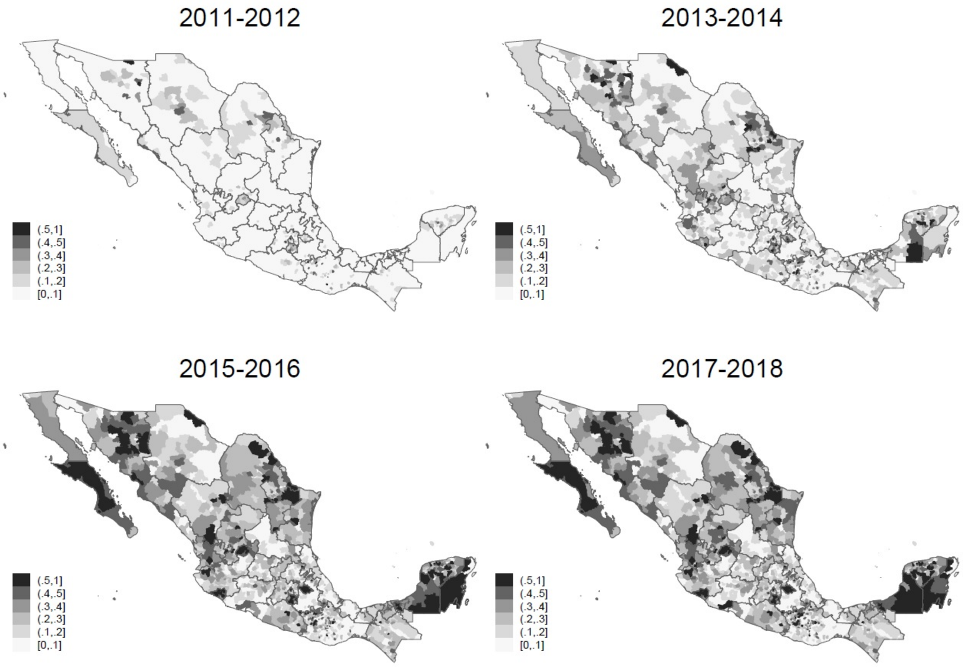

Figure 3.2: Program Rollout: Share of FTS by Municipality and School Year

Source: Authors' analysis using data requested from the Ministry of Education.
Notes: – The share of FTS is calculated from administrative data on the universe of schools in Mexico.

2017/2018, all states were covered by the program and 76% of the municipalities had at least one FTS (CONEVAL, 2018a).

To observe if the areas where the FTS was implemented with more intensity are particularly vulnerable or disadvantaged, we illustrate the marginalization level at the municipality level for 2010 (before the implementation of the program) and 2015 (after the implementation of the program) in Figure 3.A1 in the Appendix. In comparison to Figure 3.2, which shows the FTS program roll-out, we can see that while poverty is more prevalent in the south of Mexico, the share of FTS increases in municipalities with a high and low marginalization level. The roll-out of the program was therefore not exclusively determined by the marginalization level in the municipality. Even if the program's guidelines establish that priority should be given to disadvantages communities, the capacity of the municipality to implement the program is an important constraint for implementation.

Next, to ease the comparison of pre-program descriptive statistics, we follow Havnes and Mogstad (2011b) and Blanden *et al.* (2016), to define treatment and control municipalities. Therefore, we split municipalities according to those that implemented the FTS program above (treament) and below (control) the median i.e., high- vs low-intensity municipalities. This simple binary classification is only used to provide descriptive statistics and test the robustness of the results. In the empirical analysis, however, we exploit expansion of the program by focusing on the proportion of treated schools at the municipality level at time t to identify the effect.

In Table 3.A1 in the Appendix we show pre-program descriptive statistics at the child level for the year 2009 using this classification. The table shows significant differences between children living in municipalities that implemented the program with high- and low-intensity. However most of these differences are very small. Children in high-intensity areas are slightly more disadvantaged e.g., they are slightly more likely to work (17.6% vs. 16.2%), but tend to work fewer hours in market (15.3 vs 16.7 hours) and domestic work (18.3 vs 18.9 hours). We find larger differences in the proportion of children who receive additional government support in high-intensity areas (26.9% vs 20.4%). Children in high-intensity areas are more likely to have mothers with no education (8.1% vs 6.5%), and to live in municipalities with a higher share of the population living in poverty (33.1% vs 31.4%).

We further compare additional poverty indicators at the municipality level in 2010 before the national roll-out of the FTS program. Table 3.A2 in the Appendix shows that the poverty indicators are similar in municipalities that implemented the program below and above the median. The table shows that high-intensity municipalities are slightly better off with e.g., a smaller share of the population who cannot read or write, without primary education, or living in localities with less than 5 thousand inhabitants, and a higher share of municipalities with a low marginalization degree.

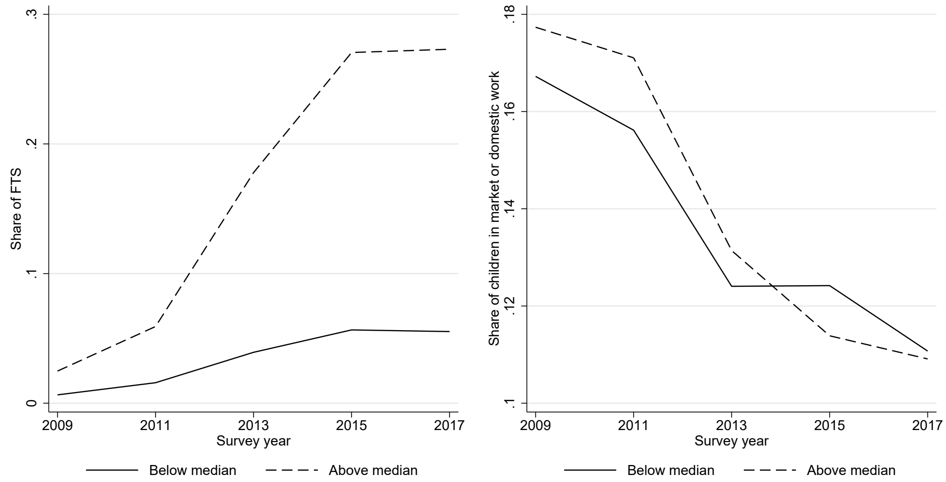

Figure 3.3: Schooling and Market Work by Tercile
Source: ENOE – Módulo de Trabajo Infantil (MTI), authors' analysis.
Notes: – The share of FTS is calculated from administrative data on the universe of primary and secondary schools in Mexico. The share of children in market and domestic work is calculated using the MTI databases available biennially from 2009 to 2017.

Next, we present graphical evidence on the correlation of the roll-out of the program and child labor rates. Figure 3.3 shows the evolution of the FTS program and child labor using the binary classification of high- and low- intensity municipalities. The first graph shows the roll-out of the FTS program. In 2009, before the program was scaled up at the national level, the share of FTS was lower than 5% for both groups. In 2017, the share of FTS remained lower than 10% in low-intensity municipalities, but reached almost 30% in high intensity municipalities.

The second graph shows the evolution of the child labor rate for the same groups. Three main observations stand out. First, before 2013, a level difference in the child labor rate can be observed. Low-intensity municipalities have a lower child labor rate than high intensity municipalities. Second, despite the existing level differences in child labor, the pre-program trends are similar for both groups. Before the national roll-out in 2013, the child labor rate decreased for both groups. This decrease may indicate that other factors unrelated to the FTS program could be driving the decrease in child labor. The figure shows that the share of working children was already decreasing slightly faster in high-intensity municipalities before the national roll-out in 2013.

In the empirical analysis, we include a rich set of control variables and fixed effects and take into account time-varying characteristics that could be correlated with the implementation of the FTS program at the municipality level such as the proportion of young people out of school and the proportion of the population living in poverty. In addition, we provide dynamic estimates that allow us to rule out the existence of pre-treatment differences in municipalities implementing the program at different rates. In Section 3.5.3, we further include state-specific linear time trends to account for pre-existing trends in the outcome. Third, after 2012 when the FTS was launched as a national program, child labor rates decreased faster in high-intensity municipalities and after 2015, the child labor rate was even lower than in low-intensity municipalities.

Finally, we test whether the child labor rate determines the roll-out of the program by regressing the share of FTS in a municipality at time t on the respective child labor rate. The estimated coefficients are close to zero and not statistically significant (see Table 3.A3 in the Appendix). The results are similar if we include lagged values of the child labor rate at the municipality level, which confirms that the current and lagged child labor rates at the municipality level are not a determinant of the roll-out of the program.

3.4 Identification Strategy

To examine the effect of the FTS program on schooling and labor outcomes of children, we exploit the staggered implementation of the FTS at the municipality level from school calendar year 2009/2010 to 2017/2018 and estimate the following model:

$$Y_{imt} = \alpha + \beta FTS_{mt} + \theta'\mathbf{X}_{imt} + \kappa'\mathbf{P}_{imt} + \lambda'\mathbf{M}_{mt} + \sigma_m + \gamma_t + \epsilon_{imt} \tag{3.1}$$

where Y_{imt}, denotes either school enrollment, time spent on schooling activities, or labor outcomes of child i in municipality m at school-year t. For the labor outcomes, we explore (i) the total number of hours worked per week[18], (ii) a binary variable indicating whether the child works (extensive margin), and (iii) the number of hours worked conditional on working (intensive margin). We further distinguish between market and household work.

FTS_{mt} is the share of full-time schools. The share takes into account the number of schools in the program in municipality m during the school calendar year t out of the total number of schools in the municipality. To identify the effect, we exploit the variation of the FTS share at the municipality level from 2011 to 2017 depicted in Figure 3.2. Since we cannot observe if a child attends a FTS, identification occurs through regional differences in access to the program during the time of implementation. In addition, while the program was not implemented randomly, the share of schools covered is exogenous to individual households. The coefficient of interest, β, captures differences in children's outcomes according to the different FTS-coverage across municipalities and can be interpreted as the ITT (intention-to-treat) effect.

X_{imt} is a vector of child characteristics that are likely to affect schooling and labor outcomes including age, gender, a binary indicator whether the child receives government

[18]The number of hours worked also includes the zeros for children who are not employed.

support e.g., *Oportunidades*, number of siblings, and birth order to control for a higher probability of working for older siblings. P_{imt} is a categorical variable controlling for parental education level of the mother and father of the child. That is, if parents have primary, secondary, high-school, vocational training or university degree. Parental education controls capture the preference to send children to school and/or work and are a proxy of household income. We also control for locality size dummies to capture whether children reside in urban or rural areas. Localities are smaller geographical units than municipalities. These dummies capture differences in the implementation of the program within a municipality e.g., priority to rural areas because they are more vulnerable. M_{mt} is a vector of time-varying municipality characteristics that capture labor market and local economic conditions which may affect children's outcomes. These include the lagged values of the share of the poor population, share of children out of school, and share of women active in the labor force.

We include municipality fixed effects σ_m to capture time-invariant characteristics related to the implementation of the program such as heterogeneity in schooling conditions at the municipality level. γ_t captures common yearly shocks such as additional policies implemented by the education reform in 2013 which could directly impact schooling quality e.g., the introduction of a national system to evaluate teachers[19], and ϵ is the error term. Standard errors are clustered at the municipality level.

We run an additional specification including state-by-year fixed effects to capture only the variation of the program within municipalities located in the same state. The state-by-year fixed effects control for common unobserved yearly shocks such as differences in the budget allocation of the FTS program (or in the total education budget) at the state level.

The main threat to our identification strategy is that the roll-out might be correlated with unobserved characteristics at the municipality level. For instance, the official guidelines of the program establish that priority should be given to vulnerable areas. If municipalities

[19] See INEE (2018c) for a more detailed description on the reform.

that have a higher coverage of FTS are simultaneously implementing other initiatives, which directly or indirectly affect the rate of children working, it would question the validity of our results. Thus, the main identifying assumption is that in the absence of the FTS program, changes in the child labor rate in municipalities with different FTS-coverage should have been similar.

To show that our results are not driven by unobserved factors correlated with the roll-out, we provide graphical evidence on pre-program trends as well as the results of alternative models. In the previous section, we show that before the national roll-out of the FTS program, the evolution of child labor was similar for municipalities with different coverage rates. We further show that the child labor rate at the municipality level at time t and $t-1$ is not a determinant of the share of FTS at time t, and that municipality characteristics are similar in municipalities with low- vs. high-FTS coverage.

In the results section we further rule out the existence of pre-trends by presenting the results by year. In addition, we show the results of alternative models: i) we estimate an IV approach and focus on the predicted share of FTS to estimate the effect, ii) we show the results of simple Difference-in-Difference (DiD) models dividing treatment and control municipalities according to the median of FTS coverage following Havnes and Mogstad (2011b); Blanden *et al.* (2016), and iii) we present the results of an event study design for the overall sample and by tercile of implementation. This model also allow us to rule out pre-existing trends and evaluate if the impacts are persistent several years after the municipality is treated. Finally, we conduct several robustness tests to show that the coefficients are stable to a number of alternative specifications of our baseline model, e.g., we exclude highly marginalized municipalities, municipalities part of the pilot FTS phase, and the top 5% implementing municipalities, and robust to correcting the results to possible biases in two-way fixed effects estimates (see e.g., Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021)

An additional concern of our empirical framework is that the model only allows us to estimate an *intention-to-treat* effect. Although we do not observe if a child attends a FTS or not, it is plausible to assume that the higher the share of FTS in the municipality the higher the likelihood that a child is part of the program. Intention-to-treat estimates represent a lower bound of the true treatment effect; however, we conduct several regressions that interact the treatment variable with demographic characteristics of the children to analyze the main drivers of the true effect.

The final concern is that with the introduction of the FTS program, children living in poor households might be changed from FTS to part-time schools to continue working as the school choice in Mexico is free. We cannot test this directly with the data at hand. However, as the FTS program increases coverage in a municipality, the choices of part-time schools decrease. Thus, the decision to change schools might be more costly for parents if e.g., the distance from the household to the part-time school choices increase. An alternative is that parents decide to pull their children out of school. However, we find no evidence that the FTS program leads to a higher probability to drop out. Alternatively, parents with a higher preference for schooling over work may decide to enroll their children in FTS schools (full-time) instead of the traditional ones (part-time). Indeed the results shown by Padilla-Romo (2022) show that the FTS program increases the probability to switch from a part-time to a full-time school. However the proportion of switchers is small (on average 5.6%) and mostly concentrated in urban areas and for students with a higher socioeconomic status. Students with a lower socioeconomic status seem not to systematically sort into part-time schools. Therefore, the concern that our results are biased due to switchers remains small.

3.5 Results

3.5.1 Baseline Results

3.5.1.1 Schooling and Child Labor

For the empirical analysis, we start by estimating the effect of an increase in the share of FTS on school enrollment and weekly hours spent on schooling activities following our main specification in Eq. 3.1. The direction of the effect of additional schooling hours on child labor is not clear a priori. On the one hand, lengthening the school day could increase the marginal returns to education, if education quality is improving or, if indirect schooling costs are decreasing (see e.g., Edmonds, 2007). Therefore, the parent would decide to increase schooling time and decrease the child's working or leisure time. On the other hand, lengthening the school day could put additional pressure on income-constrained families that rely more heavily on the child's work inside and outside the household (see e.g., Dammert *et al.*, 2018). If the child has less time to engage in productive activities due to longer school hours, the household income would be directly affected and the parent could decide to decrease schooling time, for example, through decreasing school enrollment or schooling time at home, and to increase the number of hours the child spends working.

The results are reported in Table 3.2. With respect to school enrollment, column I shows that an increase in the share of FTS from 0 to 1 (full coverage)[20], has no effect on the probability that a child is enrolled in school. The estimated coefficient is not statistically significant and close to zero. Using only the variation of municipalities located in the same state yields similar results (column II). Although school enrollment is almost universal and this avenue of adjustment could be exhausted, the main concern is that due to the program

[20] In 2017, only 5% of municipalities had more than 53% of schools covered by the FTS program.

parents parents who rely more on child work could decide to pull their children out of school. These results alleviate this concern.

With respect to schooling hours, columns III and IV of Table 3.2 show a positive and statistically significant effect of the share of FTS on the number of hours spent on schooling activities. Although this increase is not observed in the descriptive statistics, after controlling for municipality and year fixed effects, as well as individual characteristics, the results reveal that the weekly schooling hours for children in municipalities that went from having none to all schools operating on a full-time basis increases by 4.7 hours. The size of the coefficient is smaller than expected; the increase in schooling hours on a weekly basis should amount to 10-20 hours. However, our schooling time measure does not capture exclusively time spent in school, but also time spent on other schooling activities such as homework.

Table 3.2: Effect of FTS Program on School Enrollment

Dependent variable:	School enrollment		Schooling hours	
	I	II	III	IV
Share of FTS	−0.003	0.002	5.789***	4.609***
	(0.008)	(0.012)	(1.238)	(1.379)
Girl	0.003***	0.003***	0.347***	0.347***
	(0.001)	(0.001)	(0.071)	(0.069)
Receives gov. support	0.071***	0.071***	2.506***	2.531***
	(0.004)	(0.004)	(0.156)	(0.153)
Number of siblings	−0.007***	−0.007***	−0.428***	−0.417***
	(0.001)	(0.001)	(0.042)	(0.041)
Birth order Ref: First born				
Middle born	0.003*	0.003*	−0.000	0.012
	(0.002)	(0.002)	(0.097)	(0.097)
Last born	0.002**	0.002**	0.043	0.036
	(0.001)	(0.001)	(0.089)	(0.090)
Both parents present	−0.035***	−0.035***	−0.886***	−0.913***
	(0.006)	(0.006)	(0.304)	(0.301)
Mother education Ref: None				
Primary education	0.057***	0.057***	1.743***	1.766***
	(0.005)	(0.005)	(0.241)	(0.244)
Secondary education	0.079***	0.079***	2.751***	2.739***
	(0.006)	(0.006)	(0.258)	(0.259)
High-school	0.085***	0.085***	3.243***	3.256***
	(0.006)	(0.006)	(0.285)	(0.287)
Vocational training	0.088***	0.088***	3.410***	3.406***
	(0.006)	(0.006)	(0.340)	(0.338)
University degree	0.085***	0.085***	4.386***	4.365***
	(0.006)	(0.006)	(0.316)	(0.311)
Father education Ref: None/Father not present				
Primary education	0.032***	0.032***	0.899***	0.924***
	(0.006)	(0.006)	(0.283)	(0.275)
Secondary education	0.049***	0.049***	1.511***	1.518***
	(0.006)	(0.006)	(0.289)	(0.286)
High-school	0.051***	0.050***	1.638***	1.653***
	(0.006)	(0.006)	(0.311)	(0.312)
Vocational training	0.053***	0.053***	2.051***	2.071***
	(0.006)	(0.006)	(0.352)	(0.347)
University degree	0.051***	0.050***	2.278***	2.298***
	(0.006)	(0.006)	(0.346)	(0.341)
Locality size Ref: >100,000 inhabitants				
15,000-99,999 inhabitants	0.001	0.001	−0.394	−0.334
	(0.003)	(0.003)	(0.437)	(0.440)
2,500-14,999 inhabitants	−0.007***	−0.007***	−0.215	−0.269
	(0.002)	(0.002)	(0.454)	(0.459)
Less than 2,500 inhabitants	−0.020***	−0.020***	−0.915**	−0.942**
	(0.003)	(0.003)	(0.408)	(0.409)
Municipality characteristics				
Share age 7-17 out of school	−0.067***	−0.065***	−4.380**	−4.834**
	(0.023)	(0.023)	(2.195)	(1.912)
Share living in poverty	0.001	0.003	−0.302	0.326
	(0.009)	(0.010)	(1.082)	(1.069)
Share women in the LF	0.012	0.013	−0.965	−0.920
	(0.010)	(0.010)	(1.162)	(1.176)
Constant	0.808***	0.812***	32.261***	29.885***
	(0.020)	(0.023)	(1.211)	(1.448)
Birth cohort FE	yes	yes	yes	yes
State-by-year FE	no	yes	no	yes
Observations	230,256	230,256	230,256	230,256
R^2	0.116	0.117	0.134	0.152

*Notes: – Results are obtained from OLS regressions. – Standard errors in parentheses (clustered at the municipality level). All columns control for municipality FE. – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

3.5.1.2 Market and Domestic Work

As a second step, we investigate the effect of the program on child labor by aggregating both market and domestic work. Table 3.3 shows the results of the effect of the program on total hours worked (columns I and II), the extensive margin, i.e., a binary variable indicating if the child works (columns III and IV), and the intensive margin, i.e., hours worked conditional on working (columns V and VI). The results from our preferred specification (using the variation of municipalities within the same state) show that children in municipalities where the share of FTS increase from 0 to 1 (full coverage) experienced a reduction in the number of total hours worked by 1.9 hours. The reduction in the number of hours worked is mainly driven by the extensive and not by the intensive margin. Due to the FTS program, the probability that a child is working decreases by 6.6 percentage points. At the intensive margin, the coefficients are negative and are significant at the 10% level when we account for state-by-year fixed effects.[21]

The results suggest that children who worked few hours per week are less likely to work after the FTS program. To make the results consistent with the roll-out of the program, we consider a one standard deviation increase in the share of FTS, i.e., an increase of 14 percentage points in the share of FTS, which would translate into a decrease in the probability that children work by 0.9 percentage points, which is equivalent to a 12% reduction in child labor.

Further results from Table 3.2 and 3.3 worth mentioning are: girls have both a higher probability of being enrolled in school and spending more time in schooling activities, but they are also more likely to work. The birth rank is an important determinant of schooling and work.

[21]We estimate the baseline specification using non-linear models and the results confirm that the effect is driven by the extensive margin. The effect for the extensive margin is similar in terms of magnitude and significant at the 10% level. See Table 3.A4 in the Appendix.

Table 3.3: EFFECT OF FTS PROGRAM ON CHILD LABOR

Dependent variable:	**Total hours worked**		**Extensive margin**		**Intensive margin**	
	I	II	III	IV	V	VI
Share of FTS	−1.311***	−1.868***	−0.057***	−0.066***	−1.708	−3.615*
	(0.468)	(0.584)	(0.021)	(0.024)	(1.414)	(1.934)
Girl	1.223***	1.225***	0.032***	0.032***	0.108	0.119
	(0.047)	(0.047)	(0.002)	(0.002)	(0.203)	(0.199)
Receives gov. support	−0.793***	−0.785***	−0.015***	−0.015***	−3.759***	−3.803***
	(0.096)	(0.095)	(0.003)	(0.003)	(0.275)	(0.273)
Number of siblings	0.254***	0.254***	0.011***	0.011***	0.523***	0.513***
	(0.027)	(0.027)	(0.001)	(0.001)	(0.093)	(0.093)
Birth order Ref: First born						
Middle born	−0.598***	−0.603***	−0.024***	−0.024***	−0.924***	−0.885***
	(0.065)	(0.065)	(0.003)	(0.003)	(0.251)	(0.252)
Last born	−0.922***	−0.923***	−0.035***	−0.035***	−1.679***	−1.661***
	(0.055)	(0.055)	(0.003)	(0.003)	(0.264)	(0.265)
Both parents present	0.315	0.313	0.018**	0.018**	−0.332	−0.345
	(0.211)	(0.210)	(0.009)	(0.009)	(0.544)	(0.546)
Mother education Ref: None						
Primary education	−0.892***	−0.864***	−0.025***	−0.024***	−1.542***	−1.535***
	(0.183)	(0.179)	(0.007)	(0.007)	(0.487)	(0.487)
Secondary education	−1.371***	−1.345***	−0.036***	−0.035***	−3.143***	−3.131***
	(0.191)	(0.188)	(0.008)	(0.008)	(0.513)	(0.507)
High-school	−1.673***	−1.647***	−0.042***	−0.042***	−3.939***	−3.887***
	(0.197)	(0.194)	(0.008)	(0.008)	(0.585)	(0.574)
Vocational training	−1.890***	−1.860***	−0.059***	−0.058***	−3.918***	−3.888***
	(0.203)	(0.200)	(0.009)	(0.009)	(0.612)	(0.612)
University degree	−2.116***	−2.087***	−0.064***	−0.063***	−4.099***	−4.021***
	(0.206)	(0.202)	(0.008)	(0.008)	(0.654)	(0.650)
Father education Ref: None/Father not present						
Primary education	−0.621***	−0.607***	−0.018**	−0.018**	−1.107**	−1.098**
	(0.204)	(0.203)	(0.008)	(0.008)	(0.499)	(0.502)
Secondary education	−1.007***	−1.001***	−0.037***	−0.037***	−2.166***	−2.102***
	(0.202)	(0.201)	(0.008)	(0.008)	(0.524)	(0.527)
High-school	−1.095***	−1.097***	−0.046***	−0.046***	−2.525***	−2.517***
	(0.207)	(0.206)	(0.009)	(0.009)	(0.577)	(0.576)
Vocational training	−1.090***	−1.085***	−0.046***	−0.046***	−1.468*	−1.386*
	(0.248)	(0.248)	(0.010)	(0.010)	(0.807)	(0.811)
University degree	−1.602***	−1.587***	−0.065***	−0.064***	−2.528***	−2.584***
	(0.217)	(0.215)	(0.009)	(0.009)	(0.644)	(0.644)
Locality size Ref: >100,000 inhabitants						
15,000-99,999 inhabitants	0.258*	0.280*	0.005	0.006	1.750***	1.798***
	(0.150)	(0.149)	(0.007)	(0.007)	(0.629)	(0.621)
2,500-14,999 inhabitants	0.268*	0.282**	0.008	0.008	0.994**	0.991**
	(0.140)	(0.141)	(0.006)	(0.006)	(0.503)	(0.493)
Less than 2,500 inhabitants	0.758***	0.761***	0.032***	0.032***	1.339***	1.407***
	(0.137)	(0.136)	(0.006)	(0.006)	(0.471)	(0.465)
Municipality characteristics						
Share age 7-17 out of school	1.012	0.697	0.009	−0.006	6.068**	5.418**
	(0.925)	(0.911)	(0.039)	(0.038)	(2.825)	(2.590)
Share living in poverty	0.475	0.605	0.026	0.035	1.814	1.547
	(0.500)	(0.487)	(0.023)	(0.023)	(1.533)	(1.487)
Share women in the LF	1.000**	0.815*	0.014	0.004	3.524**	3.279**
	(0.490)	(0.469)	(0.020)	(0.020)	(1.547)	(1.484)
Constant	11.988***	11.814***	0.380***	0.376***	22.168***	21.569***
	(0.703)	(0.785)	(0.031)	(0.036)	(1.599)	(1.979)
Birth cohort FE	yes	yes	yes	yes	yes	yes
State-by-year FE	no	yes	no	yes	no	yes
Observations	230,256	230,256	230,256	230,256	30,595	30,595
R^2	0.221	0.224	0.146	0.148	0.231	0.240

*Notes: – Results are obtained from OLS regressions. – Standard errors in parentheses (clustered at the municipality level). All columns control for municipality FE. – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Therefore, compared to first-born children, middle- and last-born children are more likely to be enrolled in school and spend more time in schooling activities. Similar to the results in Dammert (2010), we find that middle- and last-born children are less likely to work and conditional on working, they work fewer hours than the first-born child. Parental education also plays an important role, which is consistent with the literature on parental intergenerational transmission of schooling; i.e., higher levels of parental education increase school enrollment, schooling time, and decrease work at the extensive and intensive margins (see e.g., Pronzato, 2012; Lundborg *et al.*, 2018). Finally, compared to urban localities, children living in rural localities are less likely to go to school, are more likely to work, and work more hours.

Table 3.4: Effect of FTS Program on Market and Domestic Work

Dependent variable:	**Total hours worked**		**Extensive margin**		**Intensive margin**	
	I	II	III	IV	V	VI
A. Market work						
Share of FTS	-0.607^{**}	-0.935^{***}	-0.048^{***}	-0.050^{***}	0.146	-4.258
	(0.252)	(0.351)	(0.015)	(0.018)	(2.895)	(3.389)
Observations	230,256	230,256	230,256	230,256	12,651	12,651
B. Domestic work						
Share of FTS	-0.704^{*}	-0.932^{**}	-0.023	-0.032	-1.090	-1.158
	(0.372)	(0.475)	(0.017)	(0.020)	(1.129)	(1.534)
Observations	230,256	230,256	230,256	230,256	19,609	19,609
State-by-year FE	no	yes	no	yes	no	yes

*Notes: – Results are obtained from OLS regressions. – Standard errors in parentheses (clustered at the municipality level). The regressions include the full set of control variables, age dummies, and municipality FE. – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Next, we look at market (panel A) and domestic work (panel B) and estimate the impact of the FTS program on each type of work separately. Table 3.4 presents the results for total hours worked (column I and II), the extensive (column III and IV) and the intensive (column V and VI) margins, and reveals that the baseline coefficients are mainly driven by a reduction in market work. In municipalities where the share of FTS increased from 0 to

1, children decrease time spent on market work by almost 1 hour. Similar to the baseline estimates, this reduction is mainly driven by the extensive margin with a decrease in the probability of engaging in market work by 5 percentage points. Turning to domestic work, the estimated coefficients are negative in all columns; however, they are only statistically significant for total hours worked (columns I and II).

3.5.1.3 Impacts by Year: Individual and Municipality Level

We estimate the baseline model including an interaction term between the share of FTS and year dummies using the data at the individual level and at the municipality level. Figure 3.4 reports the results at the individual level and Figure 3.5 reports the results aggregating the data at the municipality level. For the latter, we calculate the average school enrollment rate, child labor rate, and hours worked at the municipality level by year. We run fixed effects regressions controlling for the interaction of the share of FTS with yearly dummies, municipality fixed effects, municipality time varying characteristics, and state-by-year fixed effects.

This approach allows us to evaluate pre-existing differences before the roll-out of the program. In both figures, we observe that the point estimates for 2009 and 2011 are not statistically significant. The results on school enrollment rates are very close to zero and not statistically significant in all years. The results for the child labor indicators show that the impact kicks-in after 2011, which corresponds to the national roll-out of the program.

The figures show negative point estimates for the total hours worked, the extensive, and intensive margins. The point estimates become more negative in 2013 and 2015 which is consistent with Figure 3.3 showing that the share of FTS increased the most during this period. The size of the coefficients is also in the same range of our baseline estimates. Due to the coarser aggregation level, the standard errors are larger when estimating the model at the municipality level. The Figures also show that in 2017, the impact of the FTS program vanishes. As previously shown, implementation of the program slowed down considerably after 2015, which could explain why the effect is not significant in 2017. To explore whether the impact truly vanishes after several years of implementing the program at the municipality level, we conduct an event study design in the robustness section. The event study shows that the effects are persistent several years after the municipality joins the program and are particularly strong for municipalities implementing the FTS program at a higher intensity.

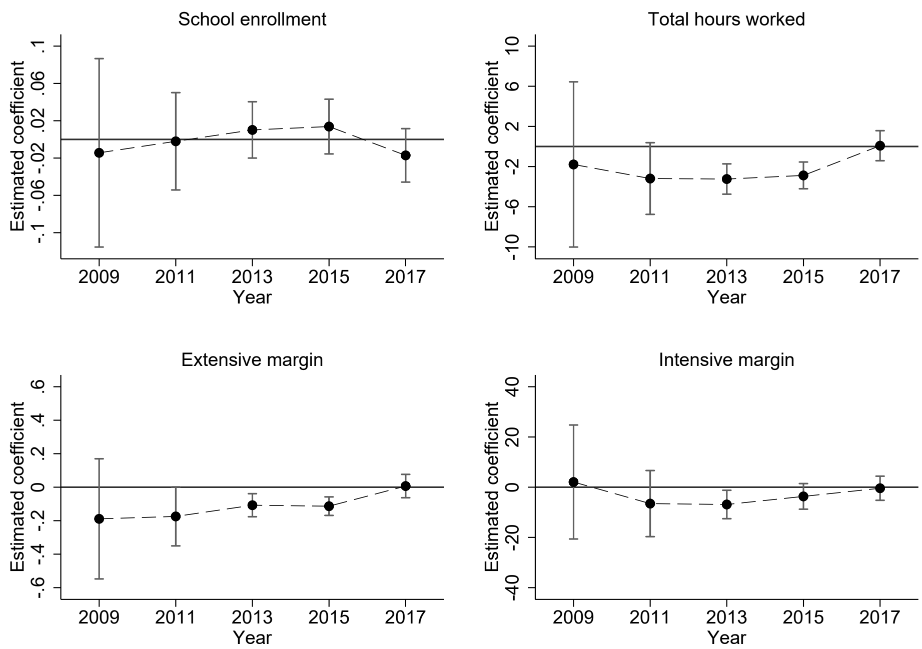

Figure 3.4: Effect of the Share of FTS by Year: Individual Level Estimates
Source: ENOE – Módulo de Trabajo Infantil (MTI), authors' analysis.
Notes: – The figure shows the point estimates and confidence intervals at the 95% level of regressions. The regressions include the interaction of the share of FTS with year dummies, as well as the full set of control variables and fixed effects. The standard errors are clustered at the municipality level.

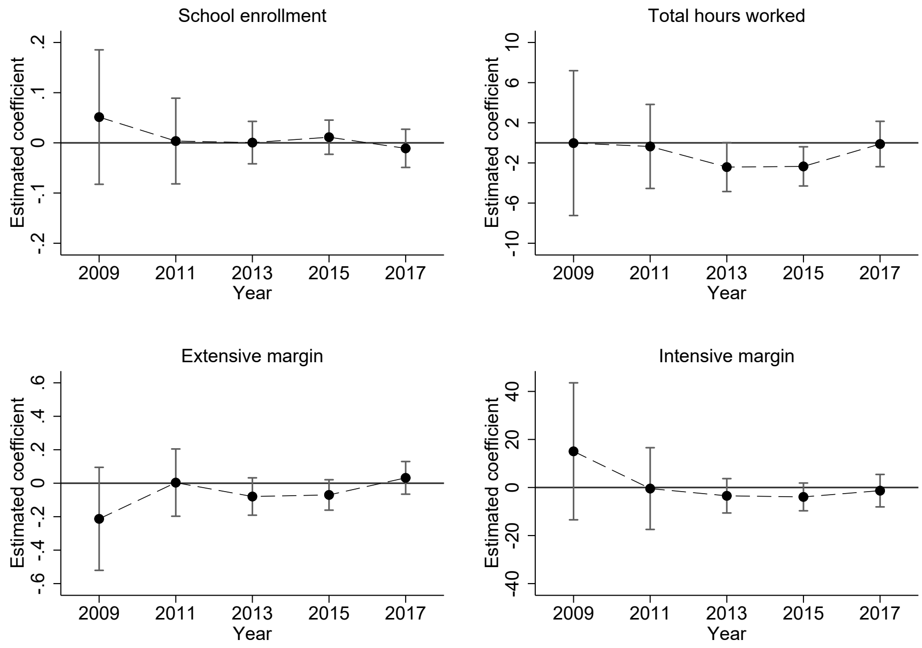

Figure 3.5: Effect of the Share of FTS by Year: Municipality Level Estimates

Source: ENOE – Módulo de Trabajo Infantil (MTI), authors' analysis.

Notes: – The figure shows the point estimates and confidence intervals at the 95% level of regressions using data at the municipality level. The regressions control for the interaction of the share of FTS with year dummies, municipality time varying characteristics, municipality fixed effects, and state-by-year fixed effects. The standard errors are clustered at the municipality level.

3.5.2 Heterogeneous Effects

To investigate gender differentials by type of work, we interact the share of FTS with a gender dummy. Table 3.5 reports the coefficients for the total hours worked (panel A), the extensive margin (panel B), and the intensive margin (panel C).

In column I, we focus on our aggregate definition of work, in column II on market work, and column III on domestic work. The results (panel A, column I) show a similar pattern as in the baseline results: for boys, we observe a decrease in total hours worked by 1.5 hours, which is driven by a reduction in the probability to work by 6 percentage points and a decrease in conditional hours worked by 4 hours, but the latter is only significant at the

10% level. In this column, we find a stronger reduction in total hours worked for girls in comparison to boys.

Table 3.5: Effect of FTS Program by Gender

	Any work	Market work	Domestic work
	I	II	III
	A. Total hours worked		
Share of FTS	-1.517^{**}	-1.365^{***}	-0.151
	(0.596)	(0.379)	(0.494)
Girl	1.301^{***}	-0.771^{***}	2.072^{***}
	(0.061)	(0.057)	(0.063)
Girl x Share of FTS	-0.713^{***}	0.873^{***}	-1.586^{***}
	(0.270)	(0.219)	(0.257)
Observations	230,256	230,256	230,256
	B. Extensive margin		
Share of FTS	-0.060^{**}	-0.071^{***}	-0.002
	(0.025)	(0.019)	(0.021)
Girl	0.034^{***}	-0.040^{***}	0.078^{***}
	(0.003)	(0.003)	(0.003)
Girl x Share of FTS	-0.012	0.043^{***}	-0.062^{***}
	(0.014)	(0.012)	(0.013)
Observations	230,256	230,256	230,256
	C. Intensive margin		
Share of FTS	-3.978^{*}	-4.258	-1.205
	(2.081)	(3.547)	(1.793)
Girl	0.060	-1.029^{**}	1.147^{***}
	(0.254)	(0.425)	(0.227)
Girl x Share of FTS	0.650	0.000	0.066
	(1.294)	(2.179)	(1.048)
Observations	30,595	12,651	19,609

*Notes: – Results are obtained from OLS regressions. – Standard errors in parentheses (clustered at the municipality level). The regressions include the full set of control variables, municipality and state-by-year fixed effects. – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

We also find significant differences for boys and girls when looking at market and domestic work separately. The gender dummy reveals that compared to boys, girls spend less hours in market work and more hours in domestic work. This is true not only for total weekly hours worked, but also for the extensive and intensive margins. The interaction term shows that due to the FTS program boys are 7.1 percentage points less likely to engage in market work, while for girls the effect is about half of the size (2.8). In contrast, girls are 6.2 percentage points less likely to participate in excessive domestic work, while for boys the coefficient is close to zero and not statistically significant.[22] Our results are consistent with previous findings related to CCTs which find a stronger reduction in market work for boys (de Hoop and Rosati, 2014; Skoufias *et al.*, 2001; Ferreira *et al.*, 2009; Galiani and McEwan, 2013) and in domestic work for girls (Corona and Gammage, 2017).

We explore other sources of heterogeneity by interacting the share of FTS with a dummy variable indicating: (i) if the child resides in a rural or urban area, (ii) household income quintile, (iii) household poverty level, (iv) the child's age group, and (v) the child's birth rank. The results are reported in Table 3.6.

The results in panel A show that the estimated coefficient is larger for rural areas, however the difference is not statistically significant. This may seem surprising, but child labor is not only concentrated in the rural sector in Mexico. In fact, the MTI data shows that out of all children who are working 33% reside in urban areas.[23]

To measure the impact by poverty level, we construct two indicators in Panel B and C. For the first definition, we interact the share of FTS with a categorical variable indicating the household income per person in quintiles (panel B). The second definition, is based on a categorical variable indicating if the family lives in extreme poverty, moderate poverty, or is above the poverty line (panel C).[24]

[22]The results are similar if we estimate the regressions for the sample of boys and girls separately.

[23]45% of children who work and reside in urban areas report trade as their main activity, followed by 21% who work in restaurants, 17% provide services, 10% work in manufactures, and 5% in construction, the rest report other activities.

[24]This variable is a more precise measure for the household income indicating if it is below the basic basket of goods including only food items (extreme poverty), if the household income is below the basic basket of goods including food and non-food items (moderate poverty), or if the household income is above the basic basket of goods including food and non-food items. We use information the yearly average costs of the basket for rural and urban areas provided by provided by the CONEVAL (2019).

Table 3.6: Heterogeneous Effects of the FTS Program on Child Labor

Dependent variable:	Total hours	Ext. margin	Int. margin
	I	II	III
	A. Rural		
Share of FTS	-1.551^{**}	-0.055^{*}	-2.593
	(0.666)	(0.030)	(2.386)
Rural x Share of FTS	-0.477	-0.017	-1.248
	(0.503)	(0.024)	(1.919)
Observations	230,256	230,256	30,595
	B. HH income quintile		
Share of FTS	-1.521^{**}	-0.052^{*}	-2.715
	(0.726)	(0.029)	(2.418)
2nd x Share of FTS	-0.375	0.007	0.768
	(0.533)	(0.029)	(1.900)
3nd x Share of FTS	-0.877	-0.023	-2.852
	(0.534)	(0.024)	(1.993)
4th x Share of FTS	-1.071^{*}	-0.060^{**}	-1.421
	(0.555)	(0.024)	(2.153)
5th x Share of FTS	-0.753	-0.037	-2.767
	(0.523)	(0.024)	(2.172)
Observations	208,720	208,720	29,171
	C. Household poverty level		
Share of FTS	-2.460^{***}	-0.086^{***}	-5.773^{***}
	(0.643)	(0.028)	(2.221)
Poverty x Share of FTS	0.387	0.011	2.506
	(0.377)	(0.019)	(1.709)
Extreme poverty x Share of FTS	0.687^{*}	0.033	3.208^{**}
	(0.399)	(0.022)	(1.625)
Observations	208,720	208,720	29,171
	D. Age group		
Share of FTS	-0.652	-0.032	1.967
	(0.690)	(0.029)	(3.417)
Age: 9-10 x Share of FTS	-0.438	-0.002	-0.884
	(0.277)	(0.017)	(2.758)
Age: 11-12 x Share of FTS	-1.533^{***}	-0.052^{**}	-6.585^{**}
	(0.462)	(0.024)	(2.999)
Age: 13-14 x Share of FTS	-2.902^{***}	-0.083^{**}	-6.833^{**}
	(0.867)	(0.037)	(3.099)
Observations	230,256	230,256	30,595
	E. Birth order		
Share of FTS	-2.078^{***}	-0.074^{***}	-3.938^{*}
	(0.594)	(0.026)	(2.025)
Middle born x Share of FTS	0.105	-0.000	0.869
	(0.355)	(0.016)	(1.367)
Last born x Share of FTS	0.443	0.020	0.083
	(0.331)	(0.019)	(1.518)
Observations	230,256	230,256	30,595

*Notes: – Results are obtained from OLS regressions. – Standard errors in parentheses (clustered at the municipality level). The regressions include the full set of control variables, municipality and state-by-year fixed effects. – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

The results show a general decline in child labor across all income quintiles. The coefficients turn more negative as the income quintile increases, suggesting that there is a smaller impact for children at the bottom of the income distribution. However, for most of the quintiles the differences are not statistically significant.

We find significant differences when focusing on the more precise definition for poverty measure at the household level i.e., a definition of poverty focusing on the lowest income quintiles that establishes slightly different poverty thresholds for rural and urban areas. The results show that although there is a decrease in child labor for all groups, but the decrease is smaller for children who are living in extreme poverty (columns I and III).

These results indicate that children above the poverty line are the ones who can afford to work less and suggest that a "wealth paradox" exists. Child labor is not only present for families in extreme poverty and there is a non-linear relationship between child work and economic status of the household (Bhalotra and Heady, 2003; Edmonds, 2005; Basu *et al.*, 2010). We argue that the decrease in child labor is smaller for households with higher poverty levels because they rely more on the work from all family members to cover their subsistence needs. For these families, poverty alleviation programs such as CCTs are effective in decreasing child labor because they address income and credit constrains (de Hoop and Rosati, 2014). The FTS program, however, has a larger impact for households which are less income-constrained because they are better able to substitute the child's work with labor from other household members.

We also find significant differences when looking at different age groups (panel D). The table shows that compared to children aged 7-8, the effect is larger for older children and this is true for all outcome variables. These results are in line with the descriptive evidence provided in Figure 3.1, which shows that child labor rates are higher for older children, and that older children experienced a larger reduction in the rate from 2011 to 2017. The results show that the effect is mainly driven by children in the age groups 11-12 and 13-14, who experienced a reduction in the probability to work and in conditional hours worked. This result can be explained by the fact that the likelihood to work increases with age. The data shows 3% of children aged 7 are engaged in domestic or market work and this share increases to 29.2% by age 14. The load of work also increases with age. At age 7, children work on average 13.7 hours per week and at age 14, 22.2 hours. Therefore, the results capture a significant decrease in child labor for the group of children who are more likely to work and have a heavier work load. Finally, we find no significant differences when looking at the birth order (panel E).

3.5.3 Robustness Tests

In this subsection we address the concern that the roll-out of the program is not random, we do so by implementing alternative models to take into account the unobserved heterogeneity. As a first step, we start by using an IV approach to predict the share of FTS. We instrument the share of FTS using the interaction of the share of eligible schools at the municipality level with the allocated yearly budget for the program at the state level. Both the state level budget and the share of eligible schools are important predictors of the roll-out and should be uncorrelated with individual outcomes.

The first-stage results, reported in Table 3.A5 in the Appendix, show that both the share of eligible schools and the budget are positively correlated with the share of FTS, but only the latter is statistically significant. The interaction term is negative, which implies that with a larger share of eligible schools the effect of the budget on the share of FTS becomes smaller. We estimate the marginal effects and plot them in Figure 3.A2. This figure shows that the higher the budget at the state level the higher the share of FTS; however, these effects are smaller in areas with a higher share of eligible schools. While this relationship may seem counter-intuitive, a simple explanation is that the share of eligible schools decreases as additional schools are covered by the program i.e., there is a strong negative raw correlation between these two variables. Therefore, by construction the budget will have stronger effects in places where the share of eligible schools is smaller because these places have a larger proportion of FTS covered by the program. The results of the second-stage are reported in panel A of Table 3.7 and are in line with the results in the baseline specifications.

Table 3.7: EFFECT OF THE SHARE OF FTS ON CHILD LABOR: ALTERNATIVE SPECIFICATIONS

Dependent variable:	School enroll.	Total hours	Ext. margin	Int. margin
	I	II	III	IV
	A. Predicted share of FTS			
Pred. share FTS	0.000	−0.898**	−0.045**	−0.811
	(0.007)	(0.435)	(0.018)	(1.136)
Observations	230,256	230,256	230,256	30,595
	B. Simple Diff-in-Diff			
Above median x Post 2012	−0.003	−0.365**	−0.018**	−0.381
	(0.004)	(0.185)	(0.008)	(0.614)
Observations	230,256	230,256	230,256	30,595

*Notes: – Results are obtained from OLS regressions. – Standard errors in parentheses (clustered at the municipality level). All regressions include the full set of control variables, municipality and state-by-year FE. – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Second, we estimate a simple DiD model following Havnes and Mogstad (2011b) and (Blanden *et al.*, 2016) in Panel B. We define treatment and control municipalities according to the median of the roll-out. The treatment variable takes the value 1 if the municipality had a share of FTS equal or higher than the median during the school year 2017/2018. The policy variable takes the value 1 after the national roll-out in 2013. The results confirm our baseline results and indicate that in treated municipalities child labor rates decreased more after 2012 in comparison to control municipalities.

Third, we estimate an event study design using a similar approach as in Fischer and Argyle (2018) and define the control group as municipalities with a very small share of schools covered by the program.

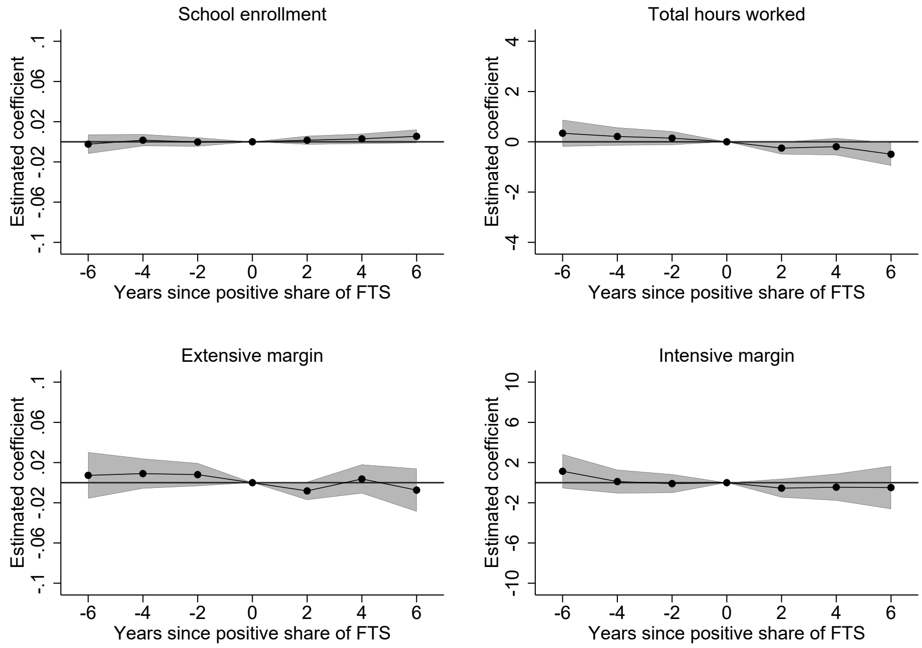

Figure 3.6: Event Study: Impact of FTS Program on Schooling and Child Labor

Source: ENOE – Módulo de Trabajo Infantil (MTI), authors' analysis.

Notes: – The set up of the event study design uses the time since the municipality exceeded the 25th percentile of average coverage i.e., 8% of schools in the municipality are covered by the program. The regression includes the full set of controls and fixed effects. The area in grey represents the confidence interval at a 95% level. The standard errors are clustered at the municipality level.

In this case, we cannot use zero FTS as the threshold as most municipalities in our sample have at least one school covered by the program. Thus, we define time zero (in event time) as the first year when the municipality exceeds a coverage above the 25th percentile i.e., about 8% of schools in the municipality are part of the program.[25] Figure 3.6 reports the results. In general, the estimated coefficients are close to zero and not statistically significant. However,

[25] These results are also robust to taking a lower threshold of implementation into account such as the 10th percentile and are available upon request.

as this design does not take into account differences in implementation at the municipality level, we further show the results dividing the sample by tercile of implementation using the share of FTS in 2017. Figure 3.7 shows the results focusing on total hours worked and the extensive margin.

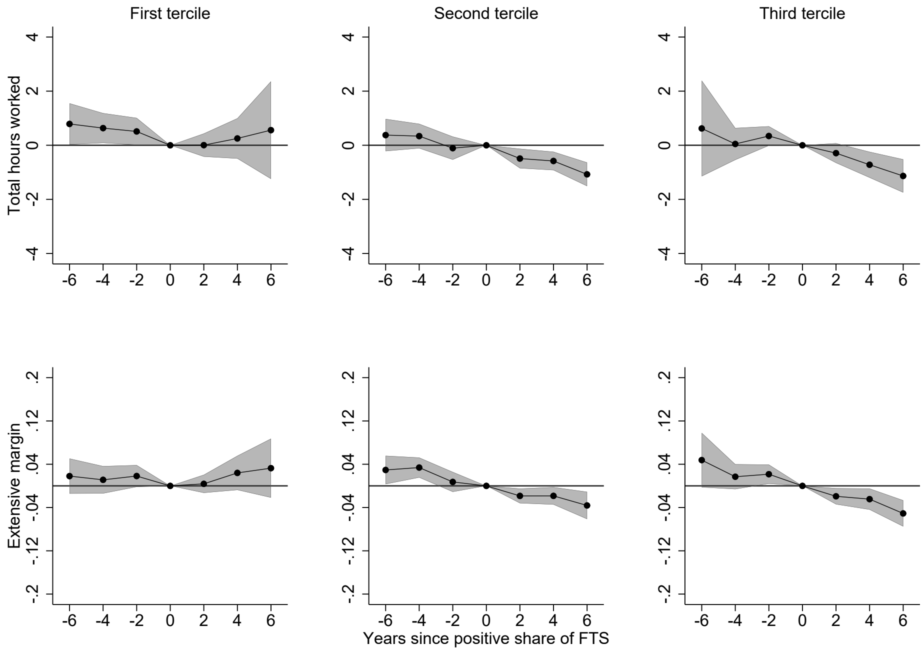

Figure 3.7: Event Study by Tercile: The Impact of FTS Program on Schooling and Child Labor

Source: ENOE – Módulo de Trabajo Infantil (MTI), authors' analysis.

Notes: – The set up of the event study design uses the time since the municipality exceeded the 25th percentile of average coverage i.e., 8% of schools in the municipality are covered by the program. The results are provided by tercile based on the coverage of the program in 2017. The regression includes the full set of controls and fixed effects. The area in grey represents the confidence interval at a 95% level. The standard errors are clustered at the municipality level.

The figure shows no significant differences for individuals living in municipalities in the first tercile i.e., municipalities with the smallest coverage of the program. For individuals residing in municipalities classified in the second and third terciles, we observe a decrease in total hours worked and in the extensive margin. These effects are persistent several years after the municipality is first classified as treated. These figures further support that parallel trends exist as most point estimates during the pre-treatment periods are not statistically significant.

Finally, this design allows us to test if our results are robust to possible biases in two-way fixed effects models with heterogeneous treatment effects (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021). In order to avoid our results to be driven by forbidden comparisons, we further restrict the control group to never-treated municipalities. We estimate the model using the framework suggested by Callaway and Sant'Anna (2021) and report the results in Figure 3.A3 in the Appendix. The figure shows no significant differences in the pre-treatment periods, a small and statistically significant increase in school enrollment, and a decrease in hours worked in the post-treatment periods. We find no statistically significant differences in the extensive and intensive margins, which could be explained by the number of observations that are dropped from the sample to restrict the comparison to never-treated municipalities. However, the strong post-treatment decrease in hours worked goes in line with the baseline results.

In addition to testing different models, we further show in Table 3.A6 in the Appendix whether the baseline results are sensitive to a number of alternative specifications. In panel A, we show that the baseline results are not driven by our choice of control variables at the individual level. For this specification, we exclude all individual control variables i.e., child and parental characteristics, and control only for municipality and state-by-year fixed effects. Since, Figure 3.2 shows that during 2017 the roll-out of the FTS program slowed down and the share of FTS remained at a similar level as in 2015, we estimate in panel B, the impact of the FTS program for the period 2011-2015. The results remain robust after excluding information from 2017.

In panel C, we show that the results are robust to the inclusion of a state specific linear time trend. This specification accounts for diverging trends in child labor at the state level due to e.g., changes in economic circumstances or state-level policies that could indirectly impact child labor. This addresses the concern that the drop in child labor rate observed in Figure 3.1 is driven by pre-existing trends at the state level.

Our baseline specification includes both primary and secondary schools. As some secondary schools offered a slightly longer school-day than primary schools, before the official roll-out of the program, we estimate a robustness test focusing only on primary schools and primary-aged children. In this case, the roll-out variable indicates the share of full-time primary schools at the municipality level (out of all primary schools in the municipality). The results reported in panel D, show that our results are robust.

In panels E, F, and G, we restrict the sample and exclude certain municipalities to show that they are not driving the results. In panel F, we exclude municipalities where the share of FTS was very large during the piloting phase. To do so, we identify municipalities where the share of FTS exceeded the median (12%) before 2012 and excluded them from the analysis. In panel F, we exclude the top 5% implementing municipalities to show that they are not driving the results. In this case, the municipalities dropped from the sample had more than 53% of schools covered by the program. Finally in panel F, we exclude highly-marginalized municipalities as it is likely that specific poverty-reduction programs are implemented in these municipalities. To identify highly-marginalized municipalities we use the CONEVAL data for 2010 and exclude municipalities where the marginalization index scored very-high and high. Our results are robust to the exclusion of certain municipalities.

Finally, in panel H, we show that the baseline results are also robust to an alternative definition of the program. Instead of focusing on the number of schools that participate in the program at the municipality level, we focus on the share of students enrolled in FTS by municipality and school calendar year. This definition captures capacity at the municipality level as it reflects the number of full-time seats available by school calendar year. Using the number of students covered instead of the schools covered yields similar results.

3.5.4 Mechanisms

A potential mechanism to consider is the highly subsidized meal provided by schools operating on an eight-hour basis. Access to a school meal results in lower schooling costs because meals are an implicit subsidy to the parents. In addition, school lunches can increase the returns to education because they foster learning via access to better nutrition (see e.g., Bhattacharya *et al.*, 2006; Jayaraman and Simroth, 2015). On average for the school calendar year 2017/2018, 53% of FTS operate on an eight-hour basis. We cannot directly test the effect of children who have access to the subsidized meal due to data limitations, but we know the share of schools at the municipality level that are part of the program and offer the food service. To explore whether the results are driven by access to eight-hour schools, we estimate the baseline model controlling for the share of eight-hour schools out of the total FTS at the municipality level ($FTS_8/Total\,FTS$). The data shows that during the school year 2017/2018 out of the total number of FTS, 52% offered a warm meal. This share is similar in rural (52%) and urban (53%) areas..

Table 3.A7 in the Appendix shows that the coefficients for the share of eight-hour schools are negative and statistically significant in panel A and B. The estimated coefficient for the share of FTS at the municipality level is larger in magnitude.[26] The results combined suggest that the additional time spent in school is the main driver of the reduction in child labor. However, the subsidized meal is also partially leading to a more negative effect. In addition, the meal could be indirectly related to keeping enrollment rates constant, due to lower schooling costs and increasing returns to education.[27]

[26]The number of observations differs from the baseline specification because the data stating if the school operates on a six or eight-hour basis is only available starting 2012.

[27]We find no significant impact on school attendance. The results are available upon request.

We further test if the FTS program led to changes in labor market outcomes of other household members. The program could indirectly affect the labor supply of other family members through two different channels. If the household depends on the income the child produces, other household members might need to increase their labor supply to compensate for the income loss by entering the labor market or by increasing the hours worked (see e.g. Manacorda, 2006). This would be an important concern if the reduction in child labor for younger siblings results in an increase in the probability to work for older siblings who are still underage. This would mean that child labor rates did not decrease, but just shifted to the slightly older groups. Alternatively, a longer school day could be an indirect subsidy to childcare which simultaneously would decrease the costs of employment of other household members e.g., a schooling day which is more compatible with the workday can lead to an increase in labor force participation of mothers, specially those with young children (Contreras and Sepúlveda, 2016).

We analyze the effect of the FTS on labor force outcomes of other household members using a similar approach as in Padilla-Romo and Cabrera-Hernández (2019).[28] However, we deviate from their work by looking not only at parental labor supply, but at the labor supply of older siblings. In addition, since spending more time at school provides an indirect subsidy for child care, that is, less need for childcare within the household (Dammert, 2010), we also focus on time spent on domestic activities for these household members. For the sample of older siblings, we focus on individuals who are in the 15-18 age range and who are not enrolled in basic education, to make sure they are not directly affected by the program. We restrict the sample to individuals younger than 18, because 18 is the age of legal adulthood in Mexico.

Although we cannot use variation at the individual level when focusing on child labor, the ENOE database allows us to build a panel and estimate the effect of the program using

[28]The authors use a difference model instead of a fixed effects model.

within-individual variation for household members older than 15. We compare individual outcomes from the first and fifth round of the survey i.e., the first and last time individuals are surveyed. We focus on this yearly measure because the share of FTS varies only once (at the start of the school year) for each individual. We estimate the following model:

$$Y_{imt} = \kappa + \delta FTS_{mt} + \eta' \mathbf{Z}_{imt} + \lambda_i + \tau_{st} + \upsilon_{imt} \tag{3.2}$$

where Y_{imt}, is the labor outcome of parent (sibling) i in municipality m at school year t. The main outcomes we look at are: a binary variable indicating if the individual is active in the labor force (column I), a continuous variable indicating the total weekly hours worked (column II), and the total weekly hours spent on domestic activities (column III) presented in Table 3.8 respectively. To avoid outliers we recode the top 1% of total hours worked and spent in domestic work as missing. Similar as before, δ is the effect of the program on the labor market outcomes of the individual. Z_{imt} is a vector of individual time varying characteristics such as age, age of the youngest child (sibling) in the household, and their respective squared terms. λ_i captures individual fixed effects, τ_{st} captures state-by-year-by-quarter fixed effects, and υ_{imt} the error term. We cluster the standard errors at the municipality level.

In addition, we estimate the impact of the program for parents (siblings) who are not living with a child younger than 14 as a placebo test. In the specific case of siblings, we use the same restrictions mentioned above. The results are reported in Table 3.A9 in the Appendix.

Looking at the response of parents to the program, Table 3.8 (Panels A and B) report the estimated coefficients for mothers and fathers. For mothers, we find a positive and significant effect on the likelihood of being active in the labor force of 7.7 percentage points. This is in line with the results in Padilla-Romo and Cabrera-Hernández (2019). While the authors find a positive and significant effect for weekly hours worked, our estimates are positive but

Table 3.8: Effect of FTS Program on Household Members: Child Aged 7-14 Lives in the Household

Dependent variable:	LFP	Market work	Domestic work
	I	II	III
		A. Mothers	
Share of FTS	0.077***	1.228	−0.119
	(0.029)	(1.174)	(1.255)
Observations	322,752	322,752	322,752
		B. Fathers	
Share of FTS	0.020	0.843	1.205*
	(0.015)	(1.479)	(0.681)
Observations	265,409	265,409	265,409
		C. Older siblings	
Share of FTS	0.026	0.868	−0.027
	(0.017)	(0.644)	(0.277)
Observations	125,267	125,267	125,267

*Notes: – Results are obtained from fixed-effects regressions. – Standard errors in parentheses (clustered at the individual level). The regressions control for age, age squared, age of youngest hh member, age of youngest hh member squared, individual and state-by-year FE. – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

not significant, which could be explained by differences in the sample composition.[29] For fathers, we observe no significant impact on labor force participation and hours worked, but a small increase in the number of hours spent on domestic activities, which could be explained by mothers taking up more work outside the household.

Looking at the response of siblings to the program, Table 3.8 (Panel C) reports the estimated coefficients. We find no significant effect on the likelihood of being active in the labor force nor on the number of hours worked. This finding further supports a reduction in child labor rates and not a shift in the supply of work from younger individuals to slightly older individuals who have not reached adulthood. Finally, the results of the placebo

[29] Padilla-Romo and Cabrera-Hernández (2019) focus on a different time period (2005-2016) and on full-time primary schools for their analysis.

regression in Table 3.A9 in the Appendix show no significant effects neither for mothers, fathers, nor for older siblings.

Combined with the results from the previous section, we conclude that it is plausible that the increase in the labor force participation of mothers is not exclusively driven by the increased subsidy to child care provided by the extension of the school day, but also through an income effect to compensate the decrease in child labor. Although we do not observe directly households where children stop working in a panel format, we show two different approaches to validate our argument. We first show that the increase in labor force participation is driven by mothers from a disadvantaged background i.e., income constrained families. Figure 3.A4 in the Appendix shows the effect of the FTS program on LFP of mothers by i) household income quintile and ii) by education level of the mother. The figures show that the increase in labor force participation is driven by mothers in the lowest income quintile. We also find significant impacts for mothers in the third income quintile. However, the results looking at the education levels support the claim that mothers from more disadvantaged backgrounds increase more their LFP. In this case, mothers with low education levels i.e., primary education drives the increase in LFP.

In addition, if the increase in LFP of mothers only operates through an increase in subsidized childcare of the FTS program, then we would expect that mother who face lower childcare costs because e.g., a grandparent (or adult woman) lives in the household, would respond less to the program. We test if the presence of a grandparent or other adult women in the household leads to significant differences in the effect. In fact, informal childcare by grandparents is very common in Mexico and 55% of children are cared for by their grandparents while the parents are working (Villegas Raya, 2019). To do this, we construct an indicator that takes the value 1 if a grandparent lives in the household and an additional indicator that takes the value 1 if women over the age of 18 live in the household (excluding the mother).[30] Then we interact the share of FTS with these indicators, respectively. The results in Table 3.A8 in the Appendix reveal that the presence of a grandparent (or adult woman) leads to no significant differences of the impact of FTS on the labor participation of mothers. The interaction coefficient is very close to zero and not statistically significant.

Both findings combined, support our argument that the labor supply of mothers does not only increase due to a childcare subsidy, but also due to a substitution effect between child and adult labor. Income constrained parents will rely on child labor to meet the subsistence needs of the household (Dammert *et al.*, 2018). In this case, as schooling time is increasing, the parents substitute child labor for adult labor to keep household consumption levels constant.

3.6 Conclusion

Several Latin American countries have achieved the goal of universal primary coverage, thus current education policies are shifting from increasing school enrollment to increasing schooling time. Several countries have implemented programs that shift the traditional part-time schooling day to full-time in order to extend learning opportunities and develop competences (UNESCO, 2015). Previous studies on full-time schools have found that full-time schools have a positive impact on test scores (see e.g., Bellei, 2009; Hincapie, 2016; Cabrera-Hernández, 2020; Agüero, 2016), a decrease in the probability that children engage in risky activities (Berthelon and Kruger, 2011), and even an increase in the participation of mothers in the labor force (Contreras and Sepúlveda, 2016; Padilla-Romo and Cabrera-Hernández, 2019). We contribute to this literature, by analyzing the effect of increasing instruction time in school on school enrollment and child labor.

[30]We also test the regressions conditional on women over 25 and find similar results. The results are available upon request.

Our empirical strategy exploits the staggered implementation of the FTS program at the municipality level in Mexico. The FTS program extended the time spent in school from four to six or eight hours and was implemented gradually from 2009 to 2018. We find that the share of FTS has no impact on school enrollment, but led to a strong decrease in child labor.

In terms of standard deviations, an increase in the FTS share by one standard deviation (14 percentage points) results in a decrease in the probability that children work by 0.9 percentage points i.e., a 12% reduction in child labor.[31] Moreover, a simple cost-benefit calculation suggests that the costs of the program per student are relatively low, while the benefits of the program in terms of both academic and non-academic outcomes are large in comparison. For the school calendar year 2017/2018, the cost of the program was on average 156.25 USD[32]. This amount represents a 5% increase of the average public spending on education per student which was equal to 2,656 USD for primary and 3,034 USD for secondary students in 2012 (OECD, 2020). A back of the envelope calculation shows that a 0.9 percentage points decrease in the child labor rate translates to 158 thousand children aged 7-14 who stopped working due to the FTS program.

Even though the program fulfilled its main objective of improving schooling outcomes (Cabrera-Hernández, 2020), in 2019, the Ministry of Education in Mexico announced that for the school calendar year 2020/2021, the budget for the FTS program will be cut by 52% (Toribio, 2019). The budget cut implies not only that no new schools will be included in the program, but also that schools currently operating on a full-time day will go back to the part-time schedule. The evidence provided in this study reveals that such a rollback of the program may result in an increase in child labor and a decrease in LFP of mothers with young children.

[31]Population estimates report that 17.9 million children aged 7-14, out of whom 7.5% were engaged in child labor.

[32]For the school calendar year 2017/2018 the program covered 3 million children enrolled in basic education and had a budget of 10 billion pesos, which implies a cost of 3 thousand pesos per child. We use the average exchange rate for 2018 which is 19.2 pesos for one USD.

Our results have important policy implications not only for Mexico, but also for other Latin American countries, where primary schooling is almost universal, but child labor rates remain a concern. First, we show that the shift from part-time to full-time school days decreases the probability to engage in child labor. Second, in line with Padilla-Romo and Cabrera-Hernández (2019) we find that the program has important spill-over effects within the household causing mothers with children aged 7-14 to increase their labor force participation. Thus, policies aimed at extending the instruction time in school can contribute to the global goal of eradicating child labor and can simultaneously increase the participation of mothers with young children in the labor force.

Appendix Chapter 3

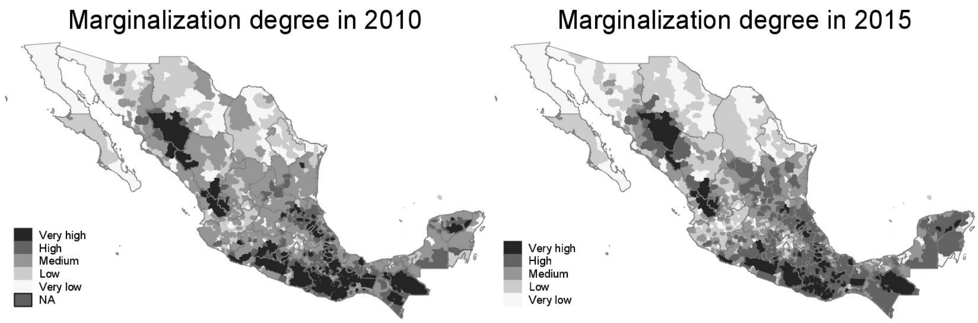

Figure 3.A1: Marginalization Degree by Municipality
Source: Consejo Nacional de Población (CONAPO), authors' analysis.

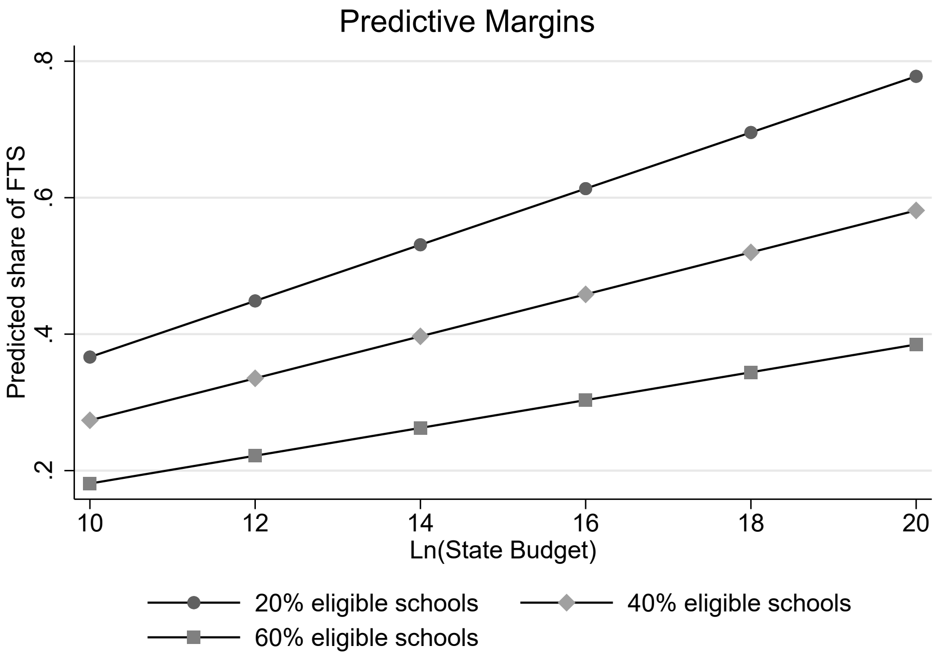

Figure 3.A2: Marginal Effects: State Level Budget and Share of Eligible Schools on Predicted Share of FTS

Source: ENOE, authors' analysis.

Note: The graph shows the marginal effects of the interaction term used to estimate the first-stage of the IV regression model. The graph shows the impact on the outcome variable at different budget levels and share of eligible schools. The values in the x-axis show different log budget levels starting at 10 because the average budget for the program at the state level is 144 million pesos.

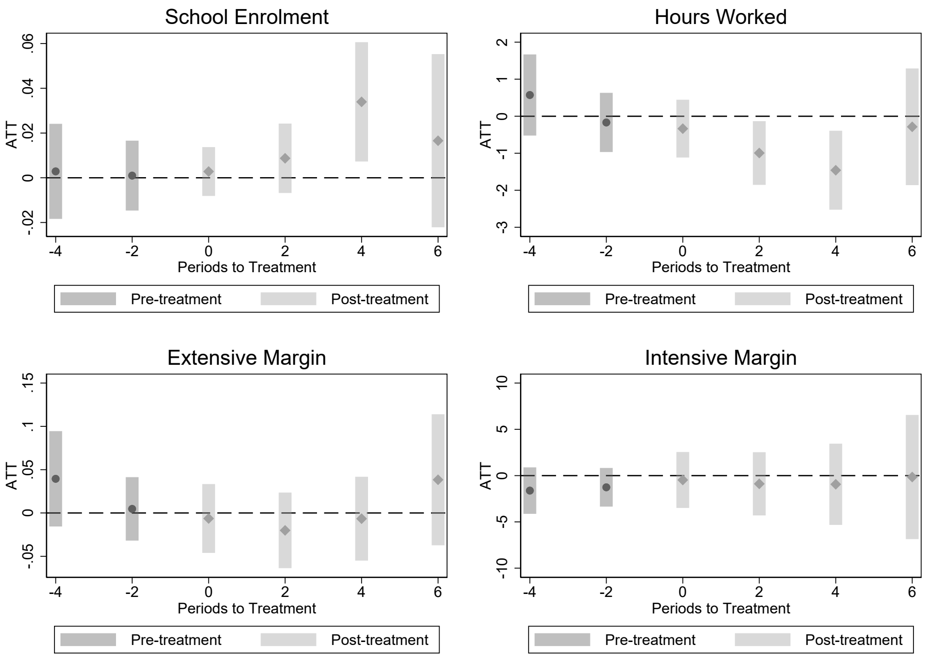

Figure 3.A3: Event Study: Impact of FTS Program on Schooling and Child Labor

Source: ENOE – Módulo de Trabajo Infantil (MTI), authors' analysis.

Notes: – The set up of the event study design uses the time since the municipality exceeded the 25th percentile of average coverage i.e., 8% of schools in the municipality are covered by the program following Callaway and Sant'Anna (2021). The regression includes municipality and time fixed effects. The control group is restricted to never-treated municipalities. The area in grey represents the confidence interval at a 95% level. The standard errors are clustered at the municipality level.

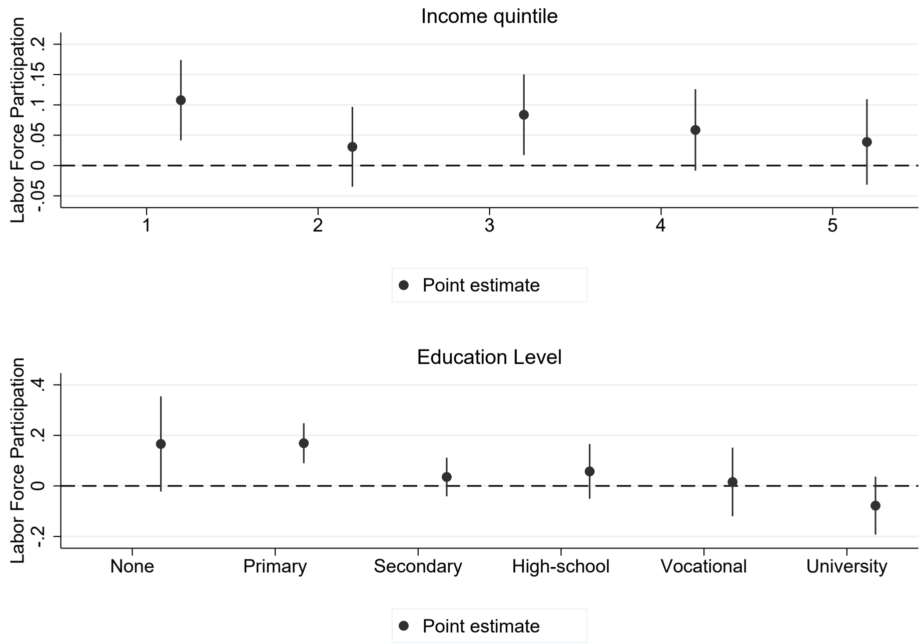

Figure 3.A4: Effect of FTS Program on LFP of Mothers by Income Quintile and Education Level

Source: ENOE, authors' analysis.

Table 3.A1: Pre-National FTS Roll-Out Descriptive Statistics

	Below median		**Above median**		
	Mean	S.D.	Mean	S.D.	Δ Mean[a]
Share of FTS	0.007	0.011	0.025	0.042	0.019***
Dependent variables					
Attends school	0.967	0.179	0.968	0.175	0.001
Weekly hours spent on school activities	30.672	11.241	30.253	11.720	−0.419**
Child is working	0.162	0.368	0.176	0.381	0.014**
Cond. weekly hours worked	20.347	12.334	19.580	11.329	−0.767*
Market work	0.068	0.252	0.070	0.256	0.002
Household work	0.102	0.303	0.116	0.320	0.014***
Cond. weekly hours worked (market)	16.660	14.724	15.299	13.967	−1.361*
Cond. weekly hours worked (domestic)	18.889	7.993	18.302	6.841	−0.587*
Child characteristics					
Age	10.636	2.284	10.592	2.290	−0.044
Male	0.511	0.500	0.509	0.500	−0.002
Receives gov. support	0.204	0.403	0.269	0.443	0.064***
Number of siblings	3.096	1.415	3.155	1.470	0.059**
Birth order					
First born	0.349	0.477	0.348	0.476	−0.001
Middle born	0.318	0.466	0.326	0.469	0.008
Last born	0.333	0.471	0.326	0.469	−0.007
Both parents present	0.827	0.378	0.812	0.390	−0.015***
Mother's education level					
No education	0.065	0.246	0.081	0.273	0.017***
Primary education	0.394	0.489	0.349	0.477	−0.046***
Secondary education	0.296	0.456	0.289	0.453	−0.007
High-school	0.106	0.308	0.103	0.304	−0.003
Vocational training	0.074	0.262	0.092	0.289	0.018***
University degree	0.065	0.246	0.086	0.280	0.021***
Father's education level					
No education	0.049	0.215	0.060	0.238	0.012***
Primary education	0.359	0.480	0.341	0.474	−0.018**
Secondary education	0.311	0.463	0.278	0.448	−0.033***
High-school	0.127	0.333	0.141	0.348	0.013**
Vocational training	0.036	0.186	0.044	0.206	0.008***
University degree	0.118	0.323	0.136	0.343	0.018***
Locality size					
More than 100,000 inhabitants	0.449	0.497	0.416	0.493	−0.033***
15,000-99,999 inhabitants	0.177	0.382	0.136	0.342	−0.042***
2,500-14,999 inhabitants	0.158	0.365	0.149	0.356	−0.009*
Less than 2,500 inhabitants	0.216	0.411	0.299	0.458	0.084***
Municipality characteristics					
Share age 7-17 out of school	0.114	0.083	0.113	0.071	−0.001
Share living in poverty	0.314	0.198	0.331	0.204	0.017***
Share women in the LF	0.475	0.138	0.464	0.141	−0.012***
Observations	14,511		35,897		

*Notes: – The sample is restricted to the year 2009 before the FTS program was rolled out. To define the groups we use the roll-out at the municipality level for the school year 2017/2018. – [a] The column shows the difference in mean values between municipalities in the below and above median classification. – Significance stars indicate the result of the respective t-test. – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table 3.A2: Marginalization Indicators by FTS Intensity of Implementation

	Below median		Above median		
	Mean	S.D.	Mean	S.D.	Δ Mean[a]
Total population (in 1,000)	65.950	170.278	78.589	165.952	12.639
Share of inhabitants:					
Who cannot read and write (older than 15)	12.510	8.092	10.830	8.309	−1.681***
Without primary education (older than 15)	31.283	12.319	28.915	11.897	−2.369***
Without access to drainage and sanitary services	5.861	8.208	7.786	10.380	1.925***
Without piped water	14.915	17.706	11.790	14.997	−3.125***
Living overcrowded	44.525	11.034	42.992	12.035	−1.533**
Living with ground floors	11.519	10.196	8.665	8.735	−2.854***
Living in localities < 5000 inhabitants	57.727	36.017	59.323	34.495	1.596
Earning less than 2 min. wages	57.264	18.947	54.300	18.351	−2.964***
Marginalization degree:					
Very low	0.158	0.365	0.172	0.378	0.014
Low	0.170	0.376	0.236	0.425	0.066***
Medium	0.412	0.493	0.391	0.488	−0.021
High	0.147	0.354	0.111	0.315	−0.035**
Very high	0.114	0.318	0.089	0.285	−0.024
Observations	634		808		

*Notes: – The marginalization level data is obtained from (CONAPO, 2019) at the municipality level. We use data for 2010 to measure pre-program municipality characteristics. – [a] The column shows the difference in mean values between municipalities in the below and above median classification – Significance stars indicate the result of the respective t-test. – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table 3.A3: Child Labor Rate and Implementation of the FTS Program

	I	II	III	IV
Child labor rate	−0.018	−0.016	–	–
	(0.018)	(0.015)		
Child labor rate t-2	–	–	0.005	−0.001
			(0.019)	(0.017)
State-by-year FE	no	yes	no	yes
Observations	4,063	4,063	3,058	2,247
R^2	0.831	0.895	0.922	0.953

*Notes: – Results are obtained from OLS regressions. – Standard errors in parentheses (clustered at the municipality level). All columns control for municipality FE and year FE. – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table 3.A4: Effect of FTS Program using Non-Linear Models

	Total hours	Ext. margin	Int. margin
	Tobit	Probit	Heckman
Share of FTS	-10.049^{*}	-0.054^{**}	-4.525
	(5.396)	(0.023)	(3.626)
Inverse Mills Ratio	–	–	1.150
			(5.491)
Observations	230,256	228,966	12,651

Notes: – Standard errors in parentheses (clustered at the municipality level). The regressions include the full set of control variables, municipality FE, and state-by-year FE. The table shows coefficients of a Tobit regression, average marginal effects of a Probit regression, and coefficients of the outcome equation of a Heckman selection model. Note that in the Probit model the sample size is reduced because there are some municipalities where all or none of the children work, and have been excluded from the sample. – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 3.A5: First-Stage Results

	I
A. First-stage results	
Share of eligible schools	0.056
	(0.171)
Share of eligible schools X Ln(State budget)	-0.052^{***}
	(0.009)
Observations	230,256

Notes: – Results are obtained from OLS regressions. – Standard errors in parentheses (clustered at the municipality level). The specification includes the full set of controls and municipality FE, and state-by-year FE. The F-statistic for the interaction term is equal to 36.28 and the critical value of the t-statistic in the regression is -6.02 . – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 3.A6: Effect of the Share of FTS on Child Labor: Robustness

Dependent variable:	School enroll.	Total hours	Ext. margin	Int. margin
	I	II	III	IV
	A. Only FE			
Share of FTS	0.011	-2.308^{***}	-0.084^{***}	-3.903^{*}
	(0.013)	(0.643)	(0.026)	(2.104)
Observations	230,256	230,256	230,256	30,595
	B. Excluding 2017			
Share of FTS	0.013	-2.829^{***}	-0.099^{***}	-4.808^{**}
	(0.013)	(0.625)	(0.026)	(2.260)
Observations	188,573	188,573	188,573	26,255
	C. Including a state time trend			
Share of FTS	-0.001	-1.476^{***}	-0.059^{**}	-1.881
	(0.012)	(0.546)	(0.024)	(1.776)
Observations	230,256	230,256	230,256	30,595
	D. Only primary age children			
Share of primary FTS	0.002	-1.367^{***}	-0.058^{***}	-2.642
	(0.010)	(0.477)	(0.019)	(1.757)
Observations	218,964	218,964	218,964	27,628
	E. Exclude pilot municipalities			
Share of FTS	0.003	-2.147^{***}	-0.091^{***}	-3.253
	(0.014)	(0.644)	(0.026)	(2.160)
Observations	207,362	207,362	207,362	27,976
	F. Exclude top 5% implementing mun.			
Share of FTS	-0.001	-2.126^{***}	-0.077^{***}	-3.564
	(0.015)	(0.694)	(0.027)	(2.332)
Observations	217,434	217,434	217,434	28,333
	G. Exclude marginalized mun.			
Share of FTS	0.003	-2.008^{***}	-0.073^{***}	-4.209^{**}
	(0.011)	(0.572)	(0.025)	(1.909)
Observations	223,949	223,949	223,949	28,940
	H. Alternative definition			
Share of students in FTS	-0.006	-1.585^{**}	-0.065^{**}	-2.279
	(0.013)	(0.637)	(0.026)	(2.117)
Observations	230,256	230,256	230,256	30,595

*Notes: – Results are obtained from OLS regressions. – Standard errors in parentheses (clustered at the municipality level). Except for Panel A, all regressions include the full set of control variables, municipality and state-by-year FE. – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table 3.A7: Effect of FTS Program Controlling for the Share of Eight-Hour Schools

Dependent variable:	Total hours	Ext. margin	Int. margin
	I	II	III
	A. Any work		
Share of FTS	-1.739^{*}	-0.088^{**}	-0.645
	(1.018)	(0.044)	(3.713)
Share of 8 hrs FTS^{a}	-0.773^{**}	-0.023^{*}	-1.356
	(0.358)	(0.014)	(1.154)
Observations	126,913	126,913	13,946
	B. Market work		
Share of FTS	-0.755	-0.055	0.411
	(0.581)	(0.034)	(7.392)
Share of 8 hrs FTS^{a}	-0.528^{**}	-0.017^{*}	-3.810
	(0.256)	(0.009)	(2.340)
Observations	126,913	126,913	5,739
	C. Domestic work		
Share of FTS	-0.984	-0.026	-3.148
	(0.876)	(0.035)	(3.544)
Share of 8 hrs FTS^{a}	-0.244	-0.007	0.604
	(0.277)	(0.011)	(1.076)
Observations	126,913	126,913	8,889

Notes: ^{a} *Share of eight hours FTS over total FTS at the municipality level. – Results are obtained from OLS regressions. – Standard errors in parentheses (clustered at the municipality level). The regressions include the full set of control variables, municipality and state-by-year FE. –* *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 3.A8: Effects of FTS Program on Mothers' LFP: Grandparent Living in HH

Dependent variable:	I	II
Share of FTS	0.077***	0.073**
	(0.030)	(0.030)
Grandparent lives in HH	0.005	–
	(0.012)	
Share of FTS X Grandparent	−0.005	–
	(0.053)	
Adult women living in HH	–	0.003
		(0.005)
Share of FTS x Adult women	–	0.018
		(0.025)
Observations	322,752	322,752

*Notes: – Results are obtained from fixed-effects regressions. – Standard errors in parentheses (clustered at the individual level). The regressions control for age, age squared, age of youngest hh member, age of youngest hh member squared, individual and state-by-year FE. – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table 3.A9: Effect of FTS Program on Household Members: No Child Aged 7-14 Lives in the Household

Dependent variable:	LFP	Market Work	Domestic Work
	I	II	III
	A. Mothers		
Share of FTS	0.040	1.327	−0.692
	(0.029)	(1.176)	(1.232)
Observations	303,755	303,755	303,755
	B. Fathers		
Share of FTS	0.013	1.273	−0.385
	(0.029)	(1.746)	(0.646)
Observations	211,038	211,038	211,038
	C. Older siblings		
Share of FTS	−0.031	−0.390	1.712
	(0.064)	(2.258)	(1.072)
Observations	103,619	103,619	103,619

*Notes: – Results are obtained from fixed-effects regressions. – Standard errors in parentheses (clustered at the individual level). The regressions control for age, age squared, age of youngest hh member, age of youngest hh member squared, individual and state-by-year FE. – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

4 The Double Burden: The Impact of School Closure on the Labor Force Participation of Mothers*

Abstract: This paper investigates the effect of school closure on the labor force participation (LFP), hours worked, extensive, and the intensive margin of women in Mexico for the years 2017 to 2021. Using a difference-in-differences approach, I analyze how school closure, due to the COVID-19 pandemic, affects the labor supply of women with school-aged children, 6 to 14 years old, versus women with nursery-aged children, 0 to 5 years old. This approach allows me to isolate the impact of school closure from the economic impact of the COVID-19 pandemic. The findings show that mothers with children younger than 14 decrease their LFP by about 2.6 percentage points. Mothers with school-aged children, however, decrease their LFP by an additional 1.7 percentage points and increase their domestic work. The decrease is observed for all women with low or middle education level, formal and informal employment, and income quantiles. However, I find no decrease for single-mothers and mothers with access to informal child care.

*This chapter contains minor revisions of: Kozhaya, M., The Double Burden: The Impact of School Closures on the Labor Force Participation of Mothers. Ruhr Economic Papers No. 956. I thank Christian Bredemeier, Kerstin Schneider, Fernanda Martínez Flores, and Franz Westermaier for their constructive comments. I also thank the participants of the internal research seminar at the University of Wuppertal. All remaining errors are my own.

4.1 Introduction

School closure is often considered as a way to slow down the spread of different diseases such as influenza or the COVID-19 virus (De Luca *et al.*, 2018; Dave *et al.*, 2021; UNICEF, 2021a). According to UNICEF (2021a) report, in the year 2020, 168 million children in 14 countries worldwide were affected by the full school closure due to the COVID-19 pandemic.[1] From March 2020 to February 2021, schools worldwide closed for about 95 days. Latin America and the Caribbean were the regions with the longest school closures, with an average of 158 days. Therefore, school closures in those regions affected women's labor force participation negatively. According to the ILO (2022a) report for Latin America and the Caribbean, the greatest impacts are shown for certain types of sectors that rely intensively on female labor force such as commerce, restaurants and hotels, and many others. Moreover, the loss was mostly observed in micro, small and medium sized enterprises as well as informal employment where women's labor force participation is also predominant. 23.6 million women lost their jobs in the second quarter of 2020, and only 19.3 percent of the jobs were recovered by the end of 2021. Accordingly, more than 4 million women were not able to join or return to their work (ILO, 2022a).

Therefore, in many countries i) schools were closed for months, and ii) we know to what extent the economic crisis related to the pandemic affected employment, but we do not know to what extent school closures had an impact on labor market outcomes. In particular, Mexico has closed its schools for 214 days, in terms of schooling days this accounts to more than 1 year of home-schooling. Therefore, in Mexico alone, out of the 168 million children affected globally by school closure, 33.2 million (20%) students missed almost all classroom instruction time, making Mexico rank as the 3rd out of the 14 most affected countries, proceeded by Bangladesh 36.8 million (22%), and Brazil 44.3 million (26%) (UNICEF,

[1] Not to mention also that more than 1 in 7 children globally have been deprived from at least three quarters of their in-person learning with teachers.

2021a). Many classes in Mexico were shifted to online or via television and many students did not have access to internet. Thus, because of the lack of support in child care and the increased need for supervision at home, the reincorporation of women in the labor force has been mitigated (Insituto Nacional De Las Mujeres, 2020b).[2]

This paper estimates the impact of the school closure in Mexico on a number of labor indicators for women with school-aged children. The studies done in the literature have analyzed i) the effect of schooling on maternal employment and ii) the overall effect of school closures and the COVID-19 pandemic on the gender inequality in the labor force market. The main results in the paper provide new evidence on labor outcome variables for women with school-aged children by i) isolating the economic impact of school closures from the COVID-19 pandemic, and ii) highlighting the importance of schools as a mean of child care.

First, the literature on mothers' labor force participation for developed countries finds mixed results when evaluating the impact of child care subsidies. Some studies have shown that by increasing child care such as investing in public child care or introducing new kindergartens would increase maternal employment (Meyers *et al.*, 2002; Gelbach, 2002; Baker *et al.*, 2008; Cascio, 2009; Nollenberger and Rodríguez-Planas, 2015; Bauernschuster and Schlotter, 2015; Brilli *et al.*, 2016; Bick, 2016). Fitzpatrick (2010); Havnes and Mogstad (2011a); Asai *et al.* (2015) show, however, that since more families are not living anymore with grandparents, families tend to substitute informal child care by child care programs provided by the government. Therefore, child care availability at home is not correlated with mothers' labor force participation. For developing countries, few studies show that the availability of child care such as increasing the enrollment in pre-schools increases maternal employment (Berlinski and Galiani, 2007; Martínez and Perticará, 2017; Dang *et al.*, 2019).

[2]By closing schools, many statistics have shown that working parents were obliged to stay home (Baldwin and Weder, 2020; Brynjolfsson *et al.*, 2020).

Second, there is an emerging literature for developed countries on the labor force participation of mothers related to COVID-19 school closures. These studies estimate the gender inequality of COVID-19 and school closures on labor force participation and the total hours worked of men and women in the labor market (Adams-Prassl *et al.*, 2020; Amuedo-Dorantes *et al.*, 2020; Landivar *et al.*, 2020; Rojas *et al.*, 2020; Collins *et al.*, 2021; Couch *et al.*, 2021; Hanzl and Rehm, 2021; Yamamura and Tsustsui, 2021). The latter studies find out that mothers with young children are the ones that are more likely to suffer and carry the burden of reducing hours worked and taking care of children at home.[3]

However, the empirical evidence for school closures for developing countries is scarce and the evidence from developed countries can not be translated to developing countries because i) sector of employment differs, ii) role of the mothers is still traditional, and iii) the lack of government financing for stay-home policies. Very few studies have looked at the general impact related COVID-19 pandemic on working conditions of households. Bundervoet *et al.* (2022) focus on 34 developing countries to evaluate the short-run impact of COVID-19 on the labor force participation of different household members. They find that 36% of the individuals stopped working just after COVID-19. The most affected were women, youth, and lower educated workers that were already considered a disadvantaged group before the COVID-19 pandemic. Egger *et al.* (2021) evaluate nine developing countries by using 16 survey samples and document that the COVID-19 pandemic caused income decreases ranging from 8% to 87% and food insecurities that lasted almost 3 months after the pandemic.

Hoehn-Velasco and Penglase (2021) explore the effect of the pandemic on formal employment in Mexico. The study shows that formal employment decreases by 5%. The authors

[3]Other studies have also checked the impact of school-closures on other outcomes such as the spread of viral diseases (see e.g. Cauchemez *et al.*, 2008; Adda, 2016; Nafisah *et al.*, 2018), stay-at home behaviors (Castillo *et al.*, 2020; Gostin and Wiley, 2020), on working remotely in different sectors (Espitia *et al.*, 2022), on productivity, work engagement, stress (Galanti *et al.*, 2021), on marital relationships (Chasson *et al.*, 2021; Shah *et al.*, 2021), and on children's obesity (Tester *et al.*, 2020; Tripathi *et al.*, 2021).

also show that men recover their jobs faster relative to women.[4] Only the study done by Alon *et al.* (2022) shows how the COVID-19 pandemic has affected women vs. men's employment and the type of employment sector that was mostly affected, using a DiD approach. The study shows that the recession caused by the COVID-19 in Nigeria causes women with school aged children to experience the largest drops in employment rates. Furthermore, by looking at the sector of employment the authors show that those mothers that had to go to the workplace and could not work remotely are the ones that suffer the most. Yet no study for developing countries has aimed so far to analyze the direct impact of school closure, independently from COVID-19, on women's labor force participation. This paper aims at filling in this gap.

In March 20, 2020, Mexico closes its schools for almost 15 months. This serves as a natural experiment to be able to disentangle the real effect of school closure on women with school-aged children 6 to 14 that were directly affected by school closure vs. women that had children in the age range 0 to 5 years that were not directly affected by school closure. To estimate the effect of school closure, this paper makes use of the Mexican Labor Force Survey (ENOE) for the years 2017 to 2021. The data is a rotating panel that interviews households for 5 quarters and is collected on a quarterly basis. It provides rich information on employment and many socio- and demographic characteristics for the households. My empirical strategy exploits school closure as a natural experiment. I look at the group of women with school-aged children that were directly affected by school closure. I implement a DiD approach that exploits school closure in the second quarter of 2020 as the natural cutoff to assign individuals to treatment or to the control group and evaluate the short-run impact of school closure on labor outcomes of women with school-aged children. In addition, due

[4]Previous literature (see e.g., Goldin and Mitchell, 2017; Juhn and McCue, 2017; Kleven *et al.*, 2019; Sieppi and Pehkonen, 2019) has shown that women with children suffer long-term effect of a child penalty which is associated to their employment participation by those women preferring family over their career.

to the panel structure of the data, I estimate within mother effects by including individual effects in the estimation to observe them only right before and after the school closure.

I assign women with school-aged children (6 to 14) in the treatment group and women with nursery-aged children (0 to 5) in the control group. Those women with school-aged children were already working more and benefiting from child care provided by the schools, and therefore they were directly affected by school closure. Women with nursery-aged children were not directly affected by school closure because i) their children are too young to attend pre-school and ii) pre-school enrollment rates are low.[5] To test the robustness of my estimations and to show that the control group is not affected by school closure, I provide the results of a placebo test, the results focusing on a control group restricted to women with children aged 0 to 3, and the results of a control group restricted to women that have no children.

Accordingly, this paper contributes to the literature in three different ways. First, this is the first paper for developing countries that evaluates the direct impact of an exogenous shock, i.e, the school closure in Mexico, caused by the COVID-19 pandemic on the extensive and the intensive margin of employment for women with school-aged children (6 to 14 years old) relative to women with nursery-aged children (0 to 5 years old) that were not affected by the school closure. Mexico serves as an ideal example because children aged 0 to 5 had low pre-school enrollment rates, giving rise to a new control group that enables to isolate the effect of school closure from the COVID-19 pandemic. I further show, using a dynamic DiD estimation, that the decrease in the labor force participation of women with school-aged children is observed almost 1 year after schools closed. This shows that women did not anticipate that school closures will last long, but since schools remained closed for more than 1 year in terms of instruction time, women were not able to adjust and started dropping out

[5]Pre-school education is part of basic education in Mexico as of the school year 2008-2009 and is for children that are aged 3 to 5 years old. Governments also provide other day care subsidies for children younger than 3 years (Yoshikawa *et al.*, 2007).

of the labor force. But, when looking at domestic work the effect is directly shown in the third quarter of 2020 when the schools closed, due to the fact that work at home increases directly due to the increase in child care at home.

Second, since mother's labor force participation relies heavily on the presence of child care, such as the presence of grandparents (Lumsdaine and Vermeer, 2015; Bratti *et al.*, 2018); studies have shown that informal child care increases the labor force participation of the mothers (Hank and Buber, 2009; Bratti *et al.*, 2018; Kanji, 2018). Therefore, I further contribute to the literature by analyzing different channels that could mitigate the effect of school closure such as access to informal child care through the presence of grandparents or adult women in the household.

Third, I expand on the work done by Couch *et al.* (2021), Yamamura and Tsustsui (2021) and Collins *et al.* (2021) by not only taking men as a control group but using alternative control groups as follows: i) women with nursery-aged children 0 to 5, ii) women with children aged 0 to 3, iii) women with no children, and iv) men with school-aged children. Couch *et al.* (2021) use a DiD approach to show that the male-female gap for employment and working hours during the COVID-19 pandemic, in the U.S. labor market, increases for women with school-aged children but not for women with younger children. The study shows that all women were more likely to have higher educational levels and to work remotely which mitigated the effect of COVID-19 in comparison to men. Yamamura and Tsustsui (2021) show how school closures in Japan affect parents work-style by using a simple OLS regression. Their findings are also inline with Couch *et al.* (2021) showing that women with primary school children increased teleworking in comparison to men and women with children in high school were not affected. Collins *et al.* (2021) further analyze the U.S. labor market and show that school closure affects women with young children by reducing their working hours up to 5 times relative to fathers. Therefore, I go further to expand my analysis and analyze different heterogeneous effects focusing not only on the labor outcomes but also on different household characteristics such as income and poverty levels, education level (except for high education), and region of residence that are affecting women differently. I also analyze different employment characteristics such as formal and informal employment, paid vs. unpaid work, and the sector of employment, because women usually work in the informal and tertiary sectors, which were most affected by the pandemic (ECLAC, 2021).

My findings show that school closure decreases the labor force participation of women with school-aged children by 1.7 percentage points and employment decreases by 1.9 percentage points. A back of the envelope calculation shows that 750 thousand women with children aged 6 to 14 have stopped working because schools closed. Those results are mainly driven by a decrease in the informal sector, paid work, and the services sector.[6] I also find that for women with school-aged children, domestic work increases by almost 1 hour per week, mainly driven by an increase in the time spent taking care of other household members. Although one additional hour of domestic work per week may seem like a small effect, the pre-treatment mean shows that women in the treatment spend 30 hours per week on domestic work and only 20 hours on market work. Furthermore, I show that all women with school-aged children, irrespective of their age, are affected negatively by school closure when looking at income levels, poverty level, education level, and rural vs. urban areas. Then, when looking at how different channels of school closure work, I find that the labor outcomes of single women or of women having a grandparent or adult women in the household are not affected by school closures. This highlights the importance of informal child care support at home after experiencing a shock like the school closure.

This paper is structured as follows: Section 4.2 presents the background and provides additional information on the school closure in Mexico. Sections 4.3 and 4.4 present my data and identification strategy, Section 4.5 shows the results, and Section 4.6 concludes.

[6] Paid work is a dummy variable whether the individual gets paid conditional on working.

4.2 Background

4.2.1 Composition of the Labor Market and Statistics

The ILO (2022c) reports that over 2 million moms globally left the labor force participation in 2020 because of job-losses and school closures. Specifically, in Latin America and the Caribbean the labor force participation of mothers with small children decreased from 56.4% in 2019 to 51.5% in 2020 (a 4.9 percentage-point decrease compared to 2.7 percentage points for men). Furthermore, the COVID-19 pandemic had negative impacts on the female labor force participation and employment conditions for female population 15 years and older. The latter decreased from 52% in 2019 to 46% in 2020. Not to mention that also 56.9% of the women are employed in sectors that were mostly affected by the pandemic.

Now by looking at Mexico in particular, in 2018, 78% of men and only 44% of women in Mexico participated in economic activities. This is one of the largest gender gaps among the OECD countries (OECD, 2021d; Insituto Nacional De Las Mujeres, 2018). Female labor force participation in Mexico also differs from the labor markets in other countries because it relies mostly on the informal sector (53.4% of the population) with 58.8% of women being more likely to work in the informal sector than men (50.1%) (ILO, 2018). Looking at the sector of employment in Mexico, the ILO report shows that 72.8% (52.3%) of women (men) work in agriculture, 46.1% (51.9%) in industry, and 60.8% (48.2%) in services. According to INEGI (2021) for the last 10 years, the economic participation of women has grown by 15.7 percentage points, from 33.3% in 2010 to 49% in 2020. However, due to the pandemic, in the months of April and May of the year 2020 female labor force participation fell to almost 35%.

Therefore, Mexico witnessed a decrease in the economically active population by 12 million, being 7 million men and 5 million women. In relative terms, this resembles a decrease by

20% of the male labor force and 22% of the female labor force (Insituto Nacional De Las Mujeres, 2020a). The statistics provided by the Insituto Nacional De Las Mujeres (2020b) show that men recover at a faster rate than women reaching a rate of 72.5% , which is only 5.5% lower than the pre-pandemic level. The rate of recovery for women seems to be at a slower rate 39%, being 12.7% lower than the pre-pandemic period.

Despite the major improvement in labor force participation only 4 out of 10 women in Mexico participate in the labor market. Therefore women's participation in paid work still lies behind that of men, due to lack of child care, inappropriate distribution of work at home, inflexible working conditions, and many others (Insituto Nacional De Las Mujeres, 2018). Moreover, women still hold the traditional role in Mexico by taking care of children, therefore, the participation rate of women in domestic work is still very high (96.1%) compared to men (65.4%) (Insituto Nacional De Las Mujeres, 2018). In 2019, the majority of women in Mexico worked part-time 36.62%, and only 19.15% of men had a part-time job (ILO, 2019).

Looking at partnered parents, estimates in 2019 show that women with children in Mexico had, in general, a labor force participation rate by about 36.5% relative to 87.6% for fathers (ILO, 2019). Therefore, the establishment of women into the labor force requires proper access to child care and to establish provisions for maternity care. According to INEGI (2018c) 77.4% of working women do not have child care or maternity services in Mexico.[7] In addition, more than half of the population of women already working (12.4 out of 21.6 million) have at least one school-aged child (Insituto Nacional De Las Mujeres, 2020a).[8] Therefore, school closures may increase the burden of domestic work of women and lead to a disproportionate impact in terms of the labor force participation, in particular, for women with school-aged children.

[7] According to article 170 of the Federal Labor Law *(Ley Federal del Trabajo)*, women have the right for a rest period of one and a half months before and after birth of the child. For an extensive overview of the maternity services provided in Mexico see (Ley Federal del Trabajo, 2012).

[8] 6.4 million women out of the 12.4 had children under the age of 6 and the other 6 million had children between 7 and 12 years old.

4.2.2 School Closure in Mexico

Taking early childhood education and care into account, since the academic year 2008/2009, all Mexican children aged 3 to 6 years old are required by law to attend three years of early childhood education (Monroy and Trines, 2019). However, preschool enrollment rates are smaller than primary school enrollment rates. In 2015, 98% of children aged 6 to 11 attended school, 93.3% were between 12 and 14 years old, and 77% were between 3 and 5 years old (INEE, 2018a).[9] In Mexico, preschool or initial education was first established in the education system after the Educational Reform done in 2019, where preschool or initial education has been recognized as part of basic education and therefore compulsory for children aged 3 to 5 years old (Centro de Investigación Económica y Presupuestaria, 2019). Nonetheless, primary school attendance rate was almost universal and pre-school attendance rates are still among the lowest enrollment rates, especially when looking at the 0 to 3 years old children (OECD, 2021c).

The national school closure was implemented on the 20, March 2020. Due to the pandemic, primary and lower secondary schools were closed for almost 214 days, and upper secondary schools closed for 264 days (OECD, 2021b).[10] Therefore, almost 33.2 million children and teenagers had to stay home. School started reopening gradually after 15 months of closure (Mexico Daily News, 2021).[11] Moreover, Kindergartens had to close as well but few women with nursery-aged children were still able to send their children to daycare unlike women with school-aged children. For example, some child care centers for children younger than 5 remained opened, mostly to allow mothers in "necessary" occupations to continue working.[12] Therefore, to rule out the concern that women with nursery-aged children are also affected

[9] School enrollment rate is 73% for individuals aged 15 to 17.

[10] The number of days closed excludes holidays and weekends.

[11] School closures happened during the period ranging from January 2020 until May 20, 2021 (OECD, 2021b).

[12] According to the news, the 221 child care centers owned by the institute of security and social services for state workers (ISSSTE) daycare centers have remained opened (ISSSTE, 2021).

by school closure, I provide further analysis with different control groups by taking other control groups that were also not affected by school closure: i) women without children, ii) women with children aged 0 to 3, and iii) men with school-aged children and show that the results remain robust to the alternative definitions of the control groups. A summary of the different definitions used for treatment and control is provided in Table 4.A1 in the Appendix.

In addition, communication between teachers and students took place mostly through digital tools during the pandemic for the school year 2019/2020. The most used tools are smart-phones 65.7%, followed by laptops 18.2%, desktop computers 7.2%, 5.3% digital television, and 3.6% using tablets. According to OECD (2020) only 57% of the students reported having access to a computer, and for students distributed at the bottom quartile only 14% of them reported having a computer.[13] Home-schooling and online learning was facilitated by teachers and parents, especially mothers. According to Bozkurt *et al.* (2020), mothers of children below the age of 12 were the ones who had the burden of home-schooling, making mothers the ultimate substitute for teachers.

4.3 Data and Descriptive Statistics

4.3.1 Data

The data used for this study comes from the Mexican National Survey on Occupation and Employment (ENOE). Since 2005, this data set collects information on a quarterly basis on households in a rotating panel for 5 quarters.[14] The ENOE survey is representative at

[13]In Mexico only 51.2% of the households have a computer and only 70% have access to internet. Those statistics are biased to wealthier families living in urban areas (Covarrubias, 2021).

[14]As of the 3rd quarter of 2020, the ENOE also started collecting data on a monthly and quarterly basis. Due to COVID-19 the information was not collected for the 2nd quarter of 2020, however, this does not influence the analysis because I am interested in the yearly trajectory change and not only in the change that happened in 2nd quarter of 2020.

the state level and records rich information on labor force participation[15], employment and weekly hours worked, parental demographic characteristics (education level, marital status, and monthly income) and household characteristics (number of children, household size, household income, and the age of the children), as well as time spent on several household activities, that is domestic work (as weekly hours spent on building the household, renovating, doing household chores, and taking care of the elderly or children or the sick).

For the main analysis, I restrict the period of observation from 2017 to 2021, i.e., three years before school closure and 1 year after. This allows me to i) account for pre-treatment differences between the treatment and the control group, and ii) estimate the causal impact of school closure on mothers with school-aged children. Next, I restrict my sample as follows: first, sample of women between 20 and 55 years old. Then, I compare women with school-aged children (6 to 14 years) who are directly affected by the school closure vs. women with nursery-aged children (0 to 5 years) who were not directly affected by school closure. For this sample I observe a total of 382,322 women. In addition, for the robustness tests I further include i) women with no kids, ii) women with children aged 0 to 3, and iii) men with school-aged children.

4.3.2 Descriptive Statistics

I start my analysis by providing graphical evidence on the development of the labor force participation and hours worked of (i) women with no children, (ii) women with school-aged children, and (iii) women with nursery-aged children.

Figure 4.1 shows that before school closure, both the labor force participation and hours worked have a level difference between the different groups of women. However, after the school closure we see that both women with no children and women with nursery-aged

[15] Labor force participation comprises of those who are employed and unemployed individuals searching for a job.

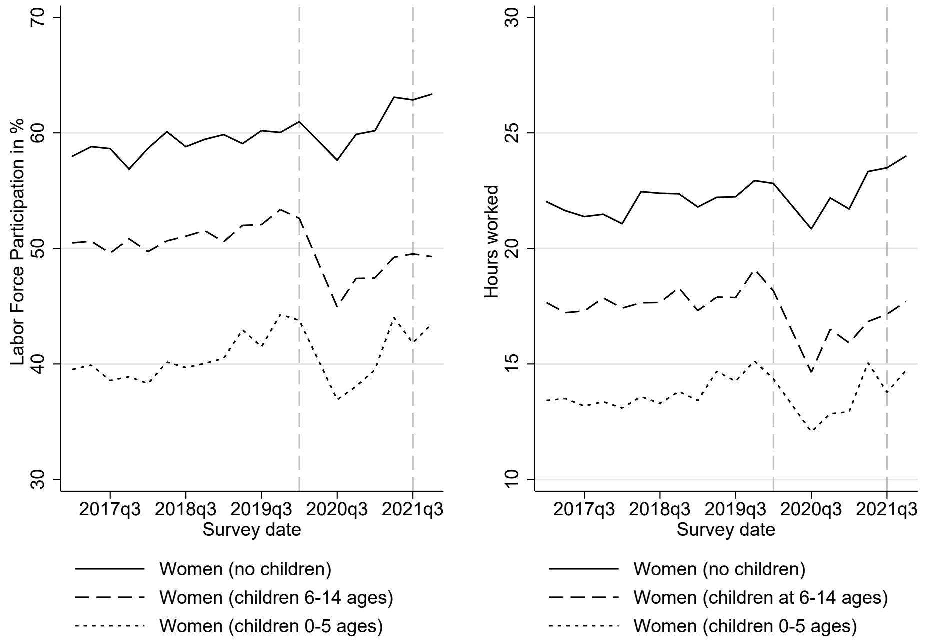

Figure 4.1: Labor Force Participation and Hours Worked of Women According to Survey Year

Source: ENOE, authors' analysis.

Notes: – The figure illustrates labor force participation and hours worked of women with no kids vs. women with school kids who are aged 6 to 14 years old and vs. women who have nursery-aged kids 0 to 5 years old. School closures happened in the second quarter of 2020. Schools started reopening in the third quarter of 2021.

children rebound fast and go back to levels similar to those before the school closure. However, when looking at women with school-aged children we see that the decrease persists causing the labor force participation and hours worked to still lie behind the levels of what they had before the school closure.[16] This preliminary descriptive inspection shows that indeed the labor force participation of women with school-aged children is more affected than that of women with younger children (or without children).

[16]As of the third quarter of 2021 we see a small drop in the labor force participation of mothers with nursery-aged children, this is can be explained because the president of Mexico started canceling some child care programs. However, this does not affect my results since I provide other definitions for control group to show that my estimation is robust.

Next, I show in Table 4.1 the descriptive statistics for all women with school-aged children (treatment group) and for all women with nursery-aged children (control group). The final column provides the difference between means for the two groups to test if the difference in means is significant. The table shows that 57% of women in treatment group participate in the labor force, 56% of them are employed, and work almost 20 hours per week in market work and 32 hours per week in domestic work. Now, conditional on working those women work on average 36 hours per week and 46% of them work in the formal sector. Moreover, almost 95% of those women work in paid employment. As for women in the control group, we observe some minor level difference for labor force participation 47%, employment 46%, and conditional on working 54% of women in the control group work in the formal sector. Women in the control group work less in market work (16 hours), and spend more time on domestic work than the treatment (43 hours).

Looking at the sector of employment, on average, 2.5% of treatment and control groups work in the primary sector (agriculture), 17% work in the secondary sector (manufacturing), and 81% in the tertiary sector (services). On average women in the sample are 38 years old and have on average 3 children. 86% of them are married and for 77% of them the spouse is present at home. 5% of them have a grandparent present in the household. As for their education, 2% do not have education, 18% have primary education, 35% have secondary degree, 26% have a high-school degree or vocational training, and almost 20% have a university degree. 58% of the women in treatment and control live in localities with more than 100,000 inhabitants, i.e., highly urbanized areas. Localities are smaller geographical units than municipalities and capture the level of urbanization (high, middle, low, or rural) in the locality the individual resides.

Table 4.1: Descriptive Statistics: Pre-School Closure of Women with Children

	All		Treatment before		Control before		T-test
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Δ Mean[a]
Dependent variables							
Labor force participation	0.548	0.498	0.572	0.495	0.475	0.499	0.097***
Employed	0.534	0.499	0.559	0.497	0.458	0.498	0.101***
Total hours worked	19.128	22.057	20.074	22.249	16.172	21.176	3.902***
Domestic Work	34.541	17.174	31.998	16.021	42.691	18.179	−10.693***
Conditional dependent variables							
Conditional hours worked	35.810	17.704	35.935	17.780	35.332	17.399	0.602***
Formal work conditional on working	0.479	0.500	0.463	0.499	0.540	0.498	−0.077***
Paid employment	0.949	0.220	0.948	0.222	0.953	0.211	−0.005***
Sector							
Primary	0.025	0.155	0.026	0.160	0.019	0.135	0.008***
Secondary	0.171	0.376	0.171	0.376	0.170	0.375	0.001
Tertiary	0.805	0.396	0.803	0.398	0.812	0.391	−0.009***
Control variables							
Women with school aged kids 6-14	0.758	0.429	1.000	0.000	0.000	0.000	1.000
School closure	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Age	37.793	8.254	40.096	6.804	30.594	8.247	9.502***
Spouse present	0.765	0.424	0.747	0.435	0.821	0.384	−0.074***
Grandparent present	0.054	0.227	0.052	0.223	0.060	0.237	−0.007***
Number of children	2.506	1.389	2.767	1.403	1.689	0.965	1.078***
Marital Status							
Married/Cohabiting	0.863	0.344	0.846	0.361	0.919	0.273	−0.073***
Separated/Divorced	0.068	0.252	0.080	0.271	0.032	0.175	0.048***
Widowed	0.016	0.126	0.019	0.138	0.006	0.077	0.013***
Single	0.052	0.223	0.055	0.228	0.044	0.204	0.012***
Mother's education level							
No education	0.019	0.138	0.022	0.148	0.010	0.098	0.013***
Primary education	0.175	0.380	0.197	0.398	0.106	0.308	0.090***
Secondary education	0.349	0.477	0.364	0.481	0.302	0.459	0.062***
High-school	0.206	0.404	0.184	0.387	0.274	0.446	−0.090***
Vocational training	0.057	0.231	0.063	0.244	0.036	0.187	0.027***
University degree	0.195	0.396	0.170	0.375	0.272	0.445	−0.102***
Locality size							
More than 100,000 inhabitants	0.577	0.494	0.575	0.494	0.585	0.493	−0.010***
15,000-99,999 inhabitants	0.136	0.342	0.136	0.343	0.134	0.341	0.002
2,500-14,999 inhabitants	0.129	0.335	0.130	0.337	0.123	0.329	0.007***
Less than 2,500 inhabitants	0.158	0.365	0.159	0.365	0.158	0.364	0.001
Observations	382,322		289,647		92,675		

Notes: – The table presents descriptive statistics for pre-program trends of women with children before the COVID-19 pandemic for the years 2017-2021. The treatment group inlcudes women who have school-aged kids (6 to 14 years) and control group includes women who have nursery age children (0 to 5 years). [a] *This column represents the difference between treatment and control and the respective t-test.*

Now comparing the pre-treatment mean of the treatment and control, for some variables, larger differences can be observed. For example, women in the treatment group spend 4 hours more per week in market work and 8 hours less per week in domestic work. In addition, women in the treatment are on average 7.4 years older than women in the control group. However, these level differences are accounted for by the DiD strategy. For the other dependent variables and demographic characteristics the differences are significant, however, they are small in value.

4.4 Identification Strategy

To evaluate the effect of the school closure caused by the COVID-19 pandemic and better control for pre-COVID trends, I estimate a DiD model exploiting the difference between women with school-aged children (6 to 14 years old) vs. women with nursery-aged children (0 to 5 years old). The model estimated is:

$$Y_{ist} = \alpha_0 + \beta_0(Treated_i \times School{-}closure_t) + \theta'\mathbf{X}_{ist} + \alpha_{st} + \epsilon_{ist} \tag{4.1}$$

where Y_{ist}, denotes labor market outcomes of woman i, residing in state s, at survey time t taking into account the quarter and the year of the survey. The labor market outcomes are (i) labor force participation, (ii) weekly hours worked[17], (ii) employment (extensive margin), and (iv) conditional hours worked (intensive margin). I further differentiate between women who work in the formal and informal sectors, paid and unpaid work, and the type of employment sector (primary, secondary, and tertiary sectors). Moreover, I analyze the effect of school closure on hours spent on domestic work. For domestic work I aggregate the

[17] Hours worked includes also the zeros for those women who are not employed.

reported weekly hours spent on (i) taking care of children or elderly people in the household, (ii) doing household chores, and (iii) renovating the house and fixing household appliances.

$Treated_i$ is a dummy variable that takes the value one for women with school-aged children (6 to 14 years) and zero for women with nursery-aged children (0 to 5 years).[18] March 2020 is the period when schools closed, therefore $School-closure_t$ is a dummy variable that takes the value one for the period after the second quarter of 2020 and zero otherwise. β_0 is the coefficient of interest as it captures the change in labor force participation (or other employment outcomes) of the treatment group relative to the control group.

X_{ist} is a vector of women demographic characteristics that are likely to affect the labor market outcomes of women, such as age, age squared, marital status, number of children in the household, household size or if the spouse is present. I also control for the presence of a grandparent in the household to account for the access of informal child care. I also add a categorical variable controlling for the education level of the mother[19], and a dummy variable for localities to take into account whether women were residing in urban areas since they might be more affected by the school closure.[20]

I also include state-by-quarter-by-year fixed effects, α_{st}, to capture state specific shocks at the quarterly level, such as reopening of schools and the different state mandates regarding the COVID-19 pandemic, which are more likely to change at the state level. I run another specification for the robustness check, to also account for state linear time trends to capture diverging trends at the state level such as the evolution of the labor force participation of mothers at the state level. In addition, I also provide the results including treatment group specific time trends. The results for both remain robust. Finally, ϵ_{ist} is the error term, and standard errors are clustered at the state-survey year level.

[18]For the treatment group women with children younger than 6 and older than 14 are not part of the sample. Same holds true for the control group, women with children older than 5 years are not considered.

[19]None, primary, secondary, high-school, vocational training, or university degree.

[20]Localities capture the level of urbanization in the regions where women live, whether high, middle, low, or rural. Localities are smaller geographical regions than municipalities.

The main identifying assumption of the DiD approach is that in the absence of school closure, both groups of women would have followed the same trend. Therefore, the first threat to the identification strategy is that women with school-aged children and women with nursery-aged children follow different pre-treatment trends. As mentioned previously, women in the treatment group are older. This, for example, could imply that they have more working experience and could be more attached to the labor force than younger women, and thus respond differently to labor shocks. To show that indeed women in the treatment and control groups follow a parallel trend I start by providing graphical evidence of the parallel trend for labor force participation and hours worked for all women (men) with no children, women (men) with school-aged children, and women (men) with nursery-aged children (see Figure 4.1 (Figure 4.A1 in the Appendix)). Finally, to rule out the existence of pre-trends and to check when the impact of school closure is observed, I follow the same logic as in Eq.(4.1) but now by estimating a dynamic DiD model by interacting the impact of school closure by a quarter-survey-year indicator and present the results of this event study in Figure 4.2.

Second, the previous specification may also lead to biased estimates if unobserved factors that occur simultaneously to the schools closure in March 2020 are systematically correlated with the employment outcomes. For instance, as the COVID-19 pandemic was unknown and uncertain, women may have different risk preferences in terms of exposure to the virus. Some women, for example, may adjust more strongly to the pandemic by e.g., limiting social contacts, dropping out of the labor force, etc.. to decrease the risk of a getting the virus. Therefore, to take into account unobserved characteristics that may be correlated with the treatment, I complement my analysis by implementing individual fixed effects model to exploit the within-mother variation to identify the effect of school closure. Taking into account mother fixed effects allows me to control for unobserved characteristics such as the individual risk assessment of the exposure to the virus. The individual fixed effects model is as follows:

$$Y_{it} = \alpha_1 + \beta_1(Treated_i \times School\!-\!closure_t) + \eta' \mathbf{P}_{it} + \rho_i + \kappa_t + \upsilon_{it} \tag{4.2}$$

where Y_{it}, denotes either labor force participation, hours worked, probability to work, or conditional hours worked of women i, at survey time t. I further test this specification for the outcome variables formal vs informal employment, paid vs unpaid work, and the type of employment sector (see Table 4.A6 in the Appendix). $Treated_i$ is a dummy variable that takes the value one for women with school-aged children (6 to 14) and zero for women with nursery-aged children (0 to 5). $School\!-\!closure_t$ is a dummy variable that takes the value one after second quarter of 2020, when the schools closed. P_{it} is a vector of women time varying characteristics such as age and aged squared. ρ_i captures individual fixed effects, κ_t captures state by-quarter-by-year fixed effects. υ_{it} is the error term and standard errors are clustered at the state-survey year level. This approach allows me to analyze the within individual impact of school closure and account for the time invariant characteristics that happen at the individual level.

However, I refrain from using it as the main specification because women are only followed for five quarters which allows me to observe only a sub-sample of the women I have in the data. This fixed effects approach allows me to restrict the sample to women who are observed at least once before and once after the school closure. This causes the number of observations in my sample to extremely decrease.

Third, the control group chosen may be partially affected by the nation-wide decision to close schools and child care facilities. 77% of children aged 3-5 attend pre-school, although this share is much lower than primary school enrollment it could imply that the control group is affected if mothers sending their children to pre-school are also affected by the closure. Therefore to rule out this concern, I estimate Eq.(4.1) by using another definition for the control group. For this I compare women with school-aged children versus women with children in the age range of 0 to 3 (and for women with no children). Then I change the treatment and control group to compare women with nursery-aged children to women with no children. Furthermore, I take into account the difference in men's labor force participation for those men with school-aged children vs. those men with nursery-aged children. In addition, I provide a placebo test showing that my estimates are not driven by differences in the labor force participation of those women.

Moreover, I make use of the information available on income level, poverty level, education level, and place of residence to test some heterogeneity effects such as whether women affected come from poor or rich families or whether they live above or below extreme poverty. The results are presented in more detail in Section 4.5.2.[21]

Finally, I control in a separate specification for father's education level and the number of COVID cases per 1,000 inhabitants as a control variable and for both my results remain robust. I refrain from including those controls in the main specification because they might be endogeneous to the labor force participation of the mothers. Results are available upon request.

4.5 Results

4.5.1 Baseline Results

In this section, I start by reporting the results from the baseline specification mentioned in Eq.(4.1), to evaluate the impact of school closure on the labor outcomes of women with school-aged children. The results are presented in Table 4.2.

[21]Another important aspect to look at is what happens to the labor force participation of those women in the treatment when schools re-open. The data is unfortunately now only available for the year 2021.

Table 4.2: Effect of School Closure on Labor Force Participation, Employment, Hours Worked, and Conditional Hours Worked

Dependent variable:	**Labor Force Participation** I	**Hours worked** II	**Extensive margin** III	**Intensive margin** IV
Women with school aged kids 6-14 x School closure	−0.017***	−0.471**	−0.019***	0.460**
	(0.005)	(0.209)	(0.005)	(0.213)
Women with school aged kids 6-14	0.055***	2.165***	0.055***	0.387***
	(0.003)	(0.137)	(0.003)	(0.144)
Age	0.041***	1.606***	0.041***	0.289***
	(0.001)	(0.037)	(0.001)	(0.048)
Age-squared	−0.001***	−0.020***	−0.001***	−0.004***
	(0.000)	(0.000)	(0.000)	(0.001)
Spouse present	−0.122***	−5.116***	−0.115***	−1.825***
	(0.004)	(0.145)	(0.004)	(0.137)
Grandparent present	0.030***	1.649***	0.030***	0.893***
	(0.004)	(0.184)	(0.004)	(0.180)
Number of children	−0.002**	−0.233***	−0.003***	−0.278***
	(0.001)	(0.041)	(0.001)	(0.052)
Household size	−0.007***	−0.171***	−0.006***	0.097**
	(0.001)	(0.037)	(0.001)	(0.044)
Marital Status: Ref.: Married/Cohabiting				
Separated/Divorced	0.193***	8.268***	0.184***	1.789***
	(0.005)	(0.247)	(0.005)	(0.184)
Widowed	0.159***	6.758***	0.155***	1.615***
	(0.007)	(0.357)	(0.007)	(0.292)
Single	0.215***	9.720***	0.206***	2.462***
	(0.005)	(0.234)	(0.005)	(0.177)
Women's education level: Ref.: None				
Primary education	0.013	0.659**	0.012	0.752*
	(0.008)	(0.319)	(0.008)	(0.392)
Secondary education	0.033***	1.425***	0.030***	1.068***
	(0.008)	(0.325)	(0.008)	(0.382)
High-school	0.072***	3.234***	0.068***	1.932***
	(0.008)	(0.320)	(0.008)	(0.412)
Vocational training	0.099***	3.869***	0.095***	1.317***
	(0.009)	(0.376)	(0.009)	(0.436)
University degree	0.233***	7.146***	0.227***	−0.769*
	(0.009)	(0.349)	(0.008)	(0.410)
Locality size: Ref.: > 100,000 inhabitants				
15,000-99,999 inhabitants	−0.012***	−0.362**	−0.010***	−0.041
	(0.003)	(0.168)	(0.003)	(0.180)
2,500-14,999 inhabitants	−0.040***	−2.076***	−0.036***	−1.527***
	(0.004)	(0.182)	(0.004)	(0.194)
Less than 2,500 inhabitants	−0.130***	−5.752***	−0.122***	−3.534***
	(0.006)	(0.209)	(0.005)	(0.246)
Constant	−0.250***	−10.227***	−0.283***	34.127***
	(0.018)	(0.677)	(0.018)	(0.876)
Controls all	yes	yes	yes	yes
State-by-quarter-by-year FE	yes	yes	yes	yes
Observations	526,706	526,706	526,706	281,532
R^2	0.130	0.099	0.122	0.026

*Notes: – Results are obtained from DiD models. The data is taken from the ENOE for the years 2017 till 2021. – Standard errors in parentheses (clustered at the state-survey year level). The regressions includes the full set of controls and state-by-quarter-by-year fixed effects.– *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Column I reports the results focusing on labor force participation, column II on hours worked, column III on probability to work (extensive margin), and column IV on conditional hours worked (intensive margin). The estimated coefficients indicate that school closure leads to a decrease in the labor force participation by 1.7 percentage points and a decrease in employment by 1.9 percentage points for women in the treatment relative to women in the control group.[22] These reductions resemble a 3 percent decrease in the labor force participation and probability of being employed relative to the pre-school closure mean and are in line with the findings from Couch *et al.* (2021) and Yamamura and Tsustsui (2021) that find that the probability to work and engage in the labor force for women with school-aged children decreases relative to other groups.

When looking at the intensive and extensive margins an interesting pattern emerges. The extensive margin confirms that indeed the proportion of women who are employed decreases. However, conditional on being employed, there is a slight increase in the number of hours worked. Therefore, school closures seem to have a direct impact on the decision to participate in the labor force (or to be employed). But if women are still in employment, they tend to adjust the hours worked just by a little. This could be explained by the fact that due to the pandemic more people were working from home and they reported working longer hours (El Heraldo De México, 2021). However, the proportion of individuals that work from home is still small, almost 19% of the workers.

Next, I exploit the impact of school closure on the same labor market outcomes by using within-individual variation in order to account for unobservables at the individual level; that is, the individual fixed effects approach. For this, I restrict the sample to women who are observed at least once before and once after the school closure. Nonetheless, since women are only observed for five quarters, this limits the time frame used to shortly before and

[22]Table 4.A4 shows that the impact of school closure on the labor force participation remains stable across different specifications, that is by providing in column I only controls, in column II adding time fixed effects, and in column III the state specific linear time trend.

after the school closure. The results for the individual fixed effects are presented in Table 4.3 showing similar findings as the baseline specification, however, the coefficients are bigger in magnitude because the effect is only measured for the sub-sample of women, that is, for the same individuals observed twice in the sample and therefore provide the immediate effect on those individuals directly after school closure. The results from the DiD take into account all women in the sample and already account for some of the recovery in 2021.

Table 4.3: EFFECT OF SCHOOL CLOSURE ON LABOR OUTCOMES: THE INDIVIDUAL FIXED EFFECTS APPROACH

Dependent variable:	**Labor Force Participation** I	**Hours worked** II	**Extensive margin** III	**Intensive margin** IV
Women with school aged kids 6-14 x School closure	-0.092^{***}	-3.439^{**}	-0.087^{**}	-2.234
	(0.034)	(1.530)	(0.034)	(2.391)
Women with school aged kids 6-14	0.047^{*}	2.124^{*}	0.030	2.017
	(0.026)	(1.168)	(0.026)	(1.823)
Individual FE	all	all	all	all
State-by-quarter-by-year FE	yes	yes	yes	yes
Observations	9,783	9,783	9,783	5,358
R^2	0.107	0.104	0.105	0.323

*Notes: – Results are obtained from DiD models. The data is taken from the ENOE for the years 2017 till 2021. – Standard errors in parentheses (clustered at the state-survey year level). The regressions includes the controls such as age and age squared, state-by-quarter-by-year-fixed effects.– *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Then, I evaluate if the school closure lead to an increase in the domestic burden of women, in this case, the outcome variable represents the number of hours per week women spend in unpaid domestic activities.

Domestic work is defined as weekly hours spent on building the household, renovating, doing household chores, and taking care of other household members. The results are presented in Table 4.4. Column I shows that because of school closure, women with school-aged children increase domestic work by 1 hour, this effect might seem small, however, looking at the pre-treatment mean we already see that women are on average working 30 hours in domestic work and spend only 20 hours on market work. This supports the fact that school closures increase the burden of child care at home. When I split the variable to hours spent doing household chores or taking care of children or the elderly, the results show that the increase is driven by taking care of other family members.

Table 4.4: Effect of School Closure on Domestic Work, Hours Caring for HH Members, or on HH Chores

	Domestic work	Hours caring	Hours HH chores
	I	II	III
Women with school aged kids 6-14 x School closure	1.021^{***}	0.545^{**}	-0.110
	(0.272)	(0.271)	(0.112)
Women with school aged kids 6-14	-7.276^{***}	-6.085^{***}	0.788^{***}
	(0.167)	(0.146)	(0.055)
Controls	all	all	all
State-by-quarter-by-year FE	yes	yes	yes
Observations	508,236	267,704	500,762
R^2	0.161	0.147	0.115

*Notes: – Results are obtained from DiD analysis. Treatment are women with school-aged children 6 to 14 years old. Control are women with nursery-aged children 0 to 5 years. The data is taken from the ENOE for the years 2017 till 2021. The sample differs here because of the missing values reported for those variables. The regressions include the full set of controls and state-by-quarter-by-year fixed effects. – Standard errors in parentheses (clustered at the state-survey year level). – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Furthermore, to check whether working or non-working women are driving this increase in domestic work, I estimate the same outcome variable as in Table 4.4 but now splitting the sample to women that work and women that do not work. That is, conditional on working how much time do working women spend on domestic work and the opposite is true, if women are not working how much time do they spend on domestic work. Table 4.A5 in the Appendix shows that women who are not working are the ones who are driving this effect, that is, they increase domestic work at home and increase their time in taking care of other household members. Women who continue to work seem to lower their time spent on doing other household chores because children are now home. This will be explored more in Section 4.5.3 by analyzing the informal child care support systems such as the presence of a grandparent.

Finally, to rule out the existence of pre-trends and to check when the impact of school closure starts to affect women in the treatment, I follow the same logic as in Eq.(4.1) but now by implementing a dynamic DiD design and estimating the impact of school closure by quarter-survey-year for an event study design to observe when the impact of school closure is observed. For this estimation, I define time zero as of the third quarter of 2021 when schools closed. The first quarter of 2017 is denoted by time -13 and the forth quarter of 2021 is denoted by time 5, respectively. The reference year is 2018. In this event study I focus on the labor force participation, employment, hours worked, and domestic work as outcome variables because they are the most affected when schools closed. Figure 4.2 shows the point estimates and the confidence interval at the 95% level by quarter-survey-year.

For the pre-treatment period, the graphs show no significant differences between women with school-aged children vs. women with nursery-aged children.[23] However, for the post-treatment period, we observe a significant difference between the treatment and control resulting in a decrease in the labor force participation, employment, and hours worked, and an increase in domestic work for women in the treatment group. More specifically, we can see that both labor force participation and employment drop in the second quarter of 2021. A plausible explanation is that at first school closure was announced to be temporary, but because it lasted about 15 months, women started dropping out of the labor force. The graphs below, show that hours worked in the market decrease directly after school closure and that the increase in domestic work also kicks-in directly after schools closed and remains positive and significant for the forth quarter of 2021.[24]

This indicates that women with school-aged children were sacrificing working hours in the market to be able to work at home and take care of other household members. These results are in line with the main specification, indicating that school closure affects the decision

[23]Here there might be a concern that the trend is already decreasing before school closure, however, the same estimation is also presented in Figure 4.A2 in the Appendix according to the survey-year and shows that the effect is observed after schools close. Moreover, I include group specific time trends to show that my results are not driven by pre-existing trends.

[24]Schools started opening in August 2021 which is the third quarter of 2021.

of women to either drop or remain in the labor market. However, they also highlight the extreme length of school closures is what finally forced women to drop out of the labor force.

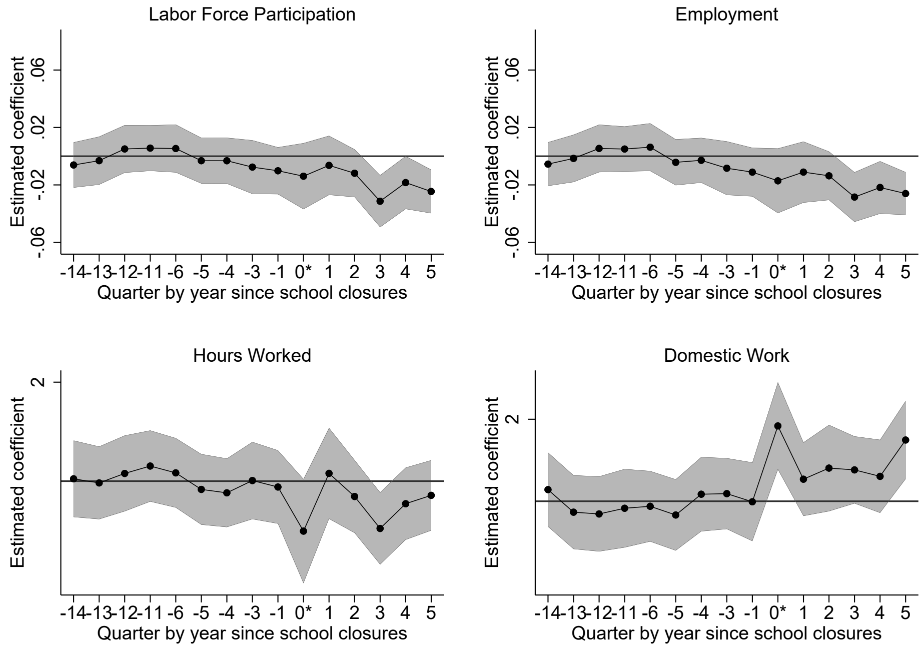

Figure 4.2: Event Study: Impact of School Closure on Labor Outcomes of Women with Children by Quarter Survey Year

Source: ENOE, authors' analysis.

Notes: – The set up of the event study design sets the time at zero since the 3rd quarter of 2020 when schools closed. The regression includes the full set of controls and state-by-quarter-by-year fixed effects. The grey area represents the confidence interval at a 95% level. time -2 is missing because no data was collected in the 2nd quarter of 2020 due to the pandemic. The year of reference is 2018, this is why the time -7 till -10 are omitted. Standard errors are clustered at the state-year level .

4.5.2 Heterogeneous Results

Now, to further analyze the main effects of the reduction in labor outcomes, I present in this section different heterogeneous analysis that take into account i) employment characteristics, and ii) household characteristics.

4.5.2.1 Employment Characteristics

First, I start by analyzing the impact of school closure on the type of employment, by testing how school closure affected formal vs. informal work, paid vs. unpaid work, and the sector of employment. The results are reported in Table 4.5.[25] When looking at formal vs. informal work (columns I and II) we observe a strong negative effect for informal work. The results for formal work are almost zero and insignificant. This highlights the fact, that during a crisis, such as COVID-19 pandemic or the school closures, the informal sector is the one that is hit the most because in countries like Latin America, labor regulations are not perfectly enforced and salaried workers can be hired formally or informally. In particular, in Mexico salaried workers, in the formal sector, are protected against firing and layoffs, and firms face a high penalty for firing those employers, in contrast to the informal sector where no regulations or penalties apply (Levy, 2010; Busso *et al.*, 2012). Moreover, statistics from the ILO (2022a) show that most of the women in Latin America self-select themselves in the informal sector by working either in restaurants, hotels, or commerce where most of the job losses occurred because of the crisis.

When looking at paid and unpaid work (columns III and IV), the results indicate that school closure had a stronger negative impact on paid work, and that unpaid work is slightly significant but the coefficient is close to zero. Furthermore, the impact of school closure on the sector of employment (columns V-VII) shows no effect on both agriculture (primary) and manufacturing (secondary), but a decrease by 2 percentage points in the services (tertiary) sector. Those findings are in line with previous literature indicating that women are more likely to work in the informal sector (Busso *et al.*, 2012; Piras *et al.*, 2014) which is mostly hit by the crisis and more specifically in the services where workers can not work remotely.

[25] The sample used here is the same sample used in the baseline results. However, the number of observations differs because the variables are set to missing for some groups. For example, for informal work the variable is set to one if women work in the informal sector, zero if the individual does not work, and missing if the individual works in the formal sector. The same logic applies for the formal, paid and unpaid employment and the type of sector.

Moreover, the results for informal and services sector remain robust when running individual fixed effects model presented in Table 4.A6 in the Appendix.

Table 4.5: EFFECT OF SCHOOL CLOSURE ON FORMAL, PAID EMPLOYMENT, AND SECTOR

Dependent variable:	Formal	Informal	Paid	Unpaid	Primary	Secondary	Tertiary
	I	II	III	IV	V	VI	VII
Women with school aged kids 6-14 x School closure	0.001	-0.033^{***}	-0.019^{***}	-0.006^{*}	-0.002	-0.005	-0.018^{***}
	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.005)	(0.005)
Women with school aged kids 6-14	0.033^{***}	0.065^{***}	0.054^{***}	0.013^{***}	0.008^{***}	0.023^{***}	0.053^{***}
	(0.003)	(0.003)	(0.003)	(0.002)	(0.001)	(0.003)	(0.003)
Mean	.484	.516	.522	.054	.027	.166	.478
Observations	381,434	390,446	513,072	259,044	252,106	293,949	469,880
R^2	0.251	0.085	0.135	0.022	0.069	0.096	0.148

*Notes: – Results are obtained from DiD models. The data is taken from the ENOE for the years 2017 till 2021. The regressions includes the full set of controls and state-by-quarter-by-year fixed effects.– Standard errors in parentheses (clustered at the state-survey year level). – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

4.5.2.2 Household Characteristics

Afterwards, I further explore the effect of school closure on different definitions such as income level, poverty level, education level, and locality size to proxy the poverty level of the household. The results are presented in Figure 4.3. They show the point estimates and the confidence interval of the effect of school closure on women with school-aged children. The figure shows marginal effects of the variable which is interacted with the respective income, poverty level, education level, or regional classification.

In panels A and B of Figure 4.3, I construct two indicators to measure the impact by poverty level. For the first definition, I interact school closure with a categorical variable indicating the household income per person per quantile (panel A). The first quantile represents households with the lowest income and the forth quantile families with high incomes respectively. Second, panel B, shows the effect of school closure interacted with the level of poverty, that is above poverty, below poverty, or below extreme poverty line.[26]

[26]For the classification of the poverty variable I rely on the information provided by the CONEVAL (2022) which uses information of the yearly average costs of the baskets of goods for rural and urban regions.

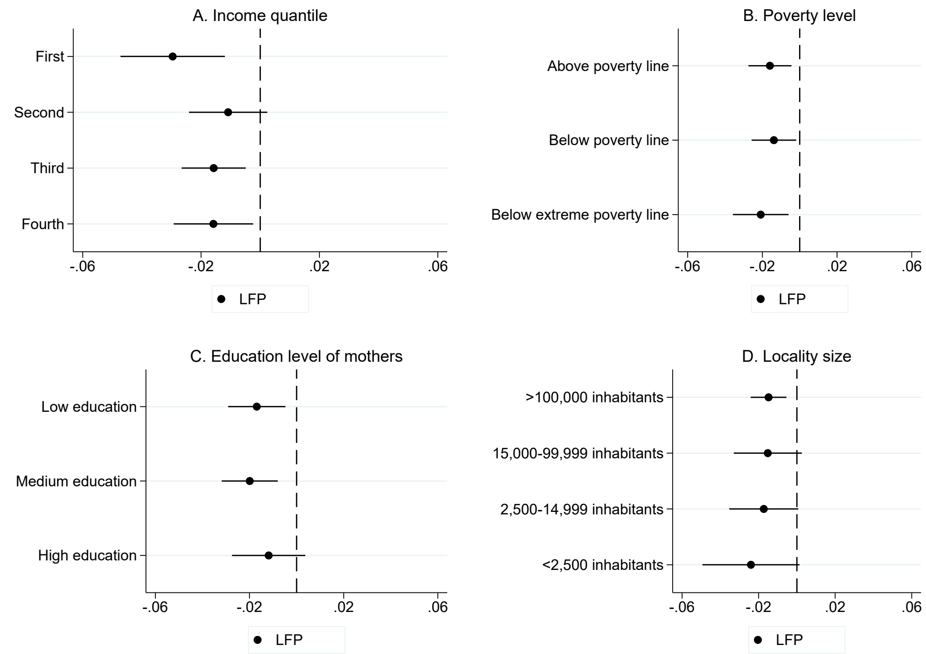

Figure 4.3: Labor Force Participation of Women with Children by Income Quantile, Poverty Level, Education Level, and Locality Size
Source: ENOE, authors' analysis.
Notes: – The figure illustrates labor force participation of women with school-aged children (6 to 14 years) versus women with nursery-aged children (0 to 5 years).

The results in panel A indicate a general decline in labor force participation of women across all income quantiles, however, the coefficients are slightly more significant for women coming from the lowest income quantile. When focusing on the more precise definition for poverty measure, the results in Panel B confirm the findings in panel A, that all women with school-aged children irrespective of their poverty or income level are mostly affected by school closure, eventhough the difference is slightly higher for women living below extreme poverty.

Third in panel C, I check the level of education of the mother because it is correlated to the labor force participation. The variable is categorized to whether the mother has low education (none, primary, and secondary), medium education (high-school and vocational training), and higher education (university degree).[27] Finally, panel D shows the interaction of school closure with the locality size to capture also the level of urbanization and the poverty of the region: >100,000 inhabitants means highly urbanized where as <2,500 means rural area. The results in panel C show that women with low and medium education levels are the ones that are affected by the school closure. When focusing on the region of residence, the estimates show that women residing in all areas whether urban or rural are affected, yet for urban areas we see more negative and significant results.[28]

As a conclusion, Figure 4.3 is important because it shows that all women were affected by school closure, and that only women who have higher education are the ones that are not affected by school closure. This could be explained by the fact that those women have probably the opportunity to work remotely or organize some type of informal child care to be able to work. Furthermore, the slightly more negative and significant effects observed for poor women residing in urban areas can be explained by the fact that poor women, coming from rural areas, have moved to the city to work without their relatives or other family members, and were therefore the most affected by school closure due to the lack of informal child care at home.

Afterwards, to estimate who is affected the most among women with school-aged children by school closure, that is, if younger or older women had to carry the burden more. I interact the impact of school closure with a categorical variable for women with different ages: (i) young women (20-29), (ii) middle-aged women (30-39), and (iii) older women (40 to 55). Figure 4.4 shows that school closure had a negative and significant impact on the labor force participation and hours worked for all women irrespective of their age.[29]

[27] This variable is categorized according to the definition of education level provided by the OECD (2021a).

[28] The results are also reported for the hours worked in Figure 4.A3 in the Appendix and show a similar pattern as Figure 4.3.

[29] Figure 4.A4 in the Appendix shows further the labor force participation of mother according to the education level.

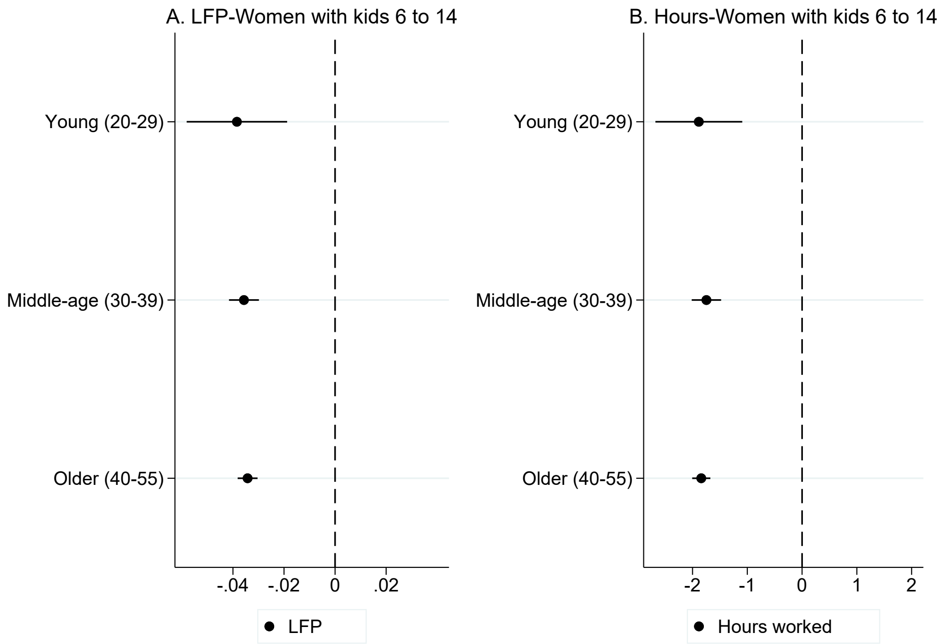

Figure 4.4: LABOR FORCE PARTICIPATION AND HOURS WORKED OF WOMEN WITH CHILDREN BY AGE DIFFERENCES OF THOSE WOMEN

Source: ENOE, authors' analysis.

Notes: – The figure illustrates labor force participation and hours worked of women with school-aged children versus women with nursery-aged children. Here women with school-aged children are split into young (20-29) to middle-age(30-39) and the older ones (40-55). School-aged children are children aged 6 to 14 years old and nursery children are children aged 0 to 5 years old.

I can conclude from the results in Figure 4.3 and Figure 4.4 that I observe negative impacts on all women with school-aged children living in urban areas irrespective of the income quantile, poverty level (slightly larger for poor women, but negative and significant for all of them), and even age. The results, indicate further that all women with school-aged children were affected by school closure and that their age does not play a role because all women had to stay home and home-school their children. This is a shock that affected all mothers, irrespective of the income quantile, poverty level, and level of urbanization. The only group that seems not to respond are highly educated mothers who are probably more attached to the labor force, could work from home, and could afford child care at home, but these women represent a small group of the sample (17%).

4.5.3 Mechanisms for Single Mothers and Informal Child Care

Another important channel to look at is access to informal child care systems such as grandparents or other relatives who live in the same area who support mothers in child care because if mothers have support like a grandparent or another adult women the probability of going to work increases (Aparicio-Fenoll and Vidal-Fernandez, 2015; Bratti *et al.*, 2018; Yamamura and Tsustsui, 2021). In fact, informal child care by grandparents is very common in Mexico and 55% of children are cared for by their grandparents while the parents are working (Villegas Raya, 2019). To do this, I construct an indicator that takes the value 1 if a grandparent lives in the household and an additional indicator that takes the value 1 if women over the age of 18 live in the household (excluding the grandparent). I exploit this information to estimate heterogeneous impacts in households with and without access to informal child care.[30]

Therefore, to test this hypothesis, I start by examining the effect of the presence of grandparent (panel A) and adult women (panel B), conditional on not having a grandparent present, in the household presented in Table 4.6. Columns I to IV (panel A and B), show that there is a level difference if grandparents or adult women are present in the household. However, the interaction term shows that mothers of school-aged children do not adjust their participation in the labor force after schools close if grandparents or adult women are living in the household. This means that those women are likely to continue working because they have informal systems at home that can alleviate child care needs.

[30]Unfortunately, the ENOE only indicates that a grandparent is present if they reside in the same household. The presence of a grandparent residing in a different household but same area is not available. The estimates provided in this framework may not be precisely estimated given that sources of informal child care outside the household are not accounted, however they provide valuable information on informal child care within the household.

Table 4.6: Heterogeneous: Effect of School Closure on Labor Outcomes

Dependent variable:	Labor force participation	Hours worked	Extensive margin	Intensive margin
	I	II	III	IV
	A. Grandparent in HH			
Women with school aged kids 6-14 x School closure	-0.017^{***}	-0.466^{**}	-0.019^{***}	0.482^{**}
	(0.005)	(0.207)	(0.005)	(0.222)
Grandparent lives in HH	0.039^{***}	2.328^{***}	0.040^{***}	1.691^{***}
	(0.008)	(0.381)	(0.008)	(0.379)
Women with school aged kids 6-14 x School closure x Grandparent	0.001	-0.263	-0.000	-0.599
	(0.014)	(0.685)	(0.014)	(0.957)
Observations	526,706	526,706	526,706	281,532
	B. Adult Women in HH			
Women with school aged kids 6-14 x School closure	-0.017^{***}	-0.413^{*}	-0.018^{***}	0.547^{**}
	(0.006)	(0.226)	(0.006)	(0.230)
Adult women living in HH	0.057^{***}	3.030^{***}	0.056^{***}	2.035^{***}
	(0.007)	(0.302)	(0.007)	(0.274)
Women with school aged kids 6-14 x School closure x Adultwomen	0.011	0.403	0.009	0.067
	(0.012)	(0.515)	(0.012)	(0.479)
Observations	498,774	498,774	498,774	264,883
	C. Single Mother			
Women with school aged kids 6-14 x School closure	-0.018^{***}	-0.491^{**}	-0.020^{***}	0.524^{**}
	(0.005)	(0.212)	(0.005)	(0.220)
Single mother	0.059^{***}	1.157^{**}	0.047^{***}	-0.678
	(0.009)	(0.525)	(0.010)	(0.447)
Women with school aged kids 6-14 x School closure x Single mother	0.021^{*}	1.003^{*}	0.022^{*}	0.061
	(0.011)	(0.580)	(0.012)	(0.530)
Observations	526,706	526,706	526,706	281,532

*Notes: – Results are obtained from DiD models. The data is taken from the ENOE for the years 2017 till 2021. – Standard errors in parentheses (clustered at the state-survey year level). The regressions includes the full set of controls and state-by-quarter-by-year fixed effects.– *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Panel C, presents the results interacted for single women. These group of women are usually highly attached to the labor market because they are the main (and most of the time only) breadwinner of their households. For them, the labor supply is generally very inelastic to shocks. The results in Panel C, confirm this by showing that school closure has almost zero impact on single women, because those women have to go to work to support their children, and therefore have already organized other means for child care.

4.5.4 Robustness Check

A potential concern is that the differences observed are driven by the differences in the labor outcomes of women in the treatment and control because women in the control group work already less and participate less in the labor force. To rule out this concern, and to show that the effects are mainly driven by school closure, I estimate the same logic in Eq.(4.1) by introducing i) a placebo school closure for the year 2018, ii) taking only men as treatment group, and iii) taking women with nursery-aged children (0 to 5) as the treatment group.

The results for the placebo test in Table 4.7 panel A show that the effect of the placebo school closure is almost zero and insignificant for labor force participation, hours worked, the extensive and intensive margins.[31]

Second, to show that only women are affected by school closure, I estimate the same baseline specification, but by taking men with school-aged children as the treatment vs. men with nursery-aged children as control presented in panel B. The interaction term in panel B shows that school closure has no impact on the labor outcomes of men (columns I to IV). This supports the results of the baseline specification, indicating that only women are affected by school closure, and more precisely those who have school-aged children.

Third, I further redefine the definition of treatment and control, to define now the treatment as the women who have nursery-aged children vs. women with no children (panel C). By this estimation I want to further emphasize that the decrease in labor force participation of women with school-aged kids is solely driven by the school closure. Therefore, I would expect to see no effect for women with very young children and women with no children. Results in panel C show indeed that school closure has no impact on those women, indicating that women who had school-aged children are affected directly and women with very young

[31] I also introduce a placebo year in 2019 to show that the effect is only driven by school closure and the results in Table 4.A7 panel A in the Appendix remain robust.

Table 4.7: Robustness: Placebo for the Effect of School Closure on Labor Outcomes

Dependent variable:	Labor force participation	Hours worked	Extensive margin	Intensive margin
	I	II	III	IV
	A. Placebo year 2018			
Women with school aged kids 6-14 x Placebo school-closure in 2018	0.000	−0.009	−0.001	0.027
	(0.005)	(0.228)	(0.005)	(0.233)
Observations	352,356	352,356	352,356	187,627
	B. Men with nursery-aged children			
Men with school aged kids 6-14 x School closure	−0.002	−0.223	−0.004	−0.067
	(0.002)	(0.171)	(0.002)	(0.157)
Observations	438,919	438,919	438,919	414,884
	C. Women with no children			
Women with nursery kids 0-5 x School closure	−0.007	−0.005	0.053	0.257
	(0.008)	(0.008)	(0.366)	(0.286)
Observations	208,921	208,921	208,921	116,660

*Notes: – Results are obtained from DiD models. In panel A, school closure is now introduced in the second quarter of year 2018 as a placebo year and the data is taken from the ENOE for the years 2017 till 2019. In panel B, the treatment group is now defined as men with school-aged children (6 to 14) and control group men with nursery-aged children (0 to 5). In Panel C, the treatment is defined as women with nursery-aged children (0 to 5) and the control group is defined as women with no children. For both panels B and C the data is taken from the ENOE for the years 2017 to 2021. – Standard errors in parentheses (clustered at the state-survey year level). The regressions includes the full set of controls and state-by-quarter-by-year fixed effects.– *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

children are not. Those findings are also in line with the findings of Couch *et al.* (2021) and Petts *et al.* (2021) that show women with very young children are not as affected as women with school-aged children.[32]

Furthermore, to explore if the results are robust using alternative control groups and that they are not driven by different trends, I redefine the control group as i) women with children aged 0 to 3, ii) women with no children, and iii) men with school-aged children 6 to 14. The results are presented in Table 4.8 in panels A, B, and C respectively.

The estimates in Table 4.8, panel A show that by redefining the control group to women with children aged 0 to 3, the results yield similar coefficients as the baseline specification,

[32] The results remain also robust when I further refine treatment as women with children in the age range of 0 to 3 vs. women with no children as the control group in Table 4.A7 panel B in the Appendix.

Table 4.8: Robustness: Alternative Control Groups

Dependent variable:	Labor force participation	Hours worked	Extensive margin	Intensive margin
	I	II	III	IV
	A. Control-women with children 0 to 3			
Women with school aged kids 6-14 x School closure	-0.014^{**}	-0.339	-0.017^{***}	0.587^{**}
	(0.006)	(0.272)	(0.006)	(0.275)
Observations	467,157	467,157	467,157	253,639
	B. Control-women without children			
Women with school aged kids 6-14 x School Closure	-0.026^{***}	-0.699^{*}	-0.026^{***}	0.424
	(0.008)	(0.387)	(0.008)	(0.274)
Observations	482,834	482,834	482,834	279,187
	C. Control-men with school-aged children			
Women with school aged kids 6-14 x School closure	-0.037^{***}	-1.650^{***}	-0.040^{***}	-0.398
	(0.007)	(0.326)	(0.007)	(0.295)
Observations	712,041	712,041	712,041	516,669

*Notes: – Results are obtained from DiD models. The data is taken from the ENOE for the years 2017 to 2021. Treatment group is defined as women with school-aged children (6 to 14). Control groups are defined as women with children aged 0 to 3 (panel A), as women without children (panel B), and men with school-aged children (6 to 14, in panel C) – Standard errors in parentheses (clustered at the state-survey year level). The regressions includes the full set of controls and state-by-quarter-by-year fixed effects.– *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

that is a decrease in the labor force participation and employment of women with school-aged children. Panel B shows the results by changing the control group to women without children, the results also remain robust. That is, the decrease for women with school-aged children is still observed and the coefficients are slightly higher in magnitude.[33]

Next, by analyzing the gender gap (panel C) for women and men with school-aged children to evaluate the impact of school closures, a similar approach like Couch *et al.* (2021), my treatment group is now defined as women with school-aged children vs. control group, men with school-aged children. The findings in Table 4.8 panel C, show a decrease in the labor force participation of women in the treatment by 3.7 percentage points in comparison

[33] I further change the age range of children to test the results for women with children 6 to 17 years old and the results still hold. Results are available upon request.

to men and that employment decreases by almost 4 percentage points. Those findings are in line with the results presented by Couch *et al.* (2021) that show that only women with school-aged children vs. men with school-aged children decrease their employment to population ratio by 4.3 percentage points, and the results by Yamamura and Tsustsui (2021) show a similar pattern, that women with school-aged children are the ones that carry the burden of child care.

Table 4.9: ROBUSTNESS: GROUP TRENDS AND DIFFERENT AGE GROUPS

Dependent variable:	**Labor force participation**	**Hours worked**	**Extensive margin**	**Intensive margin**
	I	II	III	IV
	A. Group specific time trend			
Women with school aged kids 6-14 x School closure	-0.034^{***}	-1.557^{***}	-0.038^{***}	-0.392^{**}
	(0.004)	(0.188)	(0.004)	(0.177)
Observations	526,706	526,706	526,706	281,532
	B. Women in the age 18 to 55			
Women with school aged kids 6-14 x School closure	-0.017^{***}	-0.441^{**}	-0.019^{***}	0.494^{**}
	(0.005)	(0.208)	(0.005)	(0.213)
Observations	526,706	526,706	526,706	281,532
	C. Year 2018-2021			
Women with school aged kids 6-14 x School closure	-0.016^{***}	-0.413^{*}	-0.018^{***}	0.510^{**}
	(0.005)	(0.218)	(0.005)	(0.226)
Observations	407,952	407,952	407,952	219,597

Notes: – Results are obtained from DiD models. – Standard errors in parentheses (clustered at the state-survey year level). The regressions includes the full set of controls, state fixed effects and group specific time trend for panel A. The regressions includes the full set of controls and state-by-quarter-by-year fixed effects for panels B and C.– *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Next, to better take into account the pre-existing trends between the group of women with school-aged children relative to women with nursery-aged children that might be driving the results. I show in Table 4.9 panel A, the baseline estimates by adding a group specific time trend. The coefficients in Table 4.9 show that the labor outcome variables are negative and significant for all labor variables. This shows that the results estimated are not driven by those pre-existing time trends between groups.

Finally, since the teen fertility rate in Mexico is high, I alter the age range of women from 20 to 55, to include now women with school-aged children aged 18 to 55 (Table 4.9 panel B), and the results remain robust.[34] That is a decrease in labor force participation, hours worked and the extensive margin of women in the treatment in comparison to the control group. Moreover, I also change the time period to 2018-2021 (Table 4.9 panel C) and the results remain robust to the changes in the time frame.[35]

4.6 Conclusion

This paper contributes to the literature by providing the first empirical evidence on the direct impact of school closure on the employment of women with school-aged children in developing countries. It also adds to the literature focusing on developed countries (see e.g., Adams-Prassl *et al.*, 2020; Amuedo-Dorantes *et al.*, 2020; Landivar *et al.*, 2020; Collins *et al.*, 2021; Couch *et al.*, 2021) by deviating from previous work that focuses on the gap between men and women, to focus instead on women with children in different age groups and women without children. To estimate the causal impact of school closure due to the COVID-19 pandemic, I compare women with school-aged children to women with nursery-aged children. While both groups of women were exposed to the economic shock of the COVID-19 pandemic, women with children in school age faced a sudden increase in the time children spend at home. In contrast, women with younger children were not as affected by school closures given that their children are too young to be enrolled in the schooling system.

[34]The results remain robust also when including women in the age range 20 to 60. Results are presented in Table 4.A8 panel A in the Appendix.

[35]limiting the time frame from 2019 to 2021 also does not affect the estimates, results are presented in Table 4.A8 panel B in the Appendix.

Focusing on women with nursery-aged children to build the control group instead of men, reassures that the parallel trend assumption is not violated. Visual inspection of pre-treatment evolution of treatment and control groups shows that women with children in different age groups (and even without children) despite having level difference in terms of their probability to work, follow very similar trends. In contrast, the path of men and women is hardly comparable because men usually have a much more inelastic labor supply than women due to persisting cultural attitudes (Jaumotte, 2003).

Using data from the Mexican Labor Force Survey (ENOE), I implement a DiD approach that exploits school closure in March 20, 2020 as a natural cutoff to assign women with school-aged children in the treatment group and women with nursery-aged children in the control group. My results show, in line with Couch *et al.* (2021) and Yamamura and Tsustsui (2021), that women with school-aged children are the ones that are mostly affected by school closure. The labor force participation of those women decreases by 1.7 percentage points, and their employment by 1.9 percentage points. This effect translates to a decrease in labor force participation and employment by 3%. However, I also deviate from their work to further show that my results remain robust also when accounting for unobservables by using the individual effects model.

Bundervoet *et al.* (2022) show that the vulnerable groups, like women and the low educated, in the society are most likely to be affected by such recessions. My results show indeed that school closure affects women with low and medium education level, however, this study is the first to deviate from other studies in the literature to show that not only vulnerable groups in the society are affected by the crisis in the economy. I find that school closure affects all women with school-aged children across all income quantiles, poverty levels, urbanization level, and even age. Only women that are highly educated seem not to be affected by this crisis because they can work remotely or they have arranged some type of child care. I further go beyond those studies done to show that women with access to informal child care, such as presence of a grandmother or adult women in the household, are not affected by school closure. My results, also show that single women who are the main breadwinner depend also on other sources of child care and therefore were also not affected by this shock. This points out the importance of other sources of child care for women to be able to stay or join the labor market.

Moreover, since a high proportion of the women work in the informal sector in Mexico, the decrease in the labor force participation is mostly concentrated in that sector. By looking at the type of sector, I observe that agriculture and manufacturing sectors are not affected but the services sector is the mostly affected one because this type of work has to be done in the workplace and can not be done remotely.

The results provided in this paper are highly important because they provide the direct effect of the school closure independently from COVID-19. This sheds the light on the importance of schools as child care provider and that other solutions should be thought of before closing schools. This also shows that schools are a powerful instrument for women with children to enter the labor force. The COVID-19 pandemic had negative effects on the labor force participation of women and by closing schools this negative effect was made stronger, many women were forced to stay home and care for their children. This might have a long term negative impact on the employment of those women, causing many of them to stay home and not go back to the labor market, or even be penalized for dropping out of the labor force.

My results have important policy implications not only for Mexico, but also for other Latin American countries, where school closures lasted the most, and labor force participation of women is still small. First, school closures have been shown to affect negatively student achievement and attainment (Larsen, 2020; Grewenig *et al.*, 2021; Jack *et al.*, 2022), but also affected the labor force participation of women with school-aged children. Second, schooling is an important form of child care that enables women to join the labor force (Bick, 2016; Brilli *et al.*, 2016). Third, systems are needed in place to deal with shocks that allow for children to continue to attend school in a safe way, such as opening schools for two-shifts, students wearing masks, or regular COVID-19 testing for students and staff (Di Domenico *et al.*, 2021). Fourth, the quality of jobs for women has to increase (mostly informal in the services sectors). Women need to diversify their employment in other industries and have access to decent work. Decent work is a key element for better productivity and in fighting poverty (ILO, 2022b).

Finally, both women and men need to have access to less rigid employment systems because working remotely in Mexico is rather an exception to the employment rules. Due to school closures, this paper has shown that women held the double burden of home-schooling and working in the market. Therefore, gender norms in developing countries should also change so that both men and women will be able to arrange household work and market work ensuring gender equality between both.

Appendix Chapter 4

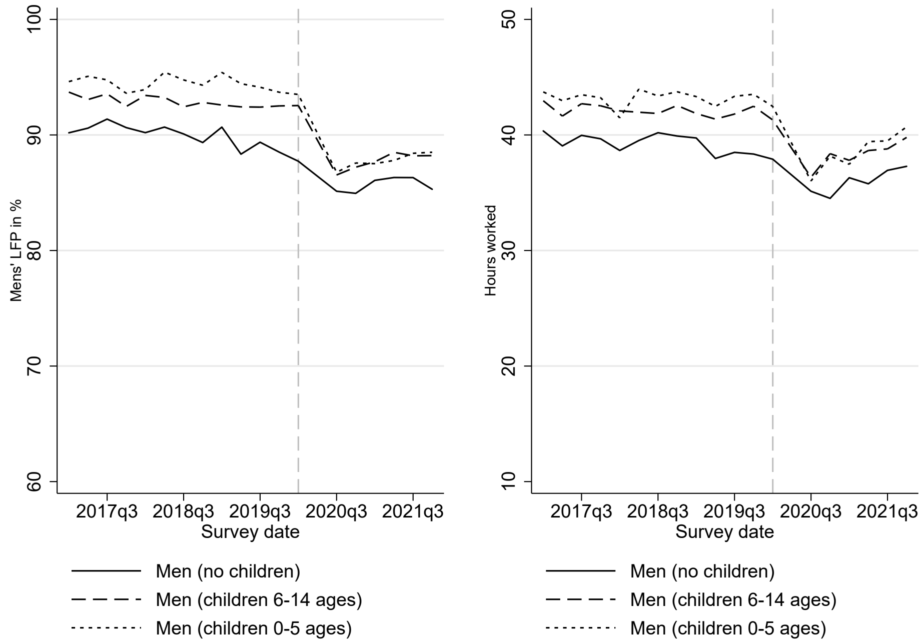

Figure 4.A1: Labor Force Participation of Men by Survey Year
Source: ENOE, authors' analysis.
Notes: – The figure illustrates labor force participation of men with no children versus men with children in the age range from 0 to 14.

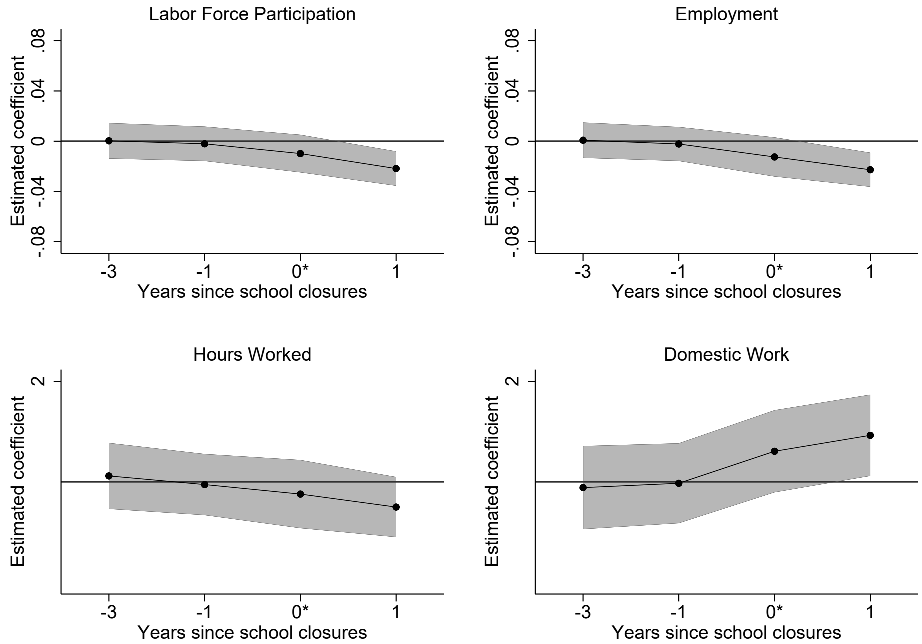

Figure 4.A2: Event Study: Impact of School Closure by Survey Year
Source: ENOE, authors' analysis.
Notes: – The set up of the event study design sets the time at zero since survey year 2020 when schools closed. The regression includes the full set of controls and state-by-quarter-by-year fixed effects. The grey area represents the confidence interval at a 95% level. Standard errors are clustered at the state-year level and the year of reference is the 2018.

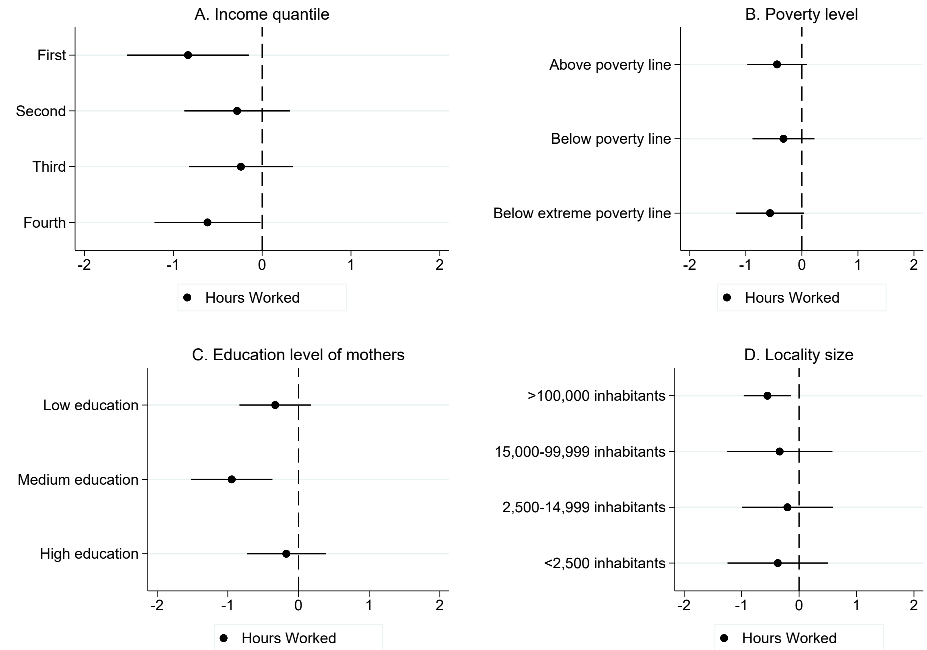

Figure 4.A3: Weekly Hours Worked of Women with Children by Income Quantile, Poverty Level, Education Level, and Locality Size
Source: ENOE, authors' analysis.
Notes: – The figure illustrates marginal effects of weekly hours worked of women with school-aged children (6 to 14 years) versus women with nursery-aged children (0 to 5 years old) .

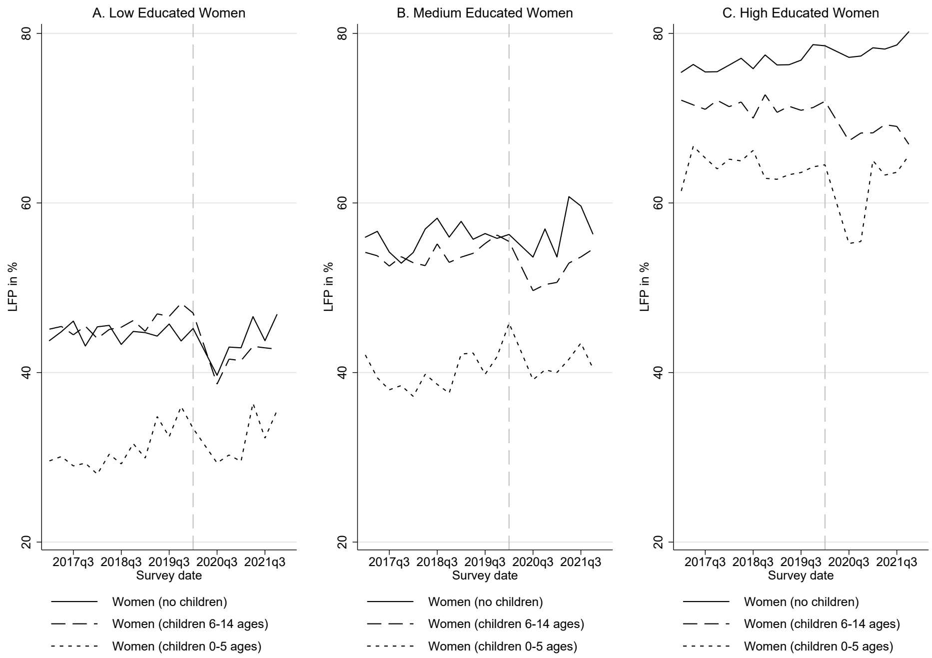

Figure 4.A4: Labor Force Participation of Women According to Education Level

Source: ENOE, authors' analysis.

Notes: – The figure illustrates labor force participation of women according to the level of education. Low educated women have either no, primary education, or secondary education. Medium educated women have high-school or vocational training. High educated women have University degree.

Table 4.A1: Summary of Treatment and Control Groups

Estimates	Treatment	Control
Baseline Results:	Women with school-aged children 6 to 14	Women with nursery-aged children 0 to 5
Heterogenoues Analysis:	Women with school-aged children 6 to 14	Men with school-aged children 6 to 14
Robustness Checks:	Women with school-aged children 6 to 14	Women with no children
	Women with school-aged children 6 to 14	Women with children 0 to 3
	Women with children aged 6 to 17	Women with nursery-aged children 0 to 5
	Women with nursery-aged children 0 to 5	Women with no children
	Women with children aged children 0 to 3	Women with no children
	Men with school-aged children 6 to 14	Men with nursery-aged children 0 to 5

Notes: – The table presents a summary of all the definitions used for treatment and control groups through out the baseline, heterogeneous, and robustness estimations.

Table 4.A2: Descriptive Statistics: Post-School Closure Women with Children

	All		Treatment before		Control before		T-test
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Δ Mean[a]
Dependent variables							
Labor force participation	0.552	0.497	0.567	0.496	0.500	0.500	0.067***
Employed	0.535	0.499	0.550	0.497	0.483	0.500	0.068***
Total hours worked	19.096	22.415	19.736	22.613	16.819	21.540	2.917***
Domestic Work	32.343	18.172	30.504	17.215	39.202	19.919	−8.697***
Conditional dependent variables							
Conditional hours worked	35.663	18.640	35.865	18.730	34.844	18.252	1.021***
Formal work conditional on working	0.498	0.500	0.490	0.500	0.530	0.499	−0.040***
Paid employment	0.955	0.207	0.954	0.209	0.960	0.197	−0.005***
Sector							
Primary	0.025	0.156	0.026	0.161	0.019	0.135	0.008***
Secondary	0.183	0.386	0.184	0.387	0.179	0.383	0.005
Tertiary	0.793	0.406	0.790	0.407	0.803	0.398	−0.013***
Control variables							
Women with school aged kids 6-14	0.781	0.414	1.000	0.000	0.000	0.000	1.000
School closure	1.000	0.000	1.000	0.000	1.000	0.000	0.000
Age	39.258	8.395	40.886	7.163	33.467	9.785	7.419***
Spouse present	0.731	0.444	0.719	0.450	0.775	0.418	−0.056***
Grandparent present	0.050	0.217	0.049	0.216	0.052	0.222	−0.003**
Number of children	2.496	1.316	2.685	1.315	1.823	1.081	0.862***
Marital Status							
Married/Cohabiting	0.842	0.365	0.829	0.377	0.887	0.316	−0.059***
Separated/Divorced	0.079	0.269	0.087	0.282	0.050	0.218	0.037***
Widowed	0.020	0.139	0.022	0.147	0.012	0.109	0.010***
Single	0.060	0.237	0.062	0.241	0.051	0.219	0.011***
Mother's education level							
No education	0.018	0.134	0.020	0.140	0.012	0.109	0.008***
Primary education	0.160	0.367	0.174	0.379	0.110	0.312	0.064***
Secondary education	0.346	0.476	0.361	0.480	0.294	0.456	0.067***
High-school	0.215	0.411	0.203	0.402	0.261	0.439	−0.058***
Vocational training	0.048	0.213	0.051	0.221	0.036	0.186	0.016***
University degree	0.212	0.409	0.191	0.393	0.287	0.453	−0.096***
Locality size							
More than 100,000 inhabitants	0.600	0.490	0.594	0.491	0.621	0.485	−0.027***
15,000-99,999 inhabitants	0.131	0.337	0.132	0.338	0.128	0.334	0.004*
2,500-14,999 inhabitants	0.123	0.328	0.125	0.331	0.113	0.317	0.012***
Less than 2,500 inhabitants	0.147	0.354	0.149	0.357	0.138	0.345	0.011***
Observations	144,384		112,709		31,675		

Notes: – The table presents descriptive statistics for post-school closure trends of women with children before the COVID-19 pandemic for the years 2017-2021. Treatment group is defined as women who have school-aged children (6 to 14 years) and control group as women who have nursery age children (0 to 5 years). [a] This column represents the difference between treatment and control and the respective t-test.

Table 4.A3: DESCRIPTIVE STATISTICS: PRE-SCHOOL CLOSURE FOR WOMEN WITH CHILDREN VS. WOMEN WITH NO CHILDREN

	All		Treatment before		Control before		T-test
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Δ Mean[a]
Dependent variables							
Labor force participation	0.732	0.443	0.538	0.499	0.706	0.456	−0.167***
Employed	0.706	0.456	0.526	0.499	0.681	0.466	−0.156***
Total hours worked	29.921	24.367	18.921	22.203	26.576	22.666	−7.655***
Domestic Work	17.835	18.194	33.231	17.412	18.855	13.072	14.376***
Conditional dependent variables							
Conditional hours worked	42.412	17.659	36.007	17.971	39.025	16.389	−3.018***
Formal work conditional on working	0.524	0.499	0.466	0.499	0.659	0.474	−0.193***
Paid employment	0.963	0.188	0.947	0.224	0.970	0.170	−0.023***
Sector							
Primary	0.070	0.254	0.024	0.154	0.012	0.109	0.012***
Secondary	0.269	0.443	0.166	0.372	0.162	0.368	0.004**
Tertiary	0.662	0.473	0.810	0.392	0.826	0.379	−0.016***
Women with children	0.925	0.263	1.000	0.000	0.000	0.000	1.000
School closure	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Age	36.275	10.335	40.342	9.036	34.012	9.742	6.330***
Spouse present	0.719	0.450	0.744	0.436	0.560	0.496	0.184***
Grand parent present	0.053	0.224	0.050	0.219	0.062	0.242	−0.012***
Number of chilldren	1.937	1.599	2.698	1.380	0.000	0.000	2.698***
Marital Status							
Married/Cohabiting	0.637	0.481	0.843	0.364	0.666	0.472	0.178***
Separated/Divorced	0.063	0.243	0.078	0.268	0.012	0.110	0.066***
Widowed	0.012	0.110	0.024	0.154	0.005	0.069	0.019***
Single	0.287	0.453	0.055	0.227	0.317	0.465	−0.263***
Mother's education level							
No education	0.043	0.204	0.025	0.155	0.013	0.112	0.012***
Primary education	0.253	0.435	0.206	0.404	0.085	0.279	0.121***
Secondary education	0.310	0.462	0.349	0.477	0.176	0.381	0.173***
High-school	0.161	0.368	0.188	0.391	0.196	0.397	−0.008***
Vocational training	0.071	0.256	0.064	0.244	0.046	0.211	0.017***
University degree	0.162	0.369	0.169	0.374	0.484	0.500	−0.315***
Locality size							
More than 100,000 inhabitants	0.604	0.489	0.579	0.494	0.709	0.454	−0.130***
15,000-99,999 inhabitants	0.133	0.340	0.136	0.342	0.116	0.320	0.020***
2,500-14,999 inhabitants	0.123	0.328	0.129	0.335	0.088	0.283	0.041***
Less than 2,500 inhabitants	0.140	0.347	0.156	0.363	0.087	0.282	0.069***
Observations	2,538,763		818,769		66,353		

Notes: – The table presents descriptive statistics for post-school closure trends of women with children before the COVID-19 pandemic for the years 2017-2021. Treatment are women with children vs. women with no children. [a] This column represents the difference between treatment and control and the respective t-test.

Table 4.A4: Effect of School Closure on the Labor Force Participation of Women with Children

	I	II	III
Women with school aged kids 6-14 x School closure	-0.016^{**}	-0.017^{***}	-0.017^{***}
	(0.008)	(0.005)	(0.005)
Women with school aged kids 6-14	0.055^{***}	0.055^{***}	0.055^{***}
	(0.003)	(0.003)	(0.003)
Age	0.041^{***}	0.041^{***}	0.041^{***}
	(0.001)	(0.001)	(0.001)
Age-squared	-0.001^{***}	-0.001^{***}	-0.001^{***}
	(0.000)	(0.000)	(0.000)
Spouse present	-0.124^{***}	-0.122^{***}	-0.122^{***}
	(0.004)	(0.004)	(0.004)
Grandparent present	0.028^{***}	0.030^{***}	0.030^{***}
	(0.004)	(0.004)	(0.004)
Number of children	-0.001	-0.002^{**}	-0.002^{**}
	(0.001)	(0.001)	(0.001)
Household size	-0.008^{***}	-0.007^{***}	-0.007^{***}
	(0.001)	(0.001)	(0.001)
Marital Status: Ref.: Married/Cohabiting			
Separated/Divorced	0.193^{***}	0.193^{***}	0.193^{***}
	(0.005)	(0.005)	(0.005)
Widowed	0.158^{***}	0.159^{***}	0.159^{***}
	(0.007)	(0.007)	(0.007)
Single	0.215^{***}	0.215^{***}	0.215^{***}
	(0.005)	(0.005)	(0.005)
Women's education level: Ref.: None			
Primary education	0.018^{*}	0.013	0.013
	(0.010)	(0.008)	(0.008)
Secondary education	0.042^{***}	0.033^{***}	0.033^{***}
	(0.011)	(0.008)	(0.008)
High-school	0.084^{***}	0.072^{***}	0.072^{***}
	(0.010)	(0.008)	(0.008)
Vocational training	0.109^{***}	0.099^{***}	0.099^{***}
	(0.012)	(0.009)	(0.009)
University degree	0.247^{***}	0.233^{***}	0.233^{***}
	(0.011)	(0.009)	(0.009)
Locality size: Ref.: > 100,000 inhabitants			
15,000-99,999 inhabitants	-0.011^{***}	-0.012^{***}	-0.012^{***}
	(0.004)	(0.003)	(0.003)
2,500-14,999 inhabitants	-0.042^{***}	-0.040^{***}	-0.040^{***}
	(0.005)	(0.004)	(0.004)
Less than 2,500 inhabitants	-0.134^{***}	-0.130^{***}	-0.130^{***}
	(0.006)	(0.006)	(0.006)
Constant	-0.219^{***}	-0.245^{***}	-0.243^{***}
	(0.020)	(0.018)	(0.018)
Controls	yes	yes	yes
Time fixed effects	no	yes	no
State specific trend	no	no	yes
Observations	526,706	526,706	526,706
R^2	0.121	0.129	0.129

Notes: – Results are obtained from DiD analysis. Treatment group are women with children aged 6 to 14 years old. Control group are women with children aged 0 to 5 years. – Standard errors in parentheses (clustered at the state-survey year level). – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 4.A5: Effect of School Closure on Domestic Work Conditional on Working or not Working

	Domestic work	Hours caring	Hours HH chores
	I	II	III
	A. Conditional on not working		
Women with school aged kids 6-14 x School closure	1.192***	0.954***	−0.194
	(0.316)	(0.302)	(0.166)
Women with school aged kids 6-14	−7.442***	−6.672***	1.526***
	(0.205)	(0.188)	(0.087)
Observations	234,729	136,188	232,283
	B. Conditional on working		
Women with school aged kids 6-14 x School closure	0.451	−0.098	−0.275**
	(0.298)	(0.299)	(0.119)
Women with school aged kids 6-14	−6.123***	−5.124***	0.778***
	(0.176)	(0.142)	(0.058)
Observations	273,507	131,516	268,479

*Notes: – Results are obtained from DiD analysis. Treatment are women with school-aged children 6 to 14 years old. Control are women with nursery-aged children 0 to 5 years. The data is taken from the ENOE for the years 2017 till 2021. The sample differs here because of the missing values reported for those variables. The regressions include the full set of controls and state-by-quarter-by-year fixed effects. – Standard errors in parentheses (clustered at the state-survey year level). – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table 4.A6: Effect of School Closure According to Sector: Individual F.E.

Dependent variable:	**Formal**	**Informal**	**Paid**	**Unpaid**	**Primary**	**Secondary**	**Tertiary**
	I	II	III	IV	V	VI	VII
Women with school aged kids 6-14 x School closure	−0.031	−0.137***	−0.071**	−0.082**	−0.016	−0.031	−0.112***
	(0.044)	(0.047)	(0.035)	(0.036)	(0.025)	(0.050)	(0.038)
Women with school aged kids 6-14	−0.088***	0.121***	0.018	0.058**	0.004	0.020	0.031
	(0.034)	(0.036)	(0.027)	(0.028)	(0.019)	(0.038)	(0.030)
Individual FE	all	all	all	all	all	all	all
State-by-quarter-by-year FE	yes	yes	yes	yes	yes	yes	yes
Observations	7,213	6,995	9,546	4,665	4,528	5,421	8,664
R^2	0.192	0.209	0.110	0.363	0.338	0.292	0.132

*Notes: – Results are obtained from DiD models. The data is taken from the ENOE for the years 2017 till 2021. The regressions includes age and age squared as controls as well individual fixed effects.– Standard errors in parentheses (clustered at the state-survey year level). – *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.*

Table 4.A7: Robustness: Placebo for the Effect of School Closure on Labor Outcomes

Dependent variable:	**Labor force participation**	**Hours worked**	**Extensive margin**	**Intensive margin**
	I	II	III	IV
	A. Placebo year 2019			
Women with school aged kids 6-14 x Placebo school-closure in 2019	−0.007	−0.101	−0.007	0.336
	(0.006)	(0.231)	(0.006)	(0.275)
Women with school aged kids 6-14	0.054***	2.023***	0.053***	0.201
	(0.004)	(0.189)	(0.004)	(0.219)
Observations	234,202	234,202	234,202	127,130
	B. Women with children 0 to 3			
Women with children 0-3 x School closure	−0.012	−0.009	−0.325	−0.084
	(0.008)	(0.008)	(0.390)	(0.315)
Women with children 0-3	−0.109***	−0.102***	−5.864***	−3.444***
	(0.007)	(0.007)	(0.318)	(0.281)
Observations	159,413	159,413	159,413	94,962

Notes: – Results are obtained from DiD models. For panel A the data is taken from the ENOE for the years starting the second quarter of 2018 until the first quarter of 2020. School closure is now introduced in the second quarter of year 2019 as a placebo year. For Panel B the data is taken from the ENOE for the years 2017 till 2021. The age range of treatment and control is now changed from 20 to 55 years old women. For panel B the treatment are defined as women with 0 to 3 aged children. Control group is defined as women with no children. – Standard errors in parentheses (clustered at the state-survey year level). The regressions includes the full set of controls and state-by-quarter-by-year fixed effects.– *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 4.A8: Robustness: Different Age Range and Survey Years

Dependent variable:	**Labor force participation**	**Hours worked**	**Extensive margin**	**Intensive margin**
	I	II	III	IV
	A. Women age range 20 to 60			
Women with school aged kids 6-14 x School closure	-0.017^{***}	-0.471^{**}	-0.019^{***}	0.460^{**}
	(0.005)	(0.209)	(0.005)	(0.213)
Women with school aged kids 6-14	0.055^{***}	2.165^{***}	0.055^{***}	0.387^{***}
	(0.003)	(0.137)	(0.003)	(0.144)
Observations	526,706	526,706	526,706	281,532
	B. Year 2019-2021			
Women with school aged kids 6-14 x School closure	-0.015^{***}	-0.409^{*}	-0.017^{***}	0.396
	(0.006)	(0.238)	(0.006)	(0.258)
Women with school aged kids 6-14	0.055^{***}	2.154^{***}	0.054^{***}	0.422^{**}
	(0.004)	(0.175)	(0.004)	(0.193)
Observations	291,747	291,747	291,747	158,311

Notes: – Results are obtained from DiD models. For Panel A the data is taken from the ENOE for the years 2019 till 2021, for Panel B the data ranges for the years 2017 until 2021. – Standard errors in parentheses (clustered at the state-survey year level). The regressions includes the full set of controls and state-by-quarter-by-year fixed effects.– *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

5 Conclusion

This dissertation is motivated by i) the high number of working children and by ii) the low labor force participation rates of women in comparison to men in Latin America. Improvements have been made throughout the last 4 years such that child labor rates dropped from 10% in 2008 to 6% in 2020 (ILO, 2020a), and women's labor force participation has been increasing from 39% to 42% for the years 1990 to 2019 (The World Bank, 2022c).

This progress made in Latin America, has been unfortunately disrupted by the COVID-19 pandemic, because of school closures, job losses, and rising poverty. On the one hand, around 100,000 to 326,000 children are expected to work again because of the income losses families have faced (UNICEF, 2021b). On the other hand, the COVID-19 pandemic has affected mostly sectors where women work. Therefore, the unemployment rate for women increased to 12.4% for the years 2020 and 2021 compared to 9.7% in 2019. The pandemic caused the employment rate of women to decrease by 3.6% in 2021 compared to 2019 (ILO, 2021). Therefore, in my dissertation, I evaluate how reform and education policies help improve the welfare of those vulnerable groups in the society by focusing on the region of Latin America, in particular looking at Mexico.

The first reform discussed in **Chapter 2** is the amendment of the minimum working age in Mexico from 14 to 15 in 2015. I analyze the impact on child labor due to the change in minimum working age. The Labor Law not only increases the minimum working age by one year, but also sets regulations and certain requirements for the work of individuals under the age of 18. To be able to analyze the effect, I look at the simple reform done in the Constitutional Amendment in 2014 where the government only announced the change in the minimum working age and compare it to the 2015 Labor Law change that sets penalties on employers. My findings show that only when the change of the law is coupled with regulations to make sure that the law is enforced, child labor decreases and school enrollment increases for the affected cohort. This is a very important policy implication, because it shows that only a mere shift in the minimum working age which is not coupled with regulations to ensure enforcement of the law and punish employers, does not reduce child labor.

The second reform discussed in **Chapter 3** is the extension of the instruction time at school from 20 to either 30 or 40 hours for the years 2009 until 2018. I analyze what happens to child labor and school enrollment since students have to stay longer in school. My findings show that due to longer instruction time, child labor decreases for children aged 5 to 17 years old and school enrollment is not affected. The insignificant effect in school enrollment alleviates the concern that parents might remove children from school to go to work. The findings further show that by increasing the instruction time, the labor force participation of the mother's of the affected group increase their labor force participation. This reform is also very important for policy makers, because it shows that by increasing the time spent in school not only child labor decreases but the labor force participation of the mothers increase as well. This show also that education or schooling drives children away from child labor.

So far, we have seen that education is important to keep children away from child labor. Moreover, my findings have also shown that because children spend more time at school, women's labor force participation increases. But what happens with women's labor force participation when policy makers shut down the schooling system due to a sanitary emergency like the COVID-19 pandemic.

In **Chapter 4** I evaluate the effect of school closures caused by the COVID-19 pandemic on the labor force participation of women with school-aged children 6 to 14 for the years 2017 until 2021. My findings indicate that due to school closures women with school-aged children decrease their labor force participation and their employment rate. The decrease is mostly observed for the informal and services sector. My analysis shows further that women who had access to informal child care such as having a grandparent at home are not affected by the school closure. This reform is highly important because it highlights the significance of schools in acting as a child care substitute for women, and therefore allowing women to enter the labor market. By closing schools women will tend to reduce their labor force participation or even stop working. Policy makers should acknowledge that and implement other policies to fight the pandemic such as having two shifts at schools or regular testing for students instead of complete shut-down. Policy makers should also realize the importance of flexible working hours and working remotely for women when they want to close schools, because in developing countries like Mexico, the normal working day is rather the rigid 8 hours and working from home is the exception.

In general, this dissertation has three key messages in the context of economic development for Latin American countries. First, a steady decrease in child labor rates can be further encouraged through regulation establishing a minimum working age coupled with i) restrictions to hire underage individuals who did not complete basic education, ii) to put regulations on the hours worked, type of activities, and working schedule of individuals above 15 and younger than 18, and iii) put penalties on employers that violate those regulations. Second, a shift in the schooling day from part-time to full-time will not only increase academic outcomes of children, but also has the potential to decrease child labor rates while encouraging the labor force participation of mothers of young children. Finally, the need of security systems to cope with sudden events such as sanitary emergencies is of utmost relevance to avoid disproportionate impacts on different vulnerable groups.

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